Love your leave, don't leave your love! Paid parental leave and children's living arrangements

Kamila Cygan-Rehm
University of Erlangen-NÃ¼rnberg

Daniel KÃ¼hnle
University of Erlangen-NÃ¼rnberg

Regina T. Riphahn
Univ. Erlangen-Nuremberg

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Correspondence to:

Regina T. Riphahn, Lange Gasse 20, 90403 Nuremberg, Germany, Email: regina.riphahn@wiso.uni-erlangen.de.

Abstract

This paper examines the causal effect of a substantial change in the German paid parental leave program on early childhood living arrangements. Our identification strategy draws on the unanticipated introduction of this reform in 2007. Using data from the Micro Census 2005-2012, we find that the reform increased the probability that a newborn child lives with non-married cohabiting parents in the first years of life. This effect results from a reduced risk of growing up with a single mother rather than from a shift away from marital unions to non-married parental cohabitation. We argue that our estimates are in line with the hypothesis that the reform incentivizes paternal involvement in childcare and thus strengthens fathersâ€™ attachment to their newborns. We demonstrate that the negative effect on single parenthood persists beyond the benefit take-up period. Interestingly, boys and girls are differently affected by the reform: particularly fathers of daughters appear to join their families after the reform, reducing a prior disadvantage of daughters.

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Kamila Cygan-Rehm, Daniel Kühnle, Regina T. Riphahn*
(Friedrich-Alexander University Erlangen-Nürnberg)

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

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**Correspondence to:*
Regina T. Riphahn
University Erlangen-Nürnberg
Economics Department
Lange Gasse 20
90403 Nuremberg
Germany
Email: regina.riphahn@fau.de

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1. INTRODUCTION

An extensive literature documents significant relationships between children's living arrangements and general 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). While analyses with more rigorous research designs (e.g., Painter and Levine 2000, McLanahan, Tach, and Schneider 2013) usually yield smaller adverse effects of fragile family structure and fathers' absence than earlier cross-sectional studies, there is a consensus in the literature that living arrangements indeed matter for children's outcomes (Blau and van der Klaauw 2013). Therefore, early childhood living arrangements may be a central source of inequality and have potentially long lasting consequences for children. However, we still know little about public policy effects on children's 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 Earned Income Tax Credit (EITC) and Temporary Assistance for Needy Families (TANF) (e.g., Dickert-Conlin and Houser 2002, Bitler et al. 2004 and 2006, Fitzgerald and Ribar 2004). Most studies examine the effects of financial incentives on women's partnering behavior and focus on samples of single mothers (e.g., Cancian and Meyer 2014).

Although a few recent studies examine the effects of these programs on children's living arrangements directly, the findings are still inconclusive. For instance, Acs and Nelson (2004) find that while more effective child support collections and family cap policies of the 1990s increased the probability that children live with both parents, other examined policies did not create any clear effects. Bitler, Gelbach and Hoynes (2006) show that the US welfare reform of 1996 reduced the probability of living with an unmarried parent and increased the probability of living with a married 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.

Generally, the evidence on the link between public policies and children's living arrangements is scarce. Our paper aims to fill this gap by examining the effect of an unanticipated German paid parental leave reform in 2007. We contribute to the literature in several ways. First, we differ from most of the literature by focusing on the child as our unit of observation, and by studying the direct effects on early childhood living arrangements. Second, 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. Third, we consider various mechanisms through which the reform might affect living arrangements by testing hypotheses that refer to the insurance motive of marriage, tax incentives, and paternal involvement in early child rearing. Furthermore, 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, in contrast to most contributions[1], we provide evidence for a country outside the US and thus for a different institutional and cultural setting.

Several studies have evaluated the German paid parental leave reform. Three papers are relevant for our question of interest: Geisler and Kreyenfeld (2012) find a significant increase in fathers' propensity to take parental leave after the reform. Using a non-representative survey, Kluve and Tamm (2013) consider fathers' share in total childcare in the first year after a birth and find no significant reform effects. Finally, Kluve and Schmitz (2014) study numerous outcomes, including the probability of being married for different subsamples of mothers. The authors find significant reductions in marriage rates through year five after a birth and argue that this is due to the reduced tax incentives for married couples after the reform. So far,

[1] A notable 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.

however, the reform effect on child living arrangements remains unaddressed. As an important refinement compared to existing studies, our analysis pays particular attention to potential heterogeneities between winners and losers of the reform described in section three.

To identify the causal effect of changes in parental leave benefits on children's living arrangements, we use a combination of a difference-in-differences approach with a regression-discontinuity design. We draw on a rich household dataset from the German Micro Census. We find that the reform increased the probability that a newborn child lives with cohabiting parents, and that the positive effect persists beyond the benefit take-up period. We find that this effect is mainly driven by a reduced risk of growing up in a single mother household, and does not result from a shift away from marital unions. Our estimates are in line with the hypothesis that the reform incentivizes paternal involvement, which strengthens fathers' attachment to their newborns. Finally, boys and girls are affected differently: particularly fathers of daughters join their families after the reform. This reduces a prior disadvantage of daughters who were significantly more likely to live with single mothers than sons.

The paper proceeds as follows. We start out with a brief review of the relevant literature and provide information about the parental leave reform. Based on this, we derive hypotheses about how the changed benefits might impact on children's living arrangements. We then describe the data and our empirical methodology. After discussing our key estimation results, we show that they are robust to a large set of sensitivity checks. We conclude that the German paid parental leave reform generated unintended, yet important, effects for children's living arrangements.

2. BACKGROUND

Many cross-sectional studies on living arrangements document a positive correlation between family stability and child outcomes (see McLanahan and Sandefur 1994). For instance, Dawson (1991) uses the 1988 National Health Interview Survey on Child Health and finds that children

raised by single mothers or without their biological father had worse schooling outcomes, more emotional or behavioral problems, and health vulnerability compared to those living with both biological parents. However, cross-sectional studies are likely to overestimate the effect of family structure on child outcomes due to positive selection into marriage.

Some studies adopt research designs to address the endogeneity of family status; e.g., Cherlin et al. (1991) and Painter and Levine (2000) use longitudinal data exploiting changes in family structure over time. Ermisch and Francesconi (2001) use sibling data to identify the effects of family structure from within family differences in exposure of children to particular family structures. Similarly, Ginther and Pollak (2004) show that educational outcomes are highest for children living with both biological parents compared to other settings. The correlation between family structure and education outcomes, however, becomes insignificant once family background is controlled for.

Some studies use "natural experiments" such as a parent's death to identify the causal effect of changes in family structure. For example, Lang and Zagorsky (2001) find that paternal death has a large impact on sons' marriage probabilities, but no economic effects in adulthood. Finlay and Neumark (2010) use an instrumental variables approach and find that unobserved factors account for the negative correlation between single motherhood and children's education.

Overall, the literature finds a positive cross-sectional relationship between family stability and child outcomes, but once the endogeneity of family status is accounted for, the effect size declines and sometimes becomes insignificant.

A fairly large literature examines the effects of welfare reform on family structure (see, e.g., Blank 2002 or Grogger and Karoly 2005, Ratcliffe et al. 2002). Most studies examine the effect of welfare reforms on the marital and cohabitation status of women using US data. The

results are mixed.[2] Most recently, Blau and van der Klaauw (2013) examine the determinants of family structure using data from the NLSY79. They find that large and significant wage effects, some tax effects, and small effects of welfare reforms on living arrangements.

Only very few studies focus on children directly. Acs and Nelson (2004) examine the effect of specific elements of TANF on children's and women's living arrangements. While their findings are mixed, some evidence shows 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, Gelbach, and Hoynes (2006) study whether the US welfare reform of 1996 affected children's living arrangements. They use state and year fixed effects for cross-sectional data on children whose parents have high school education or less. They find that after the reform, children are on average less likely to live with an unmarried parent, more likely to live with married parents, and more likely to live with neither parent. Overall, the literature has found mixed effects of welfare reforms on living arrangements; it focused almost exclusively on the US, and has not examined the effects of other public policy programs.

Several studies show that a child's gender affects marital stability and that married parents with sons exhibit greater marital satisfaction than families with only daughters.[3] Dahl and Moretti (2008) confirm for the US that sons are more likely to live with their fathers than daughters.[4] However, this finding disagrees with Diekmann and Schmidheiny (2004) who

[2] See, e.g., Bitler, Gelbach, Hoynes, and Zavodny (2004), Ellwood (2000), Dickert-Conlin and Houser (2002), Schoeni and Blank (2000), or Fitzgerald and Ribar (2004).

[3] See, e.g., Katzev, Warner, and Acock (1994), Morgan, Lye, and Condran (1988), Mott (1994), Spanier and Glick (1981), Cox et al. (1999), or Mizell and Steelman (2000).

[4] Given the strong relationship between child gender and divorce for the US, Bedard and Deschenes (2005) use the gender of the child as an instrument to identify the effect of divorce on women's economic status.

analyze data from 18 different countries and do not find that having a girl increases the risk of divorce.[5]

The key question is why families with sons are less likely to divorce than families with daughters. One possible interpretation is that fathers prefer to have sons, either intrinsically or because of social norms, and are more actively involved in the rearing of their sons compared to daughters. Indeed, the *father involvement hypothesis* (Morgan et al. 1988) posits that social norms induce fathers to spend more time with their sons than with their daughters, which in turn reduces the risk of divorce due to greater family bonding. Several studies confirm that fathers spend more time with their sons (Yeung et al. 2001) and more time with their children if they have any sons (Barnett and Baruch 1987; Harris and Morgan 1991). Furthermore, the social norm that dictates a closer bond between fathers and sons is consistent with the finding from Morgan et al. (1988) that fathers have a greater emotional attachment to their sons than daughters. Alternatively, from an economics perspective, if the comparative advantage of fathers lies in "producing" sons rather than daughters, then having a son increases the relative value of family life for fathers (Lundberg and Rose 2003). Thus, having a son will increase the marital surplus and reduce the risk of divorce.

3. INSTITUTIONS AND HYPOTHESES

The German family policy includes three relevant programs aiming at the wellbeing of parents and newborns: first, maternity leave (*Mutterschutz*) and maternity benefits (*Mutterschaftsgeld*) are available from six weeks before to eight weeks after a birth. In that period, mothers are not allowed to work and they cannot be laid off. Those employed before birth continue to receive their full net earnings, while those not employed prior to birth receive no benefits.

[5] Choi et al. (2008) investigate whether duration of stay in the fathers' household differs for West German sons and daughters; they find that the propensity to stay with the father is significantly higher for sons than for daughters.

Second, parents can take parental leave for up to three years after birth (*Elternzeit*). In that period employers are obliged to guarantee a parent's job, i.e., a job that is comparable to the job held prior to the birth. Couples are free to choose which partner uses the parental leave.

As a third institution, parental leave benefits are paid. 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.[6] The benefits were exempted from income taxation. The eligibility criteria of the means test related to the expected family income in years one and two after a birth. Parents were eligible for full child-rearing benefits if their annual net income was below a threshold.[7]

Parents of children born after the reform date, i.e., on or after January 1, 2007, are now entitled to "parents' money" (*Elterngeld*). This new 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 the birth for the parent who does not work after birth. Even parents employed part-time after a birth 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 not previously 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

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

[7] 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. Benefit eligibility in year one (two) after the birth was based on income in the calendar year prior to (after) birth. Maternal income was not considered, if the mother did not work after birth.

applicable tax rate in the progressive tax system and thus causes an increase in tax rates for taxable income (progressivity effect, *Progressionsvorbehalt*).

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; a single parent is eligible for 14 months.[8]

Administrative statistics (see, e.g., STBA 2012) indicate that the share of fathers utilizing paid parental leave jumped from about 3.5 percent before the reform to 15 percent in 2007, the first post-reform year. Figure 1 shows that the share increased continuously over time and reached 32.3 percent for births of 2013 (STBA 2015). 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.[9]

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 generated "losers" among parents who could have claimed 24 months of transfers before the reform and it generated "winners" among higher income parents who newly

[8] It is possible to double the eligibility duration of the new *Elterngeld* benefit if the monthly benefit is halved. Only about ten percent of *Elterngeld* recipients use this option (STBA 2013).

[9] 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 and 2015b). Jointly these two developments might suggest the development of a new social norm where fathers take parental leave for two months.

receive a generous transfer.[10] Clearly, behavioral responses to the reform may differ for the two groups.

Based on the literature and in view of this institutional framework, we posit four hypotheses regarding the effect of the paid parental leave reform on child living arrangements. First, if the decision to enter marriage relates to an insurance function, e.g., to safeguard the economic interests of mother and child after birth, we expect that the propensity to marry should be particularly important among those couples for whom the benefit declined after the reform. We hypothesize that the propensity of forming marital unions increases for the losers of the reform (H1). Such an increase should occur soon after the reform and should affect child living arrangements beyond the transfer period.

Second, if newly receiving paid parental leave enhances paternal involvement in childcare, it may incentivize fathers to live with their family. This suggests a decline in the risk of single motherhood. However, if earnings are too low, the family may not be able to afford the income reduction implied by a father taking up leave compared to earning a full income. The decline in single motherhood should be larger if the potential benefits of paid parental leave are substantial and fathers earn high incomes. Therefore, we hypothesize a decline in single parenthood particularly among families where the father is not the only earner, i.e., for mothers with own labor income who represent the winners of the reform (H2). The effect of paternal involvement should be long lasting.

Third, if the parental leave reform changes child living arrangements through increased paternal involvement in child rearing and if fathers' tend to favor sons over daughters, we expect differential effects depending on the gender of the child (H3).

[10] Before the reform, the child rearing benefit was paid in addition to benefits from the minimum income (social assistance) system. Until 2011, only parental leave transfers exceeding 300 Euro were newly considered in the minimum income support means test.

Our fourth hypothesis on the reform effect relates to the German income tax system, which uses a progressive tax function (see Figure 2) and provides a tax incentive for marriage.[11] The new parental leave benefit does not depend on couples' marital status but it shifts the tax schedule towards higher tax rates (progressivity effect): after the reform, the effective tax rate is determined based on the sum of household income and parental leave benefits. The effective tax rate is then applied to household income, only. As the tax rates rise with overall income, benefits contribute to an increase in average taxes which is particularly pronounced for low income households (see Table A.1). We argue that this progressivity effect reduces the general tax incentives to marry such that the propensity for parents to get married declines for the period of benefit receipt (H4). In the next section we describe our data and empirical methods to test the hypotheses.

4. DATA AND METHODS

4.1. EMPIRICAL STRATEGY

To identify the causal effect of the reform, we compare outcomes of children born shortly before and shortly after the reform came into effect. We consider a window of three months around the cut-off date January 1, 2007. To isolate possible seasonal effects from those of the policy change, we 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. This strategy combines a discontinuity design with a difference-in-difference approach.[12] We estimate a linear model of the form:

[11] 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 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.

[12] Dustmann and Schönberg (2012) use a similar strategy to evaluate expansions in maternity leave duration on children's long-term outcomes.

$$y_i = const + \alpha\, treat_i + \beta\, after_i + \gamma\,(treat_i \cdot after_i) + cohort_i' \theta + x_i' \delta + \varepsilon_i \quad (1)$$

where y_i denotes the living arrangement for a child i. We study three exclusive outcomes: living with a married couple, a cohabiting couple, and a single mother.[13] The indicator variable $treat_i$ equals one if a child belongs to the treated birth cohort, i.e., was born around the reform's cut-off date. 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. Seasonal effects are captured by the variable $after_i$, 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 at 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 key assumption to identify the coefficient of interest, γ, 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

[13] 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.

parents cannot perfectly plan the conception of a child provide convincing evidence that births in the first quarter of 2007 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 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),[14] 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 cutoff day of the reform.

4.2. DATA

We use data from the German Micro Census 2005-2012. Each survey year provides a one percent sample of the population currently living in Germany. The key advantages of the Micro Census are the availability of information on an individual's month of birth[15] and relatively large sample sizes. However, our research design focuses on a subset of the German population because 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 and include only those who reside in West Germany to obtain a more homogeneous sample. We focus on West German children for two reasons: first, they represent a vast majority (80 %) of the population

[14] More precisely, Tamm (2012) estimates that 7.8% and Neugart and Ohlsson (2012) that 5.4% of births scheduled for the last week of December 2006 were shifted to the first week of January 2007. While Neugart and Ohlsson (2012) emphasize the biological impossibility to postpone birth by more than a few days, Tamm (2012) argues that some deliveries could have been moved by more than 1 week.

[15] Because the information on the month of birth is not available in the scientific use files, we use the data via a controlled remote access.

of interest; second, West and East Germany differ in many aspects related to living arrangements in early childhood.[16]

Given these sample selection criteria, we observed roughly 1,000 children for each of the included cohorts. 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 1 illustrates the relationship between age of the included cohorts and the reporting year and provides the number of observations, as well.

Consequently, we estimate the effect of the reform for children's living arrangements at different ages. As the samples for single age years are relatively small, we aggregate the age years to crucial periods of a child's life to gain precision. To investigate the average effects of the reform on living arrangements during the benefit take-up period, we pool observations from the first and second year of a child's life (see Table 1), which gives in total 9,889 children.[17]

The distribution of interviews is random over the entire calendar year. However, by construction, children born in the first quarter of the year are on average three months younger when the data is collected than children born before the cut-off. Given that the treated and control cohorts are all affected by this age difference, we account for it in our difference-in-differences strategy. Nevertheless, we additionally control for a child's age in months to adjust for smooth age trends in child living arrangements.

[16] 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). Moreover, work by Schnabel (2015) and Haan (2005) demonstrates that East and West German women still differ substantially in their labor supply, both in terms of participation and work hours. Furthermore, the two groups differ substantially with regard to attitudes toward cohabitation and mother's labor force participation (Kreyenfeld and Geisler 2006). Finally, we are not able to separately study East Germany due to the small sample size.

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

A shortcoming of the data is that the Micro Census contains little retrospective information on respondents.[18] However, we are able to reconstruct a mother's pre-birth employment status by using the information on the start date (year and month) of her current employment for those employed and the termination date for those not employed at the time of interview. Given that maternal pre-birth labor income is not available, we use detailed information on a mother's educational attainment to proxy her human capital. Specifically, we control for her secondary school degree and the highest vocational degree. Table 2 presents descriptive statistics on the relevant variables.

The information on pre-birth employment is relevant for our analysis as the reform generated "losers" among low income families and "winners" among high income families. After the reform, the benefit amount depends on the pre-birth earnings of the parent who interrupts employment, i.e., usually the mother. Given the lack of retrospective earnings information in our data, we use a mother's employment status before childbirth as a proxy for being in the group of winners and losers. We classify mothers as working if they had done any paid work during the twelve months prior to giving birth. Benefit take-up statistics report that mothers with any pre-birth employment receive on average more than twice the benefit amount compared to mothers without pre-birth employment (STBA 2013). For example, in 2011, the average benefits for the two groups amounted to 868 and 330 EUR, respectively (STBA 2013). Clearly, non-working mothers are worse-off after the reform since the average payment of 330 EUR is only 10 percent higher than the previous payment and is 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 due to the shortening of the benefit period. Thus, the

[18] An additional shortcoming of the survey is that it allows us to match a child to its parents and siblings only if they live in the same household at the time of the interview. Although the dataset is rich in many aspects, it does not report genetic links, so that we cannot distinguish between biological, step, and adoptive relationships (for a discussion of the relevance of biological ties, see Moffitt et al. 2015).

share of "losers" is higher among the non-working and the share of "winners" is higher among the group of working mothers.

5. RESULTS

Table 3 reports our key results on the effect of the parental leave reform on child living arrangements in their first two years of life (ages 0-1). Each cell shows the estimated coefficient γ obtained from a separate linear probability model and its robust standard error. The outcome measures in columns 1 to 3 are indicator variables of whether a child is living with a married couple, with a cohabiting couple, and with a single mother, respectively. We first estimate the effects on the entire sample (panel A) and then separately by a mother's pre-birth employment status (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 compared to the average incidence before the reform of roughly 16 percent. Interestingly, the alternative living arrangements contribute in similar magnitudes to the increase in cohabitation, although statistically insignificant.

Panel B evaluates the reform effect for children whose mothers did not report any employment in the year prior to birth. 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, so that we do not find clear changes for this group which approximate the "losers" of the reform. In contrast, panel C demonstrates that the reform significantly affected living arrangements of children whose mothers worked prior to childbirth. The statistically and economically significant effects in

[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 virtually unchanged.

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 working mothers. We do not find any notable shift away from marital unions (column 1).

Overall, the estimates in Table 3 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 working mothers, i.e., the potential winners of the reform gaining generous benefits. 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 by 4.4 percentage points.[20]

How do these results relate to the hypotheses on the mechanisms behind the changes in living arrangements? First, based on the hypothesized insurance function of marriage, we do not find the expected increase in marriage rates among losers of the reform. The point estimates for children of non-working mothers suggest rather the contrary, i.e., a shift away from married unions towards cohabitation. Also, we do not find any increase in the share of married couples among the winners of the reform.

Second, the significant shift away from single motherhood towards cohabitation is in line with the hypothesis that the reform incentivizes paternal involvement and strengthened the attachment of fathers to their newborns. As expected, the risk of being raised by a single mother declines only for 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.

To test whether the positive effect from fathers' involvement differs for daughters and sons, we perform additional estimations, which include an interaction term between the reform

[20] We are not offering results for the years four and five after a birth because the information mothers' pre-birth employment status is missing in a large number of cases. In addition, these regressions can be estimated only without the post-reform cohorts which differs from our specification.

effect and the gender of the child. Table 4 reports the results. The first row in each panel evaluates the reform effect for girls. The second row shows whether significant differences exist between boys and girls.

Table 4 largely underpins our main result that the reform decreases the risk of growing up in a single mother household (see column 3). Again, only fathers in families with a working mother receive the incentive for additional paternal involvement (panel C). However, we find that these positive incentives are particularly pronounced for fathers of newborn daughters. The absolute reform effects for sons derive from summing up the reported point estimates in the first and second row in each panel. We find that the reform has virtually no effect on the probability of living 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 estimate the probability for sons and daughters to live in either type of household composition. We run linear probability models for the living arrangements separately for births prior to the reform (2005-2006) and after the reform (2007-2012). These regressions include children born in all quarters of the survey years.

Table A.3 reports the estimates of the boy indicator which is the variable of interest. The results in Panel A expose significant gender differences in child living arrangements prior to the reform, i.e., in 2005 and 2006. Consistent with evidence for the US (Dahl and Moretti 2008), we observe significantly higher probabilities for sons to live with married parents, no significant gender difference regarding cohabitation and a significantly higher propensity for daughters to live with single mothers. This suggests that prior to the reform fathers sorted into households with sons. The results in Panel B show that the differences disappear after the reform. Overall,

Tables 4 and A.3 suggest that the reform contributed to balance prior disadvantages of daughters compared to sons.[21]

With respect to our fourth hypothesis, the tax effects of the reform, Table 3 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.[22] 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. If couples respond to the new tax disadvantages, then families below the median should display lower marriage probabilities, compared to couples above the median, due to relative higher increases in average tax rates (see Table A.1 column 4). Table 5 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 the median income. We do not find any significant drop in marriage rates for families below the median income; furthermore, none of the interaction terms are statistically significant. This indicates that the different income groups do not differ in their marriage propensities.[23] Overall, we find no support for the hypothesis that the reform disincentivizes marriage during the period of transfer receipt.

[21] We also examined the sensitivity of the results in Table 4 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.

[22] In contrast to our findings, Kluve and Schmitz (2014) report statistically significant drops in marriage rates after the reform. The difference compared to our findings may relate to their inclusion of the East German sample and the different composition of control groups.

[23] In a robustness test, we considered fathers' instead of household income. We obtained identical patterns of the coefficient estimates.

6. ROBUSTNESS TESTS

In this section we examine the robustness of our results with respect to a number of concerns. In particular, we estimate our models without any control variables, excluding certain birth cohorts, excluding January and December births. We also simulate two placebo reforms taking place the year before and after the actual reform.[24] Table 6 presents the results for the various checks separately for each group. For ease of comparison, we include the baseline coefficient in the first row within each panel. For the first set of tests, we expect to find the same qualitative results as in our baseline specification, and we expect insignificant treatment effects for the two placebo reforms.

Starting with the pooled sample in Panel A, we see that the main conclusions are highly robust with respect to the sensitivity checks. In particular, model A2 shows that the effect does not depend on controlling for characteristics of the child or the mother at birth, lending credibility to the assumption that the reform was unanticipated and hence does not correlate with observable characteristics. We further vary the time window of the reform in specifications A3 and A4 to test whether our selection of the control group affects the results. Our main conclusion concerning cohabitation and single motherhood do not change. Focusing on a symmetric three year time window around the reform by excluding the birth cohorts 2004/05 and 2008/09, we find that changing the control groups does not affect our conclusions about a significant increase in the probabilities of living with cohabiting parents, or a single mother. To assess whether couples anticipated the reform by bringing the birth date forward or backward, we drop January and December births as in specification A5. The results are less precisely estimated but the qualitative patterns remain the same. Finally, we simulate two placebo

[24] 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 individuals from the analysis who moved to Germany in the year of birth or the year prior to giving birth. The results remained unchanged.

reforms in the year before (A6) and after the reform (A7).[25] As expected, the results for both A6 and A7 are all insignificant confirming the validity of our main conclusions.

Panel B focuses on the small subsample of mothers who did not work prior to the reform; given the small number of treated individuals in each group, the validity of the test results may be limited. We find that the tendency to live with cohabiting as opposed to married parents remains robust when dropping the control variables (B2) or changing the time windows (B3 and B4), with an unchanged probability of being raised by a single mothers. However, the coefficients flip sign once we exclude the January and December births, which reduces the sample size considerably; this may indicate some anticipation effects by non-working mothers, although previous studies found that this group in particular did not bring births forward (Neugart and Ohlsson 2012, Tamm 2012). We therefore place some caution on the interpretation of the results for this group, particularly given that we also find a highly significant placebo effect for the year 2005/06. To the best of our knowledge, this placebo reform date does not concur with any family policy reform, but happens to take place exactly one year after the introduction of the Hartz-Reforms which substantially changed the German income support system (Caliendo 2009).

Comparing the coefficients across rows for working mothers in Panel C, i.e., the potential winners of the reform, we see that the estimated effects are highly robust to the various specification checks. In particular, changing the control variables (C2) and time windows (C3-C5) does not change the main conclusions, and the two placebo reforms yield insignificant results.

[25] We drop the reform cohort from the placebo estimations.

7. CONCLUSIONS

We study the causal effect of a recent paid parental leave reform on children's living arrangements in Germany. A large literature documents the relevance of living arrangements for the wellbeing and long run outcomes of children and discusses the relevance of public policies for these patterns. The German reform replaced a rather small means-tested benefit (*Erziehungsgeld*) available for a subgroup of parents with a universal paid parental leave benefit (*Elterngeld*) based on prior labor income. To identify the causal effect of the reform for its winners and losers, we combine a discontinuity design with a difference in differences approach. The empirical analysis applies data from the German Micro Census, a large and representative annual survey.

We hypothesize that parental responses to the introduction of the new parental leave benefit (*Elterngeld*) may be affected by the wish to insure the economic wellbeing of mother and child, by incentives deriving from the German income tax code, and by providing new incentives for fathers to be more involved in child rearing. In addition, a large international literature suggests that the living arrangement choices of parents may vary by the sex of the child. 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 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 children's 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.

Separate analyses suggest that there is no evidence supporting insurance effects or responses to tax incentives. However, for the group of mothers who worked prior to birth and are most likely to benefit from the reform, we find a decline in single parenthood and an increase in the propensity to cohabit. This supports the hypothesis that the new benefit enhances paternal

involvement in child rearing. Among mothers who did not work prior to birth, we find no significant reform effects on living arrangements. Possibly in the group of families who did not benefit from the reform, the new incentives for paternal involvement were not sufficient to balance newly arising financial disadvantages. 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 apparently contribute to balance this disadvantage as they are exclusively observed for daughters. Finally, we show that fathers do not solely "move in and out" to receive the new benefit, as we find a sustained decline in single parenthood 2-3 years after the reform (-4.4 percentage points).

Our study contributes to our understanding of a large public policy reform: the German reform of paid parental leave produced unintended, yet important, spill-overs for children's living arrangements. Given the previous literature which documents a negative relationship between single motherhood and child outcomes (sometimes claiming causality), the effect may be beneficial. Future work needs to evaluate whether the changes in children's 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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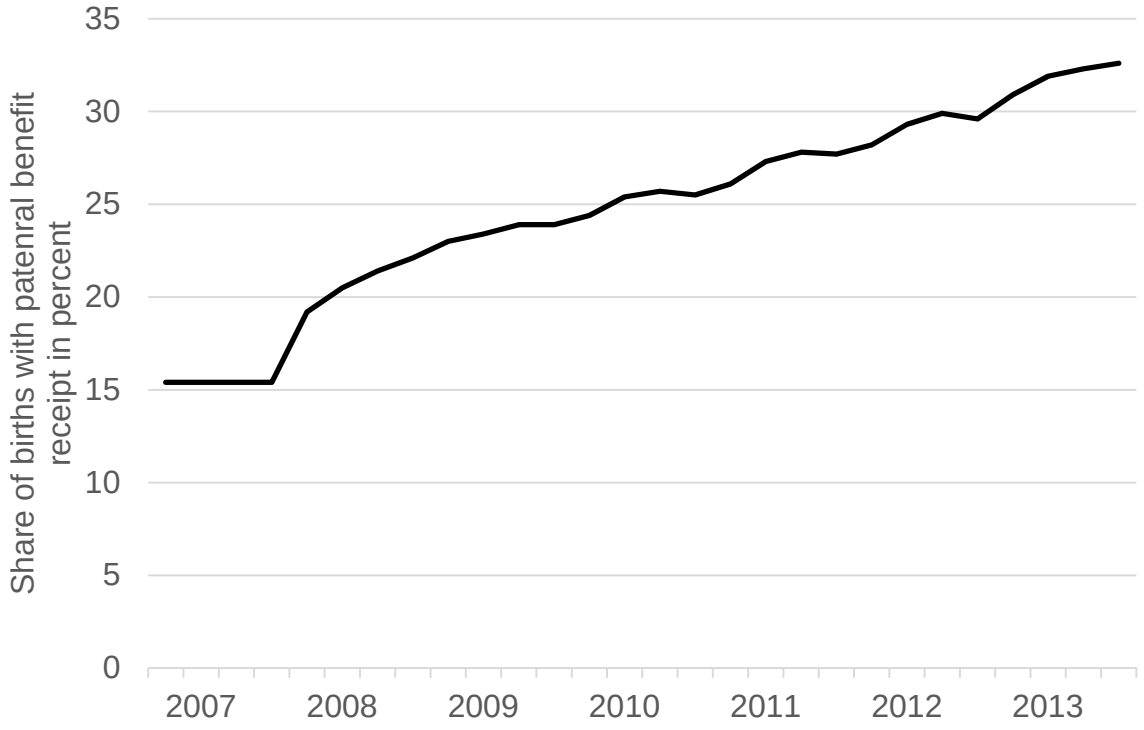
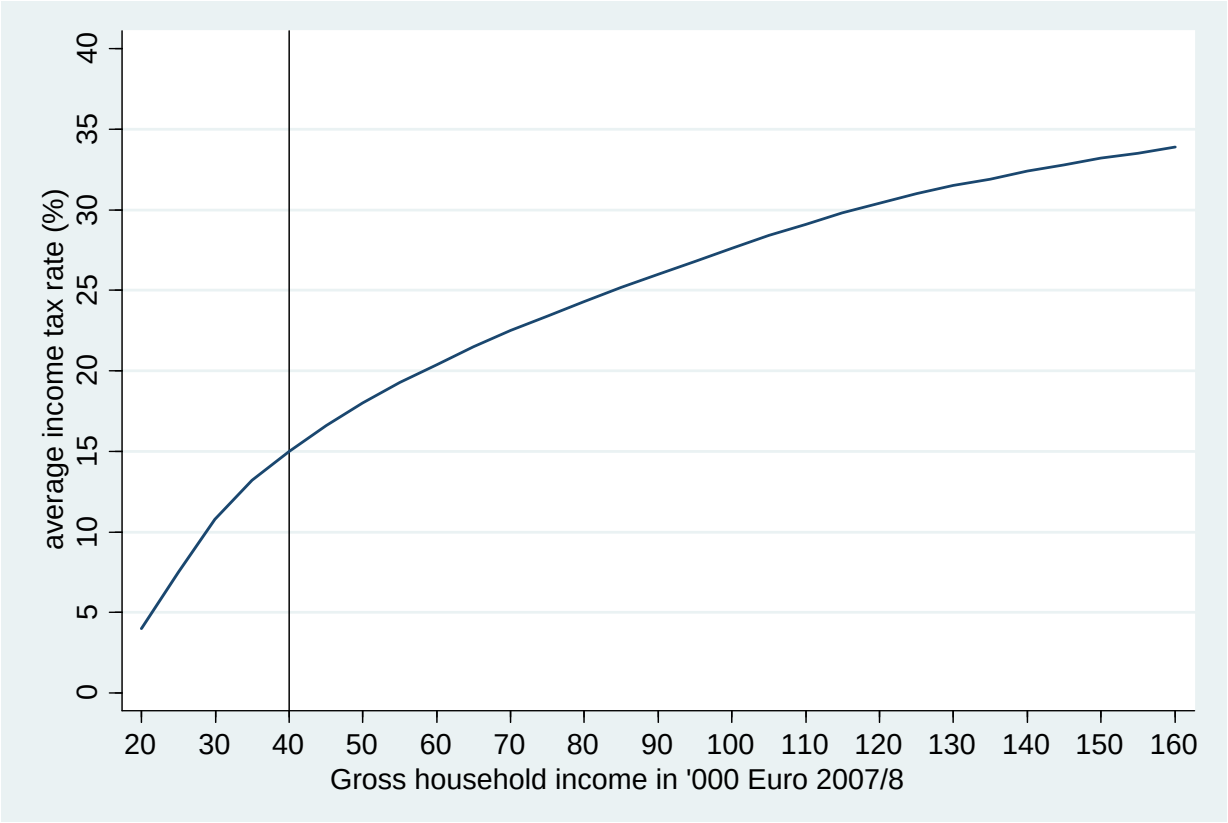
Figure 1. Share of births with paternal receipt of parental leave benefit by quarter of birth



Source: For 2007 births, we only have information for the full year (see STBA 2008); STBA (2015) provide 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.

Table 1. 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.

Table 2. Descriptive Statistics

	Mean	Std. Dev.
Child's living arrangement		
married couple	0.728	0.445
cohabiting couple	0.158	0.364
single mother	0.114	0.318
Child's characteristics		
birth cohort 2008/09	0.190	0.392
birth cohort 2007/08	0.211	0.408
birth cohort 2006/07 (treated)	0.193	0.395
birth cohort 2005/06	0.198	0.399
birth cohort 2004/05	0.208	0.406
born in 1st quarter of year	0.486	0.500
male	0.497	0.500
multiple birth	0.036	0.187
age in months	13.720	6.850
state of residence: Schleswig-Holstein	0.047	0.211
state of residence: Hamburg	0.031	0.174
state of residence: Niedersachsen	0.117	0.322
state of residence: Bremen	0.008	0.087
state of residence: Nordrhein-Westfalen	0.257	0.437
state of residence: Hessen	0.099	0.299
state of residence: Rheinland-Pfalz	0.060	0.238
state of residence: Baden-Württemberg	0.170	0.375
state of residence: Bayern	0.200	0.400
state of residence: Saarland	0.012	0.109
Maternal characteristics		
age at childbirth	28.753	5.523
school degree: no	0.030	0.170
school degree: Hauptschulabschluss	0.221	0.415
school degree: Realschulabschluss	0.354	0.478
school degree: Fachhochschulreife	0.082	0.275
school degree: Abitur	0.300	0.458
school degree: other	0.004	0.064
school degree: missing	0.008	0.090
occupational degree: no	0.205	0.404
occupational degree: Lehre	0.511	0.500
occupational degree: Berufsfachschule, Schule Gesundheitswesen, Fachschule, Meister, Beamtenausbildung	0.090	0.287
occupational degree: tertiary degree	0.179	0.383
occupational degree: other	0.010	0.100
occupational degree: missing	0.004	0.066
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

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 (N=9,889).

Table 3. Estimation results: effects on children's living arrangements (at ages 0-1)

	(1) married couple	(2) cohabiting couple	(3) single mother
Panel A: all children (N=9,889)			
treat*after	-0.018	0.038 **	-0.021
	(0.021)	(0.018)	(0.016)
Panel B: children of non-working mothers (N=2,231)			
treat*after	-0.038	0.032	0.006
	(0.043)	(0.034)	(0.038)
Panel C: children of working mothers (N=7,306)			
treat*after	-0.012	0.043 **	-0.031 *
	(0.025)	(0.021)	(0.017)
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, employment, 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 2005-2010, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany.

Table 4. Estimation results: effects on children's living arrangements (at ages 0-1), by gender

	(1) married couple	(2) cohabiting couple	(3) single mother
Panel A: all children (N=9,889)			
treat*after	0.002	0.046 *	-0.048 **
	(0.030)	(0.025)	(0.023)
treat*after*boy	-0.039	-0.016	0.054 *
	(0.042)	(0.036)	(0.032)
Panel B: children of non-working mothers (N=2,231)			
treat*after	-0.039	0.033	0.006
	(0.060)	(0.045)	(0.054)
treat*after*boy	0.002	-0.002	0.001
	(0.086)	(0.069)	(0.076)
Panel C: children of working mothers (N=7,306)			
treat*after	0.017	0.048	-0.065 **
	(0.036)	(0.030)	(0.025)
treat*after*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. All regressions include a constant. Child's characteristics comprise indicators for a child 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, employment, and migration status. A mother's working status refers to her status in the last 12 pre-birth months. *, **, 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 5. 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)	
treat*after	-0.050
	(0.033)
treat*after*(above median household income)	0.017
	(0.042)
Panel B: children of non-working mothers (N=1,663)	
treat*after	-0.059
	(0.053)
treat*after*(above median household income)	0.029
	(0.086)
Panel C: children of working mothers (N=6,092)	
treat*after	-0.040
	(0.043)
treat*after*(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, employment, 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 2005-2010, own calculations. Samples restricted to first-born children who were born in Germany and reside in West Germany and reside with both parents.

Table 6. Robustness checks

	(1) married couple			(2) cohabiting couple			(3) single mother		
Panel A: all children									
A1: baseline (N=9,889)	-0.018	(0.021)		0.038	(0.018)	**	-0.021	(0.016)	
A2: no controls (N=9,889)	-0.013	(0.023)		0.036	(0.018)	**	-0.023	(0.017)	
A3: excl. birth cohort 04/05 (N=7,833)	0.001	(0.022)		0.026	(0.019)		-0.027	(0.016)	
A4: excl. birth cohorts 04/05, 08/09 (N=5,958)	-0.002	(0.023)		0.034	(0.019)	*	-0.032	(0.017)	*
A5: excl. January & December (N=6,358)	-0.003	(0.027)		0.030	(0.022)		-0.027	(0.020)	
A6: placebo reform 2007/8 (N=7,982)	-0.031	(0.022)		0.021	(0.019)		0.010	(0.015)	
A7: placebo reform 2005/6 (N=7,982)	-0.013	(0.021)		-0.012	(0.017)		0.025	(0.016)	
Panel B: children of non-working mothers									
B1: baseline (N=2,231)	-0.038	(0.043)		0.032	(0.034)		0.006	(0.038)	
B2: no controls (N=2,231)	-0.031	(0.049)		0.024	(0.036)		0.007	(0.040)	
B3: excl. cohort 04/05 (N=1,737)	-0.015	(0.045)		0.016	(0.036)		-0.001	(0.04)	
B4: excl. cohort 04/05, 08/09 (N=1,349)	-0.031	(0.048)		0.041	(0.038)		-0.010	(0.043)	
B5: excl. January & December (N=1,434)	0.063	(0.053)		-0.043	(0.041)		-0.020	(0.049)	
B6: placebo reform 2007/8 (N=1,758)	-0.063	(0.049)		0.053	(0.041)		0.009	(0.042)	
B7: placebo reform 2005/6 (N=1,758)	0.043	(0.045)		-0.073	(0.035)	**	0.030	(0.040)	
Panel C: children of working mothers (N=7,306)									
C1: baseline (N=7,306)	-0.012	(0.025)		0.043	(0.021)	**	-0.031	(0.017)	*
C2: no controls (N=7,306)	-0.005	(0.026)		0.041	(0.022)	*	-0.036	(0.018)	
C3: excl. cohort 04/05 (N=5,821)	0.009	(0.026)		0.030	(0.022)		-0.039	(0.018)	**
C4: excl. cohort 04/05, 08/09 (N=4,404)	0.006	(0.027)		0.035	(0.023)		-0.041	(0.019)	**
C5: excl. January & December (N=4,696)	-0.023	(0.031)		0.054	(0.026)	**	-0.031	(0.022)	
C6: placebo reform 2007/8 (N=5,928)	-0.029	(0.025)		0.018	(0.022)		0.011	(0.016)	
C7: placebo reform 2005/6 (N=5,928)	-0.025	(0.024)		-0.003	(0.020)		0.022	(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, employment, 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 2005-2010, 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	4.5	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 children's living arrangements (at ages 2-3)

	(1) married couple	(2) cohabiting couple	(3) single mother	
Panel A: all children (N=10,200)				
treat*after	0.025	0.019	-0.044	***
	(0.021)	(0.015)	(0.017)	
Panel B: children of non-working mothers (N=3,413)				
treat*after	-0.006	0.027	-0.021	
	(0.037)	(0.028)	(0.032)	
Panel C: children of working mothers (N=5,904)				
treat*after	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, employment, and migration status. 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.

Table A.3 Estimation results: effect of child gender on children's living arrangements (at ages 0-1)

	married couple	cohabiting couple	single mother
Panel A: Before reform (all births in 2005-2006; N = 12,366)			
boy	0.018 **	-0.004	-0.014 ***
	(0.007)	(0.006)	(0.006)
Panel B: After reform (all births in 2007-2012; N=20,966)			
boy	0.008	-0.006	-0.001
	(0.006)	(0.005)	(0.004)
Child characteristics	yes	yes	yes
Maternal characteristics at childbirth	yes	yes	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 child and maternal 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 a mother's age in years (linear and squared), indicators for education, employment, 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 and reside with both parents.