Can financial incentives reduce the baby gap? Evidence from a reform in maternity leave benefits*

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Abstract

In this paper, I assess whether earnings-dependent maternity leave positively impacts fertility and narrows the baby gap between highly educated (high-earning) and less-educated (low-earning) women. I exploit a major maternity leave benefit reform in Germany that considerably increased the financial incentives, by up to 21,000 EUR, for highly educated and higher-earning women. Using the large differential changes in maternity leave benefits across education and income groups in a differences-in-differences design, I estimate the causal impact of the reform on fertility for up to 5 years. In addition to demonstrating an up to 23% increase in the fertility of tertiary-educated women, I find a positive, statistically significant effect of increased benefits on fertility, driven mainly by women at the middle and upper end of the earnings distribution. Overall, the results suggest that earnings-dependent maternity leave benefits, which compensate women according to their opportunity cost of childbearing, could successfully reduce the fertility rate disparity related to mothers' education and earnings.

Keywords: Fertility, fertility gaps, paid maternity leave, opportunity cost.

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1 Introduction

As women's educational attainment and labor market participation have increased, so too have concerns about decreasing birth rates and below-replacement fertility levels. Among developed countries, Germany, Italy, South Korea, and Japan are all experiencing total fertility rates below 1.4, and even the U.S. has seen its traditionally high fertility fall to a record low well under replacement level (Table 1, Panel A).

In an attempt to mitigate this decrease, all OECD countries except the U.S. now ensure paid maternity leave,[1] which provides employment protection and some degree of earnings replacement, with a primary goal of facilitating family and career compatibility and improving child welfare. Another aim of such policies, and one that has received far less research attention, is to encourage fertility by reducing the opportunity costs of childbearing. For example, according to German Chancellor Angela Merkel, the low fertility rate among highly educated women was a motivating force behind the 2007 German reform studied here, which marked a "paradigm shift in social policy" in Germany (Bundesregierung (2006)).

In addition to the decrease in overall birth rates, in many developed countries, highly educated and high-earning women are also having fewer children over a lifetime than their less-educated and lower-earning peers (Table 1, Panel B), a fertility differential that I call the "baby gap." The likeliest explanation for this negative relationship between education and completed fertility is the higher opportunity cost of childbearing for the more highly educated, who must forego a higher wage to temporarily leave the labor market (Willis (1973)).[2] In Germany, this gap manifests as a stark difference between highly educated and less-educated women in both number of children born (1.33 vs. 2.06) and the percentage of childless women. In fact, nearly one-third of the 1963-1967 cohort of highly educated women in Germany have never had a child, compared to 18% of women with no postsecondary schooling. Likewise, in the U.S., women with a college degree born between 1965-1969 gave birth to an average of 1.81 children compared with 2.56 for women who did not complete high school. In countries with traditionally generous family policies (e.g., Sweden) the disparities between education groups are smaller.

In this paper I examine whether paid maternity leave affects fertility decisions by exploiting a 2007 maternity leave reform in Germany that substantially changed maternal compensation for time out of the labor market following childbirth. Before this

[1] Because my entire analysis focuses on the effect on mothers, I refer to the benefit as "maternity leave" rather than using the umbrella term "parental leave," which encompasses maternal, paternal, and adoption leave (and sometimes even family leave for other types of care).

[2] See also Aaronson *et al.* (2014) for recent empirical evidence. As predicted by theory, fertility has been shown to decrease with a woman's potential wage (see e.g. Rosenzweig and Schultz (1985) and Heckman and Walker (1990)).

reform, German maternity leave benefits were flat means-tested transfers targeted at lower-income families that paid an average benefit of around 4,000 EUR for a maximum of two years, irrespective of the mother's pre-birth earnings. Since 2007, however, the new scheme has offered mothers a generous income replacement of at least 67% of annual pre-birth earnings, with a maximum 3,600 EUR basic transfer for women not in the workforce pre-partum. Nonetheless, although the reform raises benefits by up to 21,000 EUR for highly educated and high-earning women, the changes for very low-earning (and less-educated) women are modest or even negative.

To measure the extent to which fertility in Germany has reacted to this reform and add valuable new insights to the currently sparse empirical evidence on this effect, I apply a differences-in-differences approach that exploits the differential changes in leave benefits across earnings and education groups. As my primary data set, I use novel administrative data from the German Pension Registry, which records precise information on earnings, education, and fertility for all women insured under the statutory pension insurance scheme. I complement this information with data from the nationally representative German Microcensus, which employs rich demographic measures.

If earnings-related paid leave affects women's fertility decisions, then I should observe those who benefit most to increase fertility relative to their peers who benefit less. My empirical analysis does indeed uncover substantial pronatal effects of the reform, as well as medium-run changes in the socioeconomic structure of fertility. This finding matters not only for countries with low fertility rates, but also for governments trying to mitigate the declining fertility associated with women's increased labor market participation. First, based on vital statistics, I document discontinuous jumps in monthly birth rates of close to 4% nine months after reform implementation, which translates into 2,350 additional children born each year in the short term and an increasing trend in birth rates post-discontinuity. Second, by exploiting the large differential changes in maternity benefits across earnings and education groups, I demonstrate that the probability of having a child in a given year within the five year post-reform period increases by up to 1.15 percentage points (23%) for highly educated relative to less-educated women. At the same time, the medium-term (within five year) fertility of women earning at least 5,850 EUR net, who benefit substantially from the reform by on average 5,000 EUR, increases by 16% relative to those earning below, who on average did not benefit from the reform. In fact, the reform appears to be positively affecting the fertility of women in all earnings groups beyond the median, including the top 5th percentile. Admittedly, these reduced-form effects of the reform on fertility behavior, being policy-relevant parameters, could be driven by both increased monetary transfers and any endogenous labor supply adjustments. Third, under the assumption that the post-reform changes in paid maternity leave only affect fertility by increasing the financial incentives to have a child, I can use the reform to estimate the changes in financial incentives on fertility. My baseline estimate suggests that a 1,000 EUR increase in total potential entitlement raises the birth probability by 0.78 percentage points (2.1%) in each year post-reform. Lastly, by estimating the effect of the reform separately for different age groups, I find a strong reform-induced increase in fertility for women aged 35-39 and 40-44, who are nearing the end of their lifetime fertility and unlikely to postpone childbearing. Such an increase in these cohorts' medium-run fertility is likely to have a permanent effect and raise their completed fertility.

My findings contribute to a growing body of structural and quasi-experimental literature on family policies and fertility. Early papers in the structural literature (e.g., Moffitt (1984), Hotz and Miller (1993), Heckman and Walker (1990)) analyze female labor supply and fertility jointly in reduced-form models and confirm that fertility decreases with a woman's potential wage, but do not directly relate their findings to financial incentives or family policies.[3] More recent papers (such as Francesconi (2002), Keane and Wolpin (2010) or Adda *et al.* (2017)) build on dynamic life-cycle models. Simulating the impact of a pronatalist cash transfer, Adda *et al.* (2017) find large short-term effects but smaller long-run effects on fertility on primarily younger women. In order to address the common challenge faced by the literature to find exogenous variation in the cost of fertility (see Hotz *et al.* (1997)), Laroque and Salanié (2014) and Haan and Wrohlich (2011) exploit cross-sectional variation in financial incentives resulting from the French and German tax-transfer systems, which are driven by differences in household characteristics. Both papers find sizable fertility effects of a simulated, universal child subsidy. To my knowledge, Stichnoth (2018) provides the only structural model evidence for (short-run) effects of paid leave benefits on fertility in a discrete-choice model using survey data. Compared to the literature using dynamic life-cycle models, my quasi-experimental analysis can admittedly only identify the shorter-run effect on fertility and might not fully capture fertility adjustments of younger women. However, while most of the structural literature relies on cross-sectional variation in incentives, I can apply a tighter identification strategy that exploits reform-induced variation in benefits across socioeconomic groups over time within a simpler DID-style framework. The literature also mostly draws on survey data with smaller sample sizes, and I draw on larger administrative datasets to estimate the fertility effects of changes in paid maternity leave.

The quasi-experimental literature tends to focus on the incentive effect of child subsidies, child cash transfers, and welfare programs, all designed to set higher financial

[3] Heckman and Walker (1990) find that the effects of female wages on fertility weakened for more recent Swedish cohorts and argue that the finding might result from the introduction of Swedish family policies but are unable to to define precise measures to directly estimate the policy effect (see footnote 10 in Heckman and Walker (1990)).

incentives for lower-income women.[4] Two such studies, which exploit the variation in universal child subsidies for the third (or higher) child relative to the first or second child in Quebec (Milligan (2005)) and Israel (Cohen *et al.* (2013)) find a strong pronatal effect. Riphahn and Wynck (2017) assesses the effects of a child benefit reform in Germany and finds modest positive effect only for second-order births of higher-income households.

The literature on paid maternity leave focuses largely on policies' effects on maternal labor supply and child outcomes (see particularly Baker and Milligan (2008, 2010), Dustmann and Schönberg (2012), and Carneiro *et al.* (2014)). Evidence of the impact on fertility, in contrast, is still sparse and is focused on the impact of leave changes for a current child on subsequent higher-order fertility decisions, labelled the "current child effect" (Lalive and Zweimüller (2009), Dahl *et al.* (2016), Cygan-Rehm (2016), and Kluve and Schmitz (2018)). All papers adopt a regression discontinuity design that compares the subsequent fertility of mothers who gave birth to their current child shortly before the reform with mothers who gave birth shortly after the reform. These mothers face different paid leave benefits, both in terms of transfers and leave duration, for the baby already born (the current child), but in principle identical benefit levels for the child yet to be born (the future child). As a result, the identified current child effect has three potential drivers: changes in the benefit level and duration for the current child, a(n endogeneous) change in mothers' return to work behavior after the birth of the current child, or changes in the automatic renewal periods of benefits for the future child ("speed premium"), which can in turn affect the spacing of births. There is, however, little consensus across studies. Lalive and Zweimüller (2009) find strong effects of an Austrian expansion in the duration of paid job-protected maternity leave for the first child on the mother's (subsequent) higher-order fertility up to 10 years after the first birth. Because the Austrian maternity leave benefits were flat transfers, the reform inherently affected higher-order fertility more strongly for low-wage mothers than for high-wage mothers. In contrast, Dahl *et al.* (2016) analyze the long-run effects of a series of expansions in paid maternity leave in Norway and find little effect on completed fertility. Both Cygan-Rehm (2016) and Kluve and Schmitz (2018) analyze the effect of the 2007 German paid leave reform on higher-order fertility and birth spacing up to 5 years post-reform. While Cygan-Rehm (2016) documents that fertility of most mothers catches up with initial postponements of further births by the fifth year, Kluve and Schmitz (2018) find small negative effects on subsequent childbearing between 3 months and 5 years after the birth of the current child primarily driven by

[4]See, for instance, the paper by González (2013) on the positive immediate fertility effects of an introduction of a universal cash benefit in Spain. Moffitt (1998) provides an overview of the literature on the effects of AFDC and concludes that the evidence is inconclusive. Baughman and Dickert-Conlin (2003) and Brewer *et al.* (2011) find positive effects on the fertility of low-income women for increases in the EITC and the effect of UK welfare reforms.

young women.

I add to the quasi-experimental literature along several important dimensions. First, I provide causal evidence on how an increase in paid maternity leave for a baby yet to be born, which lowers the immediate costs associated with childbearing, can directly affect fertility decisions up to 5 years post-reform (the "future child effect"). This direct effect on future childbearing is potentially more policy relevant than the indirect effects of policy changes for the current child on subsequent higher-order births. While Lalive and Zweimüller (2009) attempt to estimate this effect by comparing the subsequent fertility of different cohorts of mothers who face the same policy for the first child but different leave duration for future children, the switch between benefit systems in Germany allows me to to estimate this future child effect with a differences-in-differences design, which is a tighter identification strategy than an across-cohort comparison. Another limitation of the existing literature is that it can only identify a fertility effect along the intensive margin of childbearing, which might be driven by women with preferences for a large number of children. I, in contrast, analyze the fertility effect of paid maternity leave programs not only along the intensive margin but also along the extensive margin. This expansion matters because evaluating the effect(s) on the decision to have a first or second child appears to be the crucial margin of interest, particularly for highly educated women, more than half of whom have at most one child during a lifetime.[5] My analysis can thus provide a far more complete picture of how financial incentives affect fertility decisions at a time when falling fertility makes it crucial to better understand the effects of earnings-dependent leave policies on fertility behavior. Finally, my quasi-experimental evidence is especially relevant because, unlike traditional family policies such as child subsidies, the earnings-dependent schemes introduced in numerous countries aim to compensate women for the opportunity costs of childbearing.

The remainder of the paper proceeds as follows. The next section explains how the changes in paid maternity leave legislation in Germany came about and describes the mechanisms through which they can affect fertility decisions. Sections 3 and 4, respectively, describe the empirical strategy and data sets, after which Section 5 presents the main results. Section 6 then briefly discusses the reform effects across age, and Section 7 concludes the paper.

[5] Among highly educated women aged 40-49 in 2012, 29% remained childless, 24.4% had one child, 34.2% two children, and only 12.4% had three or more children (see BIB (2012)).

2 Background

2.1 The maternity benefit reform

In Germany, government-provided paid leave has a long tradition.[6] Mothers have been entitled to paid leave 6 weeks before and 8 weeks after childbirth since the 1950s, and paid leave has continuously expanded in a series of reforms starting in the late 1970s. Since 1992, mothers have been granted a maximum 36 months of post-birth job protection, as well as government transfers for a maxiumum of two years whilst on leave.[7] In 2007, however, the German government implemented a major reform that fundamentally changed how the maternity benefit system compensated women for foregone earnings. Table 2 summarizes the pre- and post-reform scheme.

The old scheme, "child-rearing money" (*Erziehungsgeld*), was targeted at low-income families and paid out flat transfers under one of two options: a maximum of 300 EUR a month for up to 24 months (Option 1) or a monthly payment of 450 EUR over 12 months (Option 2) for mothers who wanted to work in the second year after childbirth. Transfers under both options were means-tested on family income during benefit receipt, effectively basing them on the spouse's income. In fact, only families earning below 30,000 EUR net (40,400 EUR gross) income (after several deductibles) were eligible for leave benefits, which in 2006 accounted for about 74% of all mothers.[8] Of the mothers eligible to between the two options, only about 15% (predominantly East German women) chose Option 2.[9] Average benefits paid to mothers in 2006 were between 3,850 and 4,440 EUR in total (based on data from from Statistisches Bundesamt (2006)).

On January 1, 2007, a new leave benefit, "parental money" (*Elterngeld*), replaced the old scheme, and all mothers with children born on or after that date were eligible. The reform, born out of a newly formed (and rather unexpected) coalition between the two largest political parties, the Christian Democrats and Social Democrats, aimed at "preventing income drops after childbirth, ... enhancing the economic independence of both parents, and allowing a fair compensation of opportunity costs of childbearing" (BMFSFJ (2008)). In contrast to the old means-tested benefits, the new transfer payments are not only more generous but provide universal coverage. Besides the key goal

[6] In contrast to the US, employer-provided paid leave schemes are very uncommon.

[7] Since 1986, fathers have been eligible for parental leave, but very few father have taken any leave, so the program is effectively a maternity leave program.

[8] Benefits were restricted to a duration of six months (total payment of 1,800 EUR) for those with an income threshold exceeding around 21,000 EUR and below 30,000 EUR, which applied to about 14% of mothers. In 2006, only about 60% of mothers were eligible for benefit payments for longer than six months.

[9] Maternal labor force participation has traditionally been low in Germany compared to other Western European countries, with only 36% of mothers with a child below age three working in 2006, which might explain the low share of mothers choosing the short-run Option 1.

of providing parents with the financial means to care for their child during the first year of life, the reform aims to increase fertility by tying benefits closely to women's net pre-birth earnings so as to compensate their opportunity cost of childbearing.[10]

For the majority of mothers, the new maternity leave benefit replaces 67% of previous net labor earnings for up to 12 months after the birth of a child, with benefits calculated on the basis of the average net earnings during the 12 months pre-birth. Women who were not previously working receive a flat minimum of 300 EUR a month, which translates into a total benefit of 3,600 EUR. Lower-earning women are granted a higher than 67% replacement ratio of previous net labor earnings, which is gradually lowered from 100% to 67% for women with monthly net earnings between 300 (total benefits of 3,600 EUR) and 1,000 EUR (total benefits of 6,700 EUR). Compared to the pre-reform scheme, those with no pre-birth earnings and very low-earning women in low-income households, who would not have fallen under the pre-reform means-testing, experience decreases in the total maternity benefits they could receive. At the top of the earnings distribution, the transfer is truncated at 1,800 EUR a month, meaning a maximum benefit of 21,600 EUR for women with average net (gross) pre-birth monthly (yearly) earnings above 2,700 EUR (around 60,000 EUR).[11] As with the old system, eligibility depends on benefit recipients not working more than 30 hours a week during transfer receipt, while benefit reduction with increasing women's labour earnings below 30 hours also disincentivizes part-time work. As a result, in 2010, only 1.7% (less than 9%) of mothers were employed part-time in the first (last) month of benefit receipt.[12] Whilst the average total benefit paid to all mothers under the new system was 7,080 EUR in 2007, the scheme paid mothers employed prepartum a benefit of 10,128 EUR in 2008, making the new system considerably more generous than the old (see Table 2). In fact, there was close to full take-up of the new leave benefits (about 96% of all mothers), with many taking advantage of the benefit for the full eligibility period. For instance, the average receipt duration in 2010 was 11.7 months, and even those at the top end of the pre-birth earnings distribution—who qualified for a monthly transfer of 1,800 EUR—took up maternity benefits for an average of 11.2 months.

The 2007 reform generated substantial changes in maternity benefits across socioeconomic groups, as shown in Figure 1, that I use to identify the effect of the paid leave

[10]The reform also introduces two additional months of leave earmarked to the partner of the primary leave recipient ("daddy quota"). In the initial reform year 2007, 15.4% (Statistisches Bundesamt (2008)) of fathers took these two additional months and the share has increased to 29.3% in 2013 (Statistisches Bundesamt (2016b)). In order to not discriminate against single mothers, they are eligible to extend their leave by an additional two months.

[11]Women with net earnings above about 33,000 EUR (around 59,500 EUR gross) receive the maximum monthly benefit irrespective of their income, so their replacement rates lies below 67%, but this only affects a marginal fraction of less than 2% of my sample.

[12]All reported benefit statistics are based on the Elterngeldstatistik for 2007-2010, which covers all benefit claims and is published by the German statistical office.

reform on fertility decisions. Figure 1A shows the almost flat total pre-reform benefits across net earnings groups,[13] with post-reform benefits continuously increasing in net earnings for most of the earnings distribution. The net ective reform effect (i.e., the difference between the two lines) is continuously increasing in earnings, ranging from -2,400 EUR for yearly earnings under 1,800 EUR to an increase in average benefits of 17,100 EUR for women with net (gross) earnings over 33,000 EUR (60,000 EUR). Figure 1B further reveals that whereas pre-reform benefits replaced very little of higher-income women's foregone earnings relative to those of lower-income women, post-reform benefits replace at least 67% of these earnings for all women up to net earnings of 33,000 EUR (59,500 EUR).

2.2 Mechanisms: The effect of reform on fertility decisions

The 2007 reform in paid leave may affect fertility decisions for several reasons. A key goal of the reform was to lower the cost of potential childbearing by increasing paid maternity leave benefits for a large part of the population. As a result, the reform has a direct financial incentive effect on fertility decisions through increasing available family resources post-birth for a future child. However, unlike cash-in-hand benefits such as universal child subsidies, paid leave benefits are paid conditional on time out of the labor market. Mothers who wish to spend more time at home with their children (and pre-reform were potentially finally constrained in doing so) may decrease their labor supply and reduce their labor earnings under the more generous regime. Depending on the magnitude of the maternal labor-supply response, the total increase in cumulative family income via the reform could be lower than the increase in maternity leave benefits.[14] However, because maternal labor supply in the first years post birth is traditionally low in Germany and most mothers in the pre-reform period preferred the longer leave under Option 2 to the faster return-to-work Option 1, the reform is likely to have mainly crowded out either unpaid leave or paid leave with less generous benefits.

The existing literature on the reform's labor-supply effects documents heterogeneous patterns post childbirth. Whereas the reform encourages taking leave during the period of benefit receipt and reduces maternal employment in the first year after childbirth,

[13]The simulated pre-reform benefits are the average benefits across various income groups calculated on the basis of the spouse's net income. More details on the simulation of pre-reform benefits are given in Section 4.3 and in Appendix A.

[14]Furthermore, the reform also provided two additional "daddy months" to parents, which may have also changed the father's labor supply and involvement in childcare. Kluve and Tamm (2013), however, do not find significant changes in paternal employment rates or time devoted to childcare in the first year after birth. Given the modest economic effects, it appears unlikely that the "daddy months" act as a strong fertility incentive.

particularly for groups who benefit strongly (e.g. Bergemann and Riphahn (2011), Kluve and Tamm (2013), Kluve and Schmitz (2018)), some studies provide tentative evidence for small employment increases in the second year after childbirth. Investigating the effects beyond the first two years after birth, the study by Kluve and Schmitz (2018) finds that the reform encouraged maternal labor supply in the medium term (2 to 5 years after childbirth), probably (at least partly) offsetting the negative short-run effects. The overall effect on the cumulative maternal labor supply and earnings after childbirth is thus likely to be small.[15] At the same time, because the reform provides universal leave benefits with high income replacement, I expect families' cumulative incomes to increase in response to the reform.

3 Empirical approach

As previously explained, I estimate the causal impact of the paid leave reform on fertility decisions by exploiting the resulting differential changes in benefits across socioeconomic groups. Below, I will first describe the baseline differences-in-differences (DID) empirical strategy that I use to estimate the reduced-form effect of the policy. Under the assumption that the reform in paid parental leave affected fertility only through increasing financial incentives, I can then use the reform to estimate the effects of changes in financial incentives on fertility in a second step.

3.1 Baseline estimation

Perhaps the most straightforward way to evaluate the reform effects on fertility is to use a simple two-group DID strategy to compare the changes in average fertility between cohorts of higher-earning versus lower-earning women (see Figure 2). Figure 2A thus compares the 2004-2012 raw birth probabilities of women with earnings above 5,850 (8,550) EUR net (gross) earnings (equivalent to the 35th percentile of the earnings distribution) with a control group of women with earnings below. Whereas before the 2007 reform, the graphs move almost in parallel, after it, the probability of giving birth increases sharply for women above the earnings threshold but stays roughly constant for women in the control group. Likewise, as shown in Figure 2B, maternity leave benefits increase between 2006 and 2007 by over 5,000 EUR, on average, for women above the

[15] Kluve and Schmitz (2018) are unable to provide estimates for the reform effect on maternal earnings, since the Microcensus they use does not contain precise earnings measures. Using the full population pension registry data and applying a similar RDD-DID estimation strategy comparing women who give birth January 2007 and December 2006 (with January 2006 and December 2005 mothers as the comparison group), I estimate the average reform effect on total cumulative labor earnings in the two years following childbirth to be small and statistical insignificant. The structure of my data does not allow me to look at earnings effect beyond the two years postpartum.

threshold but remain unchanged, on average, for women below. These figures alone offer initial support for a stronger reform effect on the fertility of higher-earning women than on that of lower-earning women.

I formally test this assumption across the pre- (2004-2006) and post-reform (2008-2012) periods using a linear probability model that assesses the relative increase in the birth probability for women with earnings above the earnings threshold (treatment group) relative to those with earnings below (control group):

$$P(Child)_{it} = \alpha_0 + \alpha_1 Treat_{it} + \alpha_2 Treat_{it} * R_t + X'_{it}\alpha_3 + \gamma_t + u_{it}, \tag{1}$$

where $P(Child)_{it}$ is the probability of having a child for woman i in calendar year t, $Treat_{it}$ is a dummy variable equal to 1 if woman i earned above 5,850 EUR net in t-1, R_t is a post-reform dummy, and $Treat_{it} * R_t$ is the interaction between the two variables. X'_{it} is a vector of observed women's characteristics, such as the woman's age dummies, education group, indicator for being in vocational training, German nationality, and region (Länder) dummies, as well as indicators for whether the woman had a child in t-2 or t-1; while γ_t denotes year dummies. To ensure flexibility of the age effects in the covariate vector X'_{it}, the model includes age dummies and differential age dummies for tertiary-educated women. The coefficient of interest, α_2, captures the average causal impact of the maternity leave benefit reform on fertility in a given year within the five year post-reform period for higher-earning women versus lower-earning women.

As an alternative, because average earnings differ across education groups, I exploit the variation in reform impact across three different levels of the woman's education: (i) no more than secondary schooling (low-skilled), (ii) completed vocational training (medium-skilled), and (iii) tertiary education (high-skilled). Figure 3A plots the unconditional birth probabilities across these three groups for 2002-2011 derived from German Microcensus data on economically active women, which allow the inclusion of more years before the reform than appear in the main data set (see Section 4.3). As in Figure 2A, across-group birth probabilities move almost in parallel before 2007, while post-reform, birth rates for highly educated women increase more strongly than for those with less education. Figure 3B, in contrast, shows that between 2006 and 2007, economically active high-skilled women enjoyed the largest increase in available maternity leave benefits, 7,136 EUR, compared with around 3,527 EUR for medium-skilled and only 255 EUR for low-skilled women.

To exploit these differences, I estimate the following linear probability model:

$$P(Child)_{it} = \beta_0 + \beta_1 Deducmed_{it} * R_t + \beta_2 Deduchigh_{it} * R_t + X'_{it}\beta_3 + \gamma_t + u_{it}, \tag{2}$$

where $Deducmed_{it}$ and $Deduchigh_{it}$ denote dummy variables equal to 1 if the woman is medium or highly educated, respectively. The coefficients of interest, β_1 and β_2, capture the average effect of the reform on fertility for medium- and high-skilled women relative to low-skilled women. The identifying assumption of the DID approach in equations (1) and (2) is that trends in fertility would have been the same for treatment and control groups in the absence of the reform. Likewise, the graphical evidence for both the earnings and education groups suggests common trends pre-reform but no occurrence of a strong, permanent change in fertility behavior across treatment and control groups until after reform implementation. I further assess the validity of this common time-trend assumption by running placebo reforms on the pre-reform period data, pretending that the reform occured in the pre-reform period. I then test my results' robustness by omitting my set of controls X. Deriving similar coefficients from estimates with and without controls would indicate that the sample composition does not corroborate the estimated reform effects, which would point to reform exogeneity with respect to the observed (and potentially unobserved) characteristics of women.

The DID parameters α_2 of equation (1) and β_1 and β_2 of equation (2) are intention-to-treat effects (ITT), measuring the reform's reduced-form impact on higher-earning and higher-skilled women. A major advantage of these ITT effects is that they capture the full impact of the reform on fertility decisions through increases in maternity leave payments as well as potential endogenous adjustments in labor supply after childbirth, which can both affect available household income after birth. Moreover, because different earnings and education groups enjoy differential increases in postpartum financial incentives, these reduced-form fertility effects can be scaled using variation in the intensity of the maternity leave benefit changes.

3.2 Effects of changes in maternity leave benefits on fertility

Although the simplest method for measuring fertility's response to paid leave changes is to divide the DID estimate by the reform-induced benefit differential, using a simple Wald estimator to compare the differential treatment of relatively broad groups does not fully leverage the variation in benefit intensity. I therefore estimate a regression model that quantifies the effects of benefit changes on fertility and allows me to calculate benefit elasticities. Let B_{it} denote the real (in 2010 EUR) maternity leave benefits in calendar year t a woman can expect to receive. Accounting for B_{it} and for a flexible function of real lagged net earnings, $\Phi(E_{it-1})$, I estimate the following regression:

$$P(Child)_{it} = \delta_0 + \delta_1 B_{it} + X_{it}'\delta_2 + \Phi(E_{it-1}) + \gamma_t + u_{it}, \tag{3}$$

where $P(Child)_{it}$ is the birth probability for woman i in calendar year t. I exploit the fact that the expected maternity leave benefits B_{it} vary considerably over time for women with the same earnings because of the policy reform. Post-reform, benefits B_{it} are a deterministic function of pre-birth net earnings in the preceding year, $B_{it}(E_{it-1})$, whereas prior to 2007, B_{it} are means-tested fixed rate transfers that do not vary in any systematic way with women's pre-birth earnings. Hence, I simulate expected benefits in the pre-reform period using 2006 Microcensus data (as detailed in Section 4.3), but calculate the post-reform benefits as a function of a woman's net labor earnings in t-1, the preceding calendar year.

In order to identify the effect of reform-induced benefit changes on fertility, I account for a flexible function of lagged net earnings in order to ensure that the variation in B_{it} comes from the reform-induced variation in benefits over time and not from the variation in net earnings levels.[16] Were I to restrict my analysis to the post-reform period, all the cross-sectional variation in benefits would be captured by the flexible earnings controls, preventing me from separately identifying the treatment effect δ_1. To identify this effect, I need both pre- and post-reform data on fertility decisions of observationally equivalent women, who experience changes in the benefit schedule over time. If the time invariant $\Phi(E_{it-1})$ is fully flexible, however, only the reform-induced cross-cohort variation in B_{it} identifies the treatment effect δ_1; that is, the incentive effect of an increase in the total maternity benefit entitlement (based on earnings in the previous calendar year *t-1* post-reform) on a woman's probability of giving birth in calendar year t.

Although women are not required to take the maximum maternity benefit to which they are entitled (e.g., some may want to return to work before the 12th month postpartum), most mothers do, in fact, take advantage of the maximum paid leave (see Section 2.1). Under this condition the "benefit estimator" in equation (3) measures the impact of a change in financial incentives on the fertility decision. This causal interpretation of the parameter hinges on an additional assumption not needed for the identification of the intention-to-treat effects. That is, the reform affects the costs of childbearing, and in turn impacts fertility decisions, only through increased potential leave transfers after birth.[17] Because receiving maternity leave benefits is conditional on not working, some women might change their labor supply because of the higher leave benefits. If the reform's impact on maternal labor supply and maternal (or family) earnings after childbirth is negligible, the higher leave benefits will fully translate into an increase in available family income.[18]

[16] I use a fifth-order polynomial in lagged net earnings in my baseline specification.

[17] Using language from the instrumental variable literature, the exclusion restriction has to be met. The reduced-form changes in fertility across earnings or education group are solely caused by changes in the potential paid leave benefits, the first stage.

[18] Note that even if the increase in transfers is not equivalent to an increase in available income after

A similar empirical approach is adopted by Dahl and Lochner (2012), who exploit large changes in the earned income tax credit to estimate the impact of family income on child achievement, and by Gruber and Saez (2002), who use tax reforms to estimate the elasticity of taxable income.[19]

The central identifying assumption of this approach, which is equivalent to the DID assumptions above, is that the relationship between shocks affecting fertility decisions and women's net labor earnings remains stable over time. This assumption would be violated if, in the absence of the reform, differential trends in the fertility decisions across different earnings groups or changes in the composition of earnings groups over time existed that would change the relationship between u_{it} and lagged labor earnings E_{it-1} (and hence benefits B_{it}).[20] In this approach, the polynomial $\Phi(E_{it-1})$ can be thought of as a (time-invariant) control function, which in my case must be flexible enough to capture the true relationship between women's earnings and fertility shocks, and which is assumed not to vary with time over my observation period. Although I test the robustness of my approach in Section 5.3, the concern may remain that the reform has motivated women already planning to have a child to increase their labor supply pre-birth in order to raise their benefit entitlement.[21] If so, an endogenous adjustment in earnings in response to the reform would change the relationship between earnings and fertility over time and render women with the same labor earnings incomparable over time, thereby invalidating my identifying assumption. Whereas the ideal solution to this problem would be to use each woman's pre-reform earnings, the data set does not provide such information (see 4.2). Instead, in the appendix, I report several checks that address the concern of endogenous adjustment in the labor supply of mothers-to-be as well as women of childbearing age as a response to the reform.

To account for the fact that a woman's earnings in the preceding year – and hence, potential benefits as a function of earnings – are potentially endogenous to the reform, I also test for robustness by using a grouping IV estimator to instrument the expected benefits (cf. Blundell *et al.* (1998)). Using education-year interactions as my grouping instrument, I exploit changes in fertility across education groups who were differen-

childbirth, I still identify the impact of increasing conditonal leave benefits on fertility, which captures the optimal adjustment of labor supply. This is still a policy-relevant parameter, but it is harder to interpret.

[19]Nielsen *et al.* (2010) and Rothstein and Rouse (2011) use a similar approach to study the effect of student aid reforms on student outcomes. My estimation strategy is also related to grouping estimators applied to estimate the labor supply effect of tax reforms (see e.g. Feldstein (1995) and Blundell *et al.* (1998)).

[20]Equivalently, I need to assume that the composition of the treated groups in the standard DID approach in equation (1) and (2) does not change in response to the reform.

[21]The problem of potential anticipation of treatment effects, which would change the composition of treatment and control groups over time, was first identified by Abbring and Van Den Berg (2003) in the case of evaluation studies when decision processes are dynamic.

tially affected by the benefit reform. The DID estimator in equation (2) can be seen as the "Reduced-form" of this IV estimator.[22] The exogeneity restriction here requires that, conditional on controls, education must not affect fertility trends over time in the absence of the reform (the common time-trend assumption discussed in 3.1). It also implies that the composition of the education groups (with respect to unobserved differences in fertility) remains stable before and after the policy reform. This assumption would be violated if economic shocks or preference shifts that affected education groups differently resulted in differential fertility time trends across education groups. Although the 2007-2009 financial crisis would seem an obvious possibility, in my case, its threat is limited because, thanks to a strong upward economic trend beginning in the mid-2000s, the German economy suffered no lasting impact.[23] The reform is also unlikely to have affected the educational attainment of the cohorts studied because they had made most educational decisions before the policy switch and there were no major educational reforms over the time span studied. Hence, endogenous switching of education groups in order to increase benefits is unlikely to pose a threat to identification.

I estimate the empirical models on a restricted sample of women over 20 and under 45[24] and exclude 2007 data from the main empirical analysis because, with the reform legislation having passed in Fall 2006, only individuals giving birth from Summer 2007 onward could have adjusted their fertility behavior in response.

4 Data

My analysis draws on three different data sources: German vital statistics, the German Microcensus, and administrative records for insured persons from the German Pension Registry (FDZ-RV (2014)).

4.1 Vital Statistics

To study the time trends in fertility and test for a discontinuous jump in the number of births 9 months after announcement of the reform, I use microdata from the German Statistical Office's (GSO) vital statistics on all births in a given month for 2000-2011. I supplement these data with aggregate GSO information on the female population by age to construct monthly birth rates.

[22]Moffitt and Wilhelm (2000) and Blundell *et al.* (1998) discuss the equivalence between DID estimators and (grouping)-IV estimators.

[23]Unemployment rates fell between 2006 and 2011 across all education groups. For a more detailed account on the evolution of employment rates see Weber and Weber (2013).

[24]Childbearing by women age 20 and under is relatively scarce; around 3% of all births in 2007 were to mothers below the age of 21. Childbearing above 44 is extremely rare as 99.8% of births are to women age 44 or younger.

4.2 Pension Registry Data

The main analysis uses administrative data on actively insured persons compiled by the German Federal Pension Insurance's Research Data Center (FDZ-RV). This pension registry covers all those who made any contribution in the reporting year to the statutory pension insurance, which is mandatory for all employed persons in the private and public sectors, including those who are marginally employed. Not included in the data are the economically inactive, civil servants (including teachers), and most self-employed, none of whom are covered by statutory pension insurance.[25] Because women receive an automatic pension contribution for child-rearing years, the social security data provide a fertility record for any woman who has ever been registered with the pension insurance, which accounts for 91% of all births recorded in the vital statistics in 2007.

For my main results, I draw on the scientific use file of the actively insured persons database for 2004-2012 (FDZ-RV (2014)), which represents 1% of the full population of insured persons and contains annual information on over 80,000 women aged 21-44. These cross-sectional data include information on fertility and employment on December 31st in the reporting and two preceding years, as well as a woman's educational level and yearly gross labor earnings for the reporting and previous year. To avoid changes in the sample composition stemming from benefit reforms earlier in the 2000s, I restrict my sample to all women aged 21-44 who had positive labor earnings in the year preceding the survey, thereby excluding women who lived solely on unemployment benefits during that time.[26]

I calculate the expected post-reform maternity benefits using a detailed maternity benefit calculator (http://www.familien-wegweiser.de/Elterngeldrechner), which generates the expected maximum benefits as the sum of monthly benefits over the total 12-month entitlement period as well as a woman's net earnings based on information on a woman's gross earning reported in the pension registry data.

4.3 Microcensus

To simulate the pre-reform benefits, assess pre-reform trends, and estimate the DID results by education, I employ a 70% subsample (the Scientific Use File (SUF)) of

[25] Kohls (2010) reports that data on insured persons for ages 20-59 covers 84.5% of German women and 86.1% of German men and 67.2% of non-German women and 75% of non-German men in the 2006 population estimation.

[26] Around 8% of economically active women were solely receiving unemployment benefits. As outlined above, the Pension Registry data contains information on economically active women (employed women as well as unemployed women), but only contains information on inactive women (women not participating in the labor market) if they are giving birth in that year. Hence I can not not determine the probability of birth for inactive women.

2003-2012 data from the German Microcensus, an annual cross-sectional survey of a random 1% sample of the German population.[27] This survey gathers household demographics on around 70,000 women aged 21 to 44 each year, including number and ages of children in the household, marital status, education and vocational training, labor market participation, and receipt of various benefits. Nevertheless, because the survey is administered continuously over the year, I can not use the information on births in the survey year to determine the probability of giving birth in that year. Rather, I retrospectively derive this probability to give birth for the preceding year from information on children's years of birth.[28] Since the latest year of data available is 2012, I derive births only up until 2011.

Whereas the representativeness of this data set (whole population, including the economically inactive, civil servants, and the self-employed) is one of its major advantages,[29] the data unfortunately only contains very broad income measures for the survey year. Nonetheless, for every household member, it does provide a measure of net income, reported in intervals, for the month preceding the survey date. Because expected pre-reform maternity benefits in essence depend solely on a partner's income, I can calculate them for each woman by applying the pre-reform benefit eligibility rules to her spouse's net income and aggregating this information by income or education-age group (from the 2006 Microcensus data) for all women aged 21-45. I then merge the averages by a woman's observable characteristics with the pension registry data, implicitly assuming that expectations of benefit level are formed on the basis of the partner's current income.[30] For my baseline results (Section 5.3), I merge pre-reform benefits by women's income groups and define the expected benefits as the sum of monthly payments over the maximum 24-month entitlement period, with the assumption that mothers maximize their total benefit entitlement (see Appendix A for simulation details for both pre- and post-reform benefits). Alternatively, I also define pre-reform benefits over the shorter 12-month entitlement option, which is equivalent to the duration of post-reform benefits.

In Table 3, I provide descriptive statistics for the estimation sample, which are based on information from both data sets. The average maternity leave benefits for which women are eligible are less than 5,000 EUR pre-reform but increase to 8,280 EUR

[27]The data used in this paper was analyzed using the remote processing tool JoSuA. JoSuA was developed by the IDSC of IZA. See Askitas (2008) for details.

[28]As Brewer et al. (2010) point out, birth probabilities estimated by this approach are potentially subject to measurement error due to infant mortality and household reconstitution, but low rates of mortality and the fact that the overwhelming majority of children stay with their natural mother in the event of family breakup reduce the effect of these factors in practice.

[29]According to my own calculations based on the 2010 Microcensus, around 8% of all (11% of working) women between age 20-44 were self-employed or civil servants.

[30]I also implicitly assume that assortative matching of partners is the same over time.

post-reform (Table 3, Panel A). Around 91% of the women in the sample are German nationals, with an average age of 33.37 years – 30.67 for new mothers – and a probability of giving birth in a given year of 4.1%. The majority have vocational training (62%), around 14% are tertiary educated, and 24% have no postsecondary degree.[31] Median earnings in the preceding year are 10,114 EUR net (14,874 EUR gross), with a women in the 90th percentile of the earnings distribution taking home around 21,540 EUR net (36,091 EUR gross). According to Table 3, Panel B, 27% of the women are mothers to one child and 24% have two children; however, more than 40% of women aged 21-44 are childless. Given the large share of potential first-time mothers, if my analysis is to truly capture the full effect of the reform, it must consider all births, including first births, rather than focusing on higher-order births only.

5 Results

To estimate the reform's impact on fertility decisions, I first derive DID estimates of its reduced-form effects and then turn to the benefit analysis that exploits the variation in financial incentives across earning groups. In addition to the results using education groups as an instrument, I also report an estimate of the benefit elasticity. Before turning to my main regression analysis, however, I perform a time-series analysis similar to that reported by González (2013) and present evidence for the immediate adjustment of fertility in response to the implementation of the reform.

5.1 Descriptive evidence from time series

In Figure 4, I first plot the seasonality adjusted (residual) monthly number of births per 1000 women over the 2004-2011 period. I separately plot a Lowess smoother for the months before and after August 2007 (0-cutoff month in Figure 4, denoted by the solid vertical line), which is 9 months after the final passage of the policy change, when it was certain to come into effect. The dotted vertical line (-5) denotes March 2007, 9 months after the announcement of the policy change (in May/June 2006).[32] Up until the cutoff date, the monthly birth rate appears to be relatively stable, but it jumps

[31] Educational information is based on the Microcensus sample, which is representative of all women. As mentioned earlier, the pension registry data does not include the self-employed and civil servants. As a result, the share of highly skilled women in the pension registry data sample is lower (8%). Around 70% (22%) of women in the pension registry data are medium-skilled (low-skilled).

[32] Kluve and Schmitz (2018) show there was a peak in newspaper reports on the proposed policy as well as a first peak in the Google search index in May 2006, which indicates that the public became increasingly aware of the potential policy regime from May 2006. However, there was no certainty that the new policy would come into effect before the fall of 2006, when an amended version of the law was passed by parliament and the second chamber in October and November.

discontinuously in the cutoff month of August 2007 and continues to increase thereafter. I also test for a discontinuity in the monthly birth rate around August 2007 by running a regression using a third-order polynomial in the running variable (birth months over time) to capture smooth fertility trends and by using calendar month dummies to measure birth seasonality.[33] This test yields an estimate of about 0.17 (standard error 0.061). Evaluated against the average monthly birth rate of 4.87 births per 1,000 women prior to August 2007, the result suggests that births per 1000 women increased significantly in August 2007 by about 3.5% over the previous month. Extrapolating to the yearly level using around 673,000 live births for 2006 (Statistisches Bundesamt (2016a)), I find that the maternity leave reform resulted in close to 2.350 additional children born each year in the very short-run. Given the three to six months needed for conception even in a fertile couple (González (2013)), however, the discontinuity is capturing only the very immediate response in successful conceptions following passage of the law and is thus likely to understate the true immediate fertility response to the policy.[34]

5.2 Differences-in-differences results for earnings and education groups

In Table 4, I report DID estimates for the impact of the reform for the treatment group of women with higher earnings up to 5 years after the reform (equation (1)), expressing the dependent variable as births in 1,000 women for ease of interpretation. I address potential selection bias by estimating the effects both with and without the set of individual controls. As column (1) shows, over the 5-year post-reform period, the reform in paid maternity leave increases the birth probability in a given year by 6.4 births per 1,000 women. Evaluated against the corresponding pre-reform birth rate of 40 births per 1,000 women, this estimate implies that the fertility of women who benefitted from the reform increases by 16% in response to the reform. To place this estimate in perspective, Lalive and Zweimüller (2009) find that extending leave for a current (future) child increases short-run fertility, i.e,. births within three years post-reform, by 15% (21%), with the estimated future-child effect most similar in nature

[33] I estimate the specification $B_m = \alpha + \beta * post + \gamma_1 m + \gamma_2 m^2 + \gamma_3 m^3 + \sum_{i=2}^{12} \delta_i Dmonth_m + \varepsilon_m$, where B_m is the respective fertiliy rate in months m, $post$ takes value 1 starting in August 2007, and m is a running variable for months in the sample period (i.e., value of 1 for January 2004). $Dmonth_m$ denotes calendar month dummies. The regressions results are available upon request.

[34] Terminations of pregnancies in turn can immediately adjust to the policy. I find some suggestive evidence using quarterly numbers of abortions that the policy appears to have discouraged abortions for married women, with no apparent trend change for single women. I also find a strong increase in aggregate data on In Vitro Fertilisation (IVF) between 2006 and the post-reform period, which likely reflects the increase in willingness to conceive, particularly by older women. The results are available on request.

to the reform effect identified here. Milligan (2005) identifies fertility effects for child subsidies of a similar magnitude, 10% for the decision to have a first child, 13% for the second, and 25% for third or higher-parity children, where the last group experienced the largest child subsidy changes.

In Columns 2 and 3, I report additional robustness checks. The fact that my baseline estimate is robust both to excluding all individual-level control variables (column (2)) and to including a second-order polynomial in earnings as a finer measure of previous year earnings (column (3)), suggests that the estimate of the reform impact is not sensitive to the composition of either the treated or control group. In column (4) I apply an alternative definition of treatment and control group and compare the birth probabilites for women with above-median earnings of 10,098 (14,800) EUR net (gross) earnings with a control group of women earning below the median. The estimate indicates a reform-induced increase in the fertility of women above-median earnings in a given year by 7.2 births per 1,000 women (18%). The last column of Table 4 then reports the estimate of a placebo test in which I pretend that the reform had already occurred in 2006. Restricting the data to the (true) pre-reform years of 2004-2006, I redefine 2004-2005 as the pre-reform and 2006 as the post-reform period. Consistent with the graphical evidence of similar pre-reform trends for both treatment and comparison groups, which only diverge sharply post-reform (Figure 2), the estimated effect for the placebo reform is insignificant, and the point estimate is of much smaller magnitude than the true reform effect in column (1).

In Table 5, I next use the Microcensus waves 2003-2012 (births 2002-2011) to calculate alternative DID estimates of the reform for medium- and high-skilled women (the treatment groups) relative to low-skilled women (the comparison group) from equation (2) (for more details on the educational coding, see Appendix A). Since the Microcensus also contains information on the economically inactive, self-employed, and civil servants, the estimated DID effects measure the reform effects for the full population of women. According to these results (column (1)), the birth probability for high-skilled women increases by 8.66 births per 1,000 women in a given year post-reform, which, when evaluated against the pre-reform mean, constitutes a 15% increase in fertility. Omitting the individual-level controls (column (2)) and adding measures of family composition (column (3)) yields very similar results.

On the other hand, estimating the reform effects for all women, including the inactive yields only a small, statistically insignificant point estimate for medium-skilled women, which is hardly surprising given that the DID estimates capture the reduced-form (ITT) effect of the policy. The reform did not raise the maternity leave benefits amount for inactive women on average, and, as a result, the corresponding first-stage coefficient (i.e., the average change in benefits for education groups) is simply smaller when the inactive are included. Conversely, because active women benefit more strongly from the reform, restricting the sample to economically active women (column (4)) while excluding civil servants and the self-employed to match the pension registry sample (column (5)) results in a higher estimated reform effect for both education groups. In particular, when only active women are included, the reform raises the fertility of highly educated, active women by close to 23% and that of medium-educated women by 9%.

The longer timespan covered by the Microcensus data also allows me to perform placebo tests over a longer period, pretending that the reform happened in 2004. I thus redefine my pre- and post-reform periods as three years before (2001-2003) and three years after (2004-2006), respectively, and estimate placebo reform effects for the whole sample (columns (6)) and the sample of economically active women (column (7)). In line with the pre-reform trends depicted in Figure 3, the placebo estimates are smaller and insignificant, suggesting that education groups experience no differential time trends prior to the reform.

As a final step, I replicate the education group analysis using my main data, the sample of active women in the 2004-2012 pension registry data (Table A1). The point estimates of the baseline reform effects (column (1)) are smaller for the highly skilled covered by the pension registry data, whose fertility the reform increases by 13%, but similar for the medium-skilled (an 8% increase). The difference in point estimates between the two datasets is, however, not statistically significant. Omitting the individual-level controls (column (2)) and adding a polynomial in real earnings (column (3)) yields very similar results.

5.3 Results for benefit estimation

Before reporting my estimation results of equation (3), I construct a discretized version of the previously outlined continuous benefit estimator by discretizing the net earnings distribution into 10 intervals. Each of the first 9 intervals are of length 3,000 EUR, while the last contains all real net earnings beyond 27,000 EUR (gross earnings of ca. 47,000 EUR), which lies above the 95th percentile of the earnings distribution (25,131 EUR net). I then estimate a linear probability model that controls for these earnings-interval dummies and their interaction with a post-reform dummy, as well as controls from my baseline specification.[35] Figure 5A plots the coefficient estimates (and 95% confidence intervals) for the earnings interactions with the post-reform dummies and reveals a very different fertility evolution along the earnings distribution. Women in

[35] I estimate the following specification, $P(Child)_{it} = \eta_0 + \sum_{e=1}^{10} \theta_e (R_t * d_{eit}) + X'_{it}\eta_1 + \sum_{e=2}^{10} \gamma_e d_{eit} + u_{it}$, where d_{eit} is a dummy variable, $d_{eit} = 1\{e_j \leq E_i < e_{j+1}\}$, indicating that earnings of woman i lie within the ten earnings intervals ($E_i \in \{1, 2..., 10\}$) of length 3000 EUR and R_t is a post (2007)-reform indicator.

the lower earnings intervals of 3,000-6,000 EUR (midpoint 4,500) and 6,000-9,000 EUR (midpoint 7,500) are not any more likely to have a child post-reform, whereas those with earnings below 3,000 EUR, which corresponds to the bottom 10%, actually experience a statistically significant decline in fertility. At the same time, this latter group also faces a decrease in the total benefit amount they could receive by around 1,360 EUR on average as a result of the shorter duration of benefits post-reform (Figure 5B). For women who are close to and above the 50th percentile (median = 10,100 EUR, around 14,800 EUR gross) of the net earnings distribution, however, the probability of having a child in any of the 5 years post-reform significantly increased – by around 3-8 children in 1,000 women or 0.3-0.8 percentage points. Even women in the top earnings interval, who are above the 95th percentile, are statistically significantly more likely to give birth post-reform (6.4 births in 1,000 children). Figure 5B shows the corresponding increase in benefits in response to the reform. In fact, the relatively stable post-reform surge in childbearing for all earnings groups above the median (Figure 5A), despite benefits steadily increasing in earnings (Figure 5B), implies that the effect of paid leave benefits on fertility may be stronger for the middle-upper part of the income distribution than for the top end.

Next, I estimate equation (3) and report results in Table 6. As previously described, my approach requires the inclusion of a flexible function of lagged net income. Before making the more conservative choice of a fifth-order polynomial, I explore different-ordered polynomials and find very similar estimates from order two and above. I define the benefit variable as the maximum sum of monthly maternity benefits that a woman could expect for giving birth, measured in 1,000 EUR (in year 2010 prices). The baseline result reported in column (1) implies that a 1,000 EUR increase in the total expected benefits raises the probability that a woman will give birth in each of the five post-reform years by 0.783 births per 1,000 women. In terms of the average pre-reform birth probability, this figure implies a rise in fertility in a given year of 2.1% per 1,000 EUR in total benefits. This estimate is similar to corresponding estimates in the existing literature. Milligan (2005) finds that a 1,000 CAD (around 690 EUR) increase in the total sum of 5-year benefits raises fertility on average by 2.6%. Lalive and Zweimüller (2009) report a future-child effect of 21% within three years (or 7% points in each year), which can be scaled by the increase in maximum maternity leave benefits during the second year of a child's life of 4,080 EUR. This estimate implies a 5% increase in fertility over the three-year period (or a 1.7% increase in a given year) per 1,000 EUR increase in benefits.[36] The opportunity for comparison with the existing studies

[36] Lalive and Zweimüller (2009) report on p. 1366 that extending leave for the future child increased fertility by 21%. I combine this number with the maximum amount of additional paid leave for the second year of a child's life (340 EUR a month*12).

on the German reform, Kluve and Schmitz (2018) and Cygan-Rehm (2016), is limited. Both papers identify the current-child effect, which measures the change in costs of the current child already born in combination with a change in the speed premium for the future child on mothers' subsequent fertility and birth timing decisions. While the results by Kluve and Schmitz (2018) suggest a small but statistically significant drop in overall subsequent fertility between three months and five years after the first birth by 5%, driven by younger and low-income mothers, the effect for older mothers appears to be positive and statistically significant. Cygan-Rehm (2016) finds merely spacing effects in the first three years and no significant overall effect on subsequent childbearing five years after the last birth, except for very low-income mothers who the reform does not benefit. Her results across subgroups are suggestive of heterogeneous patterns on subsequent childbearing roughly in line with my finding (i.e., negative effects on low-income women, positive effects for middle-income women, but surprisingly small and insignificant effects for high-income women). Estimates from the structural analysis on the short-run effects of paid leave benefits by Stichnoth (2018) confirm my quasi-experimental results; he finds a 4% increase in overall births in the short-run (similar to my estimate presented in 5.1) and simulates the reform effects to be highest on the top two quartiles of the income distribution.

In Columns 2-7, I test the robustness of my baseline result. The fact that my baseline estimate is robust to the omission of individual-level control variables (column (2)) suggests that reform implementation is exogenous with respect to observable characteristics. I next explore whether the fifth-order polynomial in lagged net earnings as my choice of baseline control function is flexible enough to account for the relationship between earnings and fertility. In column (3), I include only a second-order polynomial in net earnings, which already yields a similar estimated reform effect. In column (4), I address the concern that the effect of net earnings on fertility decisions might differ by women's characteristics by employing a more general form of the control function that is allowed to vary by a woman's education level. The estimated benefit effect, however, does not change. In column (5), I report results for a placebo reform (equivalent to the placebo test in Table 4, column (5)), assuming earnings-dependent benefits had already been introduced in 2006. Using the pre-reform data only, I test the differential changes in fertility in 2006 over the years 2004-2005. The estimated effect for this placebo reform is insignificant and the point estimate very close to zero.

So far, I have defined pre-reform benefits as the sum of benefits under the 24-month entitlement period option, which paid a maximum of 300 EUR monthly (7,200 EUR in total). However, the scheme also allowed women to opt for a shorter entitlement period (equivalent to the duration of benefits post-reform), with a maximum 450 EUR over 12 months (5,400 EUR in total). This option, which was primarily popular among East

German women, was particularly attractive for mothers wanting to return to work after 12 months. In column (6), I thus redefine the pre-reform benefits as the expected sum of benefits under the shorter 12-month option. In column (7), I test whether my results are robust to alternatively simulating and matching pre-reform benefits using women's education-age groups instead of women's earnings. Both redefinitions of pre-reform benefits have little effect on the estimated coefficient.

In the two subsections below, I demonstrate the robustness of the baseline results to (i) instrumenting the expected benefits with a grouping IV estimator and (ii) assuming different functional forms of the relationship between benefits and fertility.

IV results using variation across education groups: The estimates in Table 6 would be biased if women adjusted their earnings in response to the reform in order to increase the benefit amount they could receive. Using older women as a control group, I estimate DID specifications to test for potential labor supply adjustments of women giving birth in t (Table A2) as well as women of peak childbearing age (aged 21-34, Table A3) in response to the reform over the period 2004-2012. In Figure A1, I first provide graphical evidence that older women are a suitable control group. Trends in labor supply between younger women (aged 21-44) and older women (aged 45-55) are almost parallel over time, both at the extensive margin, as measured by the share of economically active women using data from the Microcensus (Part A, Figure A1), as well as on the intensive margin, as measured by the evolution of average gross yearly earnings in the preceding year using the AKVS-data (Part B, Figure A1). I do not find any evidence that new mothers differentially change their pre-birth earnings or working hours between the year they give birth t and the preceding year t-1 (Table A2, Panel A), nor that they are more likely to be active in the labor market (Panel B) in response to the reform compared to older women. Neither is there any evidence that women of childbearing age (see Table A3) are more likely to be working in response to the reform when compared to the older control group. Nevertheless, in column (8), I check the robustness of my estimates to instrumenting leave benefits with education-year interactions, exploiting the variation in education-year specific mean benefits for identification. Education groups explain much of the variation in benefits over time; both partial R2 of the education-year interactions of 0.111 and the first stage F-statistic of 4,859 are sizable. The IV point estimate of 1.096 is slightly larger but not statistically significantly different from the baseline estimate in column (1). This estimate implies that a 1,000 EUR increase in the total expected maternity benefits raises a woman's birth probability in each year over the five post-reform years by 1.096 births in 1,000 women, an increase of 2.9% relative to the pre-reform mean.

Robustness of benefit estimates to functional form: Table 7 presents several specifications exploring the robustness of the baseline estimates to the functional form chosen. First, specification A relates maternity benefits directly to the opportunity costs of birth, proxied by women's net earnings in t-1. Here, I define benefits in terms of their replacement ratio of net earnings, which ranges between 12% for high earners and 160% for very low earners before 2007, but lies above 67% for almost all women after the reform. The mean reform-induced increase in the replacement ratio is about 29 percentage points, with a standard deviation of 9.55 (estimated using the 2006 pension data). More specifically, I estimate that a 10 percentage point increase in the replacement ratio increases the probability of having a child by 1.37 births in 1,000 women, a 3.6% increase over the average pre-reform probability.

Because the effects of additional benefits may be stronger for lower-income women and thus decreasing with the benefit level, specification B uses the log of total expected benefits (together with a polynomial in log real net earnings) as an alternative explanatory variable rather than benefits in levels. Here, a 10% increase in benefits raises the probability to give birth by 0.71 births per 1,000 women, a 1.9% increase relative to the pre-reform mean. This estimate implies an average benefit elasticity of 0.19,[37] which conforms to the existing literature. For example, Cohen *et al.* (2013) and Milligan (2005) find benefit elasticities of child subsidies of 0.19 and 0.107, respectively, which are in the same range of estimates as those in time-series studies (see Milligan (2005), p. 551). Estimating a structural model for Germany, Haan and Wrohlich (2011) find a 10% increase in child benefits will raise fertility by 2.3% on average (elasticity of 0.23).

6 Heterogeneity by age and birth order

As previously emphasized, I can only estimate the medium-run impact of financial incentives on current rather than permanent fertility because women may change the timing rather than the total number of children born during their fertile years. The reform was perceived as a permanent change affecting childbearing costs and may have resulted in delayed reactions, particularly in younger women, which I might not fully capture. The fact that Figures 2 and 3 show a clear upward trend in the fertility behavior of higher-earning and highly educated women 5 years post-reform, however, suggests that the reform has had a lasting positive impact on the fertility behavior of affected groups. In order to assess whether the reform was able to affect permanent fertility, I explore fertility effects across different age groups, including women closer to the end of their childbearing years. Older women are of particular interest, as their remaining time to conceive is very limited and the probability of conception decreases

[37] Alternatively, I can use the DID estimates above to calculate benefit elasticities by relating the %-change in fertility to the %-change in benefits. I calculate the benefit elasticity to be around 0.15 for higher-earning and high-skilled women.

over their remaining fertile years. I expect older women women to not postpone an additional birth for too long and to adjust their fertility relatively quickly when faced with lower costs for an additional birth under the new maternity leave system. In Table 8, I report estimates for separate regressions of equation (3) for the five age categories. The reform does indeed appear to have affected fertility across all age groups (columns (1)-(5)), even though the estimated reform effects for some groups fall short of the 10% significance level. Even younger women aged 25-29 (column (2)) increased their fertility in response to the higher maternity benefits offered. Estimates from a fully interacted specification do not allow me to reject equality of the estimated reform effect across age groups other than for the oldest women, aged 40-44, for whom the effect is statistically significantly different compared to women aged 25-29 and 35-39 (columns (2) and (4)). The reform effect for the 40-44 age group (column (5)) is large in terms of the underlying probability: an additional 1,000 EUR of maternity leave benefits increases a women's probability of having a child in a given year by nearly 5%. The results for this and the 35-39 age group, both nearing the end of their fertility cycle, suggest that the increased financial incentives have had a permanent effect on fertility and will increase the completed fertility of these cohorts. My results are in line with the descriptive evidence by Bujard and Passet (2013), which suggests that the reform positively affected recuperation of births by highly educated women aged 35-44.

A number of recent studies (e.g., Laroque and Salanié (2014) and Brewer *et al.* (2011)) found different fertility responses for first births (extensive margin) than for second or higher-order births (intensive margin). The detailed information on the number and age of children provided by the Microcensus allows me to test whether the reform does indeed affect the fertility of highly educated women at different parities. Estimating a DID specification that allows for differential effects of the reform by birth order, I find that for highly educated women across all age groups, the reform has strongly impacted the decisions to have both a first and a second child (Table A4, Panel A). Of particular interest in terms of completed fertility are the reform effects across parities for women aged 40-44. The increase in permanent fertility established previously (Table 8, column (5)) is driven both by the decision to opt for a first child, but more strongly by the decision to have a(n additional) second child as a response to the reform (Table A4, Panel B). Across education groups, the gap in completed fertility at the intensive margin is large. In 2012, the majority (53.4%) of highly educated women aged 45-49 had at most one child, a 15 percentage point higher share than for women without any postsecondary education (BIB (2012)). My findings suggest that the substantial gaps both in childlessness, as well as in births beyond the first child, are likely to narrow as a result of the reform.

7 Conclusion

In this paper, I assess the ability of Germany's introduction of earnings-related paid maternity leave to increase fertility, especially for higher-earning women with high opportunity costs of childbearing. By taking advantage of the large differential changes in maternity leave benefits across education and income groups, I am able to use a DID approach to identify the direct fertility effect of the changes in paid leave for a child yet to be born. My results not only indicate that a 1,000 EUR increase in total leave entitlements raises the average birth probability by 2.1%, but also that higher benefits to higher-earning women – up to 21,000 EUR as an exogenous source of variation in entitlement – have actually changed the socioeconomic structure of fertility. Especially noteworthy is the fact that under the new system, highly educated women are more likely to have a first as well as a(n additional) second child at the end of their fertility cycle, signaling a change in their permanent fertility at both the extensive as well as the intensive margin.

Providing causal evidence on how the German policy change, perceived widely as one of the most significant reforms in German family policies in the last decades, has affected fertility patterns has important policy implications for all countries facing low fertility rates, which are a risk to the long-term sustainability of public pension systems. My findings suggest that the policy has successfully raised fertility, particularly for women with higher opportunity costs of childbearing who were given very low compensation for time out of the labor market after giving brth under the previous flat benefit scheme. Such leave therefore appears to be a more effective policy than traditional schemes like child subsidies or cash benefits for incentivizing fertility in women with higher opportunity costs. The findings thus imply that paid earnings-related maternity leave could help mitigate the general fertility declines associated with women's increasing educational attainment and labor market participation, thereby narrowing the existing baby gap between education and earnings groups.

The fact that my quasi-experimental approach identifies only reduced-form fertility responses up to 5 years post-reform raises interesting possibilities for future work. In the long run, younger women are likely to internalize lower expected opportunity costs of childbearing because of higher earnings replacement of leave payments, and they might respond by increasing their investment in human capital (see Adda *et al.* (2017)). Furthermore, changing the socioeconomic composition of fertility has potentially important distributional implications for future generations. If the introduction of earnings-related maternity leave improves education and labor market outcomes of future generations (and with that their taxable income), then the case for maternity leave programs is even stronger.

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Appendix

Appendix A: Benefit simulation and data coding

Benefit calculation

Pre-reform benefits: Prior to the reform, maternity leave payouts were means-tested on household income during benefit receipt. I thus simulate a woman's pre-reform entitlement if she gave birth to a child using 2006 Microcensus data on the current net income for the woman and her spouse, defined as either a husband or cohabitating partner. I restrict the sample to all women aged 20-44 in the survey year and approximate net yearly income based on the net monthly income variable, which is provided in 24 intervals. The pre-reform eligibility thresholds were also based on net household income during benefit receipt, excluding income from public transfers. The income variable reported in the Microcensus includes social assistance and unemployment benefits, which I set to zero for individuals designated unemployed or inactive during the survey period. Because the number of very high earners is very small, I pool all observations for net income above 43,200 EUR. I simulate the woman's maximum potential benefits based her spouse's generated net labor income, under the assumption that she has no labor income during benefit receipt. In calculating the potential entitlement, I apply the benefit eligibility rules to the spouse's current income, assigning the maximum benefit of 7,200 EUR (5,400 EUR for option 2) to women whose spouse earns under 16,800 EUR. If the spouse's net income falls between 16,800 and 22,200 EUR, the benefits range from a minimum of 1,980 EUR to a maximum of 7,200 EUR (2,700 EUR and 5,400 EUR for the short option), so I set the potential benefits equal to the midpoint of this benefit interval. Women whose spouse's net income lies between 22,200 EUR and 29,400 EUR would only have been eligible for a total benefit payment of 1,800 EUR over six months (for both options), while women with spousal net income above 29,400 EUR would have been ineligible for any maternity benefits. For women with no partner in the household, family income during benefit receipt would lie below the income threshold, so I assign them the maximum benefit. Because some eligibility cutoffs fall into an income interval, I calculate the mean simulated benefit by applying the lower and upper brackets of the income interval, respectively.

I derive my baseline results by collapsing the simulated pre-reform benefits (based on spousal income) by the respective earnings intervals of women aged 25-44. I then merge these simulated benefits with the pension data used in the main analysis. As an alternative, I simulate the pre-birth earnings for five age-group-specific education groups, which generates 15 distinct values for the simulated pre-reform benefits. I then merge these values with the pension data by age and education.

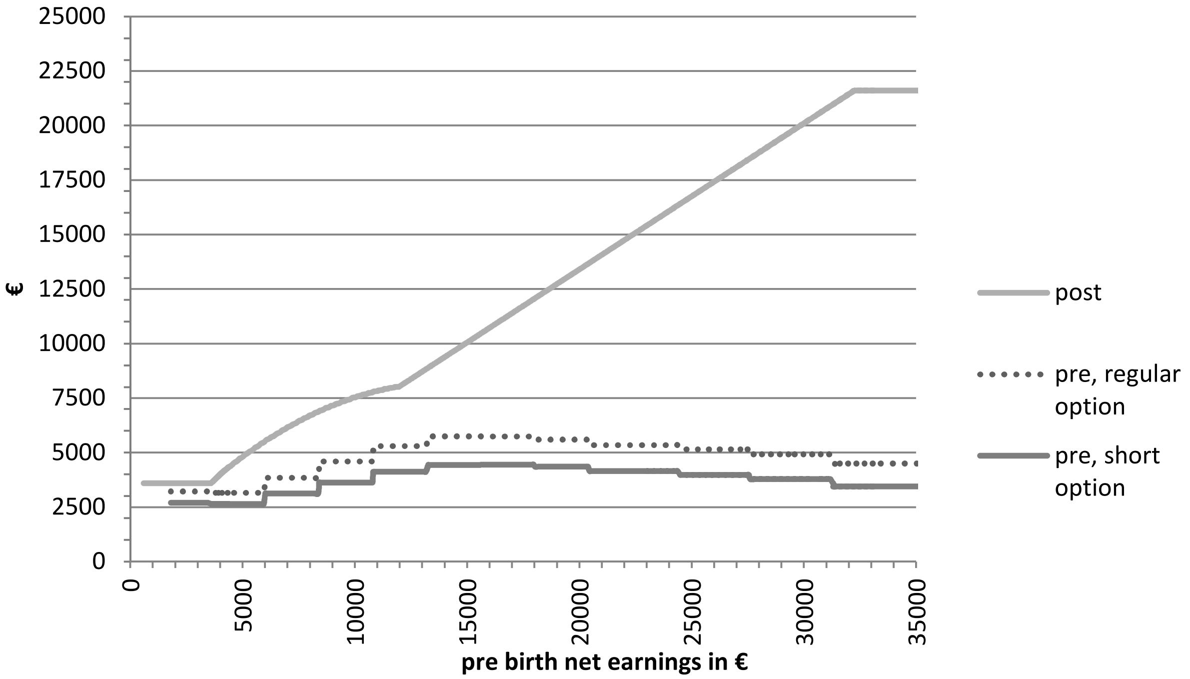
Post-reform benefits and net earnings: Because the post-reform benefits are a direct function of women's labor earnings (independent of spousal earnings), I use a detailed maternity benefit calculator (http://www.familien-wegweiser.de/Elterngeldrechner) to generate the expected maximum benefit as the sum of monthly benefits over the total 12-month entitlement period based on women's gross yearly earnings (taken from the pension registry). Given the different tax class choices allowed couples under German joint taxation rules, I assume women to be taxed under tax class IV (equivalent to tax class I for singles), the commonest option for couples with relatively equal earnings. Calculating these benefits using alternative tax classes has little effect on my estimates. For simplicity, I further assume that the women have no children, thereby eliminating any increase in calculated benefits by a small sibling premium. I calculate the net labor earnings for my estimation strategy using an implicit tax rate for gross yearly labor earnings, which is provided by the benefit calculator.

Education coding

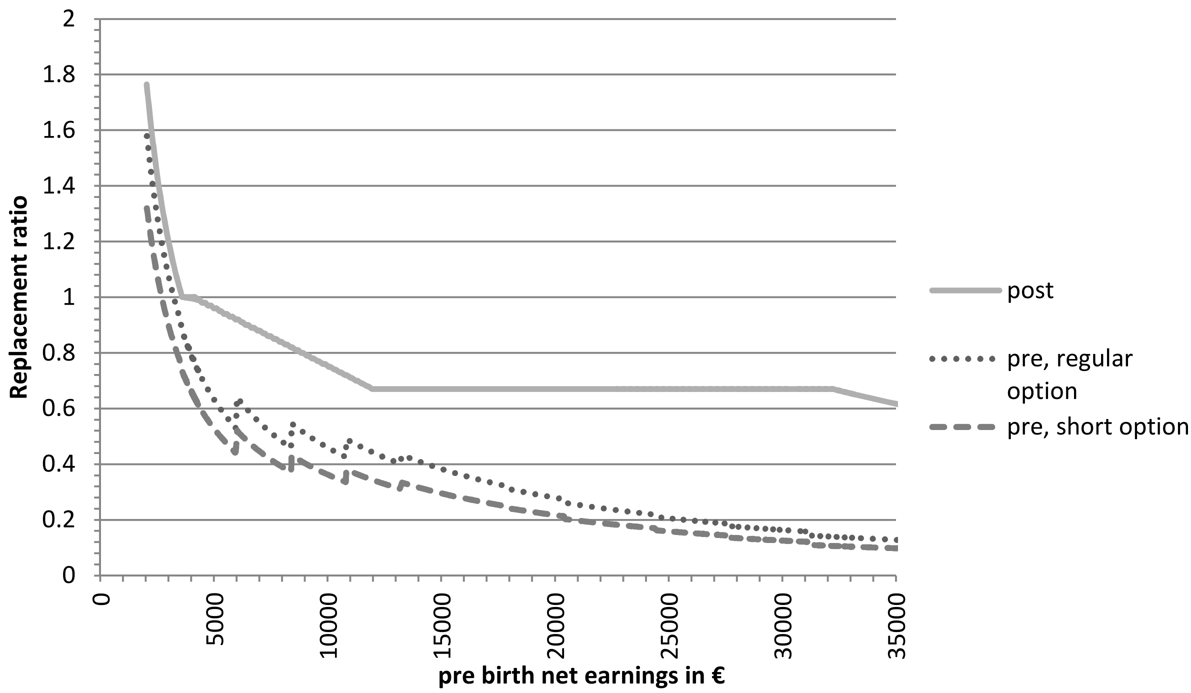
The education groups, defined based on the German educational system, differentiate between less-educated women without any postsecondary education (equivalent to category 1 and 2 of ISCED97), medium-educated women who have completed an apprenticeship (equivalent to 3, 4, and 5b of ISCED97), and highly educated women with tertiary education (categories 5a and 6 of ISCED97). In a first step, I impute missing education in the pension registry data by replacing the unknown value with the modal value for education in the woman's 3-digit occupational category. Furthermore, the modal education level of about 7% of the sample in the pension registry data is "unknown." Because these cases typically occur in low-skilled occupations, often for part-time marginal jobs for which employers specify no education details, I include them in the low education group.

Figure 1: Parental leave benefits in Germany - pre vs. post 2007 reform simulation for maximum amount of subsidies

Part A: Total benefits in EUR



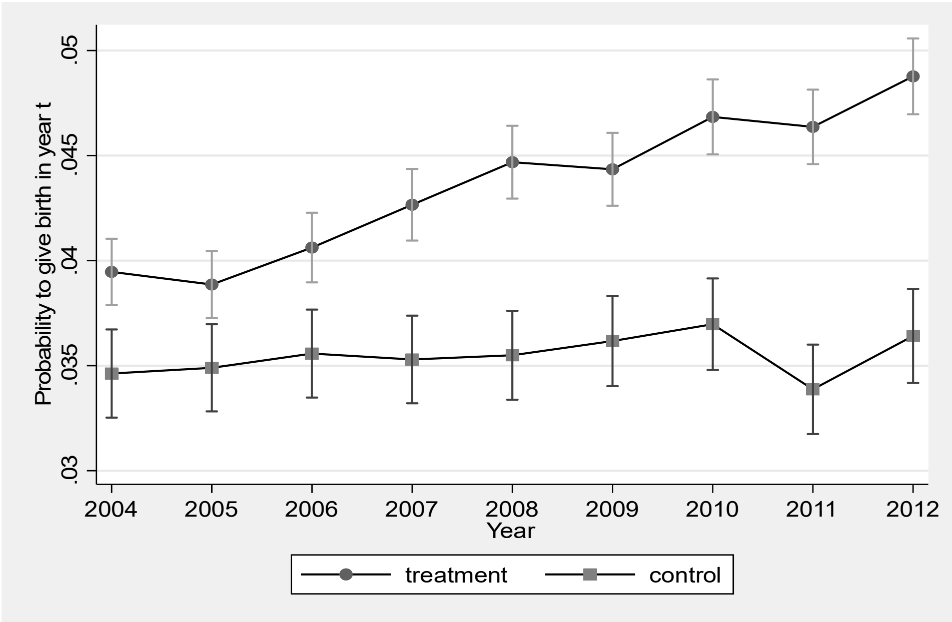
Part B: Replacement ratio of benefits in % of net earnings



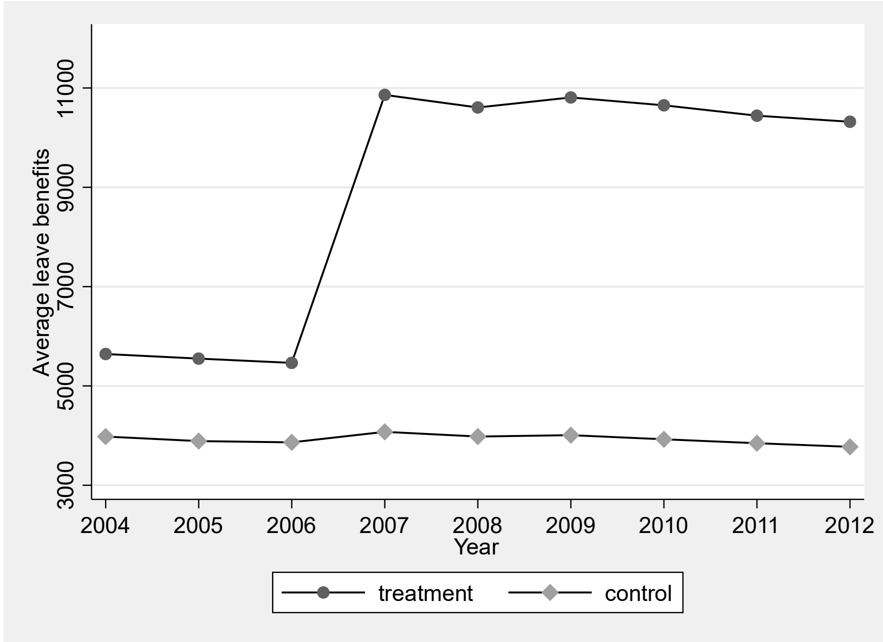
Notes: The graph in Part A shows maximum sum of benefits a woman would be entitled to against her yearly net earnings (in EUR), pre- and post-reform. In Part B the sum of benefits is expressed as the replacement ratio of net yearly earnings. The post benefits are calculated via a benefit calculator for gross earnings. The pre-reform benefits for the 24-months option and the shorter 12-month-option are simulated for discrete earnings parentheses using the Microcensus 2006 (See Appendix A).

Figure 2: Evolution of birth probabilities and benefits for women with high vs. low earnings

Part A: Evolution of birth probabilities



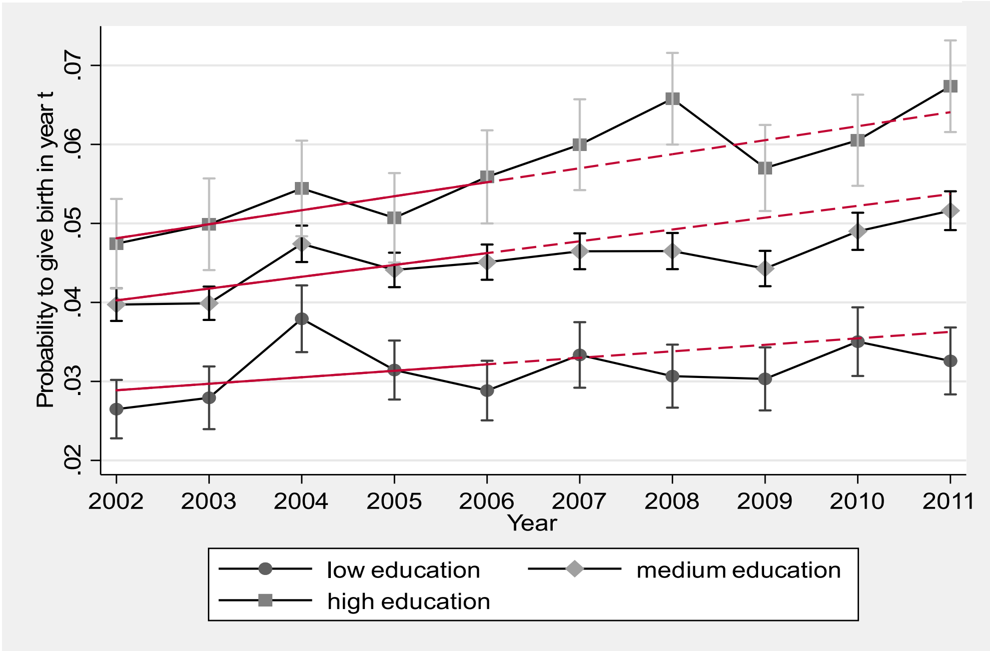
Part B: Increase in benefits post-reform



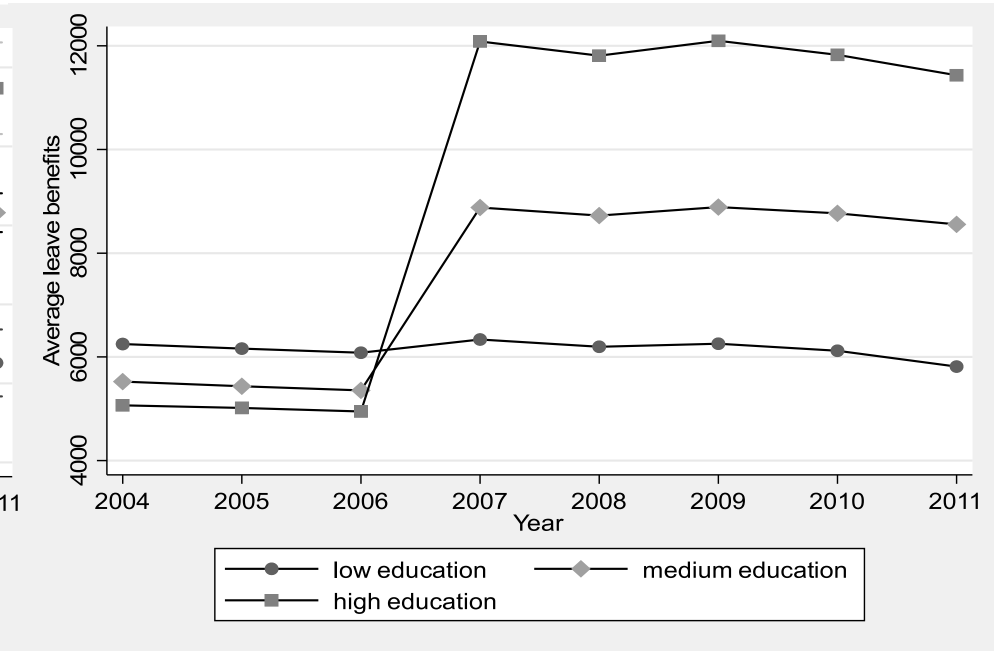
Notes : Graph in Part A shows the evolution of mean birth rates between 2004 to 2012 for women above (treated) and below (control) earnings of 5,850 EUR, equivalent to the 35th percentile of the earnings distribution (with 95% Confidence Intervals around the mean). The graph in Part B shows the evolution of average leave benefits for women in the treatment vs. control group, where pre-reform benefits are defined by a woman's income. Data Source: Pension registry data (AKVS) 2004-2012, Microcensus 2006 for pre-reform benefit simulation.

Figure 3: Evolution birth probabilities and benefits across education groups

Part A: Evolution of birth probabilities

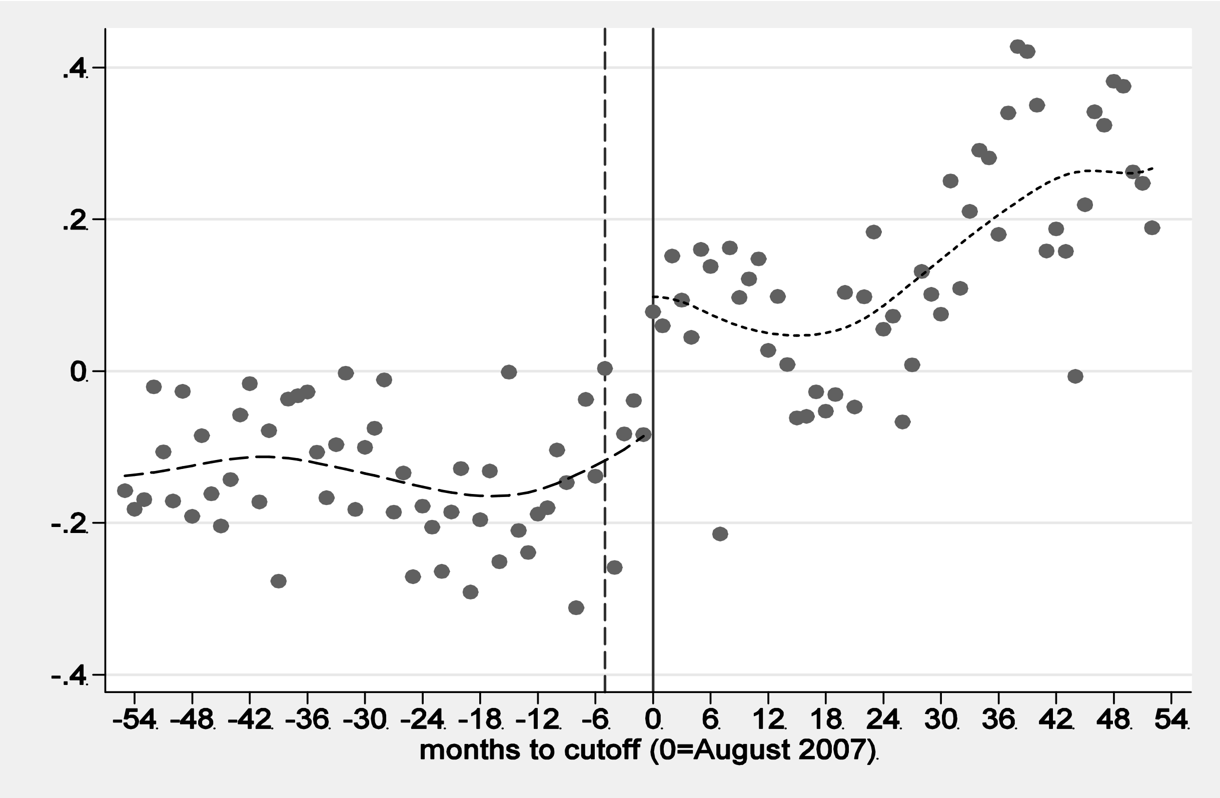


Part B: Increase in benefits post-reform



Notes : Graph in Part A shows the evolution of mean birth rates between 2002 to 2011 for high-skilled, medium-skilled and low-skilled women. The underlying data is restricted to women who are economically active or have stopped working in the potential year before birth and excludes self-employed and civil servants and women living with their parents to match the pension registry sample in Panel B. I fitted a linear trend through the pre-reform periods (solid red line) and extrapolated the trend to the post-reform period (dashed line). The graph in Part B shows the evolution of average leave benefits across groups, where pre-reform benefits are defined by a women's age and education. Data Source: SUF Microcensus 2003-2012, Microcensus 2006 for pre-reform benefit simulation.

Figure 4: Evolution of monthly births per 1000 women (aged 25-45), seasonality corrected

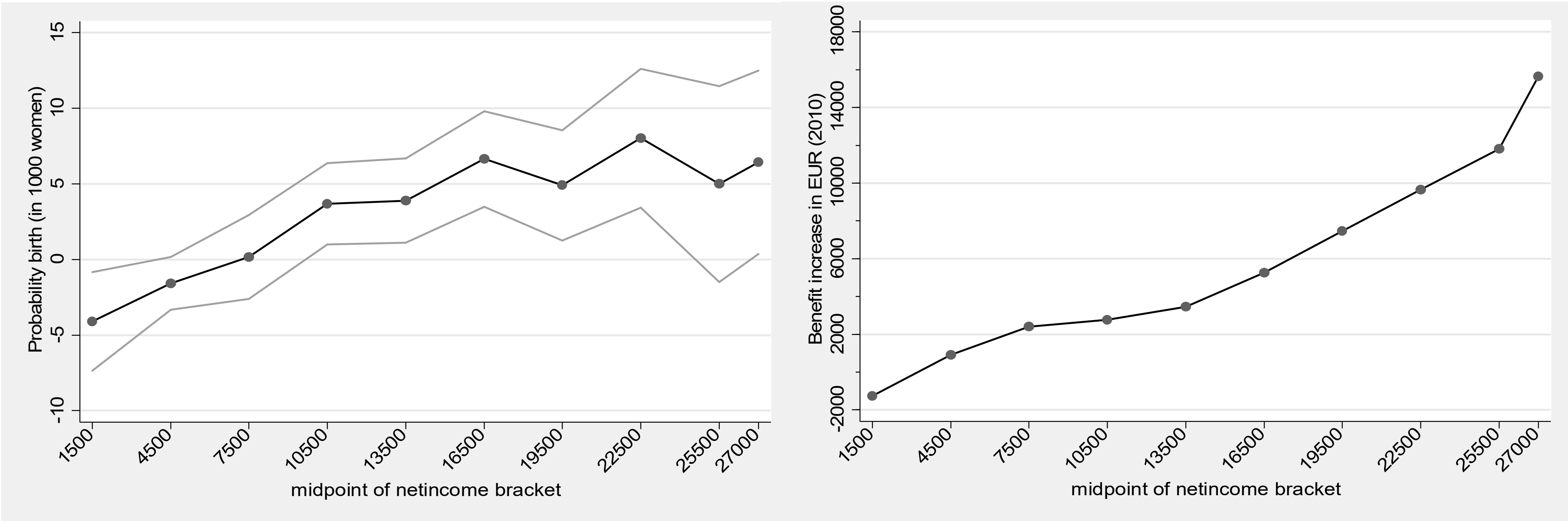


Notes : Lowess fit on both side of August 2007 (0 months to cutoff). The dashed line denotes March 2007, 9 months after announcement of the law. Data Source: Residual (month of birth adjusted) monthly livebirth per 1000 women aged 25-45 (on 31.12 of previous year), 2003-2011 GSO vital statistics.

Figure 5: Increase fertility vs. benefits post reform along earnings distribution

Part A: Increase in birth probability post-reform

Part B: Increase in benefits post-reform



Notes : Graph in Part A shows estimates (with 95% Confidence Intervals) post-reform interaction with earnings interval (intervals of 3000 EUR, starting with below 3000 EUR up to an interval containing women with net earnings above 27 000 EUR) from augmented baseline regression (seefootnote 35), including dummies for income brackets and controls from the baseline regression omitting year fixed effects (see notes for Table 3 for a description of the control variables). The graph in Part B shows the estimated coefficients for the post-reform benefit increase across the earnings intervals where pre-reform benefits are define by women's earnings. Data Source: pension registry data (AKVS) 2004-2012, Microcensus 2006 for pre-reform benefit simulation.

Table 1: Fertility across selected countries

Panel A: Total fertility rates across countries

Germany (2006)	1.33
South Korea (2011)	1.24
Japan (2011)	1.39
Italy (2011)	1.44
US (2011)	1.89
UK (2011)	1.98
Sweden (2009)	1.94

Panel B: Fertility indicators by education level (completed fertility)

	Average number of children per woman		% childless	
	low	high	low	high
US	2.56	1.81	12%	20%
UK	2	1.4	15%	30%
Germany	2.06	1.31	18%	31%
Sweden	2.1	1.8	14%	18%

Notes: Panel A reports total fertility rates across various country, information is based on Worldbank Development indicators and information by national Statistical offices. In Panel B I report completed fertility rates for cohorts born around 1965 for women without a secondary schooling degree (in the case of UK and Sweden, for women who only completed the minimum compulsory schooling) and women with tertiary education (college degree for US). Information is based on U.S. Census Bureau (2010), Ratcliff and Smith (2006) for the UK, Bujard (2012) and Statistisches Bundesamt (2010) for Germany and Boschini et al. (2011) for Sweden.

Table 2: Overview over changes in maternity leave benefit system

	pre-2007 benefits (Erziehungsgeld)		post-2007 benefits (Elterngeld)
	Option 1	**Option 2**	
Monthly benefits	300 EUR	450 EUR	ca. 67% of pre-birth net earnings; min. 300 EUR , max. 1800 EUR. Mothers without employment history entitled to 300 EUR
Means testing	yes, family income during receipt (see Notes)		no
Max. duration	24 months	12 months	12 months (average 11.7 months)
Total max. benefits	7,200 EUR	5,400 EUR	3,600-21,600 EUR
Proportion covered	65% (2006)	10% (2006)	close to 100%
Average paid (06/07)	3,850-4,440 EUR (2006)		7,080 EUR (previously employed: 10,128 EUR (2008))
Requirements	not working more than 30 h during transfer receipt		

Notes: Information on average paid and proportion covered is calculated on statistics on Elterngeld and Erziehungsgeld provided by the German Statistical office. Note that post 2007, two additional months of benefit entitlement are reserved for the other parent. Pre 2007, the income threshold (after deductibles) was 30,000 EUR for couples (23,000 EUR for single parents). Benefits were restricted to a duration of 6 months for those with an income threshold above around 21,000 EUR (and below 30,000 EUR). The income referred to the household income during benefit receipt. See Kluve and Tamm (2013) for further reform details.

Table 3: Descriptive Statistics

Panel A: Individual characteristics		
Outcome variable		
Probability to give birth in a given year	0.041	
Parental leave benefits		
Pre-reform (2010 EUR)	4986.87	
(std. dev)	(1173.661)	
Post-reform (2010 EUR)	8280.25	
(std. dev)	(4472.321)	
Selected covariates		
Age	33.37	
Age when giving birth	30.67	
German nationality	0.91	
Education (based on Microcensus)		
Share low-skilled	0.24	
Share medium-skilled	0.62	
Share high-skilled	0.14	
Percentiles of earnings distribution (in EUR)		
	net	*gross*
10th	3,262	3,262
25th	3,934	4,933
50th	10,114	14,874
75th	16,232	25,903
90th	21,540	36,091
95th	25,131	43,468
Panel B: Parity information from Microcensus (all women)		
Woman does not have a child	0.41	
Woman is mother of one child	0.27	
Woman is mother of two children	0.24	
Woman is mother of three or more children	0.08	

Notes: Panel A reports sample means of the probability to give birth in a given year (outcome variable) and the average benefits a woman could receive in the pre-reform and post-reform period (standard deviation in parantheses). It further reports selected individual charactertics of women as well as the percentile of the distribution of earnings in the preceding year in EUR, both in gross and net terms. Information of education of women is based on the Micocensus sample. Panel B reports information on the number of existing children in the household for the sample of all women aged 21-44 of the Microcensus.

Source: Panel A: SUF AKVS 2004-2012 (own calculations based on estimation sample), Calculation of pre-reform benefits as outlined in Appendix B. Information on education based on SUF Microcensus 2003-2012. Panel B: SUF Microcensus 2003-2012.

Table 4: Linear probability model (birth in 1000 women) - Differences-in-differences by earnings

	(1)	(2)	(3)	(4)	(5)
	High-earning (treated) vs. Low-earning (control)			Alternative treatment group	Placebo
	baseline (estimation equation 1)	no controls	add earnings	above (treatment) vs. below median (control)	Placebo reform in 2006 (pre-reform period 2004-2006)
treat*post2007	6.424***	7.413***	6.204***	7.235***	1.8
N =644,981	(1.008)	(1.014)	(1.008)	(0.983)	(1.663)
in % terms of pre-reform mean (39.62 (treatment group in (1)-(3)), 41.06 (above median in (4))	***16%***			***18%***	
N	644,981	644,981	644,981	644,981	253,668

Notes: All regressions show estimates for a linear probability model of giving birth in t (expressed in births per 1,000 women) and are estimated for women aged 21-44 with positive earnings in t-1 for years 2004-2006 and 2008-2012. The interaction of post-2007 and earnings dummies tests for differential time trends post-reform with respect to women below that earnings threshold (control group). In (1)-(3) the treatment group consists of women above net earnings of 5850 EUR, equivalent to the 35th percentile of the earnings distribution (control group: equal or below 5850 EUR). I control for treatment group indicators. I further controlled for year dummies, region (Länder) dummies, age dummies and separate age dummies for high-skilled women, a dummy for German nationality, education dummies, social benefit and vocational training status in t-1 as well dummies whether the woman has had a child in t-1 and t-2. In Specification (2) I drop all control variables and in (3) I additionally account for earnings and its square in t-1. In (4) the treatment group consists of women above median earnings (control group: below median earnings). In (5) I estimate the effect for a placebo reform in 2006 with the baseline set of control variables and restrict the sample period to 2004-2006 (N=253,668). Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.

Source: SUF (1%) AKVS 2004-2012.

Table 5: Linear probability model (birth in 1000 women) - Differences-in-differences by education groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Match pension registry sample		Placebo check	
	Baseline (estimation equation 2)	without controls	account for childlessness and number of children	restrict to employed or previously employed women	also exclude women who are civil servants and self-employed	placebo reform for 2004 (2001-2006 births) all women	placebo reform for 2004, match pension registry sample
medium education*post2007	1.724	1.72	1.655	3.347**	3.850***	1.308	1.296
	(1.232)	(1.246)	(1.235)	(1.302)	(1.341)	(1.463)	(1.535)
in % of pre-reform mean (47.42 (1) and 42.27 (5))					***9%***		
tertiary education*post2007	8.661***	7.371***	8.811***	11.687***	11.529***	1.295	1.944
	(1.866)	(1.881)	(1.867)	(1.941)	(2.163)	(2.275)	(2.336)
in % of pre-reform mean (57.61 (1) and 50.83 (5))	***15%***				***23%***		
N	666,722	666,722	665,731	510,053	454,563	460,975	342,592

Notes: All regressions show estimates for a linear probability model of giving birth (expressed in births per 1,000 women) and are estimated for women aged 21-44, over birth years 2002-2006 and 2008-2011 (survey years 2003-2012). The interaction between post-2007 and education tests for differential time trends of medium- and high-skilled post-reform with respect to low-skilled women (control group). I control for education group dummies (treatment group indicators) as well as year dummies. I have further controlled for region (Länder) dummies, age dummies, dummies for receiving social benefits or unemployment benefits and separate age dummies for high-skilled women, a dummy for German nationality and whether the woman was born in Germany. In (2) I drop the individual-level control variables. In (3) I account for the number of children in total and the age composition of existing children (below 3 as well as 3-5). In (4) I restrict the sample to women who are active or stopped working the year of giving birth and in (5) I additionally exclude women who are currently civil servants or self-employed (or who were civil servants or self-employed in their recorded last work). In (6) and (7) I estimate a placebo reform for 2004 on pre-reform data (2001-2006) accounting for the same set of control variables as before, for both the sample containing all women in (6) as well as the sample of women who are active or stopped working the year of potential birth in (7). Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.
Source : SUF Microcensus 2003-2012.

Table 6: Linear probability model (birth in 1000 women) for benefit estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Placebo check	*Alternative definition of pre-reform benefits*		*IV estimate*
	baseline (estimation equation 3)	no controls	second-order polynomial in real net earnings	interact control function with education group dummies	placebo reform in 2006 (restrict to 2004-2006 pre-reform years)	Budget option pre 2007	group by education pre 2007	IV: Instrument benefits with education-year interactions
Effect of total expected benefits in 1000 EUR	0.783***	0.812***	0.780***	0.763***	-0.083	0.783***	0.768***	1.096***
	(0.130)	(0.131)	(0.127)	(0.131)	(0.225)	(0.125)	(0.111)	(0.371)
Effect in % of mean birth rate pre-reform (38.05)	*2.1%*							*2.9%*
First stage F-statistic								4859.74
First stage: Partial R2								0.111
N	644,981	644,981	644,981	644,981	253,668	629,691	629,691	644,981

Notes: All regressions show estimates for a linear probability model of giving birth in t (expressed in births per 1,000 women) and are estimated for women aged 21-44 for the years 2004-2006 and 2008-2012. Benefits are defined in real (2010) 1000 EUR. All specifications include year dummies. I have further controlled for a fifth-order polynomial for real net (2010 prices) income in the preceding year, for region (Länder) dummies, German nationality, age dummies and separate age dummies for tertiary-educated women, education dummies, vocational training status in t-1 and social benefits status in t-1 and whether the woman had a child in t-1 or t-2. In (2) I drop the individual control variables except the year dummies. In (3) I account for a second-order polynomial (instead of a fifth-order) in real net earnings. In (3) I amend the baseline regression in (1) by interacting the control function in net earnings with education controls. In (5) I estimate the effects for a placebo reform on pre-reform data (2004-2006), setting the reform date to 2006. In (6) I define the total pre-reform benefit in terms of the shorter 12-months option and in (7) I merge simulated pre-reform benefits by education-age group. In (8), benefits are instrumented by education-year-interactions. Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.

Source: SUF (1%) AKVS 2004-2012, women with positive earnings in t-1.

Table 7: Robustness of baseline benefits estimates to functional form

	Effect on probability of giving birth in t (births per 1000 women)
<u>**A: Benefits expressed as replacement ratio of net earnings**</u>	
Effects of increase in replacement rate by 1%-point	0.137***
	(0.035)
Effect of 10%-point increase in replacement rate by... in % of mean births pre reform (38.05)	3.6%
<u>**B: Benefits expressed in log benefits**</u>	
Effect of increase in benefits by 100%	7.145***
	(1.229)
Interpretation of estimate: 10%-point increase in expected benefits increases births by 0.71 (per 1000)	
Implied percentage change of 10% increase in benefits (in % of pre-reform mean)	1.9%

Notes: All regressions show estimates for a linear probability model of giving birth in t (expressed in births per 1,000 women) and are estimated for women aged 21-44 for years 2004-2006 and 2008-2012. in Panel A I define benefits as the replacement rate in percentage terms of net earnings in t-1. To avoid large outliers, I restrict the sample in Panel A to women with net earnings larger than 2000 EUR in t-1 (N=607,634). In Panel B I use log benefits instead of benefits in levels. I have further controlled for a fifth-order polynomial for real net (2010) earnings in t-1 (polynomial in log real net earnings in B), year dummies, region (Länder) dummies, a dummy for German nationality, education dummies, dummies for vocational training status in t-1 and social benefits status in t-1, age dummies and separate age dummies for tertiary-educated women as well as dummies whether the woman had a child in t-1 or t-2 . Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.

Source: SUF (1%) AKVS 2004-2012, women with positive earnings in t-1.

Table 8: Results by age group - Linear probability model (birth in 1000 women) for benefit estimation

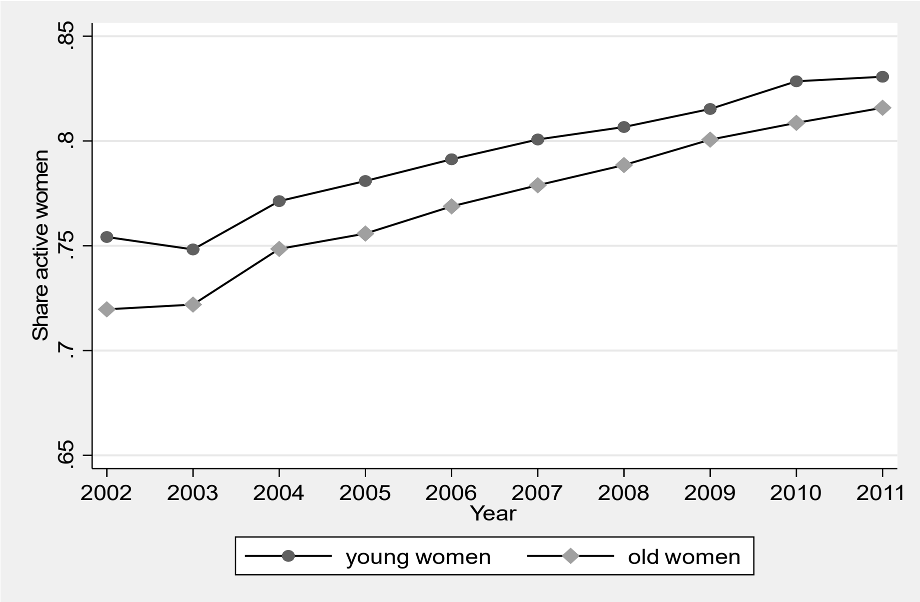
	(1)	(2)	(3)	(4)	(5)
	Age 20-24	Age 25-29	Age 30-34	Age 35-39	Age 40-44
Pre-reform mean of probability of having child (births in 1000 women)	*30.49*	*67.34*	*72.27*	*32.6*	*5.17*
Panel A: All women					
Effect of total expected benefits in 1000 EUR	0.563	0.893**	0.52	1.087***	0.250**
	(0.429)	(0.393)	(0.391)	(0.282)	(0.108)
Effect of increase in benefits by 1000 EUR in % of pre-reform mean		*1.3%*		*6.1%*	*4.8%*
N	118,846	123,025	115,011	135,335	173,782

Notes: All regressions show estimates from separate regressions for various age groups (20-24, 25-29, 30-34, 35-49, 40-44) of the linear probability model of giving birth in t (expressed in 1000 women) for years 2004-2006 and 2008-2012. Benefits are defined in real (2010) 1000 EUR. I have further controlled for a fifth-order polynomial for real net (2010) income in past year, year dummies, region (Länder) dummies, a dummy for German nationality, age dummies and separate age dummies for high-skilled women, education dummies, and dummies for social benefits status in t-1 and whether the woman had a child in t-1 or t-2. Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.

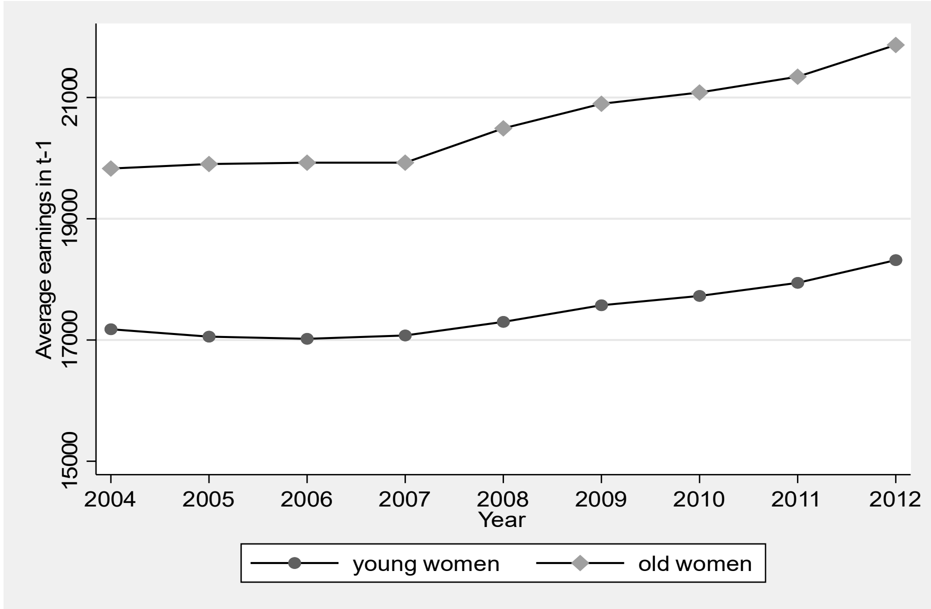
Source: SUF (1%) AKVS 2004-2012, women with positive earnings in t-1.

Figure A1: Evolution of labor supply - young vs. old women

Part A: Share of economically active (extensive margin)



Part B: Evolution of yearly earnings in t-1 (intensive margin)



Notes : Part A shows the evolution of the share of women who are economically active and are not currently in education, measured by a dummy for being either in employment in year t or having stopped working in the previous year, for young women (aged 21-44) and older women (aged 45-55). Part B shows the evolution of yearly earnings (including marginal employment) in the preceding period t-1 for young (aged 21-44) and old (aged 45-55) women who were employed in the previous year. Data Source: Part A: SUF Microcensus 2002-2012, Part B: SUF (1%) AKVS 2004-2012, women with positive earnings in t-1.

Table A1: Differences-in-differences by education groups - Results using pension registry data (AKVS 2004-2012)

	(1) Baseline (estimation equation 1)	(2) without control variables	(2) account for social benefit status, polynomial in real earnings
medium education*post2007	3.148***	2.542**	3.365***
	(1.100)	(1.107)	(1.099)
in % terms of pre-reform mean (39.87)	***8%***		
tertiary education*post2007	6.363***	4.498**	6.678***
	(2.287)	(2.293)	(2.286)
in % terms of pre reform mean (48.85)	***13%***		
N	644,981	644,981	644,981

Notes: All regressions show estimates for a linear probability model of giving birth in t (expressed in births per 1,000 women) and are estimated for women aged 21-44 with positive earnings in t-1 for years 2004-2006 and 2008-2012. The interaction of post-2007 and education dummies tests for differential time trends of medium- and high-skilled post-reform with respect to low-skilled women (control group). I control for education group dummies (treatment group indicators). I have further controlled for year dummies, region (Länder) dummies, age dummies and separate age dummies for high-skilled women, a dummy for German nationality as well dummies whether the woman has had a child in t-1 and t-2 and vocational training status in t-1. In specification (2) I omit all control variables. In specification (3) I additionally account for earnings and its square in t-1 and social benefits status in t-1. Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.

Source: SUF (1%) AKVS 2004-2012.

Table A2: Checks on employment adjustment of mothers in response to reform

Panel A: Adjustments in earnings and hours (intensive margin)- using pension registry data

Treatment group: Mothers in t, control group: Older women aged 45-55.

	(1)	(2)
	Outcome: Dummy for change in hours between t and t-1 for women working part time in t-1	Outcome: Changes in earnings in EUR per day worked between t and t-1
gave birth in t*post2007	0.002	0.076
	(0.007)	(0.305)
N	138,555	321,196

Panel B: Differential Changes in labor market status (extensive margin) using Microcensus data

Treatment group: Mothers in t, control group: Older women aged 45-55.

	(1)	(2)
Outcome: Mother is active in labor market	Baseline	add controls
gave birth in t*post2007	0.009	0.007
	(0.006)	(0.006)
N	315,555	313,682

Note: All regression show differences-in-differences estimates on various labor market outcomes, comparing women age 21-44 who gave birth in year t (treatment) to older women (ages 46-59). The interaction of post-2007 and the indicator for giving birth tests for differential time trends in labor market outcomes post-reform of new mothers with respect to older women (control). I account for a post-reform dummy and indicator for giving birth in all specifications. The treatment year 2007 is excluded. Panel A is based on the pension registry data. The outcome in column (1) is an increase in hours between year t and t-1 for women previously working part-time in t-1 (measured by a binary indicator which takes the value one if a woman changed from small to large part-time or part-time to full-time). Results in column (1) are restricted to years 2004-2010, as the coding of the part-time variable changed in 2011. The outcome in column (2) are changes in daily earnings between t-1 and t for all women who were previously employed. Panel B is based on data from the German Microcensus and measures whether a woman is economically active, measured by a dummy for being either active in year t or having stopped working in the previous year. In column (2) I additionally account for year dummies, education group dummies, German nationality and region (Länder) dummies. Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.

Source: Panel A: SUF (1%) AKVS 2004-2012, Panel B: SUF Microcensus 2005-2012.

Table A3: Check on employment adjustment of women of childbearing age in response to reform

Treatment group: Younger women aged 21-34, control group: Older women aged 45-55.

	(1)	(2)
Outcome: Woman is working	Baseline	add controls
young woman*post 2007	-0.002	0
	(0.003)	(0.003)
N	640,000	636,579

Note: The regression shows differences-in-differences estimates on labor market participation, comparing younger women of childbearing age (age 21-34, treatment) and older women (age 45-55). The regression is based on data from the German Microcensus and measures whether a woman is working in a given year. The interaction post-2007 with the indicator for being a young woman tests for differential time trends in labor market participation post-reform of women of childbearing age with respect to older women (control). I have further controlled for a dummy for being in the young age group and a post-reform dummy. In column (2), I additionally account for education group dummies, German nationality and region (Länder) dummies. Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.

Source: SUF Microcensus 2005-2012.

Table A4: Results by birth order- Linear probability model (birth in 1000 women) allowing for reform effect to differ by education - using German Microcensus (births 2002-2011)

Panel A: Probability for first or second or higher-order birth (births per 1000 women)- all age groups

	(1)	(2)	
	All women	RV sample (active before)	*Mean birth rate pre-reform (all women)*
tertiary education*post2007	7.538***	10.577***	*56.92*
	(2.336)	(2.380)	
	-4.031	0.502	*95.64*
tertiary education*post2007*second births	(4.584)	(4.764)	
	-7.313**	-9.376***	*25.83*
tertiary education*post2007*third (and higher order) births	(3.025)	(3.075)	
N	665,731	509,541	

Panel B: Heterogeneous effects on completed fertility - birth order for women aged 40-44

	(1)	(2)	
	All women	RV sample (active before)	*Mean birth rate pre-reform*
tertiary education*post2007	6.395*	8.255**	*24.7*
	(3.864)	(3.915)	
	10.591*	8.148	*27.94*
tertiary education*post2007*second births	(5.985)	(6.087)	
N	99,421	83,771	

Notes: All regressions show estimates for a linear probability model of giving birth in t (expressed in births per 1,000 women) and are estimated for years 2002-2006 and 2008-2011. I only show results for tertiary-educated women. In Panel A I look at probabilities to have a first birth, second or third (or higher) for women aged 21-44 estimating a model with additional interactions of second as well as third and higher-order births with the post-2007 dummy to test for heterogeneous reform responses for second and higher-order births. In Panel B I restrict my sample to women at the end of their fertile cycle, age 40-44, to investigate the effects on completed fertility along the extensive (first birth) and intensive margin (second birth). In column (1), I include all women observed in the Microcensus. In column (2), I restrict the sample to women who are active or have stopped working the year of giving birth to match the sample composition of the pension registry data. The interaction of post-2007 and education tests for differential time trends post-reform for first births with respect to low-skilled women (the control group). I have further controlled for a post-reform dummy, region (Länder) dummies, age dummies and separate age dummies for tertiary-educated women, a dummy for German nationality and dummies for receiving social benefits or unemployment benefits. I additionally account for dummies measuring that the woman has one or more than one children as well as age composition of existing children. Robust standard errors reported in parentheses. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level.

Source : SUF Microcensus 2003-2012.