Do financial incentives affect fertility - Evidence from a reform in maternity leave benefits*

Anna Raute†

January 18, 2014

JOB MARKET PAPER

Abstract

I assess the effects of changes in financial incentives on fertility arising from a reform in parental leave benefits in Germany. Up until 2007, German parental benefits were means-tested transfers and targeted at lower income families. From 2007 onwards, parental leave benefits are increasing in mother's pre-birth earnings with a minimum benefit being granted to all mothers. The reform increased the financial incentives to have a child for higher educated and higher-earning women considerably, by up to 21,000 €. First, I find large discontinuous jumps in overall monthly birth rates nine months after the passing of the law and an increasing trend in birth rates after the discontinuity. Second, I exploit the large differential changes in parental leave benefits across education and income groups to estimate the causal effect of parental leave benefits on fertility. I find a positive, statistically significant effect of an increase in benefits on fertility, which is mainly driven by women in the middle and upper-end of the education and income distribution. My findings suggest that earnings dependent parental benefits, which compensate women for their opportunity cost of childbearing accordingly, could be a successful means to increase the fertility rate of high-skilled and higher-earning women and to reduce the disparity in fertility rates with respect to mothers' education and earnings.

Keywords: Fertility, Parental Benefits.

JEL Classification: J13, J16, J18

---

*I am grateful to Christian Dustmann and Uta Schönberg for their generous support on this project. I also want to thank Per-Anders Edin, Brendon McConnell, Magne Mogstad, Ronni Pavan, Petra Persson, Jan Stuhler, Andreas Uthemann and many PhD-colleagues and seminar participants at University College London and Royal Holloway for helpful comments. I want to thank Tatjana Mika from the Research Data Centre of the German Federal Pension Insurance (FDZ-RV) for her support with the Pension registry data and Tom Wey for his assistance with programming the benefit-schedules. Financial support from the German National Academic Foundation and the Centre for Research and Analysis of Migration is gratefully acknowledged. All errors are my own. For the most recent version please click here.

†University College London and Centre for Research and Analysis of Migration (email:a.raute@ucl.ac.uk, website: anna.raute@wordpress.com).

1 Introduction

Over past decades, many developed countries have been facing decreasing birth rates and below replacement fertility levels. The total fertility rate lies well below replacement level with 1.33 in Germany and 1.4 in Italy, but other countries, such as the US, UK and Sweden, have fertility rates close to replacement (Table 1, Panel A). A common phenomenon across countries is the negative relationship between a woman's education level and her fertility as shown in Panel B of Table 1, which can be explained by the fact that opportunity costs of fertility are increasing with education (Willis (1973)).[1] In the United States 19.7% of women with a college degree and born in 1965-1969 remained childless and gave birth to an average of 1.81 children, whereas women of the same cohort who have not finished schooling had an average of 2.56 children (U. S. Census Bureau (2010)). In Germany there is a particularly large disparity between education groups in fertility: 31% of tertiary educated West German women remained childless and had an average of 1.33 children, 0.7 fewer children than the average woman without a secondary schooling degree (Bundesamt (2010) and Bujard (2012)). A gap of similar size can be observed for the UK, where the college educated born in 1965 have on average 0.6 children fewer children than women with compulsory schooling only (Ratcliffe and Smith (2006)). Disparities between education groups in Sweden on the other hand, a country with very generous family policies, are considerably smaller (Boschini et al. (2011)).

These observed differences in fertility across-socioeconomic groups are likely to have distributional implications for future generations if intergenerational mobility in education or income is not perfect. The intergenerational correlation between parental and child education is high and estimated to be around 0.4-0.5 for the US and most Western European countries (see Hertz et al. (2008) as well as Black and Devereux (2011) who discuss the potential underlying mechanisms for the observed correlation in education).

Countries have taken different avenues in the way they support families and subsidise childbearing, incl. tax benefits, family taxation, child care subsidies and cash child benefits. Every OECD country except the United States offers a federally funded parental leave programme which provides job-protection and some replacement of earnings (see Thévenon and Solaz (2013)). The design of benefits however varies considerably across countries. In more traditional "male-breadwinner" countries like Austria or Germany up until 2007, payments are usually flat and targeted at lower-income families. The Nordic countries on the other hand, who aim to promote gender equality and increase mothers' labor force participation through their family policies, pay generous parental

[1] 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)).

leave benefits which are closely tied to recipients' pre-birth earnings (e.g. with replacement rates of 100% in Norway and 80% in Sweden). These two types of transfer systems are fundamentally different in how they compensate women for their costs of children. Flat transfers translate into strong differences in replacement rates and increases in flat transfers will mostly benefit mothers with lower earnings pre-birth. Payments which are increasing in pre-birth earnings on the other hand compensate women's private opportunity cost of children accordingly along the earnings distribution.

A major challenge in the literature estimating the effect of financial incentives on fertility is to get exogenous variation in the cost of fertility. The financial incentive effect of parental leave benefits on fertility has not received much attention up to date. Researchers have instead focused on the incentive effect of child cash transfers or welfare programmes which are generally designed to set higher financial incentives to lower-income mothers. To my knowledge there is however no direct empirical evidence on whether financial incentives targeted at higher-earners are effective in increasing fertility for higher-earning women, whose opportunity costs of childbearing are high.

I exploit a major parental leave reform in Germany in 2007 to examine how a shift from a parental leave benefit system which targets lower-income families to a system which closely ties benefits to pre-birth earnings and pays higher benefits to higher-earning women affects fertility. In Figure 1, I contrast the two benefit schemes. Before 2007 mothers received a flat monthly transfer over 24 months post-birth, which amounted to a maximum payment of 7,200 EUR. As benefits were means-tested on familiy income during benefit receipt, the average total pre-reform payment was around 4,400 EUR, which I also show in the graph.[2] Post reform, benefits are a function of pre-birth earnings and paid for 12 months ranging from a total payment of a guaranteed minimum of 3,600 EUR for women who were previously inactive or had very low pre-birth earnings to 21,600 EUR for top-earners. the job protection period of 36 months was unchanged. The figure shows that the reform increased the financial incentives to have a child for high-earning (and higher-educated) women considerably, whereas changes in benefits for lower-earners were modest or even negative. Besides promoting gender equality through improving women's labor force participation, the reform had a clear pro-natal motivation. German chancellor Angela Merkel mentioned the low fertility rate amongst highly-educated women in a speech at the employers' association in late 2006 as one of the motivating reasons behind the reform, which she described as a "paradigm shift in social policy".[3]

[2]Due to the means-testing on family-income during benefit receipt only 60% of parents qualified for the maximum payment. I will discuss this further in Section 3.

[3]"Support of families used to be support of families in need. (...). We face the problem, that 40% of tertiary educated do not have children. A country, which calls itself highly developed, can not afford such a situation." (Bundesregierung (2006)).

First, using Vital Statistics data I document discontinuous jumps in monthly birth rates of close to 4% nine months after the reform was passed and an increasing trend in birth rates after the discontinuity. This (short-term) effect on fertility exists for the extensive margin of childbearing, i.e. the probability of having a first child, as well as for the probablity of a second or third child (intensive margin). I also find evidence for a discrete drop in the incidence of abortions for married women when the legislation was passed. Second, I exploit the large differential changes in parental benefits across income and education groups in my empirical strategy using adminstrative data which provides detailed information on earnings as well as data from the German Micro Census. I show that the probability to have a child in a given year within the four years post reform period increases by 6% for medium-educated and by 13% for high-educated relative to low-educated. My baseline instrumental variables estimates suggest that a 1000 EUR increase in total benefits a woman would be entitled to receive if she had a child raises the probability to give birth by 1.2% in each year post reform. I also estimate a benefit elasticity: a 10% increase in parental benefits leads to a 1.1% increase in the probability to have a child in a given year. The reform appears to positively affect the fertility of women of all earnings groups beyond median earnings including the top 5th percentile, but the effect of an additional 1000 EUR increase in benefits seems to be decreasing with pre-birth earnings. I find that the reform increased the probability to have a child mainly for older women aged 35-39 and 40-44. These women are nearing the end of their lifetime fertility and are unlikely to postpone childbearing, so an increase in their (short-run) fertility is likely to have a permanent effect and raise the completed fertility of these cohorts. The reform seems to have lowered the age at first birth for highly-educated women and increased the probability to have a first child for highly-educated women at the age of 30-35. In addition, I find highly-educated women to be more likely to have a(n additional) second child at the end of their fertile period post-reform. The estimated reform effect appears to be considerably larger for East German than West German women. This is likely to be explained by the fact that East and West German women face very different opportunity costs of childbearing due to diverging social norms on mother's labour force participation. An increase in benefits of the same magnitude effectively replaces fewer of the opportunity costs for West than for East German women. The result implies that the pro-natal effect of earnings-dependent benefits is likely to be higher in a society where social norms allow a faster return to the labour market after childbearing.

Taken together I find that shifting from flat parental leave benefits to earnings-related payments has substantial pro-natal effects. The switch between systems increases the financial incentives for medium-earners and higher-earners considerably. I provide empirical evidence that the reform had an effect on the socio-economic structure of fertility.

This paper proceeds as follows. In the next section I give a brief literature overview. In Section 3 I present some background on the change in the maternity leave benefit legislation in Germany. I outline my empirical strategy in Section 4 and describe my data in Section 5. I present my main results and contrast my grouping estimators using income groups and education groups in Section 6.2 followed by a discussion of important heterogeneous effects of the reform on age groups and birth order as well as East vs.West Germans in Section 7. Section 8 concludes.

2 Previous Research

A growing empirical literature questions to what extent financial incentives which lower the cost of children affect fertility. The crucial challenge is to find plausible exogenous variations in the proxies for the price of fertility, as Hotz, Klerman and Willis (1997) conclude their survey of the economics of fertility. As government family policy which provides financial and in-kind support for families with children is usually universal, it sets financial incentives to all women and does not provide a natural control group for the counterfactual situation.

The existing literature studying financial incentives on fertility can be broadly classified into five strands: A first strand of papers uses cross-country variation and finds very mixed results for the effectiveness of family policy (see e.g. Demeny (1986) and Gauthier and Hatzius (1997)). A second strand of papers relies on time series variation in government policy and analyses discontinuous changes in birth rates as a result of a policy change to identify a short-run incentive effect. A third set of papers seeks to test the incentives of welfare programmes on the fertility of low-income wome, whereas a fourth set of papers exploits variation in child subsidies for the third (or higher) child relative to the first and second child. Besides the natural experiment approaches, there exist structural approaches to identify women's responses to welfare reforms (Keane and Wolpin (2010)) and to financial incentive through the tax and benefit system (Laroque and Salanié (2013)), which rely on cross-sectional variation for identification.

Time series studies find positive financial incentive effects on fertility: Whittington, Alm and Peters (1990) show, using very long time series data, that fertility was responsive to the personal tax exemption for dependents in the US. Sinclair, Boymal and De Silva (2012) apply a structural time series model to analyse the effect of the Australian baby bonus, a cash in hand payment for every newborn child; they find a significant increase in birth numbers ten months following the announcement of the baby bonus, and also establish that this initial policy introduction effect was maintained at least up to four years after. In a similar vein, González (2013) shows that the introduction of a universal cash benefit in Spain led to an immediate increase in the number of births by 6%. In this paper, I will also provide empirical evidence for an immediate increase in births nine months after the maternal leave benefits reform was passed.

Studies analysing the effect of Aid to Families with Dependent Children, a large US welfare programme find a weak effect on fertility, with results being sensitive to the empirical approach chosen (see Moffitt (1998) for an overview). Baughman and Dickert-Conlin (2003) find that very large increases in the income support provided by the EITC encouraged first births for married women with an estimated elasticity of 0.06; a 10% increase in the total EITC increases the probability to have a first birth by 0.6%. Brewer, Ratcliffe and Smith (2011) analyse the effect of the UK welfare reforms targeted at low-income households and find an increase of births for married women by around 15% as a result of the policy. The reform effect appears to have been larger for the birth of the first and the third child than for the second.

Milligan (2005) exploits the introduction of a universal child allowance for newborns in Quebec, which paid high subsidies in particular for third or higher births. He finds a strong pro-natalist effect of the policy and estimates the average benefit elasticity to be 0.107. He does not find statistically significant differences in the reform impact by female education, but estimates a stronger response for women with higher family income. Cohen, Dehejia and Romanov (2013) exploit variation in Israel's child subsidy programme for families with at least two children and estimate an average benefit elasticity of 0.176. In contrast to Milligan (2005), they find the effect to be strongest for the lower range of the income distribution.

The literature discussed above has focused either on the effects of welfare payments targeted at low-income women or on financial incentives of universal child cash transfers, which are usually fixed payments independent of mother's earnings and are implicitly designed to set higher financial incentives to lower-income mothers. We would expect that the positive effects on fertility found in the above studies are mostly driven by low-income women and might not hold for high-income women. In this paper, I provide empirical evidence on whether financial incentives targeted at higher-earners are effective in increasing the fertility of higher-earning women, whose opportunity costs of childbearing are high. I exploit a change in the German parental leave benefit system for identification.

I contribute to the literature by analysing the fertility effects of parental leave benefit payments, for which the evidence is scarce. Lalive and Zweimüller (2009) find strong effects of an extension in the duration of paid, job-protected parental leave in Austria on mother's higher-order fertility. The parental leave benefits they study are flat payments independent of pre-birth earnings and as a result set stronger incentives for low-wage women for whom the benefits had a higher replacement ratio of foregone earnings. As expected, the authors find that extending the duration of paid parental leave affected higher-order fertility more strongly for low-wage women than for high-wage women.

Two papers provide suggestive evidence of the incentive effects of earnings-dependent maternity benefit systems: Björklund (2006) studies the evolution of completed fertility patterns for Swedish cohorts who were affected by the large extension of family policies in the 60s and 70s relative to women in neighbouring countries. His results suggest that the extension of family policies which aim at facilitating labour force participation of mothers and closely tie the level of parental leave benefits to mother's previous labour market engagement raised the level of fertility and shortened the spacing of births, but could not eliminate the negative relationship between women's educational level and completed fertility. Heckman and Walker (1990) study the effect of wages and income on life-cycle fertility in Sweden and find female wage coefficients across Swedish cohorts to become an increasingly less accurate measure of the price of fertility over time; they argue that their finding is consistent with the expansion of Swedish family policies.[4] The reform I study in this paper allows me to directly estimate the causal effect of a change from a flat-payment maternity benefit system to an earnings-related benefit system on fertility behaviour and assess the changes in the socio-economics composition of fertility.

There is evidence in the literature, that culture or social incentives matter for work and fertility behaviour and can determine the effectiveness of financial incentives. Fogli and Fernandez (2009) show that cultural proxies from a woman's country of ancestry are economically and statistically significant in explaining work and fertility behavior of second-generation American women. Manski and Mayshar (2003) as well as Cohen, Dehejia and Romanov (2013) find that the Israeli child allowance system generates a complex fertility pattern across ethnic-religious groups, which can be explained by private as well as social incentives. I add to this literature by exploiting differences in social norms on mothers' labour force participation between former socialist East Germany and more traditional West Germany to assess how differences in social norms can determine the magnitude of the incentive effect of maternity benefits on fertility.

3 The parental benefit reform

In Germany, parental leave regulation is very generous with regard to job-protection periods. Since 1979 job protection has been continuously extended from an initial 6 months of job-protected leave after childbirth to 36 months after childbirth from 1992

[4] They note that they fail to directly estimate the policy effect due to a lack of data.

onwards. During the leave period the firm is not allowed to dismiss the mother and the mother has the right to return to a job that is comparable to the job she held before childbirth. Besides job protection, mothers receive maternity benefit payments whilst on leave.[5] I summarise the pre and post-reform maternity leave benefit systems in Table 2.[6]

Up until the end of 2006, maternity benefits (up until then called Erziehungsgeld, "child-rearing money") were paid for up to 24 months and were targeted at low-income families. Under the old system two options were available. The first option was to receive a maximum of 300 EUR a month for up to 24 months (maximum payment of 7200 EUR in total). The transfers were means-tested on family income during benefit receipt, which meant that in effect they were means-tested on the spouse's income as in most cases the mother was not working, and benefit recipients were not allowed to work more than 30 hours a week in order to be eligible. Only families below an income threshold (after several deductibles) of less than 30,000 EUR (equivalent to 40,400 EUR gross earnings) were eligible for any benefit payments, which were about 74% of all mothers in 2006.[7] A short-option of a monthly payment of 450 EUR paid over 12 months (maximum 5400 EUR in total) was available for mothers who wanted to work (full-time) in the second year after childbirth as an alternative to the 24-months option. Only about 15% of women who were eligible for option 1 and 2 chose the short-term option and these were predominantly East German women. I calculated the average benefit for mothers in 2006 to lie between 3,850 and 4,440 EUR in total based on information from Bundesamt (2006).

On 1st January 2007, a new parental leave benefit (Elterngeld, which translates to "parental money") replaced the old benefit system. Parental leave benefits under the new as well as the old system are fully publicly funded. In contrast to the old benefit system, under which parental benefits were means-tested on family income, transfers under the new benefit system are on average more generous and provide universal coverage. The main aim of the reform was to provide financial means for parents to look after their child during the first year and compensate women for their opportunity cost of fertility by tying benefits closely to their net pre-birth earnings. The government stressed that the reform was a "paradigm shift" in family policies through "preventing

[5] For ease of interpretation I refer to a *maternal* benefit system. Since 1986 fathers have been eligible for parental leave, but very few father took any leave, so the programme was effectively a maternity leave programme.

[6] My discussion of parental leave reforms draws on Kluve and Tamm (2013), who give a very detailed account of the changes in parental benefits.

[7] Benefits were restricted to a duration of 6 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 6 months.

large income drops after childbirth, (..) enhancing the economic independence of both parents, and allowing a fair compensation of opportunity costs of childbearing".[8] The legislative procedure of the reform was rather quick. The reform was born out of a newly formed (rather unexpected) coalition between the two biggest parties, the Christian Democrats and Social Democrats. The earnings-dependent system was originally part of the electoral manifesto of the social-democratic party (SPD), which was predicted to lose the election by a sizable margin. The party announced its reform plans only in August 2005, shortly before the election in September 2005. It seems implausible that women adjusted their fertility behaviour before the implementation of the reform. The new family ministry -headed by the Christian Democrats- decided to implement the proposal post election. The coalition agreed on the main features in May 2006 and published a draft law in June 2006.[9] An amended version of the draft law was passed in parliament and the second chamber between September 2006 and November 2006.

The new Elterngeld replaces about 67% of previous net labour earnings for up to 12 months after the birth of a child. Benefits are calculated on the basis of the average net earnings during the 12 months before giving birth. A flat minimum of 300 EUR a month is paid to previously inactive mothers, which translates into a total sum of benefits a mother would be eligible for of 3,600 EUR. Lower-earning women are granted a replacement ratio above 67% of previous earnings; the replacement ratio is gradually lowered from 100% to 67% for women with monthly net earnings between 300 EUR and 1,000 EUR. The transfer is truncated at 1,800 EUR a month -amounting to a maximum sum of benefits of 21,600 EUR- for women with average net monthly earnings pre-birth above 2,700 EUR, which is equivalent to yearly gross earnings of around 60,000 EUR. Leave benefit recipients are not allowed to work more than 30 hours a week during transfer receipt in order to be eligible, but part-time work is disincentivised as benefits are reduced to a great extent; in 2010 only 1.7% of mothers were part-time employed in the first and less than 9% during the last month of benefit receipt.[10] In 2007, the average total benefit paid under the new system was 7,080 EUR. On average 10,128 EUR (2008) were paid to mothers who were employed prepartum. Compared to the average pre-reform benefit payments reported above, the new system is considerably more generous on average than the old system.

There is close to full take-up of the new leave payment, with around 96% of mothers taking up the transfers (years 2007-2010). Most mothers take advantage of the new ma-

[8]The reform additionally introduced a father quota of two additional months. Around 20% of fathers take these two additional months. In order to not discriminate against single mothers, these are eligible to extend their leave by an additional two months.

[9]Kluve and Tamm (2013) show the evolution of the Google Search Volume Index for "Elterngeld", which shows a first pronounced peak in May 2006.

[10]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.

ternity benefit system for the full length of the eligibility period. The average duration of benefit receipt is 11.7 months and even mothers at the top end of the pre-birth earnings distribution - mothers, who qualify for a payment of 1800 EUR/month or above - take up maternity leave benefits for an average of 11.2 months (both numbers for births in 2010).

In Figure 2, I contrast the simulated pre-reform benefits with the post-reform benefits along the distribution of net yearly labour earnings (The simulated pre-reform benefits are the average benefits for various women's income groups calculated on the basis of the spouse's net income; more details on the simulation of pre-reform benefits are given in 5.3 and in 8). Panel A shows that the total amount of benefits a woman would be eligible to if she gave birth to a child was reasonably flat across net earnings groups, but continuously increasing in net earnings post-reform for the most part of the earnings distribution. The effective reform effect which is the difference between the two lines is continuously increasing in earnings and ranges from -2,400 for incomes below 1,800 EUR a year to an an average increase in benefits by 17,100 EUR for women whose net earning lie above 33,000 EUR. Panel B shows that the benefits replaced very little of women's foregone earnings for higher-earning compared to lower-earning women pre-reform. Post-reform, benefits replaced at least 67% of former earnings for all women. My empirical analysis exploits the fact that the effective policy changes were very different across socio-economic groups.

4 Empirical approach

The primary concern with comparing the fertility behaviour of women who are eligible for higher benefits with women who would receive lower benefits in a cross-sectional comparison is that these women would typically differ in their unobserved factors affecting fertility, such as preferences for children, which would likely yield biased results. As pointed out in Section 2, the empirical evidence on financial incentives effects on fertility is still relatively scarce and has been greatly hampered by the lack of control groups for identification. Exogenous variation in financial incentives across socio-economic groups as a results of a reform can help to identify the causal effect of increased financial incentives, which decrease the cost of childbearing, on fertility decisions. To address the identification problem, I can exploit the differential changes in benefits through the 2007 reform in parental benefits to estimate the incentive effect of parental benefits on fertility decisions. Prior to 2007, parental benefits were means-tested fixed-rate transfers; since the reform in 2007 they differ considerably by pre-birth earnings of the mother (see Figure 1). Higher earning mothers experienced a much larger increase in parental benefits between pre and post-reform periods than lower earning mothers, which is central to my analysis. Due to these differential increases in financial incentives one would expect to see a much larger effect of the reform on fertility of high earning women than on low earning women.

4.1 Baseline estimation

A straightforward way to evaluate the reform is to compare the between-cohort changes in the average fertility among high-earning women with the change among low-earning women.

A simple DID estimator comparing differential treatment of relatively broad groups has the shortcoming of not using the variation in the intensity of the expected parental benefits. I estimate a linear probability model accounting for simulated real expected parental benefits in calendar year t, B_{it}, and for a flexible function of real lagged net earnings, $\Phi(E_{it-1})$:

$$P(Child)_{it} = \alpha + \beta_0 B_{it} + X'_{it}\beta_1 + \Phi(E_{it-1}) + \gamma_t + u_{it}, \tag{1}$$

where $P(Child)_{it}$ is the probability of having a child for woman i in calendar year t. Expected Parental Benefits B_{it} vary considerably over time for women with the same real earnings due to the policy reform. Post reform, benefits B_{it} are a deterministic function of pre-birth net earnings in the preceeding year, $B_{it}(E_{it-1})$. Prior to 2007, B_{it} were (means-tested) fixed-rate payments and did not vary in a systematic way with women's pre-birth earnings. For the pre-reform period I simulate the expected benefits using the 2006 Micro Census, which I will describe in Section 5.3. The post-reform benefits are calculated as a function of gross labor earnings in t-1, the preceding calendar year. X'_{it} is a vector of observed women's characteristics, such as age and education, and γ_t denotes year dummies.

I account for a flexible function of lagged net earnings to ensure that variation in B_{it} comes from variation in the benefits induced by the reform over time and not from variation in the level of net earnings.[11] If I restricted my analysis to the post-reform period only, all the cross-sectional variation in benefits would be captured by the flexible controls for earnings and I could not separately identify β_0. Identification of β_0 requires that I observe fertility decisions of observationally equivalent women both pre and post-reform. Thus with a fully flexible, but time-invariant $\Phi(E_{it-1})$ only cross-cohort variation in B_{it} induced by the reform identifies the "treatment" effect β_0. This β_0 identifies the incentive effect of an increase in the total parental benefits a woman would be entitled to receive for a potential birth, given her earnings in the previous calendar year *t-1,* on her probability of giving birth in calendar year t. Given that the

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

maximum parental benefits a woman would be entitled to needs to not be taken up fully by the woman, e.g.. if she wants to return to full-time work before the 12th month after giving birth, we can interpret the estimated effect as an intention-to-treat effect. As discussed in Section 3, there is however nearly full take-up of post-reform benefits, which means that most mothers do in fact take up the maximum amount of paid leave they are entitled to.

It has to be pointed out that the increase in expected transfers is not equivalent to an increase in available income after childbirth if the reform changed maternal labour supply and did not simply crowd out unpaid leave or leave taken under the previous system.[12] A mother, in particular a medium or higher earning mother, might find it optimal to reduce her labour supply post-birth as a response to the increased benefits in order to spend time with her child. Depending on the magnitude of the response in maternal labour supply the total increase in available income through the reform is likely to be lower than the maximum increase in the parental benefit entitlement. The effect of my explanatory variable, defined as the increase in the total amount of benefits a woman is entitled to if she has a child, implicitly captures this optimal adjustment in labour supply after potential childbirth, making it a policy-relevant parameter.

A similar empirical approach has been applied by Dahl and Lochner (2012), who exploit large changes in the Earned Income Tax Credit to estimate the impact on family income on child achievement and in earlier work by Gruber and Saez (2002), who estimate the elasticity of taxable income exploiting variation due to tax reforms as well as by Nielsen, Sørensen and Taber (2010) and Rothstein and Rouse (2011), who both study the effect of student aid reforms on student outcomes. The polynomial $\Phi(E_{it-1})$ can be thought of as a control function. It is crucial to specify a function flexible enough to capture the true relationship between women's earnings and fertility. My estimation strategy is also related to grouping estimators used to estimate the labour supply effect of tax reforms (see Moffitt and Wilhelm (2000) for a good general discussion on identification issues using Difference-in-Difference methods in the study of labor supply effects of taxation).[13]

The central identifying assumption of my approach is that the relationship between fertility decisions and women's net labour earnings is constant over time. The identifying assumption would be violated if there were differential trends in the fertility decisions across different earnings "groups" or changes in the composition of earnings

[12] I study the causal effects on post-birth labour market participation jointly with Uta Schoenberg in related work in progress.

[13] Feldstein (1995) compares the sensitivity of taxable income to changes in tax rates across groups with different pre-reform marginal tax rate in a difference-in-difference framework. Blundell, Duncan and Meghir (1998) employ grouping-IV estimators comparing the labor supply responses over time for different groups defined by cohort and education level.

"groups" over time which would change the relationship between u_{it} as well as benefits B_{it} and lagged labor earnings E_{it-1}. I will address the robustness of my approach in 6.3. A concern might be that the reform induced women who are planning to have a child to increase their labour supply in order to be entitled to higher benefits.[14] An endogenous adjustment in earnings as a response to the reform would change the relationship between earnings and fertility over time and would invalidate my identifying assumption, which - intuitively speaking - would render women with the same labour earnings to be incomparable over time. I would ideally use pre-reform earnings for each woman, but can not observe this information in my data (see Section 5.2).

4.2 IV estimation for benefit changes

In order to account for the fact that earnings of the preceding year and hence potential benefits as a function of earnings are potentially endogenous, I apply a grouping-IV estimator to instrument the expected benefits. The following IV assumptions have to hold for instrument Z_{it}:

$$Cov(B_{it}, Z_{it} \mid X_{it}, E_{it-1}, \gamma_t) \neq 0 \tag{2}$$

$$Cov(u_{it}, Z_{it} \mid X_{it}, E_{it-1}, \gamma_t) = 0. \tag{3}$$

To be a valid instrument, the instrumental variable Z_{it} has to induce variation in benefits over time (Equation 2), but also satisfy the exclusion restriction (Equation 3). A natural candidate as instruments for exogenous variation in benefits are education-year interactions. Using education as my grouping-instrument, I exploit changes in fertility across education groups who were differentially affected by the benefit reform.[15]

I differentiate between three education groups: 1) women with at most secondary schooling (low skilled), 2) women who have completed vocational training (medium skilled) and 3) women with tertiary education (high skilled). The reform changed parental benefits differentially across education groups; I estimate the change in expected benefits to be about 1,028 EUR for low-skilled women, but 3,380 EUR for medium-skilled and 7,309 EUR for high-skilled women in 2006.[16]

The exclusion restriction (Equation 3) requires that unobserved differences in the

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

[15] Moffitt and Wilhelm (2000) and Blundell, Duncan and Meghir (1998) discuss the equivalence between DID estimators and (grouping)-IV estimators.

[16] These estimates are for economically active women in the Micro Census 2006, excluding self-employed and civil servants.

likelihood of having a child, given observables, to be constant over time across education groups. In other words, conditional on controls education has to be assumed to not affect fertility trends over time. I test the common trend assumption below. It seems reasonable to assume that composition of education groups (with respect to their unobserved differences in fertility) remains stable before and after the policy reform. I am looking at a relatively short time span; cohorts did not experience major educational reforms over this time span. It is highly improbable that the reform affected educational attainment of the cohorts studied, as educational decisions have been taken before the policy change. Endogenous switching of educations groups in order to increase benefits is unlikely to pose a threat to identification.

In most specifications I restrict my analysis to women aged 25-44 to ensure that women have completed their education and are economically active, so they are represented in the administrative data I am using.[17] I exclude the year 2007 from my main empirical analysis, as mothers giving birth in that year were only partially treated by the reform; the law was passed in autumn 2006, so only individuals giving birth from Summer 2007 onwards could have adjusted their fertility behaviour as a result of the reform.

5 Data

My analysis draws on three different data sources: the German Vital statistics, the German Micro Census as well as administrative data on Insured Persons from the German Pension Registry.

5.1 Vital Statistics

In order to study the time trends in fertility and test for a discontinuous jump in the number of births nine months after the announcement of the reform I use micro-data from the German Vital Statistics (Geburtenstatistik) on all births taking place monthly in Germany for the years 2000-2011. Besides date of birth of child and mother, the data entails information on place of birth, parents' nationality, marital status as well as the parity of the child[18]. I additionally draw on aggregate statistics on quarterly numbers of abortions by marital status for the years 2000-2012. I supplement both data sets with aggregate information on the female population by age and marital status to construct birth rates and abortion rates. All three data sets are provided by the German Statistical Office.

[17] Childbearing for women under 25 is relatively scarce, not even 6% of all births in 2007 were to mothers below the age of 25.

[18] Until 2009 parity is only recorded for married mothers, which make up around 80% of all mothers.

5.2 Pension Registry Data

The main analysis uses administrative data on Insured persons (AKVS) from the German Pension Registry, which is provided by the Research Data Centre of the German Federal Pension Insurance (FDZ-RV). The data includes everyone with a contributory period towards pensions in the reporting year. In Germany, statutory pension insurance is mandatory for all employed persons in the private and public sector, including marginal employment jobs. Not included in the data are civil servants (including teachers) and most self employed.[19] Due to the administrative nature of the data, earnings are measured precisely. The main advantage of the Pension Registry data over the German Social Security records is that it includes precise information on fertility for any woman who has ever been registered with the Pension Insurance, as childbearing entails a contributory period towards pensions. In 2007, 91% of birth recorded in the German Vital Statistics were covered by the Pension Registry data (own calculations).

For my main results I use the Scientific Use File of the Data on Insured Persons of the years 2004-2010, which is a 1% sample of the full population data of Insured Persons and contains information on more than 70,000 women aged 25-44 each year. The data is cross-sectional, but entails information on fertility and employment of the 31.12 in the reporting year as well as the two preceding years and includes information on yearly gross labor earnings for the reporting and the previous year. I restrict my sample to all women aged 25-44 who have had positive labor earnings in the preceding year of the survey year and thus exclude women who have been solely on unemployment benefits in that year to avoid changes in sample composition due to benefit reforms in the earlier 2000s.

I calculate the expected post-reform parental benefits using a detailed parental benefit calculator (http://www.familien-wegweiser.de/Elterngeldrechner), which generates the expected maximum benefits as the sum of monthly benefits over the total entitlement period of 12 months as well as women's net earnings from information on women's gross earnings given in the Pension Registry data.

5.3 Micro Census

To simulate the pre-reform benefits and to test the robustness of my Difference-in-Differences results by education, I employ the Scientific Use file (70 % subsample) of the German Micro Census, an annual cross-sectional survey of a random 1% sample of the German population, for the years 2005-2010. The dataset contains information on

[19] 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 respectively in terms of the 2006 population estimation.

around 65,000 women aged 25 to 45 each year. The survey asks detailed questions on household demographics. As the survey is conducted continuously during the year, I can not determine the probability of having a birth in the survey year from information on births in the current survey year, but instead derive the probability that a woman had a birth in the preceding year from reported children's year of birth retrospectively.[20] Thus using data up to year 2010, I am only able to determine the birth probabilities up to 2009. The data also contains information on number and ages of children in the household, marital status, information on education and vocational training, labour market participation as well as information on the receipt of various benefits. The advantage of the data is that it is representative of the whole population including economically non-active women, but it unfortunately only entails relatively broad income measures for the survey year. A measure of netincome of the preceding month of the survey date exists for every household member and is given in intervals. Information on the netincome of the woman's spouse allows me to simulate the expected pre-reform parental benefits, which in essence depend on partners' income only. I simulate the expected pre-reform benefits using the 2006 Micro Census for various income groups and education-age groups of all women aged 25-45 respetively; I calculate the expected pre-reform leave benefits for each woman by applying the pre-reform benefit eligibility rules to her spouse's net income and aggregate this information for woman's income groups or education-age groups; these averages by observable characteristics are then merged to the pension registry data. I implicitly assume that expectations of benefits are formed on the basis of current income of partners.[21] For my baseline results I define the expected benefits as the sum of monthly payment over the maximum 24-months entitlement period, assuming that mothers maximise their total benefit payments. Details on the simulation of pre as well as post-reform benefits are given in Appendix A.

6 Results

6.1 Descriptive evidence from time series

Before turning to my main regression analysis, I present some evidence for the immediate adjustment of fertility in response to the announcement of the reform. Figure 3 plots seasonality adjusted (residual) monthly number of births per 1000 women over

[20]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.

[21]I also have to assume that assortative matching of partners is the same over time.

the years 2004-2011. I additionally plot a lowess smoother separately for months before and after August 2007 (0-cutoff month), which is 9 months post the final passing of the policy in November 2006. The dotted line (-5) marks March 2007, which is 9 months after the policy was annnounced (May/June 2006). Up until the cutoff, the evolution of monthly births rates appears to be fairly stable. In August 2007, the cutoff month, the birth rate jumps discontinuously and continues to increase after. A regression testing for a discontinuity around August 2007, which accounts for smooth fertility trends through a third order polynomial in the running variable of month of births over time as well as for seasonality in births by accounting for calendar month dummies[22], gives an estimate of about 0.17 (std. error 0.061), which equates to around 3.5% in terms of the pre-August 2007 average monthly birth rate of 4.87 births per 1000 women. The result hence suggests that births per 1000 women increased significantly in August 2007 compared to the previous month by about 3.5%. In Figure 4, I show the evolution of first, second and higher births for the years 2003-2009, expressed as the seasonality adjusted number of births for each parity per 1000 (married) women aged 20-45 on 31.12 in the preceding year.[23] The rate of first born has been fairly stable up until the reform cutoff and exhibits a positive and increasing trend thereafter. The immediate discontinuous increase in birth rates for first born is about 0.11 births per 1000 women, which is 4.8% of the pre-reform mean. The rate of second born and third seems to also react positively to the reform; second birth rates show an immediate increase by 0.12 (6.1 %) and third birth rates by 0.04 births per 1000 women (3.8%), but in contrast to the birth rates for first born the positive response seems to taper off over time. Taken together, both Figure 3 and Figure 4 suggest that the reform increased the aggregate (overall) number of births, with a more lasting effect for the rate of first births (extensive margin).

The discontinuities capture only the very immediate response in successful conceptions once the law was passed and are likely to understate the immediate fertility response to the policy, as it usually takes three to six months for a fertile couple to conceive as González (2013) points out.[24]

Terminations of pregnancies in turn can immediately adjust to the policy. Figure 5 plots seasonality adjusted (residual) quarterly numbers of abortions per 1000 women

[22]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.

[23]Information on the parity (birthorder) of the child is only available for married women, who constitute about 70% of all births in the Vital Statistics. I only have access to the data reporting birth order up to 2009.

[24]Another potential reason for underestimating the immediate fertility response is that I do not observe gestation in the data and thus can not determine month of conception precisely, which leads to measurement error.

aged 25-45, separately for single and married women, for the years 2000-2011. I fit quadratic trends with a 90% confidence interval separately on both sides of the third quarter in 2006. Abortion rates for single women appear to have steadily declined over the period 2000-2011 with no apparent trend change in the third quarter of 2006, when the public became aware of the policy plans. However, for abortion rates for married women, which are admittedly noisy, there appears to be a discontinuous drop in abortion rates around the third quarter of 2006, which is just below 3% of the pre-reform mean in abortion rates. The policy seems to have discouraged abortions for married women, who made up 43% of total number of abortions in 2005, in the short-term.[25]

In my empirical approach I can go beyond a regression-discontinuity type of analysis and can exploit the fact that the reform increased financial incentives differentially across the education and income distribution. Figure 6 shows the raw monthly share of tertiary educated women amongst all births in the pension registry data (calculated on the full population data). We can see that the share of tertiary educated mothers amongst all mothers has already been slightly increasing pre-reform, but is steadily and strongly increasing post-reform from around 10% just before the reform to over 12% in 2010. The trends indicate that the socio-economic composition of births has changed post-reform.

6.2 Baseline results for benefit estimation

In my baseline estimation strategy (1) I exploit the intensity in benefit changes across the net earnings distribution of women to identify the effect of maternity benefit entitlement on fertilility decisions. I compare the change in fertility behaviour post reform at any point of the income distribution and assume that my polynomial in earnings accounts for any time-invariant differences in fertility for women with different earnings. The approach can be thought of as a "continuous" Differences-in-Differences estimator. The polynomial in net earnings serves the same purpose as accounting for group dummies in a usual DID setting.

In a first step, I show a "discretised version" of my approach. I discretise the net earnings distribution using ten intervals, the first nine of length 3000 EUR and the last interval containing all real net earnings beyond 27,000 EUR (which lies above the 95th percentile of 26,800 EUR- equivalent to gross earnings of around 47,000 EUR). I estimate a linear probability model controlling for these earnings interval dummies, the

[25] There might be various reasons which can explain the heterogeneous response in abortion rates by marital status. Married wome might either benefit from higher financial benefits through the reform due to higher pre-birth earnings or are generally more responsive to financial incentives as they are more inclined to keep an "unplanned" child due to a potentially more stable socio-economic situation with potential financial support through their spouse in times of economic inactivity.

interaction of these intervals with a post-reform dummy as well as controls from our baseline specification.[26] The dependant variable is expressed as births in 1000 women for ease of interpretation.

I plot the coefficient estimates (and 95% confidence intervals) for the earnings interactions with post-reform dummies in Panel A of Figure 7. The graph shows a very different evolution in birth probabilities along the earnings distribution. Women in the lower part of the earnings distribution up to the earnings interval 9,000-12,000 EUR (midpoint 10,500) appear not to be more likely to have a child post reform. However, women who are approximately above the 50th percentile (Median: 11,540 EUR (around 17,300 EUR gross)) of the net-earnings distribution have significantly increased their probability to have a child in any year of the three years post reform by around 6-7 children in 1000 women (0.6-0.7% 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 1000 children). Panel B plots the increase in benefits post versus pre-reform which increases steadily with earnings. The fact that the post-reform increase in childbearing for above-median earners of Panel A is relatively stable despite continuously increasing maternity benefits implies that the effect of paid leave on fertility might have been stronger for the middle-upper part of the income distribution than for the top end.

Table 3 reports my baseline estimates for equation (1). I allow for very flexible age effects by controlling for age dummies as well as controlling for differential age dummies for tertiary educated women. I further account for education group dummies, a dummy for German nationality, indicators whether the woman has had a child in t-2 or t-1 as well as year and region dummies. My approach requires the inclusion of a flexible function of lagged net income (equation 1); I chose a fifth-order polynomial as a more conservative choice, but explored different-ordered polynomials and found that estimates are very similar from order two onwards.

Benefits are defined as the sum of benefits in 1000 EUR (in 2010 prices) a women would be entitled to receive if she had a child. The baseline result in column (1) implies that an increase in the total expected benefits of 1000 EUR increases the probability that a woman gives birth in each year of the three years post reform by 0.475 births in 1000 women. In terms of the average pre-reform birth probability (39.4 births per 1000 women), this implies an increase in births by 1.2% per 1000 EUR increase in total benefits. My estimates are robust to the inclusion of region specific trends (column

[26] I estimate the following specification, $P(Child)_{it} = \alpha + \sum_{e=1}^{10} \beta_e (R_t * d_{eit}) + X'_{it}\delta + \sum_{e=1}^{10} \gamma_e d_{eit} + u_{it}$, 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, R_t is a post (2007)-reform indicator.

(2)). In column (3), I allow for a more general form of the control function which I allow to vary by women's education level. This addresses the concern that the effect of net earnings on fertility decisions differs by women's characteristics. The more general control function does not change the estimated benefit effect. In (4)-(6), I present alternative specifications using different definitions of the simulated pre-reform benefits. So far I have expressed pre-reform benefits as the simulated sum of benefits under the 24-months entitlement period option, under which a maximum of 300 EUR a month was paid out (max. 7200 EUR in total). Women however were able to opt for a shorter entitlement period of a maximum of 450 EUR for 12 months (5400 EUR in total). This option was more attractive for mothers who wanted to return to the labor market (full-time) after 12 months and was mostly popular amongst East German women. In column (4), I define the pre-reform benefits as the expected sum of benefits under the shorter 12-months option, which has little effect on my estimated coefficient. In the preceding results, I have simulated and matched the pre-reform benefits by women's income. In column (5) I alternatively simulate the expected pre-reform benefit for education specific age groups (5 year-intervals). The estimated benefit effect is slightly larger, but not statistically significantly different from the baseline estimates. As the sample in (5) differs slightly[27], I estimate my baseline specification (column (1)) on that specific sample and find very similar estimated effects (column (6)). Overall, the results appear robust to alternative benefit definitions.

Robustness of benefit estimates to functional form: Table 4 presents several specifications exploring the robustness of the baseline estimates to the functional form chosen. In Specification A, I relate the parental benefits directly to the opportunity costs of birth, proxied by the net earnings in t-1. I define the benefits in terms of their replacement-ratio of net earnings, which ranges between 12% for high earners and 160% for very low earners pre 2007, but lies above 67% for almost all women post reform.[28] The mean increase in the replacement ratio due to the reform is about 29%-points with a standard deviation of 9.55 (estimated on the pension data for 2006). I estimate that an increase in the replacement ratio of 10%-points increases the probability to have a child by 0.92 births in 1000 women, which is an increase of 2.4% in terms of the average pre-reform probability.

The effects of additional benefits might be decreasing with the benefit level. I alternatively use the log of the total expected benefits as my explanatory variable (as well as a polynomial in log real earnings) in Specification B. An increase in benefits by 10% would increase the probability of having a child by 0.43 births per 1000 women,

[27]It excludes women with unknown education.

[28]Women with net earnings above about 33,000 EUR 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.

which is an increase of 1.11% in terms of the mean pre-reform birth probability. This implies an average benefit elasticity of 0.11.

6.3 Reduced form effects by education and validity of instruments

In order to identify a causal effect when estimating equation (1) , I assume that women do not (endogenously) adjust their earnings in response to the reform, which would violate the assumption that the relationship between earnings and fertility is constant over time. As discussed in Section 4, education-year interactions can serve as instruments for the benefits a women would be entitled to.

Reduced form estimates: Before reporting my IV results, I report "Reduced form" results of my IV approach.[29] Table 5 shows the results of a differences-in-differences analysis using the administrative pension data for the years 2004-2006 and 2008-2010, testing for differential trends of medium (i.e. women who completed an apprenticeship) and high skilled women (i.e. women with tertiary education) relative to low-skilled women (i.e. without postsecondary education) post reform (for a more detailed description of the educational coding see Appendix A).

The probability to give birth for high-educated women increases by 6.9 per 1000 women post reform, whereas for medium educated women it increases by about 2.75 births in 1000 women, both relative to the change in the birth probability for low educated women (see column (1)). Evaluated in terms of the underlying average pre-reform birth probabilities for women aged 25-44, which are 43.51 in 1000 for medium-skilled and 54.57 in 1000 women for high-skilled women, the estimates imply an increase in the likelihood of births due to the reform of about 6% for medium educated and 13% for high educated relative to low educated.

In Table 6 I can combine the reduced form estimates with the simulated increases in expected parental benefits for the different education groups to calculate the benefit elasticity for both skill groups. Matching the pre-reform benefits from the Micro Census 2006 to the pension registry data for 2006, I simulate that expected maternity benefits for women in my sample change by about 2352 EUR for medium-skilled and by around 5332 EUR for tertiary educated women relative to the change for low-educated. Evaluated against the estimated pre-reform benefit in 2006 of around 4760 EUR for medium-educated and 4930 EUR for high educated the relative change in the benefit level is 49% for medium skilled and 108% for high educated. The estimated benefit

[29] I estimate the specification $P(Child)_{it} = \alpha + \beta_0 Deducmed_{it} * R_t + \beta_1 Deduchigh_{it} * R_t + X'_{it}\beta_2 + \gamma_t + u_{it}$, where $Deducmed_{it}$ and $Deduchigh_{it}$ are dummies for medium and high education and R_t is a post-(2007) reform dummy.

elasticity for both groups is about 0.12; a 10% increase in expected benefits raises the probability of having a child by 1.2%.[30] These elasticities fall into the range of previous estimates despite the fact that the existing literature analyses financial incentives targeted at very different groups as discussed in 2.

I can also use these simulated benefit changes based on the 2006 sample to calculate a simple Wald estimator by dividing the estimated coefficient for each education group by its simulated reform effect for 2006. The calculated Wald estimate is 1.17 for medium-educated and 1.33 for high-educated, and indicates that a 1000 EUR increase in expected benefits raises the likelikood of having a child in any given year of the three years post reform by 1.17 births in 1000 women for medium and 1.30 births in 1000 women for high-educated.

Validity of instruments: I perform several robustness checks on my baseline estimate to test that the sample composition or differential trends do not corroborate my findings. Accounting for additional socio-economic background variables (column (2) of Table 5), such as type of work in previous year, social benefit receipt and whether a woman has had a child in the two preceding years, does not change the estimated coefficients. The results prove robust when I account for region-specific year effects (column (3) of Table 5) to capture regional trends as well as when I exclude the time dummies (and only account for a post-reform dummy). The results suggest, that fertility over the time period studied seems to have not been driven by strong trends.

The exclusion restriction in (3) requires the change in fertility before and after the reform to have been the same across education groups, conditional on observables, in the absence of the reform. Differential time trends in fertility across education groups due to economic shocks or shifts in preferences, which affect education groups differentially would violate this assumption.[31]

The pension data I use is only available from 2004 onwards, so I do not have data from longer pre-period to test for differential fertility trends across the groups. The estimated coefficients from a placebo-reform for 2006 using data for the pre-reform period only are both small and not statistically significant. In an additional check, I allow the effect to vary by year in my reduced form estimation. I estimate an augmented reduced form equation by including interactions for the two treatment groups with year dummies relative to the baseline year 2004. Figure 8 shows the estimated coefficients and confidence intervals for these interaction terms. For both medium (Panel A) as well

[30] The benefit elasticity is calculated as the %-change in fertility divided by the %-change in benefits, i.e. 0.13/(5332/4930)≈0.12 for high-educated women. Note however, that the estimated elasticity depends crucially on the choice of mean benefits and fertility level we evaluate against.

[31] In contrast to most other European countries, the financial crisis did not have a lasting impact on the German economy due to a strong economic upward trend which started in the mid 2000s. Unemployment rates have been falling between 2006 and 2011 across all education groups. For a more detailed account on the evolution of employment rates see Weber and Weber (2013).

as high educated (Panel B) I estimate consistently positive differentials relative to the lower educated group after the reform; for medium educated the differential is highest in 2008, one year after the reform, but remains positive for the years 2009 and 2010. For high educated the estimated differential grows steadily over the years 2008-2010. Prior to 2007, none of the differentials are statistically significant. If differential time trends were biasing my fertility results upwards, I should already find positive differentials in the pre-reform period relative to 2004 as well as positive effects for my placebo reform, which is not the case.

My instrumental variable strategy can deal with endogenous adjustment in earnings as a result to the reform. My results shown above are estimated on the pension registry data which is restricted to women with positive labor earnings in the preceding year. An additional concern for identification is that the reform might have also changed the participation decision of women, which might have led to changes in the composition of education groups with respect to unobservables in my sample post reform. The incentives for changes in the extensive (participation) margin of labour supply as a result of the reform are unlikely to be high due to the smoothness of the benefit function; inactive mothers are guaranteed a minimum payment, which is smoothly increasing in netincome once they increase their labor supply. I have also estimated the reduced form results using education groups on data from the Micro Census, which also contains economically non-active women.[32] The results are shown in Table 7 and are robust to the inclusion of region specific time effects and additional controls and can confirm the positive reform impact for the high educated.

6.4 IV results using education

In Table 8, I present the IV results for my benefit estimation. In column (1) I report my baseline estimates from Table 3. In column (2) I show the estimate for my discretised version of (1), where I instrument the continuous benefits with interactions between a post-reform dummy and dummies for the ten discrete earnings-groups.[33] The estimated coefficients are very similar to the ones in column (1). In column (3) I instrument the benefits with education-year-interactions exploiting variation in education-year specific mean benefits for identification. Education groups can explain a lot of the variation in benefits over time: The partial R2 of the education-year-interactions in the first stage is 0.097 with a partial F-statistic of 4,565. The IV point estimate of 1.14 birth per 1000

[32]The Micro Census further contains employed groups not covered by the public pension insurance, i.e. civil servants as well as most self-employed. I can only construct the probability of giving birth for years 2004-2009.

[33]Whether I account for the polynomial in earnings or the ten earnings-group dummies does not alter the results.

women is larger, but not statistically significantly different from the baseline estimate in column (1). The estimate implies that an increase in parental benefits a woman would be entitled to by 1000 EUR raises her probability to have a child in a respective year over the three years after the reform by 1.14 births in a 1000 women, which is an increase of 2.9% in terms of the pre-reform mean.[34]

The change in expected maternity benefits is increasing in earnings as a result of the reform. The baseline linear estimator in Equation (1) (or its discretised version shown in column (2)) captures the effects of benefits for those who experienced the biggest increase in benefits due to the reform, which are women at the upper end of the earnings distribution.

I discussed earlier (see Figure 8) that the increase in fertility due to the reform was relatively stable along the medium and upper-end of the earnings distribution despite the fact that benefits are increasing continuously with earnings. In Figure 9 I plot the change in the probability for a 1000 EUR increase in benefits for each earnings interval, which I obtain by dividing the fertility estimate in Panel A of Figure 8 by the increase in benefits of Panel B for each earnings interval. The point estimate is close to 2 births per 1000 women (per 1000 EUR increase in benefits) for the net earnings interval 12,000-15,000 and is decreasing to around 0.5 for the upper part of the net earnings distribution (24,000 and beyond). When I exclude women with real net earnings above 20,000 EUR, which is equivalent to the top 20% of earners, the baseline benefit estimate increases to 1.023 (column (4)), which is a point estimate very close to the IV estimate in column (3).[35]

Given that I do not find the IV estimates to be statistically significantly different from the baseline estimates, I will base my discussion of the reform responses of different subgroups in the next section on the baseline benefit specification.[36] Endogenous adjustment of income as a response to the reform seems to not be a big concern: I can not find any evidence that women giving birth are more likely to increase working hours or change jobs the year before giving birth in the post-reform vs. the pre-reform period, using the switching behaviour of women beyond their fertile age as a comparison group. Secondly, I analyse a short time-period, so considerable adjustments in earnings seem infeasible.

[34] Note that these estimates are very close to the Wald estimates I calculate in Table 6.

[35] The education-year-interactions as instruments use variation in the mean benefits across education groups and capture the reform effects on the middle and upper-middle part of the earnings distribution.

[36] When I instrument with education I obtain higher point estimates, but the patterns I find are unchanged.

7 Heterogeneity in estimates

7.1 Effects by age

When interpreting my results I have stressed that my estimates reflect the immediate impact of financial incentives on current fertility rather than the impact on permanent fertility. Women may change the timing of children rather than the total number of children they will have during their fertile years. The crucial question is whether an increase in financial incentives in fact affects the total number of children a woman wants to have and raises completed fertility levels in the longer run.

As the policy was borne out of a newly formed grand coalition between the two biggest parties, the Christian Democrats and Social Democrats, the reform was perceived as a permanent change of family politics rather than a short-term policy measure, as stressed by both parties. There is limited scope to believe that there were strong incentives for women to bring their births forward out of the fear that the policy would be abolished in the future. Women could expect changes to the benefit system and hence to their childbearing costs to be permanent.[37]

Because the reform was implemented recently and I only observe fertility behaviour three years post reform, I can not identify the effect on total fertility for all women. Younger women might not react to the policy immediately as the change in the benefit system might incentivise them to invest in their career in order to benefit from higher maternity benefit entitlements in the future. For women closer to the end of their fertility period an increase in financial incentives however is likely to lead to permanent increases in fertility. Up until the reform was announced, these women had planned their fertility under the old benefit system. Post-reform, they face higher financial incentives for an additional birth under the new maternity benefit system, which might induce them to have a(n additional) child. Facing the new benefit system, I expect older women to not postpone an additional birth for too long and adjust their fertility fairly fast; their careers are already established at that point and -probably most importantly- their remaining time to conceive is very limited with the probability to conceive decreasing over their remaining fertile years. A positive benefit effect on fertility over the three post-reform years for older women is likely to reflect an impact on their completed fertility.

Panel A in Table 9 shows results for separate regressions of my baseline benefit equation for four age categories. For the younger age groups, 25-29 and 30-34, I can not find a statistically significant effect of increased maternity benefits on their short-run fertility for the three years post reform. Older age groups however seem to have

[37] This might explain the fact that I find smaller jumps in fertility rates in my discontinuity than for example González (2013).

increased their fertility in response to an increase in potential maternity benefits; an increase in benefits a woman would be entitled to by 1000 EUR raises the probability to have a child in a respective year over the years 2008-2010 by 0.9 births per 1000 women for age group 35-39 and by 0.2 births per 1000 women for the age group 40-44. The magnitude of the effect in terms of the underlying probability to give birth is greatest for women aged 40-44, for which an additional 1000 EUR of benefits increased the probability to have a child in a respective year by nearly 4%. The results for these older age groups suggest that the increased financial incentives had a *permanent* effect on fertility and will increase the completed fertility of these cohort.

I check the "robustness" of my results to the exclusion of the top 10% of net earners in each age group and display the results in Panel B. When excluding the top earners, I find a larger and statistically significant positive effect of benefits on fertility for the age group 30-34. A regression exploring the increases in probability along the ten earnings intervals of the earnings distribution (as shown in Figure 7) for this specific age group reveals that women in the upper-middle part of the age-specific earnings distribution - between 12,000 EUR net earnings (median) to below 24,000 EUR net (90th percentile) - increase their fertility post reform. Women aged 30-34 with earnings above the 90th percentile however do not seem to be more likely to give birth in the three years post reform.[38] The estimated effects for the older age groups of Panel A on the other hand are robust to the exclusion of the top 10% earners (columns (3) and (4) in Panel B).

7.2 Effect on Parities

The evidence presented so far shows that the reform had an effect on the overall probability to have a child. The reform might have however affected first births differently than second or higher-order births. As mentioned in 5.3, only the Micro Census provides detailed information on number and age of children in the household. In Table 10, I present results for different birth orders from estimating the (reduced form) DID specification with education groups using data from the Micro Census. Panel A shows results by birth order over all ages. We can see that the reform had a strong impact on first births of high-educated mothers. We have to bear in mind that the results reported are reduced form estimates. I can not scale them by the increase in expected benefits by parity as I do not observe individual pre-birth earnings in the Micro Census. We would however expect the change in benefits for women who do not have any children to be larger than for women who are already mothers, as most childless women will be working full-time. The fact that the financial incentives are likely to be highest for

[38] Women aged 30-34 at the top end of the earnings distribution might simply delay their fertility adjustment to an older age and still prefer to invest in their career.

childless women could explain why the probability to have a first child was affected the most by the reform.

In Panels B and C, I show results for the first and second child for different age groups. The positive impact on first births for high-educated mothers from Panel A seems to be driven by increases in first births for the age group 30-35 (column (2), Panel B). The age at first birth for highly-educated women decreased by 0.4 years from the pre-reform to the post-reform period. Taken together, the results indicate that the reform induced highly-educated women to start their childbearing slightly earlier. There is evidence in the literature that postponement of the first birth has negative impacts on fertility levels (see Bratti and Tatsiramos (2012)). If the policy is successful at inducing higher-educated women to have their first child earlier, it will potentially be successful at increasing their subsequent fertility level as well.

The positive effect in column 4 of Panel C suggests that the reform increased the probability to have a second child for highly-educated women nearing the end of their fertile years. I estimate the age at second birth of high educated to have increased by about 0.4 years between the pre and post-reform period. The effect on second births for the age group 40-45 most likely reflects an increase in permanent fertility, a finding I have established in the previous section. Highly-educated women appear to be more likely to have two children in total instead of one child during their childbearing years as a response to the reform.

7.3 Importance of societal norms - East vs. West

The German setting might shed some light on the question whether differences in cultural and societal norms can play a role in determining the effectiveness of public policy. Table 11 shows results from separate regressions for women residing in West and East Germany (incl. Berlin). East German women seem to react more strongly to an increase in financial incentives. Conditional on socio-economic characteristics incl. age and pre-birth earnings, an increase in expected parental transfers by 1000 EUR raises the probability of having a child in the three years post reform by 0.4 births per 1000 women for West German women, but by 0.85 additional births per 1000 women for East German women, which equates to an increase in the birth probability of 1% vs. 2.2% in terms of the pre-reform mean.[39] Which factors can potentially explain the seemingly stronger incentive effects of increased parental benefits on East German women?

Social norms regarding mothers' employment differ fundamentally between East

[39] I also find higher estimated reform effects for East German women when I instrument with education.

and West. During the divided years, East German institutions strongly encouraged female employment as well as fertility in the socialist tradition. East German women were encouraged to return to work full-time after one subsidised "baby year" whilst their children were looked after at universal early childcare centres. West German society held more traditional views on female and in particular mothers' labour market participation; this was reflected by its conservative family and tax policy, which disincentived women from working, especially full-time. Even more than twenty years after the reunification, there are still substantial differences in terms of female labour market behaviour across East and West Germany. East German women return to work faster after childbirth and work longer hours. In the pre-reform years 2004-2006 about 38% of East German mothers worked at least 16 hours a week after two years post childbirth versus only 20% of West German women. This differential return behaviour is also reflected by the fact that under the pre-reform parental benefit system the shorter 12-month option, which paid less in total than the regular 24-months option but allowed full-time work in the second year after childbirth, was disproportionally taken up by East German women who were intending to return to work full-time. Bauernschuster and Rainer (2011) show using German social survey data for 1991-2008 that East German men and women are significantly more likely to hold egalitarian sex-role attitudes regarding mother's employment than West Germans and find that these differences are persistent - if not increasing - over time.[40]

Due to these different social norms on mother's employment, opportunity costs of childbearing differ between East and West. The foregone earnings of East German women after childbirth are lower.[41] An increase in parental benefits by the same amount will cover a greater share of a woman's opportunity costs, and reduce the price for having a child more strongly for East than for West German women. My finding that an increase in short-run fertility due to a 1000 EUR increase in benefits is twice as high for East than for West German women strongly supports this argument.

My estimation results indicate that implementing an earnings-related parental benefit system in a society or country with higher female labor force attachment and hence lower opportunity cost of childbearing might be more effective in increasing fertility than in a society with more traditional gender roles.

[40]According to the authors' results, being East Germany reduces the likelihood of agreeing with statements such as "It is better for all if the husband works and the wife stays at home taking care of the household and the children" by 22 percentage points.

[41]A faster return to work might additionally limit the depreciation of human capital, which is another factor determining the price of childbearing.

8 Discussion and Conclusion

Understanding the financial incentives effects of family policies on fertility is an important research question, but has been plagued by the lack of exogenous variation in financial incentives. Besides providing cash-transfers to families, many governments provide paid parental leave in order to facilitate family and career compatibility and lower the cost of childbearing. The design of benefits however varies considerably across countries. Flat parental benefits -similar to cash-subsidies- translate into strong differences in replacement rates across the earnings distribution. Increases in flat transfers will mostly benefit mothers with lower earnings pre-birth. Transfers which are increasing in pre-birth earnings and have constant replacement rates compensate women's private opportunity cost of children accordingly. However, it has not been studied in the literature how different benefit systems affect the socio-economic composition of fertility, which has important implications for future generations.

I exploit a major parental leave reform in Germany implemented in 2007 to examine how a shift from a parental leave benefit system which targets lower-income families to a system which closely ties benefits to pre-birth earnings and pays higher benefits to higher-earning women affects fertility. My empirical approach exploits the large differential changes in parental benefits across education and income groups by up to 21,000 EUR as exogenous source of variation in benefits a mother would be entitled to.

I find that the parental benefit reform led to (short-run) increases in overall fertility by documenting discontinuous jumps in monthly birth rates nine months after the reform was passed. I show that the probability to have a child in a given year three years post reform increases by 6% for medium educated and by 13% for high educated, both relative to low educated. These results indicate that the reform changed the socio-economic structure of fertility. My baseline instrumental variables estimates suggest that a 1000 EUR increase in total benefits a woman would be entitled to if she had a child raises the probability to give birth by 1.2%. I estimate that the reform increased the fertility of women of all earning groups beyond median earnings including the top 5th percentile. The effect of a 1000 EUR increase in benefits however seems to be decreasing with pre-birth earnings.

The observed fertility response up to four years post reform might be transitory rather than permanent. The strong financial incentive effects for women aged 35-39 and 40-44 I estimate however suggest that the positive effect on (short-run) fertility is likely to have a permanent effect - at least for these subgroups. I find that high educated women are more likely to have a(n additional) second child at the end of their fertility cycle under the new benefit system than under the old one. I also find that the reform induced higher-educated women to have their first child at a slightly younger age.

My findings suggest that earnings-related parental leave benefits are successful at increasing fertility overall. Moreover, the reform successfully raised fertility in particular for women with higher opportunity costs of childbearing, who were given very low financial incentives under the previous flat benefit scheme. A more complete answer to whether earnings-related parental leave benefits can substantially narrow the fertility gap between education groups can only be sought once affected groups have completed their fertile years. An interesting avenue for future research would be to explore the dynamic implications on labour market behaviour of women as a result of the reform as well as studying the effects of changes in the socio-economic structure in fertility on the children's generation.

References

Abbring, Jaap H. and Gerard J. Van Den Berg. 2003. "The Nonparametric Identification of Treatment Effects in Duration Models." *Econometrica* 71(5):1491–1517.

Bauernschuster, Stefan and Helmut Rainer. 2011. "Political regimes and the family: how sex-role attitudes continue to differ in reunified Germany." *Journal of Population Economics* 25(1):5–27.

Baughman, Reagan and Stacy Dickert-Conlin. 2003. "Did Expanding the EITC Promote Motherhood?" *American Economic Review* 93(2):247–251.

Björklund, Anders. 2006. "Does Family Policy Affect Fertility." *Journal of Population Economics* 19(1):3–24.

Black, Sandra E. and Paul J. Devereux. 2011. Chapter 16 - Recent Developments in Intergenerational Mobility. In *Handbook of Labor Economics*, ed. David Card and Orley Ashenfelter. Vol. Volume 4, Part B Elsevier pp. 1487–1541.

Blundell, Richard, Alan Duncan and Costas Meghir. 1998. "Estimating Labor Supply Responses Using Tax Reforms." *Econometrica* 66(4):827–861.

Boschini, Anne, Christina Håkanson, Åsa Rosén and Anna Sjögren. 2011. Trading off or having it all? Completed fertility and mid-career earnings of Swedish men and women. Working Paper Series 2011:15 IFAU - Institute for Evaluation of Labour Market and Education Policy.

Bratti, Massimiliano and Konstantinos Tatsiramos. 2012. "The effect of delaying motherhood on the second childbirth in Europe." *Journal of Population Economics* 25(1):291–321.

Brewer, Mike, Anita Ratcliffe and Sarah Smith. 2011. "Does welfare reform affect fertility? Evidence from the UK." *Journal of Population Economics* 25(1):245–266.

Bujard, Martin. 2012. *Talsohle bei Akademikerinnen durchschritten? Kinderzahl und Kinderlosigkeit in Deutschland nach Bildungs- und Berufsgruppen.* BiB working paper. 2012/04.

Bundesamt, Statistisches. 2006. "Erziehungsgeldstatistik 2006.".

Bundesamt, Statistisches. 2010. "Mikrozensus 2008- Neue Daten zur Kinderlosigkeit in Deutschland.".

Bundesregierung. 2006. "Rede von Bundeskanzlerin Angela Merkel beim Arbeitgebertag 2006." http://archiv.bundesregierung.de.

Cohen, Alma, Rajeev Dehejia and Dmitri Romanov. 2013. "Financial Incentives and Fertility." *Review of Economics and Statistics* 95(1):1–20.

Dahl, Gordon B and Lance Lochner. 2012. "The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit." *American Economic Review* 102(5):1927–1956.

Demeny, Paul. 1986. "Pronatalist Policies in Low-Fertility Countries: Patterns, Performance, and Prospects." *Population and Development Review* 12:335–358.

Feldstein, Martin. 1995. "The Effect of Marginal Tax Rates on Taxable Income: A Panel Study of the 1986 Tax Reform Act." *Journal of Political Economy* 103(3):551–572.

Fogli, Alessandra and Raquel Fernandez. 2009. "Culture: An Empirical Investigation of Beliefs, Work, and Fertility." *American Economic Journal: Macroeconomics* 1(1):146–77.

Gauthier, Anne Helene and Jan Hatzius. 1997. "Family Benefits and Fertility: An Econometric Analysis." *Population Studies* 51(3):295–306.

González, Libertad. 2013. "The Effect of a Universal Child Benefit on Conceptions, Abortions, and Early Maternal Labor Supply." *American Economic Journal: Economic Policy* 5(3):160–188.

Gruber, Jon and Emmanuel Saez. 2002. "The elasticity of taxable income: evidence and implications." *Journal of Public Economics* 84(1):1–32.

Heckman, James J. and James R. Walker. 1990. "The Relationship Between Wages and Income and the Timing and Spacing of Births: Evidence from Swedish Longitudinal Data." *Econometrica* 58(6):1411–41.

Hertz, Tom, Tamara Jayasundera, Patrizio Piraino, Sibel Selcuk, Nicole Smith and Alina Verashchagina. 2008. "The Inheritance of Educational Inequality: International Comparisons and Fifty-Year Trends." *The B.E. Journal of Economic Analysis & Policy* 7(2).

Hotz, Joseph V., Jacob Alex Klerman and Robert J. Willis. 1997. Chapter 7 The economics of fertility in developed countries. In *Handbook of Population and Family Economics*, ed. Mark R. Rosenzweig and Oded Stark. Vol. Volume 1, Part A Elsevier pp. 275–347.

Keane, Michael P. and Kenneth I. Wolpin. 2010. "The Role of Labor and Marriage Markets, Preference Heterogeneity, and the Welfare System in the Life Cycle Decisions of Black, Hispanic, and White Women*." *International Economic Review* 51(3):851–892.

Kluve, Jochen and Marcus Tamm. 2013. "Parental leave regulations, mothers' labor force attachment and fathers' childcare involvement: evidence from a natural experiment." *Journal of Population Economics* 26(3):983–1005.

Kohls, Martin. 2010. "Das demografische Verhalten der auslï¿œndischen Bevï¿œlkerung in Deutschland Analysen mit Daten des FDZ-RV." *DRV-Schriften* 55:149 – 173.

Lalive, Rafael and Josef Zweimüller. 2009. "How Does Parental Leave Affect Fertility and Return to Work? Evidence from Two Natural Experiments." *The Quarterly Journal of Economics* 124(3):1363–1402.

Laroque, Guy and Bernard Salanié. 2013. "Identifying the Response of Fertility to Financial Incentives." *Journal of Applied Econometrics* p. n/a–n/a.

Manski, Charles F. and Joram Mayshar. 2003. "Private Incentives and Social Interactions: Fertility Puzzles in Israel." *Journal of the European Economic Association* 1(1):181–211.

Milligan, Kevin. 2005. "Subsidizing the Stork: New Evidence on Tax Incentives and Fertility." *The Review of Economics and Statistics* 87(3):539–555.

Moffitt, R. 1998. The effect of welfare on marriage and fertility. In *Welfare, the Family, and Reproductive Behavior: Research Perspectives*, ed. R. Moffitt. National Academies Press pp. 50–97.

Moffitt, Robert A. and Mark Wilhelm. 2000. Taxation and the Labor Supply - Decisions of the Affluent. Economics Working Paper Archive 414 The Johns Hopkins University,Department of Economics.

Nielsen, Helena Skyt, Torben Sørensen and Christopher Taber. 2010. "Estimating the Effect of Student Aid on College Enrollment: Evidence from a Government Grant Policy Reform." *American Economic Journal: Economic Policy* 2(2):185–215.

Ratcliffe, Anita and Sarah Smith. 2006. Fertility and Women's Education in the UK: A Cohort Analysis. The Centre for Market and Public Organisation 07/165 Department of Economics, University of Bristol, UK.

Rosenzweig, Mark R. and T. Paul Schultz. 1985. "The Demand for and Supply of Births: Fertility and its Life Cycle Consequences." *The American Economic Review* 75(5):992–1015.

Rothstein, Jesse and Cecilia Elena Rouse. 2011. "Constrained after college: Student loans and early-career occupational choices." *Journal of Public Economics* 95(1–2):149–163.

Sinclair, Sarah, Jonathan Boymal and Ashton De Silva. 2012. "A Re-Appraisal of the Fertility Response to the Australian Baby Bonus." *Economic Record* 88:78–87.

Thévenon, Olivier and Anne Solaz. 2013. Labour Market Effects of Parental Leave Policies in OECD Countries. OECD social, employment and migration working papers Organisation for Economic Co-operation and Development Paris: .

U. S. Census Bureau. 2010. "Fertility of American Women: 2010 – Detailed Tables." Internet. Fertility. Data section. Demographic data and reports from the U.S. Census Bureau.
URL: *http://www.census.gov/hhes/fertility/data/cps/2010.html*

Weber, Brigitte and Enzo Weber. 2013. "Qualifikation und Arbeitsmarkt, Bildung ist der beste Schutz vor Arbeitslosigkeit." *IAB Kurzbericht* 4.

Whittington, Leslie A., James Alm and H. Elizabeth Peters. 1990. "Fertility and the Personal Exemption: Implicit Pronatalist Policy in the United States." *The American Economic Review* 80(3):545–556.

Willis, Robert J. 1973. "A New Approach to the Economic Theory of Fertility Behavior." *Journal of Political Economy* 81(2):S14–64.

Appendix A: Details on benefit simulation and data coding

Details on benefit calculation

Pre-reform benefits: Pre-reform benefits were means-tested on household income during benefit receipt. I simulate the pre-reform benefits a woman would be entitled to if she gave birth to a child using information on current net income for the woman and her spouse, who is defined as either her husband or cohabitating partner, from the Micro Census 2006. I restrict the sample to all women aged 25-44 in the survey year. From the net monthly income variable, which is provided in 24 intervals, I approximate the net yearly income. The eligibility thresholds for the pre-reform benefits were based on the net household income during benefit receipt, excluding income from public transfers. The income variable reported in the Micro Census includes social assistance as well as unemployment benefits which I then set to zero for individuals who are reported to be unemployed or inactive during the survey period. I also pool all observations for net income above 43,200 EUR as numbers of observations for very high earnings are very small. I simulate the maximum benefits a women would be entitled to on basis of the generated net labour income of a woman's spouse, assuming that the woman would not have any labour income during benefit receipt. I calculate the potential benefits applying the eligibility rules for benefit receipt to the current income of the spouse: I assign the maximum benefit of 7,200 EUR (5,400 EUR for option 2) to women with a spouse with earnings lower than 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 and a maximum of 7,200 EUR (2,700 EUR and 5,400 EUR for the short option) and 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 be eligible for benefit payments of a duration of six months, which equals 1,800 EUR in total (both options). Women with spousal net income above 29,400 EUR would not be eligible for any maternity benefits. In case the woman does not have a partner in the household, she is assigned the maximum benefit as her family income during benefit receipt would lie below the income threshold. As some of the eligibility cut-offs fall into an income interval, I calculate the mean of simulated benefits applying the lower bracket of the income interval and the upper bracket respectively.

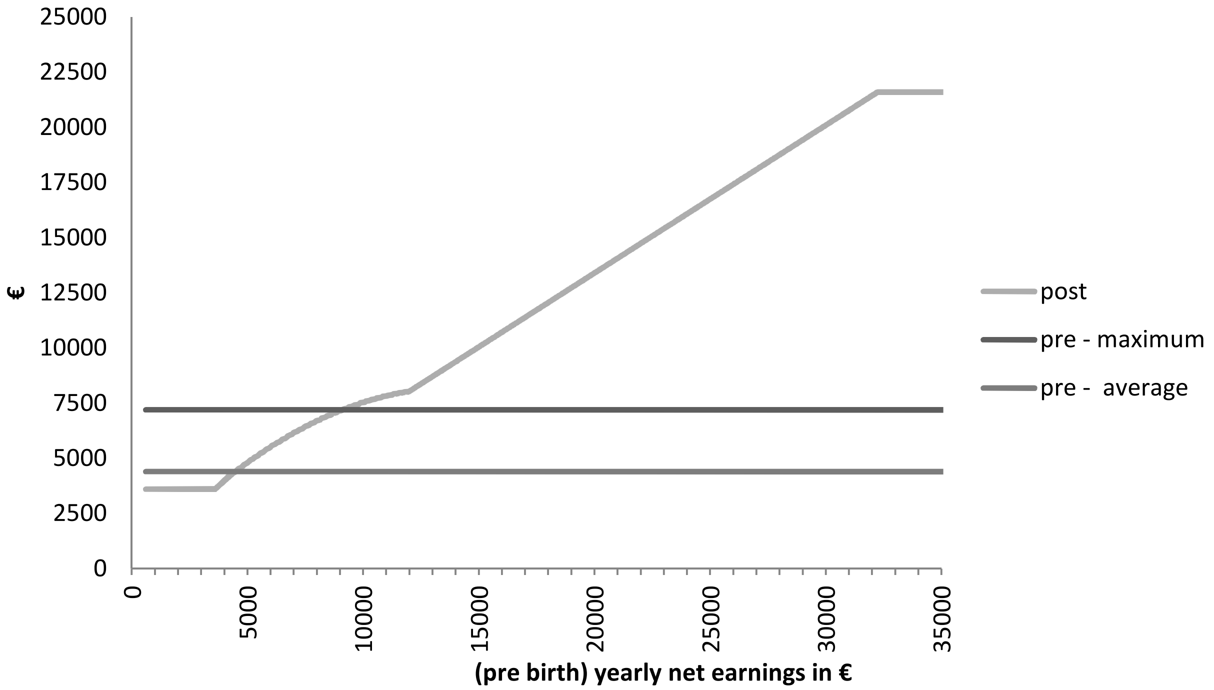
In my analysis, I focus on women's labour earnings: For my baseline results I collapse the simulated pre-reform benefits, which are based on spouse's income information as described, by the respective earnings intervals of women aged 25-44. I can merge the simulated benefits by earnings intervals to the pension data used in my main analysis. Alternatively, I also simulate the pre-birth earnings for age-specific (5 age group) education groups, which generates 15 distinct values for simulated pre-reform benefits. I can then merge these values by age and education to the pension data.

Post-reform benefits and net earnings: Benefits under the new Elterngeld rule are a direct function of women's labour earnings (independent of spousal earnings). I use a detailed parental benefit calculator (http://www.familien-wegweiser.de/Elterngeldrechner) which generates the expected maximum benefit as the sum of monthly benefits over the total entitlement period of 12 months on the basis of information on women's gross yearly earnings, which I observe in the Pension Registry data. As women can choose between different tax classes under the German system of joint taxation of couples, I assume women to be taxed under tax class IV (equivalent to the tax class of singles (I)), which is the tax class chosen by couples with relatively equal earnings. I also calculated the benefits applying alternative tax classes, which does not affect my estimates a lot. I assume for simplicity that women do not have any children, which would otherwise increase my calculated benefits by a small sibling premium. I calculate the net labour earnings I use in my estimation strategy applying an implicit tax rate, which is provided by the the benefit calculator, to the gross yearly labour earnings.

Details on education coding

In my analysis I define education groups according to the German educational system and differentiate between low educated (i.e. women without postsecondary education (equivalent to category 1 and 2 of ISCED97)), medium educated (i.e. women who completed an apprenticeship (equivalent to 3, 4 and 5b of ISCED97)) and high educated women (i.e. women with tertiary education, categories 5a and 6 in ISCED97). In the pension data I use imputed education in some cases: In a first step, I have replaced women's education with the modal value for the education in the detailed (3-digit) occupation they are working in, if their educational information was missing. For somen women, about 7 % of the sample, education is coded as unknown. These women typically work in low skilled occupations for which employers do not report education and are often on part time marginal employment. I have included them in the low-education group.

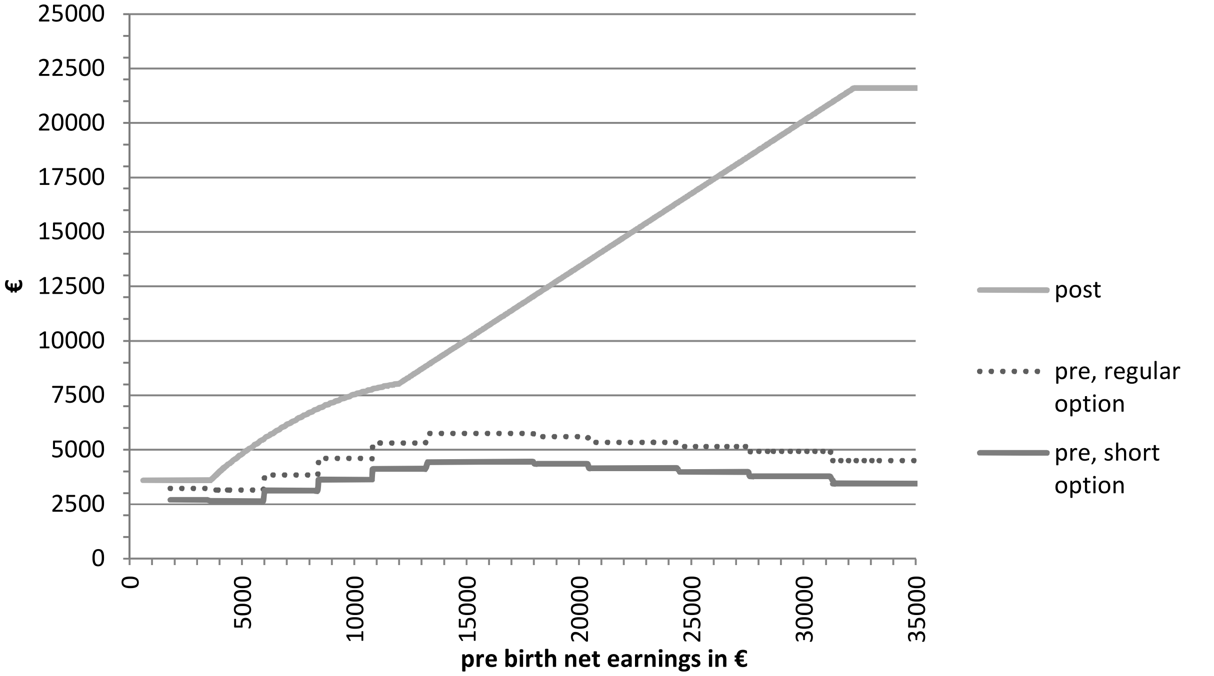
Figure 1: Maximum parental leave benefits (total) in Germany -pre vs. post 2007 reform



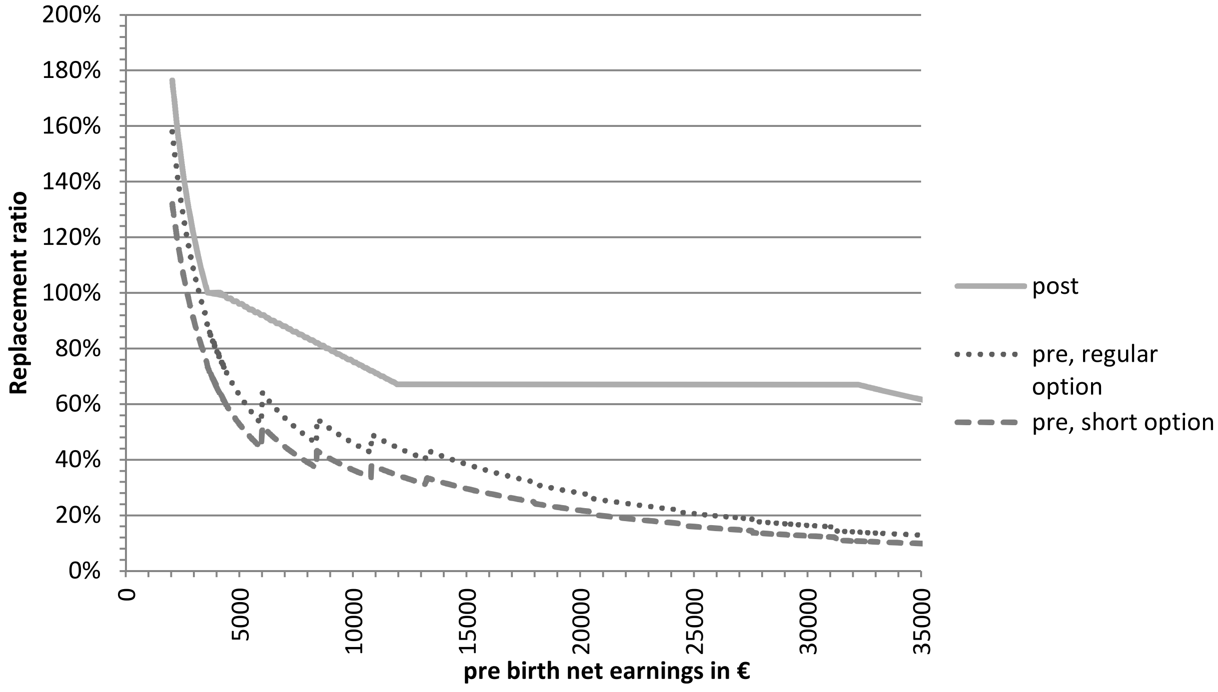
Notes : The graph plots stylised maximum sum of benefits a woman would be entitled her against her yearly net earnings (in EUR), pre and post-reform. For simplicity I show the maximum entitlement of 7200 EUR (see Section 3) for the pre-reform, which overstates the actual amount women were eligible for. I also show the upper bound for the average payment mothers received in 2006.

Figure 2: Parental leave benefits in Germany -pre vs. post 2007 reform simulation for maximum amount of subsidies

Panel A: Total benefits in EUR

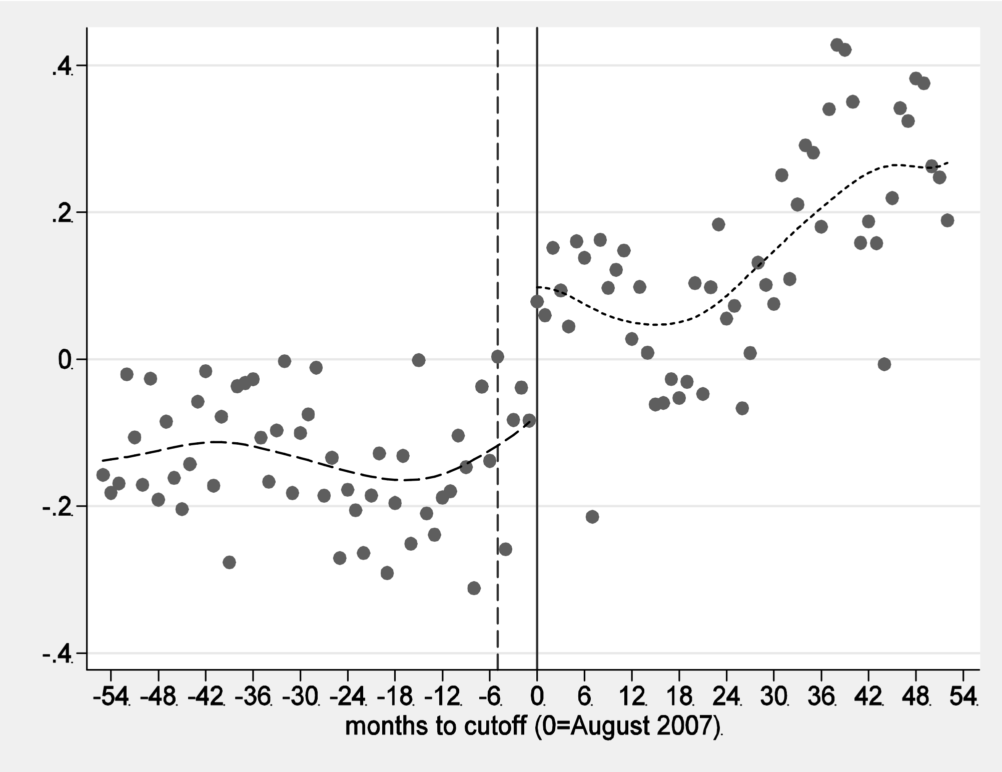


Panel B: Replacement ratio of benefits in % of net earnings



Notes : The graph in Panel A shows maximum sum of benefits a woman would be entitled to against her yearly net earnings (in EUR), pre and post-reform. In Panel 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 brackets using the Microcensus 2006 (See Appendix A).

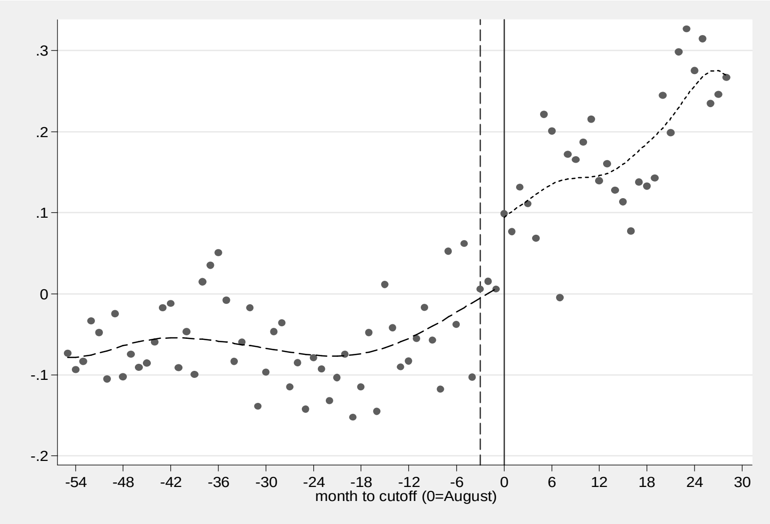
Figure 3: Evolution of monthly births per 1000 women (aged 25-45), seasonality corrected



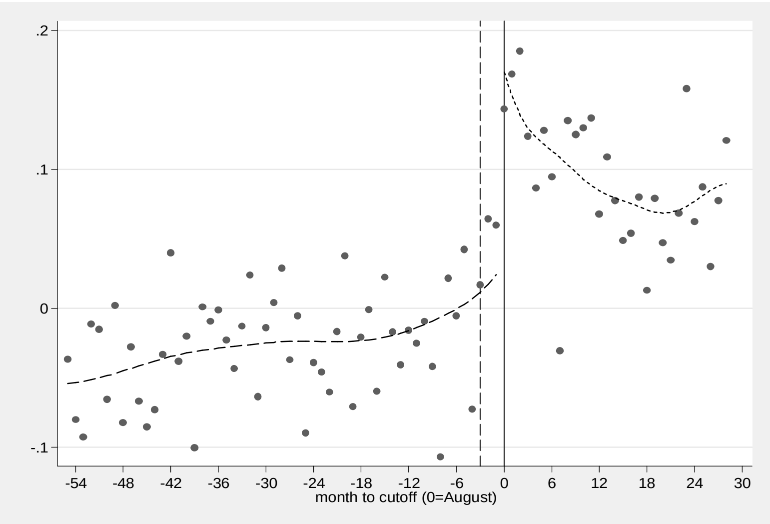
Notes : Lowess fit on both side of August 2007 (0 months to cutoff). The dashed line denotes March 2007, 9 months after announcement of the law. Data: Residual (month of birth adjusted) monthly livebirth per 1000 women aged 25-45 (on 31.12 of previous year), 2003-2011, Vital Statistics.

Figure 4: Evolution of (seasonality adjusted) monthly births by parity per 1000 women - married only

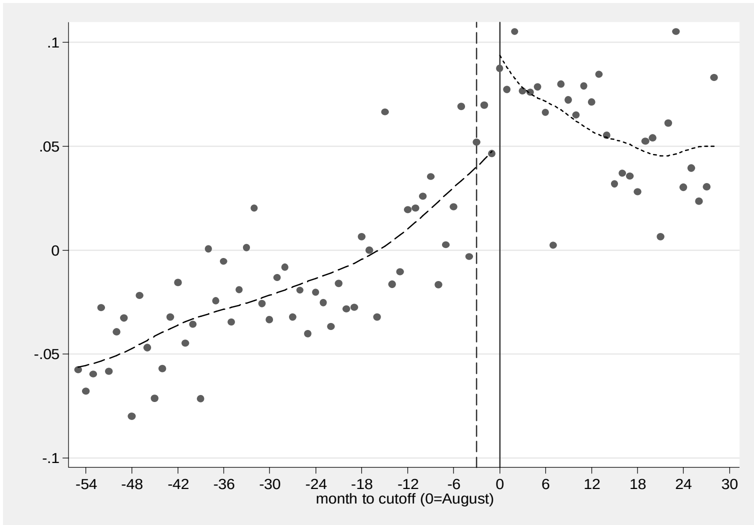
a) First child



b) Second child

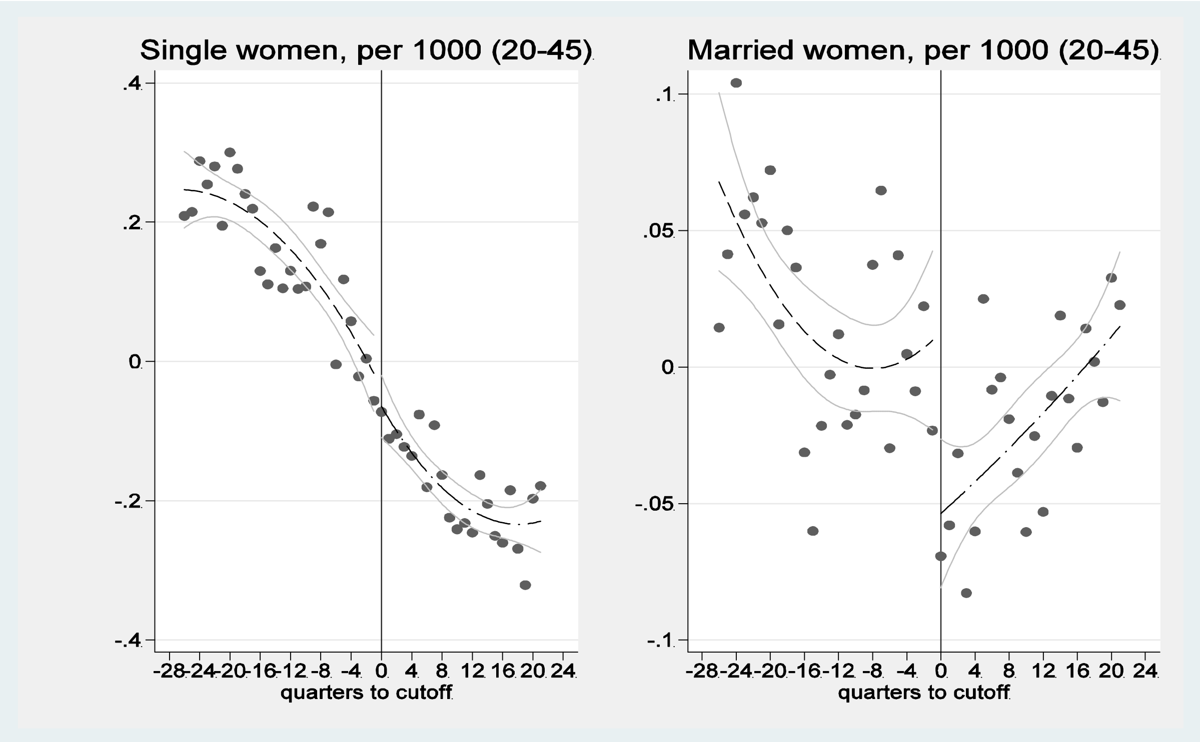


c) Third or higher



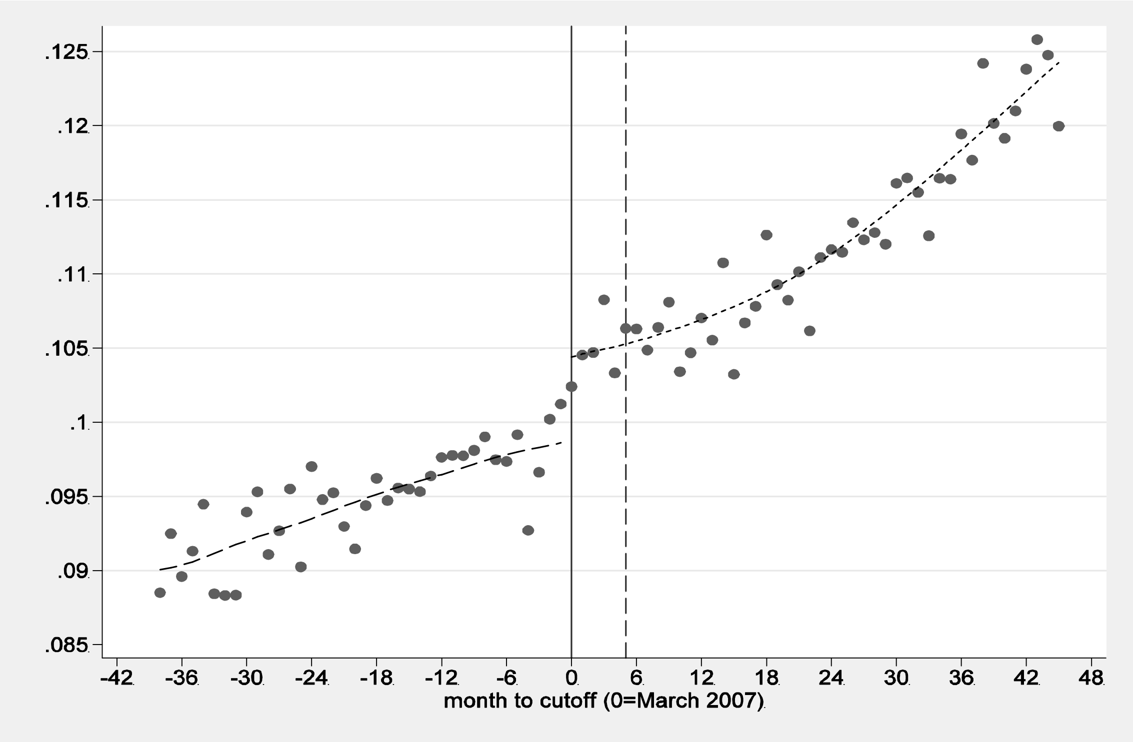
Notes : The graph shows residual (seaonality adjusted) monthly birth rates (per 1000 married women (age20-45)). I plot a Lowess fit on both side of August 2007 (0 months to cutoff). The dashed line denotes March 2007, 9 months after announcement of the law. Data: Vital statistics 2003-2009, married women only (seasonality adjusted).

Figure 5: Evolution of quarterly number of abortions per 1000 women (aged 25-45) by marital status, seasonality corrected



Notes: Quadratic fits on both sides of third quarter of 2006 (months to cutoff=0). Data: Residual (seasonality adjusted) quarterly number of abortions per 1000 women aged 20-45, by marital status, 2000-2011 Abortion Statistics.

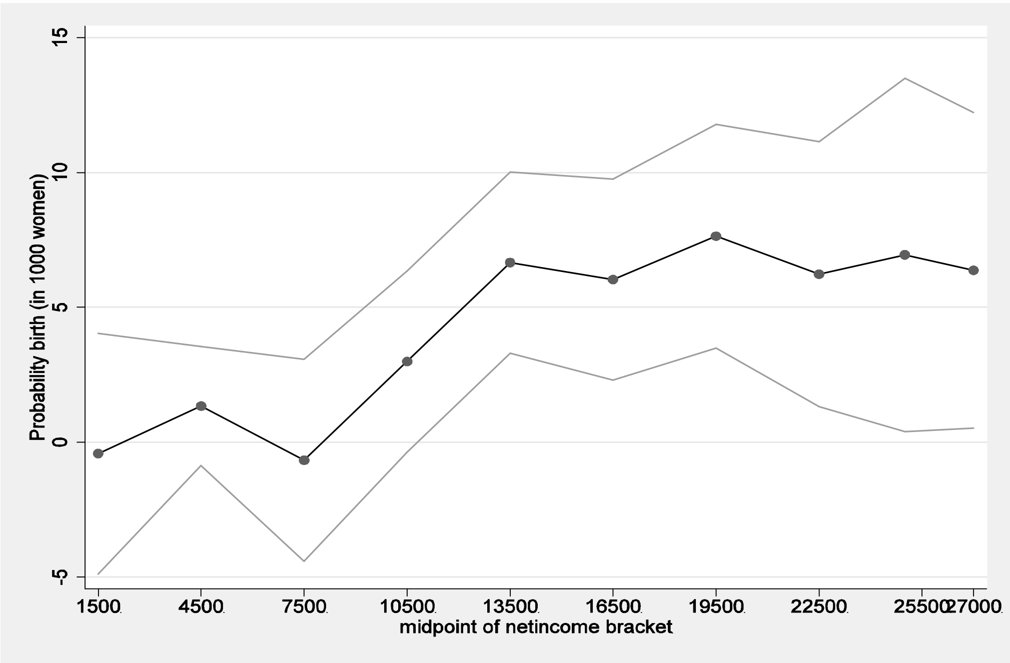
Figure 6: Evolution of monthly share of tertiary educated mothers, 2004-2010



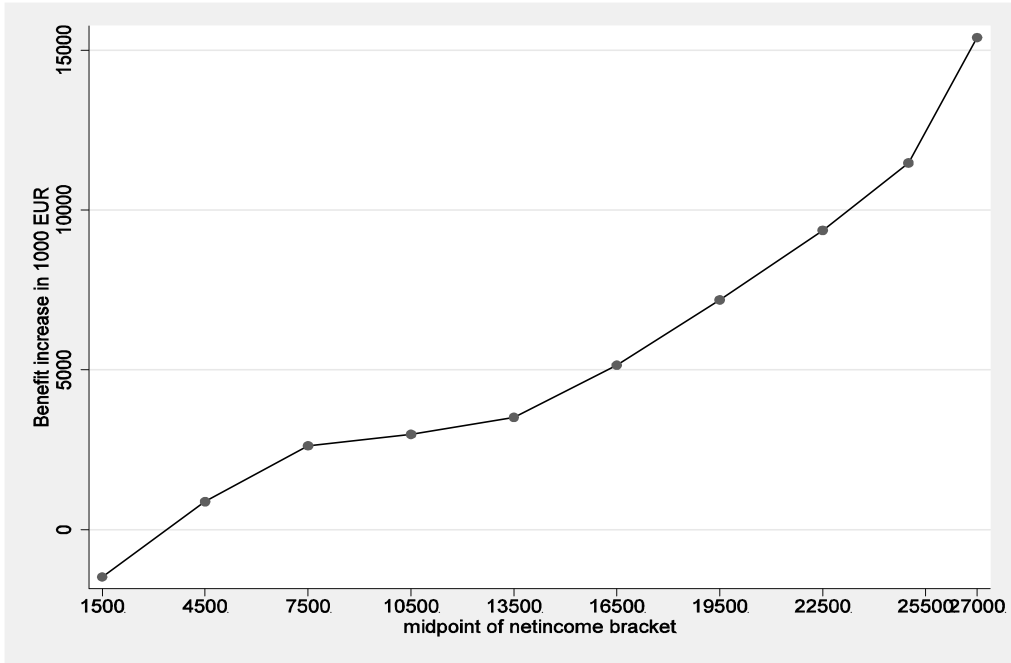
Note: The share of tertiary educated mothers is defined as the number of births to tertiary educated mothers over all births in that calendar month. Lowess fit on both side of March 2007 (0 months to cutoff). The dashed line denotes August 2007, 9 months after passing of the law. Data Source: Pension Registry (AKVS) 2004-2010, full population data.

Figure 7: Increase fertility vs. benefits post reform along income distribution

Panel A: Increase in Birth probability post-reform



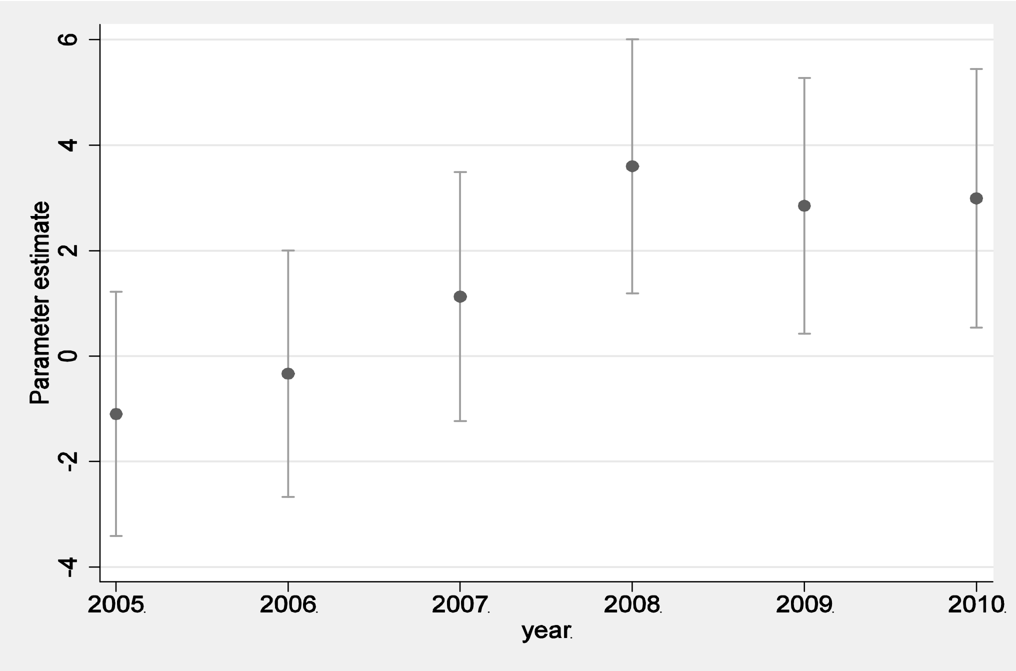
Panel B: Increase in benefits post-reform



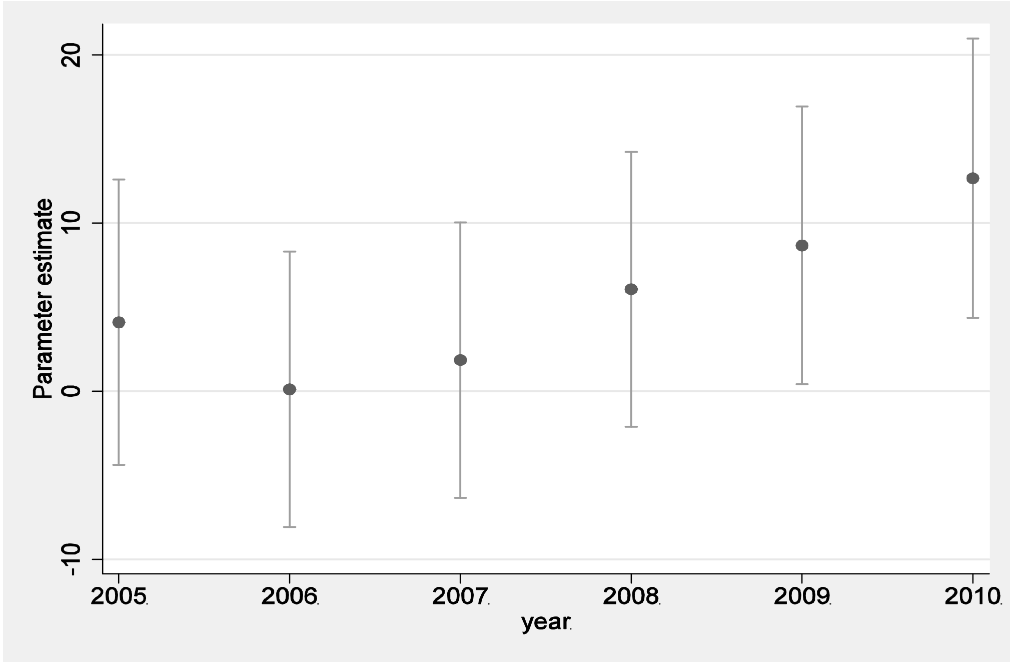
Notes : Graph in Panel A shows estimates (with 95% Confidence Intervals) post-reform interaction with income interval (intervals of 3000 EUR , starting with below 3000 up to a to interval containing women with netincome >27 000) from augmented baseline regression, incl. dummies for income brackets and the controls in baseline regression (omitting year fixed effect). The graph in Panel B shows the estimated coefficients for the post-reform benefit increase across the earnings intervals. Data Source: Pension Registry data 2004-2010, Micro Census 2006 for pre-reform benefit simulation.

Figure 8: Birth differentials between treatment groups and low educated

Panel A: Medium vs. low educated

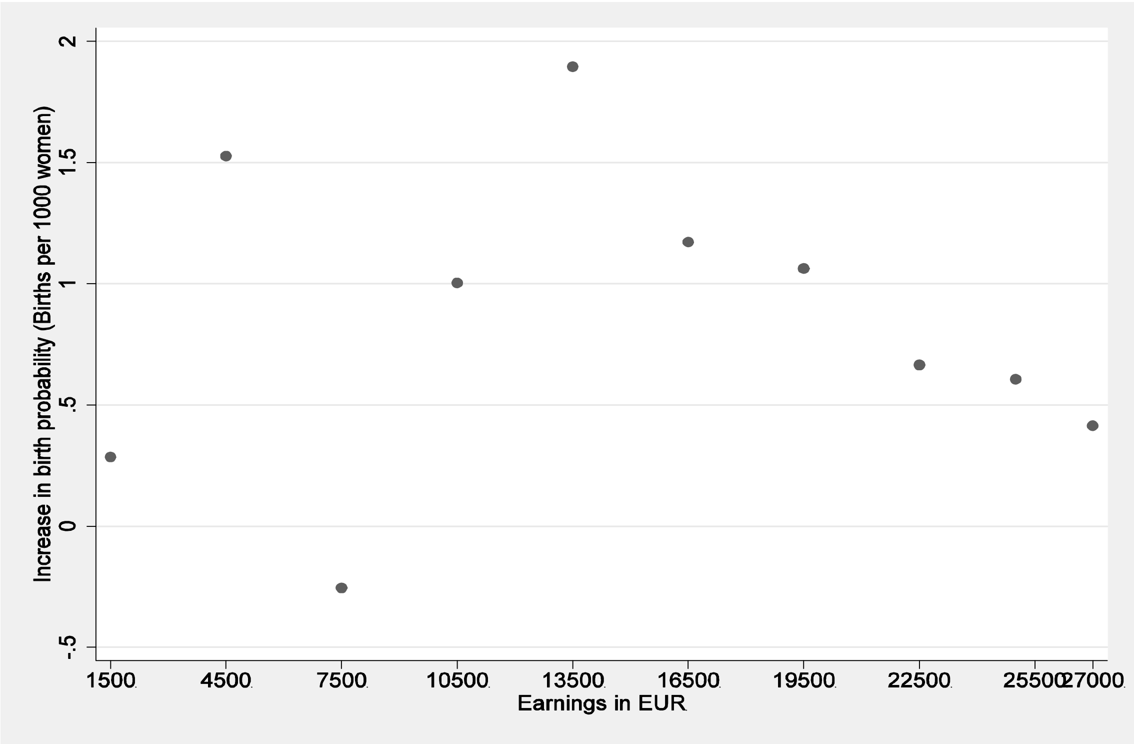


Panel B: High vs. low educated



Notes : The graphs show the coefficient estimates with 95% confidence intervals for interaction terms between medium-educated (Panel A) and high-educated (Panel B) with year-dummies from an augmented baseline regression in which I account for interactions between years and treated groups for years 2005 onwards defining 2004 as the reference year. Data Source: Pension Registry data 2004-2010.

Figure 9: Wald estimate for income intervals -Increase fertility per 1000 EUR benefit post reform



Notes: The graph shows Wald estimates for every income interval (intervals of 3000 EUR , starting with below 3000 up to a to interval containing women with netincome >27 000), which are calculated on the basis of estimates shown in Figure 7; the estimated fertility increase (Panel A, Figure 7) is divided by the estimated mean benefit increase in 1000 EUR (Panel B). The estimates are from augmented baseline regression, incl. interactions of income interval with a post-reform dummy, controls for income brackets and controls accounted for in in baseline regression equation 2 (omitting year fixed effect). Data Source: Pension Registry data 2004-2010, Micro Census 2006 for pre-reform benefit simulation.

Table 1: Fertility across selected countries

Panel A: Total fertility rates across countries

Germany (2006)	1.33
Italy (2011)	1.4
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 (or for 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 parental 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: Linear Probability model (birth in 1000 women) for benefit in EUR- Baseline and Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
				alternative definition of pre-reform benefits		
	baseline	use regional specific year dummies	interact control function with education group dummies	Budget option pre 2007	group by education pre 2007	(1) on sample (5)
Effect of total expected benefits in 1000 EUR	0.475***	0.477***	0.459***	0.493***	0.601***	0.495***
	(0.156)	(0.156)	(0.156)	(0.150)	(0.137)	(0.160)
Effect in % of mean births pre (39.1)	*1.2%*					
adj. R2	0.028	0.028	0.028	0.028	0.028	0.028
N	425,087	425,087	425,087	425,087	396,171	396,171

Notes: All regressions show estimates for a linear probability model of giving birth in t (expressed in 1000 women) and are estimated for women aged 25-44, years 2004-2006 and 2008-2010. Benefits are defined in real (2010) 1000 EUR. I have further controlled for a fifth order polynomial for real net (2010 prices) income in past year, for Länder dummies, German nationality, age dummies and separate age dummies for tertiary educated women, education dummies, year dummies and social benefits status in t-1 and whether the woman had a child in t-1 or t-2. In (2) I account for Länder-spefic year dummies. In (3) I amend the baseline regression in (1) by interacting the control function in net earnings (of baseline estimates) with education controls. In (4) I define the pre-reform benefit in terms of the shorter 12-months option and in (5) I merge simulated pre-reform benefits by education-age group. As the sample of (5) differs (unknown education gets omitted) I test my baseline specification on that restricted sample in (6). Robust standard errors reported in brackets. * indicates signifcance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level. Source: SUF (1%) AKVS 2004-2010, women with positive earnings in t-1.

Table 4: Robustness of baseline benefits estimates to function 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 in 1%-point	0.092**
	(0.046)
Effect of 10% point increase in replacement rate by... in % of mean births pre-reform (39.1)	2.4%
<u>**B: Benefits expressed in log benefits**</u>	
Effect of replacement rate in %-points	4.313***
	(1.499)
Interpretation of estimate: 10%-point increase in expected benefits increases births by 0.43 (per 1000)	
Implied percentage change of 10% increase (in % of pre-reform mean)	1.1%

Notes: All regressions show estimates for a linear probability model of giving birth in t (expressed in 1000 women) and are estimated for women aged 25-44, years 2004-2006 and 2008-2010. in Panel A I define benefits as a 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 netincomes larger than 2000 EUR in t-1 (N=403656). 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) , for Länder dummies, German nationality, education dummies age dummies and separate age dummies for tertiary educated women, year dummies and social benefits status in t-1 and whether the woman had a child in t-1 or t-2 . Robust standard errors reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level. Source: SUF (1%) AKVS 2004-2010, women with positive earnings in t-1.

Table 5: Linear Probability model (birth in 1000 women) allowing for reform effect to differ by education

	(1)	(2)	(3)	(4)
	Baseline (estimation equation 1)	account for employment/social benefit status, polynomial in real wage	Laender specific year dummy	without year effects (only post-reform dummy)
medium education*post2007	2.753**	2.988**	2.763**	2.748**
	(1.371)	(1.371)	(1.381)	(1.371)
in % terms of pre reform mean (43.51)	***6%***			
tertiary education*post2007	6.931**	7.105***	6.918**	6.952**
	(2.716)	(2.715)	(2.725)	(2.716)
in % terms of pre reform mean (54.57)	***13%***			
Year dummies	Yes	Yes	No	No
Type of employment dummies	No	Yes	No	No
Laender dummies	Yes	Yes	No	Yes
Region Year interaction	No	No	Yes	No
N	436,208	436,156	436,208	436,208

Notes: All regressions show estimates for a linear probability model of giving birth in t and are estimated for women aged 25-44, years 2004-2006 and 2008-2010. The interaction with post 2007 and education tests for differential time trends post reform with respect to low skilled women (control group) . I have further controlled for Länder dummies, age dummies and separate age dummies for tertiary educated women, a dummy for German nationality as well dummies whether the woman has had a child in t-1 and t-2, year dummies in (1)-(3),and in (2) for income and its square in t-1, type of work dummies (part time, etc.), and social benefits status. Robust standard errors reported in brackets. * indicates signifcance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level. Source: SUF (1%) AKVS 2004-2010.

Table 6: Simulated benefit increases (EUR) across education groups and interpretation of estimates

	Control group	Treatment groups	
	Low education (control group)	**medium education**	**high education**
Panel A: Changes in benefits:			
A. simulated pre reform benefit	4373.12	4760.57	4929.91
B. estimated mean change benefits individual	1564.78	3916.462	6896.718
C. difference in benefits with respect to low		**2351.68**	**5331.94**
D. Change in % of pre reform (C/A)		49%	108%
Panel B: Changes in fertility (from Table 1):			
E. Estimated relative increase fertility (births per 1000 women) (Table 1)		**2.763**	**6.918**
F. Fertility increase relative to underlying mean pre-reform probability (Table 1)		6%	13%
Panel C: Interpretation of results			
G. Elasticity (F/D)		0.12	0.12
H. Wald Estimate (Increase Fertility per 1000 EUR benefits) (1000*E/C).		1.17	1.30
I. % change in birth probability (evaluated against pre-reform mean) per 1000 EUR (1000*F/C)		2.6%	2.4%

Notes: Panel A table shows simulated changes in maternity leave benefits women of different education group would be entitled to if they had a child the next period. The simulations are based on matched the pre-reform benefits from the Micro Census 2006 to the pension registry data for 2006. In Panel B I report the reform effects on fertility from Table 5 (see notes Table 5). In Panel C I combine results form Panel A and B to interpret the reform effect.

Table 7: Linear Probability model (birth in 1000 women) allowing for reform effect to differ by education - using Micro Census (births 2004-2009)

	(1) Baseline (estimation equation 1)	(2) add region-specific year dummy	(3) account for childlessness and number of children	(4) without year effects (only post-reform dummy)
medium education*post2007	-3.073	-3.69	-3.76	-3.102
	(2.292)	(2.307)	(2.298)	(2.292)
tertiary education*post2007	6.384**	6.095**	5.881*	6.334**
	(3.030)	(3.043)	(3.032)	(3.030)
in % terms of pre reform mean (59.78)	***11%***			
R-squared adj.	0.027	0.027	0.036	0.027
N	306,707	306,707	306,224	306,707

Notes: All regressions show estimates for a linear probability model of giving birth in t and are estimated for women aged 25-44, years 2004-2006 and 2008-2009. The interaction with post 2007 and education tests for differential time trends post reform with respect to low skilled women (control group). I have further controlled for Länder dummies, age dummies and separate age dummies for tertiary educated women, a dummy for German nationality and whether the woman has been born in Germany as well as year dummies. In (2) I add region-specific year dummies, in (3) I account for the number of children in total and an indicator whether the woman was childless before potential birth. In (4) I omit the year dummies and only control for a post-reform indicator. Robust standard errors reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level. Source: SUF Micro Census 2004-2010.

Table 8: Benefit estimation vs. IV-results - Linear Probability model (birth in 1000 women) for benefit in EUR

Outcome: Probability of birth, expressed as births per1000 women	(1) Baseline	(2) IV with interactions post-reform dummy with earnings-	(3) IV: Instrument benefits with education-year-interactions	(4) Check baseline results (1),when excluding women with earnings >20,000
Effect of total expected benefits in 1000 EUR	0.475***	0.552***	1.139**	1.023***
	(0.156)	(0.158)	(0.483)	(0.297)
Effect in % of mean births pre-reform (39.1)	*1.2%*		*2.9%*	
adj. R2	0.028	0.028	0.028	0.027
N	425,087	425,087	425,087	349,602

Notes : All regressions show estimates for a linear probability model of giving birth in t (expressed in 1000 women) and are estimated for women aged 25-44, years 2004-2006 and 2008-2010. Expexted benefits are defined in real (2010) 1000 EUR. I have accounted for region dummies, German nationality, education dummies (medium and high), age dummies and and a tertiary-education specific polynomial in age, year dummies and social benefits status in t-1 and whether the woman had a child in t-1 or t-2. I have accounted for a fifth-order polynomial in real net earnings in t-1 in (1), 10 earnings interval dummies and square in earnings in (2) and the ten earnings dummies in (3) (adding higher order polynomials in earnings does not affect the results in (2) and in when using education as an IV earnings controls do not affect the results in (3)). In (2) I instrument benefits with ten interaction terms between a post-reform dummy (post 2007) and the ten earnings interval dummies.. In (4) I exclude all women with real net earnings above 20,000 EUR. Robust standard errors are reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level. Source: SUF (1%) AKVS 2004-2010.

Table 9: Results by Age group - Linear Probability model (birth in 1000 women) for benefit in EUR

	(1)	(2)	(3)	(4)
	Age 25-29	Age 30-34	Age 35-39	Age 40-44
Pre-reform mean of probabiliy of having child	*67.22*	*72.93*	*32.83*	*5.18*
Panel A: All women				
Effect of total expected benefits in 1000 EUR	0.486	0.293	0.889***	0.221*
	(0.446)	(0.447)	(0.320)	(0.123)
Effect of increase in benefits by 1000 EUR in % of pre-reform mean			*2.7%*	*3.8%*
N	93,332	86,661	108,561	136,533
Panel B: exclude top 10% earners of each age group				
Effect of total expected benefits in 1000 EUR	0.8	1.235*	0.838*	0.294*
	(0.695)	(0.639)	(0.438)	(0.153)
Effect of increase in benefits by 1000 EUR in % of pre-reform mean		*1.7%*	*2.6%*	*5.1%*
N	82,622	77,893	97,621	122,773

Notes: All regressions show estimates from separate regressions for various age groups (25-29, 30-34, 35-49, 40-44) of the linear probability model of giving birth in t (expressed in 1000 women) and are estimated for women aged 25-44, years 2004-2006 and 2008-2010. In Panel A I estimate on the sample of all women with positive earnings in t-1. In Panel B I exclude women with earnings in t-1 above the 90th percentile of net earnings for each respective age group. 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, for Länder dummies, German nationality, age dummies and separate age dummies for tertiary educated women, education dummies, year dummies and social benefits status in t-1 and whether the woman had a child in t-1 or t-2. Robust standard errors reported in brackets. * indicates signifcance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level. Source: SUF (1%) AKVS 2004-2010, women with positive earnings in t-1.

Table 10: Results by Birth order- Linear Probability model (birth in 1000 women) allowing for reform effect to differ by education - using Micro Census (births 2004-2009)

	(1)	(2)	(3)	
Panel A: Probability for different birth order (births per1000 women)	**First Child**	**Second child**	**Third child**	
medium education*post2007	0.446	-6.174	-3.644	
	(4.284)	(3.977)	(2.252)	
tertiary education*post2007	**10.879****	**1.578**	**-1.063**	
	(4.908)	(6.232)	(3.375)	
post2007	-3.311	3.426	3.961*	
	(4.060)	(3.671)	(2.092)	
N	155,491	111,578	143,295	
	Age 25-29	Age 30-34	Age 35-39	Age 40-44
Panel B: Probability of first birth by age group	(1)	(2)	(3)	(4)
medium education*post2007	-5.381	3.721	0.928	2.986
	(12.210)	(12.326)	(9.369)	(2.742)
tertiary education*post2007	**4.506**	**23.032***	**2.713**	**5.745**
	(12.442)	(13.015)	(11.415)	(4.735)
post2007	0.598	-4.462	-0.179	-3.64
	(11.786)	(11.451)	(8.533)	(2.441)
	40,081	27,662	22,886	31,739
Panel C: Probability of second birth by age group	(1)	(2)	(3)	(4)
medium education*post2007	-19.168	-12.594	-3.366	-1.633
	(14.001)	(14.395)	(9.351)	(2.904)
tertiary education*post2007	**-22.903**	**-12.794**	**10.18**	**13.140****
	(17.482)	(18.306)	(14.592)	(6.090)
post2007	10.1	6.537	6.768	1.566
	(13.005)	(13.314)	(8.596)	(2.667)
N	16,687	19,240	23,807	35,187

Notes: All regressions show estimates for a linear probability model of giving birth in t and are estimated for years 2004-2006 and 2008-2009. In Panel A I look at probabilities to have a first birth, second and third (or higher) for women aged 20-45. In Panel B and C I analyse probabilities separately by age group. In B I restrict the sample to women who are childless in the preceeding year and in Panel C to women who report to have one child in the household in the preceeding year. The interaction with post 2007 and education tests for differential time trends post reform with respect to low skilled women (control group, captured by post 2007 dummy). I have further controlled for Länder dummies, age dummies and separate age dummies for tertiary educated women, a dummy for German nationality and whether the woman has been born in Germany as well as year dummies. For regressions on second and third births I additionally account for age dummies of existing children. Robust standard errors reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level. Source: SUF Micro Census 2004-2010.

Table 11: Results for East vs. West - Linear Probability model (birth in 1000 women) for benefit in EUR

	(1)	(2)
	West Germany	East Germany
Effect of total expected benefits in 1000 EUR	0.395**	0.850**
	(0.172)	(0.373)
Pre-reform mean of probabiliy of having child	38.9	38.3
Effect of increase in benefits by 1000 EUR in % of pre-reform mean	*1.0%*	*2.2%*
adj. R2	0.027	0.036
N	341,421	83,666

Notes: All regressions show estimates from separate regressions for West and East Geman women of the linear probability model of giving birth in t (expressed in 1000 women) and are estimated for women aged 25-44, years 2004-2006 and 2008-2010. The sample is split by the region of residence of women, where East incl. Berlin. 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, for Länder dummies, German nationality, age dummies and separate age dummies for tertiary educated women, education dummies, year dummies and social benefits status in t-1 and whether the woman had a child in t-1 or t-2. Robust standard errors reported in brackets. * indicates signifcance at 10%, ** indicates significance at 5%, *** indicates significance at 1% level. Source: SUF (1%) AKVS 2004-2010, women with positive earnings in t-1.