Reducing the Child Penalty by Incentivizing Part-Time Work? Evidence from a Paid Parental Leave Reform in Germany

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Abstract

Although many paid parental leave (PL) schemes allow parents to work part-time while receiving PL benefits, there is little empirical evidence on the effects of such part-time options on maternal labor market outcomes. We study how a German paid PL reform, which financially incentivizes mothers to work part-time during the two years following child birth, affects maternal labor market outcomes up to 4.5 years after child birth. We use German social security records and exploit the fact that only mothers whose child is born in or after July 2015 are eligible for the new part-time PL option in a Difference-in-Differences strategy. We find that the policy's pro-part-time incentives increase the probability that high income mothers return to work during the first year after child birth by 3pp ($\approx$ 15%), without reducing working hours of mothers who would already be working full-time in absence of the policy. Low income mothers do not choose the new part-time option, most likely due to financial constraints, and are unaffected by the reform. The policy's part-time work incentives do not impact maternal employment along the intensive margin (part-time or full-time work) in the long run, leaving the child penalty unaffected. Our analysis shows that part-time options in paid PL schemes can increase maternal labor market attachment directly after child birth and alleviates concerns that such options reduce working hours of mothers in the long-run ("lock-in" effect into part-time employment).

Key words: paid parental leave, child penalty, part-time incentives, public child care
JEL Codes: J13, J16, J18, J22, J48

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1 Introduction

Gender-based earnings inequalities in the labor market continue to exist even in today's most advanced economies (OECD, 2019; EC, 2022). Many factors that explain these inequalities, such as differences in the educational attainment between women and men, have largely disappeared over the last decades. However, the importance of the child penalty, i.e. the negative labor market effect mothers experience after having a child, in explaining earnings inequalities has doubled since the 1980s (Kleven et al., 2019). Simultaneously, policy makers in many countries seek to increase the compatibility of work and family life, for example through parental leave policies that promote part-time work for parents. Approximately one third of all upper-medium and high income countries (14 out of 43) surveyed in Blum et al. (2018) offer parents a part-time option in their paid parental leave (PL) system. Yet, the evidence for how labor market outcomes of mothers who take up such pro-part-time policies are affected is scarce.[1]

Concerns about potential negative effects of pro-part-time policies on maternal labor market outcomes in the long-run have been raised recently (e.g. Boneva et al., 2021). Kunze (2022) points to the risk of a "lock-in" effect into part-time work, according to which mothers who work part-time directly after child birth are less likely to change to higher-paying, full-time jobs once the child has grown older. This might be caused by pro-part-time habit formation in labor supply choices (Woittiez and Kapteyn, 1998; Kubin and Prinz, 2002) or by discrimination by the employer. On the other hand, one can also make the case for positive long-run labor market effects of pro-part-time policies. These could arise if mothers return to work more quickly after child birth and, thus, experience less human capital depreciation by reducing the time they are absent from their job. Additionally, employers might reward parents who return to work earlier by granting pay raises or promotions more easily in the long run.[2] Thus, the long-run labor market effects of pro-part-time policies are theoretically ambiguous.

In this paper we analyze how increased incentives for part-time work directly after child birth (instead of working full-time or not working at all) affect maternal labor market outcomes. To this end we exploit a reform of the paid PL system in Germany in 2015. With the objective of increasing the compatibility of work and family life for parents, the reform gives them the option to choose between a new part-time scheme, called *Parental Benefit Plus* (hereafter *PB+*; German: *Elterngeld Plus*) and the already existing *Parental Benefit* (hereafter *PB*; German: *Elterngeld*). We study the policy's effects on maternal labor market outcomes in the short run (the first 24 months in which parents are eligible for PL benefits), as well as in the long run (up to 4.5 years after child birth).[3] Additionally, we assess whether there exist complementarities between the availability of public child care and the take-up of the new part-time paid PL scheme, since the latter provides incentives for mothers to return to work earlier after child birth.

Under the already existing (old) scheme, PB, each parent is eligible for benefit payments for 12 months after

[1] Fernández-Kranz and Rodríguez-Planas (2021) study the effect of an *unpaid* parental leave policy, which incentivizes parents with children that are younger than six to work part-time, on labor market outcomes of *all* mothers, i.e. not only those who take up the policy.
[2] Tô (2018) conceptualizes the timing of return to work in a signaling model.
[3] We do not analyze the policy's effects on paternal outcomes, since we do not observe fathers in our data. Our study period is limited to 4.5 years after child birth, because the latest child birth cohorts in our sample enter the period of the COIVD-19 pandemic thereafter, making it impossible to distinguish between effects caused by the policy and those caused by the pandemic.

child birth.[4] This benefit amount positively depends on income before child birth. Parents can work up to 30 hours per week while receiving PL benefits, however, the benefit amount decreases in post-child birth income. Compared to the old scheme, the new PB+ incentivizes part-time work by paying a higher *total* benefit amount (i.e. over the entire benefit period) to parents who work part-time in the 24 months after child birth. However, since parents can take PB+ for 24 months (instead of 12 months under PB), the new scheme pays most of these parents less each *month* than under the pre-existing PB to limit the increase in the total benefit amount. The combination of a higher *total*, but a lower *monthly* benefit amount in the benefit structure of the new policy leads to an income threshold in the take-up of the policy as explained below. Furthermore, the introduction of PB+ coincides with a change in the *unpaid* PL legislation, which allows parents to take two instead of one year of unpaid PL between the child's third and eight birthday leaving the total number unpaid PL years unchanged.[5]

Only parents of children born on or after July 1^{st} 2015 are entitled to choose between PB and PB+. Parents of children born before this threshold date are eligible for PB only. We compare parents whose child is born in the two months after (treated) to those whose child is born in the two months before (control) the threshold date using Difference-in-Differences models.[6] Additionally, we net out seasonality in parental characteristics by including parents who have a child in the years before the reform, i.e. from 2011-2014. The fact that the policy passed the German parliament less than 9 months before the implementation date makes it unlikely that parents sorted across the threshold in anticipation of the policy.[7] In line with this interpretation, we do not find systematic differences in characteristics at conception between parents who give birth before and after the threshold date.

Our analysis is based on German social security records, from which we obtain detailed employment histories of roughly 400'000 mothers who gave birth between 2011-2015. These data are particularly suitable for our analysis for two reasons: first, social security records are ideal to analyze the subset of mothers to whom the policy is particularly attractive, namely mothers that are employed before child birth. Second, since these data allow us to draw on the universe of employed women in Germany, the sample size is sufficiently large (20'000 births per month) to identify the policy effect based on child births occurring in a few months around the threshold month. This ensures that mothers in the treated and control groups are comparable. Additionally, we combine these data with information on the local availability of public child care.

As theoretical framework for the mothers' short-run labor supply decisions in response to the policy, we use a simple two-period labor-leisure (child care) model. This framework captures the policy's changes in the benefit structure and offers the following predictions:[8] first, only mothers with sufficiently high income prior to child

[4] Two extra months are offered if both parents take paid PL for at least two months.

[5] While all parents are entitled to three years of unpaid PL until the child's eighth birthday, parents who are eligible for PB+ can also choose to use two out of three years (previously one out of three) of unpaid parental leave between the child's third and eighth birthday. We are not able to precisely disentangle the effect of the change in the unpaid PL regulation from the one in the paid PL regulation. However, this does not affect our ability to rule out a lock-in effect, since it implies that our zero effect on full-time employment is a lower bound of the true effect size.

[6] Note that we do not use a Regression-Discontinuity-Design (RDD) since the child's date of birth is measured with error in our data. This introduces too much noise in the running variable to implement an RDD.

[7] To exclude the possibility of parental anticipatory sorting and taking into account that the children's birth date in German social security data relies on a proxy, we exclude births in the months directly preceding and following the implementation date, i.e. June and July 2015.

[8] This simple framework abstracts from inter-temporal externalities of labor choice, such as the labor decision in period one affecting wages or child quality in later periods, and intra-household dynamics.

birth take up the new scheme. This is mainly due to the fact that PB+ replaces a *lower* fraction of pre-child birth earnings each month compared to the alternative scheme. Only mothers with sufficiently high pre-child birth income can compensate this lower replacement rate. Second, mothers that choose PB+ are expected to smooth their labor supply over the first two years. More precisely, PB+ changes the benefit structure such that working becomes relatively cheaper in the first year (by lowering the replacement rate) and more expensive in the second year (by partially replacing pre-birth income). Thus, parents are expected to increase their labor supply in the first and lower it in the second year with respect to the control group. Third, parents return to work earlier after child birth as a result of increasing employment (at the extensive margin) in the first year of benefit reception.[9]

Germany makes for a particularly suitable setting to study how increased incentives to part-time work affect mothers. Compared to similarly developed countries, in Germany *i)* the maternal child penalty is relatively large ($\approx$ 60% 10 years after giving birth according to Kleven et al. (2019)), *ii)* the incidence of maternal part-time work is particularly high, as many mothers that worked full-time before child birth work only part-time thereafter (OECD, 2019) and *iii)* attitudes towards gender roles are relatively conservative. This is exemplified by fathers being little involved in child care and mothers being expected not to work or at most work part-time while her child is in school age or younger (Boneva et al., 2021).

In line with our theoretical prediction of an income threshold, we find that only high income mothers (i.e. upper 60% of the pre-child birth income distribution) take up the policy and are 3 pp ($\approx$ 15% compared to the sample mean) more likely to be employed in the first year after child birth. These mothers (compliers) return to work during the first year instead of in later years as a result of the policy. Although PB+ monetarily incentivizes mothers to reduce their working hours during the first two years after child birth (relative to their pre-child birth labor supply), we do not observe a reduction in working hours for any subsample of mothers. The policy does not affect maternal labor supply during the second year after child birth. This implies that high income mothers do not smooth their labor supply, work more and have higher earnings during the first two years after child birth as a result of the policy. Thus, the policy increases labor market attachment and reduces the child penalty of high income mothers in the short-run. Low income mothers do not take up the new part-time paid PL option and are unaffected by the policy.

Furthermore, a higher local availability of affordable public child care does not facilitate the observed return to work of mothers during the first year after child birth. Potential explanations are that *i)* high income mothers, who choose the part-time paid PL option, can afford more expensive private child care options or *ii)* that fathers, who are incentivized to work less by the part-time paid PL reform, engage more in child care.[10]

We do not find that the policy's pro-part-time incentives lead to a lock-in effect into part-time employment in the long run. While a lock-in effect would be observed as a shift in the probability from full-time to part-time employment, we do not find changes in employment along the intensive margin for high income mothers

[9] Recent research rationalizes the fact that some mothers return to employment before exhausting the paid PL period through a signaling model in which the timing of return to work provides employers with private information about future labor market choices and productivity (Tô, 2018). In such a model, PB+ lowers the costs of early return to work.

[10] Unfortunately, since we do not have data on non-public child care providers or on the fathers' labor supply decisions, we are not able to distinguish between these explanations.

(compliers) or low income mothers.[11] The long run effect on monthly earnings is close to zero, yet signficantly negative in some months (at a 5% significance level), due to a temporary drop in the probability of being employed around four years after child birth, which we attribute to the change in the *unpaid* PL legislation rather than the paid PL's pro-part-time incentives.[12] In line with this interpretation, this temporary drop in employment is statistically insignificant among mothers who choose the part-time paid PL option (high income mothers). The policy's pro-part-time incentives do not affect the child penalty in the long run.[13]

To corroborate our findings, we conduct a series of robustness checks. Placebo tests, in which we consecutively define one of the years from 2011-2014 as the treatment year (i.e. before the actual implementation year), show that mothers do not adjust their labor supply in response to these "fake" policies. Our results are also robust to reducing the birth month window from two months to one month on either side of the threshold date and to clustering the standard errors at the week of birth-level (instead of the birth county).

Our results offer important insights for the design of paid PL schemes (see section 7 for a detailed discussion). First, monetarily incentivizing part-time-work can increase maternal labor market attachment through earlier return to work without harming long-run labor market outcomes through a lock-in effect into part-time employment. Second, the German paid PL reform we analyze shows that the government can achieve these effects without increasing public spending on paid PL benefits, since mothers who work while receiving paid PL benefits receive a lower total benefit transfer than mothers who stay at home.[14] Third, the fact that only high income mothers choose the part-time paid PL option highlights the importance of taking the benefit structure's impact on different subgroups into account when designing policies.[15]

This paper makes various contributions to the literature. Numerous papers have investigated the impact of (changes in) paid PL policies on maternal labor supply (see Olivetti and Petrongolo, 2017 and Rossin-Slater, 2017 for excellent review articles). These papers analyze changes in the duration (Lalive and Zweimüller, 2009), in the benefit amount (Asai, 2015) or in both combined (Schönberg and Ludsteck, 2014, Kluve and Schmitz, 2018).[16] The role of part-time options in paid PL systems is almost absent from this literature. The only exception is Joseph et al. (2013), who study a 6-months part-time option in the French paid PL system using a survey of 3000 mothers with self-reported labor market outcomes. They find that it negatively impacts wages of high-income mothers two years after child birth, potentially explained by the fact that mothers can return to their pre-child birth job during an employment protection period of three years. Our analysis differs form the aforementioned paper in

[11] This is also true once the employment protection period ends three years after child birth. Since employers are legally obliged to allow mothers to return to their pre-child birth job, only labor supply choices after the employment protection period can be considered "final".

[12] This change allows parents to take two of the three years of unpaid PL (instead of one out of three) between the child's third and eighth birthday (see section 2 for more details).

[13] Monthly earnings show a temporary drop in some months around four years after child birth. Since this effect coincides with the temporary drop in the employment probability, we attribute it to the change in the *unpaid* PL legislation previously mentioned. Apart from this temporary drop, the point estimates of the policy's effect on monthly earnings are close to zero and statistically insignificant. Thus, the newly introduced pro-part-time incentives in the paid PL system do not have a statistically significant effect on monthly earnings and the child penalty.

[14] Additionally, mothers who return to work earlier generate additional tax revenue.

[15] Since official documents do not state that PB+ is designed to specifically target high income mothers, the exclusive take-up among this group of mothers is likely unintended.

[16] In summary, this strand of the literature finds that paid PL affects maternal labor supply negatively in the short-run, as labor income is substituted with PL benefits, while the effect is zero in the longerrun. Schönberg and Ludsteck (2014) show that negative long-run effects do emerge if the employment protection period is shorter than the paid PL period

that we *i)* use large-scale social security records allowing for a cleaner identification of the causal effect, *ii)* measure employment outcomes after the employment protection period ends and *iv)* study a reform with a significantly longer benefit period and larger, income-dependent benefit payments.

Another strand of the literature studies the effects of (subsidized) public child care on maternal employment. This literature focuses on changes in the availability of public child care through changes in prices or capacity and finds positive effects (Givord and Marbot, 2015; Nollenberger and Rodriguez-Planas, 2015; Ravazzini, 2018; Nix and Andresen, 2019) or null effects (e.g. Havnes and Mogstad, 2011) on maternal employment.[17] Instead, we study the *complementarities* between the regional availability of public child care and paid PL. To the best of our knowledge, the only paper with a similar approach is Girsberger et al. (2021), who find that the introduction of the first paid PL scheme in Switzerland positively affects fertility in the long-run but does not impact maternal employment. We contribute to this literature by studying a setting in which the paid PL reform incentivizes employment, while the introduction of the *first* paid PL system in the aforementioned paper reduces employment in the short run.

The paper is structured as follows: in section 2 we provide details about the institutional setting and the changes to the paid PL system in Germany introduced with PB+. Our theoretical framework for the parents' short-run labor supply and its predictions are presented in section 3. In section 4 we describe the empirical strategy and the data we use in more detail. The results are presented in section 5, followed by a discussion of the policy implications in section 7. We conclude in section 8.

2 Institutional setting

2.1 The child penalty in Germany

Recent studies have documented the existence and the extent of the child penalty in a large number of countries. Kleven et al. (2019) estimate the child penalty to be 61% five to ten years after child birth in Germany, situating it among the highest when compared to other industrialized countries. Scandinavian countries are found to have the lowest child penalties (21% and 26% in Denmark and Sweden), followed by English-speaking countries (31% and 44% in the United States and the United Kingdom respectively) and German-speaking countries (51% and 61% in Austria and Germany respectively).

The magnitude of the child penalty is determined both by the extensive margin of maternal employment, i.e. whether mothers return to employment after giving birth, and the intensive margin, i.e. how many hours these mothers work. In the case of Germany, the intensive margin explains the majority of the child penalty according to Kleven et al. (2019). Figure A1 shows that the share of mothers who work part-time is among the highest in Germany when compared to other OECD countries. Furthermore, figure A2 suggests that the high levels of part-time work among mothers in Germany indeed arise after child birth: when comparing the incidence

[17] Brewer et al. (2022) show that expanding public child care from half-day to full-day, rather than the availability of free half-day child care, positively affects maternal employment.

Table 1: Family policies for the treatment and and the control group

		Control	Treatment
Employment protection		3 years	3 years
Unpaid PL	child's age ≤ 8	3 years	3 years
	$3 <$ child's age ≤ 8	1 year	2 years
Paid PL		PB	PB or PB+

Explanation: the employment protection period refers to the period after child birth during which parents can return to their pre-child birth employer. Each parent is entitled a total of three years of unpaid PL, which can be taken until the child's eight birthday. The control (treatment) group can take 1 (2) of these years between the child's third and eight birthday. PB is the only paid PL option available to the control group. The treatment group can choose between PB and PB+ (see table 2 for a comparison).

of part-time work among women aged 25-29 (proxy for before child birth) and women aged 40-44 (proxy for after child birth), mothers in Germany experience the largest *increase* of part-time work among similarly developed countries.[18]

Recent research offers various explanations for the large magnitude of part-time work in Germany. Cultural norms play an important role. For example, mothers of young children in Germany *believe* that friends and family want them to stay at home, or at most work part-time (Boneva et al., 2021). There is evidence that these beliefs are accurate, as more than 60% of the population in Germany state that mothers with children under school age or in school should stay at home instead of working (Kleven et al., 2019). Moreover, Boneva et al. (2021) point to the limited availability of affordable child care as a constraint on maternal (full-time) employment. In this paper, we analyze whether public policies can affect maternal labor supply in the long run through monetary incentives to return to (part-time) work earlier after child birth.

2.2 Family policies in Germany

Similar to other high income countries, Germany has a set of family policies that aim at making child care and work more compatible for parents. In general, this set of policies consists of three types: first, an employment protection period sets a maximum period directly following the birth of their child, during which parents can choose to leave their job and return to an equivalent job in terms of responsibilities and pay at the same employer. Second, unpaid PL policies allow parents to reduce their working hours or to go on employment leave for some time while their child is younger than a certain age. The employer has to be previously notified about the length and timing of these periods of absence or reductions in working hours. Third, paid PL policies entitle parents to receive government funded benefit transfers for some months immediately following child birth. In most countries, these transfers are made on a monthly basis and their amount depends on pre-child birth labor market income.[19] Importantly, if parents wish to receive paid PL benefits after child birth and adjust their labor supply

[18] Age groups as a proxy for before/after child birth are motivated by the fact that we are not aware of a data set that offers consistent information across a set of countries on part-time incidence and information on (past) child births.

[19] All OECD countries - with the exception of the United States - have passed national legislation that offers mothers paid leave for some time around child birth. In the United States, federal legislation entitles mothers who are employed at companies with at least 50 employees to 12 weeks of *unpaid* leave after child birth since 1993 (FMLA). Some states go beyond the federal legislation and offer mothers additional paid and/or unpaid maternal leave.

(i.e. work less or stay at home completely), they have to use their available unpaid PL time for these labor supply adjustments.

For our analysis it is important to understand the set of family policies in place in Germany before the implementation of PB+ (see tables tables 1 and 2 for a summary). An employment protection period allows parents to return to their pre-child birth employer during three years following child birth. Parents can take unpaid PL for a total duration of three years until their child is eight years old. However, for births prior to the introduction of PB+ (i.e. the control group), only one of these three years could be taken while the child is between three and eight years old.

Prior to the paid PL reform that we analyze, *Parental Benefit* (PB) was the only available paid PL scheme in Germany. PB entitles parents to a maximum of 14 months of benefit payments per birth. These months can be shared between the parents as long as each parent takes at least two but at most 12 months of PL. Thus, if only one parent takes *PB*, the maximum duration is reduced to 12 months.[20] The benefit amount is proportional to the average labor market income during the 12 months before child birth.[21]

More precisely, under *PB* the monthly benefit amount is calculated as follows:

$$benefit(inc^{pre}, inc^{post}) = (inc^{pre} - inc^{post}) \cdot r, \tag{1}$$

where inc^{pre} and inc^{post} are the net labor market income prior to child birth (average over 12 months) and after child birth, respectively. The replacement rate r is 65% for the large majority of recipients, however, it rises to 100% for low-income recipients. The benefit amount is limited from below at 300€ for unemployed or low-income parents and from above at 1800€ for high-income parents. Parents are allowed to work a maximum of 30 hours per week while receiving PB, however, any post-child birth labor income is subtracted from the earnings base with which the benefit amount is calculated as equation 1 reveals. Thus, *PB* disincentivizes work after child birth. Thus, of any additional Euro earned after child birth, a parent effectively only retains $(1 - r)$ Euros.

The design of PB shapes the maternal labor supply as illustrated in figure A3. Most mother stay at home while receiving PB payments. While only 20% of mothers return to work during the first year after child birth, this number quickly rises to 50-60% in the subsequent 2-3 months. Furthermore, the share of mothers who work part-time doubles from 20% at conception to 40% (75% conditional on being employed) two or more years after child birth. Mothers who return to work two or more years after child birth mostly enter part-time work and the share of mothers that works full-time remains constant.

[20] This rule is inspired by Scandinavian PL schemes that intend to incentivize *paternal* PL take-up

[21] Since prior to 2007 a flat amount, independent of pre-birth income was paid, the *PB* reform in 2007 left low-income parents with lower monthly benefit payments while high-income parents received higher benefit payments. Additionally, the benefit duration was reduced from 24 months to 12-14 months. Kluve and Schmitz (2018) and Raute (2019) analyze the effects of the 2007 *PB* reform on fertility and labor market outcomes, respectively.

2.3 Parental Benefit Plus: the reform

A new paid PL option, called *Parental Benefit Plus* (PB+, German: ElterngeldPlus) was added to the already existing *PB* in July 2015. The main goal of PB+ is to increase the compatibility of work and family life, the latter mostly referring to child care duties, in a gender-neutral way.[22] Given that more than 80% of mothers stay at home during the first year after child birth, the policy is designed to facilitate their earlier return to work during the first year.[23] Furthermore, the new scheme monetarily incentivizes working part-time (i.e. at most 30h per week) during the first two years after child birth. With this modification, mothers whose child was born on or after 1^{st} July 2015 have the option to choose between the old and the new scheme (see table 2 for a comparison of the two schemes).[24] In the period that we study, roughly 20% of mothers choose PB+ (DESTATIS, 2019).

PB+ introduces two changes to the paid PL system: first, it doubles the maximum benefit duration from 12 months to 24 months. The additional two months given to couples in which both parents take paid PL for at least two months is also doubled to four months. The maximum duration was increased to prevent couples from running out of paid PL eligibility in case both parents take paid PL in the first 7 months after child birth. Second, the reform partially removes the disincentives to work while taking paid PL by changing the calculation of the benefit amount in the following way:

$$benefit(inc^{pre}, inc^{post}) = \left\{ \begin{array}{ll} \frac{1}{2} \cdot inc^{pre} \cdot r & inc^{post} \leq \frac{1}{2} \cdot inc^{pre} \\ (inc^{pre} - inc^{post}) \cdot r & inc^{post} > \frac{1}{2} \cdot inc^{pre} \end{array} \right\}$$

As long as $inc^{post} <= \frac{1}{2} \cdot inc^{pre}$ (case 1), the benefit amount is independent of inc^{post}. However, since the maximum length is doubled only half of the amount $inc^{pre} \cdot r$ is paid as benefit, i.e. the replacement rate r is halved. If $inc^{post} > \frac{1}{2} \cdot inc^{pre}$ (case 2), the benefit amount is calculated like under *PB*. In both cases 1 and 2, the *total* benefit amount received under *PB+* is at least as high as under *PB*, while the *monthly* benefit amount is weakly lower in the new compared to the old scheme.[25] The possibility to receive a higher total benefit amount while receiving a lower monthly benefit amount, makes PB+ particularly attractive for mothers who are not financially constrained as we show in our theoretical framework in section 3.

Figure 1 illustrates the differences in the calculation of the benefit amount between PB and PB+ for a given level of pre-child birth income graphically. The black line shows that the monthly benefit amount (vertical axis) decreases in post-child birth income (horizontal axis) until reaching the minimum benefit amount of 300€ ($PB+^{min}$). Under PB+, if the parents' post-child birth income is at most 50% of their income before child birth, the monthly benefit amount is independent of their income after child birth. However, the replacement rate is halved to limit the increase in the total benefit amount. In the area where parents earn more than 50% of their pre-child birth

[22] See https://www.bmfsfj.de/bmfsfj/themen/familie/familienleistungen/elterngeld/elterngeld-73752?view= for more information available in German (as of 22 September 2022).

[23] In the case of fathers the policy intends to achieve a reduction of working hours such that they can allocate more time to child care during this period. We abstract from the policy's effect on fathers, since fathers are not covered in our data and paternal PB+ take-up is low.

[24] Parents can combine both. In practice, parents who take PB+ for at least one month, take it for 19.2 months on average, leaving only 2.5 months for PB until reaching the maximum benefit duration (DESTATIS)

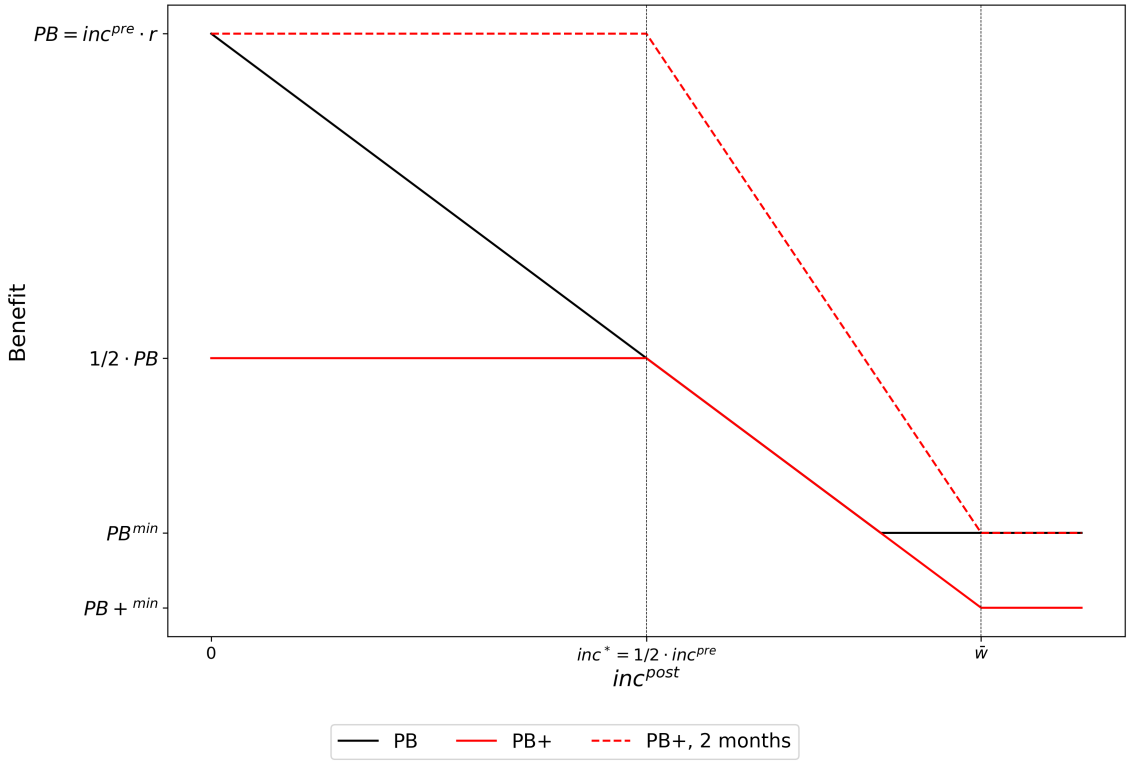
[25] Note that working such that $inc^{post} = \frac{1}{2} \cdot inc^{pre}$ maximizes the total *PB+* payoff

Table 2: Comparison of paid PL schemes

	PB (old)	PB+ (new)	
Duration	12 months	24 months	
Benefit calculation	$(inc^{pre} - inc^{post}) \cdot r$	$inc^{pre} \cdot \frac{1}{2} \cdot r$	if $inc^{post} \leq \frac{1}{2} \cdot inc^{pre}$
		$(inc^{pre} - inc^{post}) \cdot r$	if $inc^{post} > \frac{1}{2} \cdot inc^{pre}$
	Monthly amount:	PB $\geq$ PB+	
	Total amount:	PB $\leq$ PB+	

Explanation: paid PL benefits can be received for twice as long under PB+ compared to PB, i.e. 24 instead of 12 months. The paid PL benefit under each scheme is calculated as reported in the second row.

Figure 1: Monthly benefit payment by scheme



The black and the red solid lines show the monthly benefit amount as a function of post-child birth labor income for a given level of income before child birth under PB and PB+, respectively. The red dotted lines represents twice the monthly benefit amount under PB+, illustrating the fact that PB+ can be taken for twice as long as PB (i.e. one PB month equals two PB+ months).

income, PB+ and PB result in the same monthly benefit amount, the only difference being the reduction in the lower limit of the benefit (150€ instead of 300€). Considering that the duration of PB+ doubles compared to PB, the red dotted line illustrates that the *total* benefit amount (scale on the right) under PB+ can be substantially larger under PB+ relative to PB.

The introduction of PB+ coincides with a change in the *unpaid* parental leave legislation. While all parents are entitled to three years of unpaid PL until the child's eighth birthday, parents who are eligible for PB+ can also choose to use two out of three years (previously one out of three) of unpaid parental leave between the child's third and eighth birthday. This change in the unpaid PL policy might additionally reduce maternal labor supply after the child's third birthday.[26] We discuss how this change affects the interpretation of our results when presenting our findings in section 5. The employment protection period remains unchanged over the whole period of analysis. Tables 1 and 2 show a summary of the family policies available to the control and the treated groups and the differences between the new part-time paid PL option and the pre-existing scheme.

Given the complexity of the changes in the paid PL benefit structure, an important question is how parents understand these modifications. Both in government sources and on third-party websites that offer advice about the German parental leave system, the policy's modification are explained in rather general terms and with a few examples for benefit calculations.[27] The general explanations highlight that *i)* parents can receive PB+ for twice as long as PB, *ii)* if parents do not work, the *monthly* benefit is halved, and *iii)* if parents work, the *monthly* benefit under PB+ can be as high as under PB, implying that parents can receive a substantially higher *total* benefit amount due to the longer duration. The general recommendation is that PB+ is beneficial for mothers who would like to return to work earlier, in particular those who would like to work part-time (up to 30 hours per week as defined under PB+). Additionally, several example calculations of the benefit calculation highlight the fact that PB+ pays a (weakly) lower *monthly* but a (weakly) higher *total* benefit amount if parents work (see table A1). In sum, the vast majority of parents are likely familiar with these simplified ideas behind the policy rather than with its exact details.[28]

3 Theoretical framework: short run labor supply under PB and PB+

To study the mothers' paid PL choice, i.e. whether to choose PB or PB+, we use a simple 2-period labor-leisure model as our conceptual framework. This theoretical framework models maternal labor supply decisions in the first two years after child birth, which correspond to the period in which mothers in the treatment group are eligible for PB+. The model accurately captures the benefit structure of the paid PL schemes, however, it abstracts from aspects such as intra-household dynamics and spillovers of labor choices in period one on future wages or

[26] We are not able to precisely disentangle the effect of the change in the unpaid PL regulation from the one in the paid PL regulation. However, this does not affect our ability to rule out a lock-in effect, since it implies that our zero effect on full-time employment is a lower bound of the true effect size, i.e. it might be positive in absence of the change in the unpaid PL regulation.

[27] This paragraph is based on Bundesministerium für Familie (2020) and https://www.elterngeld.net.

[28] Note that the official explanations are a simplification of the actual benefit calculation. For example, while the official explanations stress part-time employment, the actual benefit calculation depends on the relation of an individual's post-child birth to pre-child birth earnings.

child quality. In this framework, a representative agent faces the following optimization problem:

$$\max_{c_1,c_2,l_1,l_2} U(c_1,k_1)+\beta\cdot U(c_2,k_2) \tag{2}$$

$$s.t. \quad c_1+\frac{c_2}{(1+r)} = \overbrace{(1-\tau_1)\cdot w\cdot l_1+\gamma_1\cdot y_0}^{\text{Period 1}} + \frac{\overbrace{(1-\tau_2)\cdot w\cdot l_2+\gamma_2\cdot y_0}^{\text{Period 2}}}{1+r}$$

$$k_t = 1-l_t$$

The agent chooses consumption c_t and hours worked l_t (and thereby implicitly child care k_t) in periods 1 and 2. She faces a standard inter-temporal budget constraint augmented by two terms that capture the paid PL scheme in the two periods $t \in \{1,2\}$: $\gamma_t \cdot y_0 - \tau_t \cdot w \cdot l_t$, where the first term is the pre-child birth income dependent amount that a parent receives irrespective of post-birth income and the second term is the amount by which the benefit amount is reduced if a parent works post-child birth under PB (and under some conditions also under PB+). In this specification, γ_t and τ_t represent the fraction of the pre-child birth earnings that are replaced by the PL scheme and the fraction of post-child birth earnings that are subtracted from the former amount, respectively. Pre-child birth income y_0 is exogenously given and independent of preferences over labor choices or the wage rate.

We derive the predictions of the theoretical framework by solving the agent's problem with an additively separable utility function of the form $U(c_t,k_t) = \log(c_t)+\log(k_t)$. To do so, we solve for the optimal labor supply choices in both periods as a function of the exogenous policy parameters, namely γ_t and τ_t, as well as pre-child birth income y_0 (see figure A4).[29] We infer the agent's preferred paid PL scheme based on the associated utility level (see figure A5).[30] Details are provided in section A.3 in the appendix.

Figure 2 illustrates the choices of labor supply in periods one and two on the vertical axis as a function of pre-child birth labor income on the horizontal axis for the control (black dashed) and treatment (red solid) groups. These optimal labor supply choices allow us to derive the following three predictions: first, there exists an income threshold since only mothers with sufficiently high income prior to child birth, i.e. $y_0 > y_0^*$, choose PB+.[31] This income threshold is explained by the fact that the lower replacement rate under $PB+$, which is halved relative to PB, can only be afforded by individuals that are not financially constrained prior to child birth.[32] Second, since the changes to the benefit calculation introduced with $PB+$ make working less costly in the first and more costly in the second period compared to PB, PB+ takers smooth their labor supply across periods, i.e. $l_1^{treated} > l_1^{control}$

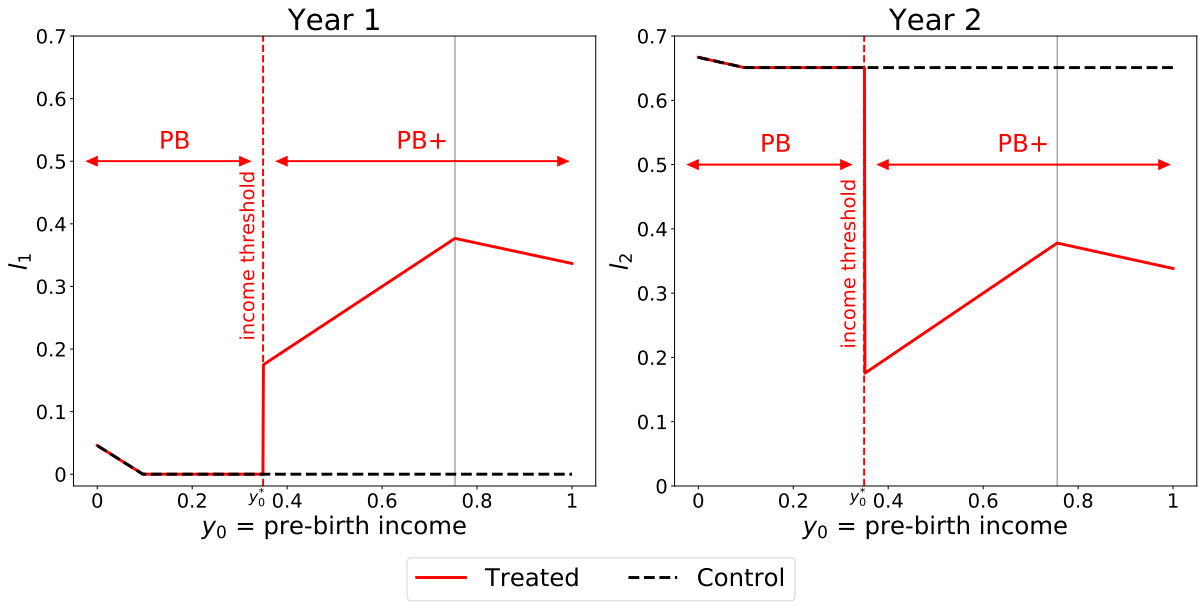
[29] The exogenous policy parameters are reported in table A2.

[30] In reality parents can choose combinations of the three alternatives, from which we abstract in this model. Official statistics show that mothers who take at least one month of PB+, receive PL benefits for 19 months on average, implying that the vast majority of benefit months are PB+ and leaving room for at most three months of PB (DESTATIS).

[31] As the more detailed figure A6 illustrates, an agent with pre-child birth income between $l_{0,t}^{high}$ and $l_{0,t}^{low}$ is indifferent between $PB+^{high}$ and $PB+^{low}$ and chooses $PB+^{low}$ for a higher level of pre-child birth income. However, in our empirical analysis we abstract from the distinction between $PB+^{high}$ and $PB+^{low}$.

[32] For a smaller fraction of mothers with $inc^{post} > \frac{1}{2}\cdot inc^{pre}$, the income threshold is explained by the fact that PB+ is only attractive if the part of the benefit scheme that depends on pre-child birth income in the second year ($\gamma_2 \cdot y_0$, where $\gamma_2 = 0.65$) is sufficiently high, such that it compensates for the "tax" τ_t on post-child birth income ($\tau_2 \cdot w \cdot l_2$, where $\tau_2 = 0.65$).

Figure 2: Labor supply choices as predicted by 2-period model



The figures on the left and right show the labor supply choices as a function of pre-child birth income in the first and second year after child birth, respectively. The red (black) line represents mothers who are (not) eligible for PB+.

and $l_2^{treated} < l_2^{control}$. Third, compliers return to work earlier after child birth than individuals in the control group. This follows directly follow from labor supply smoothing as it raises the individual's labor supply in the first period above a level of zero.

4 Empirical strategy and data

4.1 Empirical strategy

To estimate the causal effect of the policy on maternal labor market outcomes we exploit the fact that only parents whose child is born on or after July 1^{st} 2015 are eligible for *PB+*. While these parents have the option of choosing between *PB+* or *PB* (or a combination of the two), parents whose child is born before the implementation date are only entitled to *PB*. This institutional feature allows us to estimate a series of Difference-in-Differences (DD) models for outcomes p months after child birth.[33] In these DD models we compare outcomes of mothers whose child was born in the months after vs. before the threshold date in the year of the reform (2015) vs. the pre-reform years (2011-2014). The interaction of these two differences captures the policy's treatment effect. Intuitively, the treatment effect equals the difference in outcomes between mothers with children born in August - September 2015 and those with children born in April - May 2015 net of the average seasonal difference in outcomes between these

[33] The reform's implementation setting, in which a threshold date determines whether an individual belongs to the control or the treatment group, is usually exploited within a Regression-Discontinuity-Design (RDD). However, in the IEB data the date of birth is proxied by the date on which the mother goes on maternity leave, which starts six weeks before birth on average (Müller and Strauch (2017); see section 4.2). This measurement error in the date of birth makes a DD strategy preferable to an RDD.

two groups of birth months in previous birth years.[34] We restrict our data to two birth months on each side of the implementation month. Since PL take-up is not observed in our data we estimate the policy's Intention-to-Treat effect and interpret adjustments in maternal labor supply during the first two years after child birth as a proxy for the take-up of PB+.

In our baseline specification we estimate the policy effect p months after child birth in p separate regressions of the following form:

$$y_{i,p} = \alpha_p + \beta_p \cdot (treatYear_i \times treatMonths_i) + \delta_p \cdot treatMonths_i + \phi_{j(i),p} + \phi_{c(i),p} + \boldsymbol{\gamma_p} \cdot \boldsymbol{X_i} + \epsilon_{i,p} \tag{3}$$

In equation 3, $y_{i,p}$ stands for individual i's outcome of interest p months after child birth, $treatYear_i$ and $treatMonths_i$ are dummies for births that occur in the reform year (2015) and in the post-implementation months (August and September in each year), respectively. The $treatMonths_i$ indicator, in combination with the inclusion of four pre-reform child birth cohorts from 2011-2014, enables us to net out seasonality in parental characteristics across months. We estimate the policy's effect within 401 German counties and five birth years (2011 - 2015) by including birth-county and birth-year fixed effects, namely $\phi_{c(i),p}$ and $\phi_{j(i),p}$. The vector $\boldsymbol{X_i}$ controls for additional individual-level characteristics, such as pre-child birth income, part-time work, age, the job's skill-level and industry fixed effects. All of these control variables are measured at conception. The standard errors are clustered at the county-level throughout the analysis.

The identifying assumption of our empirical strategy is that, in absence of the policy, the differences in outcomes between mothers with children born in August - September of 2015 and April - May of 2015 would not have been different from the ones observed for these same groups (August - September and April - May) in the previous years (2011 - 2014). While this assumption is not verifiable by definition, we show that there are no trends in differential outcomes between the two groups of mothers with children born in the years prior to the implementation year (see placebo tests in section 6).[35] Additionally, the characteristics of mothers in the treatment and in the control groups are required to be balanced at baseline (i.e. conception) to ensure comparability. Any difference in such characteristics that exists already before child birth, for example a higher probability of working part-time, is likely to reappear after child birth and would mistakenly be interpreted as a treatment effect. For this reason we show that the there are virtually no statistically significant differences in observable characteristics at conception by estimating our baseline specification with a set of employment-related and personal characteristics as independent variable (see figure A14).

A potential concern for the validity of the causal relationships estimated in this paper is whether the enactment of the law could be anticipated by parents. The German parliament passed the law on November 7^{th} 2014, implying that parents could be certain about the availability of the new part-time option under PB+ eight

[34] A similar approach has been used previously, for example in Schönberg and Ludsteck (2014).

[35] Note that this exercise corresponds to the test for trends in outcomes between the treated and the control group prior to treatment assignment (i.e. pre-trends) in a classical Difference-in-Differences setting.

months before its implementation. Importantly, this is less than the nine months congestion period and even in a fertile couple three to six months are needed for conception (González, 2013, Raute, 2019). Thus, anticipation is very unlikely to play a role in explaining the observed effects. The balance in characteristics at conception discussed in the previous paragraph supports this interpretation (see figure A14).

4.2 Data

The main data source in this project are the Integrated Employment Biographies (IEB) which are provided by the German Institute for Labor Market Research and based on social security records. These data consist of the entire employment history of all social security covered employees in Germany excluding public employees and the self-employed. These administrative records include highly detailed information on the gross daily wage, the start and end of an employment contract, the employer, the industry, the type of contract and the skill-level.[36] From these data we first select all mothers who give birth between the years 2008 and 2018. Since births are not directly recorded in the IEB we follow Müller and Strauch (2017) and identify all women who experience a maternity leave-related employment interruption as mothers.[37] This approach identifies women who go on mandatory maternity leave six weeks before the child's expected date of birth in 89% of the cases. The (expected) date of birth is then taken to be six weeks after the start of maternity leave.

We only consider first-time mothers who give birth to their first child during 2011-2015 (as observed in our sample) and are employed at conception. These mothers are particularly likely to take up PB+, since the latter incentivizes mothers to return to work in the months after child birth. In contrast, mothers who are already unemployed before giving birth are unlikely to respond to the policy, since they are only entitled to the minimum benefit amount (300€ and 150€ under PB and PB+, respectively). Similarly, mothers who have already given birth to a child prior to the child birth that we observe are likely less responsive to the introduction of PB+, since they are already less attached to the labor market (i.e. many of them already work part-time before the subsequent child birth). Additionally, we restrict the sample to mothers who are between 20 and 38 years old, since the birth identification procedure described above works most reliably in this age group.

Data on the availability of public child care for 0-2 year olds at the county (Kreis) level for the years 2011 - 2015 come from administrative records of the Statistical Offices of the German provinces. Child care availability is measured as the fraction of available slots for children aged 0-2 in the number of children in the same age group in a given county. Public child care for children under three years old is highly subsidized such that parents only cover 14% of the total operating costs on average. The fees paid by parents depend negatively/positively on family size/income and range from 0 to 600€ (Sandner et al., 2020). These data are merged to the data described above

[36] Unfortunately, these data do not record some relevant information at the personal and/or household level, such as whether an individual is married or cohabits with a partner.

[37] This approach makes use of the IEB's information on employment interruptions and their underlying reasons which employers are obliged to notify. Müller and Strauch (2017) flag all women who experience an employment interruption "due to entitlement to other compensation by the statutory health insurance provider (value 51 of 'grund' [variable])" as *potential* mothers. This reason for employment interruption can be due to maternity allowances, which are paid during paid maternity leave to actual mothers, and sickness allowance. The authors use further restrictions on the age of potential mothers and the lengths of the employment interruption to disentangle the two underlying reasons for employment interruptions

based on the year of child birth and the mother's county of residence at conception. We then use this information to assess whether their exist complementarities between the availability of affordable, public child care and the take-up of the part-time work option in the paid PL system in Germany. As shown in figure A7 there is large variation in the availability of public child care across counties, ranging from 5% to 45%.

Our final sample consists of roughly 380'000 mothers who give birth to a child in the months of interest (May, April, August, September) in the years 2011 - 2015, corresponding to 20'000 births per month. Descriptive statistics of the full sample are presented in table A3. Mothers are on average close to 30 years old at conception and 23% have a university degree. Furthermore, 27% work part-time prior to birth and the average gross monthly labor income equals 2500€.[38] These numbers closely match official statistics on the characteristics of mothers prior to birth and, thereby, corroborate the validity to our strategy of identifying mothers.

5 Results

5.1 All mothers

The policy's effects on labor market outcomes of all mothers are reported in figure 3. The three panels report the Intention-to-Treat effects for employment, part-time work (less than 20 hours per week) and full-time work (20 hours or more per week). The absence of pre-trends, i.e. significant differences in labor market outcomes between the treated and the control group before the take-up of child birth, validates that mothers in the treated and the control groups are indeed comparable. Furthermore, it also suggests that parents do not strategically alter their labor supply prior to child birth.

The first panel in figure 3 shows that the reform leads to an increase in employment of approximately 1-2pp ($\approx$ 5-10%) in the first year after child birth, i.e. when treated mothers are eligible for both PB and PB+. Since there is no positive employment effect in subsequent years, these mothers choose PB+ to return to work earlier, i.e. they would have returned to work at a later point in absence of the policy.[39] In the absence of individual-level data on paid PL usage, we interpret the employment effect in the first year as a proxy of PB+ take-up throughout the analysis.[40] The policy does not affect the probability of employment along the intensive margin in the second year. This implies that, in contrast to the predictions of our theoretical framework, mothers do not smooth their labor supply across the two first years after child birth. As a result, the policy increases maternal labor supply and earnings during the first two years after child birth.

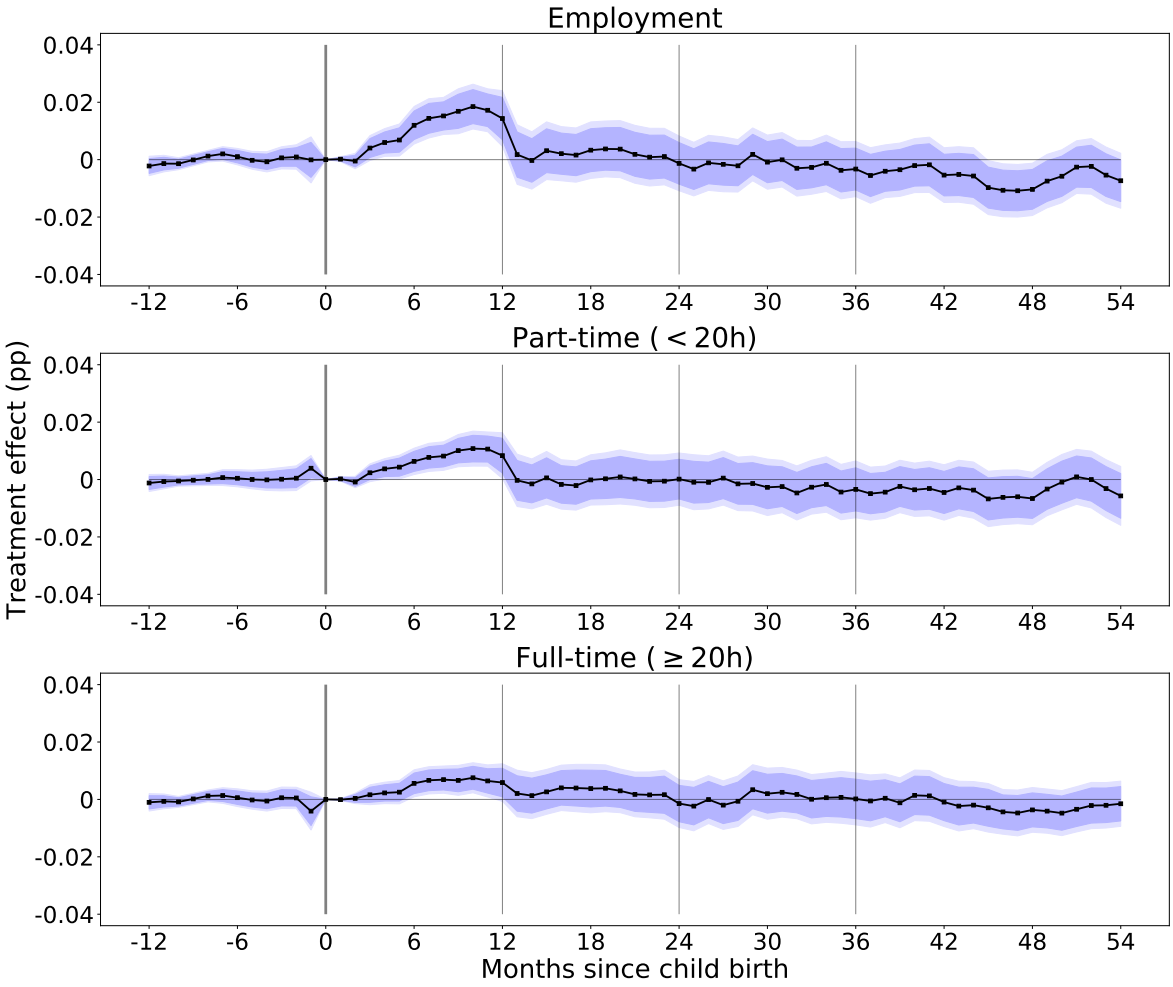
In line with the effects on employment, monthly earnings increase by 1.5 log points in the first year after

[38] Note that the benefit amount in the German paid PL system is calculated based on the net rather than the gross monthly income. A significant share of mothers change the taxation class (e.g. switch classes with their partner), which determines the tax rate in Germany, in the year before birth to increase the benefit amount they receive. Since we do not know which mothers indeed change their taxation class, we cannot infer the benefit amount they receive after child birth.

[39] Roughly two thirds of the employment effect in the first year are explained by transitions to part-time work while the rest comes from increased full-time employment as shown in the second and third panels.

[40] Note that interpreting the employment effect in the first year as a proxy for PB+ take-up only captures mothers that take-up PB+ *and* adjust their labor supply (compared to the control group). Since mothers can also choose PB+ without changing their labor supply, our proxy of PB+ take-up is lower than the take-up reported in official statistics (DESTATIS, 2019).

Figure 3: Employment outcomes of all mothers



The figures above report the DD coefficient of separate regressions for outcomes p month after child birth as specified in regression 3. The outcome variables in the first, second and third panels are a dummy for individual is employed, part-time employed and full-time employed, respectively. The three last vertical lines mark the end of PB (12 months), the end of PB+ (24-28 months) and the end of the employment protection period (36 months), respectively. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals. See the corresponding table A4.

child birth and are unaffected in the following year as shown in the first panel of figure A8. Thus, the policy reduces the child penalty in the first year. The second panel of the same figure illustrates that employer continuity, i.e. the probability of working for the same employer after as before child birth, increases by almost 2pp in the first year as well. This means that almost all mothers who return to work earlier as a result of the policy, do so by returning to their pre-child birth employer. Employer continuity is unaffected in the second year.

The absence of labor supply smoothing is most likely explained by a combination of the framing of the policy and the pre-existing patterns in maternal labor supply. More precisely, the policy is framed as an option for mothers who want to return to the labor market earlier after child birth, in particular during the first year after child birth, while less attention is given to labor supply adjustments thereafter. Given the complexity of the benefit amount calculation, this might result in the observed pattern in which mothers return to the labor market earlier in the first year and leave their labor supply unchanged during the second year. Additionally, roughly two thirds

of women employed in the second year after child birth worked in part-time jobs in the pre-reform years (see figure A3). Thus, already before PB+ was implemented, a large fraction of mothers adjusted their labor supply in the second year as in line with the general recommendations the new part-time scheme, which emphasize that the policy is particularly beneficial in combination with part-time work.

After the employment protection period ends (i.e. three years after child birth), treated mothers are temporarily 1.5pp less likely to be employed as depicted in panel 1 of figure 3. We attribute this effect to the change in the unpaid PL legislation (i.e. 2 instead of 1 year of unpaid PL from the child's 3^{rd} to 8^{th} birthday) rather than to the pro-part-time incentives in the paid PL system for two reasons:[41] first, parents who go on employment leave using their unpaid PL time are recorded as unemployed in our data. Second, among high income mothers in West Germany, who are particularly likely to make use of PB+, the temporary drop in employment around year 4 is no longer statistically significant (see section figure 4 and section 5.2.1). This suggests that the temporary drop in employment is driven by a different subsample than the one that responds to pro-part-time incentives.

Panels 2 and 3 of figure 3 show how the reform affects employment at the intensive margin in the long run. For most months after the third year, the point estimates are negative, however, close to zero and not statistically significant. In a small number of months the coefficients are statistically significant at a 5% significance level. However, those are precisely the months that coincide with the temporary drop in employment in panel 1. Thus, these slightly negative estimates for part-time and full-time work are most likely the results of the change in the unpaid PL legislation. The fact that the effect on full-time employment is a precise null effect among the sample of high-income mothers in West Germany, for which the temporary drop in unemployment is less pronounced (see figure 4), corroborates this interpretation. Based on this evidence, we conclude that the reform's pro-part-time incentives are unlikely to affect employment at the intensive margin in the long run.

In absence of any labor supply adjustments along the intensive margin, the temporary drop in employment around 4 years after child birth translates into a temporary reduction in labor market earnings of just below 0.1 log points as shown in figure A8. Thus, in the sample of all mothers the child penalty is slightly increased in the long-run. Following the explanation in the previous paragraphs, this is most likely caused by the reform's change in the unpaid PL legislation rather than by its pro-part-time incentives. Figure A8 also shows that employer continuity after the third year turns slightly negative, which is driven by the negative effect on employment. We do not find evidence for the policy affecting other labor market outcomes, such as job quality, or fertility.

5.2 Heterogeneity

In this section we focus on mothers that are particularly likely to make use of PB+. In absence of individual-level information on the take-up of PB+, we rely on the hypotheses of our theoretical framework, which identifies pre-child birth income as an important determinant for PB+ take-up (see section 3), and on the existing literature

[41] In Germany, parents can take three years of unpaid parental leave until the child is 8 years old. Unpaid parental leave can be taken in 3 parts and a maximum of two years can be taken between the child's third and eight birthday. Parents need to accompany paid with unpaid parental leave in order to modify their working hours.

Table 3: Take-up of PB+

Outcome:				
Employment in month	6	9	6	9
treatMonths=1 × treatYear=1	-0.002	0.002	0.016***	0.020***
	(0.005)	(0.007)	(0.003)	(0.003)
treatMonths=1 × treatYear=1 × earnings (Q2)	0.006	0.002		
	(0.007)	(0.008)		
treatMonths=1 × treatYear=1 × earnings (Q3)	0.023***	0.021**		
	(0.007)	(0.009)		
treatMonths=1 × treatYear=1 × earnings (Q4)	0.020***	0.023***		
	(0.007)	(0.008)		
treatMonths=1 × treatYear=1 × earnings (Q5)	0.019**	0.028***		
	(0.007)	(0.009)		
treatMonths=1 × treatYear=1 × east=1			-0.020***	-0.016**
			(0.006)	(0.007)
Mean of outcome	0.097	0.137	0.097	0.137
SD of outcome	0.30	0.34	0.30	0.34
Controls	Yes	Yes	Yes	Yes
County FEs	No	No	Yes	Yes
Sample size	380717	380717	380717	380717
Number of clusters	401	401	401	401

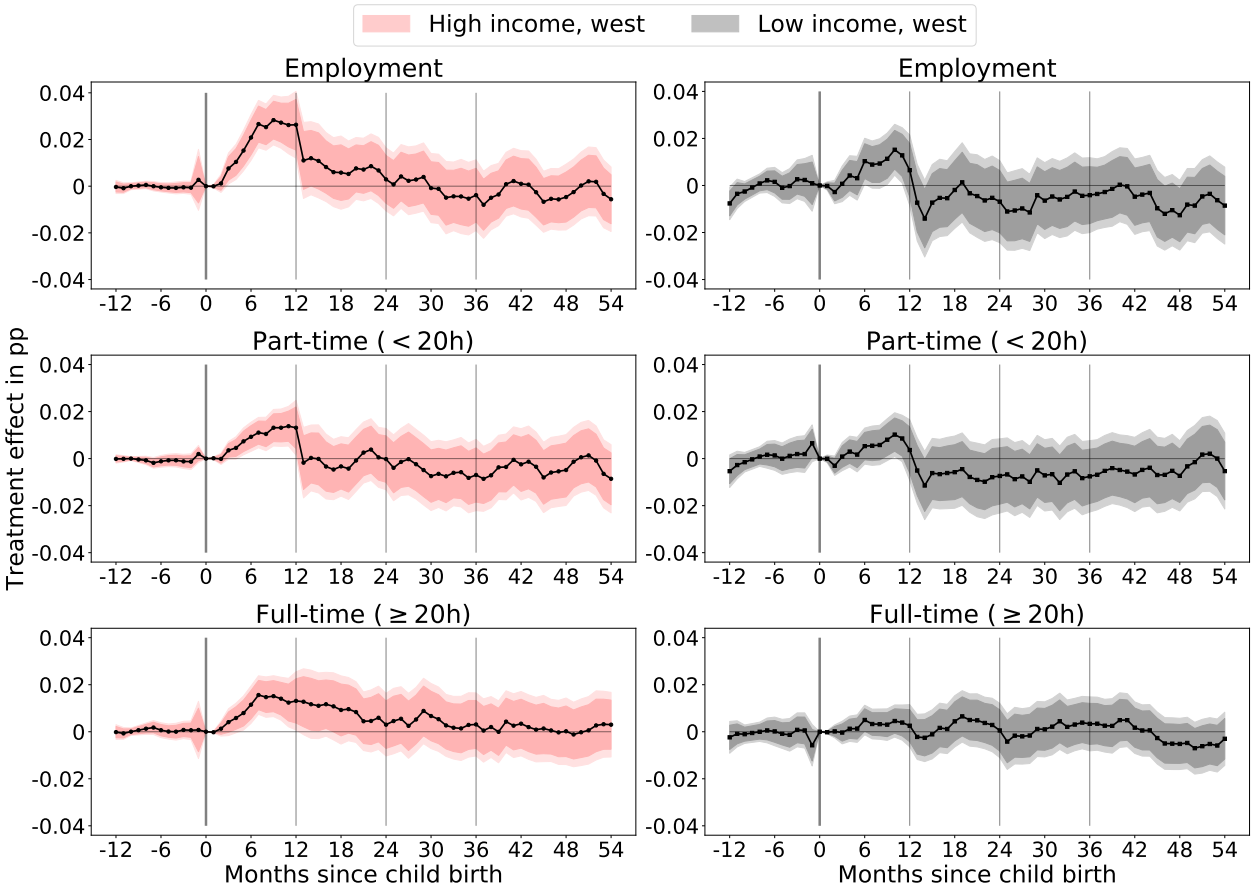
Note: the table above reports the coefficients of interest of the baseline model (equation 3) fully interacted with *i)* pre-child birth earnings (measured at conception and binned into quintiles) in rows 1 - 5 and *ii)* a dummy for residing in East Germany in rows 1 and 6. Row 1 represents the reference category. The outcome variables are employment 6 months after child birth in columns 1 and 3 as well as 9 months after child birth in columns two and four. Standard errors are clustered at the county level (401) and reported in parentheses. Descriptive statistics are reported at the bottom of the panel. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

on maternal labor supply in Germany. The latter points to two heterogeneities that might be relevant for the take-up of PB+: first, there are important cultural differences between East and West Germany that result in different maternal labor supply patterns in these regions (Boelmann et al., 2020). These might lead to a differential take-up of PB+ in East and West Germany. Second, mothers perceive a lack of child care availability as a constraint on maternal employment (Boneva et al., 2021), suggesting that the take-up of PB+ might be facilitated by a higher local provision of affordable public child care.

5.2.1 High and low income mothers in West Germany

To empirically examine whether the theoretically predicted income threshold exists, we interact the main effect in the baseline specification with monthly pre-child birth earnings in table 3. By binning the earnings into five categories (quintiles) we are able to assess the interaction of the policy's main effect over the distribution of monthly earnings. The first two columns in table 3 report the resulting coefficients for employment in the first year after child birth (months 6 and 9 months). The first five rows show that the there is no employment effect for the bottom 40% of the income distribution (rows 1 and 2) and that the upper 60% entirely drive the employment effect observed in the main sample. As a result, the point estimates in the upper 60% range from 1.8 - 2.8 pp and

Figure 4: Employment outcomes for high and low income mothers in West Germany



The figures above report the DD coefficient of separate regressions for outcomes p months after child birth as specified in regression 3. The outcome variables in the first, second and third panels are a dummy for individual is employed, part-time employed and full-time employed, respectively. High and low income mothers are defined as being in above and below the 50^{th} percentile in the pre-child birth income distribution, respectively. The three last vertical lines mark the end of PB (12 months), the end of PB+ (24-28 months) and the end of the employment protection period (36 months), respectively. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals. Also see the corresponding tables A5 and A6.

are slightly larger than the effects observed in the full sample. Moreover, within the upper 60% of the income distribution the effect is homogeneous. The positive employment effect in the first year being a proxy for PB+ take-up, this empirically confirms the existence of an income threshold.

In the last two columns of table 3 we interact the policy's main effect with a dummy for whether a mother resides in East Germany at conception. We do so to understand whether there are differences in the policy take-up between East and West Germany. The first row shows that the policy increases employment in West Germany (reference category) during the first year by almost 2pp. This effect in West Germany is slightly higher than the one observed in the whole sample, which is due to the fact that mothers in East Germany do not make use of the policy. As can be seen in the last row of table 3 the interaction of the policy's main effect with the for East Germany dummy (1.9-1.6pp), reduces the effect in the first row to 0. The reason is likely that East German mothers tend to return to work earlier after child birth than their West German counterparts (Boelmann et al., 2020). PB+ partially closes this gap in the first year after child birth.

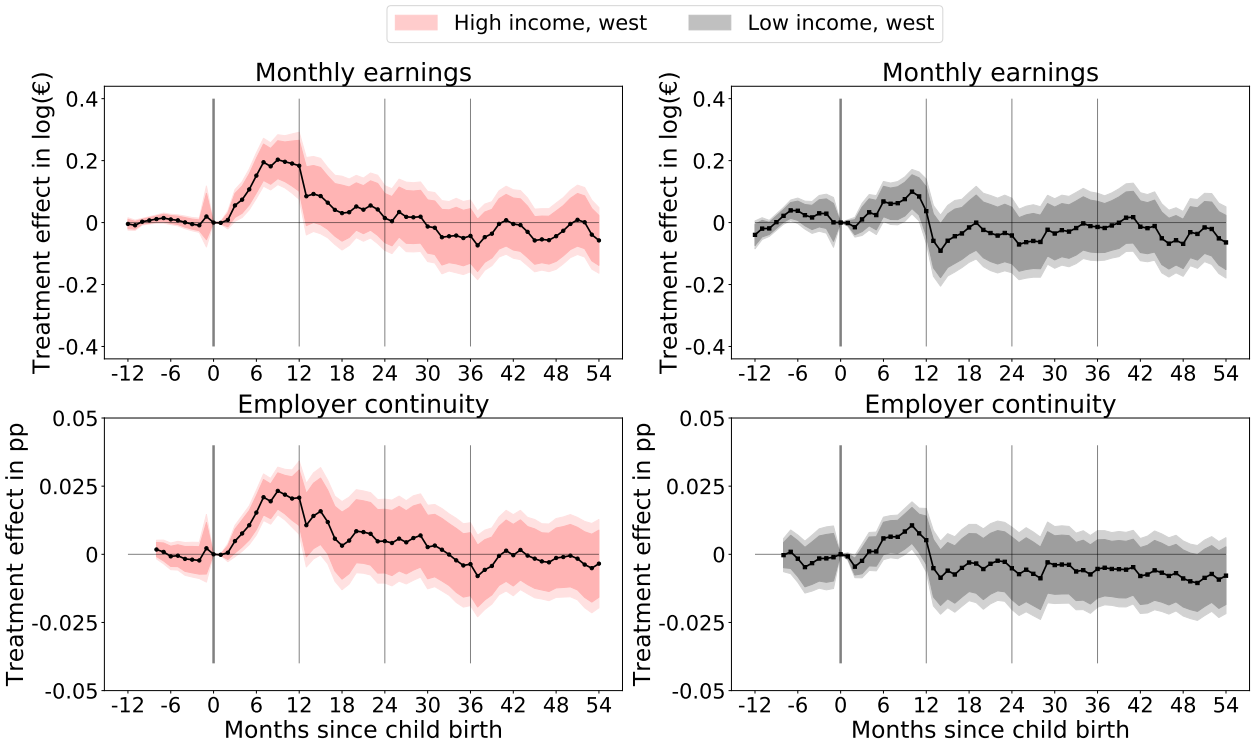
Motivated by this analysis, we analyze the full range of employment outcomes for high and low income mothers in West Germany in the panels on the left and right hand side of figure 4, respectively.[42] The first panel on the left illustrates that the employment effect in the first year is substantially larger (2.5 - 3 pp) among high income mothers when compared to the sample of all mothers. This is due to the fact that low income mothers barely take up PB+ as can be seen in the first panel on the right. The temporary drop in the employment probability after the third year is less pronounced for both high and low income mothers. The absence of a negative temporary effect on employment for high income mothers in the long run supports our interpretation that the drop in employment probability for all mothers (see figure 3) is not driven by the pro-part-time incentives of the paid PL reform. Given that the long-run effects on employment along the extensive margin are similar among both high and low income mothers, i.e. mothers who take up PB+ and those who do not, they are most likely due to the change in the *unpaid* PL legislation, which makes the timing for employment leave more flexible for all mothers irrespective of whether they take PB+ or not.

To assess whether pro-part-time incentives lead to a lock in effect into part-time employment we analyze the policy's effects on part-time and full-time employment in the panels in the second and third rows of figure 4. While we would observe such a lock-in effect as a higher probability of working part-time and a lower probability of working full-time, we do not find any statistically significant effect on part-time or full-time employment among high or low income mothers after the first year. Focusing on high income mothers, who strongly respond to the policy's pro-part-time incentives, the second panel shows that the point estimates on part-time employment after the third year closely match the ones observed for employment at the extensive margin (panel 1). This suggests that high income mothers who go on unpaid PL during that period do so by taking time off their part-time work. Furthermore, the effect on full-time employment among high income mothers after the third year is a precisely estimated zero (panel 3, left). Given that some full-time employed mothers might go on *unpaid* PL (i.e. unemployment) during this period, our estimate represents a lower bound for the true effect of pro-part-time incentives shortly after child birth on full-time employment. These results allow us to rule out that pro-part-time incentives lead to a lock-in effect into part-time employment in the case of the policy that we analyze.

We do not find a consistent, statistically significant effect of the policy on monthly earnings for high or for low income mothers in the long run as illustrated in the first panel of figure 5. In some months after the third year, the negative point estimates are statistically significant at the 5% level, however, turn insignificant and close to zero thereafter. This temporary drop in monthly earnings closely matches the policy's effect on employment along the extensive margin, which is most likely driven by a more flexible unpaid PL legislation (see discussion of figure 4). Thus, also among high income mothers, who are particularly likely to take up the part-time paid PL scheme, we do not find that the reform's pro-part-time incentives significantly affect the child penalty up to 4.5 years after child birth. Employer continuity is largely unaffected by the policy. In the case of low income mothers the point estimate of approximately 1pp becomes significant at the 5% level, which is driven by a lower probability

[42] To ensure equal sample sizes in both sub samples, we define high and low income mothers as being above or below the 50^{th} percentile of the pre-child birth income distribution, respectively.

Figure 5: Additional employment outcomes of high and low income mothers in West Germany



The figures above report the DD coefficient of separate regressions for outcomes p months after child birth as specified in regression 3. The outcome variables in the first and second panel are monthly earnings and employer continuity, i.e. a dummy that equals 1 if a mother works for the same employer after child birth as at conception. The two first vertical lines roughly coincide with the end of PB (12-14 months) and the end of PB+ (24-28 months). The third vertical line represents the end of the employment protection period. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals.

Table 4: PB+ take-up and public child care availability

Outcome: *Employment in month*	3	6	9	12	15	18	21	24	27
treatMonths=1 × treatYear=1	0.033***	0.040***	0.054***	0.066***	0.002	-0.005	0.002	0.001	0.007
	(0.011)	(0.015)	(0.016)	(0.023)	(0.032)	(0.029)	(0.022)	(0.021)	(0.025)
treatMonths=1 × treatYear=1 × child care	-0.001*	-0.001	-0.001	-0.001	0.000	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean of outcome	0.051	0.096	0.141	0.310	0.573	0.640	0.649	0.661	0.666
SD of outcome	0.22	0.29	0.35	0.46	0.49	0.48	0.48	0.47	0.47
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	151743	151743	151743	151743	151743	151743	151743	151743	151743
Number of clusters	400	400	400	400	400	400	400	400	400

Note: the table above reports the coefficients of interest for employment as outcomes p months after child birth (in columns) as specified in regression 3. Three birth months on each side of the implementation month are included. The first row reports the OLS estimates of the DD model's main effect. The second row reports the main effect's interaction with the child care availability, which is measured at conception and standardized to mean = 0 and standard deviation = 1. Standard errors are clustered at the county level (400) and reported in parentheses. Descriptive statistics are reported at the bottom of each panel. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of employment in the same period (see figure 4).

5.2.2 Complementarities with public child care

Previous research documents that German mothers identify the insufficient availability of child care as a major constraint for maternal employment (Boneva et al., 2021). Therefore, we examine whether the policy's effect is larger in counties with a higher availability of public child care. In Germany, public child care is inexpensive by international standards due to high subsidies, however, the availability is limited. This makes it reasonable to proxy the likelihood of being able to access public child care by the availability of child care slots, disregarding financial considerations.

To assess our prediction of complementarities between PB+ take-up and public child care availability, we interact the baseline treatment effect with the public child care availability at the county level (see section 4.2 for details on measurement). We focus on the subsample of high income mothers in this exercise since low income mothers do not take up PB+ because of financial constraints.

Table 4 reports the resulting coefficients. Surprisingly, we do not find evidence for complementarities between the maternal employment effect, i.e. the take-up of the policy, and the availability of public child care. This result is robust to using more flexible specifications, such as interacting the main effect with a squared term of public child care. The absence of complementarities between the availability of public child care and employment in the first years after child birth seems to contrast previous survey evidence in the literature (Boneva et al., 2021). These differences can potentially be explained by the fact that only high income mothers return to work earlier as a result of the policy. Being less financially constrained, these mothers might have better access to other child care options, such as private providers. Additionally, fathers, who are incentivized to work less by PB+, might (partially) compensate for the absence of mothers by engaging more in child care duties at home.[43] Taken together,

[43] Unfortunately, we are not able to distinguish between these two potential explanations due to the lack of data on paternal labor supply and non-public child care options.

we do not find that the availability of public child care fosters employment for high income mothers in the first year after child birth.

6 Robustness checks

In this section we present a series of robustness checks to corroborate our findings and, in particular, their causal interpretation.

Placebo tests If the observed results are indeed caused by the introduction of PB+, we should not observe any statistically signifcant treatment effect in other years. To examine whether this is the case we perform placebo tests, in which we estimate our baseline specification (equation 3) and sequentially define births in the pre-reform years 2011 - 2014 as treated. To ensure that the control group is not affected by the policy, parents who give birth in the actual reform year are dropped from the sample when estimating the placebo regressions (2011-2014 treated). Figure A9 shows the estimates of the true implementation year in panel 1 and of the placebo treatments in panels 2-5. It is evident that the positive employment impact is only present when the treatment status is assigned using the true implementation year. For all placebo treatments, the coefficients in the first year is close to zero and statistically insignificant.

Month of birth window around implementation date In our baseline results we restrict the sample of analysis to two birth months, excluding the ones immediately preceding/following the implementation month. Since the policy was introduced on 1 July, our sample of analysis includes the birth months April, May, August and September. In this way we ensure that parents on either side of the threshold are more comparable than if we included all births, given the seasonal differences in characteristics of parents by birth month (Buckles and Hungerman, 2013). As a robustness check, we test whether our main findings hold when further restricting birth month window to one month on either side of the threshold. Figures A10 to A13 show that the effects on post-child birth employment patterns under this alternative specification are very similar.

Characteristics at conception The causal interpretation we give to our findings requires that the introduction of the policy was not anticipated by parents. If it was not anticipated, there should not be any differences in parental characteristics, measured at conception, between parents whose child is born before or after the implementation date (net of seasonality). We test whether such differences exist by estimating our baseline model (as specified in equation 3 but excluding controls) in which we take a set of parental characteristics at conception as outcomes. Figure A14 reports the corresponding results. The Difference-in-Differences model's main effects for each characteristic is reported in rows. The differences in characteristics at conception are small in magnitude ($<$ 0.01 sd) and statistically insignificant at a 5% significance level, which makes anticipatory timing of births unlikely and corroborates the causal interpretation of our results.

7 Discussion

In this section we discuss to what extent the PB+ reform (i.e. the pro-part-time incentives and a longer benefit duration) has achieved its goals and what policy recommendations can be drawn from our analysis more broadly. The policy's goal is to increase the compatibility of work and family life in a gender-neutral way.[44] This means that the policy intends to *i)* encourage mothers to return to work earlier after child birth such that their time off work is reduced, *ii)* make fathers work less such that they can allocate more time to child care duties and *iii)* encourage couples to share child care duties more evenly.

Our analysis shows that the policy clearly achieves its first goal of encouraging mothers to return to work earlier as the share of mothers who return to work during the first after child birth, instead of later years, increases by approximately 10% on average. However, it is not clear whether this higher maternal labor market attachment in the short run is desirable from a policy perspective as it does not translate into labor market gains for mothers (i.e. a reduction of the child penalty) in the long run. To answer this question, our study of maternal labor market effects would need to be complemented with additional analyses. An assessment of the policy's effects on child development, which depend on the quality of child care with which the mother's care is substituted, would be needed. Prior research suggests that the period in which PB+ incentivizes mothers to return to work (i.e. the first year after child birth) is particularly important for child development (e.g. Heckman, 2008; Cunha and Heckman, 2007). Additionally, the evidence concerning the effects of early childhood education on children from high-income households, which are precisely the ones that are affected by PB+, is mixed (e.g. Drange and Havnes, 2019; Fort et al., 2019).

Another important factor for the assessment of the policy's overall effects, is whether PB+ encourages fathers to reduce their labor supply and to contribute more to child care duties, in particular in couples in which mothers return to work earlier as a result of the policy. The absence of complementarities between the availability of public child care and the take-up of PB+ among mothers could be explained by a higher fraction of fathers taking care of the child as mothers return to work earlier. However, according to official statistics, five times more mothers than fathers choose PB+ in 2015, suggesting that paternal child care could account for at most a fraction of the increase in the supply of non-maternal child care (DESTATIS, 2019).[45] Alternatively, private child care centers or informal agreements with friends and family could compensate the lower maternal child care involvement. As our analysis is limited by the lack of data on child development, paternal labor supply and alternative child care arrangements, further research is needed to provide a comprehensive assessment of PB+.

Furthermore, as we show theoretically and empirically, the benefit structure introduced with PB+ encourages only high income mothers to take up the policy. Since PB+ pays a lower monthly benefit amount compared to the alternative PB for most mothers, the new scheme is unattractive for low income mothers. As official documents do not make any reference to specifically targeting high income mothers, this is most likely an unintended feature of

[44] See https://www.bmfsfj.de/bmfsfj/themen/familie/familienleistungen/elterngeld/elterngeld-73752?view= for more information available in German (as of 22 September 2022).

[45] According to DESTATIS (2019), 71'000 and 13'000 mothers and fathers choose PB+ in 2015, respectively.

the policy and emphasizes that the effects of incentive structures on different subgroups need to be taken into account when designing policies.

In terms of public finances, the part-time paid PL option does not increase public expenditure on paid PL benefits for mothers. This is explained by the fact that in Germany, as in most other countries, the total amount of paid PL benefits a mother receives if she works after child birth is lower than the amount she receives if she does not work. Based on back-of-the-envelope calculations, we estimate that mothers in the control group receive $\approx$ 9800€ in benefit payments through PB in total, while eligible mothers who take up the part-time scheme receive a total benefit amount of $\approx$ 9200€.[46] This shows that governments can - if deemed desirable - shorten the time mothers spend away from work after child birth without increasing public expenditure.

8 Conclusion

In this paper we analyze how monetary incentives to part-time work within a paid PL scheme affect maternal labor market outcomes up to 4.5 years after child birth. To do so, we study a German paid parental leave reform which allows eligible mothers to choose a new part-time paid PL option that monetarily incentivizes mothers to work at reduced hours during the first 24 months after child birth.

We find that only high income mothers (compliers) choose the new part-time paid PL option, while low-income mothers are unaffected by the new scheme due to financial constraints and the design of the benefit amount calculation. High income mothers return to the labor market earlier after child birth as a result of the policy, which reduces their child penalty during the first year after child birth. We do not find that the policy's pro-part-time incentives affect maternal employment along the intensive margin (part-time or full-time work) or the child penalty in the long run. Thus, our analysis alleviates concerns of a lock-in effect into part-time employment, according to which mothers stick to part-time employment in the long-run if incentivized to work at reduced hours directly after child birth (e.g. Kunze, 2022; Joseph et al., 2013).

The policy achieved its primary goal of encouraging mothers to return to work earlier after child birth. It did so without increasing public expenditure on paid PL benefits for mothers. However, the fact that returning to work earlier does not translate into labor market gains for mothers in the long run raises the question of whether this effect is desirable from a policy perspective. While facilitating the return to work for mothers, irrespective of their long run labor market outcomes, can be considered beneficial in itself, it could also lead to detrimental effects on child development as mothers are less involved in child care. Additionally, a complete assessment of the policy would also require an analysis of its effects on paternal involvement in child care and the division of child care duties within couples. We see our analysis as a first step towards understanding these overall effects of pro-part-time options in paid PL schemes.

Our study raises additional questions for future research. The paid PL reform we study disproportionately

[46] These numbers are based on our estimated take-up of the policy (proxied by the employment effect in the first year) and the labor income after compared to before child birth. Furthermore, we assume that mothers receive paid PL benefits for the maximum number of available months under each scheme. Our estimates match official statistics provided by the Federal Statistical Office (DESTATIS, 2019).

affects high income mothers. Studying paid PL systems in countries that employ different incentive structures to foster part-time employment could create a broader understanding of the effects of pro-part-time policies for mothers with other characteristics. Moreover, incentives for part-time work during *unpaid* PL, for which parents are typically eligible when the child is already older, might have a different effect on (long-run) labor market outcomes.[47] Since, in contrast to our setting, a large fraction of mothers already work when taking up unpaid part-time PL, such policies are arguably more likely to lead to *reductions* in working hours in the short run. Consequently, the long-run effects potentially differ from the ones we find.

[47] For example, Spain currently offers parents to reduce working hours to part-time employment while their child is less than 12 years old.

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A Appendix

A.1 Background: maternal employment in Germany

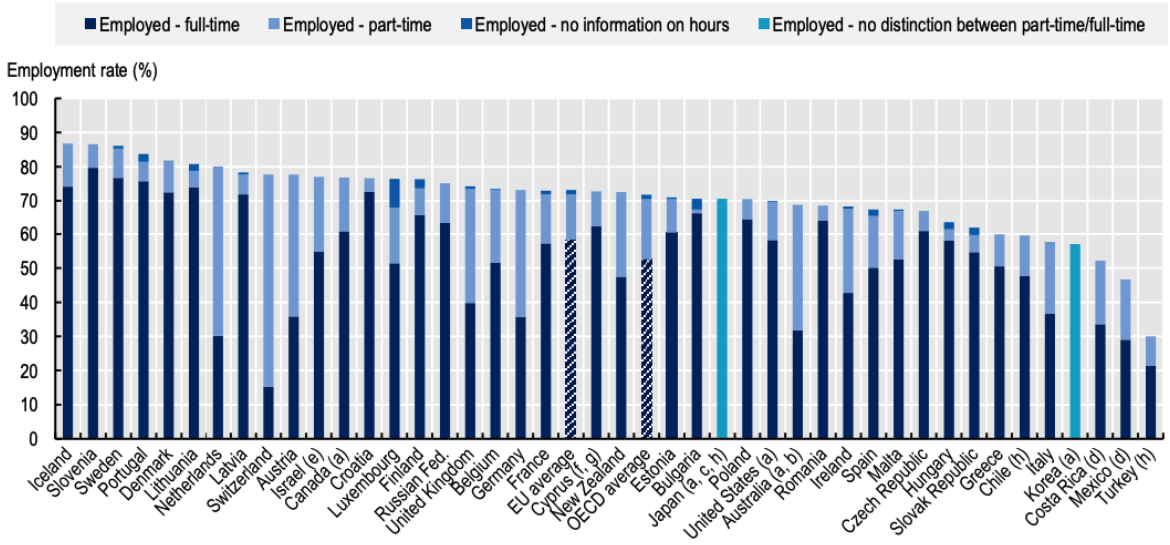
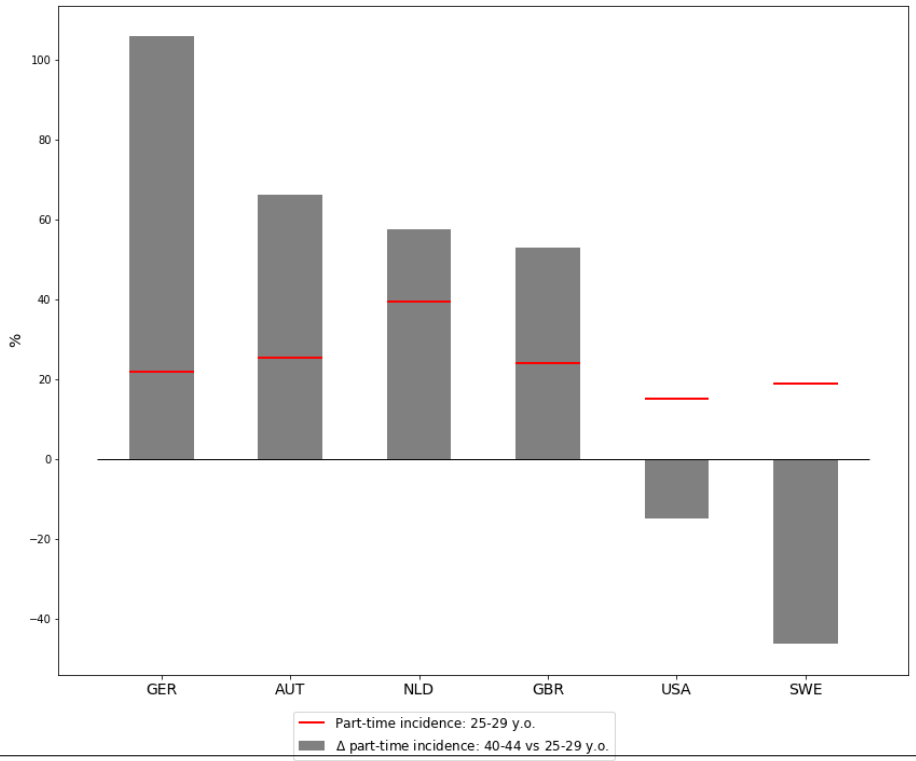


Figure A1: Part-time work across countries (OECD Family Data Base)

Figure A2: Part-time work across countries: before and after birth (OECD Family Data Base)



The figure above shows the incidence of part-time work among women aged 25-29 (proxy for before child birth) in red as well as the change in part-time incidence between women aged 40-44 (proxy for after child birth) and those aged 25-29 in grey. Age groups as a proxy for before/after child birth are motivated by the fact that we are not aware of a data set that offers consistent information across a set of countries on part-time incidence and information on (past) child births. The data come from the OECD Employment Database

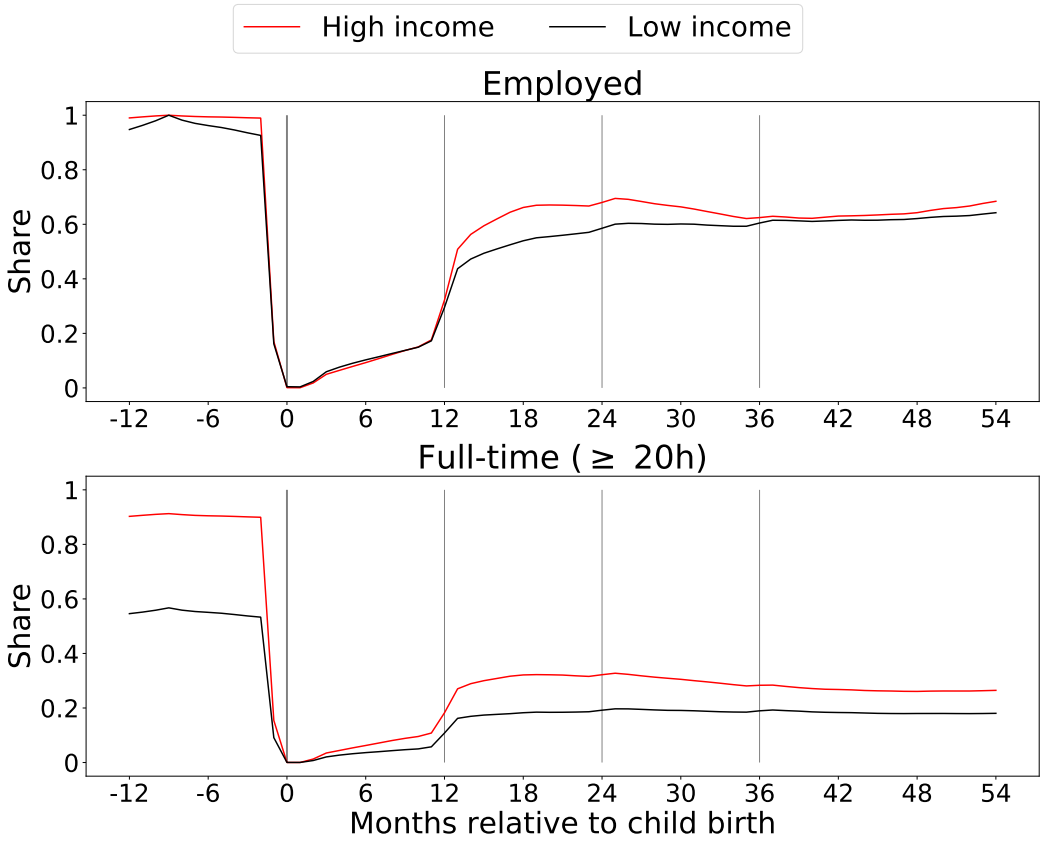
A.2 Institutional setting: additional notes

Table A1: Examples of benefit calculations in official documents

	PB			PB+	
Post-birth income	Monthly	Total	Limit	Monthly	Total
0	1300	15,600	650	650	15,600
500	975	11,700	650	650	15,600
1200	520	6,240	650	520	12,480

Note: The examples above are calculated for a parent with a net income prior to child birth of 2000€. It shows the monthly and total benefit amount paid under PB and PB+ in columns 2 - 3 and 5 - 6, respectively. The limit in column 3 refers to the maximum monthly benefit paid under PB+ which equals 50% of the monthly benefit under PB without post-child birth earnings.

Figure A3: Parental labor supply choices before PB+



The figure above shows employment patterns for mothers and fathers from one year before child birth to 4.5 years after child birth. The data correspond to the married mothers and fathers samples excluding the reform year (2015).

A.3 A 2-period model: solution

This section provides more details on the solution of the 2-period labor-leisure model outlined in section 3. In our model a representative agent faces the following optimization problem:

$$\max_{c_1,c_2,l_1,l_2} U(c_1,k_1)+\beta\cdot U(c_2,k_2) \tag{4}$$

$$s.t. \quad c_1+\frac{c_2}{(1+r)}=(1-\tau_1)w\cdot l_1+b_1+\frac{(1-\tau_2)w\cdot l_2+b_2}{1+r}$$

$$k_t=1-l_t$$

$$b_t=\tau_t\cdot w\cdot l_0$$

Additionally, PB+ imposes the following conditions on post-child birth labor supply (see below for details):

$$\text{Under } PB+^{high}: \qquad l_1>0.5\cdot l_0 \quad \text{and} \quad l_2>0.5\cdot l_0 \tag{5}$$

$$\text{Under } PB+^{low}: \qquad l_1\leq 0.5\cdot l_0 \quad \text{and} \quad l_2\leq 0.5\cdot l_0 \tag{6}$$

The agent chooses consumption c_t and hours worked l_t (and thereby implicitly child care k_t) in periods 1 and 2. She faces a standard inter-temporal budget constraint augmented by several terms that model the paid PL scheme: $b_t-(\tau_t\cdot w\cdot l_t)=\gamma_t\cdot w\cdot l_0-(\tau_t\cdot w\cdot l_t)$, where b_t is the pre-birth income dependent amount that a parent receives irrespective of post-birth income and the second term is the amount by which the benefit amount is reduced if a parent works post-child birth under PB (and under some conditions also under PB+). In this specification, γ_t governs the fraction of the pre-birth earnings that are replaced by the PL scheme and τ_t defines the fraction of the post-birth earnings that are subtracted from the former amount. The policy parameters thagt correspond to each paid PL option are reported in table A2.

	First year		Second year		
Scheme	γ_1	τ_1	γ_2	τ_2	Condition
PB	0.65	0.65	0	0	
$PB+^{low}$	0.65/2	0	0.65/2	0	$l_1,l_2\leq\frac{1}{2}\cdot l_0$
$PB+^{high}$	0.65	0.65	0.65	0.65	$l_1,l_2>\frac{1}{2}\cdot l_0$

Table A2: Policy parameters

The Kuhn Tucker conditions (equations 5 and 6) are derived from the inequalities imposed by the benefit amount calculation under PB+. The resulting first-order-conditions of the maximization problem are as follows:

$$\frac{U_k(k_1)+\lambda_2}{U_c(c_1)} = w\cdot(1-\tau_1) \tag{7}$$

$$\frac{U_k(k_2)+\lambda_3}{U_c(c_2)} = w\cdot(1-\tau_2) \tag{8}$$

$$\frac{U_c(c_1)}{U_c(c_2)} = \beta\cdot(1+r) \tag{9}$$

$$\frac{U_k(k_1)+\lambda_2}{U_k(k_2)+\lambda_3} = \frac{(1-\tau_1)}{(1-\tau_2)}\beta\cdot(1+r) \tag{10}$$

$$-\lambda_1\cdot(c_1+\frac{c_2}{(1+r)}-(1-\tau_1)\cdot w\cdot l_1-b_1-\frac{(1-\tau_2)\cdot w\cdot l_2+b_2}{1+r}) = 0 \tag{11}$$

$$-\lambda_2\cdot(l_1-0.5\cdot l_0) = 0 \tag{12}$$

$$-\lambda_3\cdot(l_2-0.5\cdot l_0) = 0 \tag{13}$$

The Lagrange multipliers λ_1, λ_2 and λ_3 in equations 7 to 13 correspond to the intertemporal budget constraint and the constraints on labor supply in periods one and two, respectively. Assuming logarithmic utility, i.e. $U(c_t,k_t) = ln(c_t)+ln(k_t)$, we can solve for the optimal labor supply choices in periods 1 and 2, i.e. l_1^* and l_2^*, and their partial derivatives with respect to all policy parameters of interest:

$$l_1^*(\tau_t,\gamma_t,l_0,r,\beta) = 1-\frac{1}{2+2\beta}-\frac{1-\tau_2}{(2+2\beta)(1+r)(1-\tau_1)}-\frac{\gamma_1\cdot l_0+\frac{\gamma_2\cdot l_0}{1+r}}{(2+2\cdot\beta)(1-\tau_1)} \tag{14}$$

$$l_2^*(\tau_t,\gamma_t,l_0,r,\beta) = 1-\frac{1-\tau_1}{1-\tau_2}\cdot\beta(1+r)\cdot(1-l_1^*) \tag{15}$$

$PB{+}^{low}$ and $PB{+}^{high}$ can only be chosen if $l_1,l_2 \leq \frac{1}{2}\cdot l_0$ and $l_1,l_2 > \frac{1}{2}\cdot l_0$, respectively. This gives rise to the following threshold values $\overline{l_{0,1}^j}$ for $t\in\{1,2\}$ and $j\in\{\text{PB+}^{low}, PB{+}^{high}\}$:

$$l_1^* = 0.5\cdot l_0: \quad \Rightarrow \quad l_0 = \frac{1+2\beta-\frac{1-\tau_2}{(1+r)(1-\tau_1)}}{1+\beta+\frac{\gamma_1+\frac{\gamma_2}{1+r}}{1-\tau_1}} = \overline{l_{0,1}}^j \tag{16}$$

$$l_2^* = 0.5\cdot l_0: \quad \Rightarrow \quad l_0 = \frac{2-\beta(1+r)(\frac{1-\tau_1}{1-\tau_2}-\frac{1}{1+r})}{1+\beta(1+\frac{1+r}{1-\tau_2}(\gamma_1+\frac{\gamma_2}{1+r}))} = \overline{l_{0,2}}^j \tag{17}$$

The optimal labor supply choices l_1^* and l_2^* together with the threshold values $\overline{l_{0,1}^j}$, which define the areas in which optimal labor supply is constrained, are illustrated in figure A4.

To solve for the agent's choice between PB, $PB{+}^{low}$ and $PB{+}^{high}$, we compute the utility level resulting from each of these options (see figure A5). The optimal policy choice depends on the agent's pre-child birth labor supply, which equals pre-child birth income since wages are constant in all periods:

$$\underset{PB,\ PB+^{low},\ PB+^{high}}{\text{argmax}} \quad Problem \quad (4) = \left\{ \begin{array}{cc} PB & \text{if } l_0 <= \overline{l_{0,t}}^{high} \\ PB+^{high} \text{ or } PB+^{low} & \text{if } \overline{l_{0,t}}^{high} < l_0 <= \overline{l_{0,t}}^{low} \\ PB+^{low} & \text{if } \overline{l_{0,t}}^{low} < l_0 \end{array} \right\}$$

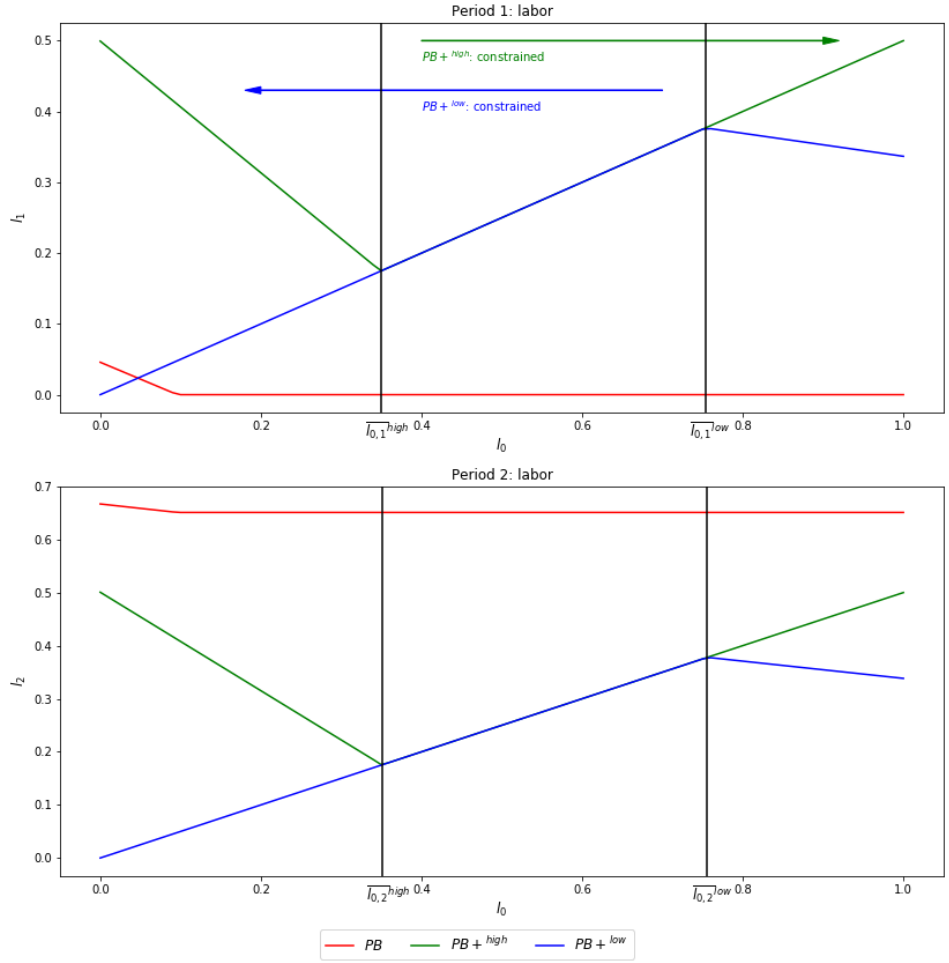


Figure A4: Labor choices by policy options

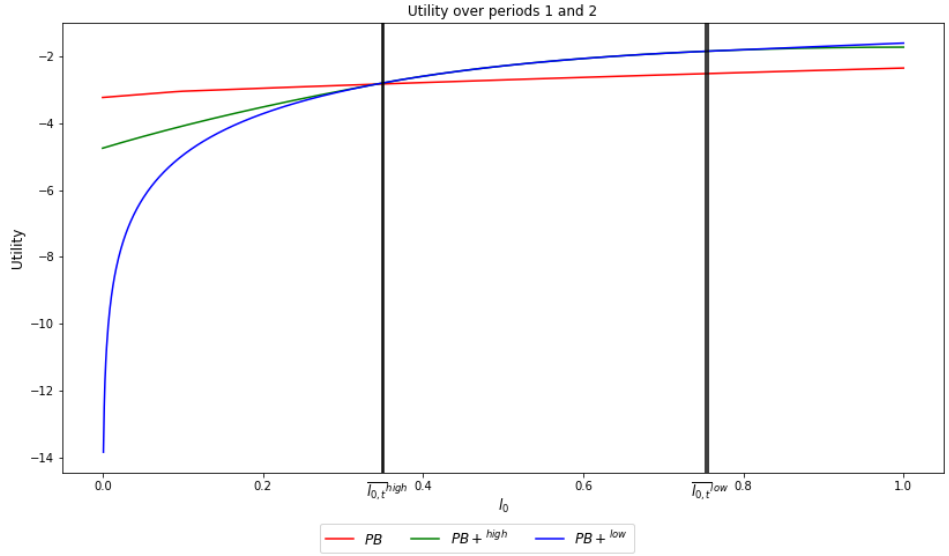


Figure A5: Utiliy by policy options

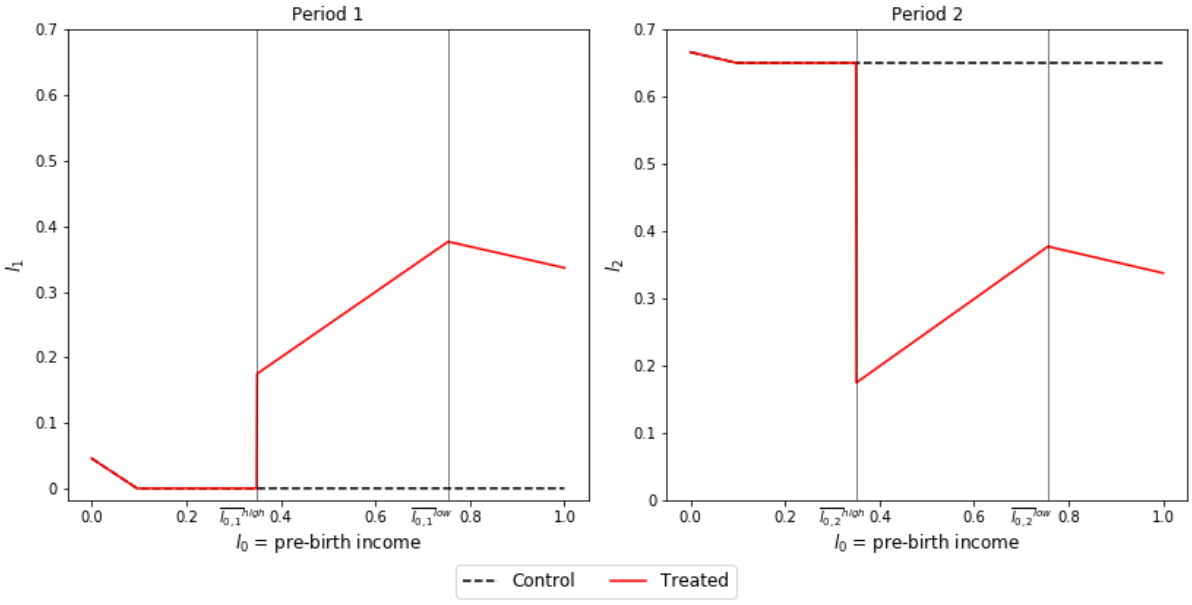
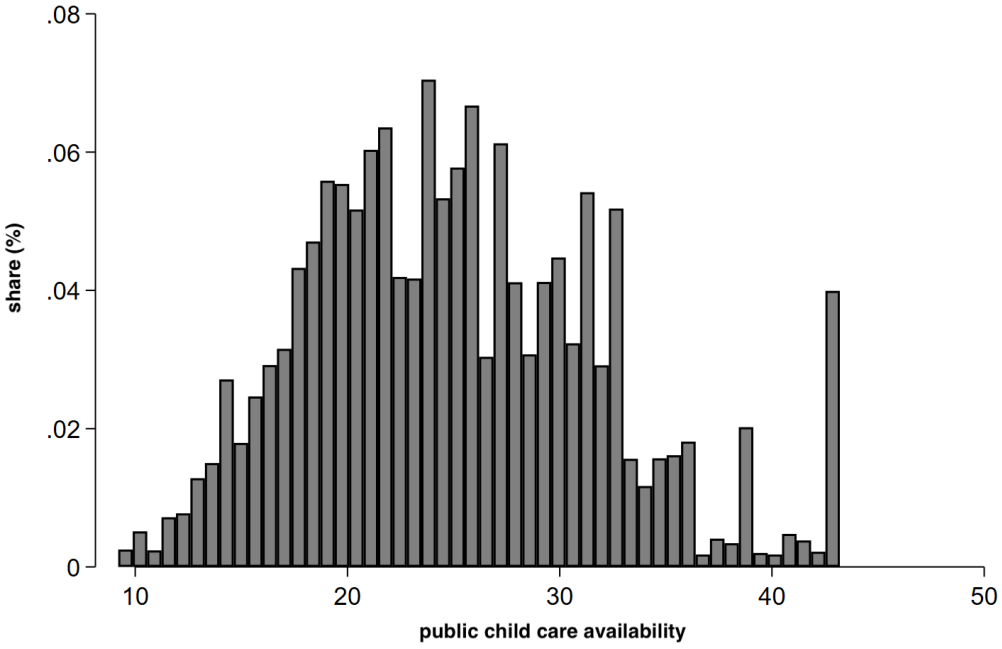


Figure A6: Labor supply choices as predicted by 2-period model

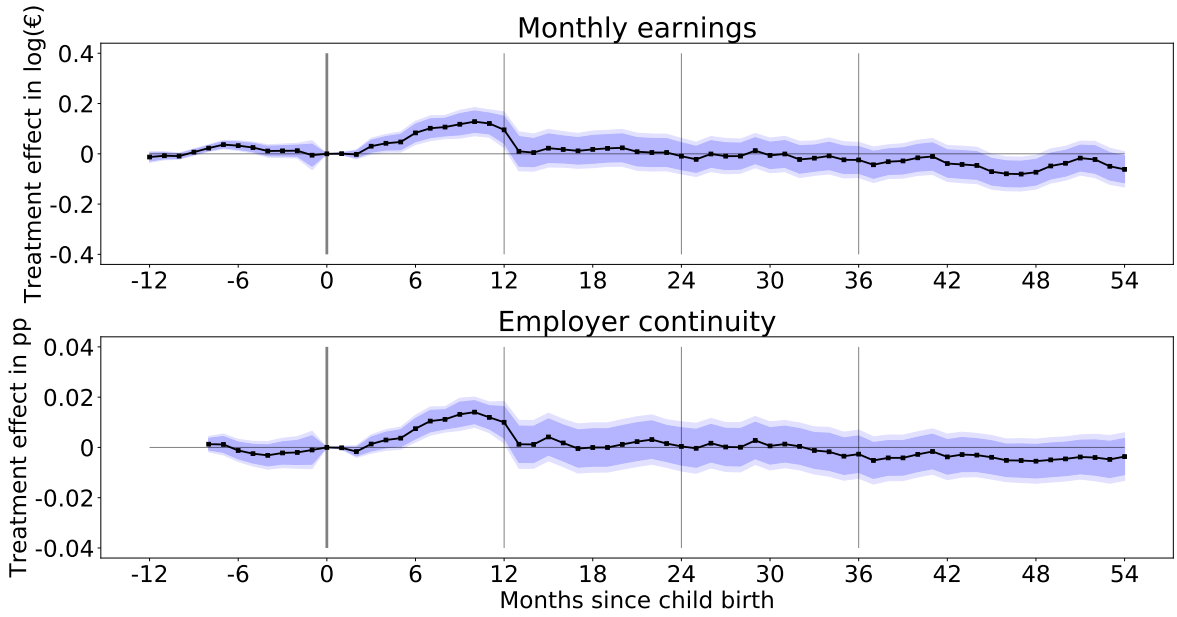
A.4 Additional results: figures

Figure A7: Public child care availability by county



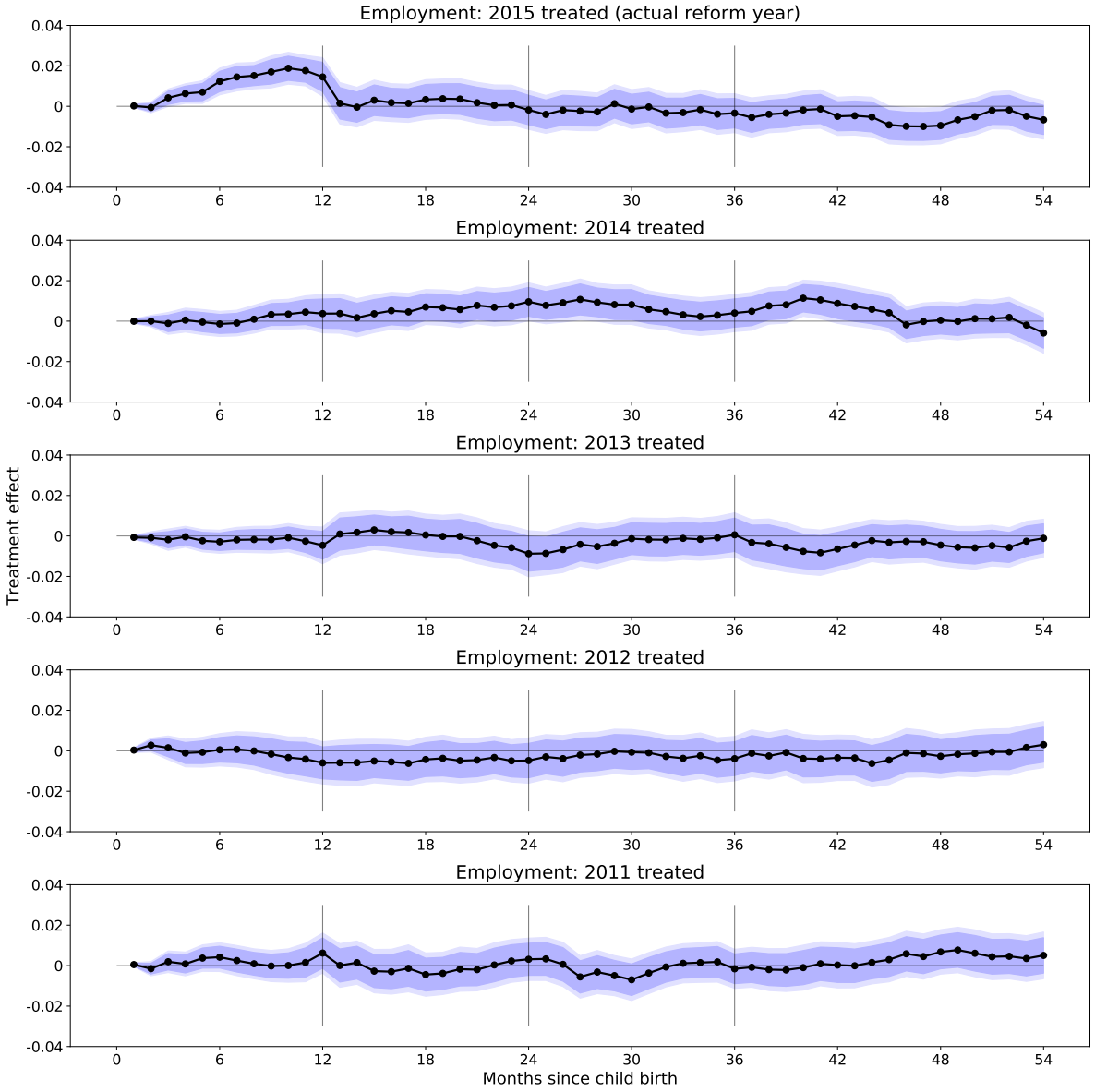
The figure above shows the distribution of public child care availability for 0-2 year old children at the county level (Kreis). The data have been trimmed at 98^{th} percentile.

Figure A8: Additional employment outcomes of all mothers



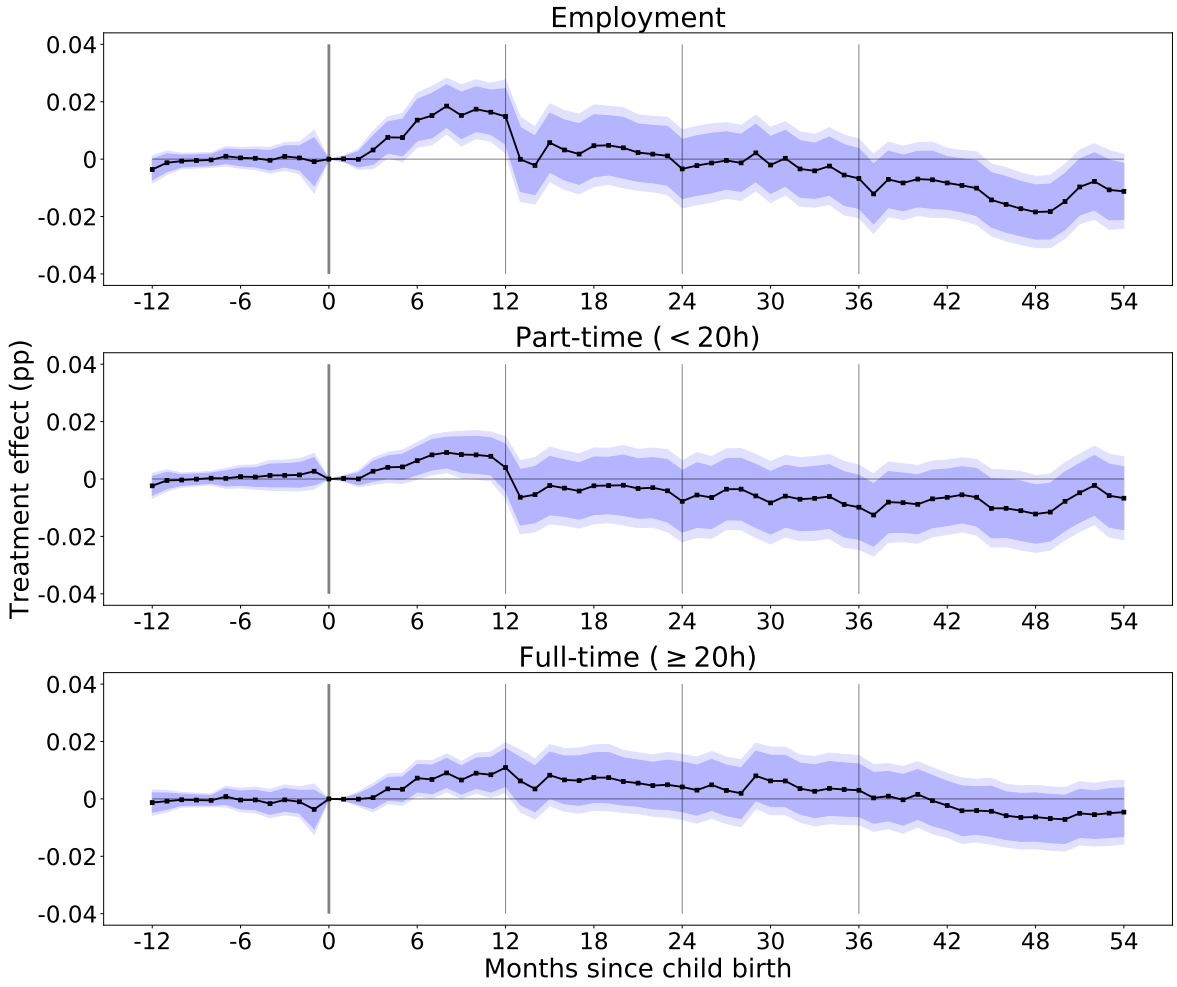
The figures above report the DD coefficient of separate regressions for outcomes p months after child birth as specified in regression 3. The outcome in the first panel is the monthly earnings (log). The outcome in panel two is a dummy taking a value of 1 if an individual is employed at the same employer as before child birth and 0 otherwise. The two first vertical lines roughly coincide with the end of PB (12-14 months) and the end of PB+ (24-28 months). The third vertical line represents the end of the employment protection period. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals.

Figure A9: Placebo tests for all mothers



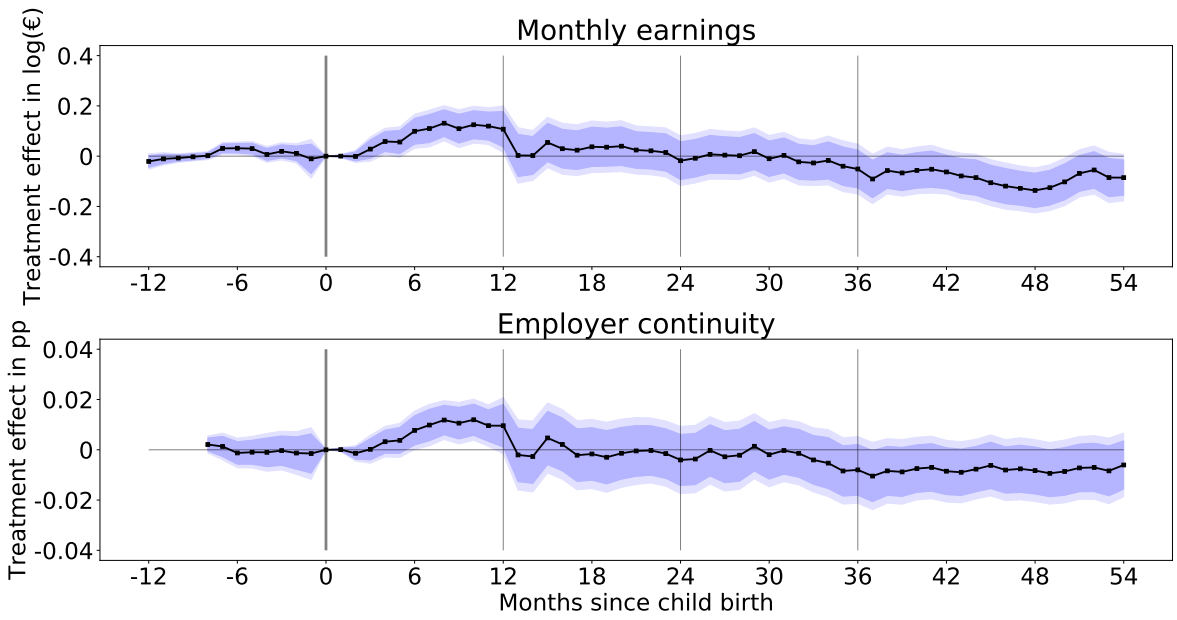
The figures above report the DD coefficient of separate regressions for employment as outcome p month after child birth as specified in regression 3. Two birth months on each side of the implementation month are included. In the first panel, the actual reform year (2015) is specified as treatment variable, while the years 2011 to 2014 are used for treatment assignment as a placebo test in panels 2-5. The actual reform year (2015) is excluded from the sample in panels 2-5. The two first vertical lines roughly coincide with the end of PB (12-14 months) and the end of PB+ (24-28 months). The third vertical line represents the end of the employment protection period. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals.

Figure A10: Employment outcomes - one month birth window



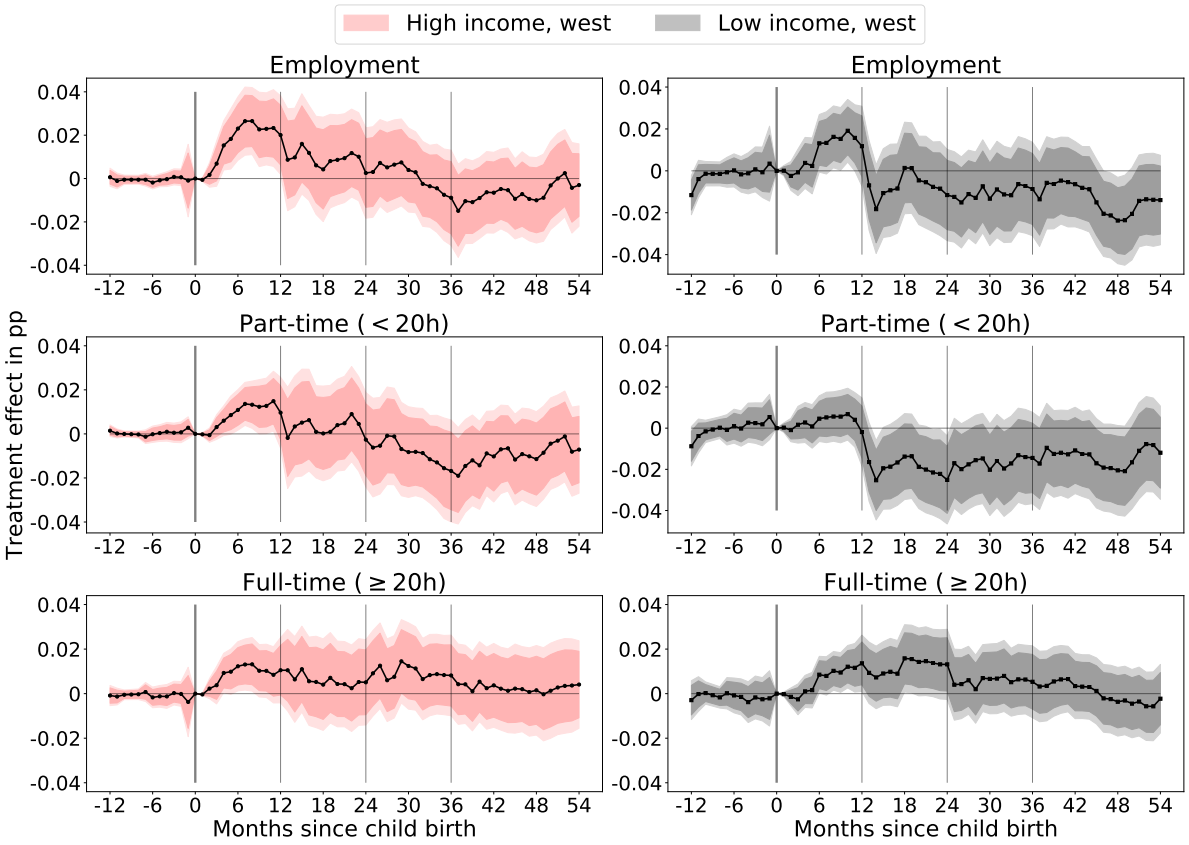
Note: the figures above report the DD coefficient of separate regressions for outcomes p month after child birth as specified in regression 3. One birth month on each side of the implementation month are included. The outcome variable in the first panel is a dummy for whether an individual is working. The outcome in panel two (three) is a dummy for whether an individual has a part-time (full-time) work contract, i.e. less than (more than) 20 hours of work per week. The two first vertical lines roughly coincide with the end of PB (12-14 months) and the end of PB+ (24-28 months). The third vertical line represents the end of the employment protection period. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals.

Figure A11: Employment outcomes - one month birth window (cont.)



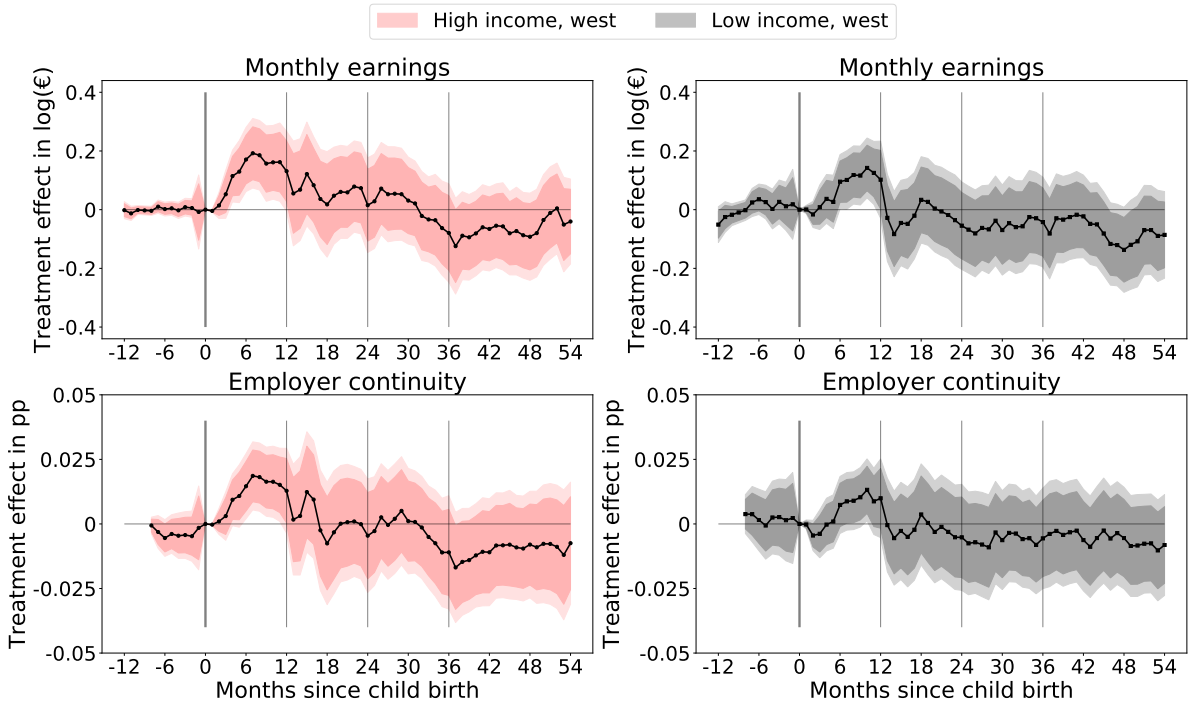
Note: the figures above report the DD coefficient of separate regressions for outcomes p month after child birth as specified in regression 3. One birth month on each side of the implementation month are included. The outcome variables are monthly earnings and employer continuity, respectively. The two first vertical lines roughly coincide with the end of PB (12-14 months) and the end of PB+ (24-28 months). The third vertical line represents the end of the employment protection period. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals.

Figure A12: Employment outcomes by income - one month birth window



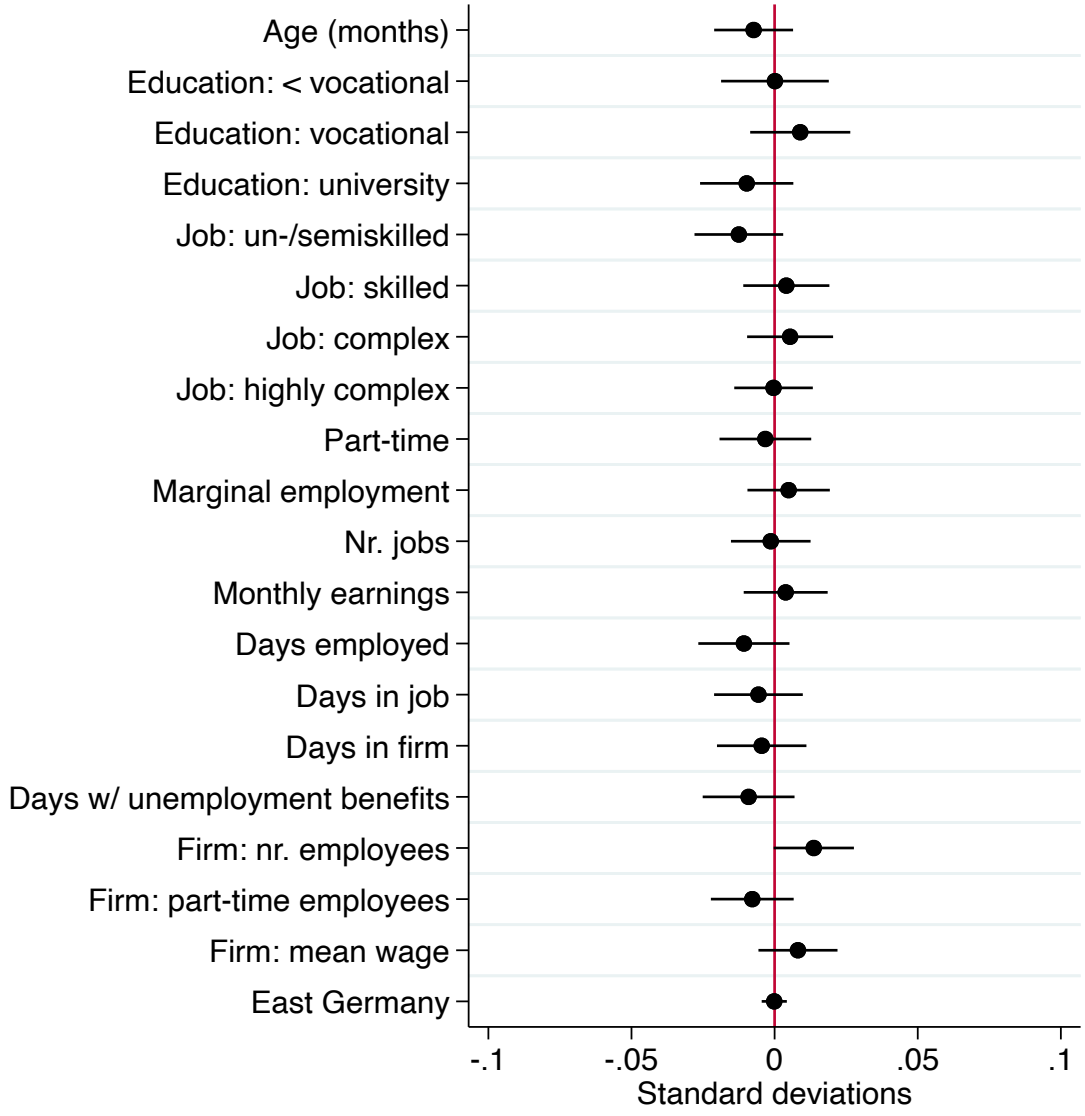
Note: the figures above report the DD coefficient of separate regressions for outcomes p month after child birth as specified in regression 3. One birth month on each side of the implementation month are included. The outcome variable in the first panel is a dummy for whether an individual is working. The outcome in panel two (three) is a dummy for whether an individual has a part-time (full-time) work contract, i.e. less than (more than) 20 hours of work per week. The two first vertical lines roughly coincide with the end of PB (12-14 months) and the end of PB+ (24-28 months). The third vertical line represents the end of the employment protection period. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals.

Figure A13: Employment outcomes by income - one month birth window (cont.)



Note: the figures above report the DD coefficient of separate regressions for outcomes p month after child birth as specified in regression 3. One birth month on each side of the implementation month are included. The outcome variables are monthly earnings and employer continuity, respectively. The two first vertical lines roughly coincide with the end of PB (12-14 months) and the end of PB+ (24-28 months). The third vertical line represents the end of the employment protection period. Standard errors are clustered at the county level (401). The shaded areas represent the coefficients' 95% and 99% confidence intervals.

Figure A14: Characteristics at conception - all mothers



Note: Each row in the figure above represents a separate estimate of the Difference-in-Differences model's main effect (as specified in equation 3, excluding controls) with the variable labeled on the left hand side as dependent variable. The dependent variables are standardized s.t. mean = 0 and sd = 1. Two birth months on each side of the implementation month are included. Standard errors are clustered at the county level (401). The bars represent the coefficients' 95% confidence intervals.

A.5 Additional results tables

Table A3: Descriptive statistics of all mothers

Characteristics at conception	mean
age (months)	29.44
part-time	0.26
monthly earnings (€, gross)	2,541.09
Education:	
$<$ vocational	0.06
vocational	0.71
university	0.23
Skill level (job):	
un-/semiskilled	0.09
skilled	0.64
complex	0.12
highly complex	0.15
Employment history:	
days employed	2,523.50
days in job	1,266.76
days in firm	1,305.99
days w/ unemployment benefits	109.09
Firm characteristics:	
nr. employees	87.36
part-time employees	0.33
female employees	0.66
mean wage (€, daily)	93.69
Observations	380,717

Note: The table above reports mean values in observable characteristics, measured at conception for the full sample of analysis

Table A4: Employment outcomes for all mothers

Outcome:									
Employment in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.012***	0.014***	0.003	-0.001	-0.001	-0.003	-0.005	-0.010***	-0.007**
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Mean of outcome	0.097	0.308	0.601	0.634	0.633	0.614	0.623	0.632	0.664
SD of outcome	0.30	0.46	0.49	0.48	0.48	0.49	0.48	0.48	0.47
Part-time in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.006***	0.008***	-0.000	0.000	-0.003	-0.003	-0.005	-0.007*	-0.006
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Mean of outcome	0.048	0.163	0.349	0.377	0.385	0.378	0.397	0.411	0.441
SD of outcome	0.21	0.37	0.48	0.48	0.49	0.48	0.49	0.49	0.50
Full-time in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.006***	0.006**	0.004	-0.001	0.002	0.000	-0.001	-0.004	-0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mean of outcome	0.049	0.144	0.251	0.257	0.248	0.236	0.225	0.220	0.222
SD of outcome	0.22	0.35	0.43	0.44	0.43	0.42	0.42	0.41	0.42
Earnings in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.083***	0.095***	0.018	-0.010	-0.006	-0.024	-0.039	-0.073***	-0.062**
	(0.018)	(0.027)	(0.028)	(0.027)	(0.027)	(0.027)	(0.028)	(0.026)	(0.027)
Mean of outcome	0.691	2.225	4.408	4.680	4.686	4.568	4.627	4.713	4.944
SD of outcome	2.13	3.36	3.62	3.58	3.59	3.63	3.62	3.61	3.54
Employer continuity in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.007***	0.010***	-0.000	0.000	0.001	-0.003	-0.004	-0.005	-0.004
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
Mean of outcome	0.071	0.222	0.409	0.403	0.376	0.342	0.323	0.308	0.309
SD of outcome	0.26	0.42	0.49	0.49	0.48	0.47	0.47	0.46	0.46
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	380717	380717	380717	380717	380717	380717	380717	380717	380717
Number of clusters	401	401	401	401	401	401	401	401	401

Note: the table above reports the DD models' main effect on outcomes p months after child birth (in columns) as specified in regression 3. The panels (from top to bottom) show the following outcomes: a dummy for employment, for part-time work (less than 20 hours per week), for full-time work (20 hours per week or more), monthly earnings and employer continuity (a dummy for working for the same employer after child birth as at conception). Standard errors are clustered at the county level (401) and reported in parentheses. Descriptive statistics are reported at the bottom of each panel. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Employment outcomes for high income mothers in West Germany

Outcome:									
Employment in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.021***	0.026***	0.006	0.003	-0.001	-0.004	0.001	-0.005	-0.006
	(0.004)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Mean of outcome	0.096	0.310	0.640	0.662	0.647	0.606	0.616	0.630	0.675
SD of outcome	0.29	0.46	0.48	0.47	0.48	0.49	0.49	0.48	0.47
Part-time in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.009***	0.013***	-0.003	-0.000	-0.007	-0.007	-0.002	-0.005	-0.009
	(0.002)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mean of outcome	0.032	0.140	0.342	0.360	0.360	0.341	0.365	0.386	0.427
SD of outcome	0.18	0.35	0.47	0.48	0.48	0.47	0.48	0.49	0.49
Full-time in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.011***	0.013***	0.009	0.003	0.007	0.003	0.003	0.000	0.003
	(0.003)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Mean of outcome	0.064	0.170	0.298	0.302	0.286	0.265	0.250	0.244	0.249
SD of outcome	0.24	0.38	0.46	0.46	0.45	0.44	0.43	0.43	0.43
Earnings in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.152***	0.183***	0.030	0.015	-0.013	-0.043	-0.005	-0.044	-0.058
	(0.028)	(0.042)	(0.046)	(0.040)	(0.044)	(0.045)	(0.043)	(0.041)	(0.041)
Mean of outcome	0.733	2.359	4.883	5.081	4.975	4.686	4.746	4.862	5.202
SD of outcome	2.27	3.54	3.70	3.67	3.71	3.80	3.78	3.76	3.64
Employer continuity in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.015***	0.021***	0.003	0.005	0.003	-0.004	-0.000	-0.001	-0.003
	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mean of outcome	0.078	0.245	0.482	0.478	0.446	0.398	0.382	0.373	0.384
SD of outcome	0.27	0.43	0.50	0.50	0.50	0.49	0.49	0.48	0.49
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	152368	152368	152368	152368	152368	152368	152368	152368	152368
Number of clusters	401	401	401	401	401	401	401	401	401

Note: the table above reports the DD models' main effect on outcomes p months after child birth (in columns) as specified in regression 3. The panels (from top to bottom) show the following outcomes: a dummy for employment, for part-time work (less than 20 hours per week), for full-time work (20 hours per week or more), monthly earnings and employer continuity (a dummy for working for the same employer after child birth as at conception). Standard errors are clustered at the county level (401) and reported in parentheses. Descriptive statistics are reported at the bottom of each panel. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Employment outcomes for low income mothers in West Germany

Outcome:									
Employment in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.010***	0.006	-0.002	-0.007	-0.006	-0.004	-0.005	-0.013**	-0.009
	(0.004)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mean of outcome	0.108	0.274	0.493	0.541	0.560	0.569	0.586	0.595	0.618
SD of outcome	0.31	0.45	0.50	0.50	0.50	0.50	0.49	0.49	0.49
Part-time in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.005*	0.004	-0.006	-0.007	-0.007	-0.008	-0.007	-0.007	-0.005
	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mean of outcome	0.070	0.182	0.344	0.381	0.399	0.406	0.427	0.440	0.463
SD of outcome	0.26	0.39	0.48	0.49	0.49	0.49	0.49	0.50	0.50
Full-time in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.005**	0.003	0.004	0.001	0.001	0.003	0.002	-0.005	-0.003
	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Mean of outcome	0.038	0.092	0.147	0.158	0.160	0.162	0.157	0.153	0.153
SD of outcome	0.19	0.29	0.35	0.36	0.37	0.37	0.36	0.36	0.36
Earnings in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.068***	0.037	-0.016	-0.042	-0.035	-0.014	-0.013	-0.069*	-0.064
	(0.025)	(0.040)	(0.044)	(0.046)	(0.043)	(0.042)	(0.042)	(0.040)	(0.044)
Mean of outcome	0.722	1.845	3.351	3.724	3.890	3.995	4.118	4.198	4.354
SD of outcome	2.08	3.01	3.41	3.44	3.45	3.47	3.46	3.46	3.42
Employer continuity in month	6	12	18	24	30	36	42	48	54
treatMonths=1 × treatYear=1	0.006*	0.005	-0.003	-0.005	-0.004	-0.005	-0.008	-0.009*	-0.008
	(0.003)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Mean of outcome	0.070	0.170	0.283	0.287	0.274	0.260	0.244	0.228	0.222
SD of outcome	0.26	0.38	0.45	0.45	0.45	0.44	0.43	0.42	0.42
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	152368	152368	152368	152368	152368	152368	152368	152368	152368
Number of clusters	401	401	401	401	401	401	401	401	401

Note: the table above reports the DD models' main effect on outcomes p months after child birth (in columns) as specified in regression 3. The panels (from top to bottom) show the following outcomes: a dummy for employment, for part-time work (less than 20 hours per week), for full-time work (20 hours per week or more), monthly earnings and employer continuity (a dummy for working for the same employer after child birth as at conception). Standard errors are clustered at the county level (401) and reported in parentheses. Descriptive statistics are reported at the bottom of each panel. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.