Empirical Essays on the Economics of the Family

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Preface

Preface

This thesis is comprised of four stand-alone research papers in four chapters that can be read independently and stand in no particular order. In the first chapter, I develop a bargaining model of fertility and empirically test the model using preference data. In the second chapter, I empirically investigate the effects of family non-intactness during childhood on young adults' civic engagement. In the third chapter, I analyze labeling effects of child benefits on expenditure and savings. In the fourth chapter, I empirically investigate the effects of child care availability on fertility. In what follows I provide non-technical summaries and outlooks of the four chapters.

Chapter 1: "Do Couples Bargain over Fertility? Evidence Based on Child Preference Data"

The first chapter is concerned with couples' decisions over having children. I develop a bargaining model which allows for individual child preference heterogeneity to describe fertility decisions theoretically and I test the model's predictions with survey data. More specifically, I test the bargaining model against the unitary framework, which treats households as single decision makers and neglects any consequences of discordant preferences. The following paragraphs give a short overview of the model and the results.

In the beginning, I describe a theoretical framework which incorporates individual child preferences and bargaining power into a couple's maximization problem. The rationale is that decisions are outcomes of cooperative bargaining between partners. A sharing rule which weighs individual utility functions is assumed to reflect relative bargaining power. The more bargaining power an individual has, the more say she has in household decisions. In the model, relative wages are assumed to be the key determinant of bargaining power and fertility is defined as a household public good. If child preferences are discordant within couples, bargaining power influences the decision outcome. In particular, if an individual has stronger child preferences than her partner, her bargaining power positively influences birth probability. Conversely, the effect of bargaining power is negative if her child preferences are weaker than her partner's. The model also captures negative opportunity cost effects of wages on fertility.

I test the theoretical predictions in reduced form with individual survey data. I use rare child preference data to identify couples with concordant and discordant child preferences. With relative wages as a proxy for bargaining power, I estimate whether the theoretical predictions are supported by the data.

The results are interesting. An increase in the relative wage of women with stronger child preferences than their partner increases fertility. This is consistent with the predictions of the bargaining model for fertility decisions. Results are robust to a number of specification checks and the inclusion of control variables. They also hold with relative education as a more exogenous determinant of bargaining power and a number of different specifications. I conclude that a bargaining framework is better suited to describe couples' fertility decisions than the unitary model.

The most prominent contribution to the literature is the novel approach to test a bargaining model using intra-household preferences. Most previous work relies on assignable private goods to identify bargaining power effects. Here, I can test the predictions for a household public good, which opens up the opportunity to test more general classes of household decisions. The critical precondition is the applicability of individual preference data.

Chapter 2: "Political Socialization in Flux? Linking Family Non-Intactness during Childhood to Adult Civic Disengagement"

In the second chapter, my coauthors, Helmut Rainer and Thomas Siedler, and I investigate the consequences of growing up in a non-intact family for young adults' civic engagement.

In recent decades, many developed countries have witnessed a disengagement of their citizens from public affairs (e.g., voting, union membership, partisan attachment, and interest in politics). These are worrisome trends for societies as civic disengagement can hurt responsiveness of the political elite and government as well as social capital and aggregate economic activity. The decline in civic engagement has not yet been connected to the breakdown of traditional family structures. In this chapter, we formalize the sociological idea that non-intact family environments deprive children of important parental resources that shape their political character and social capital. We analyze the effect of family non-intactness on different measures of civic engagement using individual survey data. The following paragraphs provide a short overview of the analysis and results.

In a theoretical model, we formalize the concept by Coleman (1988) that social capital within the family is generated through the time and effort parents devote to their children. The model's main result shows that growing up in a non-intact family impairs the creation of social capital. Moreover, the longer the spell of non-intactness lasts, the more severe are the consequences.

In the empirical part, we compare young adults, who grew up in a non-intact family, with young adults, who grew up in an intact family, using individual survey data. Family non-intactness is comprised of births outside marriage and divorces during childhood. The dependent variable is a civic engagement index combining data on political interest, party identification, organizational involvement, and individual voluntarism. We find from analysis of repeated cross-sections and of sibling differences that growing up in a non-intact family has severe negative consequences for young adults' civic engagement. Results are robust to the inclusion of control variables and a number of specification checks. We argue that the effect is causal, because the assumptions underlying the sibling differences approach are rather weak and assessments of bias suggest that confounding unobserved factors are unlikely to explain the entire effect.

First of all, this chapter fills a void in the economic literature linking family structure during childhood and civic disengagement. With democracies in need of an active electorate and societies as a whole benefiting in many ways from high social capital, one should be concerned about the negative effect of family non-intactness during childhood. Direct implications of our research for policy makers cannot be justified. However, it seems straightforward to consider emphasizing political socialization and related fields in school curricula to counteract the rising prevalence of non-intact families.

Chapter 3: "Labeling Effects – How Child Benefits Affect Consumption and Savings"

In the third chapter, I empirically investigate labeling effects of child benefits on families' consumption and saving decisions, exploiting a policy reform in Germany in the late 1970s. I assess whether expenditure patterns are influenced in favor of child goods by the mere labeling of child benefits. The following paragraphs summarize methods and findings.

In textbook economics all income is fungible, i.e. the source and type of income cannot affect expenditure. Yet, it has long been recognized that human beings are not always behaving that rationally. Thaler (1990) suggests that individuals form mental accounts which cause them to use certain income disproportionately for a certain purpose. If an individual receives a beverage voucher at a restaurant, she would spend disproportionately more on beverages compared to food. To transfer this mechanism to child benefits would imply that the recipient spends disproportionately more on child goods.

In the empirical analysis, I exploit a policy reform which increased child benefits for a treatment group (families with three children) and did not affect a control group (families with one child). The idea is, abstracting away an income effect, that increased child benefits change the composition of income. Holding total disposable income constant, an increase in benefits can only affect consumption through a labeling effect. The difference-in-differences setup allows to eliminate expenditure changes over time as confounding factors.

I find no robust evidence of labeling effects on child good consumption, but significant positive effects on building loan savings. I argue that most of these savings can be assigned to children and robustness checks with tenant status and flat size support this assumption. Furthermore, I run tests with placebo expenditure and relative trend assumptions which all confirm the baseline results. The effects seem not to be confounded by intra-household bargaining either which I test with a peculiar payment scheme for civil servants. Disentangling the effect reveals that the result for building loan savings is largely driven by better-off households.

This chapter is not the first research to show behavioral response to labeling. There is also some literature that shows effects of child benefits on assignable child goods. Yet, to the best of my knowledge, the finding for savings is unique. With respect to long-term well-being, savings might be even more important than child-specific consumption. For policy makers the growing body of literature showing labeling effects opens promising and cost-effective opportunities for all kinds of welfare programs.

Chapter 4: "Does Expanding Public Child Care Encourage Fertility? County-Level Evidence from Germany"

In the fourth chapter, my coauthors, Stefan Bauernschuster and Helmut Rainer, and I investigate the fertility promoting effects of public child care supply for very young children. The rationale is that women with career prospects would be more likely to have a child if they can get child care arrangements a year or two after giving birth. The fear of human capital depreciation and trouble with continuing work after prolonged maternity leave might otherwise hold women back from having a child.

In recent decades, Germany's demography was characterized by below replacement fertility and population started to contract. This development has been connected to a lack of progressive family policies. In particular, availability of public child care for children under three has been much lower than demand. Starting in 2005, a set of reforms was implemented to increase the supply of public child care and to provide every third child under three with a child care slot by 2013. In this chapter, we investigate whether the increase in public child care supply has an effect on fertility. The following paragraphs give a short overview of the methods and results.

For the identification of a child care supply effect on fertility, we exploit that municipalities are responsible for the creation and maintenance of child care facilities and are thus executing the expansion. We use the widely differing pace of expansion between counties for identification using two approaches. First, we compare counties that expanded child care more than the median county with counties that expanded child care less between 2002 and 2009. We find that a 10 percentage point expansion in public child care coverage increases births per 1,000 women by 1.2 (corresponds to an increase of 2.8 percent). Second, we use a county fixed-effects model to eliminate county-specific characteristics that may affect fertility and child care expansion. Then, we find that a 10 percentage point expansion in public child care coverage increases births per 1,000 women by 1.4. Effects are heterogenous with respect to age, indicating that women between 30 and 34 are most responsive to the child care expansion. Our results are robust to the inclusion of a number of control variables on the county level, to different specifications of the lag structure for fertility responses, to detailed age compositions in county populations and, finally, to selective migration.

Only few studies have investigated the effect of child care on fertility using public policy reforms for identification. As the unprecedented increase in child care availability over a short period of time with regional variation allows us to exclude much of the possible confounding variation, we contribute results that can be understood as causal under relatively weak assumptions. Moreover, our results are important for the debate on family policy as the estimated effects are substantial. The intended increase in child care of 30 percentage points would, under the strong assumption of linearity, yield an increase in the fertility rate of about 0.13 children per woman.

This chapter fills a void in the literature: most of the existing evidence is based on data from Scandinavian medium- to high-fertility countries, whereas the context of our study is Germany – a country struggling with the ramifications of sustained low fertility. The take-home message for policy makers would be that reconciling work with family life by increasing public child care supply can be an effective way to escape the low fertility trap.

Chapter 1

Do Couples Bargain over Fertility? Evidence Based on Child Preference Data

1.1 Introduction

Knowing how households make decisions is crucial for predicting the effects of policy interventions like fertility promoting instruments, but there is no clear consensus on this subject. The neoclassical model of household behavior treats families as single decision units that maximize their utility (Samuelson, 1956; Becker, 1973). This model aggregates ad hoc individual preferences into one predetermined household utility function and makes analysis easy and convenient as results from the microeconomics of consumption can be applied to family decision making. However, there are drawbacks—the unitary model ignores effects of heterogeneity in preferences and resources within families. Bargaining and collective models (Manser and Brown, 1980; McElroy and Horney, 1981; Chiappori, 1992) shed some light on the black box of family decisions and allow for endogenous aggregation of individual preferences. In this class of models, household distribution factors determine the allocation of resources and the weighting of individual preferences. In short, the preferences of family members with high bargaining power receive more weight in decision making. The question of whether family decisions are bargaining outcomes is important because non-unitary models can yield very different conclusions. The effectiveness of policy instruments then hinges on the interaction of household decision processes and the policy's effects on resource distributions.

There is a great deal of evidence in the empirical literature that bargaining plays a role in household decision making. All major contributions in this field rely on assignability assumptions, i.e., they assume that a good is preferred more by one person in the household than by another. If that person's bargaining power increases, consumption of her preferred good should increase. The literature uses sex-specific clothes (Browning et al., 1994; Lundberg et al., 1997) and child support by remarried fathers (Ermisch and Pronzato, 2008) to find bargaining mechanisms in household consumption decisions. Other work uses sex-specific preferences as an explanation for effects of exogenous variation in female income (Duflo, 2003; Qian, 2008) and for the association of child height and parental education (Thomas, 1994). In sum, there is a vast body of evidence showing that household decisions about private goods are bargaining outcomes. However, very little is known about household public goods and in particular about fertility decisions (for one study about commitment in household decisions using preference data, see Rasul (2008)). Knowing how families arrive at their decisions about having children is important to policy makers struggling with the low birth rates prevalent in most developed countries.

So, the question is: Do couples bargain over fertility? Suppose a couple, consisting of a female and a male person, needs to make a decision about the consumption level of a household public good. One partner prefers this good more than the other. This partner's bargaining power will then be positively associated with the level of consumption of the household public good. If both partners have concordant preferences for the household public good, there will be no bargaining power effects because there is no need for bargaining. In contrast, for example, suppose the household public good the couple is deciding about is children and assume that the woman has stronger child preferences than her partner—the number of children should be positively affected by her bargaining power. If she had weaker preferences, her bargaining power should lower fertility. In a theoretical model, we derive predictions of a differential bargaining power effect on fertility depending on whether couples have concordant or discordant child preferences.

The predictions are then investigated in an empirical analysis. We use rare child preference data to distinguish between couples with concordant and discordant preferences and test whether bargaining power has differential effects on the probability of having a child. In the bargaining framework, we expect a different effect of her bargaining power on fertility if the couple has discordant child preferences compared to couples with concordant child preferences. Specifically, if her child preferences are stronger than her partner's, her bargaining power should have a positive effect on fertility. Conversely, if her child preferences are weaker than her partner's, her bargaining power should have a negative effect on fertility. The comparison group is comprised of couples with concordant child preferences. In the analysis, we account for a possible negative opportunity cost effect that may go along with relative wages. With preference data and different measures of bargaining power we are able to test the prediction of a bargaining model for a household public good. Our results suggest that bargaining does occur in the context of fertility decisions. We explore some potential biases and find the results to be largely robust.

That couples do, indeed, bargain over fertility, which is one of the most important decisions a family will ever make, has potentially large implications for policy makers. Targeted benefits have an effect on the resource distribution and the bargaining power relation within couples. Instruments like child benefits that, among other things, are aimed at increasing fertility could be more effective if they could be targeted at people who are actually interested in having children. Given there is knowledge about intra-household relative preferences, the idea of altering bargaining power by targeting benefits could be carried over to any policy concerned with household decisions.

The remainder of the paper is organized as follows. We lay out the theoretical background of bargaining over fertility in Section 1.2 and describe the empirical strategy and the data in Section 1.3 and Section 1.4. We show estimation results in Section 1.5 and conclude in Section 1.6.

1.2 Theoretical Considerations

We use a simple model of household decision making about the number of children to illustrate the expected effects of bargaining power. In our model, which is based on Iyigun and Walsh (2007), each household is comprised of one woman (f) and one man (m). Throughout we use the notation $i \in \{f, m\}$ to refer to either of the two. Individual i's preferences are represented by a strictly increasing and strictly quasi-concave utility function defined over private consumption (c_i), leisure (l_i) and the number of children (n). Since our aim is to provide a model that is tractable and, at the same time, shows how fertility choices are influenced by the relative bargaining power within the household, we consider an additively separable and homothetic utility representation of preferences:

$$U^i(c_i, l_i, n) = \ln c_i + \ln l_i + \alpha_i \ln n; \quad i = f, m. \tag{1.1}$$

where $\alpha_i > 0$ represents the child preferences of person i. Each individual can earn a wage in the labor market equal to w_i, and is endowed with one unit of time which has to be allocated between leisure and market work. In addition, each child requires child-rearing time τ, which is borne by the woman alone. The couple decides the optimal number of children n, so that τn is total child-rearing time. The woman's time endowment then becomes $(1 - \tau n)$; that of the male partner is 1. The assumption can be relaxed to allow the man to spend time on child rearing, but the extreme case of the woman being solely responsible is chosen to illustrate that even though an increase in her wage has a negative opportunity cost effect on fertility, there can be offsetting positive effects which operate over an increase in her bargaining power if $\alpha_f > \alpha_m$.

Household decisions are made by means of collective bargaining between the partners with θ being the woman's bargaining power, and $(1 - \theta)$ the man's bargaining power. We assume that the woman's bargaining depends positively on her relative wage $\phi \equiv \frac{w_f}{w_m}$. Formally, $\theta = \theta(\phi)$ with $\theta' > 0$. Couples maximize

$$\max_{c_f, c_m, l_f, l_m, n} \Omega = \theta(\phi) U^i(c_f, l_f, n) + [1 - \theta(\phi)] U^i(c_m, l_m, n), \tag{1.2}$$

subject to

$$c_f + c_m + w_f l_f + w_m l_m = w_f(1 - \tau n) + w_m. \tag{1.3}$$

As we mentioned above, the woman's potential time available for market work is reduced by the time spent child rearing τn, while the man's time endowment is unity. In general, the household allocation of resources satisfies the following three criteria:

$$\frac{U_1^f(c_f, l_f, n)}{U_2^f(c_f, l_f, n)} = w_f \tag{1.4}$$

$$\frac{U_1^m(c_m, l_m, n)}{U_2^m(c_m, l_m, n)} = w_m \tag{1.5}$$

$$\frac{U_3^f(c_f, l_f, n)}{U_1^f(c_f, l_f, n)} + \frac{U_3^m(c_m, l_m, n)}{U_1^m(c_m, l_m, n)} = \tau w_f \tag{1.6}$$

where $U_k^i(c_i, l_i, n)$ denotes the first-order partial derivative of U_k^i with respect to its k-th argument ($k = 1, 2, 3$). For each individual, the marginal rate of substitution between private consumption and leisure must be equal to the marginal rate of transformation, given by the respective market wage [equations (4) and (5)]. The optimal number of children is governed by the Samuelson condition for the optimal provision of a public good [equation (1.6)]. Stated in words, it implies that the sum of the partners' marginal rates of substitution between having children and private consumption must be equal to the private opportunity cost of an extra child, τw_f, relative to an extra unit of private consumption.

If we employ the additively separable and homothetic utility representation assumed above, the optimal number of children is given by:

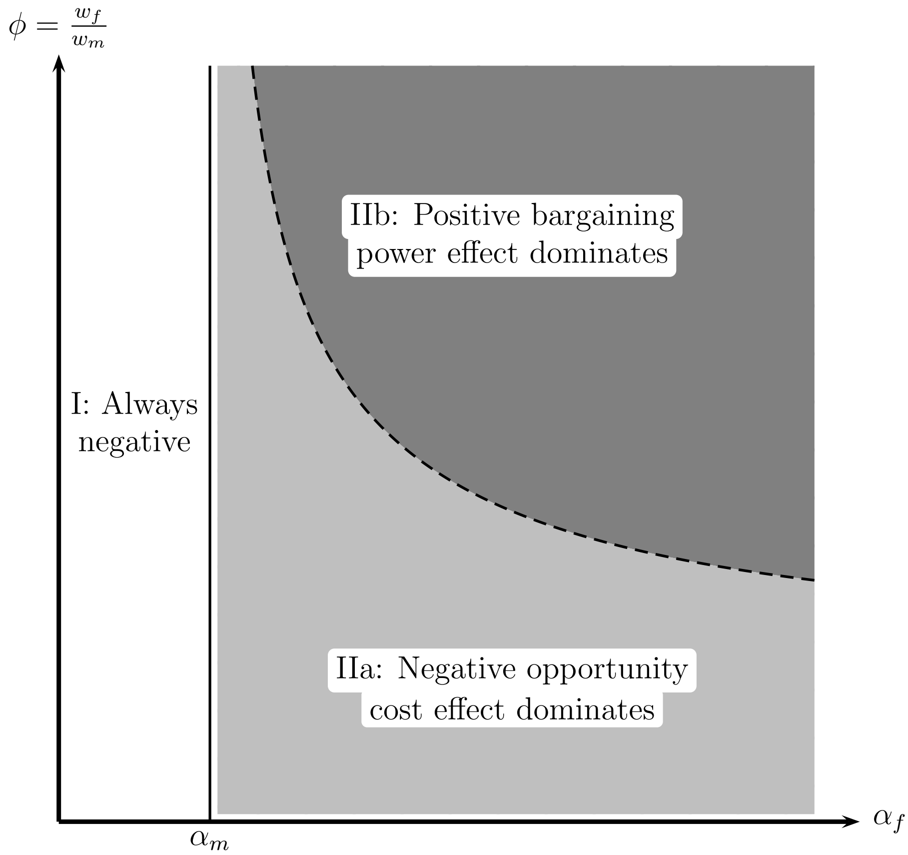
$$n^* = \frac{(\phi + 1)[\alpha_f \theta(\phi) + \alpha_m(1 - \theta(\phi))]}{\tau\phi[2 + \alpha_f \theta(\phi) + \alpha_m(1 - \theta(\phi))]}, \tag{1.7}$$

where $\phi = \frac{w_f}{w_m} > 0$. In the empirical work, we examine how fertility choices vary with the wage of women relative to that of their male partners. It is therefore useful to compute:

$$\frac{\partial n^*}{\partial \phi} = \underbrace{-\left[\frac{\alpha_f \theta(\phi) + \alpha_m(1 - \theta(\phi))}{\tau\phi^2[2 + \alpha_f \theta(\phi) + \alpha_m(1 - \theta(\phi))]}\right]}_{\text{opportunity cost effect}} + \underbrace{\left[\frac{2\theta'(\phi)(\phi + 1)(\alpha_f - \alpha_m)}{\tau\phi[2 + \alpha_f \theta(\phi) + \alpha_m(1 - \theta(\phi))]^2}\right]}_{\text{bargaining power effect}} \tag{$\star$}$$

Consider each element of ($\star$). The first term captures the marginal change in the optimal number of children due to the opportunity cost effect of rising relative female wages; its sign is negative. The second term captures a "bargaining power effect". Indeed, while rising relative wages tend to increase women's labor supply and, due to higher opportunity costs, lower fertility, they also lead to a reallocation of bargaining power from men to women. If women have stronger preferences for children than their male partners ($\alpha_f > \alpha_m$), then the reallocation of bargaining power from men to women induced by rising relative wages works to increase fertility. Conversely, when women have weaker preferences for children than their male partners ($\alpha_f < \alpha_m$), the bargaining power effect is negative. Finally, for couples with identical child preferences ($\alpha_f = \alpha_m$), there exists no conflict over the optimal number of children, and hence fertility is unaffected by a reallocation of bargaining power. In the empirical analysis, we will therefore identify bargaining power effects by

Figure 1.1: Areas of fertility response to increases in the woman's relative wage



Notes: To draw the graph we assume for simplification a linear bargaining power function such that $\theta(\phi) = \frac{w_f}{w_m}$. The graph shows areas of positive and negative fertility response to the woman's relative wage and relative child preferences. The man's child preferences are fixed at α_m and the woman's child preferences α_f vary on the horizontal axis. In area I, fertility reacts unambiguously negative, as the opportunity cost effect and the bargaining power effect work in the same direction. In area II the opportunity cost effect is negative whereas the bargaining power effect is positive. In area IIa the opportunity cost effect dominates the bargaining power effect, in area IIb the bargaining power effect dominates the opportunity cost effect.

comparing how the fertility choices of couples with discordant and concordant child preferences vary with the relative female wage within the household.

Overall, the bargaining power effect of rising relative wages could undo the corresponding negative opportunity cost effect and result in an increase in fertility. This is depicted in Figure 1, which illustrates the conditions under which an increase in ϕ yields positive and negative fertility responses, respectively. With identical child preferences ($\alpha_m = \alpha_f$), the bargaining power effect is zero and so the opportunity cost effect results in a smaller number of children. In area I, the woman's child preferences are weaker than her male partner's ($\alpha_f < \alpha_m$) and hence, the opportunity cost effect and the bargaining power effect both work to reduce fertility. In area II, the negative opportunity cost effect is counteracted by a positive bargaining power effect ($\alpha_f > \alpha_m$). In area IIa, the opportunity cost effect dominates the bargaining power effect, and so rising female wages reduce fertility. In area IIb, however, the negative opportunity cost effect is more than offset by the positive bargaining power effect, and so couples will choose to have more children as the relative female wage increases. Motivated by these observations, the primary objective of empirical analysis to follow is to disentangle the total effect of rising relative wages into an opportunity cost and a bargaining power effect.

1.3 Empirical Strategy

From the theoretical model we obtain the following main predictions as a basis of our empirical strategy:

1. There is a negative opportunity cost effect from relative wages of females on births.

2. There is a bargaining power effect from relative wages of females on births, which is

- zero if $\alpha_f = \alpha_m$,
- positive if $\alpha_f > \alpha_m$,
- and negative if $\alpha_f < \alpha_m$.

In essence, it means that female relative wage has a differential effect depending on relative child preferences within a couple additionally to its own opportunity cost effect on births. The bargaining power effect from female relative wage is positive if $\alpha_f > \alpha_m$ and negative if $\alpha_f < \alpha_m$. The net effect of female relative wage is ambiguous. These different effects can be captured by interactions between preference differences and relative wages in the following estimation equation:

$$\begin{aligned} Birth_{i,12-23months} = &\alpha + \beta_1 \phi_{i,t} + \beta_2 I(\alpha_f > \alpha_m)_{i,t} + \beta_3 I(\alpha_f < \alpha_m)_{i,t} \\ &+ \gamma_1 (\phi \times I(\alpha_f > \alpha_m))_{i,t} \\ &+ \gamma_2 (\phi \times I(\alpha_f < \alpha_m))_{i,t} \\ &+ X'_{i,t}\delta + \epsilon_{i,t}, \end{aligned} \tag{1.8}$$

with variables of couple i in period t. The dependent variable indicates a birth 12 to 23 months after the interview month. ϕ is a measure between zero and one. Here, it denotes relative wage. In the theoretical section ϕ is a determinant of bargaining power θ. Yet, we cannot directly measure bargaining power. Therefore, we use ϕ as a proxy for bargaining power in the empirical analysis. Thus, β_1 is the estimate of the opportunity cost effect. The bargaining power effects are captured by interactions of ϕ. $I(.)$ is an indicator function taking the value of one if true and zero if false. It divides the sample in three groups. $I(\alpha_f > \alpha_m)$ takes on a value of one if the female's child preferences are stronger than the male partner's. $I(\alpha_f < \alpha_m)$ takes on a value of one if the female's child preferences are weaker than the male partner's. Both are zero if the couple has concordant child preferences, reflecting the comparison group. The interactions $(\phi \times I(\alpha_f > \alpha_m))_{i,t}$ and $(\phi \times I(\alpha_f < \alpha_m))_{i,t}$ capture the differential bargaining power effects. Hence, γ_1 and γ_2 are estimates of bargaining power effects. Essentially, we estimate whether relative female wage has different effects between the groups denoted by $I(.)$. Estimates of the interaction effects are our primary target to identify bargaining over fertility. According to theory, we expect the estimates to have the properties $\gamma_1 > 0$ and $\gamma_2 < 0$.

To control primarily for different compositions of the groups denoted by $I(.)$, we include a vector of control variables X. These include the age and age squared of both partners, household income, as well as indicator variables for being a student, for foreign citizenship and second-generation immigrants, for federal states, and for the number of children, and year fixed effects. In different specifications we also control for the sum of both partners' child preferences and for the female wage rate. ϵ is an i.i.d. error term.

In a second attempt, we use an additional specification to exploit the extent of differences in child preferences between partners. The estimation equation is slightly changed to

$$\begin{aligned} Birth_{i,12-23months} =& \alpha + \beta_1 \phi_{i,t} + \beta_2 (\alpha_f - \alpha_m)_{i,t} \\ & + \zeta(\phi \times (\alpha_f - \alpha_m))_{i,t} \\ & + X'_{i,t}\delta + \epsilon_{i,t}, \end{aligned} \tag{1.9}$$

where $(\alpha_f - \alpha_m)$ can take on negative and positive values. Here, the interaction $(\phi \times (\alpha_f - \alpha_m))_{i,t}$ picks up the differential bargaining power effect, which varies with preference differences. This is consistent with the theoretical result shown in Equation ($\star$). If $\alpha_f > \alpha_m$, the difference takes on a positive value, and hence, her bargaining power effect on fertility is positive. Accordingly, if $\alpha_f < \alpha_m$, the difference takes on a negative value, and hence, her bargaining power effect on fertility is negative. We assume linearity of the preference difference in the bargaining power effect. Thus, we expect that the larger the difference in child preferences is, the larger is the bargaining power effect. This implies for the expected sign that $\zeta > 0$, denoting both positive and negative bargaining power effects. All other parameters from the estimation equation are unchanged.

In further specifications we use alternative bargaining power proxies. In order to do that, we substitute relative wages in ϕ for relative income or relative education. Predictions for bargaining power effects are unchanged, although the opportunity cost effect may be less severe.

Estimation bias At foremost, we show correlations of the theoretically relevant estimates and do not attempt to establish causality. However, it is informative for the interpretation to discuss some issues of endogeneity first. Estimations of the bargaining power effect in γ_1, γ_2, and ζ may suffer from endogeneity. The most probable reason may be omitted variable bias. It arises if unobserved factors both influence the dependent variable births and the interaction of relative wages with the preference indicators. There may be unobserved factors that affect births and relative wages. Then, for our coefficients of interest γ_1 and γ_2, we would worry if there were additionally systematic differences between the couples with concordant and discordant child preferences. Can we say anything about the direction of estimation bias? Unobserved heterogeneity will most likely be of the sort that individuals with strong child preferences have higher fertility and lower wages, which induces a downward bias for γ_1. As we expect a positive estimate, the downward bias is no threat to identification. Vice versa, individuals with weak child preferences may have lower fertility and higher wages, which induces a downward bias for γ_2. In this case, the variation would be confounding. We keep that in mind, when interpreting our results. The same arguments apply for ζ. As a robustness check we use an individual fixed-effects model that eliminates all time-constant individual heterogeneity. This model is identified by changes in relative wages over time within couples. However, less variation reduces precision of the estimates.

Another source of confounding variation may stem from selective mating. To induce confounding variation, we would need for example women with high child preferences to choose partners with lower wages. If men may also prefer to have higher bargaining power within their partnership, it becomes a zero sum game. Therefore, mating is a rather unlikely source of bias. A similar reasoning applies for strategic adjustment of bargaining power. In the face of discordant child preferences, individuals could have an incentive to increase bargaining power in order to gain say in the decision. As both partners have the same incentive, this can only confound results if either males or females were more able to do that kind of adjustment. As a robustness check, we use relative education as a more plausibly exogenous determinant of bargaining power.

1.4 Data

We use data from the German Socio-Economic Panel (SOEP) in Version 27 for our estimations.[1] We apply waves from 1990 onwards. Data include information on the socioeconomic background of individuals and households, and on intra-family relations. Partners, cohabiting or married, are matched by partner identifiers to obtain a comprehensive couple data set. We include only couples in which the woman is of reproductive age (between 18 and 49 years old). The anchoring person

[1] See Wagner et al. (2007) for further information on the data.

in our data set is the woman. Her characteristics along with matched partner characteristics are used in the empirical analysis. Given the setup, we have two variables of interest: child preferences and proxies for bargaining power.

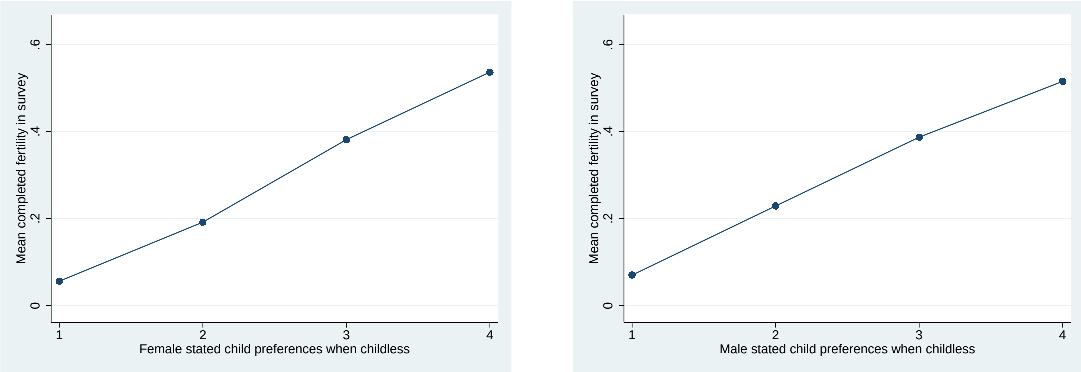
Child preferences For child preferences we use proxy data from a question, which asks about the importance of having children. The precise question is: "Are the following things ("Have children") currently ... very important (4), important (3), not very important (2) or totally unimportant (1) ... for you?"[2] The question is only included in waves 1990, 1992, 1995, 2004, and 2008; this restricts our sample period.

As our preference variable is in fact a proxy of child preferences, we make a simple attempt to verify its validity. Our child preferences are not fertility preferences in the sense of a desired number of children, but self-reported valuation of how important having children is. This variable may reflect a variety of underlying preferences and valuations of current situations. In part, the variable can be understood as the parents' evaluations of being a family with children, how satisfied they are with their family or how much they care about their offsprings. The number of current children will thus be correlated with stated child preferences. For our purpose, we need at least in some sense a connection of the preference variable to the real preference for having another child. We show in Figure 1.2 how the stated preferences of **childless** couples translate into completed fertility in later waves. The assumption is that childless couples cannot evaluate their current situation with their offsprings when answering the survey question and therefore state a preference for a family with children. Completed fertility can be measured by following the same couples over time in the panel. In Figure 1.2 we depict the average number of children at the latest observation on the ordinate, reflecting average completed fertility, and the answer to the preference question on the abscissa. We see a clear positive correlation of stated preferences when childless and average completed fertility, both for males and females. In general, our stated child preferences have predictive power for completed fertility. Hence, our child preference variable seems to be a good proxy for actual child preferences.

In Table 1.1 we cross-tabulate the number of answers to the child preference question from women in rows and from their male partners in columns. The two highest categories contain by far the most answers, although males are less likely to answer in the highest category than females. We define all observations on the diagonal as couples with concordant child preferences. Couples with stronger child preferences of females are found to the northeast of the diagonal, such that $I(\alpha_f > \alpha_m)$ takes on the value of one and is zero otherwise. Accordingly, observations to the southwest of the diagonal denote couples with weaker female child preferences,

[2]This coding is in reversed order compared to the original data. Hence, for our analysis larger numbers correspond to stronger preferences.

Figure 1.2: Completed fertility and initial child preferences



Notes: The figure in the left panel depicts a scatter plot with the average, latest observed number of children on the ordinate for each female child preference answer while childless. The figure in the right panel depicts a scatter plot with the average, latest observed number of children on the ordinate for each male child preference answer while childless.

such that $I(\alpha_f < \alpha_m)$ takes on the value of one and is zero otherwise. Thus, we have 8,755 observations of couples with concordant child preferences, 3,099 observations of couples with higher female child preferences and 1,513 observations of couples with lower female child preferences.

Table 1.1: Child preference combinations

Male partner: Woman:	Very important	Important	Not very important	Totally unimportant	Total
Very important	6,037	2,207	278	47	8,569
Important	962	2,027	402	52	3,443
Not very important	70	319	510	113	1,012
Totally unimportant	11	48	103	201	363
Total	7,080	4,601	1,293	413	13,387

Notes: The table shows numbers of observations in as a cross-tabular table with female child preferences in rows and male child preferences in columns.

We report some general descriptive statistics of the data in Table 1.2. The sample is restricted to non-missing values for the dependent and all control variables which are used later on. 23 percent of couples show stronger female child preferences and thus are subsumed under the indicator variable $I(\alpha_f > \alpha_m)$. 11 percent of couples belong to the other group with discordant child preferences, in which $I(\alpha_f < \alpha_m)$ equals one. 64 percent of couples have concordant child preferences.

In another specification we exploit the extent of the difference in child preferences between partners. We define the difference in preferences as $\alpha_f - \alpha_m$. The variable can take on values between -3 and +3, with negative values indicating lower female child preferences and positive values indicating higher female child preferences. The variable is zero for couples with concordant child preferences. We

Table 1.2: Descriptive statistics

Variable	N	Mean	Std. Dev.	Min	Max
Dependent variable					
Birth$_{12-23months}$	11553	4.16	19.98	0	100
Variables of interest					
$I(\alpha_f > \alpha_m)$	11553	0.23	0.42	0	1
$I(\alpha_f < \alpha_m)$	11553	0.11	0.31	0	1
$(\alpha_f - \alpha_m)$	11553	0.14	0.68	-3	3
ϕ : Relative wage	11352	0.35	0.26	0	1
ϕ : Relative labor income	11386	0.29	0.26	0	1
ϕ : Relative years of schooling	11231	0.50	0.05	0.33	0.72
ϕ : Relative ISCED score	11331	0.49	0.10	0	1
ϕ : Relative non-wage income	3353	0.66	0.44	0	1
ϕ : Relative full individual income	11527	0.30	0.22	0	1
Control variables					
Sum of child preferences	11553	6.89	1.36	2	8
Wage	11553	8.32	9.10	0	241
Household income (monthly)	11553	3179.57	1608.27	42	56759
Age	11553	36.75	7.55	18	49
Partner's age	11553	39.74	8.55	19	87
In education	11553	0.06	0.25	0	1
Non-German	11553	0.10	0.30	0	1
Second gen. migrant	11553	0.02	0.16	0	1
Number of children	11553	1.58	1.11	0	11

Notes: Descriptive statistics of the sample including: number of observations (N), mean, standard deviation (Std. Dev.), minimum (Min) and maximum (Max). All variables are defined from the perspective of women with male partners.

see in Table 1.2 that on average the variable is 0.14, which corresponds to slightly stronger female child preferences.

Proxies for bargaining power Our second variable of interest is the proxy for bargaining power. We apply a number of different definitions in order to make most robust statements. Following the theoretical model, in our baseline estimates we use relative wages ϕ as the bargaining power determinant. Wage rates are measured as labor income in the preceding year over total hours worked in the preceding year. Relative wage is defined as female wage over the sum of the female and the male partner's wage ($\frac{w_f}{w_f+w_m}$). According to Table 1.2, female wage rates are on average 35 percent of the total wage rate within couples. Deviating from theory, we also define ϕ using alternative proxies for bargaining power.

As a second proxy for bargaining power we define ϕ as relative labor income, despite the fact that this measure is less closely connected to the theory. We apply relative labor income, because one could argue that bargaining power is not reflected by earnings potential but rather by how much one actually earns. The variable is defined as female income over the sum of both partner's income. Women in the sample earn on average 29 percent of total labor income. This percentage is a bit lower than for the wage rate, which indicates that women on average work fewer hours.

As a robustness check we additionally use non-wage income and total individual income as proxies for bargaining power. Non-wage income has the advantage that it is less affected by individual decisions. On the contrary, there is much less variation to exploit. Total individual income combines non-wage and labor income. Definitions of the relative measures follow the same principle as above. Females have about 66 percent of total couples' non-wage incomes. Observations are restricted to couples with positive total non-wage income, because shares are otherwise infeasible. Usability of non-wage income alone is questionable due to very few observations. Summing up non-wage income and labor earnings to full individual income reveals that the female share within couples is about 30 percent on average.

Our last proxy for bargaining power draws on relative education. The rationale is that education can more plausibly be considered as an exogenous measure. We use two specifications for relative education, first using years of schooling, second using highest educational attainment reflected in ISCED codes from zero to six. Again, definitions of the relative measures follow the same principle as above, dividing the female measure by the sum of both partners' measures. Relative years of schooling is at parity between females and males. The variable is less volatile due to compulsory schooling laws which virtually excludes individual years of schooling to be less than nine. Therefore, the range of values is restricted. The relative ISCED score is an ad hoc measure and cannot be interpreted in a meaningful way. Yet, it relates the standard educational attainment scores of two partners and therefore should reflect bargaining power. On average the relative female ISCED score is 0.49 in our sample.

Births Our dependent variable throughout the empirical analysis is births. A birth is defined as having a child 12 to 23 months after the interview. We employ a conception and gestation lag of at least 12 months because we are interested in the decision about having a child and the conditions at the time the decision was probably made. Clearly, our fertility variable is a rather noisy measure of the intention to have a child. The variable is an indicator variable taking on a value of one if the woman gives birth 12 to 23 months after the interview and zero otherwise. We observe 506 births, of which 327 are from couples with concordant child preferences, and 179 from couples with discordant child preferences. The majority of 131 fall into the group with higher female child preferences, 48 into the group with lower female child preferences. The dependent variable as depicted in Table 1.2 is an indicator for a birth 12 to 23 months after the interview. The measure is multiplied by 100 as it is used in the estimations to get more convenient coefficients. Hence, there are on average 0.0416 births per year and woman.

Control variables In our sample, as depicted by the control variables in Table 1.2, the sum of couples' child preferences (sum of the categorical child preference variable with values from 1 to 4) is on average 6.89, corresponding to the mostly strong child preferences seen earlier. Female hourly wage rate is 8.32 Euro. Household net income averages at about 3200 Euro monthly. Women are on average 36.75 years old, which comes about technically by restricting to the reproductive age from 18 to 49 years. Their male partners are on average 3 years older. About 6 percent participate in some kind of education or training. 10 percent have or had a non-German citizenship. 2 percent are second generation immigrants. The average number of children is 1.58.

1.5 Empirical Results

In the results section we first show baseline estimates and continue with a robustness analysis.

Baseline estimates: Relative wage and preference indicators We report baseline estimates from linear probability regressions in Table 1.3 using the model as in equation (1.8). In columns (1) to (4), the proxy ϕ for bargaining power is defined as relative female wage. Again, following theory, we expect a negative opportunity cost effect and differential bargaining power effects on birth probability depending on preferences within couples. Bargaining power effects are denoted by the estimates of $\phi \times I(\alpha_f > \alpha_m)$ and $\phi \times I(\alpha_f < \alpha_m)$. We see in the first row of column (1) by the coefficient of the interaction effect that relative wage has a differential positive association with birth probability if female child preferences are stronger ($\alpha_f > \alpha_m$). The estimate is statistically significant at the 5 percent level, implying that the association of relative wage with birth probability is significantly different from couples with concordant child preferences. This is consistent with a bargaining power effect on fertility. Excluding the negative opportunity cost effect, the estimate suggests that an increase in relative wage of 10 percentage points increases birth probability of couples with stronger female child preferences by 0.4 percentage points, as denoted by the estimate of the interaction $\phi \times I(\alpha_f > \alpha_m)$. In the second row, the interaction effect of relative wage with lower female child preferences is expected to be negative. In fact, it shows a negative sign, but it is insignificantly different from couples with concordant child preferences, as denoted by the estimate of the interaction $\phi \times I(\alpha_f < \alpha_m)$. The lower precision of this estimate could be due to the smaller incidence of births in that group. In the third row, we see that relative female wage is negatively associated with birth probability. Confirming theory, the opportunity cost effect of wages denoted by ϕ decreases birth probability. An increase in relative female wage of 10 percentage points decreases birth probability by 0.23 percentage points. However, regarding the total effect of relative wage, we see that for the group with stronger female child preferences the positive bargaining power effect dominates the negative opportunity cost effect. Combining the opportunity cost effect and the interaction effect, a 10 percentage point increase in relative wage increases birth probability by 0.20 percentage points for couples with stronger female child preferences. For couples with weaker female child preferences the opportunity cost effect and the bargaining power effect would both be negative. In the fourth and the fifth row, we show estimates for the child preference indicators $I(\alpha_f > \alpha_m)$ and $I(\alpha_f < \alpha_m)$. As we would expect, birth probability is generally lower for couples with discordant child preferences compared to couples with concordant child preferences. A significant difference is found for couples with stronger female child preferences.

In column (2), we add control variables to the estimation equation to account for individual differences. We see that the differential bargaining power effects are only marginally affected by the inclusion of additional control variables. Results are still consistent with the predictions from theory and yield the same level of statistical significance. Household income shows a plausible positive correlation with birth probability, reflecting the normality of the household public good. In column (3), we add the sum of couples' child preferences to the estimation equation. Controlling for it would eliminate variation that may be caused by generally weaker child preferences for couples with discordant child preferences. The estimate turns out positive and significant. Our main results for the differential bargaining power effects from row one and two are robust to the inclusion of the sum of child preferences. The estimate in the first row for the interaction of relative wage with the stronger female child

Table 1.3: Baseline–Bargaining power effects for wage and income with preference indicators

Bargaining power proxy ϕ:	Relative wage				Relative income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times I(\alpha_f > \alpha_m)$	4.3439**	4.1462**	3.8058**	3.8000**	5.0421***	4.4684**	3.9242**	3.9257**
	(1.8422)	(1.8143)	(1.8100)	(1.8105)	(1.8711)	(1.8475)	(1.8364)	(1.8353)
$\phi \times I(\alpha_f < \alpha_m)$	-2.8125	-2.1185	-2.4234	-2.4298	-2.5953	-2.6750	-3.0836*	-3.0818*
	(2.0359)	(1.9897)	(1.9950)	(2.0021)	(1.8577)	(1.8633)	(1.8684)	(1.8713)
ϕ	-2.3261**	-0.6278	-0.3161	-0.2692	-3.2042***	-0.9909	-0.4564	-0.4696
	(0.9428)	(0.9844)	(0.9867)	(1.1356)	(0.9579)	(1.0092)	(1.0121)	(1.0939)
$I(\alpha_f > \alpha_m)$	-1.5439**	-1.4552*	-0.3127	-0.3113	-1.4820**	-1.3001**	-0.1099	-0.1101
	(0.7455)	(0.7425)	(0.7438)	(0.7434)	(0.6582)	(0.6568)	(0.6585)	(0.6581)
$I(\alpha_f < \alpha_m)$	-0.7482	-0.8446	0.2801	0.2821	-0.9797	-0.7983	0.3455	0.3450
	(1.0051)	(0.9731)	(0.9787)	(0.9794)	(0.8622)	(0.8472)	(0.8545)	(0.8544)
Household income		0.0002**	0.0002	0.0002		0.0002**	0.0001	0.0001
		(0.0001)	(0.0001)	(0.0001)		(0.0001)	(0.0001)	(0.0001)
Sum of preferences			1.6754***	1.6755***			1.6969***	1.6968***
			(0.1847)	(0.1848)			(0.1850)	(0.1851)
Wage				-0.0022				0.0008
				(0.0210)				(0.0204)
Number of children fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	11,709	11,352	11,352	11,352	11,744	11,386	11,386	11,386

Notes: Coefficients from linear probability estimations with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times I(\alpha_f > \alpha_m)$ denotes the positive bargaining power effect and the estimate of $\phi \times I(\alpha_f < \alpha_m)$ the negative bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

preference indicator decreases slightly to 3.8, but remains statistically significant. In column (4), we furthermore add female wage rates to the estimation equation. The inclusion may be problematic due to collinearity with relative wage and household income. However, our main result in the first and second row is not affected by the inclusion of female wage rates at all.

In sum, the theoretical prediction of a differential bargaining power effect depending on discordance of child preferences can be found in the data. We find significant effects for couples with stronger female child preferences and insignificant effects for couples with weaker female child preferences. Estimates are robust to the inclusion of a number of additional control variables. Given that some of the associations found for control variables are not robust over different specifications, we may argue that the included control variables are relevant in the estimation equation.

Baseline estimates: Relative income and preference indicators In column (5) to (8) of Table 1.3 we replace relative wage in ϕ by relative labor income. The rationale is that it could be more important in practice how much an individual can purchase [income] rather than what her earnings potential is [wage]. In column (5) with no control variables, we see that the theoretical prediction of differential bargaining power effects also holds for relative income. The coefficient for the interaction of ϕ with stronger female child preferences is 5.0 and thus close to the estimate in column (1). Although, results cannot be compared over different definitions of ϕ, as different estimates could also be due to different relations of ϕ with actual bargaining power. The interaction coefficient of relative income with weaker female child preferences is negative but again not statistically significant. The bargaining power proxy ϕ again shows a negative opportunity cost effect of -3.2 in the third row. The overall effect of relative income on birth probability is positive for couples with stronger female child preferences, as can be seen by adding up the two coefficients. In column (6) we see that the inclusion of control variables decreases the interaction coefficient for couples with stronger female child preferences by 0.5. Though, the overall picture is unchanged. In column (7), we add the sum of couples' child preferences to the estimation equation. The interaction effect of ϕ with stronger female child preferences reduces to 3.9, but is still statistically significant. In this specification, the interaction with lower female child preferences increases in value to -3.1 and is statistically significant at the 10 percent level. In column (8), the inclusion of female wage rates in the estimation has no effect whatsoever on the coefficients of interest.

Our baseline results suggest a generally negative opportunity cost effect on birth probability, which is inherent in wage and income bargaining power measures for females. Yet, the association is not statistically significant in all specifications. The most important result is that we find a differential bargaining power effect depending on intra-couple differences in child preferences. If female child preferences are stronger than her partner's, her bargaining power is positively associated with births.[3]

To increase the female's say within a couple thus enables decisions more in favor of her preferences. This result is robust to adding a number of control variables and to applying relative wages and income as bargaining power proxies. Results also seem reliable, as unobserved heterogeneity would arguably lead to a downward bias in this coefficient (see discussion in the section on the empirical strategy). The equivalent prediction for couples with weaker female child preferences is a negative bargaining power effect. We see interaction coefficients with a negative sign, but the estimates turn out to be only marginally significant in few specifications. Fewer events in the dependent variable births may explain the weak associations. As a downward bias here could imply confounding variation, we do not put too much emphasis on the result for lower female child preferences. To say the least, the overall pattern of the results does not contradict the theoretical predictions.

Baseline estimates: Continuous preference differences In a second attempt, we exploit the extent of the differences in child preferences between partners. Instead of an indicator variable for discordant child preferences, we use a continuous measure of the difference between two partners. Specifically, the male partner's child preference is subtracted from the female's child preference, so formally the variable is defined as $\alpha_f - \alpha_m$. Regarding the predictions, the difference measure combines the predicted negative interaction effect of the bargaining power proxy ϕ with weaker female child preferences and the positive interaction effect with stronger female child preferences into one prediction: The bargaining power effect is increasing in the preference difference $\alpha_f - \alpha_m$. In contrast to the specification with preference indicators, it is implicitly assumed that the bargaining power effect is of the same magnitude for weaker and stronger female child preferences and is linear in the difference measure. We apply an estimation model as shown in equation (1.9). The bargaining power effect is denoted by the estimate of the interaction $\phi \times (\alpha_f - \alpha_m)$.

In the first row of Table 1.4, the estimates of the interaction effect of relative wage ϕ with the difference in child preferences $(\alpha_f - \alpha_m)$ show a positive sign and are statistically significant. Again, this result is consistent with our theory. In column (1), estimates with no control variables suggest that a 10 percentage point increase in female relative wage is associated with a 0.21 percentage point reduction in birth probability for couples with concordant child preferences. The 0.26 percentage points are multiplied by the difference in child preferences ranging from -3 to +3. Hence, for each point of difference between the female and her

[3]We also estimated probit models with the binary dependent variable birth. The interaction effects in odds ratios as well as average marginal coefficients of changes in the preference indicator evaluated at different values for bargaining power yield qualitatively comparable results, i.e., we find the coefficients of interest to be of the same sign and to be statistically significant. We prefer the linear probability estimates for their more readily interpretable interaction coefficients.

Table 1.4: Baseline—Bargaining power effects for wage and income with preference differences

Bargaining power proxy ϕ:	Relative wage				Relative income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times (\alpha_f - \alpha_m)$	2.5522**	2.3876**	2.2320**	2.2304**	2.8402***	2.6912***	2.4215**	2.4218**
	(1.1180)	(1.0775)	(1.0829)	(1.0819)	(1.0501)	(1.0298)	(1.0297)	(1.0290)
ϕ	-2.0569***	-0.2992	-0.0387	0.0147	-2.7876***	-0.6951	-0.2443	-0.2564
	(0.7542)	(0.7907)	(0.7928)	(0.9410)	(0.7424)	(0.7886)	(0.7918)	(0.8823)
$(\alpha_f - \alpha_m)$	0.3512	0.3673	0.0372	0.0369	0.2996	0.3318	-0.0323	-0.0322
	(0.5259)	(0.5014)	(0.5064)	(0.5063)	(0.4525)	(0.4360)	(0.4395)	(0.4395)
Household income		0.0002**	0.0002	0.0002		0.0002**	0.0002	0.0002
		(0.0001)	(0.0001)	(0.0001)		(0.0001)	(0.0001)	(0.0001)
Sum of preferences			1.6659***	1.6660***			1.6876***	1.6875***
			(0.1801)	(0.1801)			(0.1805)	(0.1806)
Wage				-0.0026				0.0008
				(0.0208)				(0.0203)
Number of children fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	11,709	11,352	11,352	11,352	11,744	11,386	11,386	11,386

Notes: Coefficients from linear probability estimations with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times (\alpha_f - \alpha_m)$ denotes the bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

partner's child preference (-3 to +3), a 10 percentage point increase in relative wage has an additional positive or negative effect of 0.26 percentage points on birth probability, depending on the sign of $(\alpha_f - \alpha_m)$. Thus, consistent with the theory, the bargaining power effect is positive if female child preferences are stronger and negative if female child preferences are weaker. Furthermore, with a difference in preferences of just +1, the negative opportunity cost effect of relative wages can be overcompensated by the bargaining power effect. In column (4) of Table 1.4, with the full model specification, the opportunity cost effect of relative wage is zero. The additional effect for each point in child preference difference is 0.22 percentage points per 10 percentage point increase in relative wage. The estimate is statistically significant. Thus, our result is very robust to controlling for additional variables, including the sum of partners' child preferences, wage rates and household income. Moreover, positive and statistically significant bargaining power effects are found in columns (5) to (8), with relative labor income in ϕ as a proxy for bargaining power. Results are equally robust to the inclusion of control variables and similar in size. Using continuous preference differences yields more robust results compared to estimations with indicators of discordant child preferences. We may conclude, that the results presented here strongly support the above findings of bargaining power effects in the fertility decision.

1.5.1 Robustness Analysis

Relative education as bargaining power proxy As a robustness check, we again alter the definition of our bargaining power proxy ϕ and include relative education as a more plausibly exogenous determinant of bargaining power. We use two education measures: relative years of schooling and relative educational attainment in ISCED codes. Education has an advantage over wages and income as it cannot be changed easily. When fertility decisions are about to be made most people have finished their education. Thus, education and, in our analysis, relative education are rather exogenous measures. Apart from selective mating there is not too much adults can do about it. Moreover, mating as a means to improve own bargaining power is basically a zero sum game for two potential partners. Furthermore, if in couples with discordant preferences one partner attempts to strategically improve earnings to have larger say in an upcoming fertility decision, education levels are practically immune against this sort of strategic adjustment. In this regard, estimations with relative education as a proxy for bargaining power can be understood as a reduced form of an instrumented regression with endogenous relative wages. Relative education is indeed highly correlated with relative wages.[4] However, relative education may have many channels to affect birth probability

[4]Auxiliary regressions of relative wages and its interaction on relative years of schooling and its interactions confirm a high correlation. Illustrative results can be found in Tables A.1 and A.2 in the appendix.

other than relative wage. It covers a broader set of bargaining power aspects such as intelligence, eloquence, negotiation skills or knowledge that all can affect fertility decision outcomes. All these possible channels are welcome to be at work. As long as they represent bargaining power effects of some kind, we are willing to include them and to abstract from specific mechanisms.

We show results for relative education as the bargaining power proxy ϕ in Table 1.5. In columns (1) to (4) we define ϕ as relative years of schooling and in columns (5) to (8) as relative educational attainment in ISCED codes. In contrast to the baseline results, in the first row the interaction effect of years of schooling as in ϕ with stronger female child preferences shows very imprecise estimates not even close to statistical significance. Whereas in the second row, the interaction effect of ϕ with weaker female child preferences is negative and statistically significant in all eight specifications. With the full set of control variables for years of schooling in column (4) the result suggests that a 10 percentage point increase in ϕ reduces birth probability by 4.2 percentage points if female child preferences are weaker, abstracting from a direct effect of ϕ. It implies that more say in the household for the person with lower preferences decreases birth probability, which is in line with the theoretical predictions for bargaining power effects. Effect sizes are much larger here, because the range of relative years of schooling is much lower than for relative wage due to compulsory schooling laws. It implies that a 10 percentage points change in relative years of schooling entails a much greater deal in bargaining power than an equal percentage points change in relative wages. For the relative ISCED scores the coefficient is about one quarter the size, implying a 1.1 percentage points reduction in birth probability following a 10 percentage point increase in ϕ.

In columns (1) to (4) of Table 1.5, we find a strong positive association of birth probability and the indicator variable for weaker female child preferences. The coefficient must be interpreted in combination with the interaction effect, implying that the effect comes about if $\phi = 0$. However, this would be implausible. If relative years of schooling are about 0.5 the two effects cancel out. The same is true for the marginally significant associations in column (7) and (8). Some of the coefficients for ϕ which should pick up opportunity cost effects are positive and statistically significant. This is at odds with the theory. However, using relative education as a bargaining power proxy does not necessarily yield a negative opportunity cost effect as in the case of wages. Relative education should here be considered as a robustness test and an auxiliary variable in the bargaining framework. Other control variables behave comparable to the baseline estimates. In particular, we find a very similar association of birth probability with the sum of preferences of 1.6.

In Table 1.6, we use the continuous variable for differences in child preferences instead of the indicator variables and interact it with ϕ defined as relative education. In columns (1) to (4) with relative years of schooling as the bargaining power proxy, the interaction denoting the bargaining power effect is positive and statistically significant throughout all specifications. Effect size is quite stable, suggesting that

Table 1.5: Education–Bargaining power effects for education with preference indicators

Bargaining power proxy ϕ:	Relative years of schooling				Relative ISCED scores			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times I(\alpha_f > \alpha_m)$	-5.1622	-3.8059	-4.9364	-4.9370	1.9905	2.2884	1.7810	1.7793
	(8.5678)	(8.5795)	(8.5253)	(8.5283)	(4.4792)	(4.4831)	(4.4488)	(4.4500)
$\phi \times I(\alpha_f < \alpha_m)$	-43.4329***	-41.1270***	-42.4736***	-42.4737***	-11.8025**	-10.7521**	-10.9696**	-10.9686**
	(11.5001)	(11.0543)	(11.0899)	(11.0901)	(5.3321)	(5.2723)	(5.2312)	(5.2314)
ϕ	11.3158**	7.9510	8.3230*	8.3260*	2.7099	1.3466	1.4478	1.4648
	(4.9456)	(4.9003)	(4.8999)	(4.9017)	(2.4616)	(2.4029)	(2.3999)	(2.4082)
$I(\alpha_f > \alpha_m)$	2.3370	1.7384	3.2857	3.2860	-1.0572	-1.1428	0.0933	0.0939
	(4.2177)	(4.2283)	(4.2028)	(4.2039)	(2.1777)	(2.1857)	(2.1680)	(2.1684)
$I(\alpha_f < \alpha_m)$	19.8404***	18.9652***	20.6113***	20.6113***	3.9413	3.7075	4.8032*	4.8029*
	(5.8560)	(5.6081)	(5.6320)	(5.6321)	(2.7099)	(2.6678)	(2.6485)	(2.6486)
Household income		0.0002*	0.0001	0.0001		0.0002*	0.0001	0.0001
		(0.0001)	(0.0001)	(0.0001)		(0.0001)	(0.0001)	(0.0001)
Sum of preferences			1.6273***	1.6273***			1.6326***	1.6324***
			(0.1835)	(0.1835)			(0.1846)	(0.1846)
Wage				-0.0002				-0.0020
				(0.0178)				(0.0178)
Number of children fixed eff.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	11,579	11,231	11,231	11,231	11,682	11,331	11,331	11,331

Notes: Coefficients from linear probability estimations with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times I(\alpha_f > \alpha_m)$ denotes the positive bargaining power effect and the estimate of $\phi \times I(\alpha_f < \alpha_m)$ the negative bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

Table 1.6: Education–Bargaining power effects for education with preference differences

Bargaining power proxy ϕ:	Relative years of schooling				Relative ISCED scores			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times (\alpha_f - \alpha_m)$	11.5378**	11.1838**	11.4028**	11.4029**	4.3537*	4.3268*	4.3976*	4.3959*
	(5.3879)	(5.2559)	(5.2392)	(5.2406)	(2.6438)	(2.6284)	(2.6003)	(2.6012)
ϕ	3.1704	0.3714	0.2801	0.2790	1.2107	-0.0027	-0.0653	-0.0489
	(3.9625)	(3.9102)	(3.9008)	(3.9029)	(1.9729)	(1.9412)	(1.9333)	(1.9388)
$(\alpha_f - \alpha_m)$	5.3209**	5.2208**	5.0638*	5.0638*	1.5786	1.6463	1.4150	1.4143
	(2.7034)	(2.6399)	(2.6336)	(2.6342)	(1.3164)	(1.3091)	(1.2966)	(1.2969)
Household income		0.0002*	0.0001	0.0001		0.0002*	0.0001	0.0001
		(0.0001)	(0.0001)	(0.0001)		(0.0001)	(0.0001)	(0.0001)
Sum of preferences			1.6079***	1.6079***			1.6215***	1.6213***
			(0.1790)	(0.1790)			(0.1798)	(0.1799)
Wage				0.0001				-0.0019
				(0.0178)				(0.0178)
Number of children fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	11,579	11,231	11,231	11,231	11,682	11,331	11,331	11,331

Notes: Coefficients from linear probability estimations with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times (\alpha_f - \alpha_m)$ denotes the bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

a 10 percentage point increase in female relative years of schooling increases birth probability by 1.1 percentage points per value in preference differences. This again meets the theoretical prediction for bargaining power effects. In column (5) to (8) with relative ISCED codes defining ϕ, interaction effects are positive and marginally statistically significant in all specifications. The coefficients of about 4.4 are virtually unaffected by the inclusion of various control variables. Also, associations of the control variables themselves with birth probability are very similar to the baseline results. It suggests that we use a well-defined model specification. Overall, using the full range of preference differences stabilizes the estimates. As before, results are more robust than with indicators for discordant child preferences. If we believe that education is less prone to estimation bias, these results are strongly supportive of bargaining power effects in fertility decisions.

First child fertility The decision about having a first child may be very different from the decision about having a second or third child. For example, intra-family allocations and specialization may have changed since birth of the first child. However, an important difference between the two decisions is probably knowledge and experience. After having the first child, a couple learns about child rearing, the time costs involved, and the true utility from having children. Experience then will play a role in the decision about whether to have another child. To account for possible heterogeneity we restrict the sample to couples without children in the following robustness test.

Results with the dependent variable being the decision on first births are shown in Table 1.7. In column (1) to (4) we again use the standard definition of ϕ defined as relative female wage. We see that the interaction of ϕ with the indicator of stronger female child preference $I(\alpha_f > \alpha_m)$ is positively associated with the probability of a first birth. However, the estimates are not statistically significant. The interaction effect in the second row for couples with weaker female child preferences is even more imprecisely measured and changes sign. In column (5) to (8) we use income as the bargaining power proxy in ϕ. In the first row, coefficients for the interaction of ϕ with stronger female child preferences are positive and statistically significant. Results suggest larger associations than in the baseline specification, which included all couples with and without children. In the second row, interaction effects are again not statistically significant. Less precise estimates for childless couples may be partly explained by the smaller sample.

In Table 1.8, we use preference differences instead of the indicator variables for stronger and weaker female child preferences, again in the sample with childless couples. We now find positive estimates for the interaction of ϕ with preference differences in all specifications. Although only two of the estimates are marginally statistically significant, signs are consistent with the theoretical prediction. Overall, the bargaining power effect for first child fertility seems to be a bit larger than

Table 1.7: First births only–Bargaining power effects with preference indicators

Bargaining power proxy ϕ:	Relative wage				Relative income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times I(\alpha_f > \alpha_m)$	9.9171	9.9409	8.3690	8.5208	13.8141**	15.4170**	14.1456**	14.2043**
	(6.2729)	(6.2896)	(6.2890)	(6.2867)	(6.1459)	(6.2291)	(6.2022)	(6.2010)
$\phi \times I(\alpha_f < \alpha_m)$	-0.7101	2.1814	2.5544	2.7943	-1.1767	1.5031	1.9789	2.0794
	(5.1775)	(4.6929)	(4.6950)	(4.7119)	(4.5420)	(4.2630)	(4.2735)	(4.2741)
ϕ	-2.7927	-3.1190	-2.2520	-3.5868	-2.8353	-4.1430	-3.0998	-3.8439
	(3.1100)	(3.1508)	(3.1658)	(3.4473)	(2.8475)	(2.9307)	(2.9442)	(3.0799)
$I(\alpha_f > \alpha_m)$	-0.1924	-1.7547	-1.0745	-1.1115	-1.6396	-3.8671	-3.3694	-3.3633
	(3.0528)	(3.0654)	(3.0674)	(3.0638)	(2.8635)	(2.9227)	(2.9150)	(2.9163)
$I(\alpha_f < \alpha_m)$	-1.2626	-2.7908	-2.7332	-2.8021	-1.0976	-2.4955	-2.5023	-2.5081
	(2.8569)	(2.6101)	(2.5940)	(2.5947)	(2.5772)	(2.4154)	(2.4020)	(2.4004)
Household income		0.0005	0.0005	0.0003		0.0005	0.0005	0.0003
		(0.0003)	(0.0003)	(0.0003)		(0.0003)	(0.0003)	(0.0003)
Sum of preferences			2.8261***	2.8398***			2.8601***	2.8687***
			(0.3622)	(0.3631)			(0.3617)	(0.3625)
Wage				0.0720				0.0508
				(0.0576)				(0.0549)
Number of children fixed effects								
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	2,237	2,167	2,167	2,167	2,251	2,180	2,180	2,180

Notes: Coefficients from linear probability estimations with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times I(\alpha_f > \alpha_m)$ denotes the positive bargaining power effect and the estimate of $\phi \times I(\alpha_f < \alpha_m)$ the negative bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

Table 1.8: First births only–Bargaining power effects with preference differences

Bargaining power proxy ϕ:	Relative wage				Relative income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times (\alpha_f - \alpha_m)$	3.6525	2.7957	2.0954	2.0908	5.1425*	5.0709*	4.4286	4.4290
	(2.8131)	(2.7441)	(2.7493)	(2.7450)	(2.7134)	(2.7152)	(2.7008)	(2.6984)
ϕ	-0.7405	-0.4266	0.1744	-0.9951	0.1487	-0.2447	0.6274	-0.0296
	(2.2880)	(2.3087)	(2.3073)	(2.5960)	(2.1482)	(2.2390)	(2.2330)	(2.4264)
$(\alpha_f - \alpha_m)$	-0.7804	-0.5020	-0.7600	-0.7622	-0.2336	0.3832	0.1628	0.1624
	(1.3807)	(1.3554)	(1.3537)	(1.3507)	(1.2957)	(1.3010)	(1.2928)	(1.2912)
Household income		0.0005	0.0004	0.0003		0.0005	0.0004	0.0003
		(0.0003)	(0.0003)	(0.0003)		(0.0003)	(0.0003)	(0.0003)
Sum of preferences			2.8414***	2.8542***			2.8789***	2.8869***
			(0.3626)	(0.3636)			(0.3621)	(0.3629)
Wage				0.0674				0.0472
				(0.0578)				(0.0557)
Number of children fixed effects								
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	2,237	2,167	2,167	2,167	2,251	2,180	2,180	2,180

Notes: Coefficients from linear probability estimations with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times (\alpha_f - \alpha_m)$ denotes the bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

for overall fertility. However, low precision of the estimates advises caution when drawing rigorous conclusions from this exercise.

Further robustness tests So far we used relative wage, relative individual income and relative education as proxies for bargaining power. As a robustness test we also apply relative individual non-wage income, which is not affected by labor market decisions. As individual non-wage income is zero for many individuals, the sample reduces to couples with at least one partner having positive non-wage income. If none of two partners has any non-wage income, relative non-wage income is undefined. In addition, we define a more comprehensive variable for full individual income, which includes both non-wage income and labor income as used in the baseline estimation. Results can be found in Tables A.3 and A.4 in the appendix for the preference indicator variable and the continuous preference difference variable. Estimated interaction effects with relative non-wage income are very imprecise in both specification and far from being statistically significant. Estimates of the bargaining power effects with relative full individual income are comparable to the baseline estimates for relative labor income and consistent with the theoretical predictions.

As another robustness test and in order to account for unobserved time-invariant heterogeneity, we apply a fixed-effects model which adds a couple fixed effect to the estimation equation. In this approach, estimates are only identified by within-couple variation over time, i.e., only changes in bargaining power and child preferences over time contribute to the identification. This also means that only couples with at least each an observation without and an observation with a birth are used in the identification. With five observations over time at maximum the variation left is arguably little. However, we report some results in the appendix for the preference indicator in Table A.5 and for the continuous preference difference in Table A.6. In columns (1) to (4) in both tables, we apply relative wages as the proxy ϕ. The interaction effects we are interested in have the expected signs but are statistically insignificant. In columns (5) to (8), ϕ is defined as relative years of schooling instead. Now, we find some statistically significant interaction effects of ϕ with weaker female child preferences that correspond to the theoretical prediction. However, the other estimates are imprecisely estimated. By and large, fixed-effects estimates do not contradict our findings, but are too weak to release concerns about unobserved confounding variation.

1.6 Concluding Remarks

In this paper, we derive a simple model of fertility decisions with heterogenous partners that has two main conclusions: female relative wage as a bargaining power determinant can have a negative opportunity cost effect and an ambiguous bargaining power effect on fertility. If her child preferences are stronger than her partner's, the bargaining power effect is positive. If her child preferences are weaker than her partner's, the bargaining power effect is negative. With concordant child preferences, the bargaining power effect is zero. We take this prediction to the data and find a differential bargaining power effect dependent on relative child preferences. The finding is robust to the inclusion of a number of control variables and different model specifications. Furthermore, we test the predictions using other bargaining power determinants, among them education as well as individual labor and non-wage income, and come up with consistent bargaining power effects. Even if endogeneity cannot be ruled out completely as an alternative explanation, we have vast descriptive evidence in support of the theoretical predictions.

For further research, using concordant and discordant preferences within couples is a new and alternative way of investigating bargaining in decision making about household public goods in general. Here, we have demonstrated the case of fertility, which is an important issue for policymakers. Taking into account relative preferences and bargaining power could be valuable especially when thinking about targeted transfers. If they change bargaining power within couples, they also affect the decision about household public goods according to relative individual preferences.

Appendix A

A.1 Supplementary Tables

Table A.1: Auxiliary regression–Relative wage and relative years of schooling with preference indicators

Dependent:	Relative wage	Relative wage $\times I(\alpha_f > \alpha_m)$	Relative wage $\times I(\alpha_f < \alpha_m)$
Relative education	*0.7818****	-0.0250***	-0.0187***
	(0.0738)	(0.0078)	(0.0054)
Relative education $\times I(\alpha_f > \alpha_m)$	-0.3147**	*0.6111****	-0.0015
	(0.1231)	*(0.1110)*	(0.0070)
Relative education $\times I(\alpha_f < \alpha_m)$	-0.0245	-0.0043	*0.8771****
	(0.1620)	(0.0151)	*(0.1647)*
Control variables	Y	Y	Y
F statistics	44.32	13.04	14.11
(All three auxiliary variables)			
F statistics	*112.3*	*30.33*	*28.35*
(Relevant auxiliary variable)			
Observations	11048	11048	11048

Notes: Coefficients from linear probability estimations. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

Table A.2: Auxiliary regression–Relative wage and relative years of schooling with preference differences

Dependent:	Relative wage	Relative wage $\times I(\alpha_f - \alpha_m)$
Relative education	*0.7010****	-0.0700*
	(0.0624)	(0.0414)
Relative education $\times I(\alpha_f - \alpha_m)$	0.0301	*0.7895****
	(0.0226)	*(0.0529)*
Control variables	Y	Y
F statistics	66.04	111.7
(All three auxiliary variables)		
F statistics	*126.2*	*222.7*
(Relevant auxiliary variable)		
Observations	11048	11048

Notes: Coefficients from linear probability estimations. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

Table A.3: Non-wage income–Bargaining power effects with preference indicators

Bargaining power proxy ϕ:	Relative non-wage income				Relative full individual income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times I(\alpha_f > \alpha_m)$	-1.8106	-2.2028	-2.1106	-2.1183	5.0814**	3.9055*	3.3179	3.3128
	(1.9435)	(1.9388)	(1.9303)	(1.9316)	(2.1468)	(2.1087)	(2.0978)	(2.0956)
$\phi \times I(\alpha_f < \alpha_m)$	0.2759	1.1519	1.3414	1.3388	-3.7979	-4.0631*	-4.5427*	-4.5470*
	(2.1877)	(2.3144)	(2.3084)	(2.3106)	(2.3429)	(2.3152)	(2.3199)	(2.3213)
ϕ	3.4432***	1.4365	1.2810	1.1726	-2.6819**	-0.6763	-0.0031	0.0389
	(1.0897)	(1.1133)	(1.1100)	(1.1169)	(1.0426)	(1.1313)	(1.1335)	(1.2369)
$I(\alpha_f > \alpha_m)$	-0.3202	0.2275	1.0878	1.0749	-1.5789**	-1.1979*	-0.0083	-0.0075
	(1.4295)	(1.4192)	(1.4647)	(1.4643)	(0.7264)	(0.7197)	(0.7203)	(0.7196)
$I(\alpha_f < \alpha_m)$	-3.9955***	-4.1387***	-3.4186***	-3.4424***	-0.6131	-0.3140	0.8406	0.8418
	(1.2180)	(1.2897)	(1.3015)	(1.3047)	(0.9519)	(0.9242)	(0.9324)	(0.9322)
Household income		0.0006	0.0005	0.0006		0.0002**	0.0002	0.0002
		(0.0004)	(0.0004)	(0.0004)		(0.0001)	(0.0001)	(0.0001)
Sum of preferences			1.1997***	1.1975***			1.6692***	1.6695***
			(0.3221)	(0.3220)			(0.1831)	(0.1832)
Wage				-0.0403				-0.0021
				(0.0347)				(0.0204)
Number of children fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	3,411	3,353	3,353	3,353	11,886	11,527	11,527	11,527

Notes: Coefficients from linear probability estimations with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times I(\alpha_f > \alpha_m)$ denotes the positive bargaining power effect and the estimate of $\phi \times I(\alpha_f < \alpha_m)$ the negative bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** $p<0.01$,** $p<0.05$,* $p<0.1$. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

Table A.4: Non-wage income–Bargaining power effects with preference differences

Bargaining power proxy ϕ:	Relative non-wage income				Relative full individual income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times (\alpha_f - \alpha_m)$	-0.8888	-1.3769	-1.2099	-1.2172	3.2777**	2.9465**	2.7029**	2.7016**
	(1.1099)	(1.1114)	(1.1032)	(1.1042)	(1.2882)	(1.2470)	(1.2499)	(1.2488)
ϕ	3.3620***	1.3713	1.1484	1.0452	-2.4636***	-0.7247	-0.1497	-0.1133
	(0.8327)	(0.8856)	(0.8836)	(0.8963)	(0.8481)	(0.9305)	(0.9347)	(1.0513)
$(\alpha_f - \alpha_m)$	-1.0553	-1.3690*	-1.6506**	-1.6522**	0.4685	0.4532	0.1045	0.1044
	(0.6924)	(0.7067)	(0.7054)	(0.7058)	(0.5109)	(0.4859)	(0.4900)	(0.4900)
Household income		0.0006	0.0005	0.0006		0.0002**	0.0002	0.0002
		(0.0004)	(0.0004)	(0.0004)		(0.0001)	(0.0001)	(0.0001)
Sum of preferences			1.4118***	1.4120***			1.6566***	1.6568***
			(0.3157)	(0.3155)			(0.1786)	(0.1788)
Wage				-0.0384				-0.0019
				(0.0345)				(0.0204)
Number of children fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	3,411	3,353	3,353	3,353	11,886	11,527	11,527	11,527

Notes: Coefficients from linear probability estimations with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times (\alpha_f - \alpha_m)$ denotes the bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

Table A.5: Fixed-effects estimation–Bargaining power effects with preference indicators for wage and schooling

Bargaining power proxy ϕ:	Relative wage				Relative years of schooling			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times I(\alpha_f > \alpha_m)$	0.7392	1.6152	1.4885	1.5419	-0.9398	-8.8997	-9.0802	-9.6888
	(2.6850)	(2.6864)	(2.6778)	(2.6820)	(16.1552)	(15.8416)	(15.7638)	(15.7613)
$\phi \times I(\alpha_f < \alpha_m)$	-3.1517	-2.5290	-2.3995	-2.1907	-43.7635**	-40.8197*	-40.5735*	-41.2356**
	(2.8725)	(2.8639)	(2.8616)	(2.8650)	(20.9732)	(20.9266)	(20.9729)	(20.8300)
ϕ	2.5662	1.7820	1.8095	0.6888	-21.0111	-23.5139	-24.1252	-25.2299
	(1.8547)	(1.9458)	(1.9450)	(2.1522)	(20.5634)	(20.3668)	(20.2522)	(20.2397)
$I(\alpha_f > \alpha_m)$	0.3138	-0.0430	0.5356	0.5281	0.5163	4.5296	5.1461	5.4668
	(1.1716)	(1.1786)	(1.1713)	(1.1716)	(7.9224)	(7.7753)	(7.7275)	(7.7276)
$I(\alpha_f < \alpha_m)$	-0.0345	-0.2087	0.3149	0.2938	20.6030**	19.2276*	19.6466*	20.0691*
	(1.4263)	(1.4105)	(1.4138)	(1.4131)	(10.5036)	(10.4672)	(10.5009)	(10.4268)
Household income		0.0010***	0.0010***	0.0010***		0.0010***	0.0010***	0.0009**
		(0.0004)	(0.0004)	(0.0004)		(0.0004)	(0.0004)	(0.0004)
Sum of preferences			1.1463***	1.1360***			1.1294***	1.1154***
			(0.3440)	(0.3430)			(0.3396)	(0.3390)
Wage				0.0526				0.0786**
				(0.0431)				(0.0383)
Number of children fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	11,709	11,352	11,352	11,352	11,579	11,231	11,231	11,231
Couples	6,207	6,058	6,058	6,058	6,118	5,973	5,973	5,973

Notes: Coefficients from fixed-effect within-estimations at individual level with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times I(\alpha_f > \alpha_m)$ denotes the positive bargaining power effect and the estimate of $\phi \times I(\alpha_f < \alpha_m)$ the negative bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

Table A.6: Fixed-effects estimation–Bargaining power effects with preference differences for wage and schooling

Bargaining power proxy ϕ:	Relative wage				Relative years of schooling			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\phi \times (\alpha_f - \alpha_m)$	0.8720	1.2106	1.0732	1.0363	12.8148	7.2111	7.0674	7.0887
	(1.6774)	(1.6284)	(1.6274)	(1.6258)	(11.0970)	(11.1543)	(11.1277)	(11.0702)
ϕ	2.2868	1.7242	1.7389	0.6786	-30.4213	-33.9248*	-34.1774*	-35.4842*
	(1.5967)	(1.6796)	(1.6797)	(1.8806)	(19.9559)	(19.7836)	(19.6827)	(19.6472)
$(\alpha_f - \alpha_m)$	-0.4721	-0.2669	-0.4289	-0.4256	5.9825	3.2404	3.0458	3.0886
	(0.7961)	(0.7807)	(0.7817)	(0.7809)	(5.4890)	(5.5194)	(5.5048)	(5.4747)
Household income		0.0010***	0.0010***	0.0010***		0.0010***	0.0010***	0.0009**
		(0.0004)	(0.0004)	(0.0004)		(0.0004)	(0.0004)	(0.0004)
Sum of preferences			1.1231***	1.1080***			1.1161***	1.0941***
			(0.3397)	(0.3385)			(0.3376)	(0.3368)
Wage				0.0515				0.0763**
				(0.0428)				(0.0380)
Number of children fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes		Yes	Yes	Yes
Observations	11,709	11,352	11,352	11,352	11,579	11,231	11,231	11,231
Couples	6,207	6,058	6,058	6,058	6,118	5,973	5,973	5,973

Notes: Coefficients from fixed-effect within-estimations at individual level with the dependent variable birth 12 to 23 months after the interview and multiplied by 100. The estimate of $\phi \times (\alpha_f - \alpha_m)$ denotes the bargaining power effect. ϕ is defined as the female's measure over the sum of both partners' measures. Robust standard errors from clustering on individuals in parentheses,*** p<0.01,** p<0.05,* p<0.1. Number of child fixed effects include an indicator variable for each contemporaneous number of children. Additional control variables include age and age squared of both partners, an indicator variable for being a student, indicator variables for foreign citizenship and second-generation immigrants, indicator variables for federal states, and year fixed effects. Data source: SOEP.

Chapter 2

Political Socialization in Flux? Linking Family Non-Intactness during Childhood to Adult Civic Disengagement[1]

[1]Joint with Helmut Rainer and Thomas Siedler

2.1 Introduction

Since the 1970s, economic and social changes in western societies have resulted in families that are more complex in their structure. Family breakdown through separation or divorce has become common, and non-marital birth rates have increased dramatically and appear to be continuing to rise.[2] Since most divorces involve children, there is now a substantially higher probability that children will have a lone parent than was once the case.[3] Projections of future changes in family structures indicate that the number of single-parent families is likely to further substantially increase over the next two decades in most OECD countries (see Figure 2.1).

An extensive body of research across a range of disciplines has identified childhood family structure as a key determinant of children's later-life socio-economic outcomes, emphasizing that children who grow up in a non-intact family: (i) tend to perform less well in school and to gain lower educational qualifications than children from intact families (Case et al., 2001; Ermisch et al., 2004; Gruber, 2004); (ii) are more likely to leave home when young and to become sexually active or pregnant at an early age (McLanahan and Sandefur, 1994); (iii) tend to report higher levels of smoking (Francesconi et al., 2010). However, one aspect of childhood family structure has remained largely neglected in the literature: its impact on children's later-life civic engagement.[4] The primary contribution of this paper is to fill that void.

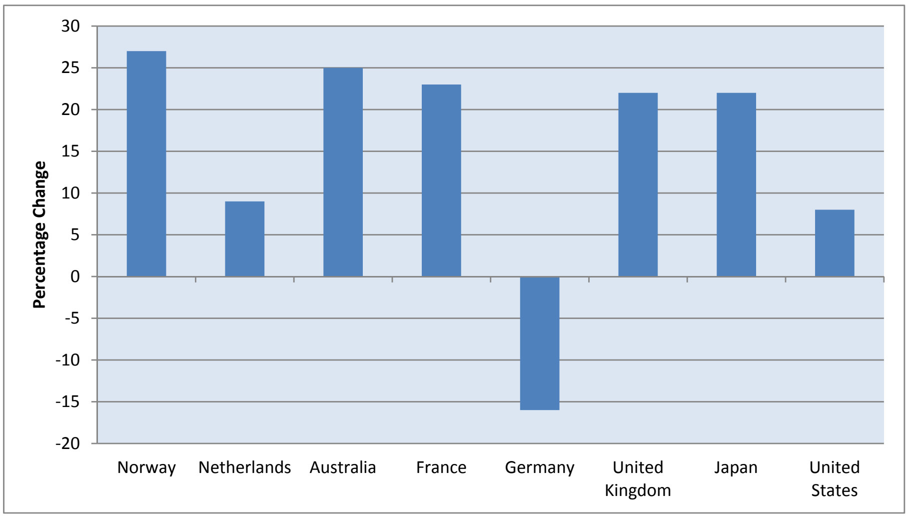
In discussions on children's socialization into politics and civic affairs, it has long been recognized that the family reproduces interest in the public domain (Hyman, 1959). This idea is based not only on evidence of a transmission of basic civic responsibilities and political orientations from parents to their offspring (Giuliano and Alesina, 2011), but also on findings in the area of partisan commitment and electoral behavior indicating high intergenerational agreement (Jennings et al., 2009). However, the research has also shown that the success of parental socialization of beliefs and values may differ systematically with family structure. It has been suggested in particular that growing up in a non-intact family frequently deprives children of important parental and community resources, in turn leaving them with a lack of knowledge and skills to operate effectively in society. One hypothesis, therefore, is that non-intact family structures during childhood have a negative causal effect on adult children's civic engagement, since they undermine and in some cases prevent the processes and activities through which parents shape their

[2] Across the OECD, the average divorce rate has doubled from 1.2 divorcees per 1000 people in 1970 to 2.4 in 2009, while the average proportion of births outside marriage has tripled from 11 percent in 1980 to 33 percent in 2007 (OECD, 2011a).

[3] On average, in developed countries, almost 1 out of 6 children today live with one parent only (Iacovou and Skew, 2010; OECD, 2010).

[4] Civic engagement can take many forms, from volunteer work at an individual level to involvement in larger organizations or political parties. In the present paper, we use civic engagement as an umbrella term to refer to these various types of civic engagement.

Figure 2.1: Projected percentage increase in the number of sole-parent families in selected OECD countries



Source: OECD (2012b). The periods over which changes are projected are as follows: Australia (2000 to 2026), Austria (2007 to 2030), France (2005 to 2030), Germany (2000 to 2025), Japan (2000 to 2030), the Netherlands (2009 to 2030), New Zealand (2006 to 2031), Norway (2002 to 2031), United Kingdom (2006 to 2031) and United States (2005 to 2030).

children's political orientations (*causation hypothesis*). The opposite hypothesis is that factors which increase the risk of family breakdown are also linked to children's civic and political engagement, implying that at least part of the relationship has to be seen as selective (*selection hypothesis*).

To gain a deeper understanding of these issues, we use data from the German Socio-Economic Panel on about 6,000 adult children with matched parental marital histories and family characteristics. We derive measures of family non-intactness during childhood by exploiting extramarital births and parental divorces, and perform both cross-sectional and sibling difference analyses of different indicators of civic engagement. The latter estimation method allows us to compare adult children to their own siblings and to control for time-invariant fixed effects of family background that may influence both childhood family structure and young adults' political behavior. Both exercises reveal a significant negative relationship between growing up in a non-intact family and children's civic, social and political engagement as adults. For example, sibling difference estimates suggest that adult children who lived in a non-intact family during childhood have a 10 percentage points lower likelihood of being interested in politics. This is a sizeable effect, given that 24 percent of adult children report being interested in politics. This means that we can reject the selection hypothesis in favor of a causal explanation for the negative link between growing up in a non-intact family and children's later involvement in public affairs.

Our results are robust to a number of specification and sensitivity checks including heterogeneity to gender, mother's education, and residential area, as well as an alternative measure of family non-intactness. Moreover, we also calculate the ratio of the impact of omitted variables relative to the explanatory variables needed to fully explain away the effect of family non-intactness on adult civic engagement. In line with the sibling difference estimates, the results of the "observables-unobservables" estimator support the causation hypothesis.

2.2 Civic Engagement: Trends, Relevance and Theories

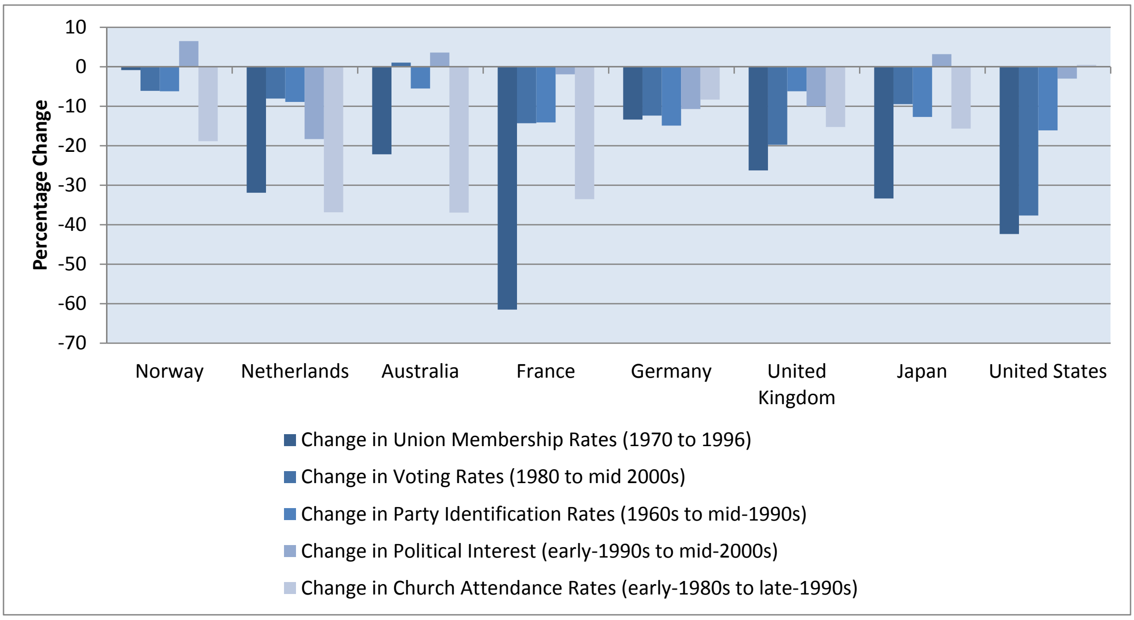
In the last few decades, the developed world has seen not only radical changes in family structures, but also another significant social development: the increasing disengagement of citizens from public affairs. Established democracies are managing to motivate an ever smaller proportion of the electorate to exercise the right to vote. The broad pattern of reduced civic engagement is confirmed by evidence in other domains as well: union membership, partisan attachment, political interest, and church attendance (see Figure 2.2). Indeed, Putnam (2002) argues that waning participation in political parties, unions and churches is almost universal. While the decrease in civic engagement is prevalent throughout the population, additional evidence shows that declines in voting, political interest and association membership are much more pronounced among younger cohorts than among older cohorts (Putnam, 2000).

These trends are a matter of concern for at least two important reasons. First, it is well understood that civic engagement strengthens civic values among the population and enhances the responsiveness of government and political elites to citizens' concerns (Fukuyama, 1995; Uslaner, 2002). Second, higher levels of civic engagement tend to go hand in hand with higher levels of social capital. This not only enhances democratic representation by facilitating interest aggregation and articulation (Putnam, 1993), but also promotes collective and collaborative action (Arrow, 1974). Moreover, a growing body of research suggests that social capital also has a positive impact on a wide range of macroeconomic and microeconomic outcomes. For example, Knack and Keefer (1997) provide strong evidence that norms of civic cooperation have significant impacts on aggregate economic activity.

The importance of civic engagement has made it a major focus of analysis and commentary in the social sciences. Explanations for the contemporary phenomenon of growing civic disengagement can be broken down into three main theories.[5] The "social capital" thesis (Putnam, 2000) associates declining civic engagement

[5] A comprehensive exposition and discussion of the key theories of contemporary civic disengagement can be found in Hay (2007).

Figure 2.2: Trends in different forms of civic engagement in selected OECD countries



Source: Figure 2.2 draws upon data from a variety of sources: (a) *union membership*: change in the percentage of employees who are union members, 1970 to 1996 (Source: OECD, 2012a, own calculations); (b) *voting*: change in voting rates in parliamentary elections, 1980 to most recent election (Source: OECD, 2011b, own calculations); the latest available year is 2005 for Germany, Japan, Norway and United Kingdom; 2006 for the Netherlands and the United States; and 2007 for Australia and France; (c) *party identification*: change in the percentage of people who identify with a democratic party in the long term (Source: Dalton, 2002, 25-26, own calculations); the period over which changes are computed are as follows: Australia (1967 to 1998), France (1975 to 1996), Germany (1972 to 1998), Japan (1962 to 1995), the Netherlands (1971 to 1998), Norway (1965 to 1993), United Kingdom (1964 to 1997) and United States (1952 to 1996); (d) *political interest*: change in the percentage of respondents who are "very or somewhat interested in politics" (Source: World Value Survey, own calculations); the period over which changes are computed is as follows: France, Germany, the Netherlands, United Kingdom, and United States (1990 to 2006), Japan (1990 to 2005), Norway (1990 to 2008), Australia (1995-2005); (e) *church attendance*: change in the percentage of respondents who"attend church at least once a month" (Source: World Value Survey, own calculations); the period over which changes are computed are as follows: France, Germany, the Netherlands, United Kingdom, and United States (1981 to 1999), Japan (1981 to 2000), Norway (1982 to 1996), Australia (1981-1995).

with an accelerated tendency towards individualism, leading to a disintegration in the community bonds that once held society together. The "critical citizens" thesis (Norris, 2002) suggests that younger cohorts of voters are more difficult to please than their parents' or grandparents' generations, and that they tend more to express dissatisfaction through abstention from political affairs. Finally, the "voting age" thesis (Franklin, 2004) argues that the lowering of the voting age in most advanced democracies can account almost entirely for recent declines in voter turnout. Of these explanations, the social capital hypothesis is the most pertinent to the study at hand. For Putnam (2000), lack of civic engagement and declining respect for the obligations of citizens in democracies are the result of the pervasive individualism that accompanies the disintegration of communities. Our findings add a new dimension to this view by showing that the *family disintegration* may also be a root cause of civic disengagement.

2.3 Why Does Family Structure Matter?

As we have seen above, the decline in civic engagement over recent decades coincided with a breakdown in traditional family structures. Despite this, Putnam (2000) dismisses the decline in the traditional family as a possible explanatory factor for the erosion of civic engagement:

> "... apart from youth- and church-related engagement, *none* of the major declines in social capital and civic engagement that we need to explain can be accounted for by the decline in the traditional family structure. In my view, there are important reasons for concern about the erosion of traditional family values, but I can find no evidence that civic disengagement is among them." Putnam (2000, p. 279)

It is important to note that Putnam's (2000) argument not only lacks a systematic empirical foundation but is also theoretically problematic in a number of ways. In this paper, we seek to contribute a more comprehensive understanding of how childhood family structure affects civic engagement in young adults.

As recently shown by Jennings et al. (2009), parents can have an enormous influence on their children's political learning in pre-adulthood. On the one hand, this finding coincides with childhood socialization theory, which emphasizes the role of the family in maintaining continuity in social ideologies over time (Glass et al., 1986, and references therein). More generally, however, it is also compatible with the idea that social capital within the family is of key importance for a child's intellectual development. In this regard, Coleman (1988, 109–113) argues that: (i) social capital within the family depends on the physical presence of adults and the time and effort spent by the adults with a child on intellectual matters; (ii) the physical absence of adults can be described as a structural deficiency in family social capital; and (iii) the most prominent element of structural deficiency in modern families is the single-parent family. Given these arguments, one might expect that the decision of parents to live separately—e.g., as a result of a divorce—damages the social capital that might have been available to the child had it been raised jointly by both parents. We now present a simple theoretical framework that formalizes this idea.

2.3.1 Social Capital Investments and Family Structure

Consider a family that is comprised of two parents, f and m, with one child. Since social capital is an important resource for individuals and may positively affect their ability to operate in society and their perceived quality of life, the adult child's level of social capital is assumed to be a public good to the parents. In other words, the parents are altruistic towards their child. Following Coleman (1988), we view the child's social capital as determined by the attention given by the parents to the child during childhood. Examples of the "attention" that we have in mind is the time spent by the parents with the child on intellectual matters, which we consider to positively influence the process of political learning through which the child acquires a clear picture of civic responsibility.

We assume that the parents can make time investments in their child's social capital during two distinct periods of childhood. In one of these periods (call it a), social capital investment takes place in an intact family environment, and we assume that parental decision-making can be characterized as *cooperative* in this case. In the other period (call it b), social capital investment takes place in a non-intact family environment, and we assume that parental decision-making is better characterized as *non-cooperative* in this case. We will be specific about this below.

We normalize the duration of the entire childhood to unity, and denote by $\pi^b \in [0, 1]$ the proportion of the childhood characterized by a non-intact family environment. Thus, $\pi^a = 1 - \pi^b$ is the fraction of the childhood spent in an intact family environment. In each period k ($k = a, b$), the parents can devote their time endowment, π^k, either to producing social capital, to working outside the home, or to leisure. When working outside the home, each parent can earn a wage w. In addition, each parent has non-labor income y. Social capital in period k is produced according to

$$S = f(\ell_f^k, \ell_m^k),$$

where ℓ_i^k is the time input of parent i ($i = f, m$). Each parent's preferences are represented by the additively separable utility function

$$U^i = v(x_i^a) + v(x_i^b) + g(h_i^a) + g(h_i^b) + z(S^a) + z(S^b),$$

where x_i^k and h_i^k denote personal consumption and personal leisure in period k, respectively. We assume that $v(\cdot)$ and $g(\cdot)$ are strictly increasing and concave functions. For simplicity, we let the composition of the utility function $z(\cdot)$ with the social capital function $f(\cdot)$ be given by

$$z(f(\ell_f^k, \ell_m^k) = \xi\mu(\ell_f^k) + (1-\xi)\mu(\ell_m^k),$$

where $\mu(\cdot)$ is a strictly increasing and concave function. The parameter ξ captures parent f's productivity in social capital investment relative to parent m.

Intact Family Suppose first that the entire childhood period is characterized by an intact family environment ($\pi^a = 1$). In this case, our approach to modeling social capital investments entails characterizing parental decision-making as cooperative. In adopting this approach, we follow the dominant premise in the economic theory of the family that intact households are able to reach efficient outcomes (Becker, 1991). One good reason for assuming this is that affective relationships among family members provide a foundation for low transaction costs, which are a prerequisite for the successful implementation of cooperative agreements (Pollak, 1985).

There are several ways to characterize an efficient allocation of family resources. Our approach is to maximize the utility of one parent subject to the other achieving a given utility and to the resource and social capital production constraints.[6] An interior solution implies that the social capital investment chosen by the two parents must satisfy

$$(2.1) \qquad \frac{1}{v'(x_f^a)} + \frac{1}{v'(x_m^a)} = \frac{1}{\mu'(\ell_f^a)}\frac{w}{\xi} \qquad \text{and} \qquad \frac{1}{v'(x_f^a)} + \frac{1}{v'(x_m^a)} = \frac{1}{\mu'(\ell_m^a)}\frac{w}{1-\xi},$$

respectively. This is the Samuelson condition for the efficient provision of a public good. Stated in words, it implies that the sum of the parents' marginal rates of substitution between the child's social capital and private consumption must equal the private cost of an extra unit of social capital relative to an extra unit of private consumption. For illustration, let $U^i = \ln(x_i^a) + \ln(h_i^a) + ln(S^a)$ and $S = (\ell_f^a)^\xi(\ell_m^a)^{1-\xi}$. In this case, optimal parental decision-taking implies a social capital level of

$$(2.2) \qquad \bar{S}^a = \mathbf{K}^a\left[1 + \frac{y}{w}\right] \qquad \text{where} \qquad \mathbf{K}^a = \left[\frac{\xi}{1-\xi}\right]^\xi \left[\frac{2(1-\xi)}{3}\right].$$

Non-Intact Family Suppose next that the entire childhood period is characterized by a non-intact family environment ($\pi^b = 1$). In this case, we assume that the parents cannot cooperate because they cannot communicate effectively with each other. Thus, we view them as behaving non-cooperatively.[7] Thus,

[6] Formally, the efficient outcome must maximize $v(x_f^a) + g(h_f^a) + z(S^a)$ subject to (a) $v(x_m^a) + g(h_m^a) + z(S^a) \geqslant \overline{u}$, (b) $(1 - h_f^a - \ell_f^a)w + (1 - h_m^a - \ell_m^a)w + 2y = x_f + x_m$ and (c) $z(S^a) = \xi\mu(\ell_f^a) + (1-\xi)\mu(\ell_m^a)$.

[7] In making this assumption, we follow the seminal contributions of Weiss and Willis (1985) and Del Boca and Flinn (1995), who provide formal analyses of the non-cooperative behavior of divorced parents in terms of child support transfers and expenditures on children.

each parent i maximizes $v(x_i^b) + g(h_i^b) + z(S^b)$ subject to $(1 - h_i^b - \ell_i^b)w + y = x_i$ and $z(S^b) = \xi\mu(\ell_f^b) + (1-\xi)\mu(\ell_m^b)$. In an interior equilibrium, the social capital investment chosen by the two parents solve:

$$\frac{1}{v'(x_f^b)} = \frac{1}{\mu'(\ell_f^b)}\frac{w}{\xi} \quad \text{and} \quad \frac{1}{v'(x_m^b)} = \frac{1}{\mu'(\ell_m^b)}\frac{w}{1-\xi}, \tag{2.3}$$

respectively. This says that, for each parent, the marginal rate of substitution between the child's social capital and private consumption must be equal to the marginal cost of socializing the child relative to that of the private good. If we employ the particular form of the parents' utility functions and the social capital production function used above, the equilibrium level of social capital is given by:

$$\bar{S}^b = \mathbf{K}^b\left[1 + \frac{y}{w}\right] \quad \text{where} \quad \mathbf{K}^b = \left[\frac{\xi}{2+\xi}\right]^{\xi}\left[\frac{1-\xi}{3-\xi}\right]^{1-\xi}. \tag{2.4}$$

2.3.2 Implications

Underinvestment in Social Capital The key implication of the above analysis is that family non-intactness disrupts the production process through which social capital within the family is created. Indeed, as with most privately provided public goods, the social capital available to the child will be underprovided when parents live apart and fail to coordinate their choices. To demonstrate this, we use equations (2) and (4) to compute:

$$\frac{\bar{S}^b}{\bar{S}^a} = \frac{\mathbf{K}^b}{\mathbf{K}^a} = \frac{3}{2}\left[\frac{1}{(2+\xi)^{\xi}(3-\xi)^{1-\xi}}\right] < 1.$$

Figure 2.3 shows that $\hat{S}^b/\hat{S}^a < 1$ for all $\xi \in [0,1]$. Thus, the non-cooperative equilibrium produces too little social capital. Moreover, the degree of underprovision is particularly high when the productivity in social capital investment differs between the parents (i.e., as $\xi \to 0$ or $\xi \to 1$). The intuition for underprovision is simple and follows from the public goods character of family social capital: if parents fail to cooperate, the actions that create social capital suffer from bidirectional externalities, and so it is not in each parent's interest to choose the socially efficient actions. Thus, an analogy can be drawn here to Coleman's (1988) idea that family non-intactness constitutes a structural deficiency in family social capital.

Duration of Family Non-Intactness So far, we have assumed that the entire childhood period is characterized either by an intact ($\pi^a = 1$) or by a non-intact ($\pi^b = 1$) family environment. Now suppose that the parents make investments in their child's social capital in two periods: one period of intact and one period of non-intact family environment. Let $\pi^b \in [0,1]$ be the fraction of childhood characterized

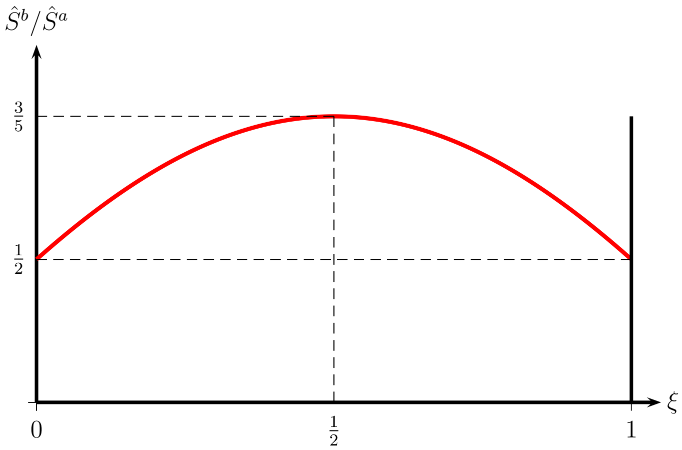


Figure 2.3: Family Non-Intactness and Underinvestment in Social Capital

by a non-intact family environment, while $1 - \pi^b$ is the fraction of the childhood spent in an intact family environment. With the particular utility and production functions assumed above, family social capital created over both periods is

$$\tilde{S} \equiv \tilde{S}^a + \tilde{S}^b = \mathbf{K}^a \left[1 - \pi^b + \frac{y}{w}\right] + \mathbf{K}^b \left[\pi^b + \frac{y}{w}\right].$$

It now follows directly that

$$\frac{\partial \tilde{S}}{\partial \pi^b} = -\mathbf{K}^a \left[1 - \frac{\mathbf{K}^b}{\mathbf{K}^a}\right] < 0.$$

Stated in words, the social capital created during the entire childhood period decreases with the duration of family non-intactness.

Summary In the empirical analysis, we focus on two related family structure measures that the analysis above suggests are important for young adults' social capital, including their civic engagement. The first measure of childhood family structure is whether a child spent any time living in a non-intact family. We would expect that young adults who experience family non-intactness as children are less likely to be civically engaged than those from intact families. The second measure does not indicate the mere occurrence of family non-intactness during childhood, but expresses the number of years a young adult experienced a non-intact family environment as a child. Our hypothesis is that adult civic engagement decreases with the duration of family non-intactness during childhood.

2.4 Data

Our data source is the German Socio-Economic Panel (SOEP)[8], a representative longitudinal survey of private households in Germany. We combine information from the first 26 annual interview waves (1984-2009). In the SOEP, individuals are re-interviewed each successive year. If children move out of their parents' home to form a new household, they are followed and all adults living in the new household are invited to become SOEP respondents as well. Moreover, children living in SOEP households become adult respondents in their own right the year they turn 17. The SOEP also collects retrospective lifetime marital, fertility and employment histories.[9] We combine retrospective information provided by mothers with their annual interview data. By using mother-child identifiers, we match all maternal characteristics to adult children. We then reconstruct a respondent's childhood family structure by combining his or her birth date and the mother's marital history. The data is therefore particularly well suited for the analysis of childhood family structure and its effects on young adults' outcomes.

For the pooled cross-sectional analysis (individual sample) in which we study repeated observations of civic outcomes in relation to childhood family structure, we select individuals who: (i) were 18 or younger in their first year as SOEP respondents; (ii) were living with their biological mother for at least one year during the panel; and (iii) have complete information on their mother's family history. We impose condition (i) to ensure an age structure that captures young adults in particular. Condition (ii) is necessary to match mothers' characteristics to young adults, as both generations had to be interviewed as adults. Condition (iii) ensures that childhood family structure can be consistently reconstructed for all young adults.

In a second step, we estimate sibling difference models (sibling sample), which requires us to impose further conditions: (iv) an individual must have at least one sibling; (v) civic outcome measures of the siblings must be observed in the same year. Conditions (i) through (v) are used to construct the sibling sample. For similar sample selection approaches and discussions, see Ermisch et al. (2004) and Francesconi et al. (2010).

2.4.1 Civic Engagement

As the main dependent variable in our empirical analysis, we use an index of civic engagement that averages together four component measures of civic engagement: (i) political interest; (ii) party identification; (iii) organizational involvement; and

[8]We use SOEP distribution v26.

[9]Note that, for many respondents, the retrospective information spans the pre-panel period, i.e., years before 1984.

(iv) individual voluntarism. In order to come to as broad a conclusion as possible, we not only present findings for the summary index, but also report results for its components. We start by providing a description of our outcome variables.

Political Interest In line with recent research suggesting that citizens with a greater interest in politics are more likely to be involved in public affairs (Bekkers, 2005), we view political interest as one precondition for civic engagement. In the empirical work, we make use of a survey question which reads: "Generally speaking, how much are you interested in politics?". We create an indicator variable which equals one if an individual reports being interested in politics ("very much" or "much"), and is zero for those who declare that they are not interested ("not so much" or "not at all").

Party Identification In the past few decades, the concept of party identification has reached an important position in electoral research because public attachment to political parties is seen as a key determinant of many different aspects of political behavior (Dalton, 2002). For example, partisan ties motivate people to participate in parties, elections, and the processes of representative government. One survey question asks: "Many people in Germany lean towards one party in the long term, even if they occasionally vote for another party. Do you lean towards a particular party?". We construct an indicator variable that equals one if a respondent reports a long-term identification with a democratic party,[10] and zero otherwise.

Organizational Involvement While official membership in formal organizations is only one aspect of civic engagement, it is regarded as a useful indicator of community involvement (Putnam, 2000). To construct a measure of organizational involvement, we use a question that reads: "Which of the following activities do you take part in during your free time? Please check off how often you do each activity: at least once a week, at least once a month, less often, never." We construct a dummy variable that equals one for individuals who report some kind of "involvement in a citizens' group, political party, or local government", and is zero for respondents who report no involvement at all.

Individual Voluntarism For Putnam (2000, 132–133), volunteering—i.e., the readiness to help others—is an important aspect of good citizenship and political involvement. He argues, for example, that volunteers are more interested in politics and less cynical about political leaders than non-volunteers are. To quantify individual voluntarism, we create an indicator variable that equals one for respondents who

[10]The right-wing extremist parties NPD, Republikaner, and DVU are not considered democratic parties.

report doing "voluntary work in clubs or social services" ("at least once a week", "at least once a month", "less often"), and is zero for individuals who report doing no volunteer work ("never").

For this outcome variable and for the outcomes *organizational involvement*, and *individual voluntarism*, we will also use the ordinal nature of the original survey question by estimating ordered probit models.

Index of Civic Engagement Our main variable of interest is an index of civic engagement that aggregates the four component measures described above. We derive our index of civic engagement from all survey years in which each of the four component measures is collected. Our information about *political interest* and *party identification* is derived from questions asked in the SOEP survey in the years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey in 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009.

The aggregation improves the statistical power by identifying effects that point in the same direction as different outcome measures with identical domains. As suggested by Kling et al. (2007), the summary index of civic engagement is an equally weighted average of its components' z-scores. The component measures used are such that higher scores reflect higher civic engagement. To compute the z-scores of each component, we subtract the mean in the estimation sample, in which none of the other components and none of the control variables has missing information, and divide it by the standard deviation. Thus, each standardized component has a mean of zero and a standard deviation of one. The index then aggregates the components with equal weights. Estimated coefficients on dichotomous explanatory variables can therefore be interpreted as percentage point changes in standard deviations of each component.

2.4.2 Childhood Family Structure

In terms of explanatory variables, the analysis that follows focuses on two measures of childhood family structure.

Our first measure, *ever lived in a non-intact family*, is derived using the self-reported marital histories of biological mothers. The sample is restricted to individuals whose mothers have complete marital histories spanning the individual's entire childhood. An individual is defined as having experienced family non-intactness during childhood if the child's mother was ever unmarried before he or she reached the age of 16, either because the parents ended their marriage (*divorced parents*), or because the mother was unmarried when she gave birth and did not marry within the next year (*born outside marriage*).

The variable *divorced parents* captures individuals who lived with divorced parents for at least one year of their childhood (ages 0-16). In light of Coleman (1988) and our simple theory, we would expect children of divorced parents to suffer from a structural deficiency in family social capital, and therefore be civically less engaged than children from intact families. The variable *born outside marriage* captures individuals either born to lone mothers or to cohabiting parents. It is important to note that children born outside marriage have not necessarily experienced family non-intactness, since cohabiting parents may have provided an intact family environment throughout the entire childhood period. However, it is well understood that mothers who were not in a relationship at the birth of their child are more likely to continue living as a lone parent than those in a serious relationship (Kiernan and Mensah, 2010). Moreover, a greater fragility of cohabiting unions compared with marital ones has been observed in most developed nations (Bumpass and Hu, 2000; Andersson, 2002). Since our data do not allow us to identify cohabitation history, we use the variable *born outside marriage* as a proxy for the family disruptions that affect children born into cohabitation or other unions than marriage.[11]

Our second measure of childhood family structure, *duration of family non-intactness*, is based on the first measure. However, instead of indicating the mere occurrence of family non-intactness during childhood, it expresses the number of years a child had lived with a divorced or unmarried mother. Given our theory, our expectation is that adult civic engagement decreases with the duration of family non-intactness during childhood.

2.4.3 Control Variables

Cross-sectional relationships between childhood family structure and adult civic engagement could be driven solely by selection, i.e., factors that increase the risk of family non-intactness may also be linked to children's civic engagement. We therefore choose potential observable confounders as control variables. Bedard and Deschenes (2005) show that the gender of the first child is related to parents' propensity to divorce. Gender is also likely to affect the political participation and civic engagement of adult children. Therefore, gender is among our control variables in the cross-sectional estimation.

Maternal education is also likely to affect both the probability of family non-intactness and the political behavior of children. While the classical Beckerian prediction for the effect of education on divorce is ambiguous (Becker et al., 1977),

[11] Because the variable *born outside marriage* captures not only children who experience family non-intactness at some time during their childhood, but also those who experience an intact family environment with long-term cohabiting parents, the estimates for it represent a conservative lower bound for the effect of family non-intactness on civic engagement.

we would expect parental education to go hand in hand with political and civic participation. We control for mothers' education by including three mutually exclusive indicator variables that equal one if they have completed intermediate secondary school (Realschule), advanced secondary school (Abitur), or a university degree, and zero otherwise.[12]

Closely related to the educational attainment of mothers is their labor market attachment. To control for a correlation between labor market participation and non-intactness and possible adverse effects on children's development (Ruhm, 2004), we add the number of years a mother worked part-time and the number of years she worked full-time during the individual's childhood.[13]

To complete our set of control variables we add usual socio-demographic measures, which may cause selection bias if not adjusted for. Among these are the mother's age at birth, a maximum set of age dummy variables, federal state dummy variables ("Bundesländer"), year of interview dummy variables, an only child indicator, and dummy variables for children's birth order. We also control for the SOEP respondent samples to account for peculiarities in survey-specific selection.

We deliberately do not control for household income during childhood. It should be noted that, as stated in Adda et al. (2011), reduced income is one channel through which family dissolution affects life outcomes, and this is not what we are interested in. Household income during childhood after departure of the father is likely to consist mainly of mother's income and is probably not a good measure of financial constraints affecting children if the children also benefit from an absent father's income.[14]

2.4.4 Summary Statistics

We present means and standard deviations for the individual sample and the sibling sample in Table 2.1, measured in the last survey year adult children are observed in the sample. It is evident that a large percentage of young adults are interested in politics, identify with a democratic party, and do volunteer work in clubs or social organizations. Organizational involvement is less common, with only 7 percent of young adults reporting volunteer work in a citizen's group, political party, or local government.

[12]Paternal education is not included, because we rarely observe fathers' educational attainment after a divorce or in extramarital birth circumstances.

[13]Note that paternal labor market history is missing too often in the case of non-intact families to ensure a non-selective sample when included.

[14]Moreover, household income is almost inevitably lower for lone mothers than for couples, such that controlling for income may lead to strong multicollinearity which might pick up most variation in non-intactness.

Table 2.1: Summary statistics, by sample

	Basic sample		Sibling sample	
	Means	Standard deviation	Means	Standard deviation
Dependent variables[a]				
Political interest	0.24	0.43	0.24	0.43
Party identification	0.30	0.46	0.30	0.46
Organizational involvement	0.07	0.26	0.07	0.26
Individual voluntarism	0.33	0.47	0.35	0.48
Explanatory variables				
Ever lived in a non-intact family	0.21	0.41	0.18	0.38
Parents divorced	0.15	0.35	0.12	0.32
Born outside marriage	0.09	0.28	0.06	0.22
Duration of family non-intactness	1.57	3.75	1.09	3.02
Duration: parents divorced	1.02	2.91	0.77	2.46
Duration: born outside marriage	0.55	2.48	0.32	1.79
Control variables				
Age	24.19	6.33	24.61	5.76
Female	0.50	0.50	0.51	0.50
Mother's age at birth	26.65	5.09	26.36	4.78
Only child	0.13	0.33		
Firstborn child[b]	0.40	0.49	0.38	0.48
Second born child[b]	0.40	0.49	0.42	0.49
Third born child or higher birth order[b]	0.20	0.40	0.20	0.40
Mother's highest educational attainment:				
Secondary school certificate or less	0.46	0.50	0.45	0.50
Intermediate school qualification	0.34	0.47	0.35	0.48
High school	0.04	0.19	0.04	0.20
Technical college or university degree	0.16	0.37	0.16	0.37
Mother's employment during childhood:				
Number of years part-time employed	4.76	5.31	4.83	5.23
Number of years full-time employed	5.90	6.35	5.23	6.10
Number of individuals	*5828*		*3325*	

Notes: Figures shown are sample means computed in individual last survey year for which individuals are observed. The sibling sample is constrained to the observations used in the sibling-fixed effects approach, i.e. two siblings must be observed in the same year. [a]Only for non-missing values in the dependent variables in individual last survey year. [b]Computed for children with siblings only.

21 percent of our respondents (18 percent of the sibling sample) lived in a non-intact family at some point during childhood; 15 (12) percent experienced the divorce of their parents and 9 (6) percent were born outside marriage. The dummy variables *parents divorced* and *born outside marriage* are not mutually exclusive. As a consequence, 3 (0) percent of respondents were born outside marriage and experienced the subsequent marriage and divorce of their mothers. The average age of adult children is 24 (25) years and both samples are balanced with respect to gender. Maternal educational attainment is rather low compared to contemporary standards, with 46 (45) percent of all mothers having completed lower secondary school at best and 34 (35) percent intermediate secondary school. Mothers were on average 27 (26) years old when they gave birth to their child. Maternal employment during childhood averages 4.76 (4.83) years in part-time and 5.90 (5.23) years in full-time work.

2.5 The Effect of Family Non-Intactness during Childhood on Adult Civic Engagement

We start our empirical analysis by estimating cross-sectional models to understand the overall relationship between family non-intactness during childhood and children's civic engagement later in life. Thereafter, we present sibling difference regressions which rest on weaker identifying assumptions for estimating the effect of family non-intactness on adult civic engagement. These models are meant to eliminate family-specific characteristics (e.g., parenting style, parents' political and social values, neighborhood environment) that are assumed to be the same across siblings. In a third step, we run a number of robustness tests including a bias assessment and examine differences by gender, residential area, and mother's education.

2.5.1 Cross-Sectional Analysis: Selection on Observables

Empirical model We start our empirical investigation by estimating pooled cross-sectional regressions of the form

$$OUTCOME_{ijt} = \beta_0 + \beta_1 NONINTACTNESS_{ij} + \beta_2 x_{ijt} + e_{ijt}, \tag{2.5}$$

where $OUTCOME_{ijt}$ is one of our five dependent variables described above for adult child i from mother j at time t. For the binary outcome variables, we estimate non-linear probit models. For the continuous index variable, we estimate ordinary least-squares regressions. The key variable $NONINTACTNESS_{ij}$ denotes the various childhood family structure measures for adult child i from mother j. The vector x_{ijt} includes all other control variables. Consistent and unbiased estimation of our key coefficient β_1 requires that all explanatory variables are uncorrelated with the error term e_{ijt}, obviously a very strong assumption which is unlikely to hold in the present context. Throughout the analysis, we compute standard errors that are robust to arbitrary forms of heteroscedasticity, as there are multiple observations of civic outcomes per individual over time.

Baseline results Table 2.2 reports the results from the pooled cross-sectional regressions. For convenience, we only report the estimates of our key explanatory variables. Panel A reports the estimates for the dichotomous explanatory variable whether adult children ever lived in a non-intact family during childhood. Panel B reports the estimated effects for the explanatory variables *parents divorced* and *born outside marriage*. The results in Table 2.2, Panel A, point to a negative and statistically significant relationship between growing up in a non-intact family and the majority of civic engagement outcomes. The point estimate in panel A, column 1 suggests that young adults who have lived in a non-intact family show a 10.4 percent of a standard deviation lower civic engagement (significant at the 1 percent level). This is a sizable effect, as it implies a 10.4 percent of a standard deviation decline in each of the index components, on average. Moreover, respondents who have ever lived in a non-intact family during childhood are 4 percentage points less likely to identify with a democratic party; 3 percentage points less likely to participate in a citizen's group, political parties or the local government; and 9 percentage points less likely to engage in volunteer work. These marginal effects are all precisely estimated and are statistically significant at the 1 percent level. Moreover, these are sizeable effects given that 30 percent of young adults report attachment to a democratic party, 33 percent volunteer in clubs or social services, and 7 percent are active in citizen's groups or political parties. The only point estimate that is not precisely estimated in Panel A is the one for the outcome political interest.[15]

Disentangling the two sources of non-intactness in Panel B reveals that a considerable portion of the negative associations between family non-intactness and the majority of outcomes are driven by offsprings of divorced parents. The civic engagement index is negatively associated with both growing up with divorced parents and being born outside marriage. We see a decline of 11.7 percent of a standard deviation for adults with divorced parents (1 percent significance) and a decline of 6.2 percent of a standard deviation for respondents born outside marriage (5 percent significance). However, the estimates cannot be distinguished statistically from one another, as indicated by the p-value of a Chow test for equality of coefficients at the

[15] We also estimated relationships between growing up in a non-intact family and another outcome of interest, the intention to vote. The corresponding survey question was asked only in 2005 and 2009, which leaves us with a very small sample. Although we found negative associations of family non-intactness with the intention to vote in the cross-sectional estimates, we decided not to report them in the paper, because we cannot verify the results in sibling difference estimations. Results can be found in Table B.1 in the Appendix.

Table 2.2: Childhood family structure and civic engagement
(Cross-sectional estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Ever lived in a non-intact family	-0.104**	-0.013	-0.041**	-0.027**	-0.088**
	(0.019)	(0.013)	(0.014)	(0.005)	(0.013)
Panel B					
Parents divorced	-0.117**	-0.019	-0.031+	-0.030**	-0.102**
	(0.020)	(0.015)	(0.016)	(0.005)	(0.013)
Born outside marriage	-0.062*	-0.005	-0.049*	-0.012	-0.041*
	(0.030)	(0.019)	(0.021)	(0.009)	(0.016)
Equality of coefficients (p-value)[1)]	*0.14*	*0.56*	*0.51*	*0.07*	*0.01*
Person-year observations	*18503*	*42913*	*40947*	*19738*	*19754*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Figures are marginal effects from OLS [(1)] or probit [(2),(3),(4),(5)] regressions. Probit effects are evaluated at the mean of all covariates. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Other explanatory variables are years of age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, east Germany, indicators of SOEP-samples, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

[1)] Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a chow test. We report p-values for the null hypothesis of equal coefficients.

bottom of the table. The marginal effects in columns 2-5 all point to a negative relationship between having experienced parental divorce or being born outside marriage and the civic engagement measures. Note, however, that only half of them are statistically significant. The results of the Chow tests for equality of coefficients suggest that the estimated coefficients for the two different non-intactness measures are only statistically different from each other for organizational involvement (p-value: 0.07) and individual voluntarism (p-value: 0.01). The test statistic reveals that the correlations with divorced parents is larger in magnitude (more negative) than with being born outside marriage.

Ordered probit estimates Three outcome measures—*political interest*, *organizational involvement* and *individual voluntarism*—are ordinal measures with four parameter values each in the original data. Next, we apply ordered probit estimation to use the full information contained in the data. This enables us to investigate the conditional incidence in each category between young adults from intact and non-intact families. The results are informative, as they give an idea about the nature of differences in civic engagement and whether changes occur in highly or marginally involved groups. Table 2.3 reports the marginal effects from ordered probit regressions. For the outcome political interest, we now find a statistically significant association with family non-intactness. Most notably, having experienced family non-intactness during childhood is associated with a 2.1 percentage points increase in not being interested in politics at all (5 percent significance) and a 1.6 percentage points decline in having much interest in politics (5 percent significance). The marginal effects for the outcomes organizational involvement and individual voluntarism are all precisely estimated and confirm the results in Table 2.2.

Duration of family non-intactness As laid out in the theory section, non-cooperative behavior between parents as a result of a dysfunctional relationship can decrease their children's civic engagement as young adults. The theory also reveals that the longer a period of childhood is spent in a non-intact family, the more severe the effects on social capital.

Table 2.4 reports the results for our explanatory variables, which measure the number of years young adults lived in a non-intact family. All estimates in Panel A of Table 2.4 point to a negative association between the number of years spent in a non-intact family and the civic engagement measures later in life. With the exception of the marginal effect for the outcome political interest, the point estimates are precisely estimated. For example, the result for the civic engagement index suggests that each additional year spent in a non-intact family is associated with a 0.8 percent decrease in standard deviations of civic engagement. Having spent half the childhood in a non-intact family would therefore imply a 6.4 percent decrease. The marginal effects indicate that each additional year spent in a non-intact family

Table 2.3: Ordered probit results
(Cross-sectional estimates)

Dependent variable:	(1) Political interest	(2) Organizational involvement	(3) Individual voluntarism
Answer category:			
Not at all	0.021*		
	(0.010)		
Not much	0.001+		
	(0.001)		
Much	-0.016*		
	(0.007)		
Very much	-0.006*		
	(0.003)		
Person-year observations	*42923*		
Answer category:			
Never		0.030**	0.084**
		(0.007)	(0.013)
Less frequently		-0.023**	-0.026**
		(0.005)	(0.004)
Every month		-0.005**	-0.018**
		(0.001)	(0.003)
Every week		-0.003**	-0.040**
		(0.001)	(0.006)
Person-year observations		*19738*	*19754*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Figures are marginal effects from ordered probit regressions evaluated at the outcome categories. The figures are to be interpreted as the difference in probability that the treated individuals (treatment: family non-intactness) respond in that category compared to the non-treated. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Other explanatory variables are years of age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, east Germany, indicators of SOEP-samples, and a constant. The information about *political interest* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

during childhood is associated with a 0.4 percentage point smaller probability of party identification (5 percent significance), a 0.2 percentage point lower probability of active involvement in a citizen's or political group (1 percent significance) and a 0.9 percentage point lower probability of volunteering (1 percent significance).

Table 2.4, Panel B, shows the results for distinct sources of family non-intactness. Although all correlation coefficients are negative for both explanatory variables, only four of the ten estimates are precisely estimated. For instance, each additional year with divorced parents is associated with a 1.2 percentage points reduction in standard deviations of civic engagement (1 percent significance). According to the Chow test, this effect is significantly different from the estimated coefficient on the number of years having lived with parents outside marriage. Overall, the results in Table 2.4 are in line with our hypothesis that adult civic engagement decreases with the duration spent in a non-intact family during childhood.

2.5.2 Sibling Difference Analysis: Selection on Unobservables

Empirical model Our cross-sectional results cannot be readily interpreted as causal. The major threat to causal identification in our setting is omitted variable bias or selection bias. Reverse causality is arguably not the main concern for identification. Our outcomes are behavioral measures of adults, and these are not likely to cause family dissolution during childhood. Furthermore, we controlled for selection on observable characteristics in our pooled cross-sectional estimations to exclude the main sources of selection bias. However, unobservable factors may still confound the estimates. Parents who are likely to divorce may have different unobserved preferences, values and abilities than parents who are unlikely to divorce. These unobserved characteristics may in turn affect civic engagement of their offspring later in life, confounding the negative correlation in our cross-sectional estimates. We address this issue with sibling difference estimation (mother fixed-effects) of the form

$$OUTCOME_{ijt} = \beta_0 + \beta_1 NONINTACTNESS_{ij} + \beta_2 x_{ijt} + \eta_{jt} + e_{ijt}, \tag{2.6}$$

where η_{jt}, an unobserved effect for siblings, is added to equation (1) in order to eliminate time-invariant unobserved background characteristics from mother j. Sibling difference estimation eliminates all observed and unobserved time-constant mother-specific factors which are assumed to be the same for siblings and which might be associated with both family non-intactness and children's civic engagement later in life. Hence, the unobserved mother-specific error term η_{jt} cancels out from our sibling difference regressions. The identifying assumption for unbiased

Table 2.4: Duration of family non-intactness and civic engagement
(Cross-sectional estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Duration of family non-intactness	-0.008**	-0.001	-0.004*	-0.002**	-0.009**
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Panel B					
Duration: parents divorced	-0.012**	-0.001	-0.004+	-0.004**	-0.014**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Duration: born outside marriage	-0.003	-0.001	-0.003	-0.001	-0.002
	(0.004)	(0.002)	(0.003)	(0.001)	(0.002)
Equality of coefficients (p-value)[1)]	*0.03*	*0.99*	*0.88*	*0.07*	*0.00*
Person-year observations	*18503*	*42913*	*40947*	*19738*	*19754*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Figures are marginal effects from OLS [(1)] or probit [(2),(3),(4),(5)] regressions. Probit effects are evaluated at the mean of all covariates. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Other explanatory variables are years of age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, east Germany, indicators of SOEP-samples, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.
[1)]Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a chow test. We report p-values for the null hypothesis of equal coefficients.

sibling difference estimators is uncorrelatedness of sibling differences in family non-intactness with sibling differences in unobserved individual characteristics.[16] This is a much weaker identifying assumption than imposed by the cross-sectional estimator which needs family non-intactness and unobserved mother-specific factors to be uncorrelated. We estimate sibling differences in the same observational year t, and use mother level clustering for estimating standard errors to account for correlations across siblings.

Other concerns for causal identification in sibling difference estimations are inevitable age and birth order differences between siblings. Among children born outside marriage, the affected child is always older and of lower birth order than the compared and unaffected child. When estimating the effects of growing up with divorced parents, the affected sibling is usually younger, and of higher birth order than the unaffected child. Therefore, we add age and birth order fixed effects to the sibling difference estimation that are likely to eliminate confounding variations from these sources.

It is important to keep in mind that sibling difference estimates are identified through adult siblings with differing family non-intactness during childhood years. Hence, the sibling difference estimates may deviate from cross-sectional estimates and require a different interpretation. First, sibling difference estimates are based on a sibling sample, and only children are excluded from the sample. Second, sibling differences occur only in particular situations. The individual affected by family non-intactness during childhood is compared to a sibling who did not grow up in a non-intact family. As they have the same mother, both siblings experience the event of a non-intact family, but at different times in their lives. For instance, the younger sibling might experience the divorce of his parents during childhood, at a point in time when the older sibling was already an adult and had already moved out on her own. If we assume that non-intactness has a negative effect on civic engagement, even if it occurs when children are adults and no longer live at home, our estimates can be regarded as lower bounds.

Sample description Identification of sibling difference estimation hinges on within-sibling variation in family non-intactness during childhood and within-sibling variation in civic engagement. Table 2.5 summarizes the number of sibling pairs per year with differences in both family non-intactness and civic outcome measures. We can draw on over 1,000 sibling differences in family non-intactness when analyzing political interest and party identification. This number drops just below 500 for the outcomes organizational involvement and individual voluntarism. Within-sibling differences in outcomes generally occur more often than differences in family non-intactness. Table 2.5 also reports the number of observations when splitting the

[16] Recent studies using a similar estimation method are Ermisch and Francesconi (2012), Currie et al. (2010) and Francesconi et al. (2010). Ermisch and Francesconi (2001) provide a detailed discussion of the advantages and disadvantages of the sibling difference approach.

sample by (i) gender; (ii) mother's education and (iii) residential area. Note that, for the majority of subsamples, we still have a comfortable number of differences in family non-intactness, but in some cases the differences used for identification fall below 200. Interpretation of results based on relatively small samples will be made with caution.

Baseline results Sibling difference estimates of the effect of family non-intactness on civic engagement are presented in Table 2.6. In the first column of Panel A, effects on the civic engagement index are negative and statistically significant at the 1 percent level. Growing up in a non-intact family reduces civic engagement by 15.7 percent in standard deviations. In Table 2.7 we report cross-sectional estimates within the same sample as in the sibling difference estimation.[17] We see that the effects of the sibling difference estimation do not differ from the corresponding cross-sectional point estimates. For the component outcome measures in the sibling difference analysis, family non-intactness during childhood decreases the occurrence of political interest by 9.4 percentage points, decreases identification with a democratic party by 7.7 percentage points, and decreases individual voluntarism by 8.1 percentage points. These marginal effects are precisely estimated and statistically significant at the 5 percent level. We also find a negative effect in the magnitude of 2.8 percentage points on organizational involvement, but we cannot statistically distinguish it from zero. In sum, the sibling differences results support our findings from the cross-sectional estimations. Therefore, the negative relationship between family non-intactness and adult children's civic engagement are unlikely to be entirely driven by unobserved heterogeneity.[18] Regarding the magnitude of effects on the component measures, the sibling difference estimates compared to cross-sectional estimates are slightly larger for political interest and party identification, but slightly smaller for organizational involvement and individual voluntarism. As these differences vanish when estimating the effect on the index of civic engagement, unobserved mother characteristics seem not to be very influential. We conclude that there is not much negative selection of low civic engagement families into non-intactness. However, the cross-sectional results in the siblings sample are larger than the cross-sectional results in the full sample which points to a somewhat stronger effect of family non-intactness in families with more than one child.

Turning to the separate effects of the two sources of family non-intactness, *divorced parents* and *born outside marriage*, reveals a similar picture. Effects of divorced parents on the civic engagement index are minus 17.1 percent in standard deviations (5 percent significance) and minus 13.3 percent in standard deviations for

[17]Note that due to the larger set of control variables in the cross-sectional estimate, we loose some observations.

[18]Conditional logit marginal effects for the components assuming fixed effects of zero are shown in Table B.2. Effects show the same negative sign but are less precisely measured.

Table 2.5: Means of civic engagement measures and within-siblings variation

	Political interest			Party identification			Organizational involvement			Individual voluntarism		
		Number of sibling pairs with differences in:			Number of sibling pairs with differences in:			Number of sibling pairs with differences in:			Number of sibling pairs with differences in:	
	Mean	Family structure[a]	Outcome[b]	Mean	Family structure[a]	Outcome[b]	Mean	Family structure[a]	Outcome[b]	Mean	Family structure[a]	Outcome[b]
All	0.25	1027	3574	0.35	948	3330	0.08	481	654	0.33	480	1762
By gender[c]												
Women	0.17	239	708	0.32	220	759	0.07	114	136	0.31	114	434
Men	0.32	310	1080	0.38	300	975	0.09	152	198	0.36	151	518
By mothers education												
Less than high school degree	0.23	832	2822	0.34	777	2732	0.07	393	532	0.33	392	1466
High school or university degree	0.31	165	740	0.40	158	590	0.08	83	122	0.36	83	292
By residential area:												
Rural	0.20	249	768	0.30	241	792	0.09	117	164	0.38	118	463
Urban	0.26	529	1586	0.20	506	1420	0.07	253	265	0.30	252	711

Notes: [a] The number of sibling pairs with differences in the experience of family structure during childhood, e.g. one sibling grew up in a non-intact family and the other sibling in an intact family. [b] The number of sibling pairs with differences in the political outcome measure. [c] Both siblings of the same sex.

Table 2.6: Family non-intactness and civic engagement
(Sibling difference estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Ever lived in a non-intact family	-0.157**	-0.094**	-0.077*	-0.028	-0.081*
	(0.052)	(0.036)	(0.036)	(0.018)	(0.040)
Panel B					
Parents divorced	-0.171*	-0.048	-0.069	-0.040+	-0.101+
	(0.082)	(0.051)	(0.052)	(0.022)	(0.058)
Born outside marriage	-0.133*	-0.126**	-0.066	-0.018	-0.037
	(0.056)	(0.043)	(0.042)	(0.021)	(0.047)
Equality of coefficients (p-value)[1)]	*0.70*	*0.25*	*0.96*	*0.48*	*0.40*
Person-year observations	*8892*	*20613*	*19679*	*9445*	*9448*
Number of sibling-year pairs	*4423*	*9751*	*9663*	*4478*	*4479*
Birth order FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Results are sibling-difference estimates at same survey time. Estimates from linear fixed-effects models. Robust standard errors in parentheses. Standard errors are clustered on mother's identification number, as there are multiple observations for sibling pairs. Other explanatory variables are years of age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

[1)]Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a chow test. We report p-values for the null hypothesis of equal coefficients.

Table 2.7: Family non-intactness and civic engagement
(Cross-sectional estimates in sibling sample)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Ever lived in a non-intact family	-0.157**	-0.052**	-0.067**	-0.038**	-0.121**
	(0.026)	(0.018)	(0.021)	(0.007)	(0.019)
Panel B					
Parents divorced	-0.150**	-0.036+	-0.037	-0.044**	-0.134**
	(0.029)	(0.021)	(0.025)	(0.007)	(0.020)
Born outside marriage	-0.134**	-0.074**	-0.102**	-0.014	-0.060+
	(0.040)	(0.023)	(0.032)	(0.013)	(0.032)
Equality of coefficients (p-value)[1)]	*0.74*	*0.22*	*0.12*	*0.02*	*0.05*
Person-year observations	*8,873*	*20,560*	*19,612*	*9,409*	*9,425*

Notes: Results from the same sample as in for the sibling difference approach. Observations with missing information in cross-sectional control variables are dropped. Each column in each panel reports the results of a regression for the outcome listed in that column. Figures are marginal effects from OLS [(1)] or probit [(2),(3),(4),(5)] regressions. Probit effects are evaluated at the mean of all covariates. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Other explanatory variables are years of age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, east Germany, indicators of SOEP-samples, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

[1)]Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a chow test. We report p-values for the null hypothesis of equal coefficients.

children born outside marriage (5 percent significance).[19] The estimated effects on the component outcome measures are all negative, but only three point estimates are precisely estimated. The sibling difference estimates are again close to the cross-sectional estimates in Table 2.7. The effect of divorced parents on civic engagement is slightly larger in the sibling difference estimation (-0.171) than in the cross-sectional estimation (-0.150), but overall differences are moderate.

Duration of family non-intactness We show estimates of the sibling difference model with duration of family non-intactness as explanatory variable in Table 2.8. In Panel A, we find a negative effect of each additional year in a non-intact family on civic engagement of 2.4 percent in standard deviations (5 percent significance). According to this point estimate, having had spent half of childhood (8 years) in a non-intact family decreases civic engagement by 19.2 percent. For the component measures, we find negative significant effects of the duration of family non-intactness on party identification (1 percent significance). The estimated effects on political interest, political participation and civic engagement are all negative, but imprecisely estimated. Compared to the cross-sectional estimates in Table 2.9 within the same sample the sibling difference estimates are somewhat larger in magnitude. In the cross-sectional estimation the association of civic engagement and duration of family non-intactness is only minus 1.3 percent. Thus, mother specific factors seem to play a larger role here.

When turning to Panel B and distinguishing the sources of family non-intactness, the experience of parental divorce shows a slightly larger negative effect of 2.7 percent in standard deviations on the civic engagement index (5 percent significance) than being born outside marriage. The estimated coefficient of minus 1.9 for the explanatory variable *born outside marriage* is not precisely estimated, and not statistically distinguishable from the estimated effect on the variable *parents divorced*. Moreover, we find significant negative effects of growing up with divorced parents on adult children's party identification and organizational involvement (5 percent significance). Being born outside marriage is estimated to have negative significant effects on political interest (1 percent significance) and party identification (5 percent significance). By and large, the results for family non-intactness duration resemble the effects found for the occurrence of non-intactness. Furthermore, the estimates are consistent with our theory.

Sibling differences—Robustness Comparison of siblings who both experienced a very similar family background during childhood may raise some concerns. On the one hand, it is a prerequisite for our identification strategy to have very similar backgrounds of compared siblings. This is essential to exclude family omitted

[19] Note that these two point estimates are not different from each other at conventional significance levels.

Table 2.8: Duration of family non-intactness and civic engagement
(Sibling difference estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Duration of family non-intactness	-0.024*	-0.011	-0.026**	-0.002	-0.002
	(0.011)	(0.008)	(0.009)	(0.004)	(0.001)
Panel B					
Duration: parents divorced	-0.027*	0.004	-0.024*	-0.007*	-0.010
	(0.012)	(0.010)	(0.011)	(0.003)	(0.011)
Duration: born outside marriage	-0.019	-0.039**	-0.024*	0.004	0.008
	(0.019)	(0.011)	(0.012)	(0.007)	(0.015)
Equality of coefficients (p-value)[1)]	*0.71*	*0.00*	*0.98*	*0.14*	*0.34*
Person-year observations	*8892*	*20613*	*19679*	*9445*	*9448*
Number of sibling-year pairs	*4423*	*9751*	*9663*	*4478*	*4479*
Birth order FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Results are sibling-difference estimates at same survey time. Estimates from linear fixed-effects models. Robust standard errors in parentheses. Standard errors are clustered on mother's identification number, as there are multiple observations for sibling pairs. Other explanatory variables are years of age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

[1)] Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a chow test. We report p-values for the null hypothesis of equal coefficients.

Table 2.9: Duration of family non-intactness and civic engagement (Cross-sectional estimates in sibling sample)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Duration of family non-intactness	-0.013**	-0.006*	-0.007*	-0.002+	-0.013**
	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)
Panel B					
Duration: parents divorced	-0.015**	-0.003	-0.003	-0.006**	-0.020**
	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)
Duration: born outside marriage	-0.011*	-0.013**	-0.015**	0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.002)	(0.004)
Equality of coefficients (p-value)[1)]	*0.41*	*0.03*	*0.04*	*0.04*	*0.00*
Person-year observations	*8,873*	*20,560*	*19,612*	*9,409*	*9,425*

Notes: Results from the same sample as in for the sibling difference approach. Observations with missing information in cross-sectional control variables are dropped. Each column in each panel reports the results of a regression for the outcome listed in that column. Figures are marginal effects from OLS [(1)] or probit [(2),(3),(4),(5)] regressions. Probit effects are evaluated at the mean of all covariates. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Other explanatory variables are years of age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, east Germany, indicators of SOEP-samples, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

[1)] Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a chow test. We report p-values for the null hypothesis of equal coefficients.

variable bias. On the other hand, if siblings are born within a very small time span, small differences in age between the siblings will likely bias our results downwards, because both siblings will be affected almost identically. This might result in underestimating the effect of family non-intactness. As a robustness check, we estimate the intergenerational effects for a smaller sample of siblings who are born more than two years apart. In fact, the estimated coefficients are larger in magnitude and more precisely estimated than the sibling difference estimates in Table 2.6. The results from this exercise support our main finding that growing up in a non-intact family seems to have negative and long-lasting influences on children's civic engagement. Results can be found in the Appendix in Table B.6.

2.5.3 Assessing Omitted Variable Bias

In the cross-sectional estimations we dealt with bias from observable characteristics, in the sibling difference estimations we took care of unobservable mother-specific characteristics. Yet, bias from unobserved time-varying or individual heterogeneity could still be a threat to identification if it confounds the estimates. Here, we ought to assess whether omitted variable bias is a serious threat to our results. We take up on an approach from Bellows and Miguel (2009) to quantify how strong the covariance of unobserved factors with the variable of interest would have to be relative to the covariance of control variables with the variable of interest in order to explain away the entire estimated effect.[20] The intuition is that our controls capture many sources of selection into family non-intactness and that it is unlikely that we omit factors which are correlated many times stronger than that. We explore the importance of omitted variable bias for both cross-sectional and sibling differences results, both in the siblings sample. We denote omitted factors as $\tilde{q} = \eta + \nu$, with $\tilde{q}$ being omitted factors in cross-sectional estimation and ν being omitted factors in sibling differences estimation after accounting for mother fixed-effects η. Derivation of the estimators and underlying assumptions are explained in detail in the appendix.

In the first row of Table 2.10, we see that the covariance between family non-intactness and unobservable factors ($Cov(N, \tilde{q})$) has to be 3.489 times as strong as the covariance between family non-intactness and the observed control variables in order to explain away our cross-sectional estimate for civic engagement. Although it is arbitrary to compare absolute numbers across different setups, Bellows and Miguel (2009) argue that factors above three or above five would be sufficient to believe that bias cannot be fully confounding. In the second row of Table 2.10, we see that the factor for the sibling difference estimate is 3.532. It means that the covariance of between family non-intactness and unobserved individual and time-varying factors ($Cov(N, \nu)$) must be 3.532 times as strong as the covariance between family non-intactness and observable controls including mother fixed-effects to explain away

[20] A related approach by Altonji et al. (2005) assumes that the selection on observables is the same as the selection on unobservables in order to estimate the bias.

Table 2.10: Bias assessment of estimates
(Sibling sample)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A:					
Non-Intactness					
Cross-sectional: $\frac{Cov(N,\tilde{q})}{Cov(N,X'\beta)}$	3.489	5.487	2.258	n.a.	5.806
Sibling differences: $\frac{Cov(N,\nu)}{Cov(N,X'\beta+\eta)}$	3.532	n.a.	13.726	2.491	1.527
Panel B:					
Divorce					
Cross-sectional: $\frac{Cov(N,\tilde{q})}{Cov(N,X'\beta)}$	3.297	1.568	1.175	n.a.	9.626
Sibling differences: $\frac{Cov(N,\nu)}{Cov(N,X'\beta+\eta)}$	7.039	15.637	n.a.	6.093	2.524
Born outside marriage					
Cross-sectional: $\frac{Cov(N,\tilde{q})}{Cov(N,X'\beta)}$	2.475	n.a.	3.367	2.638	1.219
Sibling differences: $\frac{Cov(N,\nu)}{Cov(N,X'\beta+\eta)}$	2.474	n.a.	1.298	n.a.	0.610

Notes: The factors denote how many times larger the covariance between the variable of interest and unobserved factors would have to be to explain away the entire effect compared to the covariance of the variable of interest and observed controls. *N.a.* denotes that the bias is not confounding, i.e. that it operates in the opposite direction as the estimated effect. All estimates are undertaken in the siblings sample as OLS estimations. Control variables in the cross-sectional estimations are years of age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, east Germany, indicators of SOEP-samples, and a constant. Control variables in the sibling differences estimations are years of age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant.

the entire sibling differences estimate for civic engagement. Unobserved factors left in the sibling difference estimation and in the cross-sectional estimation are thus unlikely to explain the entire effect of family non-intactness on civic engagement.

Turning to Panel B and divorce as the explanatory variable, we find a factor of 3.297 for the cross-sectional estimate on civic engagement. For the sibling difference estimate we find a larger factor of 7.039. Confounding variation to explain away the entire effect of divorce during childhood on civic engagement is therefore very unlikely in the sibling difference estimation. By excluding unobserved mother-specific factors, we gain confidence in the unbiasedness of the estimates.

For effects of born outside marriage the cross-sectional factor is only 2.475, the sibling difference factor is 2.474. With these comparatively low factors we cannot be as confident that the results are not driven by unobserved factors.

We can conclude for the civic engagement index results that the sibling difference estimates are a serious improvement compared to the cross-sectional estimates. Among the sibling difference estimates the most robust and therefore trustworthy estimates seem to be those for divorce during childhood. The bias assessment supports the view to understand the estimates as causal effects.

Turning to the disaggregated components of civic engagement in columns (2) to (5) in Table 2.10, we see rather mixed results. The most credible results according to the bias factors are again the estimates of divorce during childhood from sibling difference estimations. In some instances, when the suggested bias works in the opposite direction than the estimated effect, we do not report the non-interpretable factor.

2.5.4 Effect Heterogeneity

We have seen substantial effects of family non-intactness on civic engagement later in life. These effects may not be universally relevant for different groups of the population, and effect heterogeneity may arise for a variety of reasons. For instance, when thinking about the effect of family non-intactness during childhood on civic engagement, we have in mind certain modes of nurturing that might have adverse consequences for children's development. In the majority of families, non-intactness implies an absent father, and daughters and sons might therefore be affected differently by growing up in a non-intact family.[21] In pooled cross-sectional regressions, we did not find clear heterogeneous effects of non-intactness by gender. However, in the sibling difference estimations, we found negative and statistically significant effects for men only, and the magnitude of the effects of growing up in a non-intact family was larger for men than for women in most regressions.

[21] Note that we cannot actually distinguish effects of missing fathers and missing mothers as there are too few observed lone fathers.

Nurturing capacity or the effectiveness of parenting may as well produce heterogeneous effects of family non-intactness. In the case of missing fathers, mothers with higher capacity should be more able to compensate for the potential loss of paternal involvement in their children's development. On the other hand, highly educated women are likely to be married to highly educated men, and the absence of highly educated fathers might be more deleterious for children. We therefore distinguish between adult children with highly educated mothers (completed upper secondary school or university degree), and those with lower educated mothers. Overall, the results did not point to substantial differences in the effects of family non-intactness by mother's education.

Putnam (2000) notes that social connectedness among inhabitants of densely populated urban areas is weaker than in smaller communities. This sense of connectedness and familiarity in everyday interactions makes it more rewarding to be engaged in public life and activities. The vastly different prospects for civic engagement between rural and urban residential areas may also produce heterogeneous effects of family non-intactness. However, distinguishing between individuals who grew up in urban and rural areas did not point to heterogeneous effects.[22]

In sum, the effect of family non-intactness on civic engagement later in life is not restricted to either males or females, children of low or highly educated mothers, or rural or urban residential areas. We find some indication of more severe effects for boys and slightly weaker effects for girls. Due to the low precision of some estimates, there remains some uncertainty about the nature of heterogeneities. However, the overall phenomenon of adverse effects of non-intact families during childhood on civic engagement seems to be strikingly universal.[23]

2.6 Final Remarks

With the nuclear family gradually declining as the dominant social unit in contemporary society, effects of family non-intactness are non-negligible. Democracies depend on an active and informed community: waning civic engagement should therefore be taken seriously. In this research paper, we find robust evidence of a causal link of growing up in a non-intact family on civic disengagement later in life. It is especially disturbing that this effect does not seem to be restricted to certain groups defined by socio-demographic measures but rather constitutes a universal phenomenon. The question remains whether these are lasting disruptions or rather short-term consequences of broader social changes and adjustments to more diverse ways of life. Future research should therefore find ways to disentangle the effects

[22] We also examined another potential source of heterogeneity: the age of the child at the time of parents' divorce. Again, our results did not point to important heterogeneous effects.

[23] Results can be seen in the appendix in Tables B.3, B.4 and B.5

of family non-intactness regarding the channel of emotional stress and stigma or acceptance.

Appendix B

B.1 Supplementary Material on how to Approximate the Importance of Omitted Variable Bias

We ought to assess how strong the relation to omitted variables would have to be to explain away our estimates of interest from the cross-section and the sibling difference. We follow the approach in Bellows and Miguel (2009) and extend it to the inclusion of fixed effects.

Abstaining from person and time indices, our model can be characterized by

$$Y = \alpha N + \gamma q + \epsilon, \tag{2.7}$$

where Y is the outcome, N is family non-intactness, α is the estimate of interest and q is an unobserved index of characteristics. The unobserved index is a determinant of selection into non-intactness and, if left out of the estimation as in

$$Y = \alpha N + \epsilon, \tag{2.8}$$

leads to standard omitted variable bias in OLS estimates with no control variables as in

$$plim\, \hat{\alpha}_{OLS,no} = \alpha_0 + \gamma \frac{Cov(N,q)}{Var(N)}. \tag{2.9}$$

Then we observe control variables X that are related to the selection index q as in

$$q = X'\beta + \tilde{q}, \tag{2.10}$$

with $\tilde{q}$ being the unobserved component of q. Further assume that controls X are only related to non-intactness N through q, which implies that the control variables are exogenous and that the unobserved components are uncorrelated with controls X. We also assume that the impact of the selection on observable factors is equal to the impact of selection on unobservable factors. Now, we estimate the equation including controls

$$Y = \alpha N + X'\beta + \epsilon. \tag{2.11}$$

The new probability limit of the estimate of interest then is

$$plim\, \hat{\alpha}_{OLS,con} = \alpha_0 + \gamma \frac{Cov(N, \tilde{q})}{Var(N)}. \tag{2.12}$$

By taking the difference of the two estimates we get an the identifiable expression

$$\hat{\alpha}_{OLS,no} - \hat{\alpha}_{OLS,con} = \gamma \frac{Cov(N, X'\beta)}{Var(N)}. \tag{2.13}$$

The included controls X do not perfectly resemble the selection index q. Unobserved heterogeneity in $\tilde{q}$ can still induce bias in the estimate of interest. Without being able to compute bias by unobservables, we would like to assess how strong the covariance between N and $\tilde{q}$ must be to explain away the entire effect found for the estimate of interest after controlling for X. We can express this covariance as a fraction of covariance between N and the control variables. The effect is explained away if $\alpha_0 = 0$. Assuming this yields

$$\frac{\hat{\alpha}_{OLS,con}}{\hat{\alpha}_{OLS,no} - \hat{\alpha}_{OLS,con}} = \frac{Cov(N, \tilde{q})}{Cov(N, X'\beta)}. \tag{2.14}$$

This fraction of estimates from different regressions then denotes how strong the covariance between family non-intactness and unobserved factors $\tilde{q}$ relative to the covariance of family non-intactness N and the control variables has to be to explain away the entire effect. The larger this fraction is, the more unlikely it is that unobserved factors are fully confounding our estimate. This is the Bellows and Miguel (2009) result and we extend it to fixed-effects in the following.

Assume now that unobserved factors $\tilde{q}$ are the sum of mother (or sibling pair) fixed-effects and other unobserved factors that are individual-specific, such that

$$q = X'\beta + \tilde{q} = X'\beta + \eta + \nu, \tag{2.15}$$

with η being the mother fixed-effect and ν other unobserved factors. The fixed-effects model is denoted by

$$Y = \alpha N + X'\beta + \eta + \nu. \tag{2.16}$$

We cannot get consistent estimates of η due to the incidental parameter problem (Neyman and Scott, 1948). As a thought experiment, assume for now that we can get consistent estimates. Then, we would get an estimate of interest according to

$$plim\,\hat{\alpha}_{OLS,\eta,con} = \alpha_0 + \gamma\frac{Cov(N,\nu)}{Var(N)}. \tag{2.17}$$

The difference to a simple estimate without control variables is given by

(2.18)
$$\begin{aligned}\hat{\alpha}_{OLS,no} - \hat{\alpha}_{OLS,\eta,con} &= \gamma\frac{Cov(N,X'\beta) + Cov(N,\eta) + Cov(N,\nu)}{Var(N)} - \gamma\frac{Cov(N,\nu)}{Var(N)} \\ &= \gamma\frac{Cov(N,X'\beta) + Cov(N,\eta)}{Var(N)}.\end{aligned}$$

If we again assume $\alpha_0 = 0$, we get the following expression

$$\frac{\hat{\alpha}_{OLS,\eta,con}}{\hat{\alpha}_{OLS,no} - \hat{\alpha}_{OLS,\eta,con}} = \frac{Cov(N,\nu)}{Cov(N,X'\beta) + Cov(N,\eta)}. \tag{2.19}$$

This fraction tells us how large the covariance between family non-intactness and individual specific unobserved factors has to be relative to the covariance between family non-intactness and observable controls including mother fixed-effects.

As Borghans et al. (2008) show in an appendix, bias in sibling difference models with siblings i and j has to be evaluated by characterizing the estimation equation as

$$Y_i - Y_j = \alpha(N_i - N_j) + (X_i' - X_j')\beta + (\eta - \eta) + (\nu_i - \nu_j). \tag{2.20}$$

We can assume that $Var(N_i) = Var(N_j)$, $Cov(N_i,\nu_j) = Cov(N_j,\nu_i)$ and $Cov(N_i,\nu_i) = Cov(N_j,\nu_j)$. The estimate of interest then has the following property

(2.21)
$$plim\,\hat{\alpha}_{SD,\eta,con} = \alpha_0 + \gamma\frac{Cov(N_i - N_j,(\nu_i - \nu_j))}{Var(N_i - N_j)} = \alpha_0 + \gamma\frac{Cov(N_i,\nu_i) - Cov(N_i,\nu_j)}{Var(N_i) - Cov(N_i,N_j)}.$$

By assuming that

$$\frac{Cov(N_i,\nu_j)}{Cov(N_i,N_j)} = \frac{Cov(N_i,\nu_i)}{Var(N_i)}. \tag{2.22}$$

we get as an approximation the known expression for

$$\frac{\hat{\alpha}_{SD,\eta,con}}{\hat{\alpha}_{OLS,no} - \hat{\alpha}_{SD,\eta,con}} = \frac{Cov(N,\nu)}{Cov(N,X'\beta) + Cov(N,\eta)} = \frac{\hat{\alpha}_{OLS,\eta,con}}{\hat{\alpha}_{OLS,no} - \hat{\alpha}_{OLS,\eta,con}}. \tag{2.23}$$

B.2 Supplementary Tables

Table B.1: Childhood family structure and voting intention
(Cross-sectional and sibling difference estimates)

Dependent variable:	Voting intention	
Regression model:	Cross-section	Sibling difference
Panel A		
Ever lived in a non-intact family	-0.072**	0.041
	(0.022)	(0.066)
Panel B		
Parents divorced	-0.074**	0.094
	(0.025)	(0.090)
Born outside marriage	-0.060+	0.009
	(0.031)	(0.077)
Person-year observations	*5006*	*2648*
Number of sibling-year pairs		*1313*
Birth order FE	*Yes*	*Yes*

Notes: Each column in each panel reports the results of a regression of voting intention. Cross-section figures are marginal effects from probit regressions evaluated at the mean of all covariates. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Sibling difference figures are sibling difference estimates at same survey time from linear fixed-effects models. Robust standard errors in parentheses. Standard errors are clustered on mother's identification number. Other explanatory variables according to the other cross-sectional and sibling difference estimation approaches. The information about *voting intention* is asked in survey years 2005 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

Table B.2: Conditional logit
(Sibling difference estimates)

Dependent variable:	(1) Political interest	(2) Party identification	(3) Organizational involvement	(4) Individual voluntarism
Marginal Effects, assuming fixed effect of zero (Ever lived in a non-intact family)				
Average	-0.037	-0.074	-0.066	-0.120+
	(0.056)	(0.054)	(0.111)	(0.066)
Male				
18 years old	-0.100	-0.053	-0.011	-0.126*
	(0.096)	(0.052)	(0.022)	(0.063)
23 years old	-0.100	-0.060	-0.015	-0.125*
	(0.084)	(0.053)	(0.031)	(0.061)
28 years old	-0.083	-0.071	-0.017	-0.114+
	(0.080)	(0.056)	(0.036)	(0.068)
Female				
18 years old	-0.050	-0.061	-0.009	-0.126*
	(0.080)	(0.056)	(0.019)	(0.061)
23 years old	-0.049	-0.068	-0.013	-0.122+
	(0.066)	(0.056)	(0.028)	(0.063)
28 years old	-0.038	-0.078	-0.014	-0.105
	(0.051)	(0.055)	(0.032)	(0.071)
Person-year observations	*6910*	*6407*	*1246*	*3419*
Number of sibling-year pairs	*3129*	*2916*	*552*	*1531*
Birth order FE	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Sibling difference estimates at same survey time. Estimates from conditional logit models. Conditional logit can only estimate for the subsample of changers in the outcome variable, which reduces sample size. Robust standard errors in parentheses. Standard errors are clustered on mother's identification number, as there are multiple observations for sibling pairs. Other explanatory variables are years of age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

Table B.3: Heterogeneity analysis
(Cross-sectional estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A: By gender					
Women	-0.129**	-0.012	-0.058**	-0.033**	-0.079**
	(0.025)	(0.014)	(0.019)	(0.005)	(0.016)
N	*9117*	*21352*	*20211*	*9704*	*9811*
Men	-0.083**	-0.013	-0.022	-0.016+	-0.096**
	-0.028	(0.021)	(0.021)	(0.009)	(0.019)
N	*9386*	*21563*	*20736*	*9893*	*9935*
Equality of coefficients (p-value)[1)]	*0.44*	*0.87*	*0.15*	*0.01*	*0.71*
Panel B: By mother's education					
Less than high school degree	-0.117**	-0.013	-0.039*	-0.027**	-0.101**
	(0.021)	(0.014)	(0.016)	(0.006)	(0.014)
N	*15274*	*35523*	*33863*	*16326*	*16341*
High school or university degree	-0.055	-0.018	-0.045	-0.025*	-0.037
	(0.041)	(0.033)	(0.032)	(0.012)	(0.029)
N	*3229*	*7381*	*7070*	*3365*	*3412*
Equality of coefficients (p-value)[1)]	*0.30*	*0.81*	*0.81*	*0.77*	*0.03*
Panel C: By residential area					
Rural	-0.133**	-0.002	-0.044+	-0.035**	-0.133**
	(0.035)	(0.022)	(0.026)	(0.010)	(0.028)
N	*4595*	*10648*	*10260*	*4746*	*4899*
Urban	-0.072**	-0.017	-0.032+	-0.016*	-0.048**
	(0.026)	(0.018)	(0.019)	(0.007)	(0.016)
N	*9328*	*21126*	*20457*	*9812*	*9841*
Equality of coefficients (p-value)[1)]	*0.15*	*0.67*	*0.66*	*0.17*	*0.02*

Notes: Each estimate represents a regression result for the outcome listed in that column on *ever lived in a non-intact family*. Rows denote different samples. Figures are marginal effects from OLS [(1)] or probit [(2),(3),(4),(5)] regressions. Probit effects are evaluated at the mean of all covariates. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Other explanatory variables are years of age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, east Germany, indicators of SOEP-samples, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

[1)] Test for equality of coefficients tests whether the coefficients between the two samples can be distinguished statistically in a chow test. Equality of coefficients, not marginal effects, means that the effect would be of equal size had the two sample populations equal characteristics.

Table B.4: Heterogeneity analysis
(Sibling difference estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A: By gender					
Women	-0.034	-0.074	0.022	-0.021	0.001
	(0.082)	(0.060)	(0.084)	(0.033)	(0.077)
N	*4378*	*10230*	*9704*	*4685*	*4685*
Men	-0.293**	-0.070	-0.153**	-0.054+	-0.202*
	(0.091)	(0.052)	(0.052)	(0.028)	(0.080)
N	*4514*	*10383*	*9975*	*4760*	*4763*
Equality of coefficients (p-value)[1)]	*0.03*	*0.96*	*0.08*	*0.44*	*0.07*
Panel B: By mother's education					
Less than high school degree	-0.134*	-0.068+	-0.060	-0.026	-0.065
	(0.056)	(0.039)	(0.042)	(0.021)	(0.045)
N	*7348*	*17120*	*16298*	*7822*	*7825*
High school or university degree	-0.319+	-0.270*	-0.174+	-0.058	-0.166
	(0.194)	(0.128)	(0.102)	(0.050)	(0.134)
N	*1525*	*3442*	*3334*	*1601*	*1601*
Equality of coefficients (p-value)[1)]	*0.30*	*0.08*	*0.27*	*0.54*	*0.47*
Panel C: By residential area					
Rural	-0.173	-0.080	-0.112	-0.037	-0.114
	(0.108)	(0.074)	(0.074)	(0.037)	(0.085)
N	*2245*	*5234*	*5027*	*2380*	*2387*
Urban	-0.088	-0.040	-0.062	-0.024	-0.068
	(0.077)	(0.052)	(0.065)	(0.025)	(0.061)
N	*4143*	*9375*	*9121*	*4338*	*4333*
Equality of coefficients (p-value)[1)]	*0.55*	*0.75*	*0.63*	*0.76*	*0.66*

Notes: Each estimate represents a regression result for the outcome listed in that column on *ever lived in a non-intact family*. Rows denote different samples. Results are sibling difference estimates at same survey time. Estimates from linear fixed-effects models. Robust standard errors in parentheses. Standard errors are clustered on mother's identification number, as there are multiple observations for sibling pairs. Other explanatory variables are years of age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

[1)] Test for equality of coefficients tests whether the estimates for family non-intactness differ between the groups in a regression of a fully interacted model. We report p-values of the interactions with the variable of interest.

Table B.5: Heterogeneity—Age at parents' divorce
(Cross-sectional and sibling difference estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A: Cross-sectional estimates					
Parents divorced at ages:					
0-5 years	-0.133**	-0.027	-0.035	-0.025**	-0.111**
	(0.030)	(0.023)	(0.025)	(0.009)	(0.021)
6-10 years	-0.102**	-0.006	-0.022	-0.029**	-0.102**
	(0.031)	(0.022)	(0.026)	(0.008)	(0.021)
11-16 years	-0.065*	-0.005	-0.010	-0.022**	-0.073**
	(0.029)	(0.023)	(0.025)	(0.007)	(0.020)
Person-year observations	*18503*	*42913*	*40947*	*19738*	*19754*
Panel B: Sibling difference estimates					
Parents divorced at ages:					
0-5 years	-0.178*	-0.001	-0.102+	-0.033+	-0.035
	(0.090)	(0.053)	(0.056)	(0.019)	(0.064)
6-10 years	-0.154+	-0.026	-0.076	-0.042+	-0.076
	(0.086)	(0.070)	(0.066)	(0.024)	(0.075)
11-16 years	-0.138	-0.042	-0.010	-0.040	-0.135*
	(0.087)	(0.057)	(0.063)	(0.025)	(0.062)
Person-year observations	*8873*	*20562*	*19632*	*9423*	*9426*
Number of sibling-year pairs	*4417*	*9739*	*9649*	*4473*	*4474*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. In Panel A, figures are marginal effects from OLS [(1)] or probit [(2),(3),(4),(5)] regressions. Probit effects are evaluated at the mean of all covariates. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. In Panel B, results are sibling difference estimates at same survey time. Estimates from linear fixed-effects models. Robust standard errors in parentheses. Standard errors are clustered on mother's identification number, as there are multiple observations for sibling pairs. Other explanatory variables according to the other cross-sectional and sibling difference estimations. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

Table B.6: Robustness—Siblings more than two years apart
(Sibling difference estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Ever lived in a non-intact family	-0.200**	-0.092*	-0.131**	-0.033+	-0.102*
	(0.060)	(0.042)	(0.034)	(0.020)	(0.044)
Panel B					
Parents divorced	-0.236**	-0.054	-0.123*	-0.058*	-0.140*
	(0.086)	(0.060)	(0.053)	(0.026)	(0.057)
Born outside marriage	-0.153*	-0.138**	-0.113**	-0.010	-0.028
	(0.070)	(0.049)	(0.037)	(0.023)	(0.057)
Equality of coefficients (p-value)[1)]	*0.45*	*0.29*	*0.88*	*0.18*	*0.17*
Person-year observations	*5708*	*13233*	*12676*	*6039*	*6042*
Number of sibling-year pairs	*2702*	*5984*	*5941*	*2729*	*2729*
Birth order FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Results are sibling difference estimates at same survey time. Estimates from linear fixed-effects models. Robust standard errors in parentheses. Standard errors are clustered on mother's identification number, as there are multiple observations for sibling pairs. Other explanatory variables are years of age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

[1)] Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a chow test. We report p-values for the null hypothesis of equal coefficients.

Table B.7: Baseline full estimation table
(Cross-sectional estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Female	-0.196**	-0.164**	-0.071**	-0.026**	-0.060**
	(0.016)	(0.010)	(0.010)	(0.005)	(0.011)
Mother's highest educational attainment					
Intermediate school	0.150**	0.117**	0.077**	0.009**	0.050**
	(0.020)	(0.033)	(0.015)	(0.006)	(0.013)
High School	0.222**	0.127**	0.122**	0.032*	0.015**
	(0.048)	(0.033)	(0.033)	(0.019)	(0.035)
Technical college or university degree	0.314**	0.214**	0.205**	0.042**	0.106**
	(0.026)	(0.020)	(0.019)	(0.010)	(0.019)
Mother's age at birth	0.008**	0.005**	0.005**	0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Only child	-0.044	-0.004	-0.028	-0.007	-0.050**
	(0.028)	(0.018)	(0.019)	(0.009)	(0.018)
Birth order					
Second child	-0.087**	-0.050**	-0.044**	-0.000	-0.022
	(0.020)	(0.012)	(0.014)	(0.006)	(0.013)
Third child	-0.122**	-0.062**	-0.072**	0.004	-0.041*
	(0.030)	(0.017)	(0.020)	(0.009)	(0.019)
East Germany	0.006	0.035	0.048	-0.050*	-0.006
	(0.073)	(0.046)	(0.054)	(0.020)	(0.054)
Mother's employment during childhood years					
Number of years part-time employed	0.003	0.001	0.002	0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Number of years full-time employed	-0.003*	-0.002	-0.000	-0.000	-0.004**
	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)
Ever lived in a non-intact family	0.104**	-0.013	-0.041**	-0.027**	-0.088**
	(0.019)	(0.013)	(0.014)	(0.005)	(0.013)
Person-year obs.	*18503*	*42913*	*40947*	*19738*	*19754*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Figures are marginal effects from OLS [(1)] or probit [(2),(3),(4),(5)] regressions. Probit effects are evaluated at the mean of all covariates. Panel robust standard errors in parentheses. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Other explanatory variables are years of age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, east Germany, indicators of SOEP-samples, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

Chapter 3

Labeling Effects – How Child Benefits Affect Consumption and Savings

3.1 Introduction

A basic principle from microeconomics says: income is treated as fungible. Fungibility of income implies that type and source of income do not affect its use. Put differently, there cannot be compositional effects of income on expenditure. However, this may not always be the case. Indeed, a recent body of work has found contradictory evidence of labeling effects. For example, Abeler and Marklein (2010) show that non-distortionary beverage vouchers increase beverage consumption simply due to the attached label. This means that how much individuals spend on particular goods can be influenced by changing the characteristics of income without changing its amount. It is easy to see the potential benefit for welfare programs and other government interventions. Labeling cash payments is virtually costless and could be used to encourage a desirable use of resources. Countries struggling with low private savings rates may label parts of the income or tax benefits as *savings* cash before applying administratively costly measures. On the other hand, unwittingly labeled payments may distort decisions in an undesirable manner.

A natural candidate for labeling are family policies. In many countries, prominent family assistance measures wear labels related to children (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). One thing these programs have in common is the intention to mitigate financial constraints of families and prevent child poverty. A labeling effect would shift consumption disproportionately towards child goods on top of the pure budgetary effect.

In this paper, we exploit a policy reform to estimate how labeling of child benefits affects child-related consumption and 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 control group of families with one child and apply difference-in-differences estimation methods. Thus, we can eliminate confounding variation over time that is common to all families. The rationale of the estimation is that child benefits should be used just as any other income if there was no labeling effect. Therefore, after we control for total disposable income, the treatment is a change in the composition of income with child benefits taking a larger fraction. Only if labeling effects exist, there should be a reaction in consumption patterns. In the German Income and Expenditure Survey (EVS), we identify one assignable child good and two assignable adult goods in repeated household cross-sections. We also argue that a specific form of building loan contracts can be considered as child-specific savings. We can confirm the plausibility of this assumption based on robustness tests with tenant status and flat size. Potentially, savings are a more important means of child well-being in the long-run compared to contemporaneous consumption. To our knowledge, savings have not been investigated in studies of labeling effects until now.

In the empirical analysis, we find positive effects of the child benefits reform on the child good toys. However, these effects are not robust to the inclusion of important control variables and thus we find no labeling effect on toys. There is no robust evidence of a labeling effect on adult good consumption either. Yet, there is a robust labeling effect of child benefits on child-specific savings. With a treatment of about 1,000 DEM (500 EUR) per year, the probability to hold a building loan contract increases by 4 percentage points and the average value of building loan savings increases by 13 percent due to a labeling effect of child benefits. With respect to the heterogeneity of the effect it turns out that the result for savings is largely driven by better-off households. The results are robust to the inclusion of a number of control variables. Additional tests with placebo expenditure confirm the validity of our estimation methods. The effects seem not to be confounded by intra-household bargaining either, which we test with a particular payment scheme for civil servants. Our difference-in-differences estimation relies heavily on the common trend assumption and the control of household income. We carefully investigate the credibility of the underlying assumptions by including age and income controls as well as applying an alternative relative trend assumption using a difference-in-relative-differences estimation. It seems that the estimated effects are causal, although we can never rule out completely that the treatment and the control group differ systematically over time in expenditure behavior.

The remainder of the paper is structured as follows. We discuss the labeling effects literature in Section 3.2. In Section 3.3, we describe the empirical approach and continue with a description of the data in Section 3.4. We report results and various robustness checks in Section 3.5. We conclude the analysis in Section 3.6.

3.2 Literature on Labeling Effects

The theory of labeling effects has its roots in insights from behavioral economics posited to explain anomalies in observed decision outcomes. According to standard microeconomic theory, rational individuals spend their income according to utility maximization irrespective of the income's sources. There are no compositional effects of income sources on expenditure decisions as income is assumed to be fungible, i.e., all marginal propensities to consume out of 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. A model explaining behavior more accurately is the mental accounts framework by Thaler (1990). The idea is that individuals categorize their income depending on the type and reserve it for special purposes. Transferred to our research question, we may call this a labeling effects framework. It implies that, for example, the labeled child benefits are spend mostly on child goods, whereas other income is distributed to all expenditure. Formally, the marginal propensity to consume child goods out of child benefits is larger than the marginal propensity to consume child goods out of any other income.

Existing empirical literature on labeling effects of child benefits is scarce and somewhat inconclusive. The usual procedure in this literature is to estimate marginal propensities to consume *assignable* goods out of child benefits in comparison to regular income, where assignable goods are mutually exclusively consumed by children or parents. There is some evidence of positive labeling effects of child benefits on consumption of children's clothes in Dutch data (Kooreman, 2000), although no such effect is found in the United Kingdom, where even an unexpected positive effect on adult clothing is found (Blow et al., 2012). Alcohol and tobacco, which are assignable to parents, are found to be positively affected by child benefits in one study (Blow et al., 2012), but not in another (Edmonds, 2002). Overall expenditure on clothing (Edmonds, 2002), and food (Edmonds, 2002; Blow et al., 2012) is found to be unaffected by labeling.

Evidence in the literature is broadly concerned with the same subject matter, but these studies differ substantially in methodology. Kooreman (2000) evaluates the labeling effect of Dutch child benefits by using repeated cross-sections and one change in child benefits between over and under 6 year olds over time 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. In contrast, 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. Also, Edmonds (2002) does not use assignable clothing as a dependent variable, but overall clothing expenditure, which is less suitable in studying labeling effects as the composition of expenditures could be altered in favor of children's clothes. Blow et al. (2012) draws 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. The UK Child Benefit was given to the mother, which makes it difficult to distinguish the child benefit labeling effect from a distributional effect,[1] but the authors overcome this problem by validating their results for lone parents, where there cannot be distributional effects. 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.

[1]Lundberg et al. (1997) shows that targeting child benefits to the mother leads to higher expenditure for children's clothes due to the mother's increased bargaining power.

The literature on labeling effects of child benefits relies mostly on variation over time and thus needs strong assumptions about spending patterns over time to establish causality. Furthermore, evidence is limited to few assignable consumption goods.[2] We ought to fill that void using difference-in-differences techniques with a control group and investigating behavioral consequences for savings as well.

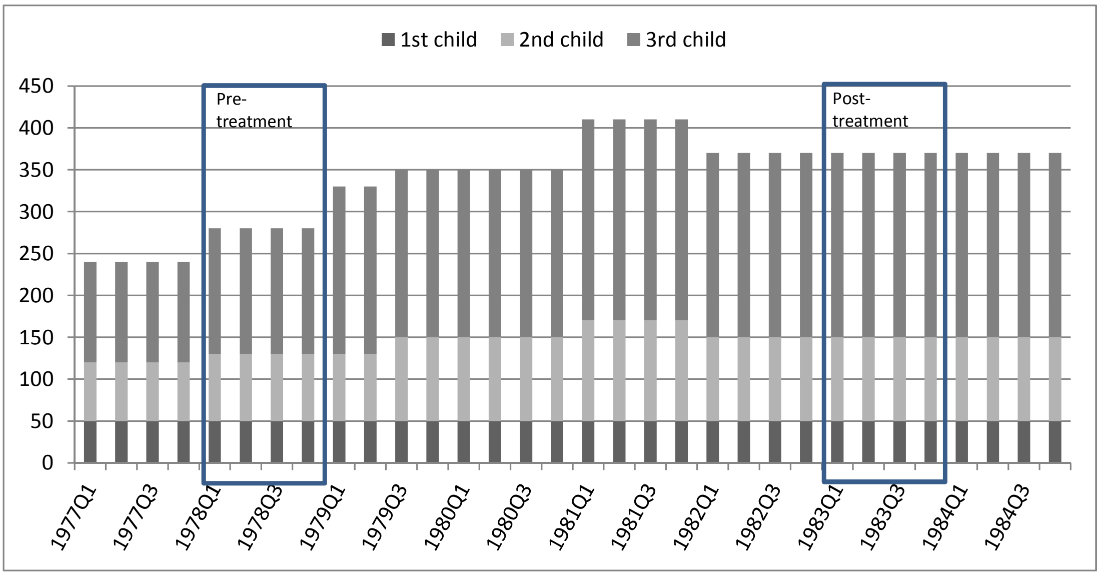
3.3 Empirical Approach

One means of analyzing child benefit effects on consumption is to compare estimated marginal propensities to consume out of child benefits with that out of other income. This kind of estimation can often be cumbersome, as changes in benefits most often increase over time. Then, independent changes over time in the dependent variable can be confounding in estimations. And consumption patterns can easily change over time. Moreover, omitted variable bias problems are present if families' unobserved characteristics change over time, which in turn influences their expenditure pattern. To overcome these endogeneity problems, we employ a difference-in-differences (DiD) model by exploiting a series of child benefit reforms.[3] See Lyssiotou (2009) for a similar approach. Child benefits increased for three-child families while child benefits for one-child families remained unchanged between 1978 and 1983, as shown in Figure 3.1. Therefore, we choose three-child families as the treatment group and one-child families as the control group. This enables us to eliminate all common changes of the treatment and the control group over time. The increase in child benefits for three-child families amounts to about 30 percent while for two-child families the increase is rather modest and is completely absent for one-child families. We discard two-child families in our baseline approach as the increase in their benefits is a rather modest treatment, but it disqualifies them as a control group. Yet, we apply two-child families as a treatment group in the robustness analysis. A detailed description of child benefits according to the rank of the child can be seen in Table 3.1.

[2]In a more general manner, Abeler and Marklein (2010) show in a field and a laboratory experiment, in which they randomly allocate non-distortionary vouchers, that labeling effects influence consumption decisions.

[3]The lump-sum child benefits in Germany are not means tested, paid each month and are available to every family with dependent children at least until 16 years. There are other family benefits that may affect family expenditure and behavior. One of the biggest is child allowances in the income tax system. In the period in question, however, these do not affect the analysis. Child allowances were not present during the period under study, although they were reintroduced in 1983 at a very moderate level. This does not affect our results, as allowances become effective after tax return at the end of a year. As to the institutional details of child benefits and their reform, the increase was unanticipated. Announcement of the reform that introduced our first treatment in 1979 was made in the *Law Gazette* on November 18, 1978, meaning that only very few pre-reform observations could be affected by early announcement.

Figure 3.1: 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 3.1: 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.

Formally, the treatment effect in the DiD model is a combination of two differences. The first difference is the difference in the dependent variable within the groups before and after the treatment. Then, we take the difference in first differences between the treatment group and the control group. This eliminates 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 that no group-specific effects on the outcome variable occur simultaneously with the increase in benefits, i.e., all unobserved changes are valid for both family types and there is no self-selection into treatment.

We use the regression form of the DiD estimator

$$Y_{st} = \alpha_0 + \alpha_1 Treated_s + \alpha_2 Post_t + \delta Treated_s \times Post_t + X_{st}\beta + \epsilon_{st}, \tag{3.1}$$

on expenditure outcomes Y_{st} in family s and period t, where $Treated$ is the treatment group indicator, which is unity for the treatment group and zero for the control group, $Post$ is an indicator for the post-reform period (1983), and the second difference is depicted by the interaction of $Treated$ and $Post$. δ is the estimate of the treatment effect and can be interpreted as an average treatment effect on the treated (ATT). The model yields estimates of a constant (α_0), the baseline difference between treatment and control group (α_1), the common trend over time (α_2), and for the vector of control variables X_{st} (β). ϵ is an i.i.d. error term.

The DiD approach is based on two crucial assumptions. First, the common trend assumption between treatment and control group means that both follow the same trend over time in the absence of a treatment. The implicit assumption is that they both would experience the same absolute change in the dependent variable. However, inherent differences between the treatment and the control group may make an assumption of equal relative changes in percent of the dependent variable just as plausible. As a robustness check, we use an alternative relative trend assumption to check the validity of the common trend assumption. This difference-in-relative-differences model is developed later in the paper. Furthermore, the larger families in the treatment group may react differently to income increases due to economies of scale, which is captured by the baseline difference for the treatment and the control group. Yet, the ATT interpretation of the treatment effect is unaltered by possible economies of scale for the treatment group.

Second, the DiD approach assumes no self-selection into treatment. Take-up is not a concern as applying for child benefits is a one-time effort without explicit costs. However, increases in child benefits could cause a rise in fertility. If families expand to three-child families because of the increase in child benefits, they would change the composition of the groups and violate the standard DiD assumption.[4]

[4]We perform an unreported robustness check by excluding families with children under four years of age to exclude the possibility of self-selection into treatment.

Moreover, there is some ambiguity in the data regarding assignment to the treatment group. We observe whether a household has one, two, three, or more children, but this may not include all the children of the family or even all who are eligible for child benefits. Basically all children up to the age of 16 years are eligible. There are some exceptions though, e.g., older children who are still in school remain eligible unless they earn above a certain income threshold. We alleviate this problem to some extent by excluding from our sample households with children over 16 years. There could still be some error in assignment, however, if one child has moved out but is still eligible. As we only observe children who currently live in the household, we would count too few children per family. If we assign a one-child-family to the control group that actually has another eligible child living outside the household, the stable unit treatment value assumption (SUTVA) would be violated. We cannot fully exclude this possibility as we do not observe individuals outside the household. The assignment error would lead to a downward bias of the estimates if the control group erroneously contained treated families. Wrong assignment due to an unobserved fourth child would yield an upward bias, but is less likely because of the low prevalence of four-child families.

Now we turn to the control variables. Our 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 framework. 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 (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 treatment and control group. Therefore, we interact income with the treatment group indicator variable to account for possible differential income effects between the treatment and the control group.

Other variables correlated with the outcomes could vary between pre- and post-reform periods. Therefore, we include some more background characteristics of the two groups that could violate identifying assumptions. We include age of the parents, as consumption patterns could vary over the life. As female labor force participation changed substantially during the period under study, 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, as well for the urbanization level based on 9 categories. 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.

3.4 Data

For the empirical analysis we employ the German Income and Expenditure Survey (EVS)[5] of 1978 and 1983 as repeated cross-sections. The EVS is a representative survey of currently about 60,000 households that is conducted every five years, starting in 1978.[6] The data include a complete set of expenditure variables at the household level by various groups of goods and a set of income variables. Detailed expenditure is measured over four weeks and less detailed expenditure and income information is collected over the whole year.

To make inferences about labeling effects we need to identify child-specific expenditures. We use *assignable* goods to draw conclusions about the intra-household welfare effect of consumption for individual members of the family. A good is assignable to children if it is to some degree of greater value to children in the household than it is to adults and vice versa. In our analysis, assignable child goods are toys, which are mostly of value to children. Our toy variable is defined as yearly expenditures for toys. Furthermore, we use building loan contracts as a savings measure and argue that it is assignable to children. A peculiarity in German savings allows us to assign building loan contracts to children. These building loan contracts serve the primary purpose of financing construction or buying real estate. They usually have a maturity of more than five years and act as a savings account

[5]Source: Statistisches Bundesamt.

[6]An earlier survey conducted in 1973 has not been digitized and is unavailable.

during the contract period. When terminated, the contract provides the saver with automatic access to low interest building loans at the contract institution. As such, it is a device parents can use to make their children better off in the future when they leave parental home and establish their own families. If the saver does not wish to purchase real estate, the savings including interest can just be withdrawn and used for any purpose. Although we cannot observe whose name is written on the contracts, we assume that most of them are assignable to the saver's children for the following reasons. First, couples with children are unlikely to save for real estate *after* their children are born as their most pressing need for space usually occurs when their family is expanding. Second, by anecdotal evidence it is very common to save for one's children via a building loan contract in Germany and, indeed, major providers offer special building loan contracts for this very purpose. Third, we run some checks on changes in flat size and renting behavior to avoid wrong assignment to children. According to these tests, adults do not use these building loans for own consumption of real estate. Corresponding variables in our regressions are an indicator variable for building loan contracts and a continuous variable for their accumulated value.[7]

As assignable adult goods we use luxury goods and clothing. Luxury goods include jewelry, leather goods, and watches.[8] 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 household members are male. Then, female clothes are adult goods. Accordingly, male clothes are assigned to the father if all children are female.

In the robustness check subsection we use various non-assignable consumption expenditures and savings to test the validity of our approach. These include general savings accounts, life insurance, luxury foodstuffs, restaurant meals, and flat size in square meters.[9]

Means of the dependent variables are reported in Table 3.2 for one- and three-child-families, which are the control and treatment group, respectively. We have 11,385 family-year observations in the control group and 3,125 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

[7]During the analyzed period, a government program intended to encourage building loans savings was instituted. Under this program, 17 percent was added to the money deposited by eligible savers in building loan contracts. In 1982, the fraction was reduced to 14 percent. This program is not an obstacle to our identification if it affected both the treatment and the control group equally. Eligibility criteria for building loan savings subsidies based on income limits and the size of premiums for children were not affected by the reform.

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

[9]We excluded food from the list due to inexplicable totals for items categorized as food in the data. Due to great variability in the data between the two periods, we suspect that there were changes in the methods of collecting data.

Table 3.2: 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				
Toys	21.90	21.55	29.40	35.09
Building loan (1/0)	0.59	0.65	0.64	0.76
Building loan savings	11,784.85	11,974.42	11,113.37	13,561.44
Luxury goods	195.97	225.70	182.41	170.94
Father's clothes	912.37	758.37	771.62	650.73
Mother's clothes	1539.01	1189.01	1512.59	1038.97
General savings (1/0)	0.96	0.95	0.96	0.95
General savings value	14,892.05	10,925.90	14,461.36	9,964.97
Life insurance (1/0)	0.85	0.83	0.88	0.88
Life insurance deposit	1642.21	2061.01	2142.96	2564.41
Luxury foodstuffs	187.88	169.19	179.08	167.07
Restaurant meals	216.65	180.85	199.47	160.75
Square meters	92.64	97.72	115.45	123.03
Control variables				
Age father	38.54	37.90	40.01	38.66
Age mother	35.37	34.82	36.81	35.39
Average child age	8.01	7.43	9.53	8.07
Household net income	63,865.66	64,232.69	72,059.79	74,059.67
Number of earners	1.58	1.63	1.43	1.40
Durable goods	9,168.10	7,813.30	9,164.30	8,033.44
Tenant status (1/0)	0.60	0.54	0.39	0.32
N	5911	5474	1734	1391

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 and average child age.

Deutsche Mark (DEM), which had a nominal exchange rate of 1.95583 to 1 Euro, and are deflated by CPI with base year 1995. Most of the expenditure variables show time trends in the control group which makes it important to use a difference-in-differences strategy to account for unobserved changes over time.

From the group means over time, we can already see how the difference-in-differences results without control variables turn out to be. 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 35.09 DEM, suggesting a treatment effect of about 6 DEM. Prevalence of building loan contracts increases in the control group from 59 to 65 percent, whereas it increases from 64 to 76 percent in the treatment group, suggesting a treatment effect of about 6 percentage points. Values of building loan contracts increase from 11,785 DEM to 11,974 DEM in the control group, but from 11,113 DEM to 13,561 DEM in the treatment group, suggesting a treatment effect of more than 2,200 DEM. Expenditures for adult luxury goods increase in the control group from 196 DEM to 226 DEM, while they decrease in the treatment group from 182 DEM to 170 DEM, suggesting a negative treatment effect of about 42 DEM. Expenditures for the father's clothes (identified only for subgroup with female children) decrease from 912 DEM to 758 DEM in the control group and from 771 DEM to 650 DEM in the treatment group. This yields a small treatment effect of 33 DEM. Expenditures for the mother's clothes (identified only for subgroup with male children) decrease from 1,539 DEM to 1,189 DEM in the control group and from 1,513 DEM to 1,039 DEM in the treatment group. This yields a negative treatment effect of about 120 DEM. In the more sophisticated 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. As a robustness check, we will also discuss results for non-assignable expenditure that is included in the table for the sake of completeness.

We see in Table 3.2 from the means of control variables that the differences between the treatment and control group and differences over time are modest. Age of the fathers averages at 38 to 40 years, age of the mothers at 35 to 37 years. Average child age is a bit larger in the treatment group, however, we use dummy variables as described earlier in the analysis. Household net income increases in the control group from 63,866 DEM to 64,233 DEM, whereas it increases from 72,060 DEM to 74,060 DEM in the treatment group, which makes clear that income controls are important in the analysis. The number of earners increases slightly in the control group and decreases slightly in the treatment group. Expenditures for durable goods decrease from about 9,168 DEM to 7,813 DEM in the control group and from 9,164 DEM to 8,033 DEM in the treatment group. Tenant status, indicating renting of accommodation, declines from 60 to 54 percent in the control group and from 39 to 32 percent in the treatment group. Overall, the comparable figures suggests that the control group is suitable and that compositional changes within groups are moderate.

3.5 Estimation Results

In this section, we investigate whether child benefits affect consumption and savings through a labeling effect. To repeat the rationale of our approach, a labeling effect means that the marginal propensity to consume particular goods out of child benefits is different from the marginal propensity to consume out of regular income. The *child* label attached to the benefit is expected to increase child-specific consumption as it induces a moral obligation for parents to spend that money in the best interest of their child, which may take the form of either immediate consumption expenditure on the child's behalf or investment in a long-term savings account, i.e., a building loan contract.

In the following sections, we report the results of the DiD estimations. We begin with baseline results with three- and one-child families as treatment and control group and then continue with a heterogeneous treatment effect analysis. We finish by conducting a number of robustness checks to investigate the validity of various underlying assumptions.

3.5.1 Baseline Estimation Results

Table 3.3 shows the results of a DiD estimation without control variables for different consumption expenditures and savings. Results are from OLS regressions and can be interpreted as marginal effects. We use robust standard errors to account for heteroscedasticity. The DiD coefficient for the $Treated \times Post$ term is the treatment effect. We also report the treatment effect as a percentage change in the last row of the table. The percentage change is calculated as the treatment effect over the sum of the pre-reform period value of the dependent variable and the counterfactual change from the control group. Estimations for parents' clothes use fewer observations because we can only distinguish male and female clothing expenditures. Thus, father's clothes are measured only for households in which all other members are female and vice versa for mother's clothes.

Expenditure on toys in the treatment group increases by 6.04 DEM per year compared to the control group of one-child families. This is a substantial increase of 21 percent in expenditure on toys. According to the post-reform indicator estimate, expenditures do not increase significantly purely due to the passage of time, i.e., consumption in the control group in 1983 is similar to consumption in 1978. The baseline difference between the groups is substantial. Treatment group families consume 7.50 DEM per year more than the control group.

Table 3.3: Baseline DiD regression

	Toys	Building loan (1/0)	Building loan savings	Luxury goods	Father's clothes	Mother's clothes
Treated× *Post*	6.035**	0.065***	2,258.495***	-41.196	33.103	-123.613
	(2.834)	(0.019)	(821.704)	(31.097)	(62.445)	(116.542)
Treated	7.502***	0.046***	-671.479	-13.564	-140.743***	-26.428
	(1.837)	(0.013)	(556.947)	(28.314)	(42.882)	(99.844)
Post	-0.346	0.055***	189.573	29.729***	-153.994***	-349.998***
	(1.019)	(0.009)	(385.896)	(11.433)	(19.226)	(29.618)
Additional controls	No	No	No	No	No	No
Observations	14510	14510	14509	14510	5970	6290
R-squared	0.006	0.010	0.001	0.001	0.013	0.024
Treat. eff. in %	21%**	9%***	20%***	-19%	5%	-11%

Notes: The set of additional control variables comprises the following variables. Age composition controls are indicator variables for each year of age of the oldest child and each age indicator is interacted with the treatment group indicator. Household income controls include full household income including child benefits and is interacted with the treatment group indicator. Additional control variables are age of the parents, household net income squared, the number of earners, indicator variables for the regional urbanization level, the amount spent on durable goods in the particular year, an indicator variable for renting accommodation, and indicator variables for the federal states. Results are estimates from OLS regressions, $Treated \times Post$ denotes the treatment effect in a difference-in-difference model. Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% level.

In the savings equations we find positive and significant effects of the child benefit reform: the probability of having a building loan contract increases by 6.5 percentage points due to the reform and the value of these contracts increases by 2,258 DEM. This corresponds to an increase in the propensity to save via a building loan contract by 9 percent and an increase in the value of 20 percent. A back-of-the-envelope calculation emphasizes the significance of this result. Summing up the additional child benefits compared to the base year 1978 for four consecutive years yields nominally 1,144 DEM for the second child and 4,420 DEM for the third child. Of the additional child benefits due to the reform, corrected for CPI, about one third on average is saved in building loans. The baseline difference between treatment and control group, denoted by the coefficient for $Treated$, is plus 4.6 percentage points in probability of having a building loan account. There is no statistically significant difference in the value of building loan contracts.

Our assignable adult goods are luxury goods, father's clothes, and mother's clothes. Treatment effect estimates on luxury goods and clothes expenditures by fathers and mothers show high standard errors and are not statistically significant. The effect on mother's clothes is large and negative and on father's clothes positive but small. Thus, the increase in child benefits does not translate into significant changes in adult expenditures according to our estimates.

Results with all additional covariates including the crucial income controls are reported in Table 3.4. Results from this regression are more reliable, as the income controls eliminate the direct income effect of the child benefits reform. The treatment effect on the child good toys decreases substantially to 2.29 DEM and becomes statistically insignificant. Although the point estimate is still positive, we cannot conclude that there are labeling effects of child benefits on toys.

Our previous results for savings, however, are robust to the inclusion of control variables. We continue to find positive and significant effects on building loan savings. Both estimates are considerably smaller, but the overall picture is persistent. Prevalence of building loan contracts increases by 4.1 percentage points, significant at the five percent level. This corresponds to an increase of 6 percent in the probability of saving. The value of building loans increases by 1.428 DEM due to the reform, significant at the ten percent level. This corresponds to an increase in the average value of 13 percent. In sum, we find robust labeling effects of child benefits on child-specific savings, but not on consumption. As before, none of the adult goods is significantly affected.[10]

[10]The results largely hold when excluding all families with children under the age of four that could have self-selected into the treatment group as a response to the higher child benefits for higher-order children. This possible selection bias is overcome if we consider only families that could not have reacted to the reform with increased fertility. Assuming a one year conception and gestation lag, a child as a result of the reform would be born at the earliest in the beginning of 1980 and would be three years old in our post-reform period 1983. The coefficients for savings are similar, but the standard errors increase and the results become insignificant in the specification

Table 3.4: Baseline DiD regression with full set of controls

	Toys	Building loan (1/0)	Building loan savings	Luxury goods	Father's clothes	Mother's clothes
$Treated \times$ $Post$	2.299	0.041**	1,428.106*	-57.260	42.722	-142.504
	(2.659)	(0.018)	(791.161)	(35.534)	(58.640)	(110.043)
$Treated$	-8.841	-0.039	-1,577.507	-104.618	114.354	-505.332*
	(2.659)	(0.018)	(791.161)	(35.534)	(140.349)	(273.726)
$Post$	-1.321	0.062***	171.849	45.887***	-133.665***	-311.242***
	(1.152)	(0.009)	(402.973)	(13.963)	(19.592)	(30.072)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14510	14510	14509	14510	5970	6290
R-squared	0.046	0.142	0.113	0.052	0.163	0.198
Treat. eff. in %	8%	6%**	13%*	-27%	7%	-12%

Notes: The set of additional control variables comprises the following variables. Age composition controls are indicator variables for each year of age of the oldest child and each age indicator is interacted with the treatment group indicator. Household income controls include full household income including child benefits and is interacted with the treatment group indicator. Additional control variables are age of the parents, household net income squared, the number of earners, indicator variables for the regional urbanization level, the amount spent on durable goods in the particular year, an indicator variable for renting accommodation, and indicator variables for the federal states. Results are estimates from OLS regressions, $Treated \times Post$ denotes the treatment effect in a difference-in-difference model. Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% level.

3.5.2 Income Heterogeneity of Treatment Effects

A goal 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 in above- and below-median income families. The treatment effect interaction (of the treatment group indicator with the post-reform indicator) is again interacted with a low-income indicator to reflect heterogeneity. Table 3.5 shows the results of a triple interaction with the indicator variable *lowincome*, which is set to unity for families with below-median income and zero otherwise. The treatment effect for low income families is then the sum of the triple interaction and the treatment effect interaction $Treated \times Post$. The treatment effect for high income families is $Treated \times Post$, because the triple interaction equals zero for high income households.

Results in Table 3.5 with the full set of control variables depict the treatment effect for high income households in the first row and for low income households in the second row. The treatment effect on toys is again positive but not statistically significant for both groups. No significant effect is found for adult goods either. Compared to the baseline result, the treatment effect on the probability of having a building loan contract increases to 6.0 percentage points for high income households. The estimate is highly statistically significant. On the contrary, for low income households we find a smaller effect of 1.2 percentage points which is not statistically significant. Furthermore, we find the same pattern for the value of building loan savings. For high income households we find a significant treatment effect of 2,017 DEM, whereas for low income household the estimate is 443 DEM and not significant. Although the treatment effects between high and low income families are not statistically significantly different from each other, we may conclude that the labeling effect on savings is only present for above-median households. Saving for the future expense of a home is surely beneficial for children and could make it much easier for them to embark on a career away from parental home. Housing is also believed to be a prerequisite, or at least an important factor, for starting a family. But as this effect is absent for below-median income families, one of the goals of providing child benefits, that is, reducing child poverty, seems not to be fostered by a labeling effect.

3.5.3 Robustness and Specification Checks

To validate our findings and test crucial assumptions of the DiD approach, we run a number of robustness and specification checks. We begin with estimations on non-assignable consumption and then use alternative treatment group specifications to test the suitability of our model. Furthermore, we allow for relative trends in

with control variables, possibly due to the smaller sample size. Result tables are omitted here and available upon request.

Table 3.5: DiD–Income heterogeneity

	Toys	Building loan (1/0)	Building loan savings	Luxury goods	Father's clothes	Mother's clothes
Treatment effect for high income households[a)]	1.701	0.060***	2,017.062*	-68.541	127.339	-200.250
	(3.843)	(0.022)	(1,198.800)	(50.881)	(82.733)	(167.190)
Treatment effect for income households [p-val][b)]	3.283	0.012	443.6	-18.85	-67.08	58.00
	[0.397]	[0.697]	[0.596]	[0.297]	[0.380]	[0.551]
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14510	14510	14509	14510	5970	6290
R-squared	0.046	0.143	0.114	0.053	0.164	0.202

Notes: [a)] These are results from an interacted model. The treatment effect $Treated \times Post$ is interacted with the indicator variable for low income households. The treatment effect for high income households depicted here is the base effect of the standard interaction $Treated \times Post$.
[b)] These are results from an interacted model. The treatment effect $Treated \times Post$ is interacted with the indicator variable for low income households. The treatment effect for low income households depicted here is the base effect of the standard interaction $Treated \times Post$ plus the interacted effect with the low income household indicator variable. In [] we report p-values of the hypothesis test of a zero treatment effect for low income households.
The set of additional control variables comprises the following variables. Age composition controls are indicator variables for each year of age of the oldest child and each age indicator is interacted with the treatment group indicator. Household income controls include full household income including child benefits and is interacted with the treatment group indicator. Additional control variables are age of the parents, household net income squared, the number of earners, indicator variables for the regional urbanization level, the amount spent on durable goods in the particular year, an indicator variable for renting accommodation, and indicator variables for the federal states. Results are estimates from OLS regressions. Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% level.

a difference-in-relative-differences estimation. Finally, we discuss the possibility of distributional confounders and make an attempt to test for it.

Non-assignable expenditure We cannot conduct usual placebo tests, which are used to confirm the common trend assumption in non-treated periods, as we cannot observe the data in earlier periods. However, we can estimate our model for consumption and purchase of certain household public goods that are not particularly adult or child specific. There should not be a strong response in these outcomes if our assumptions of common trends are true. As we assume that non-assignable consumption should not react to the child benefit label, we basically test whether our model is prone to biases from different trends for the treatment and the control group. Yet, it is hard to find any consumption that is surely non-assignable at all. Some of the variables we use are capable of increasing family's welfare and are also of interest to policy makers. We investigate two different savings accounts, general savings at banks and life insurance, which most likely serve as income smoothing instruments or insurance for the partner against income default. We use luxury foodstuffs as an additional consumption measure, which could be interpreted as adult assignable or as a proxy for food quality. Moreover, we observe food consumption out of the house, which is generally not assignable to adults or children.

To test whether the results for building loan contract savings are indeed assignable to children, we consider housing arrangements as dependent variables. First, we use the probability of renting a flat or a house in contrast to owning it as the dependent variable, depicted by $Tenant(1/0)$. If there is an effect on the tenant status, our building loan equations could be biased. We would expect the contracts to be terminated if someone buys or builds real estate. Second, we use the size of the flat in square meters as a dependent variable for the same reason. Moreover, flat size is an indicator of well-being for the whole family.

Table 3.6 reports the results for alternative dependent variables. Savings which are not attributable to a specific child-related purpose, such as general savings at banks or life insurance, are not affected by the reform. All treatment effect estimates from both estimations without and with control variables are not statistically significant. Neither the probability of having an account nor the value of savings or deposits show a reaction to the treatment. If anything the coefficients are negative. Thus, the treatment group does not disproportionately increase savings in general. Our results for building loans as a child-specific savings rate are therefore not explained by an uncontrolled trend towards higher savings. Estimates of a labeling effect on child-specific savings seem reliable. Turning to placebo consumption, luxury foodstuffs and meals out of the house are not significantly affected according to Table 3.6.

The probability of renting a house or flat is not affected either. Although, for the size of the home there seems to be a small positive change in the first row,

Table 3.6: Non-assignable expenditure

	General savings (1/0)	General savings value	Life insurance (1/0)	Life insurance deposit	Luxury foodstuffs	Restaurant meals	Tenant (1/0)	Flat size (m^2)
no covariates								
$Treated\times$ $Post$	-0.005 (0.009)	-530.241 (679.297)	0.013 (0.014)	2.644 (125.534)	6.688 (6.204)	-2.922 (10.012)	-0.011 (0.020)	2.498* (1.480)
Additional controls	No	No	No	No	No	No	No	No
Treat. eff. in %	-1%	-5%	2%	0%	4%	-2%	-3%	2%*
full model								
$Treated\times$ $Post$	-0.010 (0.009)	-760.315 (675.492)	0.010 (0.014)	-125.817 (125.660)	5.388 (6.333)	-8.450 (10.130)	0.005 (0.017)	0.026 (1.304)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treat. eff. in %	-1%	-7%	1%	-5%	3%	-5%	2%	0%

Notes: General savings (1/0) equals one if the household has a general savings account at a bank and is zero otherwise. General savings value measures the accumulated value of these accounts. Life insurance (1/0) equals one if a household member has a life insurance and is zero otherwise. Life insurance deposits measure the amount that is paid in during a year. Luxury foodstuffs and restaurant meals denote expenditure for the two. Tenant (1/0) equals one if the household rents accommodation and is zero otherwise. Flat size is the size of the accommodation in square meters. The set of additional control variables comprises the following variables. Age composition controls are indicator variables for each year of age of the oldest child and each age indicator is interacted with the treatment group indicator. Household income controls include full household income including child benefits and is interacted with the treatment group indicator. Additional control variables are age of the parents, household net income squared, the number of earners, indicator variables for the regional urbanization level, the amount spent on durable goods in the particular year, an indicator variable for renting accommodation, and indicator variables for the federal states. The renting indicator is excluded if tenant status is the dependent variable. Results are estimates from OLS regressions, $Treated \times Post$ denotes the treatment effect in a difference-in-difference model. Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% level.

but it is not robust to the inclusion of control variables. If building loans were not primarily savings for children, we would expect many of them to be terminated and invested in housing in these families. That effect does not seem to play a role, which supports our assumption that building loans are child-specific.

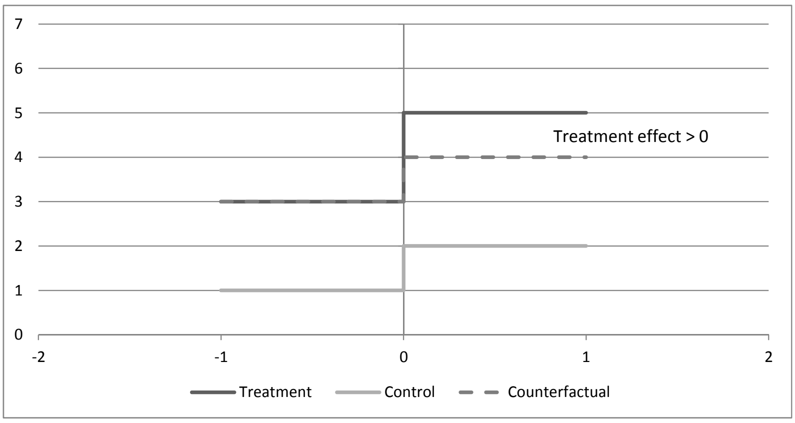
Alternative treatment group specifications The overall validity of a regression model can be checked by placebo tests that assume the same identification although there is no treatment. As noted earlier, we cannot undertake placebo tests in the usual manner by choosing a different point in time with the same treatment and control group. As a second alternative, we instead run a regression using one-child families as the treatment group and childless couples as the control group. The treatment group *receives* a placebo as there was no change in child benefits for one-child families. If we find significant effects here, there might be other unobservable factors explaining our results.

The results on toys and child-specific savings with the placebo treatment group are not significant and thus confirm the suitability of our model and our control group. This setting may not be an ideal case for toys if the control group is childless, but it should be informative for savings. The placebo results for adult goods show positive effects on luxury goods and negative effects on adult clothes. This seems to be a compositional change in spending that occurs in families. Results for the adult goods should therefore be interpreted with caution. As we do not find labeling effect on adult goods in our baseline specification, it does not affect the interpretation of the main findings. A table containing these results can be found in the Appendix in Table C.1.

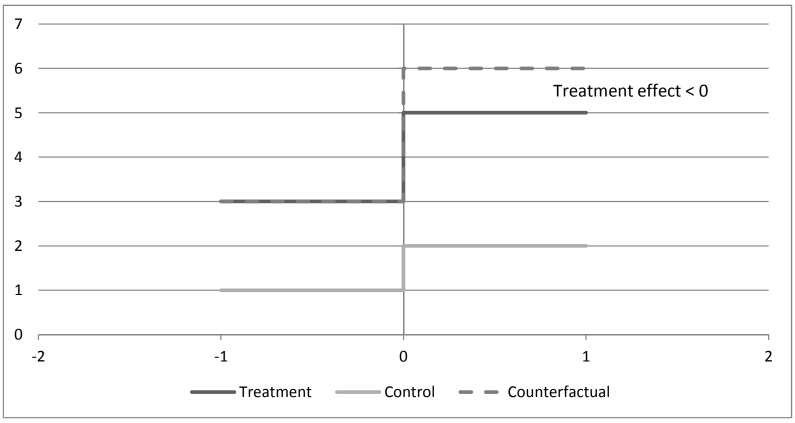
As another check of the validity of our results, we run tests using two-child families instead of three-child families as the treatment group. As expected, results point in the same direction as previously found for child-specific savings but are smaller than for three-child families, which is plausible as these families received a smaller treatment than the three-child families. The results are set out in the Appendix in Table C.2.

Difference-in-relative-differences estimation approach The DiD approach estimates deviations from the control group trend in levels. 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 with the treatment. However, the effect on relative deviations might be the more suitable choice if there is reason to believe that a proportional common change for both groups coincides. For example, this could be the case in the event of relative price changes in the dependent variable different from overall inflation. If treatment and control group have different levels of consumption, a proportional measure such as price would have a proportional effect on the outcome instead of a constant difference. More generally, imagine a member of the control group buys one piece and a treatment group member buys two pieces prior to the treatment. What is the correct counterfactual if the control group individual buys two pieces post treatment? The unobserved shift we want to eliminate could be that people just buy one more piece, which makes the counterfactual three pieces, or that people double their purchases, which makes the counterfactual four pieces. A DiD model in relative terms, a difference-in-relative-differences (DiRD) model, which captures the relative changes in the outcome variable, will capture this latter kind of change.

Figure 3.2: Hypothetical counterfactuals of DiD and DiRD models



(a) DiD



(b) DiRD

Notes: Treatment denotes the treatment group and control denotes the control group. Time is on the horizontal axis, with a hypothetical treatment at 0. The outcome is on the vertical axis. The counterfactual for the treatment group in absence of a treatment in panel (a) is computed according to an absolute difference assumption, whereas the counterfactual in panel (b) is computed according to a relative trend assumption.

In the DiRD model, the common trend assumption is altered. The counterfactual case—the dependent variable's change in the treatment group without the treatment—is no longer an equal absolute change compared to the control group but an equal relative change in the dependent variable. This means that common trends are weighted by the observed level in the base year or, put differently, are re-scaled such that the base-year level is the same across groups.[11] In the standard DiD approach, 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 for the treatment group. In contrast, in the DiRD, a one percent change in the control group's dependent variable must equal a one percent 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 3.2.

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

$$\frac{\alpha_2 + \delta}{\alpha_0 + \alpha_1} - \frac{\alpha_2}{\alpha_0}. \tag{3.2}$$

See the Appendix for the derivation. To our knowledge, Gregg et al. (2009) is the only study that uses a comparable alteration of the trend assumption.

The results for the monetary dependent variables from the DiD estimation without covariates also hold for a DiRD estimation, as can be seen in Table 3.7. The effects show the same sign and are statistically significant for toys and child assignable savings as in the baseline estimation without covariates. Toy expenditure and building loan savings increase by about 20 percent if we assume a common relative trend. None of the adult expenditures is significantly affected. Unobserved relative price changes and related reasons thus do not seem to be threats to our identification. Hence, the common trend assumption in the DiD appears to be valid. At the very least, these results mean that the assumption of a common absolute trend would not change the sign and significance of our results if the real trend was relative.

Distributional confounders A frequent concern in all studies of child benefits of this kind is that they could alternatively be explained by bargaining power shifts. This means that instead of simply imposing a label on some part of a household's income, child benefits change relative income within a couple and thus the bargaining power. As Lundberg et al. (1997) show, directing child benefits to mothers changes bargaining power within households and leads to higher consumption of children's

[11] A related approach is taken by Athey and Imbens (2006) in the scale-independent changes-in-changes (CIC) model. They generalize the DiD model by allowing for arbitrarily different distributions of unobservables U_i across groups, which enter an index function $h(U_i, T_i)$ that describes the outcome Y_i with T as a time indicator. As such, all differences between the distributions across groups are attributed to different distributions of unobservables. The CIC model, by exploiting the whole distribution of the outcomes and not only the first moments, in contrast to the standard DiD model, is invariant to the scaling of the outcome as long as the transformation is strictly monotonic.

Table 3.7: Difference-in-relative-differences regression results

	Toys	Building loan savings	Luxury goods	Father's clothes	Mother's clothes
Treated× *Post*	0.209* [3.79]	0.204** [6.64]	-0.214 [1.75]	0.012 [0.03]	-0.086 [2.01]
Additional controls	No	No	No	No	No
Observations	14510	14509	14510	5970	6290

Notes: Treated × *Post* denotes the treatment effect from a difference-in-relative-differences model obtained in OLS regressions. F-statistic of non-linear hypothesis test (coeff=0) in []. * significant at 10%; ** significant at 5%; *** significant at 1% level. p<0.1.

and female clothing. This distributional effect is not present per se in the German benefit system, as, in contrast to the United Kingdom, child benefits were not necessarily directed to mothers. Although, there are caveats with our approach. In a static framework, child benefits are family income and do not affect relative income as a distribution factor within a couple. The spouse not receiving the benefit should be fully compensated through cash transfers by the benefit recipient if they are free to choose who receives the benefit. No partner could credibly exclude the other from receiving child benefit. In a dynamic framework, one could argue that the decision about the recipient of child benefits in the past is not augmented in the face of a reform. Contracts about compensation are incomplete such that unexpected changes in benefits do in turn alter bargaining power. Assuming that women have a higher preference for child well-being, as in Lundberg et al. (1997), a fraction of mothers receiving the benefits other than 0.5 would induce bias. Most likely fathers as the families' main earners are also more often the recipients of child benefits, and then our estimates would be downward biased or in favor of the father's preferences. Moreover, in a divorce-threat model, in which outside options shape bargaining power, the mother as the likely custodial parent after a divorce would also be the recipient of child benefits and thus gain bargaining power due to the reform. Induced bias would then be towards the mother's preferences. Ultimately, we cannot fully exclude the possibility of distributional consequences of the child benefit reform, but we would argue that these are much less severe than in the case of the British *wallet-to-purse* reform studied in Lundberg et al. (1997).

In practice, it might even be enough that one parent receives child benefits to her bank account to induce some sort of bargaining power shift. The recipient of child benefits is unobservable in our data, but we have information about respondents' occupations. By administrative rules civil servants are required to receive child benefits from their employer, which we exploit to investigate distributional effects. If the father is a civil servant, we can be sure that in those families a higher fraction of fathers receives child benefits than in the average family, in which at least some fraction of mothers are recipients. In Table 3.8 we show results of interactions of an indicator variable for the father being a civil servant with our baseline DiD approach. If bargaining effects are mostly driving our results we should see a significantly different result for families with a father who is a civil servant and gains bargaining

Table 3.8: DiD–Distributional effects for civil servants

	Toys	Building loan (1/0)	Building loan savings	Luxury goods	Father's clothes	Mother's clothes
$Treated \times$	1.619	0.035*	1,027.030	-66.454	35.192	-97.793
$Post$	(3.029)	(0.021)	(825.905)	(42.257)	(69.528)	(123.139)
$Treated \times$	3.501	0.018	1,595.911	44.298	85.217	-161.685
$Post \times CivilServant$	(6.730)	(0.039)	(2,223.250)	(59.555)	(127.769)	(234.876)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Treatment effect for	5.120	0.053	2623	-22.16	120.4	-259.5
civil servants households [p-value]	[0.391]	[0.113]	[0.205]	[0.606]	[0.266]	[0.210]
Observations	14510	14510	14509	14510	5970	6290
R-squared	0.046	0.143	0.114	0.053	0.164	0.202

Notes: Results are from an interacted model with an indicator variable equal to one if the father is a civil servant. The treatment effect $Treated \times Post$ is interacted with the indicator variable. This triple interaction shows whether expenditure in families with male civil servants reacts differently from the others. The treatment effect for families with male civil servants is shown in the bottom of the table and comprises the base effect of the standard interaction $Treated \times Post$ plus the interacted effect with the civil servant indicator variable. In [] we report p-values of the hypothesis test of a zero treatment effect for low income households.

The set of additional control variables comprises the following variables. Age composition controls are indicator variables for each year of age of the oldest child and each age indicator is interacted with the treatment group indicator. Household income controls include full household income including child benefits and is interacted with the treatment group indicator. Additional control variables are age of the parents, household net income squared, the number of earners, indicator variables for the regional urbanization level, the amount spent on durable goods in the particular year, an indicator variable for renting accommodation, and indicator variables for the federal states. Results are estimates from OLS regressions, $Treated \times Post$ denotes the treatment effect in a difference-in-difference model. Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% level.

power due to the reform. The treatment effects for the non-civil-servant families have the same sign as in the regular DiD setting but are a bit smaller. This is in line with the income heterogeneity we found, because civil servants are mostly among the above-median income families. However, only the effect on holding a building loan contract is statistically significant. The treatment effect on the value of building loan savings loses significance compared to the baseline result. None of the interactions with civil servant fathers significantly differs from the treatment effect found above. Based on the earlier research, we would expect a negative effect on child goods if the mother loses influence in the decision making. Hence, there is no indication that an increase in bargaining power for this particular group due to receiving the child benefits is confounding the treatment effect attributed to labeling. This is far from an ideal test of bargaining hypotheses. Yet, we have some indication that labeling is a substantial contributor in our approach.

3.6 Conclusion

We exploit a child benefit reform in Germany that substantially increased the amount of the transfer received by three-child families, but left the benefits for one-child families unchanged. Using data from German Income and Expenditure Surveys and applying a difference-in-differences approach, we confirm the existence of a labeling effect that favors child expenditures out of child benefit income. In particular, we find strong effects of child benefit labels on savings for children and weaker evidence for short-term consumption which is not robust to the inclusion of control variables. Instead of short-lived goods, which may be in sufficient supply for the families, parents invest in their children via long-term savings.

The literature has been rather inconclusive on the labeling effects of child benefits. Especially, evidence exploiting reforms is scarce and, thus, there are few reliable estimates of labeling effects. This paper contributes estimates that exploit a child benefit reform. Furthermore, results are less flawed by effects of intra-household distributions than some of the earlier evidence. This paper also introduces a new object of study to the literature: effects on child-assignable savings. The result is particularly interesting as long-term investments in children could be seen as favorable over short-term consumption from a welfare perspective. However, our result casts some doubt on the intended purposes of child benefits, as the effect on savings is largely driven by better-off households.

Finally, for policy makers, labeling effects are an important field of study. Even if the effects of labeling are small, it is a very cost-effective measure and can be easily applied to, e.g., existing welfare programs.

Appendix C

C.1 Supplementary Tables

Table C.1: Placebo treatment–One-child families as treatment group, childless couples as control group

	Toys	Building loan (1/0)	Building loan savings	Luxury goods	Father's clothes	Mother's clothes
Treated× *Post*	-0.112	-0.001	-289.8	33.883**	-34.802*	-62.823*
	(1.125)	(0.010)	(418.1)	(16.383)	(20.389)	(33.485)
Treated	4.115**	0.086***	-4,166.2***	87.469**	-104.814**	-24.742
	(1.602)	(0.018)	(1,099.8)	(41.879)	(43.322)	(72.286)
Post	-0.222	0.058***	677.5***	6.996	-108.929***	-274.606***
	(0.554)	(0.006)	(238.9)	(12.425)	(10.701)	(21.518)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34895	34895	34892	34895	29074	29331
R-squared	0.031	0.247	0.135	0.075	0.161	0.168
Treat. eff. in %	0%	0%	-3%	16%**	-6%*	-5%*

Notes: The set of additional control variables comprises the following variables. Age composition controls are indicator variables for each year of age of the oldest child and each age indicator is interacted with the treatment group indicator. Household income controls include full household income including child benefits and is interacted with the treatment group indicator. Additional control variables are age of the parents, household net income squared, the number of earners, indicator variables for the regional urbanization level, the amount spent on durable goods in the particular year, an indicator variable for renting accommodation, and indicator variables for the federal states. Results are estimates from OLS regressions, $Treated \times Post$ denotes the treatment effect in a difference-in-difference model. Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table C.2: Alternative treatment group—Two-child families as treatment group, one-child families as control group

	Toys	Building loan (1/0)	Building loan savings	Luxury goods	Father's clothes	Mother's clothes
Treated× *Post*	-1.640 (1.608)	0.017 (0.012)	1,166.2** (554.3)	-3.002 (14.527)	10.969 (30.111)	1.751 (48.972)
Treated	-1.199 (3.120)	-0.005 (0.029)	-4,295.2** (1,722.8)	8.864 (44.825)	-72.510 (78.675)	-280.543* (162.469)
Post	-1.120 (1.103)	0.056*** (0.009)	1.8 (394.9)	37.077*** (12.493)	-137.656*** (19.068)	-313.704*** (29.510)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23857	23857	23857	23857	8505	9002
R-squared	0.042	0.132	0.102	0.067	0.170	0.197
Treat. eff. in %	-6%	2%	10%*	-1%	2%	0%

Notes: The set of additional control variables comprises the following variables. Age composition controls are indicator variables for each year of age of the oldest child and each age indicator is interacted with the treatment group indicator. Household income controls include full household income including child benefits and is interacted with the treatment group indicator. Additional control variables are age of the parents, household net income squared, the number of earners, indicator variables for the regional urbanization level, the amount spent on durable goods in the particular year, an indicator variable for renting accommodation, and indicator variables for the federal states. Results are estimates from OLS regressions, $Treated \times Post$ denotes the treatment effect in a difference-in-difference model. Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% level.

C.2 Derivation of DiRD Estimator

The difference-in-relative-differences estimator is derived as follows, notation as in Angrist and Pischke (2009).

The control group's relative trend is described by

$$\frac{E[Y_{ist}|s=C,t=1983]-E[Y_{ist}|s=C,t=1978]}{E[Y_{ist}|s=C,t=1978]}, \tag{3.3}$$

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

$$\frac{(\gamma_C+\lambda_{1983}+\delta(0))-(\gamma_C+\lambda_{1978})}{\gamma_C+\lambda_{1978}}. \tag{3.4}$$

This reduces to

$$\frac{\lambda_{1983}-\lambda_{1978}}{\gamma_C+\lambda_{1978}}. \tag{3.5}$$

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

$$\frac{E[Y_{ist}|s=T,t=1983]-E[Y_{ist}|s=T,t=1978]}{E[Y_{ist}|s=T,t=1978]}, \tag{3.6}$$

and

$$\frac{(\gamma_T+\lambda_{1983}+\delta(1))-(\gamma_T+\lambda_{1978})}{\gamma_T+\lambda_{1978}}, \tag{3.7}$$

and

$$\frac{\lambda_{1983}-\lambda_{1978}+\delta}{\gamma_T+\lambda_{1978}}. \tag{3.8}$$

The population DiRD then is

$$\frac{\lambda_{1983}-\lambda_{1978}+\delta}{\gamma_T+\lambda_{1978}}-\frac{\lambda_{1983}-\lambda_{1978}}{\gamma_C+\lambda_{1978}}. \tag{3.9}$$

When we use the standard population DiD regression

$$Y_{ist}=\alpha+\gamma_s+\lambda_t+\delta D_{st}+\epsilon st \tag{3.10}$$

the regressors express

$$\begin{aligned}\alpha =&\gamma_C+\lambda_{1978}\\ \gamma =&\gamma_T-\gamma_C\\ \lambda =&\lambda_{1983}-\lambda_{1978}.\end{aligned} \tag{3.11}$$

Using the regressors' notation, we can simplify the population DiRD equation as

$$\frac{\lambda + \delta}{\gamma_T + \alpha - \gamma_C} - \frac{\lambda}{\alpha} \tag{3.12}$$

and equally so as

$$\frac{\lambda + \delta}{\alpha + \gamma} - \frac{\lambda}{\alpha}. \tag{3.13}$$

In the notation of our DiD model in equation 3.1, the DiRD estimator is represented by

$$\frac{\alpha_2 + \delta}{\alpha_0 + \alpha_1} - \frac{\alpha_2}{\alpha_0}. \tag{3.14}$$

Chapter 4

Does Expanding Public Child Care Encourage Fertility? County-Level Evidence from Germany[1]

[1]Joint with Stefan Bauernschuster and Helmut Rainer

4.1 Introduction

Over the past few decades, most European countries have entered uncharted demographic territory characterized by below-replacement fertility. At present, demographers even argue that some countries are locked in a “low fertility trap”. The notion of a low fertility trap (see, e.g., McDonald, 2008; Lutz et al., 2006) captures the idea that once the fertility level of a country falls below 1.5 births per woman and stays there for a while, it can lead to self-enforcing mechanisms that are difficult to reverse.[2] The anticipated economic consequences for countries in the low fertility trap are manifold, from a decline in the working-age share of the population to a slowdown of economic growth to financial difficulties in health care and pensions systems (Bloom et al., 2010).[3] With the growing realization that sustained low fertility has serious ramifications, governments in affected countries have started to take policy measures that enhance the private choice to have children. Chief among these have been efforts to expand child care availability. However, there is almost no previous experience of fertility-relevant policy making in the context of very low fertility rates (McDonald, 2007). As a result, it is still open for debate whether such efforts are an effective way to increase fertility rates where these rates are considered to be too low.

In this paper we evaluate the impact on fertility of a German policy initiative from the mid-2000s, which led to a large scale expansion of child care slots for children under the age of three in West Germany. One of the explicitly stated goals of the initiative was to increase fertility rates by making children less costly in terms of income and career opportunities (Rindfuss et al., 2010). Ever since the 1970s, Germany has been among the twenty countries with the lowest fertility rates worldwide (Population Reference Bureau, 2007). Its population reached a maximum shortly after the turn of the millennium and has started to decline thereafter as a result of sustained very low fertility rates (Dorbritz, 2008). In both political and academic debates, these demographics have been associated with a lack of progressive family policies. Especially the availability of child care for young children was severely restricted in West Germany up until very recently. For example, in a survey conducted in 2005, 35 percent of West German mothers with under three year olds stated a demand for a child care slot (Bien et al., 2007), while only roughly 5 child care slots per 100 children in this age group were available.[4] Prompted by this

[2] One such mechanism is behavioral: if very low fertility is sustained for a long period of time, preferences can begin to shift away from childbearing, and a reversal of low fertility becomes more difficult (Rindfuss et al., 2004).

[3] Currently, there are 28 countries with total fertility rates below 1.5, and the government of each of these countries considers this level of fertility too low (United Nations, 2003).

[4] A recent study by Wrohlich (2008) calculates that more than 50 percent of West German mothers with children aged 0-3 were queuing for a child care slot in the mid-2000s, suggesting that the excess demand for child care was even more severe. In East Germany, by contrast, child care coverage for children aged 0-3 were around 40 percent already in 2006.

severe rationing of public child care, the German government implemented a set of comprehensive public child care reforms during the period 2005-2008, with the explicit intention to increase fertility levels. Specific objectives were to: (i) increase the child care coverage rate for under three year olds to 35 percent by 2013, and (ii) establish the legal right to a child care slot for all preschool children aged one and above by 2013. On aggregate, the policy initiative led to a substantial expansion of child care slots for young children across West Germany's 326 counties. However, since the responsibility to create additional child care slots lay with municipalities, the counties differ distinctly in the extend to which child care coverage increased. The county-level differences that have emerged during the expansion of public child care in West Germany offer a natural way to evaluate its impact on fertility.

Our empirical strategy is as follows. First, we estimate a difference-in-differences model which compares the county-level birth rate (i.e., births per 1,000 women) in West Germany before and after the child care reforms, between counties where child care coverage expanded a lot (i.e., the treatment group) and counties with little increase in child care coverage (i.e., the control group). In order to verify that the key identifying assumption of this approach—i.e., the "common trend" assumption—is plausible, we run placebo treatment difference-in-differences estimations in pre-treatment years. Second, we analyze a more flexible county-fixed effects model of the birth rate which allows for time-invariant heterogeneity between counties and controls for common shocks through the inclusion of year dummy variables. In order to become confident that the results from the fixed-effects model are valid, we perform a battery of sensitivity checks. For example, we provide evidence that our results are not confounded by selective migration of potential mothers to counties with high child care availability.

Our results are interesting. We find consistent and robust evidence of a positive effect of public child care expansion on fertility. To be concrete, our estimates suggest that a 10 percentage point increase in public child care coverage increases the number of births per 1,000 women by 1.4, or roughly 3.2 percent of the baseline birth rate. Under the strict assumption of linearity, this result has a striking implication: moving from having child care slots available for 5 percent of children under three years old to having slots available for 35 percent will lead the average woman to have roughly 0.13 more children. This, in turn, suggests that the policy initiative under study may help Germany escape the "low fertility trap".[5] Closer examination of the data reveals that there is substantial heterogeneity in the fertility effects of public child care expansion across different age groups. Indeed, compared to the mean effect, the effect on the number of births is virtually doubled for 30 to 34 year old women. Given that the identification proposed here is quite clean and robust to

[5]The total fertility rate of Germany was around 1.4 in 2010. Our estimates suggest the following counterfactual: if Germany's child care coverage rate for under three year olds had reached the 2013-target of 35 percent already in 2009 (instead of an actual 14.3 percent), the fertility level would have been 1.49 instead of 1.4.

several robustness checks, it is reasonable to argue that what we have found here is important for the public debate on family policies and low fertility in Western Europe. In particular, our analysis provides some first causal evidence suggesting that low fertility may be reversed as policy makers start to facilitate the combination of motherhood and labor force participation.

The remainder of the paper is organized as follows. Section 4.2 provides a review of the related literature. Section 4.3 gives the background policy context for the analysis by describing the development of public child care in Germany. Section 4.4 outlines the empirical strategy, while Section 4.5 describes the data. Section 4.6 presents the results on the impact of expanding publicly-financed child-care on fertility. Section 4.7 checks the sensitivity of our findings and Section 4.8 discusses these results and concludes.

4.2 Related Literature

We build on an impressive amount of research that examines the consequences of policy efforts to improve the availability and affordability of public child care. These studies can be divided into two broad types. The first examines the effects of child care policy innovations on maternal employment. The picture that emerges from this literature in general is that of positive effects from the introduction or expansion of affordable child care on the labor supply of mothers—particularly sole mothers (e.g., Gelbach, 2002; Blau and Tekin, 2007; Baker et al., 2008; Lefebvre and Merrigan, 2008; Cascio, 2009). One notable exception is the study by Havnes and Mogstad (2011), which exploits the introduction of universal child care in Norway in the late 1970s and finds no positive causal relationship between public child care and maternal employment. Despite this, the total available evidence supports the widely held perception that the availability of public child care assists families in reconciling work with family life.

The second type of study examines the link between child care and child development. Several lines of evidence emerge from these studies. First, there exist studies offering both negative (e.g., NICHD – Early Child Care Research Network, 2003a; Baker et al., 2008), neutral (e.g., Lefebvre and Merrigan, 2002) and positive (NICHD – Early Child Care Research Network, 2004; Havnes and Mogstad, 2012) effects on the developmental outcomes of young children of the amount of time spent in child care. Second, there appears to be a positive relationship between child care quality and child development (e.g., Field, 1991; NICHD – Early Child Care Research Network, 2003b). Finally, while the amount of time spent in child care contributes independently to child development, the impact of family background characteristics is generally stronger.

Much less can be said for our knowledge of the impact of public child care on fertility. From a theoretical perspective, the link between child care and fertility is clear. As child care becomes available, mothers are able to return to work sooner after childbirth. This, in turn, reduces the opportunity cost of having children—e.g., forgone wages or loss of skill development while out of the labor force—and so increases fertility (e.g., Becker and Lewis, 1973; Willis, 1973; Ermisch, 2003). However, this theoretical prediction receives weak empirical support and the evidence is ambiguous at best – see, for example, Mason and Kuhlthau (1992); Kravdal (1996); Del Boca (2002); Hank and Kreyenfeld (2003). What distinguishes our work from these studies is that we exploit a set of policy innovations which led to a large scale expansion of child care slots for young children within a short period of time.

To the best of our knowledge, there exist only two other studies that use policy reforms to assess the impact of child care on fertility. Rindfuss et al. (2010) examine a policy reform from the mid-1970s in Norway, which led to a substantial expansion of public child care. Simulations based on estimates from a municipality-level fixed effects model suggest that moving from having no child care slots available for preschool children to having slots available for 60 percent of preschool children leads the average woman to have 0.5 more children. Mörk et al. (2010) exploit the exogenous variation in user fees caused by a Swedish childcare reform to identify the causal effect of childcare costs on fertility. The results suggest that the reduction in child care costs increased the number of first and higher order births, but only seemed to affect the timing of second births.

Both of the aforementioned studies are conducted within the Scandinavian context, which is highly unrepresentative of many other developed countries demographically. As a consequence, the wider applicability of the results is unclear. By contrast, the context of our study is Germany – a country already struggling with the ramifications of sustained low fertility. Thus, our results speak more directly to the policy debate as to whether or not low fertility can be reversed through changes in public policy that allow women to combine employment and motherhood.

4.3 Background and Context

The German child care system is characterized by a marked East-West-divide. Throughout the history of the former German Democratic Republic, the East German government strongly supported the use of institutional daycare for children of all ages. Apart from ideological purposes, the government's primary aim was to guarantee the compatibility of motherhood and female employment. The East German child care system survived the German reunification, with more than one-half of all East German children under the age of three attending a child care center in the mid-1990s. At the turn of the millennium, parents in East Germany demanded fewer child care slots for children under three than were potentially available (Hank et al., 2001). The picture today still is that there is no shortage of child care slots for children under three in East Germany.

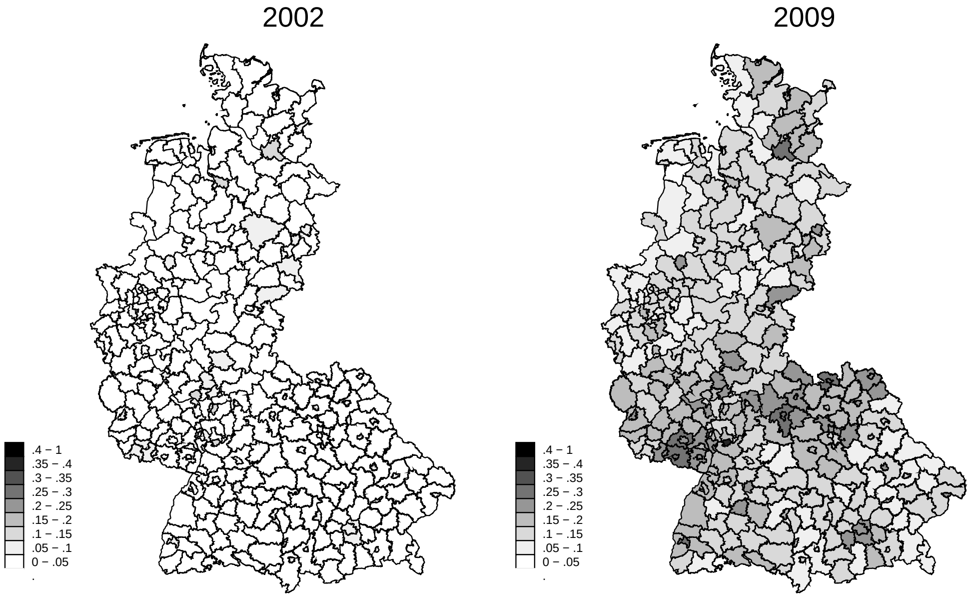
The situation was much different in West Germany, where social policies supported private care by mothers over universal childcare for preschool children until after reunification (Moeller, 1993). In the late 1990s, the German government enacted legislation that grants children aged three to six the right to a place in kindergarten, which ultimately led to full provision of public child care for that age group in West Germany. However, it was not until the mid-2000s that the development of institutionalized child care for children under three came to the political fore. Before 2005, less than 5 child care slots per 100 children under the age of three were available in West Germany. Thereafter the availability of child care slots for children in this age group started to expand rapidly. The expansion was brought about by three major policy initiatives:

- In 2005, the government made the legal commitment (*"Tagesbetreuungsausbaugesetz"*) to create 230,000 additional child care slots for under three year old children by 2010 in West Germany. The specific aim was a child care coverage rate of 17 percent by 2010 in West Germany.
- In 2007, a summit (called *"Krippengipfel"*) of the three federal levels—i.e., federal state, "Länder", local authorities—agreed upon increasing the child care coverage rate for under three year olds to 35 percent by 2013.
- In 2008, the law to promote children (*"Kinderförderungsgesetz"*) established the legal claim to a child care slot for all preschool children age one and above by 2013.

In the run-up to the law to promote children, the three federal levels agreed that every level bears one third of the expansion costs in order to share the burden.

These initiatives led to a major expansion of child care slots for young children. Figure 4.1 provides a map which illustrates the child care coverage rate for West Germany's 326 counties in 2002 and 2009, respectively. In each year, the child care coverage rate is defined as the percentage of under three-year-old children in child care centers. In principle, this measure reflects use rather than availability. However, throughout the period we examine, the demand for child care slots for under three year olds far exceeded the supply (Bien et al., 2007; Wrohlich, 2008). Thus, the child care coverage rate captures both availability and use. In 2002, we observe that the child care coverage rate was consistently below 5 percent across virtually all West German counties. In succeeding years, the child care coverage rate rose sharply, i.e., it more than quintupled to reach an average of 15 percent in 2009. However, it is also evident from the map that the counties differ distinctly in terms of changes in child care coverage. Indeed, the percentage point increases in child care coverage

Figure 4.1: Public child care coverage in West German counties in 2002 and 2009



Notes: The left panel shows child care coverage in West German counties in 2002, the right panel shows child care coverage in West German counties in 2009.

from 2002 to 2009 varied from a minimum of 3 percentage points to a maximum of 27 percentage points. Both identification strategies proposed in this study exploit these pronounced temporal and spatial variations in child care coverage after 2002. Apart from the child care expansion under consideration, we are not aware of any other major policy changes that could cause a problem for our estimation.

4.4 Empirical Strategy

In order to identify the effect of public child care on fertility, we start with a simple difference-in-differences approach which exploits the expansion of public child care during the last decade. In particular, we order the German counties by the absolute size of the increase in the public child care coverage rate from 2002 to 2009. Then, we define those counties whose increase in public child care coverage was above the median as the treatment group, while those counties whose increase was below the median constitute the control group. Further, we use the year 2002 as the pre-treatment year and the year 2009 as the post-treatment year in our basic two-period difference-in-differences model. There are two reasons for choosing the year 2002 as the pre-treatment year: First, since public child care was not a major political issue until the year 2005, the year 2002 is certainly a year which is unaffected by any political decisions fostering the expansion of public child care. Second, the year 2002 is the last year where administrative data on public child care coverage on the county level is available before public child care actually became a political issue in 2005.

We can express our simple two-period difference-in-differences strategy that identifies the treatment effect δ in the following way:

$$\begin{aligned} &\{E(y_{ct+1} \mid D_c = 1, T_t = 1) - E(y_{ct+1} \mid D_c = 1, T_t = 0)\} \\ -&\{E(y_{ct+1} \mid D_c = 0, T_t = 1) - E(y_{ct+1} \mid D_c = 0, T_t = 0)\} = \delta \end{aligned} \tag{4.1}$$

where y_{ct+1} is the number of births by 1,000 women aged 15 to 44 living in county c in year $t+1$. The outcome variable is measured in the year $t+1$ because there are at least 10 months from the decision to have a child to the actual birth. D_c is the treatment group indicator for county c, which is unity for counties of the treatment group, i.e., for those counties with above median increase in public child care coverage, and zero for counties of the control group. $T_t = 0$ is the post-treatment year indicator, which is unity for all observations in the post-treatment year 2009, and zero for all observations in the pre-treatment year 2002.

This difference-in-differences model can be rewritten in regression form:

$$y_{ct+1} = \alpha + \beta D_c + \gamma T_t + \delta D_c T_t + \epsilon_{ct} \tag{4.2}$$

In this regression, the treatment effect δ is identified by the coefficient on the interaction of the treatment group indicator D and the post-treatment indicator T, while α captures the number of births per 1,000 women aged 15 to 44 in the control group in the pre-treatment year 2002, β captures the difference of the treatment group from the control group in the pre-treatment year 2002, and γ gives the difference between the post- and the pre-treatment year for the control group. A vector of covariates X can be introduced into the model in a straightforward way. The key identifying assumption for this difference-in-differences model is that treatment and control group follow the same time trend in absence of the treatment. We will test whether this crucial common-trend assumption is plausible by looking at the trends of treatment and control group in pre-treatment periods.

The main specification in this paper, however, is a more generalized fixed-effects framework that allows for unobserved time-invariant differences between counties. The respective regression equation can be written in the following way:

$$y_{ct+1} = \eta_c + \mu_t + X'_{ct}\lambda + \rho d_{ct} + \zeta_{ct} \tag{4.3}$$

where η_c is a county fixed effect for county c, μ_t is a year fixed effect for year t, and X'_{ct} is a vector of covariates of county c that vary over time t. The variable d_{ct}

represents the public child care coverage rate of county c in year t. Accordingly, ρ captures the causal effect of public child care coverage on fertility. As before, y_{ct+1} is our outcome variable measuring the number of births per 1,000 women aged 15 to 44 living in county c in year $t+1$. Later, we will also use the number of births per 1,000 women aged 15 to 44 in $t+2$ as well as the arithmetic mean of births per 1,000 women aged 15 to 44 in $t+1$ and $t+2$ as outcome variables in order to more flexibly address timing issues. Note that in this fixed-effects specification, identification comes from within-county variation in public child care coverage over time. Consequently, the key identifying assumption of this fixed-effects framework is that there are no unobserved characteristics of a county that vary over time and are correlated with the variable of interest d and the outcome variable.

4.5 Data on Child Care Coverage and Fertility

In our analysis we use official data from German Statistics Office (*Statistisches Bundesamt*) and Federal Employment Agency (*Bundesagentur für Arbeit*) aggregated on the county level (*Landkreise*). We use a balanced panel of all 326 West-German counties from 1998 to 2010. A detailed description of the data and variable definitions can be found in the data appendix.

Child care data for children less than three years old is available for 1998, 2002, 2006, 2007, 2008, and 2009.[6] We define our variable of interest, child care coverage, as child care slots over the population of children less than three years old. Table 4.1 shows that child care coverage averages 7.8 percent over the whole period of observation. It varies widely from 0 to 41.6 percent. We can see in Table 4.2 that child care coverage was very low in 1998 and 2002. The average over the 326 counties was 1.7 respectively 2.3 percent. The rise is already evident in 2006, when child care coverage reached 7.4 percent. In the following years it rose steadily to on average 14.3 percent in 2009.

Our dependent variable births per 1,000 women is calculated as the sum of births over 1,000 women in reproductive age, i.e., between 15 and 44 years. In Table 4.1 we show two specifications from our regression analysis measured in period $t+1$ respectively $t+2$. Births per 1,000 women in $t+1$ average at 44.163, with a standard deviation of 3.979. Figures range from 29.890 in the lowest fertility county-year observation to the highest figure of 65.440. For births per 1,000 women in $t+2$, figures are comparable in size and variation, but by forwarding we lose one wave of data corresponding to 326 observations.

We also employ a number of control variables in the estimations. Population density is defined as full population divided by surface area in square kilometers. Counties differ much in population density ranging from 40 to over 4,000 people per

[6] Data for 2010 is not used, as our dependent variable is forwarded at least one year.

Table 4.1: Descriptive statistics

Variable	N	Mean	S.D.	Min	Max
Child care coverage	1956	0.078	0.063	0.000	0.416
Births per 1,000 women (t+1)	1956	44.163	3.979	29.890	65.440
Births per 1,000 women (t+2)	1630	44.292	3.904	29.207	62.887
Population density	1956	575.626	712.438	40.720	4286.211
Employment rate (m)	1956	0.604	0.060	0.406	0.737
GDP per capita	1956	28.030	10.797	11.238	86.079
Population fraction 15-19	1956	0.057	0.006	0.039	0.073
Population fraction 20-24	1956	0.057	0.008	0.040	0.118
Population fraction 25-29	1956	0.059	0.011	0.039	0.105
Population fraction 30-34	1956	0.064	0.014	0.042	0.112
Population fraction 35-39	1956	0.076	0.009	0.050	0.106
Population fraction 40-44	1956	0.083	0.007	0.062	0.101
Population fraction 44+	1956	0.454	0.035	0.332	0.553
Female in-migrants 18-29 (t+1)	1630	0.009	0.005	0.003	0.049
Female in-migrants 18-49 (t+1)	1630	0.014	0.007	0.006	0.096
Female out-migrants 18-29 (t+1)	1630	0.008	0.003	0.004	0.047
Female out-migrants 18-49 (t+1)	1630	0.014	0.005	0.006	0.095

Notes: The table shows descriptive statistics (number of observations, mean, standard deviation, minimum, and maximum) on the county level aggregated over all waves used in the estimations. In particular, for the variables child care coverage, population density, employment rate (male) GDP per capita as well as for all population fraction variables, the figures show aggregated values over the years 1998, 2002, 2006, 2007, 2008, and 2009. Accordingly, all variables measured in (t+1) are aggregated over the years 1999, 2003, 2007, 2008, 2009, and 2010, whereas all variables measured in (t+2) are aggregated over the years 2000, 2004, 2008, 2009, and 2010.

square kilometer. On average 576 people live per square kilometer. Male employment rate is defined as all male employees subject to social insurance contribution at place of residence divided by male working age population, with working age being 20 to 64 years. Male employment rate averages at 60.4 percent with a standard deviation of 0.06. GDP per capita is defined as GDP in 1,000 Euro divided by the full population and averages 28,030 Euro per capita. Further controls are population fractions of certain age groups in 5-year intervals from 15 to over 44. They are defined as the population in the specific age group over the full population. Descriptive statistics show that younger cohorts are smaller than older cohorts. While 15 to 19 year old people represent on average 5.7 percent of the population, people from 40 to 44 years represent on average 8.3 percent of the population.

In later robustness checks, we use in- and out-migrant flows in $t + 1$ divided by the full population as dependent variables. We do not have information from wave 1998 which reduces the sample size by 326 observations. For the sake of completeness, we also report descriptive statistics for migrant flows in Table 4.1.

4.6 Results

We start with a simple graph that shows the increase in public child care coverage for the group of West German counties with above median increase in the period from 2002 until 2009 (treatment group) as well as for the group of West German

Table 4.2: Child care coverage over time

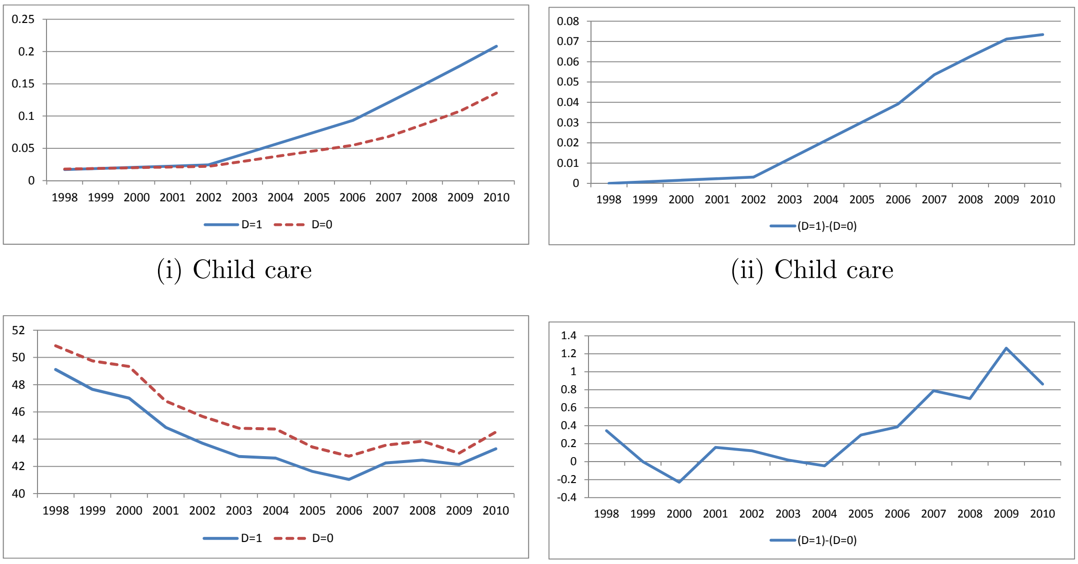
	Child care coverage				
Year	N	Mean	S.D.	Min	Max
1998	326	0.017	0.026	0.000	0.322
2002	326	0.023	0.029	0.000	0.355
2006	326	0.074	0.042	0.010	0.378
2007	326	0.095	0.047	0.022	0.398
2008	326	0.118	0.050	0.033	0.405
2009	326	0.143	0.052	0.037	0.416
2010	326	0.172	0.055	0.071	0.421

Notes: The figures show mean child care coverage rates across West German counties as well as standard deviations, minimum, and maximum values. All information is provided for the years 1998, 2002, 2006, 2007, 2008, 2009, and 2010.

counties with below median increase in the period from 2002 until 2009 (control group). As we can see from the upper left panel (i) of Figure 4.2, public child care coverage was at a low level of not even 2 percent for both groups of counties in 1998. From 1998 to 2002, we observe hardly any dynamics. By 2006, child care coverage had increased from a mere 2 percent to 5 percent in the control group and 9 percent in the treatment group. By 2010, public child care coverage was 21 percent in treatment counties and 14 percent in control counties. Thus, while treatment and control counties started out from the same low level of child care coverage in 2002, trends have diverged since then and the difference in the coverage rate has increased to more than 7 percentage points in 2010. The upper right panel (ii) of Figure 4.2 graphically depicts this increase in the difference between treatment and control counties.

Turning to the lower left panel (iii) of Figure 4.2, we see that, for both groups of counties, the number of births per 1,000 women aged 15 to 44 generally decreased from 1998 (49.1; 50.9) until 2006 (41.0; 42.7) whereas there is a slight upward movement from 2006 until 2010 (43.3; 44.5). Over the whole period of observation, the treatment group counties show lower numbers of births than the control group counties. However, the difference between treatment and control group becomes smaller over time as the treatment group slowly approaches the control group level. Normalizing the difference in births per 1,000 women in 1999 to zero, the lower right panel (iv) of Figure 4.2 depicts the dynamics of this difference over time. This difference does not systematically change from 1998 until 2004. In the following years, the number of births in the treatment group gradually increases as compared to the number of births in the control group. By 2010, the number of births has increased by 0.86 births more in the treatment group than the number of births in the control group. Thus, taken together, these graphs are compatible with the hypothesis that public child care coverage increases fertility.

Figure 4.2: Child care and fertility in treatment and control group



(i) Child care

(ii) Child care

(iii) Fertility

(iv) Fertility

Notes: The figures show averages of the treatment group (D=1) and the control group (D=0) and the difference between the treatment and the control group ((D=1)-(D=0)) over time. The figure in the upper left panel (i) depicts child care coverage, the figure in the upper right panel (ii) depicts the difference between the treatment and the control group in child care coverage. The figure in the lower left panel (iii) depicts births per 1,000 women, the figure in the lower right panel (iv) depicts the difference between the treatment and the control group in births per 1,000 women.

Difference-in-differences regression results In a next step, we bring these graphs to a multivariate difference-in-differences framework using the year 2002 as the pre-treatment year and the year 2009 as the post-treatment year, while measuring the outcome variable in the year $t + 1$. In column 1 of Table 4.3, we see that counties of the treatment group had on average 2.073 births per 1,000 women less than counties of the control group in 2003. Further, the coefficient on the post-treatment dummy shows that the number of births per 1,000 women in the control group decreased by 0.280 from 2003 to 2010. Most importantly, the coefficient on the interaction term depicts the positive treatment effect observed in Figure 4.2. Compared to the control group, the number of births per 1,000 women in the treatment group increased by 0.845 births more from 2003 to 2010. This effect turns out to be statistically different at the 1 percent level. Including regional control variables such as population density, male employment rate, and GDP per capita in column 2 of Table 4.3 does not affect this result. Taking into consideration that the treatment, i.e., the difference in the increase of child care coverage between treatment and control group between 2002 and 2009, corresponds to 6.8 percentage points, the difference-in-differences estimate of 0.815 suggest that a 10 percentage point increase in public child care coverage increases the number of births per 1,000 women by 1.2 births or by 2.8 percent.

Table 4.3: Difference-in-differences and placebo difference-in-differences estimates

	Births per 1,000 women (t+1)		Births per 1,000 women (t+1) Placebo treatment in 2002	
Treatment group x Post-treatment	0.845***	0.815**	0.018	-0.094
	(0.310)	(0.323)	(0.296)	(0.287)
Treatment group	-2.073***	-2.106***	-2.091***	-2.009***
	(0.366)	(0.346)	(0.453)	(0.392)
Post-treatment	-0.280	-0.638***	-4.956***	-5.245***
	(0.221)	(0.242)	(0.183)	(0.193)
Controls	No	Yes	No	Yes
N	652	652	652	652
R^2	0.066	0.180	0.344	0.487

Notes: The table shows results of difference-in-differences estimations where the treatment group consists of all counties with above median increase in child care coverage rates from 2002 until 2009. The control group consists of counties with below median increase in child care coverage rates from 2002 until 2009. In columns (1) and (2), the baseline period is the year 2002 whereas the post-treatment period is the year 2009. In columns (3) and (4), the baseline period is the year 1998 whereas the placebo post-treatment period is the year 2002. Control variables in columns (2) and (4) include the county's population density, GDP per capita as well as the male employment rate. Robust standard errors are clustered at the county level and given in parentheses. *** 1 percent significance level; ** 5 percent significance level; * 10 percent significance level.

The key identifying assumption of our difference-in-differences approach is that the trends in the outcome variable are the same for treatment and control group in absence of the treatment. In order to provide evidence for the plausibility of this assumption, we run placebo treatment difference-in-differences estimations in the pre-treatment years. In particular, we use the year 1998 as the pre-treatment year and the year 2002 as the placebo post-treatment year. Column 3 of Table 4.3 shows that we do not find any deviance from the common trend in this pre-treatment period. Including our regional control variables in column 4 of Table 4.3 does not affect this result. Thus, this placebo treatment exercise supports the validity of the key identifying assumption of our difference-in-differences approach, namely that treatment and control group follow the same trend in absence of the treatment.

Fixed-effects regression results Now, we turn to our more generalized county fixed-effects model which allows for time-invariant unobserved heterogeneity between counties. Apart from county and year fixed effects, we control for a county's population density, male employment rate and GDP per capita in all the following regressions. In column 1 of Table 4.4, we see that public child care coverage positively affects births per 1,000 women in the following year. The coefficient suggests that introducing full provision of public child care increases the number of births per 1,000 women by 14.049. Accordingly, a 10 percentage point increase in child care coverage leads to an increase in births per 1,000 women of 1.40. Although being slightly larger, the estimate compares fairly well to the earlier result (1.2) if we take into account the different estimation setup. Moreover, the fixed-effect treatment effect is precisely estimated and turns out significantly different from zero at the 1 percent level.

Table 4.4: Fixed-effects estimates

	Births per 1,000 women (t+1)	Births per 1,000 women (t+2)	Births per 1,000 women [(t+1)+(t+2)]/2
Child care coverage	14.049***	15.893***	16.776***
	(2.858)	(3.068)	(2.769)
Population density	0.004	0.007**	0.005
	(0.004)	(0.004)	(0.004)
Employment rate (m)	-39.458***	-31.733***	-36.536***
	(7.207)	(6.569)	(6.526)
GDP per capita	0.246***	0.234***	0.254***
	(0.042)	(0.044)	(0.041)
Year dummies	Yes	Yes	Yes
N	1,956	1,630	1,630
F-statistic	187.82	205.70	244.49

Notes: The table shows the results of county fixed-effects estimations. The outcome variable births per 1,000 women aged 15 to 44 is forwarded by one period in column (1), and forwarded by two periods in column (2); the outcome variable in column (3) is the arithmetic mean of the outcome variables from columns (1) and (2). Robust standard errors are clustered at the county level and given in parentheses. *** 1 percent significance level; ** 5 percent significance level; * 10 percent significance level.

Note that public child care coverage is measured at the 31st of March of the respective year while the number of births per 1,000 women covers the period from 1st of January until 31st of December of the respective year. Accordingly, forwarding our outcome variable by one year means that we allow fertility reactions to increases in public child care coverage within a county from 9 to 21 months after public child care coverage is observed. Yet, individuals might not be immediately aware of increases in public child care coverage. Further, even if individuals are aware, they might not react immediately to changes in public child care provision. Moreover, even if they do, women might not be successful in getting pregnant within a short period. For these reasons, we might wish to allow for some more time to observe reactions to increases in public child care coverage. In column 2 of Table 4.4, we forward the dependent variable births per 1,000 women by yet another year. In column 3 of Table 4.4, we combine the one and two year forwarding into an arithmetic mean of births per 1,000 women. Thus, we observe fertility reactions from 9 to 33 months after public child care coverage is observed. The estimates in columns 2 and 3 slightly increase as compared to column 1 while the precision of the estimates is basically unaffected. Thus, this exercise, which more flexibly deals with timing of fertility issues, confirms our result of a positive and highly significant effect of public child care coverage on fertility.

Turning to the coefficients of the covariates, we observe that GDP is associated with a higher number of births, suggesting that fertility is higher in economic upswings than in downswings. Yet, the association of fertility and male employment is of the opposite sign. This may well be due to a delayed employment adjustment in the rather rigid German labor market or due to nonlinear associations between economic cycles and fertility. Increases in population density are positively associated with the number of births per 1,000 women although this association is only statistically significant in the specification of column 2 of Table 4.4.

Child care provision may not be an equally important determinant of fertility decisions over the life-cycle. Obviously it should be more essential for women with strong labor market aspirations. Timing of education, marriage and fecundity may also play their roles in fertility decisions. To analyze heterogeneity in the response to public child care coverage, we split the dependent variable, number of births per 1,000 women, in six different dependent variables. Each of them captures fertility effects for a specific age-group, starting with 15 to 19 year old women up to 40 to 44 year old women.

Table 4.5 presents county fixed-effects results for these six outcome variables. We find substantial heterogeneity in the effect of public child care provision on the number of births over different age groups. The effect is largest for women aged 30 to 34, with a statistically highly significant coefficient of 33.068. Thus, the effect is virtually doubled for this age group compared to the average effect. Maybe surprisingly, we do not find any effects for women aged 25 to 29 with the coefficient being even negative but close to zero. The effects for women in age groups 20 to 24 and 35 to 39 are somewhat smaller than for the 30 to 34 year olds; however, they are still economically and statistically highly significant. For very young women aged 15 to 19, the effect is still smaller (6.587). For 40 to 44 year old women, we do not find any effect, which is less surprising given the generally low fertility around that age. Taken together, we find some evidence of an above average effect of child care on number of births per 1,000 women in medium age groups (with the exception of mothers aged 25 to 29), whereas we find a below average effect for women in rather old and rather young reproductive age.

4.7 Sensitivity Analysis

In our fixed-effects regressions, we can control for time-invariant heterogeneity between counties. Identification of fertility effects comes from within-county variation in public child care coverage over time. Effects from our fixed-effects models might be confounded by variables which change over time within a county and are correlated with both public child care provision and fertility decisions at the same time. To minimize this problem from the start, we control for population density, GDP per capita as well as the male employment rate in all our regressions. Another natural candidate which could potentially constitute a confounding factor is a county's age composition. On the one hand, one might think of a county's age composition as being rather stable over time. To the extent that this is true, age composition would be captured by the county fixed effect. For the rest of this section, however, we focus on the case where we think of age composition as being a time-varying construct which could constitute a potential confounding factor. If the number of women in

Table 4.5: Fixed-effects estimates: Effect heterogeneity across age groups

	Births per 1,000 women aged 15-19 (t+1)	Births per 1,000 women aged 20-24 (t+1)	Births per 1,000 women aged 25-29 (t+1)	Births per 1,000 women aged 30-34 (t+1)	Births per 1,000 women aged 35-39 (t+1)	Births per 1,000 women aged 40-44 (t+1)
Child care coverage	6.587***	14.384**	-2.047	33.068***	15.767***	1.948
	(2.399)	(5.922)	(7.428)	(7.084)	(5.092)	(1.609)
Population density	-0.009***	0.003	-0.018***	0.003	0.048***	0.013***
	(0.003)	(0.006)	(0.006)	(0.008)	(0.009)	(0.002)
Employment rate (m)	3.551	-7.900	-46.456***	-35.270**	-45.097***	-11.365***
	(4.918)	(12.341)	(15.605)	(15.981)	(11.513)	(3.361)
GDP per capita	-0.066*	-0.105	-0.024	0.350***	0.448***	0.093***
	(0.038)	(0.085)	(0.102)	(0.098)	(0.071)	(0.025)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	1,956	1,956	1,956	1,956	1,956	1,956
F-statistic	73.34	272.03	64.01	63.23	216.71	115.63

Notes: The table shows the results of county fixed-effects estimations. The outcome variables are age group-specific fertility rates, i.e., the number of births per 1,000 women aged 15-19 (column 1), 20-24 (column 2), 25-29 (column 3), 30-34 (column 4), 35-39 (column 5), and 40-44 respectively (column 6). All outcome variable are forwarded by one period. Robust standard errors are clustered at the county level and given in parentheses. *** 1 percent significance level; ** 5 percent significance level; * 10 percent significance level.

child-bearing age increases over time, this could push local authorities to increase child care coverage while, at the same time, the number of births might increase. Therefore, changes in a county's age composition clearly deserve a more in-depth analysis.

Table 4.6: Fixed-effects estimates using age structure controls

	Births per 1,000 women (t+1)	Births per 1,000 women (t+2)	Births per 1,000 women ((t+1)+(t+2))/2
Child care coverage	9.553***	10.172***	10.751***
	(2.322)	(2.620)	(2.187)
Population density	-0.015***	-0.009***	-0.013***
	(0.003)	(0.003)	(0.003)
Employment rate (m)	1.455	2.313	0.997
	(6.008)	(6.029)	(5.545)
GDP per capita	0.042	0.052	0.053*
	(0.034)	(0.036)	(0.031)
Year dummies	Yes	Yes	Yes
Age structure controls	Yes	Yes	Yes
N	1,956	1,630	1,630
F-statistic	196.19	178.61	260.30

Notes: The table shows the results of county fixed-effects estimations. The outcome variable births per 1,000 women aged 15 to 44 is forwarded by one period in column (1), and forwarded by two periods in column (2); the outcome variable in column (3) is the arithmetic mean of the outcome variables from columns (1) and (2). Age structure controls include the ratio of 15-19 year olds in the total population, the ratio of 20-24 year olds in the total population, the ratio of 25-29 year olds in the total population, the ratio of 30-34 year olds in the total population, the ratio of 35-39 year olds in the total population, the ratio of 40-44 year olds in the total population, and the ratio of over 44 year olds in the total population. Robust standard errors are clustered at the county level and given in parentheses. *** 1 percent significance level; ** 5 percent significance level; * 10 percent significance level.

Note that, to a certain extent, our outcome variable already accounts for a county's change in age composition. This is because our dependent variable does not merely count the number of births in a given year but relates them to the number of women in reproductive age living in this county in this given year. Thus, the denominator of our outcome variable makes sure that our results are not confounded by mere size effects. However, one might argue that there might be changes in the composition within the group of women in reproductive age, which could confound our results. Imagine the overall number of women in reproductive age does not change but the number of 30 to 34 year old women gains relative size as compared to the number of 40 to 44 year old women. This might well push local politicians towards providing more public child care spaces and similarly lead to more births per 1,000 women in child-bearing age. But again, we have already dealt with this potential problem by splitting our outcome variable in six outcome variables each capturing a specific age subgroup of women in child-bearing age (see Table 4.5).

As an alternative strategy to deal with this problem, we use our basic outcome variable, i.e., the number of births per 1,000 women of reproductive age, and extensively control for a county's age structure in our county-fixed-effects model. In particular, we introduce the fraction of 15-19 year olds, 20-24 year olds, 25-29 year olds, 30-34 year olds, 35-39 year olds, 40-44 year olds, and over 44 year olds in the population as covariates.[7] Table 4.6 presents the results of this extended fixed-effects framework. Again, we use births per 1,000 women in reproductive age in $t+1$ (column 1), $t+2$ (column 2) as well as the arithmetic mean over both periods (column 3) as alternative outcome variables. Across all three outcome variables, the fixed-effects estimates confirm the positive and highly significant effect of public child care provision on fertility. Taking the coefficient from column 3 as an example, a 10 percentage point increase in public child care coverage increases the number of births per 1,000 women by 1.2. Thus, the effects found in this extended fixed-effects specification are somewhat smaller than the effects found in the basic fixed-effects specification from Table 4.4. Yet, also note that they are virtually identical to the results yielded by our difference-in-differences specification from Table 4.3.

Table 4.7: Fixed-effects estimates on the effects of public child care expansion on in-migration

	Female in-migrants 18-29 (t+1)	Female in-migrants 18-49 (t+1)	Female in-migrants 18-29 (t+1)	Female in-migrants 18-49 (t+1)
Child care coverage	-0.001	-0.003	0.001	0.002
	(0.002)	(0.004)	(0.001)	(0.002)
Population density	0.000**	0.000*	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Employment rate (m)	-0.013**	-0.011	0.011	0.020
	(0.006)	(0.010)	(0.007)	(0.014)
GDP per capita	0.000***	0.000***	0.000**	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Year dummies	Yes	Yes	Yes	Yes
Age structure controls	No	No	Yes	Yes
N	1,630	1,630	1,630	1,630
F-statistic	8.75	7.06	14.59	14.66

Notes: The table shows the results of county fixed-effects estimations. The outcome variable in columns (1) and (3) is the number of female in-migrants aged 18-29 divided by the total population while the outcome variable in columns (2) and (4) is the number of female in-migrants aged 18-49 divided by the total population. Age structure controls in columns (2) and (4) include the ratio of 15-19 year olds in the total population, the ratio of 20-24 year olds in the total population, the ratio of 25-29 year olds in the total population, the ratio of 30-34 year olds in the total population, the ratio of 35-39 year olds in the total population, the ratio of 40-44 year olds in the total population, and the ratio of over 44 year olds in the total population. Robust standard errors are clustered at the county level and given in parentheses. *** 1 percent significance level; ** 5 percent significance level; * 10 percent significance level.

Although we control for an extensive set of age structure variables in our county-fixed effects framework and use the number of women in reproductive age as a denominator of our outcome variable, one remaining problem might be selective

[7] Computing these population ratios for the subgroup of females only does not change our results.

migration. Suppose couples or women who are pregnant or plan to have a child systematically move to counties that increase public child care coverage. This kind of selective migration of to-be-mothers would confound our estimates.[8] If an increase in public child care coverage can indeed redirect migration flows, this should show up in higher gross in-migration flows in counties that increase public child care coverage. In order to test whether this is the case, we run our fixed-effects regressions using the ratio of female in-migrants aged 18 to 29 in $t+1$ on the total population in t as the outcome variable (column 1 of Table 4.7). As an alternative outcome variable, we use the ratio of female in-migrants aged 18 to 49 in t+1 on the total population in t as the outcome variable (column 2 of Table 4.7). We do not find any effects of public child care coverage on in-migration of women of reproductive age. Introducing our extensive age composition controls in columns 3 and 4 of Table 4.7 does not affect this result.

Table 4.8: Fixed-effects estimates on the effects of public child care expansion on out-migration

	Female out-migrants 18-29 (t+1)	Female out-migrants 18-49 (t+1)	Female out-migrants 18-29 (t+1)	Female out-migrants 18-49 (t+1)
Child care coverage	-0.002	-0.005	-0.000	-0.001
	(0.002)	(0.004)	(0.001)	(0.002)
Population density	0.000**	0.000**	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Employment rate (m)	-0.009**	-0.007	0.008	0.014
	(0.004)	(0.008)	(0.006)	(0.013)
GDP per capita	0.000*	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Year dummies	Yes	Yes	Yes	Yes
Age structure controls	No	No	Yes	Yes
N	1,630	1,630	1,630	1,630
F-statistic	42.55	31.09	44.95	23.50

Notes: The table shows the results of county fixed-effects estimations. The outcome variable in columns (1) and (3) is the number of female out-migrants aged 18-29 divided by the total population while the outcome variable in columns (2) and (4) is the number of female out-migrants aged 18-49 divided by the total population. Age structure controls in columns (2) and (4) include the ratio of 15-19 year olds in the total population, the ratio of 20-24 year olds in the total population, the ratio of 25-29 year olds in the total population, the ratio of 30-34 year olds in the total population, the ratio of 35-39 year olds in the total population, the ratio of 40-44 year olds in the total population, and the ratio of over 44 year olds in the total population. Robust standard errors are clustered at the county level and given in parentheses. *** 1 percent significance level; ** 5 percent significance level; * 10 percent significance level.

Table 4.8 repeats the same exercise for out-migrants. Again, we do not find any effects of public child coverage on migration flows. In further regressions, we use the absolute numbers instead of the ratios of female in-migrants and out-migrants as our outcome variables. Also in these specifications, no indications for effects of public child care on in-migration emerge. There are some tentatively negative effects

[8]However, also note that movements of couples or single mothers with a baby would not confound our results since our outcome variable does not measure the number of under three year olds but the actual number of births.

of public child care coverage on out-migration. Detailed results are available from the authors upon request. Thus, taken together, we conclude that we do not find any evidence that our results might be driven by selective in-migration of to-be-mothers.[9]

4.8 Concluding Remarks

In this paper, we present new empirical evidence for positive effects of public child care supply on fertility. Our results fill a void in the literature: Most of the existing evidence uses data from Scandinavian medium- to high-fertility countries, whereas the subject of our study, Germany, classifies as a low-fertility country. For policy makers it seems to be of particular importance to draw on research results that can show a path to escape the low fertility trap. We can confirm that the encouraging evidence from earlier studies is transferable to low-fertility countries like Germany.

Our evidence is based on two methods that exploit a rapid increase in public child care for under three year olds with wide regional variation at the county level. First, we apply a difference-in-differences method that compares a treatment group of counties with above-median child care expansion to a control group of counties with below-median child care expansion. Second, we use a fixed-effects estimator to control for county-specific fertility rates. Results from both specifications show that child care expansion has a significant positive effect on fertility. In particular, our results suggest that an increase in child care coverage for under three year olds from 5 percent to 35 percent can increase fertility rates by roughly 0.13 children per woman. In other words, a 10 percentage point increase in child care coverage leads to an increase in births per 1,000 women of 3.2 percent. This is a substantial effect of a family policy that serves multiple purposes and, for example, also has positive effects on female labor supply.

There are some interesting aspects we cannot address in this study. It seems worthwhile for future research to analyze the fertility effects in greater detail. In particular, the question arises whether the fertility effect is realized at the intensive or extensive margin (higher-order births). Moreover, we could not address certainly interesting issues of timing of births. Furthermore, one might want to know more about heterogeneity of the effect with respect to educational attainment. These aspects could not be tackled in this paper due to a lack of comprehensive micro data. Also, we might wonder, whether there is a level of child care provision above which fertility effects disappear.

[9] We also estimated our basic fixed-effects models on the level of 75 more aggregated regions (*Raumordnungsregionen*) instead of counties using the number of births per 1,000 women of reproductive age as the outcome variable. The intuition is that selective migration due to increases in public child care coverage is less likely between these larger regions than between counties. These fixed-effects models yield effects very similar to the county fixed-effects models. Results are available on request.

Appendix D

D.1 Data Appendix

County level data from the German Statistics Office is provided in per year per item files. We identify counties by official id numbers and conduct a county level panel from 1997 to 2010. During this investigation period, reforms in geographic local government competency and in data aggregation rules altered county identifiers in some instances. In order to get a fully balanced panel, we made necessary changes that produced time-stable units of observations. Hannover, Aachen and Saarbruecken are merged to city-urban regions, whereas in earlier waves each consists of two separate counties. We use the merged definition throughout all waves and add up values if necessary. Our basic data is from German Statistics Office, whereas the employment related data is from the Federal Employment Agency and matched using county-year identifiers.

Our variables have different underlying measurement concepts. Child care slots, population and employment are record date measures, reported on 1st March (child care from 2006 onwards), 31st December (population, child care before 2006) resp. 30th June (employment). Births, GDP and in- and out-migrants are yearly sums. Births per 1,000 women are defined as the sum of births within the year divided by population as of 31st December. As child care from 2006 onwards is evaluated on 1st March and population on 31st December, we divide it by one year lagged population. Child care in 1998 and 2002 is measured on 31st December and is therefore divided by population in the same year.

Child care figures are conducted from two different data collections by the German Statistics Office. From 2006 onwards we observe occupied child care slots, whereas in 1998 and 2002 we observe available child care slots. As there was clearly excess demand for child care, we can credibly assume both measures to be comparable. If anything, too large values for 1998 and 2002 would lead to a downward bias in our estimation.

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