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Earnings-related parental leave benefits and subjective well-being of young mothers: evidence from a German parental leave reform

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Earnings-related parental leave benefits and subjective well-being of young mothers: evidence from a German parental leave reform

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Abstract

This study investigates the causal effect of earnings-related parental leave benefits (Elterngeld) on subjective well-being of young mothers. The new subsidy was introduced in 2007, and replaced a former means-tested benefit (Erziehungsgeld). The reform changed the total amount of benefits as well as the duration of pay. By construction of the reform, the change in benefits differs across population subgroups, depending on their eligibility for the former means-tested benefit. The reform also introduced incentives for paternal leave taking. Income effects, fathers' involvement, and social norms constitute potential channels through which the reform affects well-being. Using a regression discontinuity design, I find remarkable heterogeneities in the response to the reform. While subjective well-being of West German mothers increases, East German mothers experience decreasing life satisfaction.

Keywords: Elterngeld, paid parental leave, well-being, life satisfaction
JEL: J13, J18, J28, I31

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1 Introduction

Low fertility rates and low female labor market participation are major political issues. Declining birth rates accelerate demographic changes such as population aging and shrinking labor forces. Therefore, strengthening the labor market attachment of women is a promising way to mitigate the consequences of the demographic transition. In both dimensions - female labor market participation and fertility rates - Germany exhibits the lowest rates among OECD countries (OECD; 2013).

In 2007 the German government changed the parental leave benefit system in order to counteract low female labor market participation. The introduction of earnings-related parental leave benefits abolished a means-tested system. Thereby, subsidies became more generous by adapting regulations from the Nordic countries where labor market participation and fertility rates are generally higher (Spiess and Wrohlich; 2008; OECD; 2013).

Typical reform evaluation concentrates on objective measures of utility such as income or consumption (see, e.g., Meyer and Sullivan; 2004). In the case of the 2007 reform, previous research focuses on key reform goals, e.g., mothers' labor market attachment (e.g., Bergemann and Riphahn; 2011), fertility (Cygan-Rehm; 2013), or fathers' involvement in child-rearing (see also Wrohlich et al.; 2012). However, these objective measures have important shortcomings. A reform of parental leave benefits affects various areas in life, e.g., health and child outcomes, maternal stress, or family circumstances (see, e.g., Ruhm; 2000; Tanaka; 2005; Berger; 2010). A comprehensive policy evaluation should also consider more general outcomes to reveal possible unintended side effects (OECD; 2011). The analysis of well-being and especially subjective well-being facilitates such a general reform evaluation and gives insights objective measures cannot give (see, e.g., Luechinger; 2009; OECD; 2011).

This study evaluates the effect of the introduction of earnings-related parental leave benefits on the subjective well-being of young mothers. In a first step, I analyze long-run consequences of the reform on mothers' subjective satisfaction about 1.5 to 5.5 years after the birth of their child. Different factors may contribute to long-run effects of the

reform, e.g., higher income security because of higher labor market attachment, changes in fertility or family formation. In a second step, I analyze heterogeneity in the responses to the reform and identify potential channels through which the reform affected well-being.

The paper contributes to the literature in three important ways: first, it is the first study evaluating the reform effects on mothers' well-being for Germany. So far, there is also no international study that emphasizes effects of a similar reform on well-being. Second, the analysis of potential channels through which the reform affected well-being reveals important insights in side effects of the reform, e.g., on marriage rates. Third, I use a unique German data set - the *Panel Analysis of Intimate Relationships and Family Dynamics* - for the empirical examination. This survey facilitates the analysis of well-being and contains rich information on family background. Further, compared to other data sets with similar information, for example, the Socio-Economic Panel (SOEP), the data comprise a more sufficient number of births around the reform implementation.

A number of studies already evaluated the reform and usually find positive effects on the intentions to return to work (Bergemann and Riphahn; 2010), on the actual decision for the return to work (Geyer et al.; 2012; Kluve and Tamm; 2013), and on the involvement of fathers in child-rearing (Geisler and Kreyenfeld; 2012; Wrohlich et al.; 2012). Also, the reform affects subgroups of the population very differently depending on region of residence and eligibility for subsidies under the old regime (e.g., Wrohlich et al.; 2012; Cygan-Rehm; 2013).

The empirical strategy applies a combination of a regression discontinuity approach and a differences-in-differences design (see, e.g., Dustmann and Schönberg; 2012; Cygan-Rehm; 2013). The identification benefits from the largely unanticipated reform introduction for children born in the first quarter of 2007 (see, e.g., Kluve and Tamm; 2013). At the timing of conception the parents of these children could not have known about the reform introduction. Therefore, I compare mothers of children born in the last quarter of 2006 (not eligible for the new subsidy) and mothers of children born in the first quarter

of 2007 (eligible for the new subsidy). To control for general differences in satisfaction between mothers who gave birth in the first and the last quarter of a year, I additionally include mothers of children born at the turns of years 2003/4 and 2005/6 as control group.

The results show very different responses to the reform in subgroups. The patterns are consistent with a smaller income reduction after child birth under the new regime compared to the old regime. Thus, the results may reflect a reduction in opportunity costs of child-bearing. I also find different effects of the reform on satisfaction in East and West Germany. Whereas East German women are less satisfied under the new regime, the effect is positive for West German mothers. Potential explanations for the contrary responses include differences in social norms and different income changes. Specifically, the reform is related to a reduced marriage probability of East German mothers and may induce unintended long-term financial disadvantages resulting from the German tax system. Results pass the usual robustness checks.

2 Institutional setting and previous research on the reform

This study evaluates the introduction of the German *Elterngeld* (parents' money) for births after January 1, 2007. Wrohlich et al. (2012) state three major goals of the reform: first, income security for families in the year after childbirth; second, helping parents to secure their economic situation on their own; third, promote fathers' involvement in child-rearing. This section describes the main changes with respect to the previous regulations.

Parents of children born before the 1st of January 2007 were supported by a means-tested subsidy called *Erziehungsgeld* (child-rearing benefit). Eligible parents could either receive EUR 300 for 24 months or EUR 450 for 12 months. For each additional child the same amount was added on top. The benefit targeted low income parents. If the joint net household income in the year before birth exceeded certain thresholds, parents were not eligible. If the applying parent was not working during the period of the payment,

his/her pre-birth labor income was excluded from the means test and only the partners' income was relevant (BMFSFJ; 2005). One year after the child's birth, parents had to pass a second means test in order to stay eligible. The income thresholds in both tests depended on marital status (higher thresholds for single parents) and the chosen duration and amount of pay (EUR 300 for two years vs. EUR 450 for one year). Although parents could work up to 30 hours per week during the payment period, only mini jobs (below EUR 400 per months) were disregarded in the means test.

On the 1st of January 2007 the means-tested system was abolished and an earnings-related benefit was introduced. The new *Elterngeld* replaces 67% of prior labor earnings up to an upper bound of EUR 1800 per month and a lower bound of EUR 300 per months (BMFSFJ; 2011). All parents are eligible for the new subsidy for up to 12 months after childbirth (14 months for single parents). If parents share parental leave they are free to extend the duration of the transfer to 14 months ("daddy months").[1] Thus, all parents of children born after 01.01.2007 are eligible and both, the total amount of the subsidy and the duration of pay changed. As under the old regime, part time work below 30 hours per week is possible.

Compared to the old regime, the new subsidy is more generous because of the universal eligibility and mostly a higher total amount of benefits (see Kluve and Tamm (2013) for a detailed overview of the changes in total amounts for subgroups). The reform considerably shortens the total duration of pay for those women who were eligible under the old regime. Further, under the old regime earnings from part-time work during the transfer period were added to the relevant income for the second means test. Thus, part-time work during the first year after birth reduced the likelihood of being eligible in the second year after child birth. Under the new regime, part-time work during the transfer period reduces the amount of subsidies but does not affect eligibility.

[1] Additionally, parents can decide to receive only half of the monthly subsidy for a longer period of up to two years. However, only 9.5% of mothers and 2.4% of fathers choose this option (BMFSFJ; 2008).

3 Theory and hypotheses

Different regulations changed through the reform and may contribute to an effect of the reform on well-being: first, the change in the total amount of subsidies may affect women similar as an income effect. Second, the modified duration of the transfer affects incentives for the return to work. Third, the reform gives incentives for fathers' involvement in child-rearing. In addition, other potential outcomes such as mothers' health, child outcomes, marriage rates, and social norms may be relevant for the effect of the reform on women's well-being.

Comparing pre- and post-reform situations, the introduction of earnings-related parental benefits changed the total amount mothers get to compensate financial losses of interrupted labor market participation. However, the change in subsidies varies across subgroups of women since some women were not eligible under the old regime.

To understand why subgroups are differently affected by the reform, consider two cases for a simple differentiation: (1) a high-income family and (2) a low-income family with both parents unemployed. Under the old regime the high-income family was not eligible for benefits because the family did not pass the means test. Under the new regime the family is eligible for a 67% replacement of labor earnings for 12 months (up to EUR 1800 monthly). Consequently, compared to the old regime the high-income family (1) can gain more than EUR 20,000 (see also Kluve and Tamm; 2013). The low income family (2) was eligible for EUR 300 for 24 months under the old regime. Since 2007 the family is still eligible for EUR 300 but only for 12 to 14 months. Thus, the total amount of subsidies halves because of the shorter duration of pay. Overall, if we compare pre- and post-reform status women experience different changes in subsidies and transfer durations depending on their eligibility for the old subsidy.

The change in the total amount of subsidies between the old and the new regime may affect well-being. The change in subsidies may work similar to an income effect. The related economic literature on the effect of income on happiness predicts a positive effect (Clark et al.; 2008). However, the literature on the Easterlin paradox shows that

positive income differences promote well-being only if individuals compare their situation to others or to themselves in the past (see Clark et al. (2008) for a discussion of previous findings).[2] Here, an effect may exist if women compare themselves to a hypothetical situation before the reform, to themselves in the past, or to women who are not eligible for the new subsidy (e.g., women who gave birth shortly before 2007). In sum, women who were not eligible for subsidies under the old regime may experience a positive effect on well-being and women who were eligible under the old regime might face a negative effect.

Another major change is the shorter duration of the transfer. Whereas the old subsidy was paid up to 24 months, the duration under the new regime is only 12 month (or 14 month). For parents who were eligible under the old regime, this increases the incentive to return to work earlier. In fact, Bergemann and Riphahn (2010) and Bergemann and Riphahn (2011) find a higher intention to return to work early after child birth and Kluve and Tamm (2013) find a higher employment probability of mothers after the subsidy expires. The effect of employment on well-being of mothers is not clear ex ante. On the one hand, the literature usually finds that employment relative to (registered) unemployment increases satisfaction (see, e.g., Winkelmann and Winkelmann; 1998). On the other hand, staying at home for family reasons may affect well-being differently than unemployment. Consistent with studies on the consequences of unemployment, Berger (2010) finds that mother in full-time employment report higher subjective well-being than women who stay at home for family reasons even if income is fully compensated. Consequently, her results imply that the parental leave reform may affect mothers' well-being positively if they return to work earlier.

Another aim of the reform is to promote fathers' involvement in child-rearing. Parents are free to extend the duration of benefit receipt to 14 months if the father takes up own leave. The system is quite flexible as parents can freely split this duration. Existing evi-

[2]The previous happiness literature stresses the role of adaptation to, for example, income changes (see Deaton; 2008; Di Tella et al.; 2010). Thus, women may adapt to income changes which may rule out a long-run effect of income on well-being.

dence on the effect of the reform on fathers' involvement in child care is mixed. Wrohlich et al. (2012) and Geisler and Kreyenfeld (2012) report that fathers use paternal leave more frequently after the reform. This contrasts with the findings of Kluve and Tamm (2013) who find no change in fathers' time devoted to child care. If the reform increased fathers' involvement in child-rearing, mothers might be less stressed because of the father's support. In addition, if the father takes paternal leave, the mothers is able to return to work earlier. Overall, if a higher engagement of fathers affects well-being, I expect a positive effect on mothers' well-being.[3]

Other channels through which the reform might affect well-being include social norms and marriage behavior. Whereas the old regulations or the tax system favor traditional family types (see, e.g., Kreyenfeld; 2004), the new subsidy explicitly promotes female labor market participation. If women under the new regime can decide about their return to work without facing social stigma, I expect their well-being to increase. Further, the reform supports female economic independence of partners' incomes. Literature on marriage behavior finds that marriage rates decrease with women's labor market attachment and economic independence (Konietzka and Kreyenfeld; 2005). Consequently, the reform might affect marriage rates negatively. The literature on the effect of marriage on life satisfaction shows a positive effect at least in the first years after marriage but is ambiguous about long-term effects (see, e.g., Lucas and Clark; 2006). Consequently, if the reform decreases marriage rates, a positive effect on well-being might not be present under the new regime and may affect well-being negatively compared to the old regime.

Related mechanisms include effects of the reform on higher order fertility, health, and child outcomes. If women return to work earlier, they might adjust their higher order births (Cygan-Rehm; 2013) which in turn might also affect well-being. Further, compared to the previous regime, higher income security in the first year after birth and the possibility to stay at home during the first year might affect maternal health, stress, and

[3]In the extreme case this channel could also affect satisfaction negatively if this reform generates a new social stigma and mothers feel social pressure to return early to work. However, as other institutions in Germany, for example, long unpaid parental leave and tax splitting, still favor traditional family types (Kreyenfeld; 2004), a negative effect is rather unlikely.

breastfeeding which potentially affects children's development and mothers' well-being (Baker et al.; 2008; Berger; 2010).

Previous evidence on the reform effect stresses different responses in East and West Germany (see, e.g., Wrohlich et al.; 2012; Cygan-Rehm; 2013). Whereas West German women postpone subsequent births, East German women tend to get subsequent children earlier. Wrohlich et al. (2012) show that East German men use paternal leave more frequently compared to West German men. A number of factors may contribute to these different responses. As average income is lower in East Germany (see, e.g., Wrohlich et al.; 2012), the number of eligible women under the old regime was higher (Fendrich et al.; 2003). Regarding East Germany, labor market attachment of women is generally higher (see, e.g., Krueger and Pischke; 1995), child care availability is higher and more accepted (Hank et al.; 2004), and marriage rates are lower compared to West Germany (Konietzka and Kreyenfeld; 2005). Thus, the change in the total amount of subsidies might on average be lower in East Germany and because of the different social norms and a higher labor market attachment of women, mothers may generally respond differently to the reform.

4 Identification strategy

The introduction of the reform provides a largely unanticipated natural experiment. To estimate the causal effect of the reform on subjective well-being, the identification strategy combines a regression discontinuity design and a differences-in-differences approach (see also Dustmann and Schönberg; 2012; Cygan-Rehm; 2013). A sharp regression discontinuity design compares mothers of children born in the fourth quarter of 2006 to mothers of children born in the first quarter of 2007. Thus, identification rests on the assumption that mothers have similar characteristics in these two groups and that other factors affecting well-being remain constant within this time window. This assumption is rather strong as mothers who gave birth in the first and last quarters of subsequent years might gener-

ally differ in well-being. Potential determinants of these general differences include, for example, weather differences. By the inclusion of mothers who gave birth in the years before the introduction of the reform I can capture the effect of general differences between the fourth and the first quarter of the year. This diff-in-diff extension rests on the assumption that these seasonal differences in well-being do not change over the years.

I estimate the following equation:

$$y_i = \beta_1 \textit{reform}_i + \beta_2 \textit{quarter}_i + \textit{cohort}_i' \gamma + \mathbf{Z'}\delta + \epsilon_i \tag{1}$$

Here, y_i represents mothers' subjective well-being comprising life satisfaction, satisfaction with job and training, social networks, and family. The variable $\textit{reform}_i$ equals 1 if the mother gave birth after the introduction of the reform, thus after the 1st of January 2007. Additionally, $\textit{quarter}_i$ controls for general differences in the satisfaction of mothers who gave birth in the last and first quarter of subsequent years, and $\textit{cohort}_i$ is a set of dummies for the children's birth years.[4] Further controls such as dummies for children's age in months, mothers' age, mothers' education, partners' education and regional controls enter through $\mathbf{Z}$. ϵ_i is an error term.

The estimate for β_1 gives the causal effect of the *Elterngeld* introduction on mothers' subjective well-being (1) if parents could not anticipate the reform introduction and react accordingly and (2) if differences in satisfaction between mothers of last and first quarter births remain constant over the years.

The estimate for β_1 is biased if mothers anticipated the reform introduction and changed their behavior. One example are mothers who are highly attached to the labor market and decided to select themselves into motherhood because of the reform introduction. These mothers might in general also differ in life satisfaction. However, the previous literature shows that anticipation is rather unlikely for mothers who gave birth in the first quarter of 2007. For example, Kluve and Tamm (2013) show google search results for *Elterngeld*

[4]Here, a cohort is not defined by calendar years but by births from October to March in each year from 10/2003 to 03/2007. Thus, I include four cohorts of children: 10/03-03/04, 10/04-03/05, 10/05-03/06, 10/06-03/07.

during the years 2004 to 2008. They find that the public discussion of the reform started in May 2006. Further, parliament passed the reform in September 2006 and until that date it was not clear whether how and when the reform will be introduced. Also, parents cannot exactly plan the timing of conception such that mothers of children conceived between March 2006 and July 2006 are unlikely to have anticipated the reform introduction.

Another threat to the validity of the estimates is the shifting of births. As some women are better off under the old and some under the new regime, women with due dates in December 2006 and January 2007 had the incentive to pre- or postpone the delivery. Neugart and Ohlsson (2013) and Tamm (2013) show that such a shift actually took place. To check the influence of this potentially confounding factor, I exclude January and December births in a sensitivity analysis.

The diff-in-diff strategy fails if seasonal patterns change over the birth cohorts of children. Diff-in-diff assumes that the difference in well-being between mothers who gave birth to a child in the last quarter of the year and mothers who gave birth in the first quarter of the following year do not change over the years. Unfortunately, I cannot test this assumption. However, a graphical inspection supports no changes in seasonal trends. Further, I check the robustness of the results for different cohorts in the control group. The risk of changing trends over the years should be smaller the fewer cohorts I include in the control group.[5]

[5]Another concern might be that effects are driven by a "starting" effect, a Hawthorne effect (see, e.g., Adair; 1984). Women who gave birth in the first quarter of 2007 are the first who receive the new subsidy and we might find positive effects just because something changed. The converse might be true for the last births under the old regime. However, this should impose minor consequences for the estimates: first, I measure well-being 2.5 to 5.5 years after the reform introduction. Second, a Hawthorne effect should be smaller the older children are and I find no significant changes of the reform effects with increasing child age. Also, if mothers who gave birth in the last quarter of 2006 are especially unhappy because they barely missed the reform, satisfaction in the last quarter of 2006 should be lower than in the last quarter of 2005. I rule out this mechanism as a driving factor as I do not find this pattern in the data.

5 Data

This study exploits four waves from 2008 to 2011 of the *Panel Analysis of Intimate Relationships and Family Dynamics* (Pairfam). The Pairfam data aim at providing an empirical basis for the analysis of family dynamics and collect data of 12,400 participants annually since 2008 (Huinink et al.; 2011). The main advantage of the data is very detailed information on a range of family related characteristics including mothers' subjective well-being. A drawback of the data are small samples sizes. However, for Germany there is no other appropriate data source available comprising more observations. The Pairfam data collection started in 2008 with respondents of three birth cohorts (1991-93, 1981-83, 1971-73). In addition to the Pairfam data I use data called *Demographic Differences in Life Course Dynamics in Eastern and Western Germany* (DemoDiff). The DemoDiff panel data closely follows the design of Pairfam (since 2009) and facilitates East-West comparisons (see Kreyenfeld et al.; 2013).

The Pairfam data ask anchor persons about a wide range of family characteristics.[6] The partner, child, and parents of the anchor person answer a separate questionnaire. I restrict the sample to female anchor persons aged 20 to 31 who gave birth to children at the turns of 2003/4 to 2006/7.[7] Further, I restrict the sample to births in the first and in the last quarter of these years.[8] I consider only children aged 16 to 63 months (about 1.5 to 5.5 years). These sample restrictions leave me with 119 births under the new regime and

[6]Pairfam started in 2008 with 12,400 randomly drawn individuals from the registration office pool of 310 communities. The population of anchor persons are people living in Germany in private households. Interviews took place face-to-face between September and April of year 2008 to 2011. See Huinink et al. (2011) for further details about survey and sampling.

[7]I restrict the analysis to women aged 20 to 31 for different reasons: first, women aged 20-30 are at the beginning of their working and fertile life. Consequently, they are at the core of policy interventions which aim at increasing fertility and labor market participation. Second, the survey design of Pairfam does not facilitate an analysis of women in all fertile ages because of the cohort design of the study. In fact, I exclude only women older than 35. These women potentially differ in various observed and unobserved factors, for example, attitudes towards female employment. I leave a separate investigation of the response of this interesting subgroup for future research.

[8]The control group of the identification includes women who gave birth from 2003 to 2006. In this period also other reforms such as, for example, those of unemployment benefits in 2005 and 2006 took place (see, e.g., Riphahn and Wunder; 2013). However, this is only a concern if these reforms are systematically connected to quarterly births which is not the case. Also, child care availability was strongly promoted after 2005 (Bauernschuster et al.; 2013). To eliminate concerns about potential interactions with the reform, I control for child care availability for under 3 and 3 to 6 year olds in all regressions.

496 births under the old system (615 births in total).[9]

To explore mothers' well-being, I use information on overall life satisfaction. Mothers answer the question "Now I would like to ask about your general satisfaction with life. All in all, how satisfied are you with your life at the moment?". The survey also asks for mothers' satisfaction within specific areas of life (e.g., job and training, or family). As the reform generated different incentives concerning, for example, the return to work or fathers' involvement in child-rearing, these variables might reveal additional insights into the channels through which the reform affects well-being. Mothers evaluate their satisfaction on a Likert scale with minimum zero to maximum ten. The following Section 6.1 presents some descriptive statistics.

As discussed in Section 3, the reform affected women differently depending on their eligibility for the old subsidy. Thus, I investigate the reform effect for subgroups of women who were and who were not potentially eligible under the old regime. Eligibility depended on partners' income if the mother took leave. Unfortunately, partners' pre-birth income is not available in the data and information on mothers' leave only for a subgroup. To determine whether women were eligible under the old regime, I use information on pre-birth marital status and on partners' education as a proxy for labor income. I consider a woman likely to be eligible if she had no partner or a partner with low education (below intermediate degree) before birth.

6 Results

6.1 Descriptive analysis

Table 1 gives an overview of some characteristics of mothers, partners, and children. The first two columns give averages for births in the last quarter of 2006 and the first quarter of 2007. Columns 3 and 4 give average characteristics for births from 2003/4 to

[9] A less restrictive sample uses all children born between 07/2003 and 06/2007. This provides a sample of 1,193 births. I prefer the smaller window as it limits confounding factors. However, baseline findings do not change when I use the less restrictive sample (results available upon request).

2005/6, in the last and first quarters of these years respectively. Births in the first quarter of 2007 (column 2) represent the treatment group, columns 1, 3, and 4 control groups. Column 5 presents the difference of column 1 and 2, thus, the regression discontinuity effect in raw data. The difference in average characteristics between first and last quarter of year 2003/4 to 2005/6 represents seasonal effects. The difference is given in column 6. Finally, the difference between columns 5 and 6 give the diff-in-diff results in raw data (column 7). For average life satisfaction we observe an increase after the reform. However, this effect may be completely driven by seasonal effects. Once we consider general differences between the first and the last quarter of subsequent years (column 6), the effect is negative and close to zero (column 7). Overall, in the raw data, there is no clear pattern in differences in satisfaction.

A concern in diff-in-diff estimations are changing seasonal trends in treatment and control groups, i.e., that changes in satisfaction between mothers who gave birth in the first and last quarters of subsequent years vary between 2003 and 2007. I cannot test this assumption, but to investigate this necessary requirement for consistency, Figure 1 plots average life satisfaction of women who gave birth in last and first quarters in 2003/4 through 2006/7. Figure 1 supports similar trends in treatment and control groups over the years. The changes in satisfaction from last to first quarter births are positive and similar in magnitude in all years. If we compare the change in satisfaction from 2006 to 2007 to changes in satisfaction in the years before, we observe a slightly lower increase. This is consistent with the small negative diff-in-diff effect in Table 1. Also, Figure 1 underpins that a sharp regression discontinuity design might be misleading as we observe general seasonal trends in satisfaction which cannot be attributed to the reform. Additionally, the different levels of satisfaction show cohort effects which I control for in the following regressions.

6.2 Overall effects and results by prior eligibility status

Table 2 gives estimation results for the full sample and by eligibility status under the old regime. As the data do neither provide direct information on eligibility nor on pre-birth income, I use pre-birth marital status and partners' education to approximate eligibility for the old subsidy. Panel A gives results for the full sample, panel B for mothers with a highly educated partner (intermediate secondary or higher) and panel C for mothers without partner or with a lowly educated partner. I use four dependent variables: overall life satisfaction, satisfaction with job and training, satisfaction with social networks, and satisfaction with family. As I use panel data, standard errors are clustered at the individual level in all following regressions.

The full sample of 615 births splits in 305 and 310 births in the subsamples (panel A and B). Consequently, precision of estimates is quite low and I'm not willing interpret the magnitude of the effects. However, some meaning can be found in the sign of the effects and the patterns in subgroups. None of the estimated reform effects in the full sample (panel A) in Table 2 are statistically significant. This is not surprising as we find offsetting effects in subgroups by partners' education.

Panel B gives the results for women with highly educated partners. These women were potentially not eligible for transfers under the old regime and experience a positive income effect under the new regime. Panel C gives the results for women who would have been eligible for the old subsidy and therefore potentially face a negative income effect or a low positive income effect after the reform.

The estimates show the expected patterns. Whereas the reform effects on life satisfaction are significantly positive for women with highly educated partners, the effect is negative but imprecisely estimated for women without or with a low educated partner. This pattern is consistent with the opposing income effects in the two groups. Further, if we compare the reform effect on the different satisfaction outcomes, we find pronounced effects in column 2 and 4 of panel B. Column 2 gives the reform effect on the satisfaction with job and school. Here, we expect a pronounced effect if, for example, women return

earlier to work after child birth as found by Kluve and Tamm (2013). As previous evidence shows that working conditions, for example, training, are related to the duration of parental leave (see, e.g., Puhani and Sonderhof; 2011), a shorter career interruption may contribute to higher work satisfaction.

Column 4 gives the effects on the satisfaction with family. The satisfaction with family may reveal effects of the reform on child development or family dynamics. Women who are newly eligible for paid parental leave (panel B) are more satisfied with their family. Here, a positive effect of the reform on fathers' involvement (see, e.g., Geisler and Kreyenfeld; 2012) may contribute to an overall increase in satisfaction with family. Also, previous literature stresses the positive effect of parental leave on child health and child development (see, e.g., Ruhm; 2000; Berger; 2010). As women who were not eligible under the old regime tend to use parental leave in the first year after birth more frequently (Wrohlich et al.; 2012), we expect the found positive effect on satisfaction in general and a pronounced effect on satisfaction with family.

6.3 Heterogeneity - East-West comparison

Table 3 gives results for West Germany (panel A) and East Germany (panel B). The results support earlier research as the reform effects in East and West Germany are contrary and offsetting. Again, estimates are imprecisely estimated and I cannot reliably interpret the magnitude of the estimates. However, I find throughout positive responses to the reform in West Germany and negative or zero effects in East Germany.[10]

Next, I analyze potential determinants of the different responses in East and West Germany.[11] The share of eligible women under the old system was higher in East Germany

[10] Ideally, I would like to separate women in East and West Germany by eligibility under the old regime. Unfortunately, an investigation of the effect for further subgroups by partners' education is not possible because of small sample sizes. However, the opposing effects in subgroups shown in Table 2 potentially contribute to the low precision of the estimates in Table 3.

[11] Although the literature generally finds differences in, for example, the timing of births in East and West Germany (see, e.g., Kreyenfeld; 2004), descriptive evidence does not support a different selection of women into early motherhood (see Table A.1 in the Appendix). I neither find differences in mothers' age nor in mothers' education. An explanation might be that fertility trends in East and West Germany converge over time and are very similar for birth cohorts after 1970 (Goldstein and Kreyenfeld; 2011).

(Fendrich et al.; 2003). Thus, one important channel may be different income changes in the year after birth. As I have no information on income in the year before and after birth, I cannot test this channel. However, Kluve and Tamm (2013) report negative but insignificant income changes in East Germany between the year before and after birth. Further, Table 4 shows the reform effect on satisfaction with the financial situation of the household and on current net household income.[12] Women in East Germany are less satisfied with their financial situation although the effect is not significant. The effect on income is negative and significant at the 10% level for East Germany and small and insignificant for West Germany. Thus, the results underpin a long-run negative income effect in East Germany.

A second channel might be an effect of the reform on marriage behavior. In East Germany marriage rates are generally low because of lower religiousness, a higher labor market attachment of women, and a lower dependence on partners' income (Konietzka and Kreyenfeld; 2005). Table 4 shows that the reform affects the probability of a current marriage differently in the two regions. The effect is negative in both East and West Germany, but more pronounced and significant for East German mothers. If marriage has a (short-run) positive effect on satisfaction (Lucas and Clark; 2006), the absence of such an effect might explain the negative effect in East Germany.

The reform also promoted fathers' involvement in child-rearing. A higher involvement might have positive effects on satisfaction. Table 4 gives the effect on satisfaction with the relationship and the effect on the probability that parents share child-rearing. Here, I find significant positive effects in West Germany and zero effects in East Germany. Thus, an absent effect on fathers' involvement might also explain different responses in East and West Germany. An explanation for the absent effect, however, might be a higher level of fathers' involvement in East Germany in general (see Table A.1).

Other potential channels include effects on the return to work after birth and social norms. In East Germany, the former socialist part of Germany, women are traditionally

[12] Unfortunately, some variables are not available in all waves of the Pairfam or comprise missing values. Consequently, for some of the outcomes in Table 4 the sample size reduces.

more attached to the labor market (see, e.g., Krueger and Pischke; 1995). The 2007 reform was one of the first reforms explicitly promoting maternal labor market participation. If women feel less social pressure in the decision to return to work, there might be positive reform effects on satisfaction. However, this effect might be especially important in West Germany and explain some of the found differences between East and West Germany. Literature on reform effects on the return to work finds a positive effect in East and West Germany (Bergemann and Riphahn; 2010, 2011; Geyer et al.; 2012; Wrohlich et al.; 2012). Kluve and Tamm (2013) report an increased employment probability for East German mothers 1.5 years after child birth and no change in employment probabilities for West German mothers. Thus, a higher labor market attachment of East German women may relate to an earlier return to work. We generally expect a positive effect of employment on satisfaction. Consequently, an earlier return to work does not explain the negative effect in East Germany in Table 3.

In sum, a negative reform effect on satisfaction in East Germany may be attributed to a lower or even negative direct income effect, an absent positive effect through a higher involvement of fathers, or through an absent positive effect of marriage. The negative effect on current net household income indicates long-term financial consequences of the reform.[13] One explanation for long-term negative income effects might be the reduced marriage probability and a loss of tax benefits. As the German tax system generally favors traditional family types (see, e.g., Kreyenfeld; 2004), unmarried couples face losses in net income.

7 Sensitivity analysis

Table 5 presents results on two sensitivity analyses with different sample selection criteria. Each coefficient represents a separate linear regression. As earlier results show that

[13] Cygan-Rehm (2013) finds that East German mothers reduce birth spacing after the reform. This might also affect the financial situation of the household negatively. However, the negative effect on household income is also present if I exclude mothers with subsequent births. In addition, I am able to control for subsequent births and the estimates are robust (not presented).

estimates for the full sample are difficult to interpret because of offsetting effects in subgroups, I present each sensitivity analysis for four groups: women with a highly educated partner, women with no or a low educated partner, women living in West Germany, and women living in East Germany.

Panel A checks whether the results are affected by pre- and postponed births between years 2006 and 2007. Neugart and Ohlsson (2013) and Tamm (2013) both show that women shifted their births to become eligible for the old or new subsidy. This confounding factor potentially affects the results. Panel A strongly supports my baseline findings. When births from January and February are excluded, the estimates are more precise and similar in direction. There are two changes in the sign of the effect for women with no or low educated partners. However, the effect is statistically indistinguishable from zero.

A critical assumption for diff-in-diff strategies is that general differences between mothers who gave births in the first and last quarters of years are similar for treatment and control group. If these differences in satisfaction change, the results are biased. The more cohorts I select as control group, the more likely the assumption of constant trends is violated. Panel B gives the results for a reduced sample in which I use only births from 2005/6 and 2006/7. The sample size declines considerably and consequently the estimates loose precision. However, also with fewer cohorts considered in the control group, the estimates show the patterns reported in the baseline results.

8 Conclusion

This study analyses the effect of a reform of the German parental leave benefits on the subjective well-being of young mothers. Women who gave birth to children after 1st of January 2007 are eligible for a new earnings-related subsidy which replaces 67% of pre-birth labor earnings. Under the former regime, a means-tested subsidy supported especially low income families. Also, the new subsidy explicitly promoted fathers' involvement in child-rearing.

The reform possibly affects the subjective satisfaction of mothers through different channels. One potential channel might be an income effect. The reform changed the total amount of benefits and the duration of pay. Whereas eligible women under the old regime received the maximum amount of EUR 300 per month for two years, the duration was shortened to one year, the amount is earnings-related but with a minimum transfer of EUR 300 and a maximum of EUR 1800 per month. Consequently, the reform affects women differently depending on the eligibility under the old regime. However, also a higher involvement of fathers in child-rearing and changes in marriage behavior potentially affect well-being.

My empirical approach takes advantage of the low anticipation of the reform. At the time of conception of children born in the first quarter of 2007, the parents could not have known about the impending reform. Therefore, I use a regression discontinuity design with a diff-and-diff extension. I compare mothers of children born in the first quarter of 2007 to mothers of children born in the last quarter of 2006. Additionally, I use mothers of children born at the turns of years 2003/4 to 2004/5 as control group to account for seasonal effects in satisfaction, i.e., general differences in the satisfaction of women who gave birth in first and last quarters of subsequent years.

The empirical results show that the reform affected women differently depending on their former eligibility for the means-tested subsidy. Although small samples lead to a low precision of the estimates, women who were potentially eligible for the old subsidy are less satisfied then women who were not eligible. This pattern is in line with a reduction of income losses after child birth. Furthermore, women responded very differently in East and West Germany. Whereas West German mothers are on average more satisfied, East German mothers are less satisfied after the introduction of the new subsidy. As a potential channel, the results show West German fathers to involve more in child-rearing after the reform introduction. This positive effect on fathers' involvement is absent in East Germany. Also, I find that women in East Germany do marry less frequently after the reform and that they experience a reduction in current net household income 1.5 to

5.5 years after child birth. Thus, the results show a long-term financial disadvantage in East Germany after the reform.

One interpretation of the results is that the reform induced unintended side effects. The reform on one hand and the German tax system on the other support very different family types. A recent study from Thévenon (2013) shows that among all OECD countries Germany supports single breadwinner families the most over equal dual earner couples. The support of married couples in combination with the presented negative reform effect on marriage may lead to an unintended financial disadvantage which leads to considerable decrease in mothers' satisfaction.

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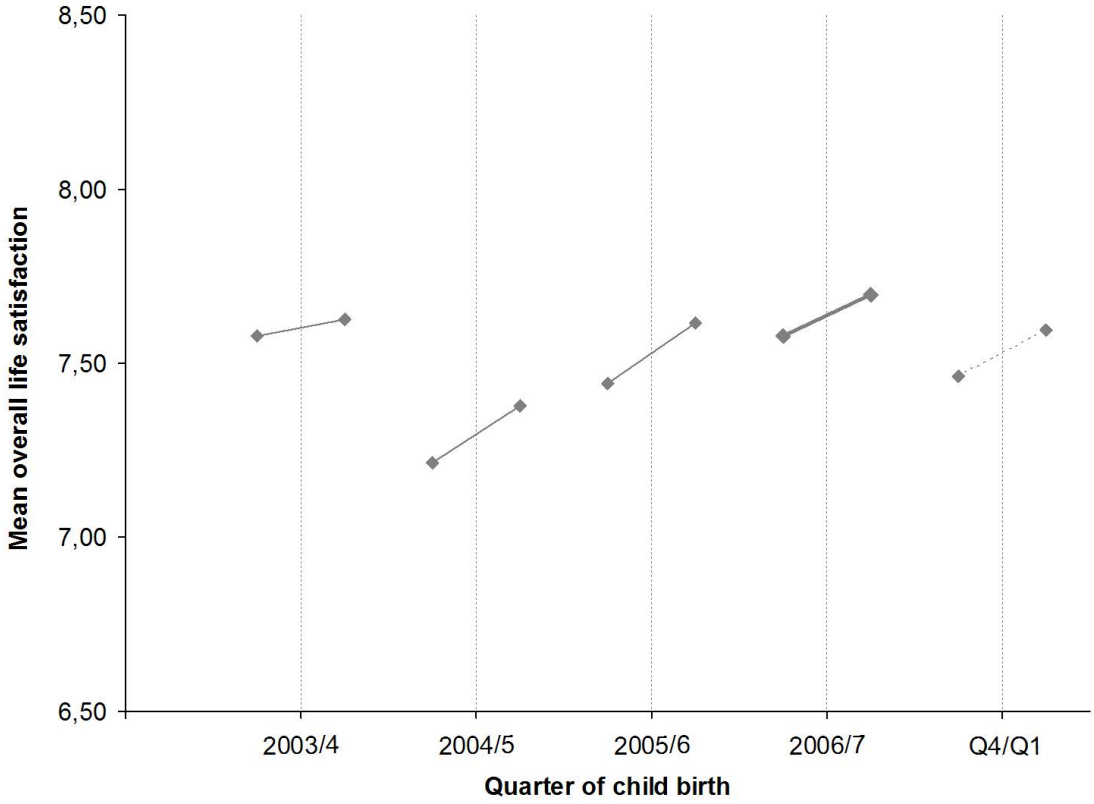
Figure 1: Average life satisfaction of mothers by quarter of child birth



Note: The plot shows raw data. Each set of connected dots compares average overall life satisfaction of mothers who gave birth to a child one quarter before and after a particular turn of the year.
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 1: Descriptives

	2006/07		2003/04 - 2005/06		Differences		
	Q4	Q1	Q4	Q1	1st	2nd	Diff-in-Diff
					(2)-(1)	(4)-(3)	(5)-(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variables: Mothers' satisfaction with							
Life overall	7.579	7.697	7.386	7.530	0.118	0.144	-0.026
School, Training, and Job	6.437	6.555	6.497	6.282	0.118	-0.216	0.334
Friends and social network	7.579	7.328	7.455	7.392	-0.252	-0.063	-0.189
Family	8.270	8.941	8.545	8.834	0.671	0.289	0.382
Mothers' characteristics							
Mothers age	27.794	27.521	26.725	26.895	-0.273	0.170	-0.443*
Lives in East Germany	0.317	0.370	0.344	0.431	0.052	0.087	-0.035
Non-German nationality	0.175	0.101	0.169	0.193	-0.074	0.024	-0.098
Secondary education							
Lower secondary	0.413	0.403	0.360	0.376	-0.009	0.016	-0.025
Intermediate secondary	0.278	0.345	0.397	0.392	0.067	-0.005	0.071
Higher secondary	0.310	0.252	0.243	0.232	-0.057	-0.011	-0.046
Postsecondary education							
Currently enrolled/no degree	0.278	0.210	0.333	0.359	-0.068	0.026	-0.093
Vocational training	0.579	0.664	0.534	0.536	0.085	0.002	0.083
Technical school / civil servant training	0.032	0.025	0.069	0.039	-0.007	-0.030	0.024
College / university	0.111	0.101	0.063	0.066	-0.010	0.003	-0.013
Pre-birth marital status							
Single	0.040	0.042	0.021	0.066	0.002	0.045	-0.043
Married	0.563	0.555	0.524	0.586	-0.009	0.062	-0.071
Divorced	0.016	0.059	0.021	0.017	0.043	-0.005	0.048
Cohabitation	0.365	0.294	0.333	0.254	-0.071	-0.079	0.008
Marital status missing	0.016	0.050	0.101	0.077	0.035	-0.023	0.058
Current partner characteristics							
Partners' age	33.222	33.124	30.981	31.755	-0.098	0.773	-0.872
Secondary education							
Lower secondary	0.492	0.378	0.328	0.337	-0.114	0.009	-0.123
Intermediate secondary	0.310	0.353	0.317	0.376	0.043	0.058	-0.015
Higher secondary	0.119	0.134	0.190	0.166	0.015	-0.025	0.040
Missing information/no partner	0.079	0.134	0.164	0.122	0.055	-0.042	0.098
Children's characteristics							
Child age in months	41.373	39.866	51.751	50.801	-1.507	-0.950	-0.557
Number of observations	126	119	189	181			

Notes: * p<0.1, ** p<0.05, *** p<0.01
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 2: The effect of *Elterngeld* on subjective well-being by partners' education

	Satisfaction with			
	Life overall	School and Job	Social network	Family
PANEL A: Full Sample				
Reform	0.027	0.200	-0.297	0.373
	(0.401)	(0.539)	(0.603)	(0.427)
1st quarter	0.213	-0.374	0.020	0.370
	(0.235)	(0.326)	(0.319)	(0.266)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	-0.502	0.172	-0.834	-0.248
	(0.429)	(0.623)	(0.559)	(0.439)
2005/06	-0.360	0.682	-0.253	0.146
	(0.642)	(0.969)	(0.898)	(0.686)
2006/07	-0.090	0.330	0.137	0.428
	(0.962)	(1.353)	(1.318)	(1.061)
N	615	615	615	615
PANEL B: Partners' education intermediate or high				
Reform	0.988 *	1.811 **	0.695	1.181 *
	(0.540)	(0.880)	(0.737)	(0.667)
1st quarter	-0.183	-0.560	-0.412	0.367
	(0.317)	(0.410)	(0.396)	(0.372)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	-0.330	-0.015	-1.283 *	-0.422
	(0.608)	(0.987)	(0.730)	(0.642)
2005/06	-0.593	0.082	-1.759	-1.211
	(1.035)	(1.650)	(1.342)	(1.083)
2006/07	-0.815	-1.839	-2.348	-1.920
	(1.470)	(2.274)	(1.893)	(1.720)
N	305	305	305	305
PANEL C: No partner or low educated partner				
Reform	-0.837	-0.722	-1.161	-0.274
	(0.600)	(0.698)	(0.813)	(0.616)
1st quarter	0.644 *	-0.196	0.577	0.434
	(0.382)	(0.507)	(0.466)	(0.382)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	-0.816	0.217	-0.329	-0.015
	(0.681)	(0.941)	(0.817)	(0.664)
2005/06	-0.148	1.255	1.370	1.528
	(0.972)	(1.373)	(1.175)	(0.995)
2006/07	0.556	1.968	2.580	2.418 *
	(1.488)	(1.963)	(1.753)	(1.428)
N	310	310	310	310

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at mothers' person number. Additional control variables include child care availability for under 3 and 3-6 year olds by state and year, dummies for child age, survey year, mothers' age, East Germany, mothers' education, mothers' nationality non-German, quartiles of fathers' age, pre-birth marital status.
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 3: The effect of *Elterngeld* on subjective well-being - East-West comparison

	Satisfaction with			
	Life overall	School and Job	Social network	Family
PANEL A: West Germany				
Reform	0.318	1.163 *	0.539	0.982 *
	(0.560)	(0.665)	(0.802)	(0.590)
1st quarter	-0.084	-0.571	-0.352	0.292
	(0.311)	(0.402)	(0.436)	(0.367)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	0.335	-0.281	-1.454	-0.074
	(0.974)	(0.797)	(1.008)	(1.279)
2005/06	1.156	0.274	-1.512	0.405
	(2.054)	(1.539)	(2.290)	(2.690)
2006/07	1.624	-1.261	-2.274	0.265
	(3.071)	(2.395)	(3.472)	(4.033)
N	388	388	388	388
PANEL B: East Germany				
Reform	-0.680	-1.064	-1.226 *	-0.035
	(0.584)	(0.942)	(0.694)	(0.643)
1st quarter	0.799 **	-0.090	0.614	0.264
	(0.399)	(0.595)	(0.479)	(0.496)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	-1.048 *	0.441	-1.333	-0.701
	(0.591)	(1.101)	(0.809)	(0.642)
2005/06	-1.092	0.594	-0.380	-0.193
	(0.814)	(1.360)	(1.138)	(0.811)
2006/07	-0.218	1.326	0.117	0.355
	(1.323)	(1.789)	(1.670)	(1.312)
N	227	227	227	227

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at mothers' person number. Additional control variables include child care availability for under 3 and 3-6 year olds by state and year, dummies for child age, survey year, mothers' age, mothers' education, nationality non-German, quartiles of fathers' age, pre-birth marital status.
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 4: Alternative outcomes: East-West differences

	Current net HH income	Satisfaction with Relationship	Financial situation	Currently married	Share Child rearing
PANEL A: Full Sample					
Reform	-130.152	-0.198	-0.139	-0.108	0.215 *
	(245.221)	(0.776)	(0.509)	(0.072)	(0.121)
N	562	304	540	615	493
PANEL B: West Germany					
Reform	-25.962	0.534	0.835	-0.027	0.241 *
	(391.424)	(1.103)	(0.708)	(0.071)	(0.140)
N	347	185	352	388	333
PANEL C: East Germany					
Reform	-515.448 *	-1.937	-0.906	-0.266 *	0.112
	(271.361)	(1.208)	(0.721)	(0.154)	(0.225)
N	215	119	188	227	160

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at mothers' person number. Additional control variables include child care availability for under 3 and 3-6 year olds by state and year, dummies for child age, survey year, mothers' age, mothers' education, mothers' nationality non-German, quartiles of fathers' age, pre-birth marital status.
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 5: Robustness checks - sample selection

	Satisfaction with			
	Life overall	School and Job	Social network	Family
PANEL A: Without December and January births				
High educated partner				
Reform	1.077	2.928 **	1.716 *	1.630 *
	(0.693)	(1.124)	(0.956)	(0.873)
N	201	201	201	201
No or low educated partner				
Reform	-1.762 **	-1.059	-0.017	0.393
	(0.725)	(1.157)	(1.095)	(0.814)
N	188	188	188	188
West Germany				
Reform	0.612	2.156 **	2.192 **	1.575 **
	(0.715)	(0.964)	(0.930)	(0.713)
N	256	256	256	256
East Germany				
Reform	-1.056	-0.327	-1.497 **	0.880
	(0.895)	(1.308)	(0.672)	(0.993)
N	133	133	133	133
PANEL B: Without birth cohort 2003/4 and 2004/5				
High educated partner				
Reform	1.275 *	1.318	0.197	1.175
	(0.677)	(0.936)	(0.834)	(0.842)
N	213	213	213	213
No or low educated partner				
Reform	-1.000	-1.145	-1.404	-0.193
	(0.679)	(0.905)	(0.853)	(0.651)
N	225	225	225	225
West Germany				
Reform	0.185	0.730	0.180	1.042
	(0.665)	(0.802)	(0.941)	(0.788)
N	263	263	263	263
East Germany				
Reform	-0.561	-0.848	-0.661	0.116
	(0.636)	(1.122)	(0.714)	(0.781)
N	175	175	175	175

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at mothers' person number. Additional control variables include child care availability for under 3 and 3-6 year olds by state, dummies for child age, survey year, mothers' age, mothers' education, mothers' nationality non-German, quartiles of fathers' age, pre-birth marital status..
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Appendix

Table A.1: Descriptive evidence: East-West differences

	West Germany	East Germany	Full sample
Current net household income	2122.594	1763.744	1985.311
	(1054.565)	(735.047)	(960.454)
Currently working	0.433	0.449	0.439
	(0.496)	(0.499)	(0.497)
Currently married	0.776	0.463	0.660
	(0.418)	(0.500)	(0.474)
Father is involved in child-rearing	0.372	0.544	0.428
	(0.484)	(0.500)	(0.495)
Current mothers age	27.080	27.264	27.148
	(1.258)	(1.259)	(1.260)
Child age in months	45.881	49.035	47.046
	(12.887)	(10.288)	(12.081)
Mothers' secondary schooling			
Lower secondary	0.412	0.335	0.384
	(0.493)	(0.473)	(0.487)
Intermediate secondary	0.358	0.366	0.361
	(0.480)	(0.483)	(0.481)
Higher secondary	0.229	0.300	0.255
	(0.421)	(0.459)	(0.436)
Social norms: Agrees to statement			
Mothers' should focus family life	0.450	0.253	0.374
	(0.498)	(0.436)	(0.484)
Working mothers' harm children aged < 6	0.407	0.167	0.314
	(0.492)	(0.374)	(0.465)

Notes: * p<0.1, ** p<0.05, *** p<0.01. Table gives cell means, standard deviations (in parentheses).
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.