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DISCUSSION PAPER SERIES

IZA DP No. 11533

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ABSTRACT

Paid Parental Leave and Families' Living Arrangements*

We examine how a paid parental leave reform causally affected families' living arrangements. The German reform we examine replaced a means-tested benefit with a universal transfer paid out for a shorter period. Combining a regression discontinuity with a difference-in-differences design, we find that the reform increased the probability that a newborn lives with non-married cohabiting parents. This effect results from a reduced risk of single parenthood among women who gained from the reform. We reject the economic independence hypothesis and argue that the reform effects for those who benefited from the reform are consistent with hypotheses related to the improved financial situation of new mothers after the reform and increased paternal involvement in childcare.

JEL Classification: J12, J13, J18, I30

Keywords: parental leave, living arrangements, marriage, cohabitation, single motherhood, child well-being, early childhood

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1. INTRODUCTION

An extensive literature documents a significant relationship between families' living arrangements and children's wellbeing. Specifically, children raised by single mothers have worse educational, labor market, and mental health outcomes compared to children living with both biological parents (McLanahan and Sandefur 1994). Analyses with more rigorous research designs (e.g., Painter and Levine 2000, Lang and Zagorsky 2001, Finlay and Neumark 2010, McLanahan, Tach, and Schneider 2013) usually yield smaller adverse effects of fragile family structure and fathers' absence than earlier cross-sectional studies. Nevertheless, Blau and van der Klaauw (2013, p. 579) argue that the differences in outcomes between children from different family structures "are generally quite large and dwarf the effects of income and maternal employment". Therefore, families' living arrangements may be a central source of inequality and have potentially long lasting consequences for children. These issues are particularly important given the trend of the past 50 years to alternative living arrangements (Lundberg, Pollack, and Stearns 2016).

Despite the robust relationship between families' living arrangements and children's outcomes, we still know little about how public policy affects families' living arrangements. So far, the literature on the intended and unintended effects of welfare reforms on living arrangements is dominated by research on US programs such as the Temporary Assistance for Needy Families (TANF) or the Earned Income Tax Credit (EITC) (e.g., Dickert-Conlin and Houser 2002; Bitler, Gelbach, Hoynes, and Zavodny 2004; Bitler, Gelbach, and Hoynes 2006; Fitzgerald and Ribar 2004; Blank 2002; Grogger and Karoly 2005; Ratcliff, McKernan, and Rosenberg 2002; or Hu 2003).[1] Most studies examine the effects on the marital and cohabitation status of women and focus on the bottom tail of the income distribution: while some studies find effects of public policy on marital status (e.g., Bitler, Gelbach, and Hoynes 2006), some

[1] An exception is the study by Gregg, Harkness, and Smith (2009) who investigate effects of UK welfare reforms on a broad range of outcomes for lone mothers.

do not (e.g., Fitzgerald and Ribar 2004), and others emphasize heterogeneous effects (Dickert-Conlin and Houser 2002, Hu 2003).[2]

Outside the US, and more closely related to our analysis, two recent contributions from Scandinavian countries examine how parental leave quotas for fathers affect family stability. Their results are inconsistent. For Sweden, Avdic and Karimi (forthcoming) examine the effect of introducing a daddy month on marital stability and find that the reform increased the risk of marital separation three years after childbirth by about eight percent. For Iceland, Olafsson and Steingrimsdottir (2016) show that extending parental leave by up to three months that are reserved for fathers decreased the risk of divorce within ten years after childbirth.

Only a few studies examine the effects of public policies on children's living arrangements. For instance, Acs and Nelson (2004) investigate the effect of specific elements of TANF on children's and women's living arrangements. They find some evidence that family caps increase the probability of children living with their parents, and that child-support enforcement measures reduce the incidence of single parenthood. Bitler et al. (2006) show that the US welfare reform of 1996 reduced the probability of living with an unmarried parent. In contrast, Blau and van der Klaauw (2013) examine various determinants of family structure and conclude that welfare benefits have rather small effects compared to the substantial effects of wage rates and tax incentives. Overall, the literature found mixed effects of welfare reforms on families' living arrangements, focused almost exclusively on the US, and has not examined the effects of other public policy programs.

The purpose of our paper is to address these gaps by examining the effect of a paid parental leave reform on families' living arrangements. The German reform we analyze replaced a means-tested benefit with a more generous universal transfer that was paid out for a shorter period. Whereas the reform's effects on maternal labor supply and family income are

[2] For additional contributions see also Ellwood (2000), Schoeni and Blank (2000), Cancian and Meyer (2014).

well-documented (see Section 2 for an overview), we still know little about its potential impact on families' living arrangements.

We contribute to the broader literature on the link between public policies and families' living arrangements in several ways. First, while most previous studies draw on samples of single mothers and welfare recipients, we examine a universal reform that affected all families across the income distribution. Given that the reform implied new or increased transfers for some families ("winners") and reduced transfers for other families ("losers"), we pay particular attention to potentially heterogeneous effects. Second, we consider various mechanisms through which the reform might affect living arrangements such as changes in economic independence, spousal bargaining powers, improved maternal financial situation, new incentives for paternal involvement in child rearing, and marriage disincentives deriving from the income tax code. Third, by investigating the potentially different effects for girls and boys, we contribute to the literature on the link between paternal preferences for a child's gender and living arrangements (e.g., Dahl and Moretti 2008). Finally, we provide evidence for a country outside the US and thus for a different institutional and cultural setting (see, e.g., Stevenson and Wolfers 2007). Our study is particularly relevant to other countries that consider introducing an earnings-dependent parental leave scheme, especially if these countries share similarly low fertility and maternal employment rates.

To identify the causal effect of changes in parental leave benefits on families' living arrangements, we combine a regression discontinuity with a difference-in-differences design. Using data from the German Micro Census, we find that the reform increased the probability that a parents cohabit, and that the positive effect persists beyond the benefit take-up period. This effect is mainly driven by a reduced risk of single motherhood and not by a shift away from marriage. These results are robust to numerous sensitivity tests. Our estimates reject the economic independence hypothesis and are consistent with alternative hypotheses related to the improved maternal financial situation and increased paternal involvement in childcare.

Unfortunately, we cannot separate these effects due to data limitations, which also prevent us from analyzing the dynamics of families' living arrangements. Finally, boys and girls are affected differently: after the reform, girls are more likely to live with their fathers compared to the pre-reform situation. The living arrangements of boys remain unchanged. This reduces a prior disadvantage of daughters who were significantly more likely to live with single mothers than sons.

The paper proceeds as follows. Section 2 provides information about the parental leave reform and related literature. Section 3 illustrates the mechanisms through which the reform might affect families' living arrangements. Section 4 describes the data and our empirical approach. Section 5 discusses our key estimation results and section 6 shows that they pass numerous sensitivity checks. Section 7 concludes that public policy reforms may generate unintended, yet important, effects for families' living arrangements.

2. INSTITUTIONS

The German family policy includes three relevant programs aiming at the wellbeing of parents and newborns: first, maternity leave and maternity benefits are available from six weeks before to eight weeks after childbirth. Second, parents can take parental leave which provides job protection for up to three years after birth (cf. Dustmann and Schönberg 2012).

Third, parents are entitled to parental leave benefits. This program substantially changed in 2007. Prior to the reform, "child-rearing benefits" (*Erziehungsgeld*) were means-tested and paid a maximum of 300 Euro per month for up to 24 months.[3] The eligibility criteria of the means test related to the family income: parents were eligible for full child-rearing benefits if

[3] The payout over 24 months is called the regular benefit version. Alternatively, parents could choose a payout of 450 Euro per month for 12 months, called the budget version. As only a minority of about 13 percent used the budget version, our description focuses on the regular benefit version.

their annual net income was below a certain threshold.[4] The benefits were exempted from income taxation.

Parents of children born after the reform became effective on January 1, 2007 are newly entitled to "parents' money" (*Elterngeld*). This program had three main objectives: to financially support young families, to strengthen mothers' employment incentives after birth, and to enhance paternal involvement in child care. The new benefit generally amounts to two thirds of average net earnings in the 12 months prior to childbirth for the parent who reduces employment after birth. Parents employed part-time after childbirth receive a transfer of 300 Euro per month as a minimum and additionally up to two thirds of the drop in earnings if a reduction in hours worked occurred after the birth. The minimum benefit of 300 Euros per month is available also for those previously not employed. The maximum *Elterngeld* transfer is capped at 1,800 Euro per month. Similar to the previous child-rearing benefits, parents' money is not subject to income tax. However, the new benefit is considered for the calculation of the applicable tax rate in the progressive tax system and thus causes an increase in tax rates for taxable income (progressivity effect, *Progressionsvorbehalt*).[5]

One parent can receive the benefit for up to 12 months and the other parent for additional two months ("daddy-months") if both live in one household with the child and if they personally care for the child. The "daddy-months" regulation was introduced to incentivize paternal involvement in child rearing and to support the return of mothers to the labor force one year after birth. Parents are free to split the total of 14 months of benefits between themselves and to use them simultaneously; a single parent is eligible for 14 months.[6]

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[4] If net income exceeded the threshold, payouts were reduced. These thresholds differed for couples and single parents and varied with the number of children in the household. They also differed for benefits to be paid in months 1-6 vs. 7-24 after a birth.

[5] LaLumia et al. (2015) discuss that the lack of pro-rated income tax deductions has distorting effects for the timing of births in the United Status. This phenomenon does not exist in Germany as fertility related tax deductibles are pro-rated.

[6] Parents can double the transfer period of the new *Elterngeld* benefit if the monthly benefit is halved. Only about ten percent of recipients use this option (STBA 2013).

Administrative statistics (see, e.g., STBA 2012) indicate that the share of fathers utilizing paid parental leave jumped from about 2.5 percent before the reform to 15 percent in 2007, the first post-reform year. After the reform, 13 percent of fathers and 2 percent of mothers who utilize paid parental leave in the first year received the maximum amount of 1.800 Euro (STBA 2008). Figure 1 shows that the share of father's taking up the new benefit increased continuously after the reform and reached 32.3 percent for births of 2013 (STBA 2015a). Geisler and Kreyenfeld (2012) point out that after the reform, leave taking increased the most among highly educated fathers. While the average duration of mothers' transfer receipt remained constant at around 11.7 months, the average duration of fathers' transfer receipt fell from 4.2 months for births in 2007 to 3.1 months for births in 2013 (STBA 2015b) conditional on benefit receipt.[7]

Compared to the prior means-tested benefit (*Erziehungsgeld*), the new *Elterngeld* benefit is more generous in terms of transfer amounts and less generous in terms of transfer durations, as it runs for only 12 (or 14) as opposed to 24 months before the reform given eligibility. The reform thus generated losers among lower income parents who lost 12 months of transfers after the reform, i.e., up to 3,600 Euro. It generated winners among higher income parents who newly receive a generous transfer of up to 21,600 Euro. In relative terms, the reform's losers lost at least 22 percent of their annual net household income since only those with an annual income of less than 16,500 Euro received the maximum benefit amount prior to the reform. The relative gain for winners reached 33 percent of net annual household income: only double-earner households with net annual incomes of at least 64,800 Euro (i.e. 2,700 Euro

[7] The share of fathers receiving benefits no longer than 2 months increased from 65.3 to 78.9 percent between 2007 and 2013. The share of mothers receiving benefits for 10-12 months increased in the same period from 86.6 to 92.4 percent (see STBA 2008, 2015b). Jointly these numbers might suggest the development of a new social norm where fathers take parental leave for 2 months.

monthly per parent) receive the maximum benefit amount. Clearly, behavioral responses to the reform may differ between the two groups.

Prior studies have examined the reform's effects on related issues (for a comprehensive review of the economic and sociological literature, see Huebener et al. 2016). For maternal labor market outcomes, numerous studies document that the reform achieved its intended aims i.e., mothers with high pre-birth earnings reduced their employment in the first year after childbirth, whereas mothers with low pre-birth earnings increased their labor supply in the second year after childbirth (e.g., Wrohlich et al., 2012; Kluve and Tamm, 2013; Bergemann and Riphahn, 2015; Geyer et al., 2015; Kluve and Schmitz, 2018). Overall, the reform raised net disposable household income in the first year after childbirth by about 20 percent (Wrohlich et al., 2012) with larger benefits accruing to children of highly educated mothers. For fathers, Geisler and Kreyenfeld (2012) find a significant increase in propensity to take parental leave after the reform. However, using a non-representative survey, Kluve and Tamm (2013) find no significant reform effects on fathers' involvement in total childcare in the first year after a birth and. Thus, the division of household labor remained rather unchanged (similar to Ekberg et al. 2013 for Sweden). Two contributions examine the potential fertility effects of the reform: Cygan-Rehm (2016) finds a change in the timing, rather than the incidence, of higher order births. Raute (2015) examines the effect on all parities and finds that a 10 percent increase in benefit levels increased the probability of giving birth in a given year by a mere 1.1 percent Finally, although focusing on maternal labor supply, Kluve and Schmitz (2018) also look at the probability of being married for different subsamples of mothers. In some specifications, they find significant reductions in marriage rates and explain this pattern with the reduced tax incentives for married couples after the reform. We substantially extend their analysis, by investigating the potential effects on various living arrangements (not only marriage) and relating our findings to alternative hypotheses on the mechanisms.

3. MECHANISMS AND HYPOTHESES

In this section, we draw from economic models of the family (Becker, 1991; Browning, Chiappiori, and Weiss 2014) and prior findings to illustrate the pathways through which the parental leave reform might affect families' living arrangements. For clarity, we focus our hypotheses in particular on the reform's effect on single motherhood versus partnerships.

First, parental leave might affect living arrangements through changes in income. In our case, the new parental leave benefit increased net disposable household income in the first year after childbirth by about 20 percent on average (Wrohlich et al., 2012). The economic independence hypothesis (Cancian and Meyer 2014) posits that additional income reduces women's need to pool resources and thus *increases* the probability of single motherhood. Alternatively, an increase in income improves the relative financial situation of women and relaxes the household budget constraint lowering financial stress and improving household welfare. These mechanisms then may *reduce* the risk of single motherhood. Thus, theory predicts ambiguous effects of additional income on single motherhood.

In addition, from the perspective of collective bargaining models (Lundberg and Pollak, 1996), an increase in income may be interpreted as an improvement in women's bargaining power. The overall effect of higher female bargaining power on single motherhood is ambiguous: since additional income increases women's well-being outside of a relationship (the "divorce threat point", Manser and Brown 1980), higher divorce threat points may *increase* the probability of single motherhood provided the expected utility outside of a current relationship is higher than the expected utility from the current union. Alternatively, women may also use their increased bargaining power to negotiate more favorable terms concerning private consumption, resources spent on children, or the division of household labor cooperatively within the existing partnership (Lundberg and Pollack 2007). These changes are likely to *decrease* the risk of single motherhood. Taken together, the predictions from collective bargaining models for the effect of additional income on single motherhood are ambiguous.

Second, the reform affected the return-to-work incentives during the first two years after childbirth. In particular, highly educated mothers reduced their labor supply in the first year after childbirth, whereas less educated mothers increased their labor supply in the second year after childbirth (e.g., Wrohlich et al., 2012; Kluve and Tamm, 2013; Bergemann and Riphahn, 2015). These changes in maternal employment can affect living arrangements in various ways. According to Becker (1991), time spent with children can be interpreted as an investment in relationship-specific public goods. Thus, if mothers delay their return-to-work and spend more time raising children, this may *reduce* the risk of single motherhood through increased time-investments in children. In contrast, an early return-to-work may negatively impact the mother-child attachment (see, e.g., Brooks-Gunn, Han, and Waldfogel 2002) and generate emotional stress within the family that *increases* the risk of single motherhood. Additionally, returning to work may have second-order effects through changes in disposable income, where the mechanisms discussed earlier apply. Thus, the overall effect on single motherhood is ambiguous.

Third, the reform generates incentives for fathers to increase their paternal leave taking through higher wage replacements and by the introduction of two "daddy months".[8] The reform appears to have succeeded in this aim as the share of fathers taking parental leave increased substantially from less than 3 percent prior to the reform to around 34 percent in 2014 (STBA 2015b). We expect higher paternal leave taking to *decrease* the risk of single motherhood for two main reasons. First, according to the *father involvement hypothesis* (Morgan, Lye, and Condran 1988), if the reform increases paternal involvement in childcare, fathers invest more time in their children which should decrease the risk of single motherhood. Second, if both

[8] Some recent studies examine the effects of "daddy months". For instance, Cools, Fiva, and Kirkebøen (2015) examine how a Norwegian paternal leave quote affected child and family outcomes; Ekberg, Eriksson, and Friebel (2013) study the household behavior and labor market effects of a Swedish reform; and Rege and Solli (2013) investigate the effect of paternity leave on fathers' long-term earnings.

parents take parental leave at the same time, the spouses can spend more time together and enjoy more consumption complementarities. Therefore, higher paternal involvement should increase the consumption value of the relationship, thereby reducing the risk of single motherhood.

However, in our setting, low-income families may not be able to afford the income reduction implied by a father taking up leave, particularly if the father is the only income earner. Indeed, Reich (2011) shows that after the reform, fathers' leave taking positively correlates with maternal employment and income. Therefore, we expect a decline in single motherhood particularly among families where the father is not the only income earner, i.e., for mothers with own labor income who represent the winners of the reform.

Fourth, the reform may generate differential effects for boys and girls. Several studies suggest that fathers may have a preference for sons: for instance, Dahl and Moretti (2008) for the US and Choi et al. (2008) for West Germany show that sons are more likely to live with their fathers than daughters. Moreover, Morgan et al. (1988) document that couples with sons exhibit greater partnership satisfaction than families with only daughters. Therefore, a child's gender may affect single motherhood. If fathers have a preference for sons in Germany (as suggested by Choi et al. 2008), we would expect that fathers spend more time with their sons than daughters which should *decrease* the risk of single motherhood for sons, but not for daughters.

Fifth, the parental leave reform tightened the time limit for low-income women who can receive the new benefit only for 12 months, as opposed to 24 months previously. The US literature (e.g., Dunifon, Hynes, and Peters 2009) has shown that welfare reforms imposing stricter time limits increase the incentive to partner up to insure against future income losses. Hence, we expect that stricter time limits *reduce* the risk of single motherhood for mothers whose entitlement period was reduced, i.e., the losers of the reform.

While the previous mechanisms affect the propensity to partner versus to be a single mother, the reform also featured a tax element which affects the probability of marriage versus cohabitation. To understand the mechanism, consider the German income tax system which uses a progressive tax function (see Figure 2) and which provides a tax incentive for marriage.[9] Although the parental leave benefit itself is not taxed, the benefit is included in total household income which determines the tax rate of married couples (the so-called *progressivity effect*).[10] The tax rate is then applied to total household income less the benefit. Thus, the new parental leave benefit contributes to an increase in average taxes, particularly for low income households (see Table A.1). We therefore expect that the progressivity effect *reduces* the propensity to marry for individuals with low incomes for the period of benefit receipt.

Taken together, this discussion shows that the overall reform effect will depend on the relative sizes of these, at times opposing, effects (see Table 1 for a summary). Moreover, the effect may vary between mothers who gained and lost out from the reform. Ultimately, the effect of the reform on living arrangements is an empirical question.

4. METHODS AND DATA

4.1. EMPIRICAL STRATEGY

To identify the causal effect of the reform, we combine a regression discontinuity design with a difference-in-differences approach (similar to, e.g. Dustmann and Schönberg 2012, Danzer and Lavy 2018). Since the treatment for entire families is determined by a child's date of birth

[9] The German income tax system applies a tax splitting rule for married couples based on the joint income: if y_M and y_F are incomes of male and female spouses and y_C is the total income of the married couple, then a progressive tax function T(.) yields that $2*T(0.5 * y_C) \leq T(y_M) + T(y_F)$. Thus, for most couples, this generates a tax benefit of being married. This tax splitting advantage is largest for couples where one spouse earns the total income.

[10] Importantly, unlike some US welfare programs, being married is not a precondition for receiving the parental leave benefit (Cancian and Meyer 2014). Similarly, work requirements and financial sanctions, which feature in many US welfare reforms and which can have direct effects on living arrangements, do not exist in the German paid parental leave program.

our unit of analysis is the child. To examine how the reform affected families' living arrangements, we first compare the outcomes of children born shortly before and shortly after the reform came into effect. Given that we only have information on a child's month of birth, we consider a window of three months around the cut-off date January 1, 2007 to maximize precision while reducing the risk of potential confounders. Furthermore, parents of children born after March 2007 could already have known about the reform at the time of conception. To isolate possible seasonal effects (e.g., popular marriage seasons or child care cutoff dates) from those of the policy change, we also include children born in exactly the same months but in two pre-reform years (2004/05 and 2005/06) and two post-reform years (2007/08 and 2008/09) as a control group.[11] This strategy uses the sharp cut-off date of the reform's introduction to assign the treatment status within a difference-in-differences approach. Using cross-sectional data, we estimate a linear model of the form:

$$y_i = const + \alpha\, cohort0607_i + \beta\ Q1_i + \gamma\ (cohort\,0607_i \cdot Q1_i) + \ cohort_i^{'}\theta + x_i^{'}\delta + \varepsilon_i \quad (1)$$

where y_i denotes the living arrangement for a child i observed at the time of the interview. We study three mutually exclusive outcomes: living with a married couple, a cohabiting couple, and a single mother.[12] In a robustness test, we also use a multinomial logit estimator to account for the correlation between the three outcomes and reach the same conclusions. While it would be interesting to study the dynamics of the outcome variables or the date or duration of partnerships, unfortunately, our cross-sectional data do not provide information on. The indicator variable $cohort0607$ equals one if a child belongs to the treated birth cohort, i.e., was

[11] In Section 6, we discuss the potential threat that seasonality effects might change over time.

[12] Given that in Germany a mother's absence at early stages of baby's life is very rare, we do not consider single fatherhood. We exclude roughly 0.3 percent of children who live without the mother from our sample.

born around the reform's cut-off date.[13] We define a cohort as children born from October through the next March, so that the treated cohort 2006/07 comprises children born in the last quarter of 2006 and in the first quarter of 2007. We observe 995 births in the first quarter of 2007. Potential seasonal effects are captured by the variable $Q1_i$, which corresponds to an indicator for being born in the first quarter of a year versus the last quarter of the previous year. The vector $cohort_i$ includes a set of indicator variables that are equal to one if a child belongs to a particular non-treated birth cohort. $cohort_i$ comprises three indicator variables, the reference cohort is 2004/05.

Additionally, x_i covers a child's demographic characteristics such as its age in months (linear and squared), gender, an indicator for multiple births, and state of residence. We also control for maternal socio-demographic characteristics measured prior to childbirth such as her age in years (linear and squared), education, employment, and migration status. The terms α, β, γ, θ, and δ represent coefficients to be estimated, and ε_i is a random error term.

The identification strategy rests on three key assumptions to identify the coefficient of interest, γ. The first one is that a child's birth date was not affected by the reform's introduction. A major validity threat is that parents would have known about the reform at the time of conception. However, Kluve and Tamm (2013) show that the public discussion started in May 2006 when the governing parties agreed on the cornerstones of the reform. Parliament passed the new benefit in September 2006 and until then it was not clear whether the reform would eventually take place. This timeline and the fact that parents cannot perfectly plan the conception of a child provide convincing evidence that births in the first quarter of 2007 and any preceding employment history were still independent of the reform. The identification strategy would also fail if mothers could have influenced a child's birth date by bringing the delivery forward or backward. Indeed, there is evidence showing that a significant number of

[13] We identify intention to treat (ITT) effects. About 96 percent of all families took up the new benefit, however, we cannot identify them in our data (STBA 2012).

women postponed December births to January to become eligible for the new benefit (Neugart and Ohlsson 2012, Tamm 2012). However, because less than 8 percent of mothers with due dates in the last December week successfully postponed delivery (Tamm 2012), the presence of such timing should be of minor importance for our results. Nevertheless, we assess the sensitivity of the results to the exclusion of births around the cut-off day of the reform.

The second crucial assumption requires that any potential differences in outcomes between births that occur in the last and in the first quarter of a year follow a systematic pattern across years, which remains stable after the reform. We justify this assumption in three ways: first, we inspect aggregate register data from natality files that describe the probability of being born to married parents by quarter of birth (Figure A.1). The numbers uncover very similar patterns across quarters for several years preceding the reform. Thus, it seems plausible to assume that the seasonal patterns would persist in absence of the reform. Second, to test for potential changes in seasonality across years, in our sensitivity checks, we modify the composition of the control cohorts and obtain similar results. Third, in Section 6, we also demonstrate that our estimates remain identical when we additionally include month of birth effects, which capture potential seasonality in a fully flexible manner.

Third, the empirical design also requires that no other reform might have differentially affected treatment and control groups in our analyses. To ensure the validity of this assumption, we inspected related fields of the German family law. Importantly, the main changes of the divorce law (1978), child benefits (2004, 2009), and child support (2008) did not affect families with births shortly before and after the analyzed reform of paid parental leave in differently. These reforms therefore cannot confound our analysis.

4.2. DATA

We use data from the German Micro Census 2005-2012. Each survey year provides a one percent cross-section of the population currently living in Germany (for details see, e.g., STBA

2016). The key advantages of the Micro Census are the availability of information on an individual's month of birth and relatively large sample sizes. To examine the effect of the reform on families' living arrangements, we match a child to its parents if they live in the same household at the time of the interview. We restrict the sample to children born in Germany and belonging to the birth cohorts 2004/05 through 2008/09. We further restrict the sample to first-born children as the time around first birth is typically associated with the highest relative risk of marriage and union formation (see Köppen 2011).[14] For three reasons, we focus on children who reside in West Germany: first, they represent the vast majority (80 percent) of the population of interest. Second, West and East Germany differ in many aspects related to living arrangements.[15] Finally, while an analysis by region is of interest, our East German sample is too small for an informative investigation.

We observe the outcomes of analyzed cohorts of children at different ages in different Micro Census waves. For example, Micro Census 2007 reports the living arrangements of the treated cohort 2006/07 in their first year of life and Micro Census 2008 in their second year of life. Table 2 illustrates the relationship between age of the included cohorts and the reporting year and provides the number of observations, as well. We estimate the effect of the reform on

[14] In Section 6, we show that our results are robust to including higher order births.

[15] The most striking are probably the substantial differences in marriage rates and out-off wedlock childbearing. For example, in 2012, 62 percent of births in East Germany were out-off wedlock, compared to 28 percent in West Germany (STBA 2014). In addition, Bauernschuster and Rainer (2012) show vast and even increasing differences in sex-role attitudes between East and West Germany. Kreyenfeld and Geisler (2006) discuss that the two regions differ substantially with regard to attitudes towards cohabitation and maternal labor force participation. Schnabel (2016) demonstrates that East and West German women differ also in their labor supply, both in terms of participation and work hours. Nevertheless, in additional estimations, we found that adding East German observations to our sample does not change our main conclusions.

families' living arrangements during the benefit take-up period.[16] Therefore, we pool observations from the first and second year of a child's life, which gives in total 9,889 children.[17]

Since the new parental leave benefits depend on the pre-birth earnings of the parent who interrupts employment, i.e., usually the mother, we use the maternal pre-birth employment status to categorize whether a mother belongs to the group of reform winners (with pre-birth employment) or losers (without pre-birth employment). We classify mothers as winners if they had done any paid work during the twelve months prior to giving birth. Benefit take-up statistics support this classification because mothers with any pre-birth employment receive on average more than twice the benefit amount compared to mothers without any pre-birth employment: in 2011, the average benefits for the two groups amounted to 868 and 330 EUR, respectively (STBA 2013). Clearly, mothers who did not work prior to childbirth are worse-off after the reform: their average benefit of 330 EUR is only 10 percent higher than the previous payment and it is now paid out for 12 as opposed to 24 months. Conversely, the gains for working mothers, either from new eligibility or increased benefit payments, will outweigh the losses incurred by the shortened benefit period. Thus, the share of losers is higher among the non-working and the share of winners is higher among the group of working mothers. We adopt this definition for the remainder of the paper.

Our three dichotomous dependent variables indicate whether a child lives with a married couple, a cohabiting couple, or a single mother at the time of the interview; unfortunately the date does not inform on biological parenthood. We present descriptive statistics on the dependent and independent variables in Table 3 for the total sample and separately for the group of reform winners and losers. Overall, panel A reveals pronounced differences in living

[16] In additional estimations, we found that the results hold up in nature (though not in precision) when we consider children aged 0 and 1 years in separate estimations.

[17] These numbers exclude 0.6 percent of children with inconsistent information on living arrangements. Specifically, the data report that the children live with a single mother, but we observe that a child's father also lives in this household. Our results are robust to inclusion of these implausible observations.

arrangements between the two groups: children of reform losers are almost twice as likely to live with a single mother (17.7 versus 9.3 percent), and 8.4 percentage points less likely to live with married parents than children of reform winners. Panel B shows that differences in the characteristics of the children are fairly small in magnitude. Panel C reveals substantial differences in maternal characteristics as mothers who lost out from the reform give birth at younger ages and have lower levels of education. These maternal differences support our categorization into reform winners and losers.

Table 4 shows the covariate balancing for our main sample (RD-DID) and for a simple mean comparison of children born in the last quarter of 2006 and the first quarter of 2007. For the RD-DID samples, we replace the dependent variable in equation 1 by the covariates and show the estimated coefficient γ obtained when omitting the vector x. The table shows that the covariates are balanced between the groups, with some minor differences (multiple births, white collar) that are economically small, but statistically significant due to the large sample size. Furthermore, the standardized differences (Rubin 2001) are always below the threshold value of 0.2. In the results section, we show that we reach the same conclusions whether we control for none or all covariates. This lends credibility to the assumption that the socio-demographic characteristics do not systematically differ between the treatment and control groups, and that differences in the composition of mothers giving birth do not drive our results.

5. RESULTS

5.1 GRAPHIC AND BASELINE RESULTS

We start out by inspecting the graphical evidence: Figure 3 describes the development of the three analyzed living arrangements over time. The x-axis shows the month of a child's birth relative to the reform's introduction, so that zero corresponds to January 2007. Each dot displays the mean probability of being observed in a particular living arrangement in the first two years of life for sampled children born in a specific month. To relate the overall

development to the reform's introduction, we fit separate linear trends for the periods before and after the cut-off date.[18]

The upper plot shows that the probability of living with a married couple has continuously decreased over time. We do not observe a substantial change in the trend at the time of the reform though there is a small break around the cutoff. In contrast, the middle plot depicts a significant jump in the probability of living with a cohabiting couple after the reform. Importantly, the graph suggests that the reform shifted the trend in the cohabitation outcomes permanently and not only in the short run wake of the reform. Finally, the bottom plot reveals that the probability of single motherhood was increasing before the reform and then the trend shifted downwards. Here, the discontinuity in the trend is statistically not significant.

Next, we turn to our estimation results: Table 5 reports our key results on the effect of the parental leave reform on living arrangements in their first two years of a child's life (ages 0-1). Each cell shows the estimated coefficient γ obtained from a separate linear probability model and its robust standard error. The mutually exclusive outcome measures in columns 1 to 3 are indicator variables of whether a child lives with a married couple, with a cohabiting couple, or with a single mother, respectively. We first estimate the effects on the entire sample (panel A) and then separately for children of reform winners and losers (panels B and C).[19]

The results in panel A show that the reform significantly increased the probability of living with cohabiting parents in early childhood by 3.8 percentage points (column 2). This is a quantitatively large effect given the average incidence of roughly 16 percent before the

[18] Figures A.2 and A.3 in the appendix show the trends separately for reform losers and winners. We can plot the group-specific averages only by quarter because the sample sizes within monthly bins are too small.

[19] The number of observations in panels B and C do not sum up to the full sample size because we do not observe mother's employment status for about 4 percent of sampled children. However, for all tables included in the paper, we repeated the estimations for panel A after excluding the observations with missing information on mother's employment, and the results remained unchanged.

reform.[20] Interestingly, both alternative living arrangements contribute in similar magnitudes to the increase in cohabitation, where the estimates are statistically insignificant.

Panel B evaluates the reform effect for children whose mothers lost out from the reform. The point estimates in columns 1 and 2 suggest a shift away from marriage towards cohabitation, but the effects are not statistically significant. The effect on the probability of living with a single mother is close to zero. In contrast, panel C demonstrates that the reform significantly affected living arrangements of children whose mothers gained from the reform. The statistically and economically significant effects in columns 2 and 3 show that the probability of parental cohabitation increased by 4.3 percentage points which results largely from a reduced incidence of single motherhood among reform winners.[21] We do not find any notable shift away from marital unions (column 1).

Overall, the estimates in Table 5 show that the parental leave reform increased the probability that a child lives with cohabiting parents in the first two years of life. This goes along with a reduced incidence of single motherhood among the potential winners of the reform. We also find that this effect is persistent over time.[22]

5.2 Discussion

[20] Note that such large reform effects are not uncommon, as Bitler et al. (2006), for instance, find that waivers reduced the probability of living with unmarried parents by 14.4 percent, and even doubled the probability of living with neither parent in some groups.

[21] Note that the confidence intervals for the estimates in panel B and C overlap, so that we cannot conclude that the reform's effect is statistically different across the groups. Nevertheless, our results indicate that the average effects in panel A are mainly driven by families who gained from the reform (panel C). An alternative specification including a triple interaction term estimated on a pooled sample leads to similar conclusions, but the estimates are imprecise.

[22] Appendix Table A.2 presents the estimation results for the samples of older children (aged 2-3). The estimates show that the positive effects on the probability of being raised by both parents persist at ages 2-3 as the probability of being raised by a single mother decreases. The results suggest that the reform affects the decision to cohabit faster (i.e. for younger children) than the decision to marry. We observe positive though insignificant reform effects on living together as married couples only for older children.

How do these results relate to the mechanisms behind the changes in living arrangements discussed in Section 3? First, considering the economic independence hypothesis, we do not find the hypothesized increase in single motherhood among winners of the reform. The point estimates for children of reform winners suggest rather the contrary, i.e., a reduction in single motherhood accompanied by an increase in cohabitation. Thus, our study provides new evidence rejecting the economic independence hypothesis.

As discussed in Section 3, the significant shift away from single motherhood towards cohabitation is consistent with a number of mechanisms, in particular the increase in women's relative financial position, bargaining power within a cooperative household model, and household welfare; all these mechanisms are expected to decrease the incidence of single motherhood for the winners of the reform. Moreover, we hypothesized that the risk of being raised by a single mother should only decline for children of reform winners, i.e., children of working mothers. In this case, the fathers are not the only income earners and can potentially afford to reduce labor supply to take up parental leave. Matching the hypothesized patterns, we indeed observe a reduced incidence of single motherhood only amongst the group of reform winners and not amongst the group of reform losers. This change for reform winners is consistent with the father involvement hypothesis, which suggests that higher paternal leave taking strengthens the father-child attachment and results in lower incidences of single motherhood.

For children of reform losers, we do not observe any changes in the probability of living with a single mother but rather a shift away from married to cohabiting parents. The lack of an effect on single motherhood is at odds with our hypotheses regarding time limits and reductions in women's bargaining power and relative financial position, and household welfare. These mechanisms rather predict decreases in single motherhood. The insignificant shift away from marital unions towards cohabitation may reflect a loss in women's bargaining power since marriage in Germany provides stronger financial and legal security for women than

cohabitation. Moreover, this shift is broadly consistent with the tax disincentives; we test this hypothesis in more detail later.

Given the evidence that paternal preferences for sons might affect living arrangements (e.g., Dahl and Moretti, 2008; Choi et al. 2008), we perform additional estimations which include an interaction term between the reform effect and the gender of the child. Table 6 reports the results. We find that the decline in single motherhood is driven by families with newborn daughters. In contrast, the reform has no effect on the probability that fathers live with a single mother for boys.

A crucial question from a policy perspective is whether the reform balances a prior disadvantage in paternal involvement experienced by girls, or whether the reform generates new gender-specific early childhood inequalities in Germany. To examine the issue, we run linear probability models for the living arrangements separately for births occurring prior to the reform (2005-2006) and after the reform (2007-2012) including children born in all quarters of the survey years. Table 7 reports the estimates of the boy indicator. The results expose significant gender differences in living arrangements prior to the reform: We observe significantly higher probabilities for sons to live with married parents (1.8 percentage points), no significant gender difference regarding cohabitation, and a significantly higher propensity for daughters to live with single mothers (1.4 percentage points).[23] This pattern was particularly pronounced in families who gained from the reform (see panel C). The corresponding results for the post-reform period show that gender differences disappear. Thus, Tables 6 and 7 suggest that the reform contributed to balance prior disadvantages of daughters compared to sons, in particular in families privileged by the reform.[24]

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[23] For the US, Dahl and Moretti (2008) find that girls are 0.5 percentage points more likely to live without a father, but they include children aged 0 to 12 years.

[24] We also examined the sensitivity of the results in Table 5 with respect to gender-specific time trends by including additional interaction terms between the child's gender and the cohort indicator variables. The main results do not change.

These observed patterns might appear at odds with our hypothesis that boys should primarily benefit from greater paternal involvements if fathers prefer sons over daughters. We rationalize the observed patterns borrowing terms from the treatment effects literature: fathers may be *always-takers* when it comes to paternal involvement in the rearing of sons, i.e., fathers will always spend time with their sons irrespective of the parental leave system. However, for girls, fathers may be *compliers:* some fathers may not have taken the time to get involved with their daughters pre-reform, but do so post-reform when they newly take parental leave. Our findings are consistent with the interpretation that higher paternal leave taking after the reform benefits girls in particular.

With respect to the tax effects of the reform, Table 5 yields weak evidence that couples respond to the new short-term tax disadvantage of marriage: while the propensity of cohabitation increased as expected, it is not clear whether this change results from reduced marriage rates. As the progressivity effect is particularly large at household incomes below the median where average tax rates increase the most for a given shift in income (see Table A.1 and Figure 2), we investigate our hypothesis further and consider households grouped by the level of their income. We split the sample based on annual household income at the median, i.e., around 40,000 Euros.[25] If couples respond to the new tax disadvantages, then families below the median should display lower marriage probabilities, compared to couples above the median (see Table A.1 column 4). Table 8 presents the results on the propensity to marry for a sample of couples, only; we interact the reform effect with an indicator for whether a couple is above median income. We do not find a significant drop in marriage rates for families below median income; furthermore, none of the interaction terms are statistically significant. Overall,

[25] The result remains robust when we use a lower cut-off value of 20,000 Euros instead.

we find no support for the hypothesis that the reform dis-incentivizes marriage during the period of transfer receipt. [26]

6. ROBUSTNESS TESTS

In this section, we examine the robustness of our results with respect to a number of potential concerns. First, we estimate our models without any control variables to assess the importance of potentially confounding factors. Next, we vary the composition of the control groups to check whether potential seasonal effects (e.g., Buckles and Hungerman 2013, Fan, Liu, and Chen 2017) are stable across different control cohorts. We include month of birth dummies to more flexibly control for seasonality patterns. For completeness, we also show a simple comparison of outcomes up to three months before and after the cut-off, which corresponds to the first difference in our main approach and ignores seasonality. We also include higher-order births to show that our focus on first births does not entirely explain our results. We exclude January and December births to assess the importance of birth shifting. To test the stability of our results, we also use a larger observation window around the cut-off date. We change the econometric specification to multinomial logit, which accounts for the correlation between our three outcomes. We also apply a regression discontinuity design (RDD) as an alternative test for a discontinuous change in living arrangements around January 2007. Finally, we simulate two placebo reforms taking place the years before and after the actual reform. Table 9 presents the

[26] We also compare our results to Kluve and Schmitz (2018) who conclude that there is a negative impact of the reform on marriage rates. Their separately estimated coefficients for "Phase 1" (3-14 months after childbirth) and "Phase 2" (15-24 months after childbirth) for first-time mothers are -0.0300 and of -0.0035 (both insignificant), respectively. Averaging these estimates over the first two years after childbirth yields approximately an effect of -0.015, which is very similar to our estimate of -0.018. Their estimates only become significant (and larger in magnitude) in Phase 1 once they condition on additional covariates, including a dummy for single motherhood. We do not include this endogenous variable among our set of controls. Our conclusion about no statistically and economically significant effect on marriage holds in numerous specifications and robustness tests presented in Section 6.

results for the various checks separately for each group. For comparison, we include the baseline coefficient in the first row within each panel.

Starting with the pooled sample in panel A, we see that the main findings are highly robust. Model A2 shows that the effects do not depend on controlling for characteristics of the child or the mother at birth, confirming that the reform was unanticipated and hence uncorrelated with relevant observable characteristics. In specifications A3 and A4, we vary the time window of the reform to test whether our selection of the control group affects the results. Our main conclusions do not change even if we include only the latest pre-reform cohort (05/06) as a control group (A4). This conservative specification alleviates any concerns that seasonal effects might change over time and that potential fertility adjustments in the post-reform period bias our results. Our main estimates remain identical even if we fully flexibly control for seasonality by including month of birth dummies (A5). In contrast, specification A6 ignores any seasonal differences in outcomes between births occurring by the end and at the beginning of a year. Although the fist-difference estimates lead to similar conclusions, they rely on stronger assumptions compared to our main results. Next, we extend our sample by including higher-order births (A7), which does not alter our main conclusions. To assess whether couples anticipated the reform by changing the birth date, we drop January and December births (A8). The results are less precisely estimated, but the qualitative patterns remain the same. In specification A9, we use six instead of three months of observations around the cut-off date: the point estimates are nearly identical. The results in A10 display marginal effects of the reform calculated after a multinomial logit estimation and yield similar effects in sign and magnitude.

Next, we turn to our alternative identification approach – RDD – by estimating

$$y_i = const + \alpha\, post_i + \Phi m_i + mob_i^{'}\theta + x_i^{'}\delta + \varepsilon_i, \qquad (2)$$

where $post_i$ is an indicator for the post-reform period starting in January 2007. The "running variable" is the month of birth, m_i, which is normalized to 0 at the cutoff date. The linear term in m_i accounts for smooth trends in the outcomes over time, but we also explored the inclusion

of higher-order polynomials. The vector mob_i includes eleven calendar month of birth effects and x_i indicates covariates as before. We start with the full sample of 30 months before and after the cutoff (A11) as displayed in Figure 3. The estimates lead to the same conclusions as our main empirical approach. Additional regressions including quadratic and cubic specifications in the running variable yield similar results (not presented to save space). We then restrict the sample progressively to 24 and 18 months (A12 and A13) surrounding the cutoff, which corroborate prior conclusions.

Finally, we simulate two placebo reforms in the year after (A14) and before the reform (A15). We drop the actual reform cohort (06/07) from these estimations to avoid biased estimates. Our preceding analysis (see A3 and A4) has shown that our main estimates are not sensitive to excluding the post-reform cohorts. Thus for comparability with our baseline results and to maximize sample size, we continue to keep the post-reform cohorts in the placebo analysis. As expected, the placebo test results are insignificant lending credibility to the common trend assumption and confirming our main conclusions.

Panel B focuses on the subsample of mothers who lost out from the reform; given the small number of treated individuals in each test, the validity of the test results may be limited. We find that the estimates remain robust after dropping the control variables (B2) or changing the time windows (B3, B4, and B9). Again, we confirm our baseline results when we control for birth month dummies (B5), ignore seasonality (B6), include higher-order births (B7), and use the multinomial logit (B10) or the alternative RDD identification strategy (B11-B13). Only the estimated effect on cohabitation in B4 is significantly different from zero. We think that this large coefficient is likely due to sample variability as the sample size is very small. Although the coefficients flip sign once we exclude the January and December births (B8), the estimates are again not statistically significant. The placebo reform (07/08) in B14 yields no significant effects. However, we place some caution on the interpretation of the results for this group given the lack of precision and a significant placebo effect for the year 05/06 (B15).

Comparing the coefficients across rows for the potential winners of the reform in panel C, we see that the estimated effects are highly robust to the specification checks. In particular, changing the control variables (C2), varying the time windows (C3, C4, C9), controlling more flexibly for seasonality (C5), estimating a first-difference comparison (C6), and including higher-order births (C7) does not change our main conclusions. Omitting December and January births (see C8) even increases the magnitude of the point estimates. Estimating a multinomial logit (C10) yields identical marginal effects. The RDD approach (C11-C13) also supports our main conclusions. As expected, the two placebo reforms (C14-C15) yield insignificant results lending credibility to the common trend assumption. Overall, our main results are robust to numerous sample and specification changes. [27]

7. CONCLUSIONS

A large literature documents the relevance of families' living arrangements for the wellbeing and long run outcomes of children. However, we still know little about how public policy affects families' living arrangements. We investigate the causal effect of a recent paid parental leave reform on families' living arrangements. The German reform that we study replaced a rather small means-tested benefit (*Erziehungsgeld*) available for a subgroup of parents with a universal paid parental leave benefit (*Elterngeld*) which depends on prior labor income. For some parents (losers) the reform implied a loss of benefits amounting to at least 22 percent of their net household income, while for others (winners) the reform implied a gain of up to 33 percent of household income. To identify the causal effect of the reform, we combine a regression discontinuity with a difference-in-differences approach. The empirical analysis uses

[27] We also checked whether selective migration might bias the estimations, e.g., because migrants might move to Germany shortly before childbirth to become eligible for parental leave benefits. We dropped mothers who moved to Germany in the year of birth or the year prior to giving birth, and the results remained unchanged. We also tested to what extent our results are driven by mothers of non-German origin and found that excluding them from the analysis does not invalidate our main conclusions.

data from the German Micro Census, a large and representative annual survey. We focus on causal reform effects in the short run, i.e., the period of benefit receipt, but show that the effects persist after the end of the take-up period.

We hypothesize that the new parental leave benefit affects families' living arrangements via economic independence effects, changes in relative financial situation and spousal bargaining processes, new incentives for paternal involvement in child rearing, and income tax (dis-)incentives for marriage. In addition, as a large international literature suggests heterogeneous living arrangements for sons and daughters, we hypothesize differential effects.

We examine the probability that children live with married parents, with cohabiting parents, or with a single mother. Our results show clear causal reform effects on families' living arrangements. In particular, the propensity to live with cohabiting parents increased on average by about 4 percentage points. This effect size is substantial given that on average 16 percent of all newborns live with cohabiting parents and is consistent with previous estimates for other welfare reform effects. Graphical analyses show that the reform shifted trends in living arrangements permanently and not only in the short run wake of the reform.

We find no evidence supporting economic independence effects or responses to tax incentives. However, for the children of parents who benefited from the reform (winners), we find a decline in the probability of living with single parents and an increase in the propensity to live with cohabiting parents. These findings are consistent with different mechanisms, e.g., better maternal financial situation or enhanced paternal involvement in child rearing; due to data limitations, we cannot separate these effects. Among children whose mothers lost out from the reform, we find no significant effect on living arrangements. However, the estimates for this group are imprecise and do not justify far-reaching conclusions.

Interestingly, we find clear differences in reform effects by child gender. Prior to the reform, daughters were at a significantly higher risk of living with a single mother than sons. The reform-induced shifts to cohabitation contribute to balance this disadvantage as they are

exclusively observed for daughters. We find a sustained decline in single motherhood 2-3 years after the reform (-4.4 percentage points). Unfortunately, our cross-sectional data do not allow us to shed more light on the dynamics behind the estimated effects on families' living arrangements; we leave this issue for future research.

This study contributes to our understanding of a large public policy reform: the paid parental leave reform we analyse produced unintended, yet important, spill-overs for families' living arrangements. Among the 1.5 million single parent families in Germany, about 40 percent receive welfare (Achatz et al. 2013). Single parent families make up about 18 percent of all welfare recipients and in 2011 received about 5.4 billion Euro of transfers (BA 2012). If the parental leave benefit reform moved just one out of ten of these families into couple households and if these households do not require welfare, this change would roughly save 500 million Euro. This back of the envelope calculation suggests that the unintended side-effects of parental leave reforms are also fiscally relevant.

Our study contributes to the international literature (e.g. Avdic and Karimi forthcoming) by showing that a universal public policy reform affected living arrangements. Despite this evidence, such unintended spill-over effects are rarely considered in policy designs. If single motherhood indeed negatively affects child outcomes, the observed effect of paid parental leave may be beneficial for children. Governments considering parental leave reforms should be aware of these side-effects. Future work needs to evaluate whether the changes in families' living arrangements actually carry over onto children's human capital, e.g., their cognitive and non-cognitive skills, in the short and longer run.

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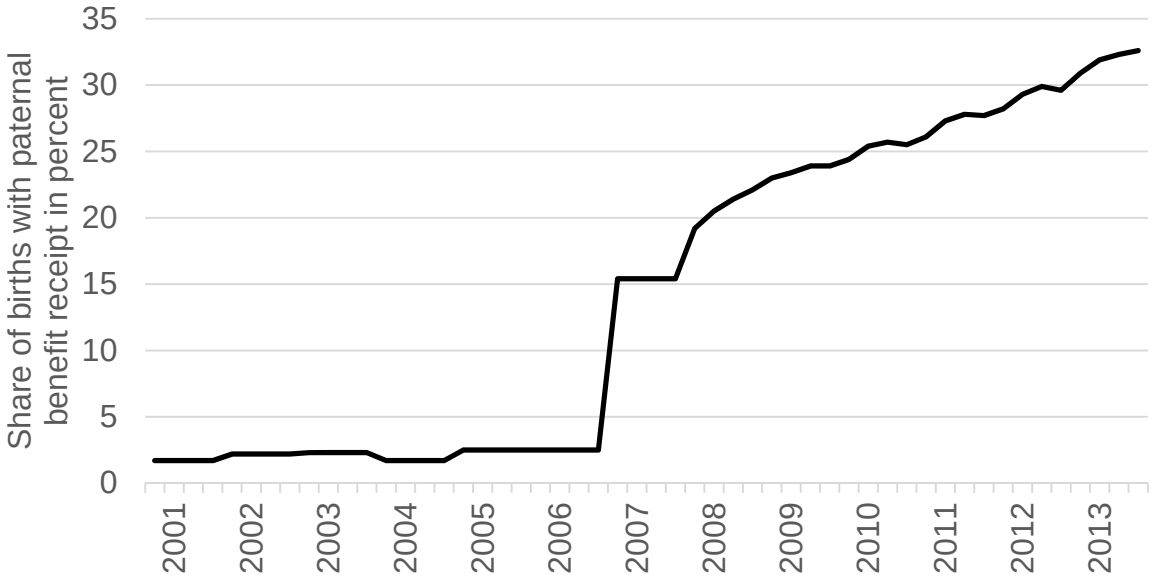
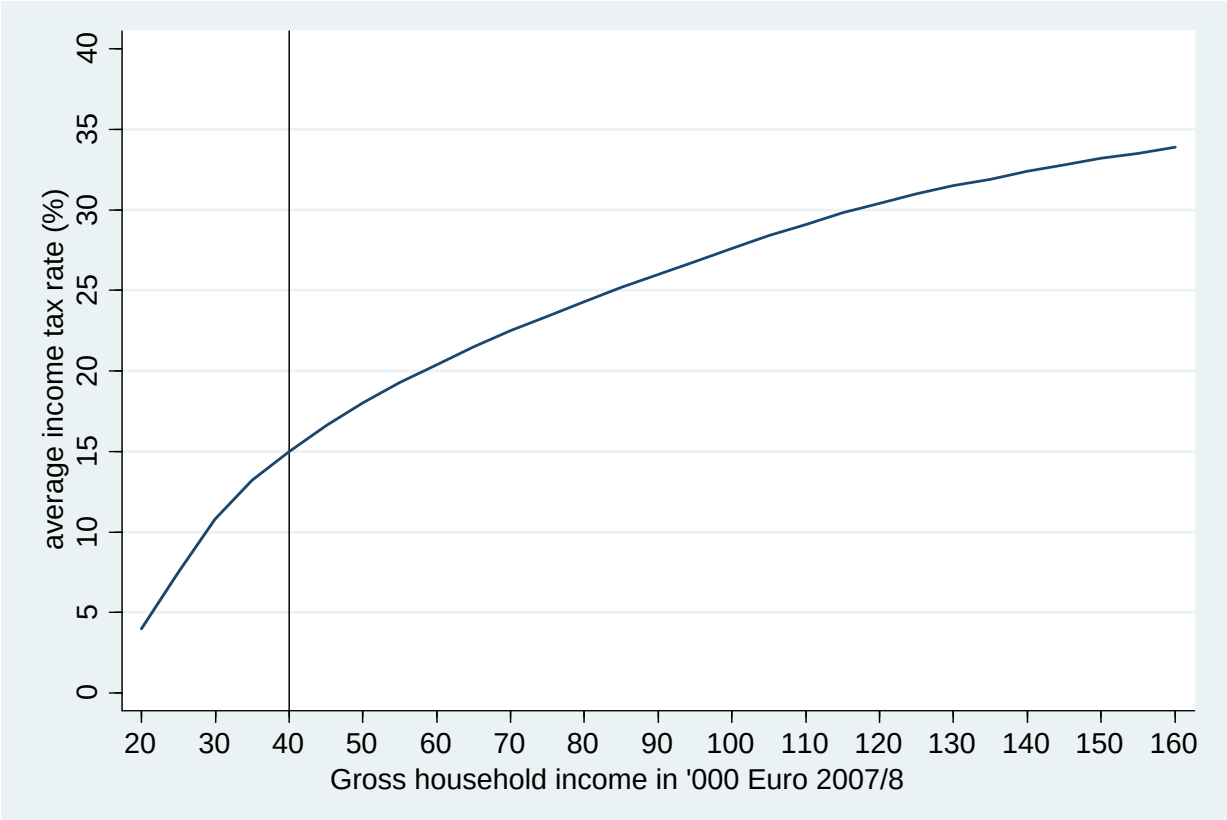
Figure 1. Share of births with paternal receipt of parental leave benefit by quarter of birth



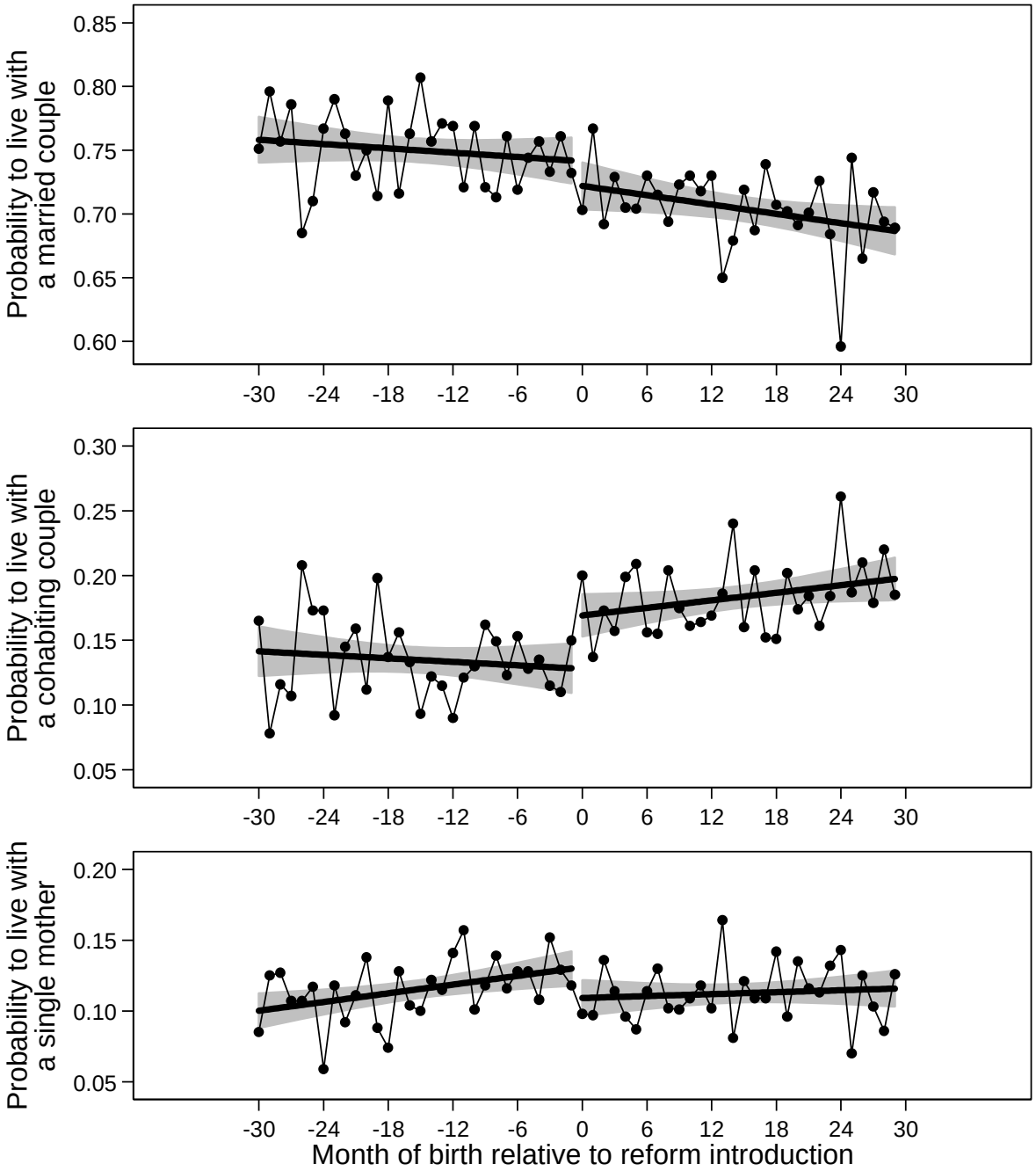
Source: The data for the pre-reform period 2001-2006 are obtained from federal Ministry of Federal Ministry for Family Affairs, Senior Citizens, Women and Youth (BMFSFJ) via personal communication; the pre-reform time series is not exactly comparable with the data for the post-reform period due to differences in data collection. For 2007 births, we only have information for the full year (see STBA 2008); STBA (2015b) provides quarterly information for births from Q1 2008 through Q4 2013.

Figure 2. Average income tax rate, by household income



Note: The bar at 40,000 Euro indicates the median gross household income in our sample of married couples. The median gross annual household income is approximated based on information on monthly net household incomes from the Micro Census.

Figure 3. Development of living arrangements over time



Note: The zero on the x-axis corresponds to January 2007. The dots show monthly means in outcomes. The solid lines represent linear trends and the shaded areas the 90 percent confidence intervals around the fits for the periods before and after the reform.
Source: Micro Census survey years 2005-2012, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany.

Table 1. Summary of hypothesized directions of mechanisms

Changes in	Hypothesized effect on outcome for	
	Reform winners	Reform losers
Income	Single motherhood ↑ (economic independence hypothesis) Single motherhood ↓ (financial situation improves) Single motherhood ↓ (higher investments in children) Single motherhood ↑ (divorce threat point increases) Single motherhood ↓ (more cooperative bargaining)	Single motherhood ↓ (economic independence hypothesis) Single motherhood ↑ (financial situation may worsen) Single motherhood ↑ (fewer investments in children) Single motherhood ↓ (divorce threat point decreases) Single motherhood ↑ (less cooperative bargaining)
Return-to-work	Single motherhood ↑↓ (mother-child attachment) Single motherhood ↑↓ through income changes	Single motherhood ↑↓ through income changes
Higher paternal leave taking	Single motherhood ↓ (father involvement hypothesis) Single motherhood ↓ (higher consumption complementarities)	
Stricter time limit on duration		Single motherhood ↓ (partnering up as insurance)
Tax-induced marriage disincentive		Marriage probability ↓ (particularly for low income couples)

Table 2. Sample construction: number of observations by survey year and birth cohort

	Micro Census survey year							
Birth cohort	2005	2006	2007	2008	2009	2010	2011	2012
2004/5	965	1,091	1,077	1,088	0	0	0	0
2005/6	0	956	1,006	1,006	1,010	0	0	0
2006/7 = treated	0	0	906	1,001	980	1,011	0	0
2007/8	0	0	0	1,004	1,085	1,034	1,009	0
2008/9	0	0	0	0	907	968	1,010	975

Notes: the colors refer to the year of a child's life (age) at the time of the survey

1st (age 0)	2nd (age 1)	3rd (age 2)	4th (age 3)

Source: Micro Census survey years 2005-2012, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany. Each entry counts the observations within the three months before and after the change of the year.

Table 3. Descriptive Statistics

	all		"Reform winners"		"Reform losers"		
	Mean	St.Dev.	Mean	St.Dev.	Mean	St.Dev.	Diff.
A. Living arrangement							
married couple	0.728	0.445	0.751	0.433	0.667	0.471	0.084***
cohabiting couple	0.158	0.364	0.157	0.364	0.156	0.362	0.001
single mother	0.114	0.318	0.093	0.290	0.177	0.382	-0.084***
B. Child's characteristics							
birth cohort 2008/09	0.190	0.392	0.194	0.395	0.174	0.379	0.020**
birth cohort 2007/08	0.211	0.408	0.214	0.410	0.195	0.396	0.019*
birth cohort 2006/07 (treated)	0.193	0.395	0.189	0.391	0.212	0.409	-0.023**
birth cohort 2005/06	0.198	0.399	0.200	0.400	0.198	0.398	0.002
birth cohort 2004/05	0.208	0.406	0.203	0.402	0.221	0.415	-0.018*
born in 1st quarter of year	0.486	0.500	0.491	0.500	0.470	0.499	0.021*
male	0.497	0.500	0.499	0.500	0.495	0.500	0.004
multiple birth	0.036	0.187	0.039	0.194	0.029	0.167	0.010**
age in months	13.720	6.850	13.298	6.809	14.970	6.752	-1.672***
C. Maternal characteristics							
age at childbirth	28.753	5.523	29.593	5.191	25.953	5.689	3.640***
school degree: no	0.030	0.170	0.010	0.097	0.095	0.293	-0.085***
school degree: Hauptschulabschluss	0.221	0.415	0.189	0.391	0.315	0.464	-0.126***
school degree: Realschulabschluss	0.354	0.478	0.384	0.487	0.263	0.440	0.121***
school degree: Fachhochschulreife	0.082	0.275	0.089	0.284	0.063	0.243	0.026***
school degree: Abitur	0.300	0.458	0.321	0.467	0.241	0.428	0.080***
school degree: other	0.004	0.064	0.003	0.054	0.008	0.089	-0.005***
school degree: missing	0.008	0.090	0.005	0.070	0.016	0.126	-0.011***
occupational degree: no	0.205	0.404	0.114	0.317	0.496	0.500	-0.382***
occupational degree: blue collar	0.511	0.500	0.577	0.494	0.303	0.460	0.274***
occupational degree: white collar	0.090	0.287	0.101	0.301	0.054	0.226	0.047***
occupational degree: tertiary degree	0.179	0.383	0.197	0.397	0.130	0.336	0.067***
occupational degree: other	0.010	0.100	0.009	0.095	0.010	0.101	-0.001
occupational degree: missing	0.004	0.066	0.003	0.054	0.007	0.084	-0.004***
pre-birth employment: non-working	0.226	0.418	-	-	-	-	-
pre-birth employment: working	0.739	0.439	-	-	-	-	-
pre-birth employment: missing	0.036	0.185	-	-	-	-	-
born in Germany	0.782	0.413	0.842	0.365	0.594	0.491	0.248***
Observations	9,889		7,306		2,231		

Source: Micro Census survey years 2005-2010, own calculations. Federal state indicators not shown to save space. Samples are restricted to first-born children who were born in Germany and reside in West Germany. We define reform losers (winners) as children of non-working (working) mothers in the last 12 months prior to giving birth. Diff. stands for the difference in the means for the reform "winners" and "losers". *, **, and *** indicate statistical significance of this difference at the 10, 5, and 1 percent level.

Table 4. Covariate Balancing.

	RD-DID sample									Q1/2007-Q4/2006			
	All			Reform losers			Reform winners			All			
	Coef.	St.Err	t-stat	Coef.	St.Err.	t-stat	Coef.	St.Err.	t-stat	Coef.	St.Err.	t-stat	Std. diff.
A. Child's characteristics													
Male	0.008	0.026	0.308	0.007	0.052	0.135	0.016	0.030	0.533	-0.019	0.023	-0.811	0.037
Multiple birth	0.034	0.009	3.778	0.028	0.014	2.000	0.035	0.010	3.182	-0.030	0.008	-3.856	0.175
Age in months	0.249	0.344	0.724	0.931	0.685	1.359	0.100	0.400	0.250	2.241	0.310	7.238	-0.332
B. Maternal characteristics													
Age at childbirth	0.170	0.281	0.605	0.206	0.588	0.350	0.232	0.310	0.748	-0.281	0.252	-1.116	0.051
School degree													
None	-0.001	0.009	-0.111	-0.013	0.030	-0.433	0.006	0.010	1.000	0.005	0.008	0.607	-0.028
Hauptschulabschluss	-0.004	0.022	-0.182	0.028	0.048	0.583	-0.011	0.020	-0.458	-0.001	0.020	-0.036	0.002
Realschulabschluss	-0.019	0.024	-0.792	0.015	0.044	0.341	-0.040	0.030	-1.379	0.029	0.022	1.348	-0.062
Fachhochschulreife	0.006	0.014	0.429	-0.020	0.027	-0.741	0.017	0.020	1.000	0.000	0.012	-0.029	0.001
Abitur	0.023	0.023	1.000	-0.010	0.045	-0.222	0.034	0.030	1.214	-0.037	0.021	-1.755	0.080
Other	-0.006	0.004	-1.500	-0.003	0.010	-0.300	-0.008	0.000	-2.000	0.005	0.004	1.220	-0.056
Missing	0.001	0.004	0.250	0.003	0.013	0.231	0.002	0.000	0.500	-0.001	0.004	-0.375	0.017
Occupational degree													
None	-0.019	0.021	-0.905	0.006	0.052	0.115	-0.022	0.020	-1.158	0.027	0.019	1.439	-0.066
Blue collar	-0.035	0.026	-1.346	-0.025	0.048	-0.521	-0.045	0.030	-1.500	0.016	0.023	0.699	-0.032
White collar	0.040	0.014	2.857	0.039	0.023	1.696	0.040	0.020	2.353	-0.039	0.012	-3.175	0.145
Tertiary degree	0.013	0.020	0.650	-0.016	0.036	-0.444	0.028	0.020	1.167	-0.002	0.018	-0.118	0.005
Other	-0.001	0.005	-0.200	-0.008	0.009	-0.889	0.000	0.010	0.000	0.001	0.005	0.195	-0.009
Missing	0.002	0.003	0.667	0.005	0.009	0.556	-0.001	0.000	-0.333	-0.003	0.003	-1.039	0.047
Pre-birth employment													
Non-working	-0.003	0.022	-0.136	-	-	-	-	-	-	0.018	0.020	0.932	-0.043
Working	0.012	0.023	0.522	-	-	-	-	-	-	-0.023	0.021	-1.126	0.052
Missing	-0.008	0.009	-0.889	-	-	-	-	-	-	0.005	0.008	0.600	-0.028
Born in Germany	0.028	0.021	1.333	-0.016	0.051	-0.314	0.048	0.020	2.182	-0.031	0.019	-1.626	0.075
Observations		9,889			7,306			2,231			1,907		

Source: The RD-DID sample applies the same sample restrictions as Table 3, and each cell reports the coefficient estimates for *cohort0607*Q1* as in equation 1 using each covariate as a separate outcome variable. The final three columns present the mean differences between children born in the last quarter of 2006 and the first quarter of 2007. The standardized difference of each variable reported in the final column is defined as the mean difference divided by the square root of the average variance of both groups (see Rubin 2001). **, and *** indicate statistical significance at the 10, 5, and 1 percent level.

Table 5. Estimation results: effects on living arrangements (at ages 0-1)

	(1) married couple	(2) cohabiting couple	(3) single mother
Panel A: all children (N=9,889)			
cohort 0607*Q1	-0.018	0.038 **	-0.021
	(0.021)	(0.018)	(0.016)
Mean dependent variable	*0.728*	*0.158*	*0.114*
Panel B: "reform losers" (N=2,231)			
cohort 0607*Q1	-0.038	0.032	0.006
	(0.043)	(0.034)	(0.038)
Mean dependent variable	*0.667*	*0.156*	*0.177*
Panel C: "reform winners" (N=7,306)			
cohort 0607*Q1	-0.012	0.043 **	-0.031 *
	(0.025)	(0.021)	(0.017)
Mean dependent variable	*0.750*	*0.157*	*0.093*
Child characteristics	yes	yes	yes
Maternal characteristics at childbirth	yes	yes	yes

Notes: Each cell represents a separate linear regression. All regressions include a constant. Child characteristics comprise indicators for a child's birth cohort, quarter of birth, gender, multiple birth, and state of residence, as well as age in months (linear and squared). Maternal characteristics at childbirth include a mother's age in years (linear and squared), indicators for education, pre-birth employment status, and migration status. We define reform losers (winners) as children of non-working (working) mothers in the last 12 months prior to giving birth. Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level.

Source: Micro Census survey years 2005-2010, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany.

Table 6. Estimation results: effects on living arrangements (at ages 0-1), by gender

	(1) married couple	(2) cohabiting couple	(3) single mother
Panel A: all children (N=9,889)			
cohort 0607*Q1	0.002	0.046 *	-0.048 **
	(0.030)	(0.025)	(0.023)
cohort 0607*Q1*boy	-0.039	-0.016	0.054 *
	(0.042)	(0.036)	(0.032)
Panel B: ”reform losers” (N=2,231)			
cohort 0607*Q1	-0.039	0.033	0.006
	(0.060)	(0.045)	(0.054)
cohort 0607*Q1*boy	0.002	-0.002	0.001
	(0.086)	(0.069)	(0.076)
Panel C: ”reform winners” (N=7,306)			
cohort 0607*Q1	0.017	0.048	-0.065 **
	(0.036)	(0.030)	(0.025)
cohort 0607*Q1*boy	-0.058	-0.010	0.068 **
	(0.050)	(0.043)	(0.034)
Child characteristics	yes	yes	yes
Maternal characteristics at childbirth	yes	yes	yes

Notes: Each column within a panel shows coefficients and standard errors from a separate linear regression. The coefficient on cohort 0607*Q1 represents the reform effect for girls, and the interaction term represents the difference in the reform effect between boys and girls. All regressions include a constant. Child's characteristics comprise indicators for a childbirth cohort, quarter of birth, gender, multiple birth, and state of residence, as well as age in months (linear and squared). Maternal characteristics at childbirth include a mother’s age in years (linear and squared), indicators for education, pre-birth employment status, and migration status. We define reform losers (winners) as children of non-working (working) mothers in the last 12 months prior to giving birth. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level.

Source: Micro Census survey years 2005-2010, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany.

Table 7. Estimation results: effect of child gender on living arrangements (at ages 0-1)

	married couple		cohabiting couple	single mother	
Panel A: All children					
Before reform (N = 12,366)					
boy	0.018	**	-0.004	-0.014	***
	(0.007)		(0.006)	(0.006)	
After reform (N=20,966)					
boy	0.008		-0.006	-0.001	
	(0.006)		(0.005)	(0.004)	
Panel B: Reform "losers"					
Before reform (N = 2,881)					
boy	-0.018		0.017	0.001	
	(0.015)		(0.012)	(0.013)	
After reform (N=4,231)					
boy	0.003		0.003	-0.006	
	(0.013)		(0.011)	(0.011)	
Panel C: Reform "winners"					
Before reform (N = 9,078)					
boy	0.027	***	-0.008	-0.018	***
	(0.009)		(0.007)	(0.006)	
After reform (N=15,847)					
boy	0.008		-0.008	0.001	
	(0.007)		(0.006)	(0.005)	
Child characteristics	yes		yes	yes	
Maternal characteristics at childbirth	yes		yes	yes	

Notes: Each cell represents a separate linear regression.. Before reform refers to all births occurring between 2005 and 2006; after reform refers to all births occurring between 2007 and 2012. All regressions include a constant and controls for child and maternal characteristics. Child characteristics comprise indicators for a child's birth cohort, quarter of birth, multiple birth, and state of residence, as well as age in months (linear and squared). Maternal characteristics at childbirth include a mother's age in years (linear and squared), indicators for education, pre-birth employment status, and migration status. A mother's working status refers to her status in the last 12 pre-birth months. Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level.
Source: Micro Census survey years 2005-2012, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany.

Table 8. Estimation results: effect heterogeneity by pre-reform income level on the propensity to have married (vs. cohabiting) parents

	married couple
Panel A: all children (N=8,001)	
cohort 0607*Q1	-0.050
	(0.033)
cohort 0607*Q1*(above median household income)	0.017
	(0.042)
Panel B: ”reform losers” (N=1,663)	
cohort 0607*Q1	-0.059
	(0.053)
cohort 0607*Q1*(above median household income)	0.029
	(0.086)
Panel C: ”reform winners” (N=6,092)	
cohort 0607*Q1	-0.040
	(0.043)
cohort 0607*Q1*(above median household income)	0.001
	(0.052)
Child's characteristics	yes
Maternal characteristics at childbirth	yes

Notes: Each cell represents a separate linear regression. Only child observations with both parents in the household are considered. All regressions include a constant and controls for the interaction of "after" with the two comparative education indicators. Child characteristics comprise indicators for a child’s birth cohort, quarter of birth, gender, multiple birth, and state of residence, as well as age in months (linear and squared). Maternal characteristics at childbirth include a mother’s age in years (linear and squared), indicators for education, pre-birth employment status, and migration status. We define reform losers (winners) as children of non-working (working) mothers in the last 12 months prior to giving birth. Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level.

Source: Micro Census survey years 2005-2010, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany and reside with both parents.

Table 9. Robustness checks

	married couple	cohabiting couple		single mother	
Panel A: all children					
A1: baseline (N=9,889)	-0.018	0.038	**	-0.021	
	(0.021)	(0.018)		(0.016)	
A2: no controls (N=9,889)	-0.013	0.036	**	-0.023	
	(0.023)	(0.018)		(0.017)	
A3: excl. birth cohorts 04/05, 08/09 (N=5,958)	-0.002	0.034	*	-0.032	*
	(0.023)	(0.019)		(0.017)	
A4: only cohort 05/06 and 06/07 (N=3,869)	-0.007	0.047	**	-0.040	**
	(0.026)	(0.021)		(0.020)	
A5: incl. birth month dummies (N=9,889)	-0.017	0.038	**	-0.021	
	(0.021)	(0.018)		(0.016)	
A6: first difference (N=1,907)	-0.023	0.040	**	-0.016	
	(0.020)	(0.016)		(0.015)	
A7: incl. higher-order births (N=20,309)	-0.016	0.022	**	-0.006	
	(0.013)	(0.010)		(0.010)	
A8: excl. January & December (N=6,358)	-0.003	0.030		-0.027	
	(0.027)	(0.022)		(0.020)	
A9: six-month bandwidth (N=19,631)	-0.012	0.032	**	-0.021	*
	(0.015)	(0.013)		(0.011)	
A10: Multinomial logit (N=9,889)	-0.021	0.040	**	-0.019	
	(0.021)	(0.017)		(0.016)	
A11: RDD 30 months (N=19,631)	-0.020	0.040	***	-0.020	**
	(0.012)	(0.010)		(0.009)	
A12: RDD 24 months (N=15,785)	-0.002	0.041	***	-0.039	**
	(0.015)	(0.013)		(0.0011)	
A13: RDD 18 months (N=11,817)	0.001	0.036	***	-0.037	***
	(0.016)	(0.014)		(0.012)	
A14: placebo reform 2007/8 (N=7,982)	-0.031	0.021		0.010	
	(0.022)	(0.019)		(0.015)	
A15: placebo reform 2005/6 (N=7,982)	-0.013	-0.012		0.025	
	(0.021)	(0.017)		(0.016)	
Panel B: ”reform losers”					
B1: baseline (N=2,231)	-0.038	0.032		0.006	
	(0.043)	(0.034)		(0.038)	
B2: no controls (N=2,231)	-0.031	0.024		0.007	
	(0.049)	(0.036)		(0.040)	
B3: excl. cohort 04/05, 08/09 (N=1,349)	-0.031	0.041		-0.010	
	(0.048)	(0.038)		(0.043)	
B4: only cohorts 05/06 and 06/07 (N=914)	-0.064	0.083	**	-0.019	
	(0.054)	(0.042)		(0.049)	
B5: incl. birth month dummies (N=2,231)	-0.036	0.028		0.008	
	(0.043)	(0.034)		(0.038)	
B6: first difference (N=473)	-0.053	0.049		0.004	
	(0.040)	(0.032)		(0.036)	
B7: incl. higher-order births (N=6,959)	-0.016	0.007		0.009	
	(0.021)	(0.016)		(0.018)	
B8: excl. January & December (N=1,434)	0.063	-0.043		-0.020	
	(0.053)	(0.041)		(0.049)	

	married couple		cohabiting couple		single mother	
B9: six-month bandwidth (N=4,432)	0.000		0.015		-0.015	
	(0.031)		(0.026)		(0.027)	
B10: Multinomial logit (N=2,231)	-0.042		0.038		0.004	
	(0.042)		(0.033)		(0.039)	
B11: RDD 30 months (N=4,432)	0.003		0.019		-0.022	
	(0.025)		(0.021)		(0.022)	
B12: RDD 24 months (N=3,578)	0.017		0.026		-0.043	
	(0.031)		(0.025)		(0.027)	
B13: RDD 18 months (N=2,672)	0.014		0.021		-0.035	
	(0.034)		(0.028)		(0.029)	
B14: placebo reform 2007/8 (N=1,758)	-0.063		0.053		0.009	
	(0.049)		(0.041)		(0.042)	
B15: placebo reform 2005/6 (N=1,758)	0.043		-0.073	**	0.030	
	(0.045)		(0.035)		(0.040)	
Panel C: ”reform winners”						
C1: baseline (N=7,306)	-0.012		0.043	**	-0.031	*
	(0.025)		(0.021)		(0.017)	
C2: no controls (N=7,306)	-0.005		0.041	*	-0.036	**
	(0.026)		(0.022)		(0.018)	
C3: excl. cohort 04/05, 08/09 (N=4,404)	0.006		0.035		-0.041	**
	(0.027)		(0.023)		(0.019)	
C4: only cohorts 05/06 and 06/07 (N=2,839)	0.005		0.043	*	-0.048	**
	(0.031)		(0.025)		(0.022)	
C5: incl. birth month dummies (N=7,306)	-0.013		0.043	**	-0.031	*
	(0.025)		(0.021)		(0.017)	
C6: first difference (N=1,378)	-0.021		0.041	**	-0.020	
	(0.023)		(0.020)		(0.016)	
C7: incl. higher-order births (N=12,510)	-0.011		0.032	**	-0.021	*
	(0.017)		(0.014)		(0.012)	
C8: excl. January & December (N=4,696)	-0.023		0.054	**	-0.031	
	(0.031)		(0.026)		(0.022)	
C9: six-month bandwidth (N=14,478)	-0.014		0.037	**	-0.023	*
	(0.018)		(0.015)		(0.012)	
C10: Multinomial logit (N=7,306)	-0.012		0.042	**	-0.031	*
	(0.025)		(0.020)		(0.018)	
C11: RDD 30 months (N=14,478)	-0.025	*	0.045	***	-0.021	**
	(0.014)		(0.012)		(0.010)	
C12: RDD 24 months (N=11,642)	-0.006		0.045	***	-0.038	***
	(0.017)		(0.015)		(0.012)	
C13: RDD 18 months (N=8,724)	-0.003		0.040	**	-0.037	***
	(0.019)		(0.016)		(0.013)	
C14: placebo reform 2007/8 (N=5,928)	-0.029		0.018		0.011	
	(0.025)		(0.022)		(0.016)	
C15: placebo reform 2005/6 (N=5,928)	-0.025		-0.003		0.022	
	(0.024)		(0.020)		(0.017)	

Notes: Each cell represents a separate linear regression. All regressions include a constant and control for child and mother’s characteristics. Child characteristics comprise indicators for a

child's birth cohort, quarter of birth, gender, multiple birth, and state of residence, as well as age in months (linear and squared). Maternal characteristics at childbirth include mother's age in years (linear and squared), indicators for education, pre-birth employment status, and migration status. We define reform losers (winners) as children of non-working (working) mothers in the last 12 months prior to giving birth. Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level. Source: Micro Census survey years 2005-2010, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany.

ONLINE APPENDIX

Table A.1 Average tax rates and progressivity effect

Houshold income p.a. (in 1,000 Euro)	Average tax rate (in percent)	Income tax payable p.a. (in Euro)	Change in average tax rate when income plus 5,000 Euro p.a. (in percentage points)
20	4	800	3.5
25	7.5	1,875	3.3
30	10.8	3,240	2.4
35	13.2	4,620	1.8
40	15.0	6,000	1.6
45	16.6	7,470	1.4
50	18.0	9,000	1.3
55	19.3	10,615	1.1
60	20.4	12,240	1.1
65	21.5	13,975	1.0
70	22.5	15,750	0.9
75	23.4	17,550	0.9
80	24.3	19,440	0.9
85	25.2	21,420	0.8
90	26.0	23,400	0.8
95	26.8	25,460	0.8
100	27.6	27,600	0.8
105	28.4	29,820	0.7
110	29.1	32,010	0.7
115	29.8	34,270	0.6
120	30.4	36,480	0.6

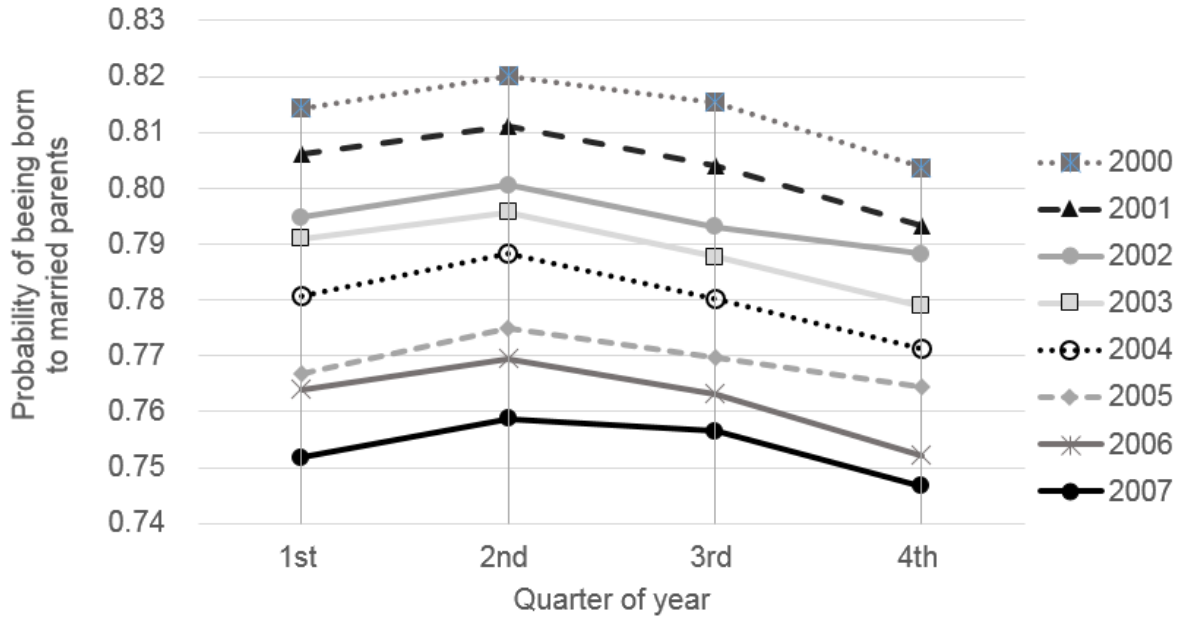
Note: Own calculations based on tax schedule for the fiscal year 2007. Column 4 presents the shift in average tax rates when a hypothetical parental leave benefit of 5,000 Euro is added to the household income in column 1.

Table A.2 Estimation results: effects on living arrangements (at ages 2-3)

	(1) married couple	(2) cohabiting couple	(3) single mother	
Panel A: all children (N=10,200)				
cohort 0607*Q1	0.025	0.019	-0.044	***
	(0.021)	(0.015)	(0.017)	
Panel B: ”reform losers” (N=3,413)				
cohort 0607*Q1	-0.006	0.027	-0.021	
	(0.037)	(0.028)	(0.032)	
Panel C: ”reform winners” (N=5,904)				
cohort 0607*Q1	0.027	0.017	-0.044	**
	(0.026)	(0.020)	(0.020)	
Child's characteristics	yes	yes	yes	
Maternal characteristics at childbirth	yes	yes	yes	

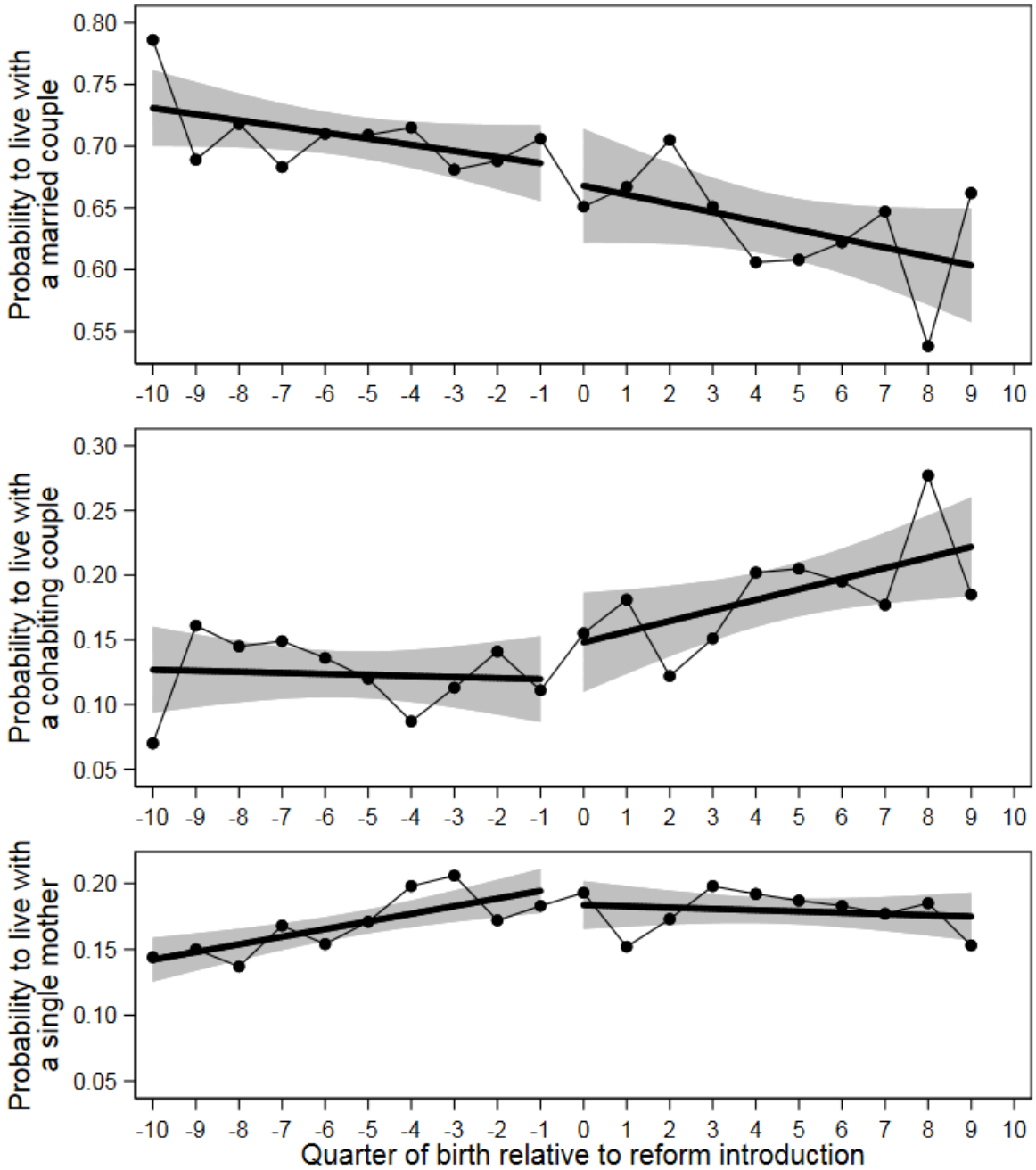
Notes: Each cell represents a separate linear regression. All regressions include a constant. Child characteristics comprise indicators for a child’s birth cohort, quarter of birth, gender, multiple birth, and state of residence, as well as age in months (linear and squared). Maternal characteristics at childbirth include mother’s age in years (linear and squared), indicators for education, pre-birth employment status, and migration status. We define reform losers (winners) as children of non-working (working) mothers in the last 12 months prior to giving birth. Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level.
Source: Micro Census survey years 2007-2012, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany.

Figure A.1. Probability of being born to married parents by quarter and year



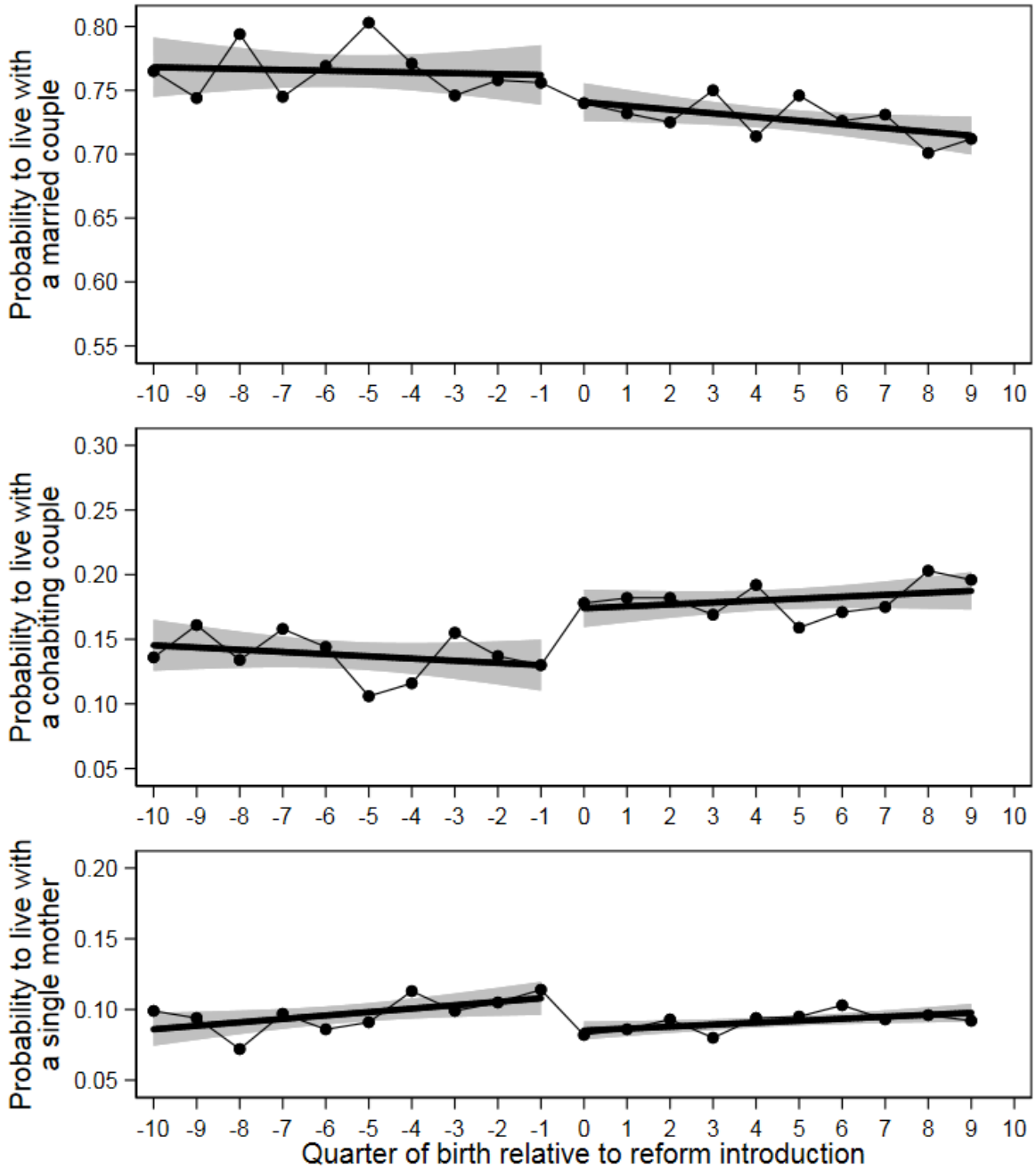
Notes: All children born in West Germany (excluding Berlin). Each dot relates the number of children whose parents provide a marriage certificate while registering a childbirth to all births in a given quarter and year.
Source: Own calculations from the absolute numbers of life births by federal state, year, and month obtained from Statistisches Bundesamt via personal communication.

Figure A.2. Development of living arrangements over time for reform losers



Note: The zero on the x-axis corresponds to the first quarter of 2007. The dots show quarterly means in outcomes. The solid lines represent linear trends and the shaded areas the 90 percent confidence intervals around the fits for the periods before and after the reform.
Source: Micro Census survey years 2005-2012, own calculations. Samples restricted to first-born children of reform losers who were born in Germany and reside in West Germany.

Figure A.3. Development of living arrangements over time for reform winners



Note: The zero on the x-axis corresponds to the first quarter of 2007. The dots show quarterly means in outcomes. The solid lines represent linear trends and the shaded areas the 90 percent confidence intervals around the fits for the periods before and after the reform.
Source: Micro Census survey years 2005-2012, own calculations. Samples restricted to first-born children of reform winners who were born in Germany and reside in West Germany.