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Fertility Effects of Child Benefits

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We exploit the 1996 reform of the German child benefit program to identify the causal effect of child benefits on fertility. Generally, the reform increased child benefits. However, the exact amount of the increase varied by household income and sibship size. We use this heterogeneity of the reform to identify causal effects on fertility using a difference-in-differences setting. We apply the large samples of the German Mikrozensus and the rich data of the German Socioeconomic Panel (SOEP). The child benefit reform did not yield robust or statistically significant fertility effects for low income couples. We find some support for positive fertility effects for higher income couples deciding on a second birth.

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1. Introduction

As many industrialized societies face the problem of demographic aging and low birth rates, it is important to study the determinants of fertility and, in particular, to understand the fertility effects of public policy. Numerous countries provide child related cash benefits for families and, yet, the literature still debates their effects on fertility.

In this study, we exploit a major reform of the German child benefit program to investigate whether child benefits affect fertility. As the program combines cash benefits and tax deductions, our analysis is comparable to US studies of fertility effects of personal tax exemptions (e.g., Whittington et al. 1990, Crump et al. 2011). The measures of the German reform varied by household income, which renders it similar to reforms of the EITC (earned income tax credit) in the US and the WFTC (working families tax credit) in the UK. Both programs have been studied for their fertility effects before (see Brewer et al. 2012, Baughman and Dickert-Conlin 2003, or Francesconi and van der Klaauw 2007).

Germany spends about 42 billion Euro annually on child benefits. This makes up the substantial share of about 12 percent of the federal budget (STBA 2015). By international comparison, Germany pays high child benefits, but remains a low fertility country (BMFSFJ 2008). In this situation, the quasi-experiment induced by the child benefit reform of 1996 facilitates an interesting case study of fertility effects, as the increase in payments was substantial and it affected different population groups in heterogeneous ways.

Up until 1995, Germany organized its family benefit system using partly income dependent cash transfers plus a system of tax-exempt child allowances. Critics argued that the system benefited high-income families. In response, the reform introduced a significant increase in cash benefits and the obligation to choose either the benefit or the tax allowance. We investigate whether this increase in the "financial return to child bearing" affected fertility.

This contributes to a broad international literature, which all but agrees on whether benefits affect fertility. While some causal analyses yield strong responses of fertility to benefit

increases, others show small or even negative effects. In the first group of studies, González (2013) studies the effect of a one-time payout of 2,500 Euro after a birth in Spain in 2007. Abortions declined and conceptions increased, resulting in an increase of the annual fertility rate by 6 percent. Cohen et al. (2013) look at changes in the Israeli child subsidy and confirm significant fertility responses to child subsidy changes particularly in the lower income families. Milligan (2005) evaluates the fertility effect of the introduction and modification of tax allowances in Quebec. He confirms substantial fertility increases of up to 25 percent for families eligible for the full amount of 8,000 Canadian dollars. Here, the effect appears to be largest in high income families.

Conversely, a number of contributions raise doubts regarding the responsiveness of fertility to financial incentives. Crump et al. (2011) revisit the studies by Whittington et al. (1990). Based on richer data and after testing various specifications they reject the hypothesis that child related tax benefits affect the level of fertility. Baughman and Dickert-Conlin (2009) evaluate whether changing incentives in the US EITC program increased fertility, but find the opposite response among white women and no significant effects for non-white women. Francesconi and van der Klaauw (2007) evaluate the UK WFTC program's effect on the fertility of lone mothers. Contrary to their hypotheses, they also find negative fertility responses. Brewer et al. (2012) studied the same reform and find that the fertility of women in couples indeed increased after the reform.

Two surveys summarize the literatures on welfare and family policy effects on fertility: Moffitt (1998) covers the literature on welfare in the United States and suggests that the findings on fertility effects are inconclusive. Gauthier (2007) summarizes the international literature on family policy effects on fertility and similarly concludes that the evidence is mixed and with small effects at best.

The German literature on child benefit effects on fertility entails two relevant contributions: Haan and Wrohlich (2011) estimate a structural, dynamic discrete choice model

of joint employment and fertility choices for women in couples. The authors apply monthly data from the 2000-2007 waves of the German Socio-Economic Panel (SOEP) and account for taxes, child care, child benefits, and out-of-work benefits. They find strong fertility responses to changes in child benefits. A hypothetical increase in child benefits by 20 percent for children under age three is associated with a significant fertility increase by 4.6 percent. The fertility effects are largest among the less educated, those in East Germany, and those without children.

Similar to the international literature, findings from the German literature do not agree: Rainer et al. (2012) evaluate the effect of the 1996 child benefit reform on several outcomes using a difference-in-differences approach. The authors use annual observations from the 1992-1998 SOEP waves, i.e., the years surrounding the 1996 child benefit reform. As the reform impact varied by income group, the authors define control and treatment groups based on a proxy for income, i.e., parental education. A positive reform effect appears at best among the low educated with no children; however, the effect is not robust.[1]

It is interesting that the ambiguous empirical findings on the relationship between child benefits and family size go along with indeterminate theoretical predictions. If we consider children as a 'normal good' the income effect of an increase in transfers should be positive and call forth higher fertility. The same positive fertility effect obtains if we consider child benefit increases to reduce the own price of children; this should generate increased demand and thus fertility. If, however, parents consider child quality and child quantity as substitutes (Becker 1991), the effect of an increase in non-labor income depends on whether the income elasticity of child quality exceeds that of child quantity. If that is the case, rising child benefits may even reduce fertility and increase investments in the child quality (Ermisch 2003, Gauthier 2007).

[1] Tamm (2010) studies the same reform with a focus on female labor market participation. He compares the labor supply of women in couples with (treatment group) and without (control group) children using data from the German Mikrozensus. Similar to, e.g., González (2013) he finds that the increase in benefits caused a decline in maternal hours of employment.

The contribution of this study is threefold: first, we contribute to the German literature with its contradictory findings by extending the analysis presented by Rainer et al. (2012). In particular, looking at evidence from the Mikrozensus in addition to SOEP data may generate a clearer picture than just using SOEP data. Second, we show a refined picture of how the reform influenced fertility by family size; this is an attempt to correctly represent the heterogeneous and complex incentives, which differ by family size and income. In particular, our hypothesis on the reform effect on second births is the opposite of that posed by Rainer et al. (2012).[2] Third, we add to the international literature by considering the case of a very low fertility country, where fertility responses to financial incentives may differ from those in, e.g., the US or the UK.

Our results confirm the findings of Rainer et al. (2012) in that there are no robust fertility effects of the reform for low income couples. We find some support for positive fertility effects for higher income couples deciding on a second birth. We provide various robustness tests, which corroborate our findings.

The structure of the paper is as follows: section 2 describes the child benefit reform of 1996 and the relevant institutional background; here we derive our two main hypotheses. Section 3 explains our empirical approach to test the causal fertility effect of the reform and introduces our data, which we take from the German Mikrozensus as well as the German Socioeconomic Panel. Section 4 presents our main results. Section 5 describes our robustness tests. Section 6 concludes.

2. Background and Hypotheses

The German Constitution mandates that families "shall enjoy the special protection of the state" (Article 6). One of the main policy instruments expressing this special protection is a transfer

[2] In addition, we consider strict definitions of the control and treatment groups, offer various robustness tests and omit observations where the pre- vs. post-reform assignment is unclear.

program labelled 'child benefit' (*Kindergeld*), which grew continuously in scope and scale, since the federal government established it in 1954. Initially, families only received 25 DM per month for the third child and for children of higher parity. Over time, the transfer was reformed in various ways: the benefits started to be provided to families with two children (1961) and with just one child (1975), benefit payout varied by income (1961-1975, 1982-1995), and transfers were raised and at times complemented by allowable income tax deductions for families with children. A decision of the German constitutional court motivated the 1996 reform of child benefits, which implemented the largest one time increase in these benefits ever. The German parliament passed the reform on Oct. 11, 1995 and it came into effect on Jan. 1, 1996. The reform integrated child benefits and the income tax system: prior to the reform, both systems complemented each other and applied simultaneously (dual system); since the reform, families have to choose between child benefits and tax deductions (option model). It depends on household income whether child benefits or tax deductions are preferable. As our analysis exploits this reform, we briefly characterize the regulations before and after the reform.

Prior to the reform, regulations on child related payments to parents contained three elements. The first element were *child benefits*, which amounted to 70 DM per month for a first child and varied by family income for higher order children (70-130 DM for a second, 70-220 DM for a third child, and 70-240 DM for fourth and higher order children). **Table 1** summarizes the child benefit amounts transferred for children of different parity over time. The second element were *income tax exempt amounts* of 4,104 DM to be deducted from taxable income per child and year. Due to the progressive income tax system, this saved parents between 0 and 181 DM per child and month; the amount increased with income.[3] The third element were *supplementary payments* to the child benefits; they amounted to between 1 and 65 DM per child and month, decreasing with income. They were to mitigate the advantage the tax exemptions

[3] Families with an annual income below 11,232 DM saved nothing, because they did not pay any income taxes anyways. Families with an annual income above 244,188 DM paid the maximum tax rate of 53% and thus saved 4,104 DM*0.53/12=181 DM per month and child.

granted to high income families. Overall, every family had a financial advantage of at least 135 DM per child and month, based on a combination of the three elements (Rainer et al. 2012).

After the 1996 reform, the child benefit supplement was abolished and child benefit payments no longer varied by income. The benefits amounted to 200 DM for the first and second child, 300 DM for the third, and 350 DM for the fourth child and any further children.[4] Furthermore, the reform increased the tax exempt amounts to 6,264 DM (see Lüdeke and Werding 1996). While parents could receive tax advantages plus child benefits prior to the reform, after the reform only the better of the two could be realized: families with low incomes benefited more from the child benefit payments whereas the tax exempt amounts per child were the more attractive option for higher income households due to progressive tax rates.[5]

While the reform generally increased the net value of child-related transfers for almost all families, the magnitude of the increase varied across the income distribution and the number of children. **Figure 1.1** depicts the total value of net child related transfers (i.e., child benefits, child benefit supplement, savings through tax deductions) before and after the reform for couples with one child. The gross income on the x-axis refers to the household income of married couples with no children.[6] Thus, the graph describes the reform-induced change in financial incentives for couples considering to have a first child.

The reform increased the net value of child related transfers almost across the entire income distribution. The only exception are couples with incomes between 143,400 and 152,500 DM, for whom net transfers decline slightly.[7] Couples with incomes above this interval

[4] The benefits rose even further in 1997 and again in 1999. By 1999 they had reached 250 DM for the first and second, 300 and 350 for the third and fourth child, respectively (cf. Rainer et al. 2012).

[5] It was not the family but the tax authority, which automatically chose the financially more attractive option for each individual family. Generally, gross family income for couples without children had to surpass about 146,500 DM after the reform to make the tax exempt amounts more attractive (about 153,000 DM for couples with one child). Based on our data, fewer than five percent of couple and family households were in this top income bracket. Therefore, the vast majority of families benefited from child benefits rather than the tax credit.

[6] As tax and child benefit laws differentiate between married and unmarried couples, the patterns for unmarried couples are slightly different.

[7] Net transfers dropped by between 9.50 DM and 18.85 DM per year, i.e. negligible amounts.

benefited from the increased tax exempt amounts, couples with incomes below this interval benefited from the increased direct transfers. Clearly, the largest absolute increase in net transfers occurred for low income couples: For couples with an income below 23,232 DM child benefits increased from 1,650 to 2,400 DM per year, i.e., by 45.45 percent.

Figure 1.2 shows net transfers relative to gross family income. At a gross income of, e.g., 20,000 DM the reform increased potential gains from 8.15% to 12% of income.[8] With rising income, the increase in relative net transfers declines. If children are a "normal good," the increase in net transfers should raise fertility. The effect should be largest for those with the largest increase in benefits. Net transfers for firstborn children increased the most for low-income families, both in absolute and relative terms. This leads us to our **first hypothesis**:

> We expect a positive fertility response to the reform among previously childless couples, which should be larger for couples with low than high incomes.

Figure 2.1 presents the net transfers couples could expect from a second child before and after the reform. The figure describes the expected net transfers due to a second child for couples who already have one child. For second children, the reform implied different shifts in incentives: after the reform, families with an income of up to 32,000 DM gain only slightly (up to 60 DM per year). Families with an income between 32,000 and 62,500 DM lose up to 190 DM per year in terms of net transfers for a second child. Couples with an income above the threshold of 62,500 DM were subject to discretionary income related reductions in child benefits before the reform. They gain, because the reform abolished these reductions. The increase in net transfers amounts to up to 520 DM per year.

Figure 2.2 depicts the net transfers due to a second child relative to household income. In the lower part of the income distribution net transfers declined with the largest drop at an

[8] The amount of 20,000 DM is not of any particular significance. The reform-induced increase is even higher at lower incomes, however, only few households are in this situation.

income level just below the threshold of 62,500 DM (i.e. at 62.400 DM), where it drops from 4.5 percent in 1995 to 3.85 percent in 1996. The largest relative increase in net transfers occurs slightly above the threshold at 63,800 DM, in particular from 2.94 percent of gross income in 1995 to 3.76 percent in 1996. Overall, the reform reduced net transfers for second born children as a share of household income for low-income families and increased them for high-income families. Note that overall these families' disposable income increased after the reform, as the increasing benefits for first born children overcompensate benefit reductions for second births (for a depiction see **Figure 2.3**). This leads to our **second hypothesis**:

> We expect an overall positive fertility response to the reform in terms of second born children. We expect this positive response to be larger for high-income than for low-income families.

Figure 3 shows the net transfers for a third child, from the perspective of a couple with two previous children. In this case, we cannot derive clear income based hypotheses about differential fertility responses for high- vs. low-income families, as the patterns are not clear. Net transfers increase for most families, but they do not rise monotonously with household income. Furthermore, families with an income between about 55,400 DM and 80,900 DM have a reduced fertility incentive after the reform. The situation is even more complicated for children of fourth and fifth parity. Therefore, we limit our analyses to the reform's fertility effect on first and second births.

Four additional institutional features and changes are relevant to our analyses (see Rainer et al. 2012): First, the number of years for which parents could claim child benefits increased in 1996 from 16 to 18 years.[9] If the reform yields heterogeneous fertility effects, then extending the payout period magnifies these effects, as it increases the net present value of net

[9] If children are in training parents can receive benefits even longer.

transfers and magnifies the difference in changes across income groups. Second, child benefits are considered in the means test for social assistance: when child benefits increase, welfare payments decline by the same amount. Therefore, the reform does not affect the fertility incentives of welfare recipients and we exclude them from our analyses.[10] Third, on January 1, 1996 a reform became effective that introduced a legal entitlement for a spot in a half-day kindergarten for three year olds. As this reform had been passed already in 1992 (*Kinder- und Jugendhilfegesetz* 1992) we do not expect to see specific fertility responses to this reform at the cutoff date of the child benefit reform in 1996. Fourth, a general income tax reform took effect along with the child benefit reform starting January 1, 1996. This reform decreased the tax burden, mostly for low-income earners (**Figure 4** presents the shift in marginal and average tax rates). In the next section, we describe our empirical strategy and explain how our strategy responds to the reforms in childcare and the income tax system, which occurred simultaneously with the child benefit reform. Finally, we describe the data we use to test our hypotheses.

3. Empirical Strategy, Identification, and Data Description

3.1 Empirical Strategy

Our strategy to identify the causal effect of the child benefit reform on fertility rests on the heterogeneity of the reform's effect for low- and high-income families. Following Becker (1991), we assume that the cost of a marginal child affects parents' desired number of children. In this framework, we take advantage of the heterogeneity of the reform effect, using a difference-in-differences approach.

Overall, we expect a positive fertility response to the reform, because child benefits increased for virtually every family. Regarding first (second) births, financial incentives increased most for low-income (high-income) families such that we expect a stronger response

[10] In principle, welfare recipients constitute a control group for the reform. However, as we observe only very few births in this subsample (56 overall), we only pursue this strategy as a robustness check.

of that group. We analyze reform effects separately for families without previous children and for families with one previous child.

As income is likely to be endogenous to fertility, we cannot use it to define treatment and control groups. Instead and in order to proxy for income, we use parents' education, which is arguably more exogenous to fertility. Couples with low vs. high educational attainment make up the treatment vs. control groups in the sample of childless couples. For the sample of couples with one child, we reverse the definition of treatment and control group, because in this case the high-income or highly educated couples experienced the largest increases in net transfers.

To isolate the treatment effect γ, we estimate a linear model of the form:

$$y_{it} = const + \alpha\, treat_{it} + \beta\, reform_{it} + \gamma(treat \cdot reform)_{it} + x_{it}'\delta + \varepsilon_{it} \quad . \quad (1)$$

The dependent variable y_{it} indicates whether couple i in period t has a recently born child (younger than one year) living in the household. The variable $treat$ denotes whether a couple belongs to the treatment group. The variable $reform$ yields whether the fertility decision occurred after the reform. The covariate vector x captures the fertility history of the mother and the socio-demographic characteristics of both parents. The coefficient of the interaction between $treat$ and $reform$ (γ) captures the treatment effect, if the identifying assumptions hold.

3.2 Identification Issues

In this section, we discuss four relevant challenges to our identification strategy: first, fertility responses in anticipation of the reform, second, the childcare reform, third, the income tax reform and fourth, uncertainty about the presence of parallel trends between lowly and highly educated couples.

The first potential problem could be that couples anticipated the reform. If they adjusted their fertility already prior to its implementation on January 1, 1996, this might bias the estimated reform effect. The German parliament passed the reform in October 1995

(*Bundeskindergeldgesetz* 1995). Thus, children conceived in (immediate) response to the new law could not have been born prior to June 1996. To describe pre-reform fertility outcomes we use data from the Mikrozensus waves 1995 and 1996. They were gathered each year in April and thus describe fertility decisions taken until July 1995.

However, the Bundestag first discussed the reform in March 1995 and its specifics were under debate until October. If (potential) parents adjusted their fertility choices already in anticipation and conceived a child as early as April 1995, births as early as January 1996 may already be in response to the reform. This is an unlikely outcome, though: first, it is uncommon that pregnancies occur immediately and, second, it would be surprising to see parents respond already to the possibility of a reform. However, if parents responded to the reform very early and anticipation occurred, we were to underestimate the true reform effect.

The second challenge to our identification strategy is the introduction of an entitlement to subsidized half-day childcare for children aged three and above. If this reform affected the fertility of our treatment and control groups differentially, our estimates might conflate the effects of the two reforms. This, however, is unlikely: while both reforms came into effect the same day, the German parliament passed the law introducing the childcare entitlement as early as summer of 1992 (*Kinder- und Jugendhilfegesetz* 1992). Therefore, fertility responses to this reform should have been realized already in the three years leading up to the enactment of the childcare reform. If this reform swayed any parents to have a child, the resulting children would have led to increased kindergarten enrollments of three year olds once the reform was in effect. As enrollment of three year olds stayed stable in the years after the enactment of the reform (BMFSFJ 2005), we argue that the childcare reform does not threaten our identification approach.

The third challenge to our identification strategy is the general income tax reform, which the Bundestag passed together with the child benefit reform. Just like the child benefit reform, the income tax reform increased the disposable income of low-income families while high-

income families were hardly affected (see **Figure 4** and Lüdeke and Werding 1996). Thus, we expect that the income tax reform also has a positive fertility effect, which decreases with income.

As we cannot control for this effect, we obtain an upward bias in our estimate of the treatment effect for childless couples. We cannot separate this bias from the treatment effect of the reform. However, the estimation of a significantly positive treatment effect would show that the fertility of the low-income group responds to changes in net income. The largest reduction of the tax burden from the income tax reform applied to couples with a taxable income of 24,100 DM. Their tax reductions amounted to about 2,500 DM while the increase in potential benefits from a first child amounted to 780 DM. Thus, 23.8% of the potential total income increase is conditional on having a first child. Therefore, about one quarter of the estimated effect is connected to the increase in child benefits.

The situation is reverse for the analysis of second births: here, the fertility response to the child benefit reform among high-income families should exceed that of low-income families. Any fertility effect of the general tax reduction for low-income families with a first child would generate a downward bias on the estimated differential effect of the child benefit reform. Here, difference-in-differences estimates thus constitute a conservative lower bound.

The fourth challenge to our identification strategy is to show that the development of fertility for treatment and control group developed along parallel paths in the periods prior to the reform (common trends assumption). **Figures 5.1** and **5.2** show birthrates of the treatment and control groups, which we categorize using two different definitions. The figures were compiled by the Federal Bureau of Statistics based on the full samples of the Mikrozensus waves 1991 to 2000.[11] The developments up until 1994 are not supportive of a common trend. The patterns since 1995 fulfill the requirements more plausibly.

[11] The Federal Bureau of Statistics has access to 100% of each Mikrozensus sample as compared to the 70% provided to us in scientific use files. In this version of the manuscript **Figures 5.1** and **5.2** use sample definitions that deviate from those applied in the analyses (to be corrected).

3.3 Data

3.3.1 Mikrozensus

Our first data source is the German Mikrozensus, which samples one percent of German households. We use the 1995, 1996, 1998, and 1999 waves of data. The scientific use files provide access to 70 percent of the overall sample.

We consider married and non-married cohabiting couples where the woman is at least 25 years old, i.e., at an age when most individuals have finished their education. We set the upper age limit for the women in the sample at 40 years, because fertility rates of older women are very small (for similar strategies see Rainer et al. 2012 and Brewer et al. 2012). We only consider West Germany, because in our observation period, right after unification, East Germany exhibited very unstable fertility patterns (Goldstein and Kreyenfeld 2011). We only keep couples where both partners are German nationals to limit the impact of heterogeneous fertility cultures (Cygan-Rehm 2014). We exclude welfare recipients because the reform did not change this group's income situation (*Bundessozialhilfegesetz* 1991). The size of our analysis sample varies depending on the definition of treatment and control groups.

The dependent variable describes whether there is a recently born child (younger than one year) living in the household.[12] We consider the observations from the Mikrozensus waves 1995 and 1996 as the pre-reform sample. Newly born children below age one observed in these surveys must have been conceived by July 1995 and therefore before the reform was passed. We observe post-reform fertility outcomes based on the Mikrozensus waves 1998 and 1999. Again, we consider children below age one in the household. The earliest possible date of conception for births observed 1998 is August 1996, well after the reform came into effect.[13]

[12] We observe only the year of birth and whether a newborn is in the household at the time of the interview, i.e., in the last week of April for every wave.

[13] We have no information on children not living with their mother.

Our sample thus covers births from April 1994 to April 1996 as pre- and from April 1997 to April 1999 as post-reform outcomes.

We use educational attainment of both partners based on the International Standard Classification of Education scale of 1997 (ISCED) to proxy the high- and low-income groups which make up our treatment and control groups. **Table 2** describes the ISCED coding in terms of school and vocational degrees. We apply two definitions of low vs. high education in our regressions: a narrow definition offers a precise approximation of the relevant income groups and a broad definition maintains reasonable sample sizes.

The *narrow definition* labels all couples where both partners have at most ISCED-level 2 as low education and all couples where both partners have ISCED-level 4 or higher as high education couples. Here, the highly educated couples clearly have higher incomes than the lowly educated: **Figure 6.1** shows the income distribution of both groups for couples without children. **Figure 6.2** shows the same for the couples with one previous child; here, the dashed line represents the turning point in terms of increasing or decreasing incentives for a second child in terms of net incomes.[14] 73.6% of the low education couples have an income below this threshold while 67.4% of the high education couples are above the threshold.

The *broad definition* of high and low education labels couples where both partners reach at most ISCED-level 3 as low education and couples where at least one partner reaches ISCED-level 4 or higher as high education couples. The advantage of this definition is that all observations can be assigned to a group. The disadvantage is that the incomes of the groups are less distinct than in the *narrow definition*. **Figures 6.3** and **6.4** show the income distributions for couples without children and for couples with one child, respectively.

Table 3 provides the sample sizes for both definitions and for couples without children and with one child. We can apply analysis samples of 11,385 (7,045) observations in the narrow

[14] This threshold lies at gross household incomes if 62,500 DM. Since we only have data on net income in the Mikrozensus, we provide the income distribution in terms of net income, for which the threshold is roughly at 53,000 DM.

education definition and of 36,304 (33,173) in the broad definition for couples at risk of a first (second) birth, respectively. Births of firstborn children declined after the reform and did so more strongly for the lowly educated. Births of second born children increased for all groups except the lowly educated in the *narrow definition*.

Our estimations consider a vector of control variables. These capture time since last birth (for families with a previous child) and age and marital status of both parents. We lag these variables by one year, to capture parents' characteristics when they decide to conceive a child. Furthermore, we control for the year of observation, the federal state, the size of the community the couple lives in, and each partner's educational status measured by ISCED-classification. We show descriptive statistics on the full set of covariates in **Appendix Tables A.1** and **A.2.**

3.3.2 German Socioeconomic Panel (SOEP)

Our second data source is the German Socioeconomic Panel, a long running household survey (Wagner et al. 1997). The SOEP provides smaller samples than the Mikrozensus, but offers full fertility biographies of women. We use the 1992-1998 survey waves of the SOEP and generate a data set, which is comparable to that based on the Mikrozensus.

Again, we consider cohabiting couples in West Germany. We exclude couples if one of the partners has a migration background.[15] We drop welfare recipients and couples where the female is outside the age range of 25-40 years. This provides us with an analysis samples of 347 (344) observations in the narrow education definition and of 1,404 (1,660) in the broad definition for couples at risk of a first (second) birth, respectively.

Again, our dependent variable describes couples' fertility outcomes. While in the Mikrozensus we observe whether there is a child in the household and impute parental

[15] In the Mikrozensus data we dropped non-nationals because there is no information on the more appropriate indicator of migration background, which we can apply with the SOEP data.

characteristics at the time of conception in the past, the SOEP allows us to connect observations on families over time. We combine a couple's characteristics at the time of the interview with their subsequent fertility outcomes, specifically 12-23 months after an observation. While the Mikrozensus data describe families and their births by the time of the survey, the SOEP data describe couples at a period of potential family planning and match their future fertility outcomes.[16]

Given that the reform was first discussed in March 1995 and the law passed parliament in October 1995 to take effect January 1, 1996, conceptions between March and October 1995 (i.e., births between about December 1995 and June 1996) fall into a transition period. As to the Mikrozensus data, we do not use the survey of 1997 which covers also 1996 births because due to missing information on the month of birth we do not know whether a child was conceived before or after the reform. In the SOEP data, we have information about month of birth and month of interview. Here, we drop all observations of interviews conducted between March and October 1995 and the subsequent fertility outcomes as this describes the transition period, which is neither clearly pre- nor post-reform.[17]

As before, we define treatment and control groups based on the ISCED education scale. **Table 4** provides the sample sizes and birthrates pre- and post-reform for the two different definitions of high and low education. Again, our covariates include both parents' age and education, federal state and couples' marital status. Additionally, we control for the month and year of the interview, and homeownership. The only variable available in the Mikrozensus but not in the SOEP is city size. **Appendix Tables A.3** and **A.4** show the descriptive statistics of all covariates, except for years and interview months**.**

[16] To test whether this difference between data sets affects results, we ran regressions on SOEP data, after copying the measurement approach of the Mikrozensus. The results yield no noteworthy differences.

[17] We also tried extending the period of possible anticipation to up to one year, i.e. dropping all observations of 1995. Except for increasing the standard errors, this did not change the results.

4. Results

Tables 5 to **8** show the estimation results of our difference-in-differences models for first and second births based on the Mikrozensus and SOEP data, respectively. In each table, we describe the findings based on two alternative definitions of treatment and control groups.

Table 5 presents the estimation results for couples with no previous children. In columns 1 and 2, we apply the *narrow definition* of educational attainment. The coefficients show virtually no overall change in fertility in the post reform period, and a generally lower fertility for low compared to highly educated couples. This difference is statistically significant only when the model does not consider additional control variables.

Our main interest rests on the coefficient of the interaction of low education and post reform indicators, i.e., the estimate of the causal effect of the child benefit increase. Contrary to our expectation of a positive treatment effect, the coefficient is negative and statistically significant in both columns, i.e. with and without additional controls. In columns 3 and 4 of **Table 5**, we use the *broad definition* of educational attainment with its large sample size. Again, we observe no general increase in fertility after the reform, a negative fertility-education correlation without and a positive one with control variables. The coefficient of the interaction term again does not yield the expected positive fertility effect for the lower income couples after the reform. It is small and statistically insignificant. Thus, the Mikrozensus data yield no evidence to support our first hypothesis.

Table 6 presents the Mikrozensus based estimation results for reform effects on the propensity of a second birth. In columns 1 and 2, we apply the *narrow definition*. Contrary to our expectation, the estimated coefficient of the post reform indicator is again negative and statistically significant. The treatment effect is positive, of a similar magnitude and statistically significant. This could indicate a general fertility reduction after the reform, which the reform mitigated for the highly educated.

The size of the treatment effect appears plausible. After the reform, the fertility of highly educated couples with one child remains fairly stable, while that of low educated couples with one child drops by 4.6 percentage points. The estimations in columns 3 and 4, which apply the alternative definition of control and treatment groups yield weaker results: however, when control variables are considered the interaction term again yields statistically significant positive yet smaller coefficients. Overall, the results based on Mikrozensus data agree with our second hypothesis from section 2.

Tables 7 and **8** show the results based on SOEP data. Now, sample sizes are reduced by at least a factor twenty compared to the Mikrozensus data. The main effects of the reform on the probability of having a first birth (row one in **Table 7**) confirm the results from the Mikrozensus: there is no support for the hypothesis that the reform yielded a significant general fertility effect. Lower educated couples tend to have fewer first births than higher educated couples, however, we find no support for hypothesis one as the interaction terms are statistically insignificant. **Table 8** shows the results of SOEP based tests of hypothesis two. While the propensity to have a second child increased after the reform we find no evidence that the reform yielded a significant positive fertility effect for the highly educated with one previous child. All estimated treatment effects in **Table 8** are negative and statistically insignificant.

The estimations based on SOEP data confirm the findings of Rainer et al. (2013) who find no robust reform effects using this data source. Overall, we obtain no support for hypothesis one and some support for hypothesis two based on the Mikrozensus samples. Next, we discuss a set of robustness tests for these findings.

5. Discussion and Robustness Tests

We performed numerous robustness tests on both datasets which confirm that there are no significantly positive overall fertility responses to the child benefit reform and that there is no convincing support for hypothesis one. We present three additional results:

First, **Table 9** presents the estimation results when the education proxies are replaced by measures of household income. Clearly, these results may not present consistent estimates of the reform effect, but if the endogeneity of the income measure generates an upward bias, we expect to find positive reform "effects" in this specification. We contrast couples in the lowest income quartile with those in the highest quartile. In the first two columns we consider only couples without prior children; here, the low income couples are the treatment group. In columns 3 and 4, we only consider couples with one prior child. Here, the high income couples are the treatment group. The results display no rise in fertility after the reform (see first row) and a negative correlation of income with fertility. None of the four models yields statistically significant treatment effects, i.e., the estimates of all 'income by post reform' interactions are statistically insignificant. This confirms that the reform most likely did not yield substantial fertility effects.

A second robustness test focuses on our sample selection. We originally omitted couples from East Germany, immigrants, and those with mothers younger than 25 or older than 40. In **Tables 10** and **11** we present the results of estimations based on Mikrozensus data when these sample restrictions are lifted.[18] The results for couples without prior children in **Table 10** confirm the previous findings using the main sample. As in **Table 5**, the treatment effect is negative and statistically significant for the *narrow definition* of treatment and control group and small and insignificant for the *broad definition* in columns 3 and 4. Thus, the result is robust against changes in the selection of the analysis sample.

Table 11 presents the estimates for the reform effect on second births. Columns 1 and 2 yield the expected positive effect for high education couples, but the effect is statistically insignificant and its magnitude is only about one third, as compared to the estimates from sample in the main regressions (see **Table 6**). The estimated reform effects in columns 3 and 4

[18] We also generated the estimation results when each of the limitations was restricted individually. The results are very similar.

are comparable in size to those in the main regressions and have smaller standard errors, corroborating prior results. Thus, we continue to find evidence in favor of a positive reform effect on second births, which appears to be robust to the selection of the sample.

Finally, we compare reform effects for households with and without welfare receipt.[19] This is of interest, as couples who received welfare were unaffected by the reform: any child benefit increases were deducted from their welfare payments and net transfers remained constant. In **Table 12,** we study the response of parents without prior children. Here, we expect the positive treatment effect in the low education group. In columns 1 and 2 of **Table 12**, we only consider low educated households based on the *narrow definition* and compare welfare recipients, which are newly added to the sample, to non-recipients. the sample. We observe in row 2 that compared to low educated welfare recipients the low educated non-recipients generally have a significantly higher propensity to have a first birth. Contrary to our expectation, the estimate of the treatment effect in row 3 is negative, statistically significant, and large. However, these results are based on a rather small sample, which comprises only 99 welfare recipients and among them occurs only a single birth over all four years of observations.

In columns 3 and 4 of **Table 12** we repeat this analysis using the *broad definition* of educational attainment. In this case, the overall sample size increases by factor ten and we observe 232 welfare recipients with 13 births. Here, we find neither a significant difference in fertility between recipients and non-recipients, nor a significant treatment effect.

In columns 5 and 6 of **Table 12**, we consider all couples without children, regardless of education. We expect that overall the child benefit reform increased incentives for additional births among non-recipients, while welfare recipients' fertility remained unaffected. However,

[19] This comparison is not plausible for couples with one child where we expect the positive treatment effect among those with high income. As high education and high income couples are not likely to be on (means-tested) welfare, we do not use the additional control group of welfare recipients to test our second hypothesis.

again all coefficient estimates of interest are statistically insignificant which suggests that those affected by the reform do not modify their fertility compared to those not affected.

6. Conclusions

In this paper, we apply a difference-in-differences approach to identify the causal effect of an increase in child benefits on fertility in Germany. We use the heterogeneity of the reform across population groups to identify treatment and control groups and differentiate couples who are more and less strongly affected by the reform. From a theoretical perspective the treatment of vast increases in child benefits can be expected to yield positive fertility effects particularly for low income parents deciding on a first birth and for high income parents deciding on a second birth; increased transfers reduced the cost of additional children more for these parents than for others.

We consider data from the German Mikrozensus and the German Socioeconomic Panel (SOEP) and approximate treatment and control groups based on educational attainment. In our main analyses, we do not find a positive treatment effect for low income parents deciding on a first child. We find some evidence of a positive fertility effect for high income couples deciding on a second child based on Mikrozensus data. We performed numerous robustness tests, which confirm our findings.

In contrast to some of the literature, our analysis does not yield convincing confirmation of a general fertility incentive provided by child benefit payments. This may indicate that the amount of payments did not change sufficiently. Alternatively, the difference of the reform impact for treatment and control groups might be insufficient. We can rule out that mismeasurement connected to using income proxies drives the lack of an effect: when we define treatment and control groups directly based on income we do not find significant correlation patterns, either. Also, it seems unlikely that the lack of a strong fertility effect is driven by a child quality-quantity tradeoff, where additional income is invested in the quality rather the

quantity of children: first, this tradeoff should not be relevant for decisions on first births. Second, we find weak evidence supporting an increase of fertility regarding second births in response to the reform.

Short-term fertility changes - as we observe them for second births of highly educated parents - may be due to shifts in the timing (e.g., anticipation or postponement) or in the spacing of births (e.g., shorter or longer intervals between births) rather than overall causal fertility effects. We have no evidence that such timing and spacing effects matter for the German child benefit reform studied here. However, we plan to provide additional tests: in particular, we will investigate whether we find a negative time trend in the propensity of second births after the reform, which may indicate a postponement of earlier planned births until the reform became effective. We will investigate whether the age of second time mothers changed over time, where older mothers similarly might indicate a postponement of births in expectation of the child benefit reform. At the same time, observing younger mothers may reflect adjustments in birth spacing that may originate in the reform. Rainer et al. (2012) offer analyses of timing and spacing based on SOEP data and find no evidence of timing and spacing adjustments in response to the reform.

Overall, not finding strong fertility effects is an important result for family policy, which often relies on cash transfers to secure the financial wellbeing of young families and to incentivize childbearing. Given the substantial resources invested in child and family support, e.g. in the case of Germany, it is important to know that they are not likely to be effective in incentivizing first births and that they are particularly ineffective for low income couples.

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Table 1. Level of child benefits over time

Year	1. Child	2. Child	3. Child	4. Child and further
1992-1993+	70	70 - 130*	140 - 220*	140 - 220*
1994-1995+	70	70 - 130*	70 - 220*	70 - 240*
1996	200	200	300	350
1997-1998	220	220	300	350
1999	250	250	300	350
2000-2001	270	270	300	350

Notes: Benefits in DM per month; + marks the years in which the *supplement payments* of up to 65 DM per month were paid to low income families; * marks income dependent cuts in *child benefits* applied.
Source: BMF 2008.

Table 2. The ISCED 1997-Scale

School degree	Vocational/University Degree			
	No degree	Vocational degree	Higher vocational degree	University degree
No degree	1	3	5	6
High school diploma	2	3	5	6
Secondary school diploma	3	4	5	6

Source: Based on Schroedter et al. 2006 and the ISCED-1997 coding in the SOEP.

Table 3. Descriptive statistics of births for couples without children and with one child from the Mikrozensus by educational group

	Couples without children			
	Narrow definition		*Broad definition*	
	Low	*High*	*Low*	*High*
N	1,567	9,818	16,522	19,782
Births	107	1,104	1,644	2,168
Birthrate prereform	9.01%	11.57%	10.14%	11.15%
Birthrate postreform	5.12%	10.96%	9.75%	10.78%

	Couples with one child			
	Narrow definition		*Broad definition*	
	Low	*High*	*Low*	*High*
N	1,754	5,291	19,428	13,745
Births	146	884	1,858	1,947
Birthrate prereform	9.91%	16.36%	9.42%	13.51%
Birthrate postreform	6.77%	17.02%	9.72%	14.80%

Source: Own calculations based on the Mikrozensus waves 1995, 1996, 1998, and 1999.

Table 4. Descriptive statistics of births for couples without children and with one child from the SOEP by educational group

	Couples without children			
	Narrow definition		*Broad definition*	
	Low	*High*	*Low*	*High*
N	14	301	580	691
Births	2	32	52	82
Birthrate prereform	16.67%	10.41%	9.21%	11.82%
Birthrate postreform	12.50%	10.83%	8.68%	11.90%

	Couples with one child			
	Narrow definition		*Broad definition*	
	Low	*High*	*Low*	*High*
N	55	257	869	648
Births	3	30	84	79
Birthrate prereform	3.70%	13.33%	8.37%	12.50%
Birthrate postreform	7.14%	10.22%	11.17%	11.90%

Source: Own calculations based on the SOEP waves 1992 to 1998.

Table 5. Estimation results: reform effect on the propensity to have a first child based on Mikrozensus data

	1 High vs. Low Education (Narrow Definition)	2 High vs. Low Education (Narrow Definition)	3 High vs. Low Education (Broad Definition)	4 High vs. Low Education (Broad Definition)
Post Reform	-0.006	-0.008	-0.003	-0.006
	(0.006)	(0.008)	(0.004)	(0.005)
Low Education	-0.025**	-0.002	-0.010**	0.011*
	(0.012)	(0.027)	(0.005)	(0.006)
Low Educ.*Post Reform	-0.033**	-0.025*	-0.001	0.001
	(0.015)	(0.015)	(0.006)	(0.006)
ISCED-Level Mother	-	-0.007**	-	0.001
		(0.004)		(0.001)
ISCED-Level Partner	-	0.010**	-	0.006***
		(0.004)		(0.001)
Married in t-1	-	0.106***	-	0.090***
		(0.007)		(0.004)
Additional Controls	No	Yes	No	Yes
N	11,385	11,385	36,304	36,304
R^2	0.0030	0.0494	0.0003	0.0493

Notes: Robust S.E. in parentheses, * p<0.10, ** p<0.05, *** p<0.010. The dependent variable in all models is *birth in t*. Columns 1 and 2 show the results for the narrow definition of low and high education, with and without control variables, respectively. Columns 3 and 4 show the same for the broad definition of low and high education. The regressions with controls (columns 2 and 4) include, additionally to the displayed regressor variables, dummies for mother's and partner's lagged age in steps of 4 years, federal state, year of observation and city size.
Source: Mikrozensus waves of 1995, 1996, 1998, and 1999, own calculations. The sample is restricted to West German cohabiting couples without children, where the woman is between 25 and 40 years old in t-1. Moreover excluded are couples where one partner is not a German national, welfare recipients and couples whose educational level does not fit into any of the groups contrasted in the respective model.

Table 6. Estimation results: reform effect on the propensity to have a second child based on Mikrozensus data

	1 High vs. Low Education (Narrow Definition)	2 High vs. Low Education (Narrow Definition)	3 High vs. Low Education (Broad Definition)	4 High vs. Low Education (Broad Definition)
Post Reform	-0.031**	-0.046***	0.003	0.003
	(0.013)	(0.015)	(0.004)	(0.005)
High Education	0.066***	-0.074*	0.041***	-0.011
	(0.013)	(0.038)	(0.005)	(0.007)
High Educ.*Post Reform	0.036**	0.037**	0.010	0.013*
	(0.017)	(0.016)	(0.007)	(0.007)
ISCED-Level Mother	-	0.002	-	0.006***
		(0.006)		(0.001)
ISCED-Level Partner	-	0.017***	-	0.010***
		(0.006)		(0.002)
Married in t-1	-	0.034***	-	0.017***
		(0.009)		(0.004)
Additional Controls	No	Yes	No	Yes
N	7,045	7,045	33,173	33,173
R^2	0.0111	0.1312	0.0053	0.1096

Notes: Robust S.E. in parentheses, * p<0.10, ** p<0.05, *** p<0.010. The dependent variable in all models is *birth in t*. Columns 1 and 2 show the results for the narrow definition of low and high education, with and without control variables, respectively. Columns 3 and 4 show the same for the broad definition of low and high education. The regressions with controls (columns 2 and 4) include, additionally to the displayed regressor variables, dummies for mother's and partner's lagged age in steps of 4 years, federal state, year of observation, city size, the time passed since the woman's first birth.
Source: See **Table 5**.

Table 7. Estimation results: reform effect on the propensity to have a first child based on SOEP data

	1 High vs. Low Education (Narrow Definition)	2 High vs. Low Education (Narrow Definition)	3 High vs. Low Education (Broad Definition)	4 High vs. Low Education (Broad Definition)
Post Reform	-0.043	-0.075	-0.014	-0.077*
	(0.044)	(0.093)	(0.027)	(0.043)
Low education	0.008	-0.265	-0.039	-0.034
	(0.104)	(0.170)	(0.026)	(0.033)
Low educ.*Post Reform	-0.018	0.068	0.034	0.046
	(0.138)	(0.132)	(0.040)	(0.038)
ISCED-Level Mother	-	-0.087**	-	-0.008
		(0.038)		(0.009)
ISCED-Level Partner	-	0.009	-	0.015*
		(0.038)		(0.008)
Married	-	0.112**	-	0.086***
		(0.052)		(0.023)
Homeowner	-	-0.092	-	-0.032
		(0.071)		(0.022)
Additional Controls	No	Yes	No	Yes
N	315	315	1,271	1,271
R^2	0.0049	0.1904	0.0024	0.0957

Notes: Robust S.E., clustered by person, in parentheses, * p<0.10, ** p<0.05, *** p<0.010. The dependent variable in all models is *birth in t+1*. Columns 1 and 2 show the results for the narrow definition of low and high education, with and without control variables, respectively. Columns 3 and 4 show the same for the broad definition of low and high education. The regressions with controls (columns 2 and 4) include, additionally to the displayed regressor variables, dummies for homeownership, year and month of observation, mother's and partner's age in steps of 4 years and federal state.

Source: SOEP waves 1992 to 1998, own calculations. The sample is restricted to West German cohabiting couples without children, where the woman is between 25 and 40 years old. Moreover excluded are couples in which one partner has a migration background, welfare recipients and couples whose educational level does not fit into any of the groups contrasted in the respective model. Furthermore excluded are observations from the period March to October 1995, in which it is not clear if couples might have anticipated the reform or not.

Table 8. Estimation results: reform effect on the propensity to have a second child based on SOEP data

	1 High vs. Low Education (Narrow Definition)	2 High vs. Low Education (Narrow Definition)	3 High vs. Low Education (Broad Definition)	4 High vs. Low Education (Broad Definition)
Post Reform	0.206	0.152*	0.052*	0.065*
	(0.161)	(0.087)	(0.028)	(0.035)
High Education	0.101***	0.103	0.037	0.010
	(0.035)	(0.137)	(0.023)	(0.041)
High Ed.*Post Reform	-0.229	-0.107	-0.048	-0.056
	(0.166)	(0.082)	(0.038)	(0.035)
ISCED-Level Mother	-	0.021	-	0.016*
		(0.030)		(0.009)
ISCED-Level Partner	-	-0.022	-	-0.001
		(0.032)		(0.013)
Married	-	0.131***	-	0.055**
		(0.040)		(0.026)
Homeowner	-	-0.114***	-	-0.003
		(0.039)		(0.020)
Additional Controls	No	Yes	No	Yes
N	312	312	1517	1517
R^2	0.0266	0.2373	0.0049	0.0900

Notes: Robust S.E., clustered by person, in parentheses, * p<0.10, ** p<0.05, *** p<0.010. The dependent variable in all models is *birth in t+1*. Columns 1 and 2 show the results for the narrow definition of low and high education, with and without control variables, respectively. Columns 3 and 4 show the same for the broad definition of low and high education. The regressions with controls (columns 2 and 4) include, additionally to the displayed regressor variables, dummies for homeownership, year and month of observation, mother's and partner's age in steps of 4 years and federal state.
Source: See **Table 7**.

Table 9. Estimation results: reform effect on the propensity to have a first or second child based on Mikrozensus data; treatment and control groups defined by high vs. low household income

	1 High vs. Low Income No Children	2 High vs. Low Income No Children	3 High vs. Low Income One Child	4 High vs. Low Income One Child
Post Reform	-0.004	-0.004	0.000	0.005
	(0.005)	(0.007)	(0.007)	(0.008)
Low Income	0.152***	0.192***	-	-
	(0.007)	(0.009)		
Low Inc.*Post Reform	0.003	0.002	-	-
	(0.010)	(0.010)		
High Income	-	-	-0.041***	-0.026***
			(0.006)	(0.009)
High Inc.*Post Reform	-	-	0.013	0.005
			(0.010)	(0.009)
Income in TDM	-	0.001***	-	-0.000
		(0.000)		(0.000)
ISCED-Level Mother	-	0.000	-	0.006***
		(0.001)		(0.002)
ISCED-Level Partner	-	0.002	-	0.010***
		(0.001)		(0.001)
Married in t-1	-	0.103***	-	0.011*
		(0.005)		(0.006)
Additional Controls	No	Yes	No	Yes
N	19,177	17,850	18,006	16,817
R^2	0.0516	0.1206	0.0032	0.1089

Notes: Robust S.E. in parentheses, * p<0.10, ** p<0.05, *** p<0.010. The dependent variable in all models is *birth in t*. "Low Income" and "High Income" are dummies, indicating that a family is in the lowest or highest income quartile in the sample. The middle quartiles are not part of the estimation. Columns 1 and 2 show the results for families without children, with and without control variables, respectively. Columns 3 and 4 show the same for families with one child. The regressions with controls (columns 2 and 4) include, additionally to the displayed regressor variables, dummies for mother's and partner's lagged age in steps of 4 years, federal state and city size. Column (4) additionally considers dummies representing the time passed since the woman's first birth. The regressor "Income in TDM" is yearly family net income in thousand DM.
Source: See **Table 5.**

Table 10. Estimation results: reform effect on the propensity to have a first child based on Mikrozensus data with an extended sample

	1 High vs. Low Education (Narrow Definition)	2 High vs. Low Education (Narrow Definition)	3 High vs. Low Education (Broad Definition)	4 High vs. Low Education (Broad Definition)
Post Reform	0.001	-0.001	0.002	0.004
	(0.004)	(0.005)	(0.003)	(0.003)
Low Education	0.006	0.006	-0.000	0.007*
	(0.006)	(0.017)	(0.002)	(0.003)
Low Educ.*Post Reform	-0.023***	-0.016*	-0.005	-0.004
	(0.008)	(0.008)	(0.003)	(0.003)
ISCED-Level Mother	-	-0.007***	-	-0.003***
		(0.002)		(0.001)
ISCED-Level Partner	-	0.003	-	0.001*
		(0.003)		(0.001)
Married in t-1	-	0.090***	-	0.077***
		(0.004)		(0.002)
Additional Controls	No	Yes	No	Yes
N	23,666	23,666	90,673	90,673
R^2	0.0005	0.0699	0.0001	0.0732

Notes: Robust S.E. in parentheses, * p<0.10, ** p<0.05, *** p<0.010. The dependent variable in all models is *birth in t*. Columns 1 and 2 show the results for the narrow definition of low and high education, with and without control variables, respectively. Columns 3 and 4 show the same for the broad definition of low and high education. The regressions with controls (columns 2 and 4) include, additionally to the displayed regressor variables, dummies for mother's and partner's lagged age in steps of 4 years, federal state, year of observation and city size.
Source: Mikrozensus waves of 1995, 1996, 1998 and 1999, own calculations. The sample is restricted to cohabiting couples without children, where the woman is between 18 and 49 years old in t-1. Moreover excluded are welfare recipients and couples whose educational level does not fit into any of the groups contrasted in the respective model.

Table 11. Estimation results: reform effect on the propensity to have a second child based on Mikrozensus data with an extended sample

	1 High vs. Low Education (Narrow Definition)	2 High vs. Low Education (Narrow Definition)	3 High vs. Low Education (Broad Definition)	4 High vs. Low Education (Broad Definition)
Post Reform	-0.007	-0.008	-0.000	0.006**
	(0.007)	(0.008)	(0.002)	(0.003)
High Education	0.003	0.009	0.007***	0.001
	(0.006)	(0.019)	(0.003)	(0.004)
High Educ.*Post Reform	0.013	0.012	0.010***	0.010***
	(0.009)	(0.008)	(0.004)	(0.003)
ISCED-Level Mother	-	-0.008**	-	-0.001
		(0.003)		(0.001)
ISCED-Level Partner	-	0.008**	-	0.004***
		(0.004)		(0.001)
Married in t-1	-	0.019***	-	0.016***
		(0.005)		(0.002)
Additional Controls	No	Yes	No	Yes
N	18,779	18,779	84,716	84,716
R^2	0.0004	0.1378	0.0007	0.1226

Notes: Robust S.E. in parentheses, * p<0.10, ** p<0.05, *** p<0.010. The dependent variable in all models is *birth in t*. Columns 1 and 2 show the results for the narrow definition of low and high education, with and without control variables, respectively. Columns 3 and 4 show the same for the broad definition of low and high education. The regressions with controls (columns 2 and 4) include, additionally to the displayed regressor variables, dummies for mother's and partner's lagged age in steps of 4 years, federal state, year of observation, city size, the time passed since the woman's first birth.
Source: See **Table 10.**

Table 12. Estimation results: reform effect on the propensity to have a first child based on Mikrozensus data considering welfare vs. nonwelfare recipients

	1	2	3	4	5	6
	Welfare vs. No Welfare Sample: Lowly Educated (Narrow Defintion)	Welfare vs. No Welfare Sample: Lowly Educated (Narrow Defintion)	Welfare vs. No Welfare Sample: Lowly Educated (Broad Defintion)	Welfare vs. No Welfare Sample: Lowly Educated (Broad Defintion)	Welfare vs. No Welfare Sample: All Observations	Welfare vs. No Welfare Sample: All Observations
Post Reform	0.019	0.049*	-0.013	0.007	-0.037	-0.029
	(0.019)	(0.028)	(0.031)	(0.031)	(0.033)	(0.033)
No welfare recipient	0.090***	0.087***	0.038	-0.007	-0.001	-0.038
	(0.011)	(0.016)	(0.023)	(0.023)	(0.026)	(0.026)
No welf. rec.*Post Reform	-0.058**	-0.084***	0.008	-0.008	0.034	0.024
	(0.023)	(0.027)	(0.031)	(0.031)	(0.033)	(0.033)
ISCED-Level Mother	-	0.001	-	0.005*	-	0.000
		(0.010)		(0.003)		(0.001)
ISCED-Level Partner	-	0.002	-	0.007**	-	0.005***
		(0.010)		(0.003)		(0.001)
Married in t-1	-	0.053***	-	0.074***	-	0.090***
		(0.013)		(0.005)		(0.004)
Additional Controls	No	Yes	No	Yes	No	Yes
N	1,666	1,666	16,115	16,115	36,605	36,605
R^2	0.0089	0.0852	0.0003	0.0599	0.0001	0.0491

Notes: Robust S.E. in parentheses, * p<0.10, ** p<0.05, *** p<0.010. The dependent variable in all models is *birth in t*. Columns 1 and 2 show the results for couples with low education in the narrow definition, with and without controls, respectively. Here the control group are low educated welfare recipients and the treatment group are the low educated who do not receive welfare. Columns 3 and 4 show the same for couples with low education in the broad definition and columns 5 and 6 for all couples, regardless of education. The regressions with controls (columns 2, 4 and 6) include, additionally to the displayed regressor variables, dummies for mother's and partner's lagged age in steps of 4 years, federal state, year of observation and city size.
Source: See **Table 5**.

Figure 1.1. Absolute net transfer for a first child

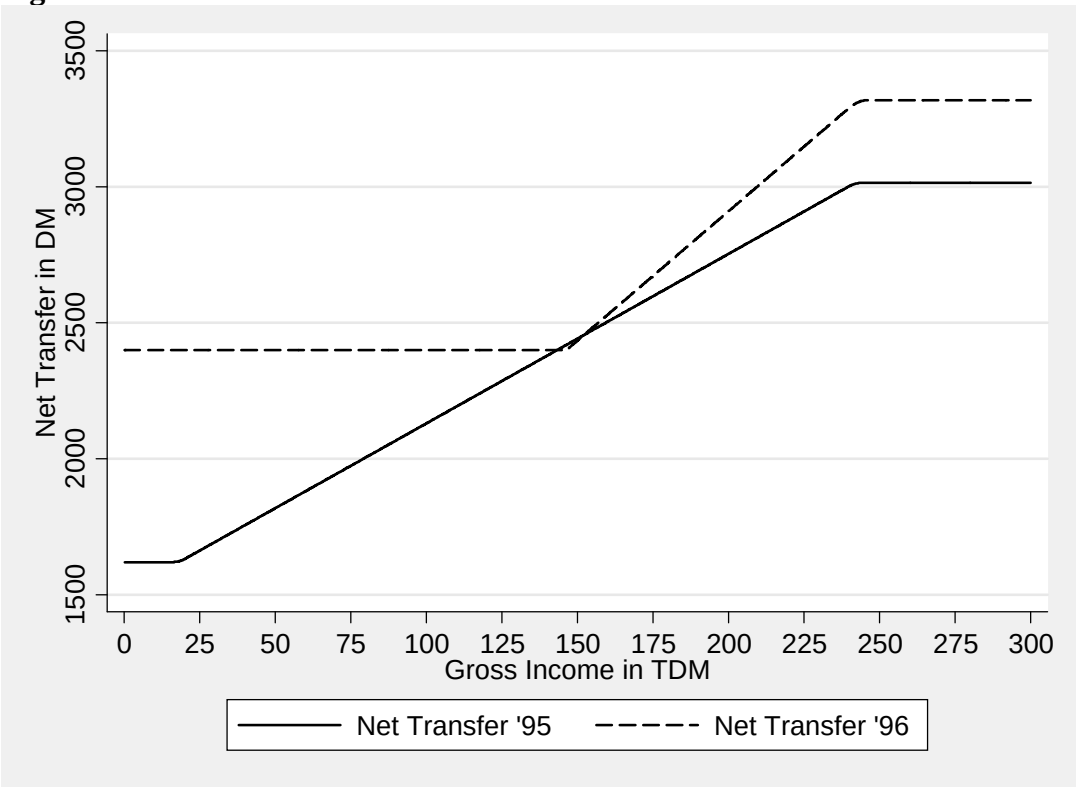
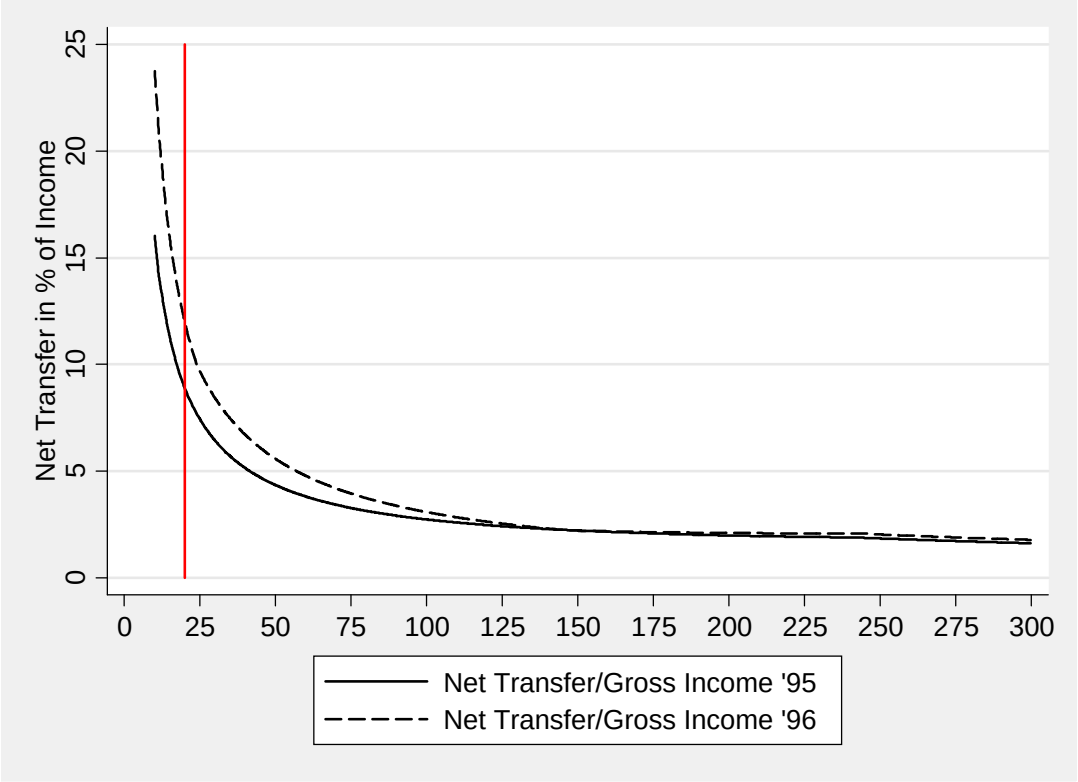


Figure 1.2. Relative net transfer for a first child



Notes: The gross income refers to yearly gross household income. Net transfer stands for the combined amount of taxes a married couple with no previous children would save and/or child benefits and child benefit supplements the couple would receive if the family had a first child. The red line marks the example of 20,000 DM, given in the text (section 2).
Source: Own calculations.

Figure 2.1. Absolute net transfer for a second child

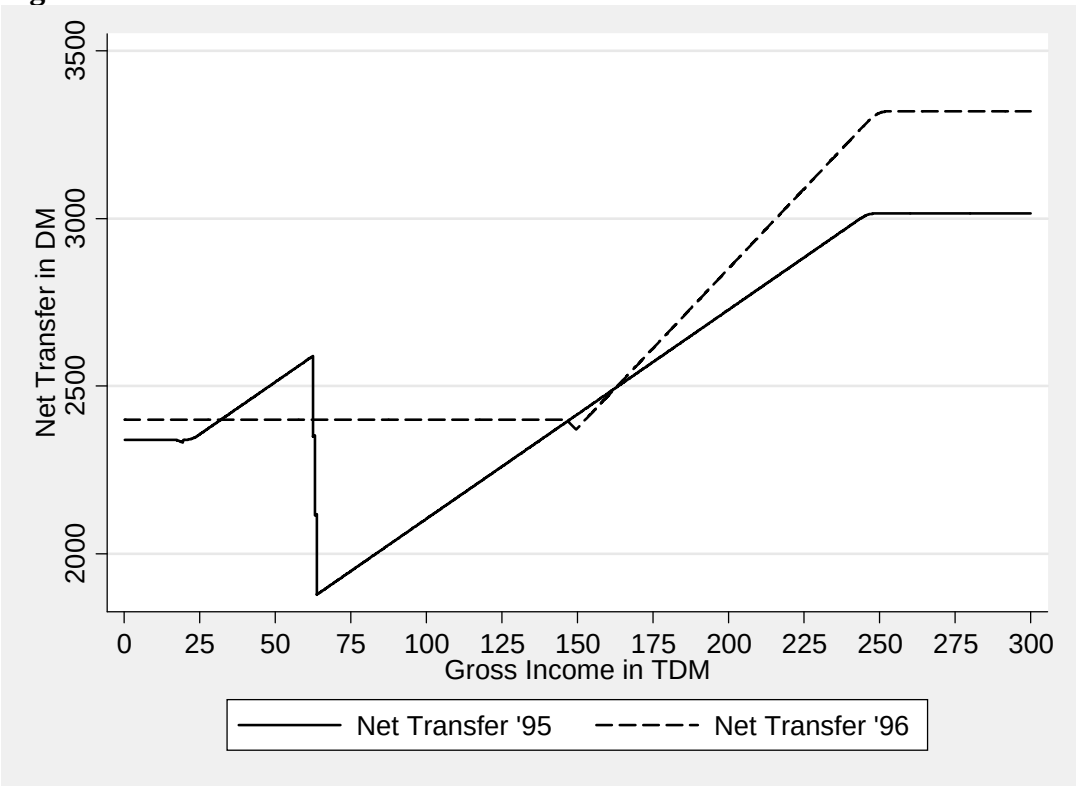


Figure 2.2. Relative net transfer for a second child

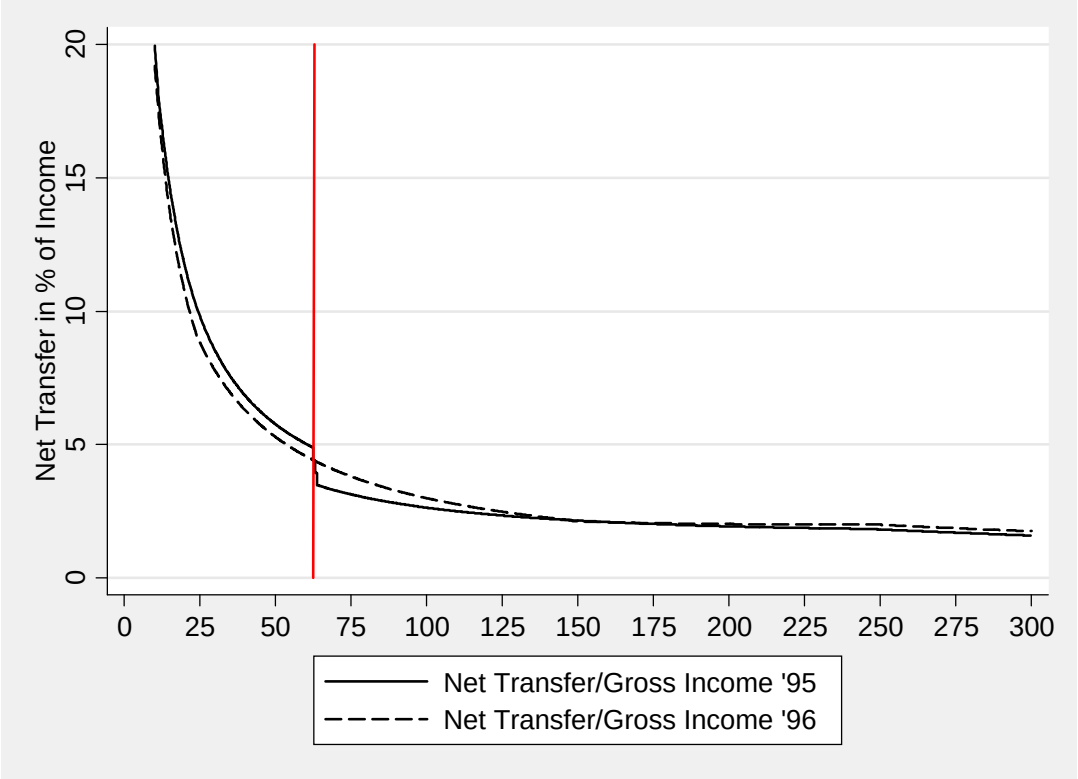
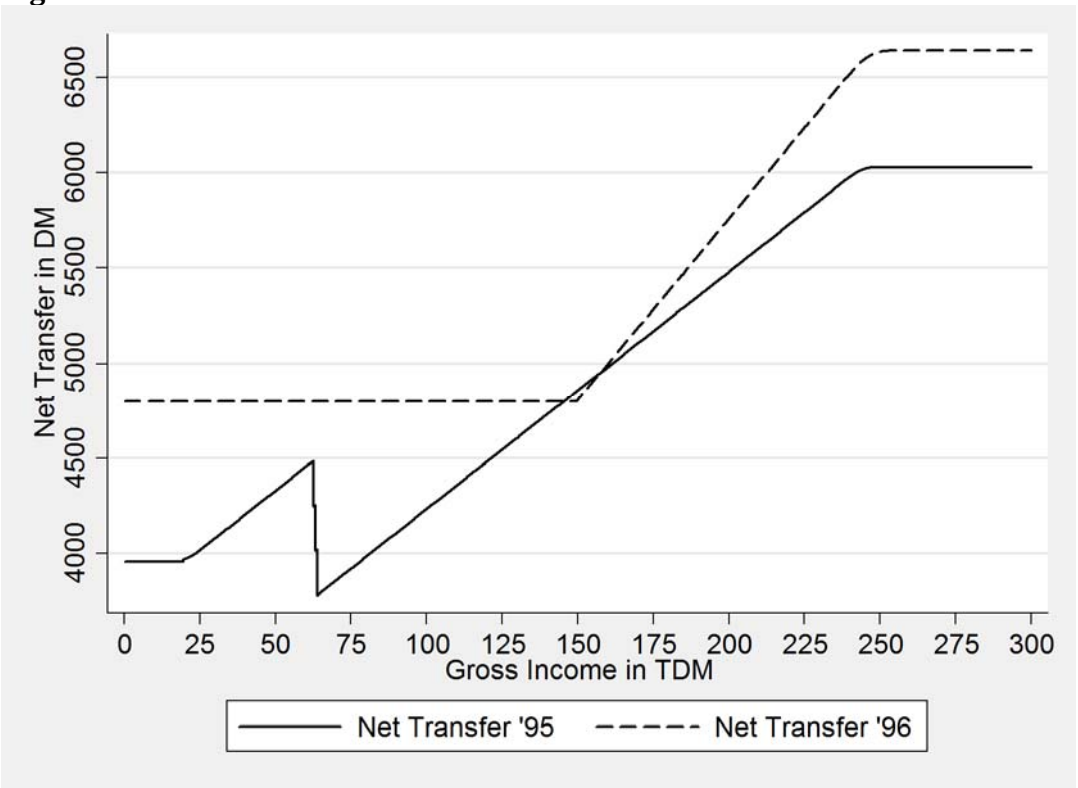
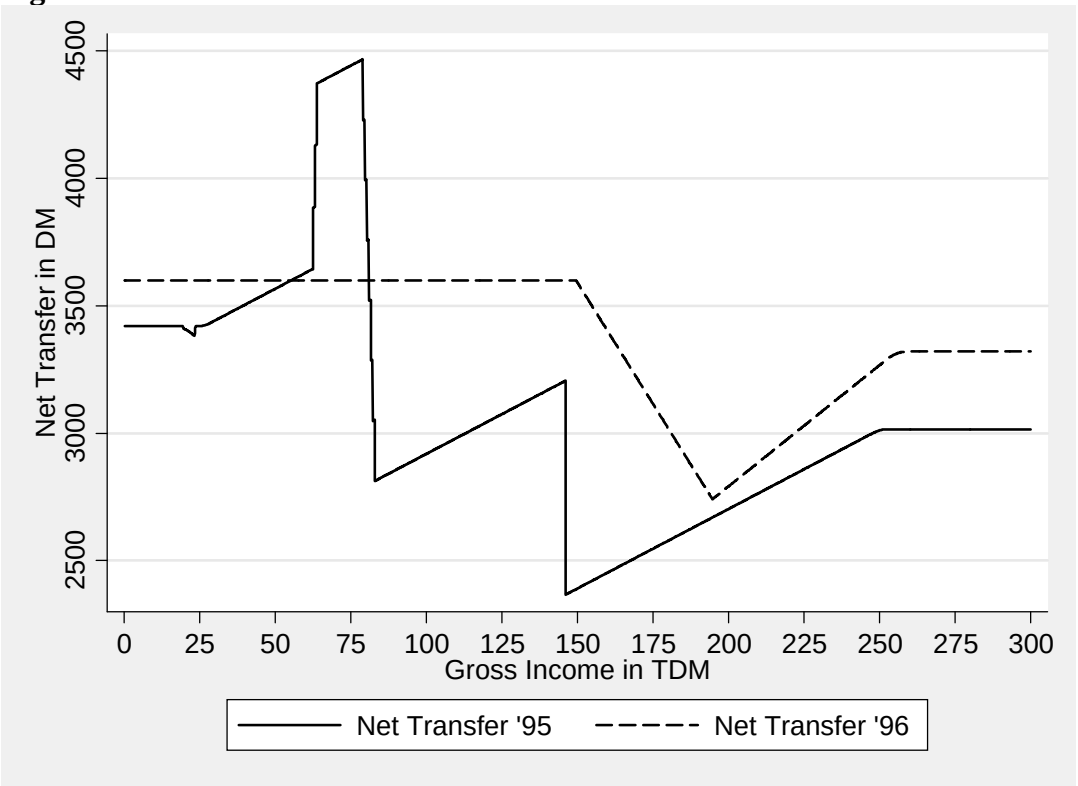


Figure 2.3. Absolute net transfer for the first two children



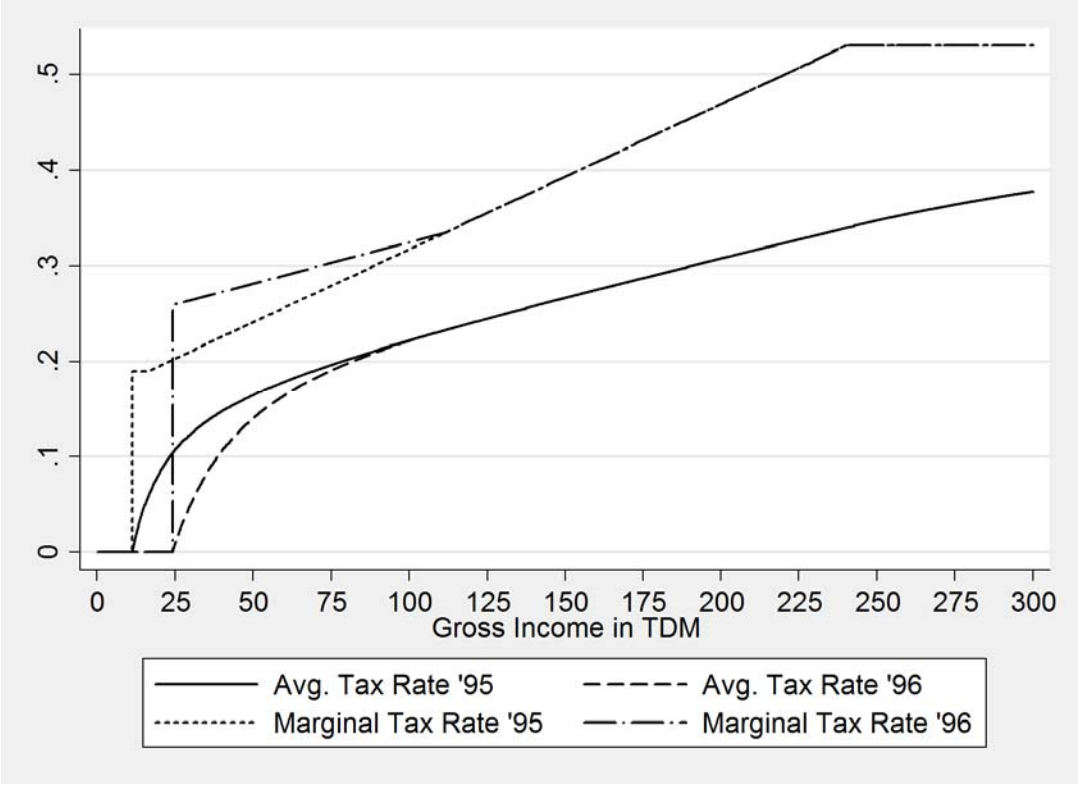
Notes: The gross income refers to yearly gross household income. Net transfer stands for the combined amount of taxes a married couple with one previous child would save and/or child benefits and child benefit supplements the couple would receive if the family had a second child. The red line in Figure 2.2 marks the income threshold of 62,500 DM, beyond which the reform increased the incentives to have a second child.
Source: Own calculations.

Figure 3. Absolute net transfer from a third child



Notes: The gross income refers to yearly gross household income. Net transfer stands for the combined amount of taxes a married couple with two previous children would save and/or child benefits and child benefit supplements the couple would receive if the family had a third child.
Source: Own calculations.

Figure 4. Average and marginal tax rates before and after the 1995 income tax reform



Source: Own calculations based on Lüdecke and Werding (1996).

Figure 5.1. Birthrates by educational groups in the *narrow definition* (preliminary)

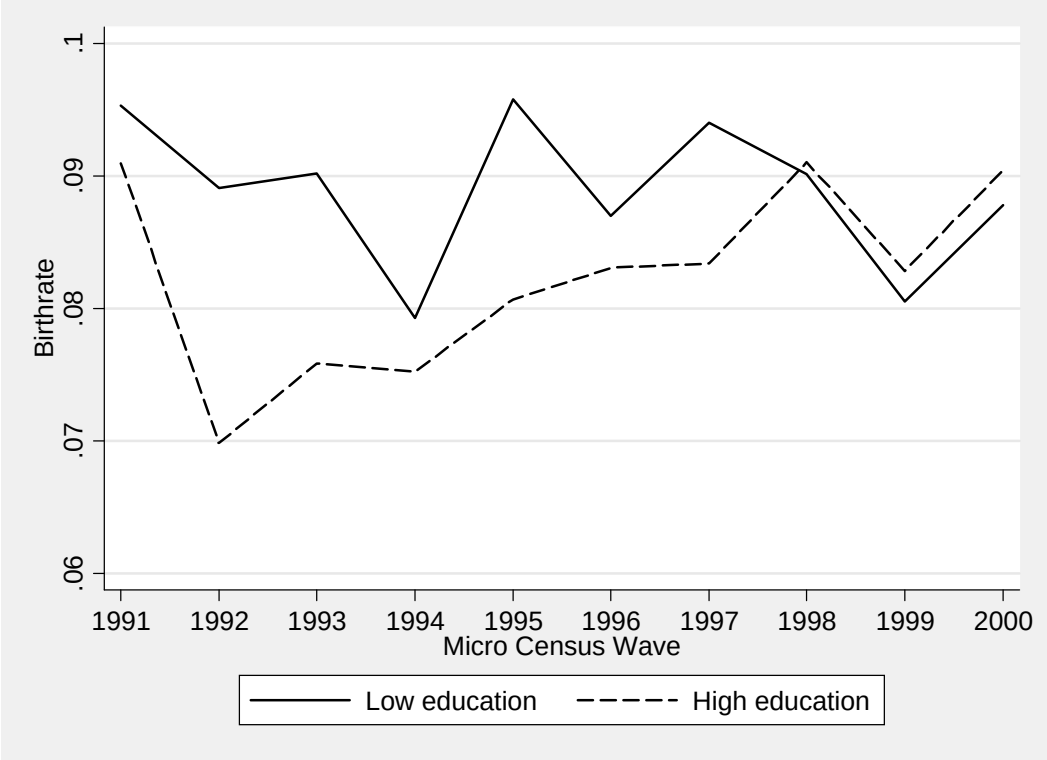
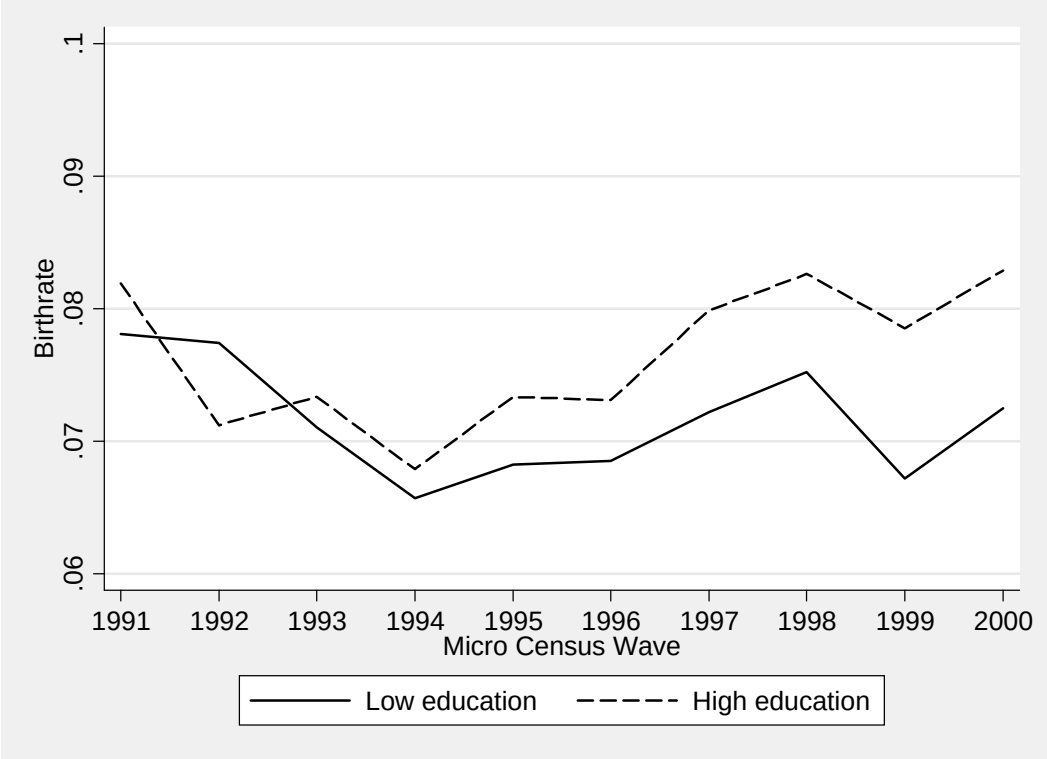


Figure 5.2. Birthrates by educational groups in the *broad definition* (preliminary)



Notes: The sample used to create these graphs cover all cohabiting couples in West Germany where the woman is age 18-44. The information for 1992 and 1994 is based on a smaller subsample. Birthrates are determined by counting children below age one living in the observed households at the time of the survey. All births are considered, independent of parity. *The graphs will be replaced by more precise reflections of the subsamples used in the analyses (to be completed).*
Source: Calculations from the Federal Bureau of Statistics based on the Mikrozenses 1991 to 2000.

Figure 6.1. Income distribution of childless couples by educational groups in the *narrow definition*

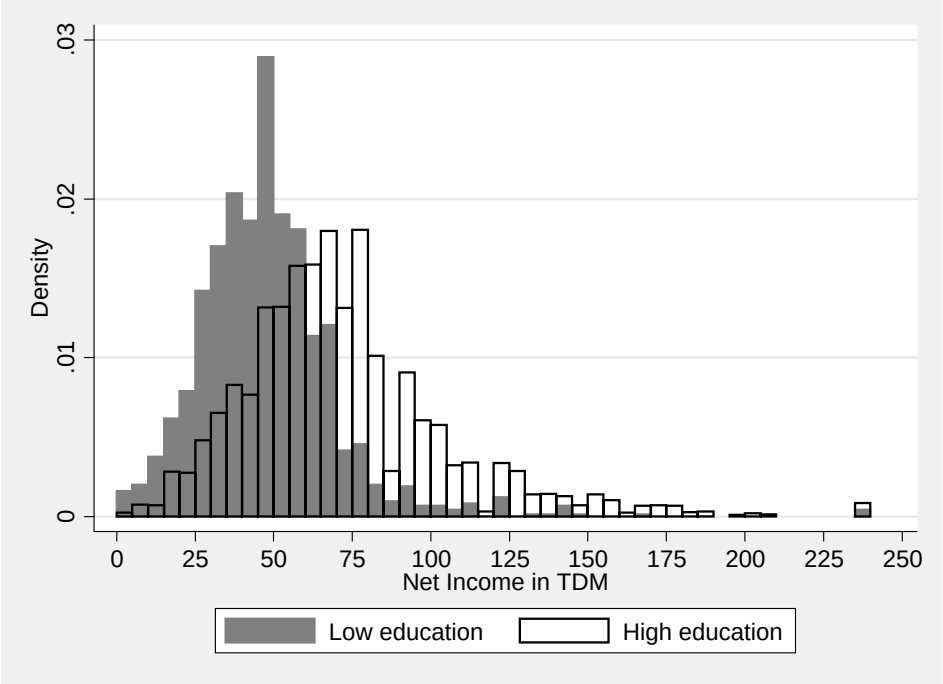


Figure 6.2. Income distribution of couples with one child by educational groups in the *narrow definition*

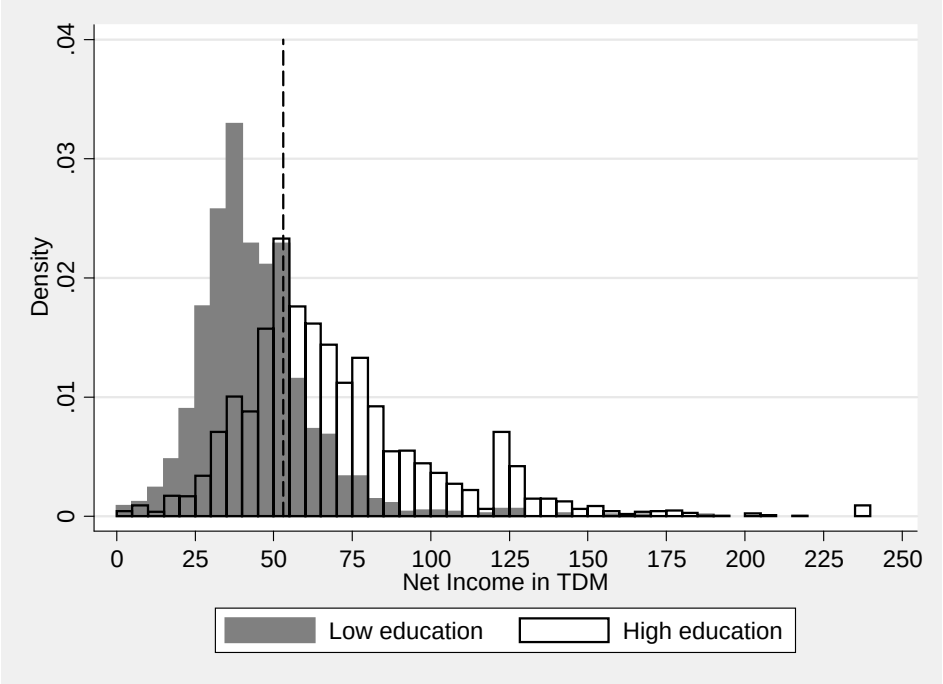


Figure 6.3. Income distribution of childless couples by educational groups in the *broad definition*

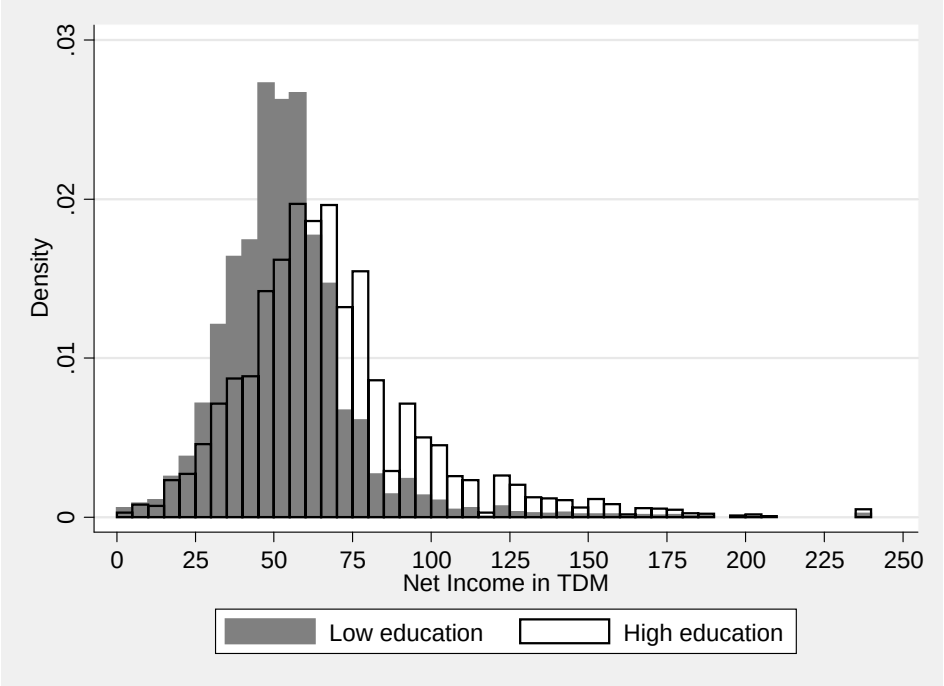
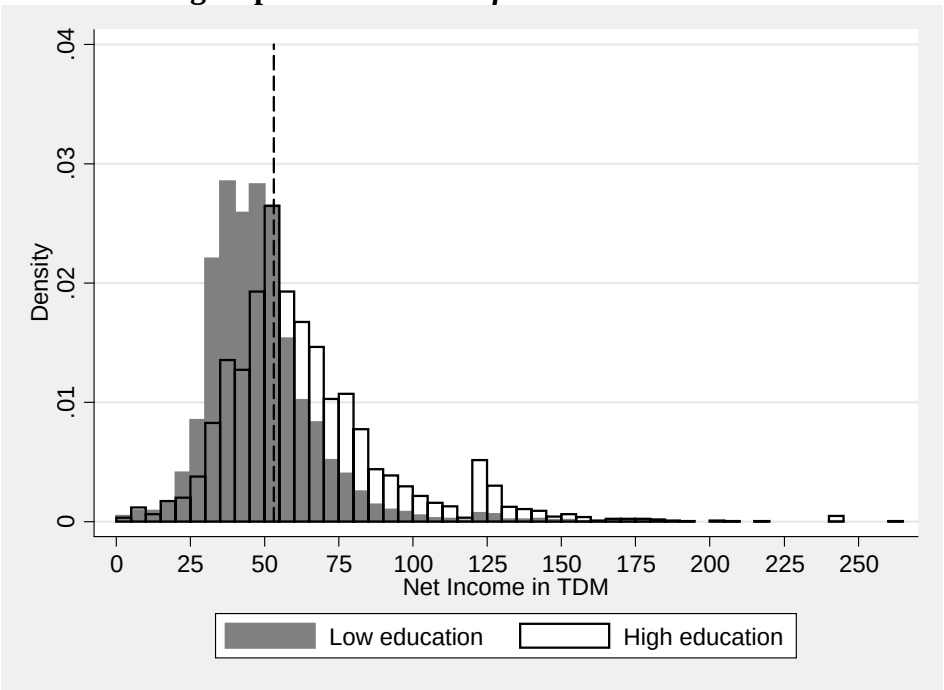


Figure 6.4. Income distribution of couples with one child by educational groups in the *broad definition*



Notes: We report income in net amounts, because the Mikrozensus does not contain information on gross income. The dashed lines in **Figures 6.2** and **6.4** represent the threshold of 53,000 DM, which is the approximate limit that divides one-child families into beneficiaries and losers of the reform in terms of increased net transfers for a second child (which lies at 62,500 DM in terms of gross income).
Source: Own calculations based on the Mikrozensus waves 1995, 1996, 1998, and 1999.

Appendix

Table A.1. Descriptive statistics of explanatory variables: couples without children; *broad definition* of educational groups; Mikrozensus

	Low	*High*		*Low*	*High*
N	16,522	19,782			
Married in t-1	50.18%	42.55%			
ISCED mother			**ISCED partner**		
1 (inadequately)	1.07%	0.06%	1 (inadequately)	0.93%	0.06%
2 (general elementary)	18.98%	3.08%	2 (general elementary)	14.37%	1.24%
3 (Abi or vocational)	79.95%	29.72%	3 (Abi or vocational)	84.71%	16.21%
4 (Abi and vocational)	-	21.06%	4 (Abi and vocational)	-	12.38%
5 (higher vocational)	-	11.28%	5 (higher vocational)	-	22.27%
6 (higher education)	-	34.80%	6 (higher education)	-	47.84%
Age of mother in t-1			**Age of partnr in t-1**		
<=20	-	-	<=20	0.16%	0.08%
21-24	-	-	21-24	2.71%	1.81%
25-28	35.14%	36.61%	25-28	17.70%	19.21%
29-32	27.16%	31.74%	29-32	25.59%	30.49%
33-36	19.16%	18.30%	33-36	20.14%	21.01%
37-40	18.54%	13.36%	37-40	14.80%	13.48%
41-44	-	-	41-44	9.65%	7.24%
45-49	-	-	45-49	5.26%	3.92%
>=49	-	-	>=49	4.00%	2.77%
State of residence					
Schleswig-Holstein	5.37%	4.70%			
Hamburg	2.40%	3.64%			
Niedersachsen	12.00%	10.03%			
Bremen	1.03%	0.91%			
Nordrhein-Westfalen	28.76%	28.28%			
Hessen	8.23%	9.86%			
Rheinland-Pfalz	6.27%	5.57%			
Baden-Wuertemberg	13.12%	14.83%			
Bayern	18.52%	17.05%			
Saarland	1.72%	1.31%			
Berlin	2.57%	3.82%			
Community/City size in thousands					
<20	42.79%	33.26%			
20 - 100	27.86%	27.22%			
100 - 500	16.01%	19.05%			
>500	13.34%	20.46%			

Source: Own calculations based on the Mikrozensus waves 1995, 1996, 1998, and 1999.

Table A.2. Descriptive statistics of explanatory variables: couples with one child; *broad definition* of educational groups; Mikrozensus

	Low	*High*		*Low*	*High*
N	19,428	13,745			
Married in t-1	83.91%	83.54%			
ISCED mother			**ISCED partner**		
1 (inadequately)	1.12%	0.17%	1 (inadequately)	0.98%	0.05%
2 (general elementary)	21.93%	5.22%	2 (general elementary)	13.06%	1.19%
3 (Abi or vocational)	76.95%	39.35%	3 (Abi or vocational)	85.95%	15.53%
4 (Abi and vocational)	-	19.38%	4 (Abi and vocational)	-	10.78%
5 (higher vocational)	-	11.90%	5 (higher vocational)	-	32.04%
6 (higher education)	-	23.98%	6 (higher education)	-	40.41%
Age of mother in t-1			**Age of partnr in t-1**		
<=20	-	-	<=20	0.04%	0.01%
21-24	-	-	21-24	0.98%	0.33%
25-28	21.79%	16.62%	25-28	10.39%	7.32%
29-32	28.04%	31.99%	29-32	22.52%	24.23%
33-36	24.09%	28.38%	33-36	23.85%	27.63%
37-40	26.08%	23.00%	37-40	19.85%	20.60%
41-44	-	-	41-44	13.97%	12.27%
45-49	-	-	45-49	5.67%	5.07%
>=49	-	-	>=49	2.74%	2.52%
State of residence			**Time since last birth in t-1**		
Schleswig-Holstein	4.31%	4.55%	0 years	9.99%	17.27%
Hamburg	1.72%	2.03%	1 year	10.65%	16.14%
Niedersachsen	12.39%	10.93%	2-3 years	16.74%	20.62%
Bremen	0.77%	0.79%	4-5 years	12.90%	11.65%
Nordrhein-Westfalen	27.40%	26.15%	6-7 years	10.33%	9.12%
Hessen	8.27%	10.35%	8-10 years	13.17%	10.34%
Rheinland-Pfalz	7.36%	6.27%	Over 10 years	26.22%	14.87%
Baden-Wuertemberg	12.80%	15.86%			
Bayern	20.49%	18.70%			
Saarland	2.26%	1.75%			
Berlin	2.23%	2.61%			
Community/City size in thousands					
<20	47.21%	42.84%			
20 - 100	27.39%	27.90%			
100 - 500	14.42%	16.46%			
>500	10.98%	12.79%			

Source: Own calculations based on the Mikrozensus waves 1995, 1996, 1998, and 1999.

Table A.3. Descriptive statistics of explanatory variables: couples without children; *broad definition* of educational groups; SOEP

	Low	*High*		*Low*	*High*
N	580	691			
Married in t	57.41%	53.26%			
Homeowner	22.93%	24.46%			
ISCED mother			**ISCED partner**		
1 (inadequately)	3.10%	0.43%	1 (inadequately)	2.24%	0.43%
2 (general elementary)	13.10%	5.35%	2 (general elementary)	22.59%	4.92%
3 (Abi or vocational)	83.79%	29.23%	3 (Abi or vocational)	75.17%	16.06%
4 (Abi and vocational)	-	15.34%	4 (Abi and vocational)	-	15.77%
5 (higher vocational)	-	20.98%	5 (higher vocational)	-	17.08%
6 (higher education)	-	28.65%	6 (higher education)	-	45.73%
Age of mother in t			**Age of partner in t**		
<=24	-	-	<=24	5.34%	2.03%
25-28	49.83%	43.42%	25-28	26.90%	20.98%
29-32	23.79%	31.40%	29-32	26.21%	33.57%
33-36	12.93%	16.64%	33-36	16.90%	19.10%
37-40	13.45%	8.54%	37-40	10.17%	9.12%
41-44	-	-	41-44	7.59%	7.09%
45-49	-	-	45-49	3.79%	3.47%
>=49	-	-	>=49	3.10%	4.63%
State of residence					
Schleswig-Holstein	4.66%	3.47%			
Hamburg	0.86%	2.17%			
Niedersachsen	16.38%	14.91%			
Bremen	0.52%	1.59%			
Nordrhein-Westfalen	24.48%	32.56%			
Hessen	9.31%	10.13%			
Rheinl.-Pf.+Saarland	13.28%	7.24%			
Baden-Wuertemberg	10.00%	13.60%			
Bayern	20.52%	14.33%			

Source: Own calculations based on the SOEP waves 1992-1998.

Table A.4. Descriptive statistics of explanatory variables: couples with one child; *broad definition* of educational groups; SOEP

	Low	*High*		*Low*	*High*
N	869	648			
Married in t	91.71%	89.66%			
Homeowner	37.74%	41.67%			
ISCED mother			**ISCED partner**		
1 (inadequately)	1.84%	0.93%	1 (inadequately)	2.30%	0.46%
2 (general elementary)	18.99%	9.41%	2 (general elementary)	15.88%	5.09%
3 (Abi or vocational)	79.17%	29.48%	3 (Abi or vocational)	81.82%	14.97%
4 (Abi and vocational)	-	15.43%	4 (Abi and vocational)	-	12.19%
5 (higher vocational)	-	27.62%	5 (higher vocational)	-	33.02%
6 (higher education)	-	17.13%	6 (higher education)	-	34.26%
Age of mother in t			**Age of partner in t**		
<=24	-	-	<=24	1.61%	1.08%
25-28	30.03%	22.99%	25-28	15.07%	9.88%
29-32	29.92%	35.80%	29-32	25.66%	30.09%
33-36	23.82%	23.15%	33-36	19.79%	27.78%
37-40	16.23%	18.06%	37-40	20.02%	16.67%
41-44	-	-	41-44	10.24%	8.49%
45-49	-	-	45-49	5.41%	3.09%
>=49	-	-	>=49	2.19%	2.93%
State of residence					
Schleswig-Holstein	3.68%	3.24%			
Hamburg	1.04%	1.54%			
Niedersachsen	13.35%	13.43%			
Bremen	0.81%	0.31%			
Nordrhein-Westfalen	28.08%	33.64%			
Hessen	6.21%	6.64%			
Rheinland-Pfalz	11.51%	8.64%			
Baden-Wuertemberg	16.00%	14.66%			
Bayern	19.33%	17.90%			

Source: Own calculations based on the SOEP waves 1992-1998.