# Parental Leave, Worker Substitutability, and Firms' Employment

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## Abstract

Motherhood and parental leave are frequent causes of worker absences and employment interruptions, yet we know little about their effects on firms. Based on linked employer-employee data from Germany, we examine how more generous leave benefits affect firm-level employment and hiring decisions. Focusing on small- and medium-sized firms, we show that more generous benefits reduce firm-level employment in the short term, which is driven by firms with few internal substitutes for the absent mother. However, firms do not respond to longer expected absences by hiring fewer young women, even when few internal substitutes available. To rationalise the findings, we show that replacement hiring occurs largely before the expected absence and that firms hire more external replacements when fewer internal substitutes are available. These findings indicate that extended leave does not harm firms when these can plan for the longer worker absences.

*Keywords:* Parental leave, worker absences, worker substitutability
*JEL:* J16, J18, J24

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# 1. Introduction

Motherhood and parental leave are frequent causes of worker absences and employment interruptions. The duration of these absences varies across countries, and designing parental leave regulations is controversial due to the inherent trade-off policymakers face: Longer and more generous parental leave schemes help parents reconcile work and family life after childbirth, but, at the same time, entail longer employment interruptions that firms need to handle. Although this may harm firms if they cannot easily replace the absent worker, longer leave may not always be negative for firms, as it may also allow them to retain more productive workers or to find more suitable replacements more easily.

Whereas a large body of literature has examined the effects of parental leave extensions on mothers' careers and earnings (e.g., Lalive and Zweimüller, 2009, Schönberg and Ludsteck, 2014, Olivetti and Petrongolo, 2017), empirical evidence on the effects on firms is still very scarce. Ginja et al. (forthcoming) study a paid parental leave reform that extended parental leave from 12 to 15 months in Sweden in 1989.[1] They find that the reform increased mothers' leave duration and that firms with greater exposure incurred larger additional wage costs. The evaluated reform was applied retroactively, which gives a high degree of internal validity. However, because mothers had started leave several months before the reform, firms had to adjust, unexpectedly, to longer absences of mothers. Firms may thus have experienced particularly high adjustment costs that could be specific to the implementation period. Seen through the lens of List (2020), their quasi-experimental setting may not be the most "natural" one because firms typically anticipate the timing and length of leave.

The main contribution of our paper is to examine how longer parental leave absences affect firms' employment when these can anticipate and plan for prolonged worker absences. Using administrative linked employer-employee data from Germany, we examine a substantial parental leave extension which is ideal to clearly identify causal effects: it was announced late enough to rule out selection effects but before mothers went on leave.

[1] In a similar vein, the unpublished manuscript by Gallen (2019) studies a parental leave extension from 8 to 10 months in Denmark in 2002, also finding negative effect on firms. Beyond that, Schmutte and Skira (2022) provide descriptive analyses for Brazil on the link between parental leave absences and firms' employment, hiring, and separations, in a regime offering four months of paid maternity leave around childbirth. Focusing on the quality of firms' output, Friedrich and Hackmann (2021) study the effects of extended parental leave of nurses in Denmark and find detrimental effects on health care services and patients' health.

Firms were thus able to anticipate and account for longer absences in their initial replacement decision before mothers left the workplace. Furthermore, we provide the first study outside the Scandinavian countries, with the important difference that gender norms are more traditional and gender gaps are larger in our setting.

We first study the effects of the German 2007 parental leave reform on the employment of firms as a proxy for their performance in imperfect labour markets (Manning, 2011, Dustmann et al., 2022). The reform affected parents of all children born on or after January 1, 2007. Our empirical estimation strategy employs a dynamic difference-in-differences design, which compares outcomes (i) between births occurring in January to June and July to December, (ii) between births occurring in July 2005 to June 2006 and July 2006 to June 2007, (iii) dynamically at several points in time before and after childbirth. As the debate about adverse effects of parental leave on firms is centred around small and medium-sized firms (Rossin-Slater, 2018), we focus on firms with up to 50 employees which make up 96.9 percent of all enterprises and employ about 40 percent of all workers in Germany (Destatis, 2018).

The reform delayed medium- and high-earning mothers' return to their pre-birth firms substantially, with no medium- to long-term impact on their employment at their pre-birth firm. We find small negative effects on firms' employment and total wage bill during the extended parental leave period, but not afterwards. The short-term gap in firms' employment is driven by firms with few internal substitutes, i.e., workers in the same firm, location, and occupation, for the mother taking leave. In contrast, we find no differences by the availability of external substitutes, i.e., the geographical density of workers in the same occupation in the local labour market. Our findings indicate that the effects of anticipated, extended leave on firm performance are negligible in the long-run despite negative short-term effects.

Given the negative effects on employment in the short-term, we next examine whether the reform affected firms' hiring decisions. If longer parental leave absences impose substantial costs on firms, they might respond by hiring fewer younger women to keep absences low. To identify such effects empirically, we contrast hiring in high-wage occupations, which received the largest incentives for extending birth-related absences, to hiring in low-wage occupations, which were much less affected by the reform. Leveraging this feature with a difference-in-differences approach, we find no evidence that firms are less likely to hire childless women of childbearing age after the reform, despite their higher propensity for longer parental leave absences. Even when we differentiate by the availability of internal substitutes, we do not observe that firms hire fewer younger women to keep absences low. We conclude that firms' additional costs of extended, but anticipated, parental leave absences are sufficiently small in our setting to not affect firms' hiring and long-term outcomes.

To better understand how firms handle absences, we explore firms' adjustment mechanisms when mothers go on leave. Using the high-frequency employment data, we document a pronounced hiring peak in the six months prior to childbirth, corresponding to 0.28 additional workers per birth, but no adjustments in separations. As hiring costs for skilled workers in Germany amount to roughly two months' wages (Muehlemann and Pfeifer, 2016), these excess hirings imply that firms incur some additional costs from births in their workforce. We further show that replacement hiring is most pronounced when few internal substitutes are available for the mother-on-leave. In contrast, replacement hiring does not differ by the availability of external substitutes. Our results provide the new insight that firms adjust to expected worker absences largely before childbirth, when mothers are still at the firm.

Finally, we explore two major reasons for the diverging findings between our study and Ginja et al. (forthcoming). To explore the role of treatment intensity, we exploit the feature that mothers in East Germany work more hours after childbirth than mothers in West Germany. We find similar results for both regions indicating that differences in the treatment intensity do not fully explain the diverging findings. Our findings are consistent with Brenøe et al. (2021) who examine the joint effect of childbirth and subsequent parental leave on firms in Denmark in a non-reform setting where firms can also anticipate the length of leave. They find only negligible costs of births and parental leave. We therefore argue that the retroactive implementation of the reform in the setting of Ginja et al. (forthcoming) explains the diverging findings.

Our paper also helps better understand how firms handle worker absences more generally. The previous literature on worker absences and substitutability mainly focuses on sickness absences (e.g., Hensvik and Rosenqvist, 2019) and worker deaths (e.g., Jäger and Heining, 2022).[2] Specifically, Hensvik and Rosenqvist (2019) show that firms keep

[2]A related strand of literature examines how the death of key figures within firms, such as CEOs,

sickness absences low for positions where workers are harder to replace, and Jäger and Heining (2022) document that firms react to unexpected worker deaths partially by retaining a larger share of their incumbent workers and partially by hiring. Yet, birth-related absences differ fundamentally from sickness absences and worker deaths: First, they are typically longer than sickness absences, but not permanent. Second, firms can anticipate birth-related absences, allowing them to plan and react early. Third, mothers often reduce their working hours when returning from parental leave. Our paper adds to this literature by showing that firms react in the months leading up to the temporary and anticipated absence, mainly with external hiring if few internal substitutes are available.

We also contribute to the scarce literature on unintended consequences of family policies for women's careers (Blau and Kahn, 2017).[3] Theoretically, generous parental leave policies can contribute to gender gaps and glass ceilings in the labour market when they are costly for firms. Cross-country comparisons show that more generous parental leave policies are associated with lower relative wages for women (Ruhm, 1998) and a lower share of women in high-level positions (Blau and Kahn, 2013). Supporting a causal interpretation of such differences with quasi-experimental evidence, Puhani and Sonderhof (2011) show that longer parental leave reduced employer-provided training for young women in Germany, and Thomas (2020) documents that mandated maternity leave benefits reduced women's promotions in the US. We add to this literature by examining whether extended parental leave reduces firms' hiring of younger women. The precise zero-effects, together with the lack of long-term effects on mothers' and firms' employment, draw an optimistic picture and suggest that parental leave of moderate length is not a major source for large gender gaps and child penalties in the labour market.

## 2. Institutional Environment

This section describes the key policy instruments that support pregnant women and mothers in the German labour market and which are also relevant for their employers: paid maternity leave, job-protected parental leave, and parental leave benefits.

superstar scientists, or inventors, affects the productivity and earnings of their co-workers (Azoulay et al., 2010, Jaravel et al., 2018, Bennedsen et al., 2020).

[3]Few previous studies examine the effects of mandated health insurance benefits (Gruber, 1994), the right to work part-time (Fernández-Kranz and Rodríguez-Planas, 2021), a combination of working-hours restrictions and maternity benefits (Zveglich and Rodgers, 2003), as well as mandated employer-provided child care (Prada et al., 2015).

*Paid Maternity Leave.* All expecting mothers are entitled to paid maternity leave which lasts from six weeks before expected delivery to eight weeks after childbirth. Mothers receive a full replacement of net earnings during this period and they must not work after childbirth. The statutory health insurance companies pay for the earnings replacements, so that firms do not incur any direct costs (Jessen et al., 2019).

*Job-Protected Parental Leave.* After the expiry of maternity leave, parents can claim job-protected parental leave (*Elternzeit*) from their employer, which allows them to return to their previous position within 36 months after childbirth. To claim job-protected parental leave, mothers must notify their employer at the latest one week after childbirth. The period for which parental leave is claimed is then binding. While on job-protected leave, parents are allowed to work part-time.

*Parental Leave Benefits.* Parental leave benefits are an important determinant of the length of parental leave (see, e.g., Schönberg and Ludsteck, 2014). In Germany, parental leave benefits are publicly funded and were substantially reformed in 2007 from a means-tested to an earnings-based scheme.

Prior to 2007, parents with low household income were eligible to receive benefits for up to 24 months after childbirth. Families qualified for benefits of 300 euro per month (about 370 USD in 2006, around eleven percent of average pre-birth net household income) if their annual net income was below a certain threshold, which varied with household structure, number of children, and time since giving birth. About 77 percent of parents were eligible to receive benefits for up to six months after childbirth (for details, see Huebener et al., 2019). Due to gradually lowered income thresholds for eligibility, the share of eligible parents fell to 47 percent for seven to 12 months after childbirth and to 40 percent for 12 to 24 months after childbirth.[4]

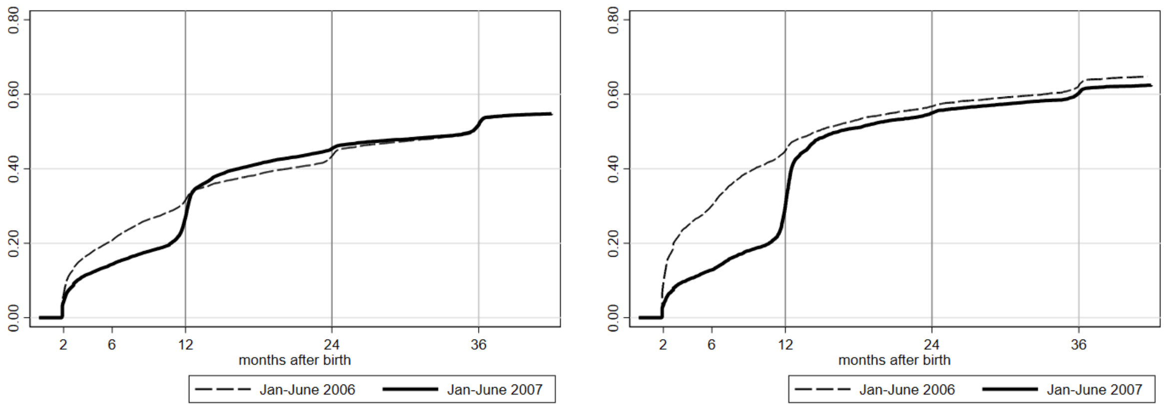
In September 2006, the German government substantially reformed the paid parental leave system, which affected parents of all children born on or after January 1, 2007. The reform replaced the previous means-tested benefits with an earnings-based benefit scheme that was paid for up to 12 months to either parent. The new benefit replaced 67 percent of the average net labour income earned in the 12 months prior to childbirth.[5]

[4]Part-time work of up to 30 hours per week was permitted during the benefit payment period. Parents eligible for benefits for up to 24 months could also choose higher benefits (450 euro) for up to 12 months. For children born in 2005 and 2006, only ten percent of all parents chose this option.

[5]Two additional months were granted for single parents or if both partners took parental leave for at

The benefit had a floor of 300 euro and was capped at 1,800 euro per month. Take-up among mothers was almost 100 percent (Destatis, 2008). The reform did not change the regulations concerning maternity leave, job-protected leave, or part-time employment rules during the job-protected period. The total minimum benefits after the reform were less generous than the maximum benefits before the reform (see Appendix Figure A.1). Specifically, individuals who did not work prior to giving birth and those with low earnings still received 300 euro per month, but now only for up to 12 months instead of 24 months.

Figure 1: Return to pre-birth firm




A: 1st earnings tercile

B: 2nd and 3rd earnings tercile

*Notes:* Figure shows the share of mothers with pre-birth earnings less than 1700 euro per month (Panel A) and 1700 euro or more per month (Panel B) who have returned to their pre-birth firm by month $t$ after childbirth. The dashed line indicates mothers giving birth between January and June 2006 (pre-reform, low means-tested parental leave benefits), the solid line indicates mothers giving birth between January and June 2007 (post-reform, earnings-related parental leave benefits for one year). Source: IEB, own calculations.

To illustrate the reform effects on maternal return-to-work, Figure 1 plots the share of mothers who had returned to their firm at different points in time, separately for mothers with low (left panel) and medium to high pre-birth earnings (right panel). We distinguish between mothers who give birth in the same calendar months—January to June—before (2006) and after (2007) the paid parental leave extension. In the policy regime with lower benefits (dashed line), mothers return gradually after the end of their maternity leave period. With extended parental leave benefits (solid line), the return within the first year is delayed, particularly for mothers with high pre-birth earnings. With the expiry

least two months. The maximum length of 14 months of paid parental leave could be split flexibly between both parents. Approximately 96 percent of parents assigned the main benefit period (>7 months) to the mother. In our observation period, 15 percent of fathers took paid parental leave, mostly for two months (Destatis, 2008).

of the parental leave benefits after about 12 months, many mothers return. This pattern is consistent with the changed economic incentives during the first year.

Our main analysis focuses on medium- and high-earning mothers for whom the reform unambiguously increased paid parental leave benefits in the first year after childbirth. In contrast, women with lower earnings experienced non-monotonic changes in incentives: as most of these mothers were entitled to higher benefits in the first twelve months, they increased their leave-taking during this period. However, they reduced their leave-taking in the second year after childbirth, when benefits expired. We exploit this differential change for low- and high-earning mothers in the analysis of firms' hiring in section 5.

## 3. Data

### *3.1. Data Source*

We use administrative data from Germany that cover the universe of firms and workers subject to social security contributions (the IAB Integrated Employment Biographies (IEB); IAB, 2019). Data are available from 1975 onward and cover about 82 percent of all workers in Germany.[6] As the information on earnings and job duration are used to calculate social security payments and benefits, they are highly reliable. Jacobebbinghaus and Seth (2007) provide a detailed description of the data.

Several features of the data render them particularly suitable for our analysis. The first advantage is that they contain the entire employment histories of all workers who have been employed at any time in the firms in our sample. Second, information on employment spells is available at the daily level as employers report the precise start and end dates of any employment spell. This level of detail is particularly important when analysing the exact timing of replacement hiring and separations relative to childbirth. Further, we can accurately assign mothers to firms at childbirth—this avoids endogeneity concerns that could arise in annual data if mothers switch employers during pregnancy. Third, we can identify single locations of multi-site firms, thus allowing us to focus on workers and their local co-workers. For simplicity, we refer to these establishments as firms

[6] Civil servants and self-employed individuals are not included in the data. This implies that information on workers in the public sector is incomplete. We exclude the public sector from our analysis. The lack of self-employment spells is not a problem for our analysis, as the main units of analysis are the firm and the workgroup. Any parental leave effects on selection into self-employment or the public sector would only affect the return to the same firm that we can fully observe.

throughout the paper. Fourth, we have detailed occupational information for workers at the 3-digit level according to the 1988 classification of occupations (with 255 unique occupations of mothers in our sample). This allows us to identify internal and external substitutes for each worker (see below for details).

In addition to the above features, the data include basic socio-demographic characteristics such as workers' gender, citizenship, education (imputed as described in Thomsen et al., 2018) and date of birth. The data also include a part-time/full-time indicator, but no further details on working hours. However, overtime pay and bonus payments are included in the earnings data and would reflect changes in working hours.

The data do not include direct information on motherhood. We follow Müller and Strauch (2017) to identify mothers and infer their expected date of delivery by exploiting the legal requirement that employers have to notify health insurance companies about the start date of the leave period.[7] We use the expected date of delivery to assign mothers to specific paid parental leave regimes.

### *3.2. Internal and External Substitutes*

To replace a mother-on-leave, firms need workers to perform her tasks. Following Cornelissen et al. (2017) and Hensvik and Rosenqvist (2019), we use 3-digit occupations to identify potential substitutes: Workers in the same occupation perform similar or identical tasks, whereas workers in different occupations perform at least somewhat different tasks. For instance, salespersons are an occupation distinct from cashiers as well as from wholesale and retail merchants, though these occupations typically have some overlap and interactions.

We define workers as *internal substitutes* if they work in the same firm, same location, and same 3-digit occupation ten months prior to childbirth. Throughout the paper, we refer to mothers' co-workers as *internal substitutes* and we use the term *workgroup* when we additionally include the mother. We define three groups of mothers based on terciles of the distribution of internal substitutes: mothers with 0-1, 2-5, and 6 or more internal substitutes.

To measure the availability of *external substitutes*, we build on the concept of labour market thickness: From a firm's perspective, a market is thick if the frequency of receiv-

[7] See Schönberg (2009) and Schönberg and Ludsteck (2014) for further details on the reliability of identifying mothers in the data.

ing suitable applicants for a given vacancy is high. As an empirical proxy for external substitutability, we calculate the density of workers in the same occupation as the mother going on leave per square kilometre in each labour market region. Our classification of labour market regions follows Kosfeld and Werner (2012) who define 141 regions in Germany based on commuting flows. We also split labour market thickness as a measure for external substitutes into terciles.

### *3.3. Outcome Variables*

Our first outcome is mothers' return to their pre-birth firm to quantify the employment gap that an increase in parental leave causes at firms. Leveraging detailed information about the employment spells, we define binary indicators for mothers working at their pre-birth firm at the monthly level, allowing us to trace out the prolonged absence of mothers in detail. As a second main outcome in the analysis of reform effects, we consider maternal earnings at their pre-birth firm. Firms could offer mothers higher wages to counteract the reform incentives for longer absences. Moreover, earnings would also capture changes in contractual working hours. As earnings are reported as a daily average over the administrative reporting period (at most one calendar year), we compute the annual earnings (including bonuses and overtime pay) of mothers and deflate earnings to a common base CPI of 2010.[8]

For firms, we focus on their employment level and total wage bill. In the absence of direct measures of firms' profits or productivity, these outcomes proxy for firm performance (Dustmann et al., 2022). Using employment levels is based on the idea that employment generates a surplus that accrues at least partly to the firm in labour markets with imperfect competition (Manning, 2011). Hence, holding other inputs and the production technology constant, lower employment implies lower profits. Similarly, the dynamic industry model with heterogeneous firms by Melitz (2003) predicts that more productive firms have a larger workforce. We measure firms' employment as the number of workers at a firm and analyse it—as for mothers—at the monthly level. We additionally examine firms' annual wage bill, which also captures changes at the intensive margin,

[8] Earnings are top-coded at the social security contribution ceiling, which affects less than one percent of mothers in our analysis sample and less than 2.5 percent of their co-workers. Top-coded earnings are assigned the coding-threshold value, i.e., we cannot capture effects above the earnings maximum. Given the low share of workers with top-coded earnings, the top-coding should not affect our results.

wage changes and bonus payments necessary to increase other workers' labour supply, and overtime pay. As firms are not responsible for providing parental leave payments to mothers, they are not reflected in firms' wage bill. Analogous to mothers' earnings, we measure the wage bill of the firm at the annual level.

To make the estimations comparable across firms of different size, we consider all firm-level outcomes relative to the baseline period. Furthermore, we censor firm outcomes above the 99th percentile to reduce the imprecision induced by outliers.

*3.4. Sample Selection and Descriptives*

In our setting, a firm is affected by the reform if a woman employed by the firm gives birth on or after January 1, 2007. As the date of birth cut-off determines the paid parental leave eligibility, this institutional rule assigns mothers and firms into a treatment group (births between January and June 2007) and a control group (births between July and December 2006). Children born before June 2007 were conceived before the parental leave reform passed the parliament in September 2006; firms could still plan for the prolonged absences in the new parental leave regime for at least three months in advance. To account for seasonality in outcomes, we further include mothers and firms with births in the previous year (July 2005 to June 2006).[9]

We impose a range of sample restrictions to construct our analysis sample from the population of firms with (first-time) births before and after the parental leave reform.[10] Appendix Figure A.2 illustrates how many observations are dropped with each sample restriction described in the following. To cleanly identify whether a firm was affected by the parental leave reform, we focus on firms that experience exactly one first-time birth between June 2005 and July 2007. With multiple first-time births in both regimes, the parental leave reform could even spill over to mothers with pre-reform births, e.g., by encouraging their earlier return if post-reform mothers' return is delayed. This would lead us to overestimate the effects of absences on firms. The one-birth restriction reduces

[9]Such seasonality could occur, for example, if women's return to the labour market depends on children's start of day care (Collischon et al., 2022).

[10]Our analysis focuses on first-time mothers. As they are more strongly attached to the labour market, we would expect their effects to be more pronounced compared to mothers with higher-order births, who often work in part-time positions after first childbirth. Moreover, higher-order births can only be identified in the data if the mother returns to work between two births. Thus, including mothers with higher-order births could yield a selective sample with respect to birth-spacing and mothers' labour force attachment.

the sample of firms by 26.2% relative to the number of firms with at least one birth in this period.

We only consider *private* firms and drop establishments that are part of the government, military, churches, and other non-profit establishments (-21.6%), as their substitution and wage setting processes substantially differ from private sector firms (Gregory and Borland, 1999, Oberfichtner and Schnabel, 2019). We focus on firms with up to 50 employees before the pregnancy occurs in the firm (-8%).

We keep only mothers with gross monthly earnings of at least 1768 euro before giving birth (-26.5%). The paid parental leave reform unambiguously increased non-labour income for these mothers during the first year after giving birth, thus monotonically increasing their financial incentives for longer absences from work (see section 2). We focus on mothers who have been at their firm for at least ten months prior to giving birth (-1.1%). This restriction avoids endogenous selection into firms and occupations during pregnancy, but as job switches are rare for expecting women, this restriction excludes only few observations. In a final step, we drop firms that experience another first-time birth within a symmetric four year window around the birth (-3.8%), see Appendix Figure A.3. We impose this symmetric four year window on all firms in our analysis sample—affecting treatment and control firms identically—and place no further conditions on higher order births. This restriction allows us to assign the treatment status of firms unambiguously and to trace dynamic effects independent of pre-reform births.

These steps yield an analysis sample containing 25,993 mothers and firms. To ensure that firms can reliably anticipate the applicable parental leave regime and to avoid misassignment of births around the cut-off, we exclude births expected to occur two weeks before and after January 1st from the analysis. Our final analysis sample contains 23,617 mothers and firms.

We report the descriptive statistics of pre-birth characteristics for our main sample of mothers and firms in column (1) of Table 1. To assess how our sample selection criteria affect the composition of our sample, the second column of Table 1 shows the characteristics of all excluded first-time mothers who gave birth in the same two-year sample period. Mothers in our sample are slightly older at birth (30 vs. 28.6 years), are more likely to have obtained higher education (39 percent vs. 31 percent), have higher monthly pre-birth earnings (2680 euro vs. 2140 euro), higher firm tenure (4.7 years vs.

3.8 years), and are more likely to work full-time pre-birth (94 percent vs. 82 percent). Consistent with the above differences, mothers in our analysis sample are more strongly attached to the labour market, as reflected by the slightly higher shares of mothers who return to the labour market within one and three years after childbirth.

Table 1: Comparison of mothers and firms in analysis sample with excluded observations

| | Analysis sample (1) | Dropped observations (2) |
|---|---|---|
| *Panel A: Mother's pre-birth characteristics* | | |
| Age in years at childbirth | 29.96 | 28.62 |
| High education | 0.39 | 0.31 |
| Monthly wage, 10 months pre-birth (1,000 euros) | 2.68 | 2.14 |
| At same firm, 10 months pre-birth | 1.00 | 0.88 |
| Tenure at pre-birth firm in years at childbirth | 4.65 | 3.80 |
| Full-time employed, pre-birth | 0.94 | 0.82 |
| German citizenship | 0.96 | 0.90 |
| Return to employment within 1 year | 0.48 | 0.42 |
| Return to employment within 3 years | 0.79 | 0.76 |
| Return to pre-birth firm within 1 year | 0.41 | 0.34 |
| Return to pre-birth firm within 3 years | 0.62 | 0.58 |
| N Mothers | 23,617 | 197,994 |
| *Panel B: Firm's pre-birth characteristics* | | |
| Firm size | 14.53 | 89.98 |
| Share of female employees | 0.61 | 0.62 |
| Average age of full-time employees | 38.57 | 37.30 |
| Median monthly wage of full-time employees (1,000 euros) | 2.56 | 2.17 |
| Location in West Germany | 0.90 | 0.82 |
| Agriculture, fishing and mining | 0.01 | 0.02 |
| Manufacturing | 0.12 | 0.17 |
| Construction | 0.04 | 0.02 |
| Wholesale and retail | 0.33 | 0.29 |
| Hotels and restaurants | 0.02 | 0.09 |
| Transport, storage, communication | 0.05 | 0.04 |
| Financial intermediation | 0.07 | 0.04 |
| Real estate, renting and business activities | 0.30 | 0.20 |
| N Firms | 23,617 | 109,591 |

*Notes:* Table shows mean values of individual mother characteristics and their pre-birth firm characteristics. Column (1) contains the analysis sample, column (2) consist of all first-time mothers (and their firms) in the analysis period (July 2005 - June 2007) identified in the data that were excluded. The sample restrictions leading to the exclusion are; employed at pre-birth firm ten months before birth, monthly earnings $\geq$ 1768 euro, one first-time birth in firm in sample period, no public sector and no firms with more than 50 employees pre-birth. We exclude the public sector. Table uses information based on June 30 2006, from the Establishment History Panel BHP (version BHP 7514 v1, described in Schmucker et al., 2016) to obtain comparable numbers for firms included and excluded from our sample. Source: IEB and BHP, own calculations.

Panel B of Table 1 shows that firms included in our sample are substantially smaller compared to excluded firms (on average, 14.5 vs. 90 employees in June 2006 with medians of 11 vs. 23 employees). Sample firms have slightly older employees (38.6 vs. 37.3 years), pay higher median gross wages (2,564 euro vs. 2,174 euro), and are more likely to come from West Germany (90 percent vs. 82 percent). Importantly, the share of female employees is almost identical between dropped and non-dropped firms (61 percent vs. 62 percent), which implies that our sampling restrictions do not exclude firms with a different gender composition in the workforce. With respect to the industry structure, we observe some minor shifts. In particular, firms in our sample are less likely to come from manufacturing or hospitality, but more likely to come from other services.

One potential concern that emerges from selecting firms with only one first-time birth during a four-year period is that potential fertility effects of the reform may cause endogenous sample selection bias. For example, if women were more likely to give births after the reform (in the medium-run), we would be more likely to exclude firms with more women of childbearing age. We evaluate this point empirically by performing covariate balancing tests within a difference-in-differences framework. Specifically, we estimate the following regression model:

$$x_i = \beta_0 + \beta_1 reform_i \times spring_i + \beta_2 reform_i + \beta_3 spring_i + \epsilon_i \tag{1}$$

where $x_i$ represents pre-birth characteristics of mother or firm $i$, $reform_i$ is a binary indicator variable equal to one if a birth occurs between July 2006 and June 2007, and $spring_i$ is a binary indicator variable equal to one if a birth occurs between January and June. Thus, the interaction term $\beta_1$ identifies potential covariates imbalances for mothers/firms with births under the new parental leave regime.

Columns (2) to (5) of Table 2 provide the means for each of the four groups, and column (6) reports the $\beta_1$ coefficient estimates from eq. (1). Overall, the balancing checks alleviate concerns about endogenous sample selection as we find no evidence for any systematic differences between treatment and control firms. Despite two individually significant differences in industry sectors, the joint F-test does not reveal statistically significant differences between the groups.

## 4. Effects of Extending Parental Leave Benefits on Mothers and Firms

### *4.1. Empirical Strategy*

To estimate the effects of the 2007 parental leave reform on mothers and firms, we implement a dynamic differences-in-differences design. We use the same estimation strategy

Table 2: Summary statistics and balancing

| | Sample window by birth cohort | | | | | |
|---|---|---|---|---|---|---|
| | All | Jul-Dec 05 | Jan-Jun 06 | Jul-Dec 06 | Jan-Jun 07 | DD coef. |
| | Mean | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Pre-birth characteristics: mother* | | | | | | |
| Age in years at childbirth | 29.964 | 29.788 | 30.135 | 29.850 | 30.114 | -0.083 |
| | | | | | | (0.104) |
| German citizenship | 0.958 | 0.960 | 0.954 | 0.962 | 0.957 | 0.000 |
| | | | | | | (0.005) |
| High education | 0.390 | 0.373 | 0.390 | 0.390 | 0.410 | 0.003 |
| | | | | | | (0.013) |
| Annual earnings in year before birth (1,000 euros) | 30.550 | 31.298 | 29.953 | 31.123 | 29.695 | -0.082 |
| | | | | | | (0.269) |
| Tenure at pre-birth firm in years at childbirth | 4.650 | 4.616 | 4.582 | 4.719 | 4.685 | -0.000 |
| | | | | | | (0.099) |
| Full-time employed, pre-birth | 0.940 | 0.943 | 0.942 | 0.939 | 0.938 | 0.000 |
| | | | | | | (0.006) |
| *Pre-birth characteristics: firm* | | | | | | |
| Location in West Germany | 0.895 | 0.900 | 0.892 | 0.894 | 0.893 | 0.006 |
| | | | | | | (0.008) |
| Firm size | 14.093 | 14.243 | 14.011 | 14.077 | 14.022 | 0.177 |
| | | | | | | (0.299) |
| Workgroup size | 5.743 | 5.837 | 5.764 | 5.695 | 5.665 | 0.044 |
| | | | | | | (0.165) |
| Share of female employees | 0.630 | 0.629 | 0.628 | 0.631 | 0.633 | 0.002 |
| | | | | | | (0.007) |
| *Pre-birth industry sector* | | | | | | |
| Agriculture, fishing and mining | 0.013 | 0.013 | 0.014 | 0.012 | 0.013 | 0.000 |
| | | | | | | (0.003) |
| Manufacturing | 0.126 | 0.130 | 0.124 | 0.125 | 0.126 | 0.007 |
| | | | | | | (0.009) |
| Construction | 0.042 | 0.042 | 0.043 | 0.046 | 0.036 | -0.011** |
| | | | | | | (0.005) |
| Wholesale and retail | 0.338 | 0.336 | 0.339 | 0.341 | 0.334 | -0.009 |
| | | | | | | (0.012) |
| Hotels and restaurants | 0.020 | 0.022 | 0.019 | 0.019 | 0.020 | 0.004 |
| | | | | | | (0.004) |
| Transport, storage, communication | 0.053 | 0.050 | 0.055 | 0.054 | 0.051 | -0.008 |
| | | | | | | (0.006) |
| Financial intermediation | 0.066 | 0.060 | 0.071 | 0.065 | 0.068 | -0.008 |
| | | | | | | (0.006) |
| Real estate, renting and business activities | 0.314 | 0.318 | 0.303 | 0.309 | 0.324 | 0.029** |
| | | | | | | (0.012) |
| Joint F-test that all coefficients in column (6) equal 0: | | | | | | $p = 0.614$ |
| N Mothers/Firms | 23,617 | 6,346 | 5,665 | 5,988 | 5,618 | 23,617 |

*Notes:* Table shows pre-determined characteristics at the individual level of the mother and at her pre-birth firm measured 10 months before first-time childbirth. Mean values are presented in columns (1)-(5). The coefficients in column (6) are obtained from a difference-in-differences specification outlined in eq. (1). The $p$-value stems from a joint estimation using the routine of Oberfichtner and Tauchmann (2021). Robust standard errors in parentheses, $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$. Source: IEB, own calculations.

for mothers and firms as we observe one first-time birth per firm. For the first difference, we compare outcomes between mothers (and their firms) giving birth up to six months before and after January 1, 2007. To account for seasonal variations and time trends in outcomes, we take a second difference using mothers giving birth one year earlier, i.e., up to six months before and after January 1, 2006. Moreover, we use the dynamic evolution of outcomes relative to the baseline period right before the onset of pregnancy. This allows us to examine the estimated treatment effects over time and to directly assess any potential pre-treatment differences between treatment and control units.

We estimate the effects of the parental leave reform on monthly outcomes with the following regression model:

$$
\begin{aligned}
y_{it} = & \sum_{\substack{t=-24, \\ t \neq -10}}^{54} \gamma_t (T_t \times reform_i \times spring_i) + \sum_{\substack{t=-24, \\ t \neq -10}}^{54} \delta_t (T_t \times reform_i) + \\
& \sum_{\substack{t=-24, \\ t \neq -10}}^{54} \tau_t (T_t \times spring_i) + \sum_{\substack{t=-24, \\ t \neq -10}}^{54} \beta_t T_t + \epsilon_{it}
\end{aligned}
\tag{2}
$$

where $y$ is the outcome of mother or firm $i$ at event-time $t$; $t = 0$ corresponds to the month of birth.[11] The variable $reform_i$ takes the value of 1 if the mother gives birth between July 2006 and June 2007, and 0 otherwise. The variable $spring_i$ indicates whether a birth occurred between January and June of a year. As we omit the event-time dummy for $t = -10$, the coefficients $\gamma_t$ estimate the treatment effect in each time period $t$ relative to ten months prior to childbirth. We bin the endpoints on either side of the effect window (for details see Schmidheiny and Siegloch, 2022). We cluster the standard errors at the mother or firm level. For maternal earnings and firms total wage bill, we use annual earnings and calculate eq. (2) in calendar years and use the pre-birth year as the reference.

To summarise our effect estimates, we also report estimates for four discrete time bins. Specifically, we use the pregnancy (10 months before birth until childbirth) as the reference period and then estimate pre-pregnancy effects (24 to 11 months before birth, $p$), short-term effects covering the paid parental leave period (2 to 14 months after birth, $s$), medium-term effects covering the remaining job protection period (15 to 36 months after birth, $m$), and longer-term effects (37 to 54 months after birth, $l$). We estimate the following regression:

[11] We do not include mother or firm fixed effects in the regressions on maternal or firm outcomes, because we use a balanced panel and, thus, their inclusion does not affect our estimates.

$$
\begin{aligned}
y_{it} = \sum_{t=p,s,m,l} \gamma_t (D_t \times reform_i \times spring_i) + \sum_{t=p,s,m,l} \delta_t (D_t \times reform_i) + \\
\sum_{t=p,s,m,l} \tau_t (D_t \times spring_i) + \sum_{t=p,s,m,l} \beta_t T_t + u_{it}
\end{aligned}
\tag{3}
$$

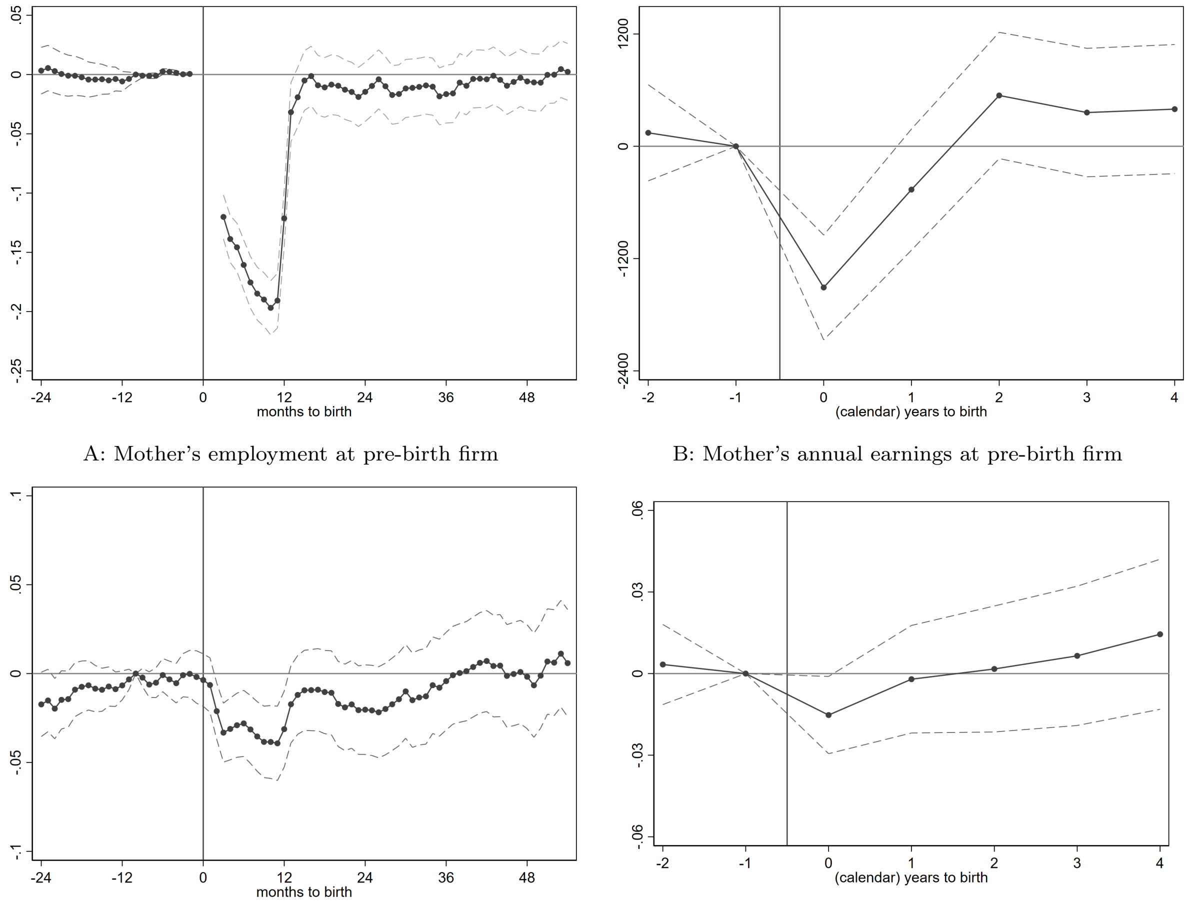
where $\gamma_t^d$ denotes the period-specific effects. For the annual earnings estimates, we use yearly data and the calendar year preceding the birth as the reference period.

*Identifying assumptions.* To interpret the $\gamma$ coefficients as the effects of the parental leave reform, our empirical strategy relies on the parallel trends assumption, i.e., the potential outcomes between treatment and control mothers and firms must follow common trends in absence of the reform. Our identification strategy could be threatened if the reform affects fertility or the selection into motherhood, or if mothers selectively time their births around the policy cut-off. Although the reform was first publicly discussed in May 2006, the final law was only passed in September 2006 (Kluve and Tamm, 2013). Raute (2019) observes first fertility responses from August 2007 onward. As our sample only contains births until June 2007, all births had been conceived prior to the passing of the reform, such that differential selection into motherhood should not bias our estimates. We substantiated this point empirically in column (6) of Table 2. As we exclude mothers giving birth two weeks before and after the reform date, mothers shifting the timing of births near the reform cut-off (as shown by Tamm, 2013, Neugart and Ohlsson, 2013, Jürges, 2017) are not an issue in our setting.

### *4.2. Effects on Mothers and Firms*

We begin our analysis by examining mothers' absences from their pre-birth firms. In Figure 2, Panel A, we observe flat pre-trends in maternal employment in the two years before childbirth, which supports the parallel trends assumption. Throughout the first year after childbirth, the parental leave reform substantially decreased mothers' probability to work for their pre-birth firms (by a maximum of 20 percentage points ten months after birth, or 57 percent). We observe no meaningful medium- or longer-term differences in the probability to work at the same firm up to 54 months after childbirth. These findings imply that the reform strongly increased mothers' absences in the first year after childbirth but had no effect on mothers' long-run absences, e.g., through increased separations as observed by Ginja et al. (forthcoming) for a parental leave extension in Sweden.[12] Our baseline specification is robust to reducing the observation window from six to three months and it performs better than alternative specifications with respect to pre-trends and seasonality in outcomes, see Appendix Figure A.4.

Figure 2: Event study of parental leave reform effects on mothers' and firms' outcomes




A: Mother's employment at pre-birth firm

B: Mother's annual earnings at pre-birth firm

C: Firm's total employment (relative to baseline)

D: Firm's wage bill (relative to baseline)

*Notes:* The figure plots event study estimates of the 2007 paid parental leave reform in Germany on maternal labour supply and firm outcomes based on eq. (2). Dashed lines indicate the 95% confidence interval; standard errors clustered at the mother / firm level. Earnings in Panels B and D are reported annually and converted to 2010 euro. *Source:* IEB, own calculations.

Panel B of Figure 2 presents effect estimates on mothers' annual earnings at their pre-birth firms. Treated mothers show no differences in earnings trajectories prior to childbirth. Consistent with the longer absence after childbirth due to the reform, earnings

[12]Several other empirical studies examine how the reform affected maternal labour market outcomes such as employment and earnings (e.g., see Kluve and Tamm, 2013, Kluve and Schmitz, 2018, Frodermann et al., 2023). Appendix Table A.1 reports comparable results for our sample of mothers where we consider employment at any firm. Return to the pre-birth firm is also analysed in Kluve and Schmitz (2018), who find that high-earning mothers are more likely to return to their pre-birth firms by two percentage points and they are more likely to hold unlimited contracts.

of treated mothers drop below those of the control group in the year of childbirth. In the following years, earnings of treated mothers are above the earnings of mothers in the control group, but the difference is small (around 400 euro, around 6% relative to the mean) and not statistically significant. We present the summary estimates based on eq. (3) for mothers in Table 3.

Table 3: Summary of event study estimates

| | Mothers | | Firms | |
|---|---|---|---|---|
| Outcome: | Employment at pre-birth firm | Earnings | Employment (relative to baseline) | Wage bill (relative to baseline) |
| | (1) | (2) | (3) | (4) |
| Pre-period | -0.001 | 144.100 | -0.008 | 0.003 |
| | (0.006) | (261.969) | (0.007) | (0.008) |
| Short term effect | -0.131*** | -1508.969*** | -0.027*** | -0.015** |
| | (0.009) | (285.881) | (0.007) | (0.007) |
| Medium term effect | -0.010 | 40.278 | -0.011 | -0.000 |
| | (0.011) | (324.003) | (0.011) | (0.010) |
| Longer term effect | -0.001 | 392.273 | 0.008 | 0.011 |
| | (0.011) | (341.554) | (0.013) | (0.013) |
| N | 23,617 | 23,617 | 23,617 | 23,617 |
| Observations | 2,408,934 | 188,936 | 2,389,986 | 187,047 |

*Notes:* Table summarises event study estimates in discrete time periods based on eq. (3). Estimates in columns (1) and (3) are based on monthly information. Pre-period is from 28 to 11 months pre-birth, the period from ten months pre-birth to one month post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in columns (2) and (4), pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to the birth year, 1-2 and 3-4 years after birth. Standard errors clustered at the mother / firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

Next, we examine how this negative, temporary labour supply shock affects firms' total employment and wage bill. In frictionless labour markets, firms would be expected to fully compensate the gap. Panel C of Figure 2 examines total employment at the firm and shows that the parental leave extension reduces employment within the first year after childbirth by up to four percent in treated firms. The treatment effect turns insignificant 12 months after childbirth and converges to zero within three years after childbirth, which is after the expiry of the job-protected period.

To account for internal adjustments like increasing the working hours of internal substitutes, we examine firms' wage bill in Panel D of Figure 2. The reform slightly reduced total labour costs in the year of childbirth by about 1.5 percent. This finding is in line with firms not being able to completely fill the gap created by mothers' longer leave. In the following years, we do not observe significant effects on the wage bill of firms. Table 3 provides the corresponding short-, medium- and longer-term estimates.

Table 4 differentiates the analysis by the availability of internal substitutes.[13] Panel A shows that the short-term employment gap of mothers is substantially larger when only few internal substitutes are available at the firm. These differences are statistically significant, as shown in Appendix Table A.2.[14] With respect to earnings, effects are slightly larger when few internal substitutes are available, but these differences are statistically not significant.

Regarding firms' outcomes, Panel B of Table 4 shows that the reform reduces relative employment at firms with at most five internal substitutes for the mother-on-leave. In workgroups with at most one substitute, employment reduces by 3.7 percent in the 14 months after childbirth, in workgroups with 2-5 substitutes, employment declines by 3 percent. The employment gap reduces over time and turns statistically insignificant in the medium- and longer-term. The total wage bill also drops significantly in the smallest workgroups in the year of birth, by 2.9 percent.[15] We do not observe any medium- to long-term effects.

To check that the differences by availability of internal substitutes are not driven by firm size, we restrict our analysis to firms with at least 11 employees, as small firms by definition cannot have large workgroups. This restriction ensures that all workgroup sizes are represented (Appendix Figure A.6). In this sample, we still find similar qualitative patters, i.e., that the reform effects are larger in small versus large workgroups (Appendix Table A.4). This supports that our findings are not explained by firm size.[16]

When we differentiate the analysis by the availability of external substitutes, i.e., the thickness of the labour market (Appendix Tables A.7 and A.8), the short-term effects are very similar across different markets. We again do not observe any medium- to long-term

[13] To support the identifying assumptions of our estimation approach, Appendix Table A.3 shows that individual and firm characteristics are balanced within those subsamples.

[14] We also examine different operationalisations of workgroup size differences in Appendix Figure A.5 and consider interactions with ln(workgroup size), 3rd vs. 1st tercile of workgroup size distribution, and a median split of workgroup size distribution. The different operationalisations lead to the same conclusions.

[15] In Appendix Table A.5 we report, analogous to mothers, estimates for firms where the treatment indicators are interacted with the workgroup size. Point estimates show in a similar direction, but we lack precision to identify statistically significant differences by workgroup size using this approach.

[16] As one may worry that these heterogeneities reflect differences in baseline levels, we report the absolute effects on employment and the wage bill in Appendix Table A.6.

Table 4: Event study estimates by internal substitutes

| | Internal substitutes | | | | | |
|---|---|---|---|---|---|---|
| | 0-1 | 2-5 | 6+ | 0-1 | 2-5 | 6+ |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Mothers* | Employed at pre-birth firm | | | Earnings at pre-birth firm | | |
| Pre-period | -0.004 | 0.003 | -0.004 | 73.041 | 563.261 | -279.173 |
| | (0.006) | (0.005) | (0.005) | (451.072) | (419.808) | (495.455) |
| Short term effect | -0.175*** | -0.159*** | -0.128*** | -1685.801*** | -1587.702*** | -1190.385** |
| | (0.016) | (0.015) | (0.017) | (476.209) | (459.680) | (563.121) |
| Medium term effect | 0.007 | -0.004 | -0.019 | 182.521 | 18.881 | -157.329 |
| | (0.017) | (0.017) | (0.020) | (542.830) | (522.663) | (632.426) |
| Longer term effect | 0.036** | -0.013 | 0.011 | 618.501 | 107.780 | 434.353 |
| | (0.017) | (0.017) | (0.020) | (572.360) | (548.670) | (670.503) |
| N Mothers | 8,573 | 8,495 | 6,549 | 8,573 | 8,495 | 6,549 |
| Observations | 874,446 | 866,490 | 667,998 | 68,584 | 67,960 | 52,392 |
| *Panel B: Firms* | Employment relative to baseline | | | Wage bill relative to baseline | | |
| Pre-period | -0.012 | -0.006 | -0.002 | -0.006 | 0.014 | 0.004 |
| | (0.014) | (0.010) | (0.010) | (0.015) | (0.012) | (0.012) |
| Short term effect | -0.037** | -0.029*** | -0.011 | -0.029** | -0.008 | -0.006 |
| | (0.014) | (0.011) | (0.011) | (0.014) | (0.011) | (0.011) |
| Medium term effect | -0.021 | -0.018 | 0.011 | -0.010 | 0.000 | 0.013 |
| | (0.021) | (0.017) | (0.018) | (0.019) | (0.016) | (0.018) |
| Longer term effect | -0.010 | 0.008 | 0.029 | -0.017 | 0.023 | 0.034 |
| | (0.025) | (0.021) | (0.023) | (0.024) | (0.021) | (0.023) |
| N Firms | 8,572 | 8,495 | 6,549 | 8,573 | 8,495 | 6,549 |
| Observations | 862,357 | 862,005 | 665,624 | 67,447 | 67,479 | 52,121 |

*Notes:* Table summarises event study estimates for the main outcomes of mothers in discrete time periods based on eq. (3). Estimates in Panel A are based on monthly information. Pre-period is from 28 to 11 months pre-birth, the period from ten months pre-birth to one month post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in Panel B, pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to the birth year, 1-2 and 3-4 years after birth. Internal substitutes are defined as the number of co-workers in the same occupation ten months prior to birth. Standard errors clustered at the mother level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

effects.

Overall, this section shows that firms, on average, do not fully close the short-run employment gap caused by longer parental leave absences. This average effect is driven by firms that have only few internal substitutes available for the mother-on-leave. Yet, extended parental leave absences have no long-term impact on firm outcomes.

## 5. Effects of Extending Parental Leave Benefits on Firms' Hiring

Given the negative effects on employment in the short-term, firms could respond to more generous parental leave by hiring fewer younger women or only hiring them into larger workgroups with more internal substitutes. Such reactions would indicate that firms statistically discriminate against younger women in response to such policies to avoid costly absences.

### *5.1. Empirical Strategy*

We aim at estimating changes in the hiring composition of firms after the parental leave reform. A challenge is to account for general changes over time, such as cohort and business cycle effects. For this reason, we leverage that the reform created stronger incentives for high-earning women to extend their parental leave absences in the first year (see section 2). In contrast, women with lower pre-birth earnings received lower benefits and smaller incentives to increase their leave absences.

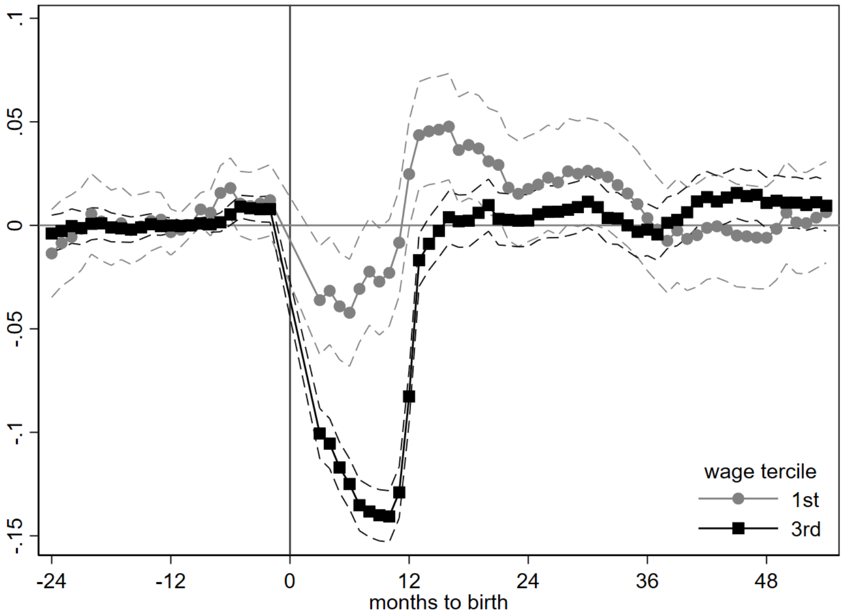
For our analysis, we use the same sample of firms as before. In total, from July 2003 to December 2009, we observe 308,261 hiring events in 54,818 workgroups across 18,471 firms.[17] We use high-wage occupations as the treatment group and low-wage occupations as the control group. To assign workgroups to one of these groups, we focus on entry wages of childless women up to age 38 by occupation before the reform. Specifically, we assign workgroups to the treatment group if entry wages in the occupation belong to the third tercile of the earnings distribution, while workgroups with entry wages in the first tercile are assigned to the control group. Figure 3 shows mothers' return-to-firm for high- and low-wage workgroups. Women in high-wage workgroups respond more strongly to the parental leave extension than women in low-wage workgroups with the maximum effect being more than three times as pronounced (14 vs. 4 pp).[18]

We are primarily interested in the effects on hiring workers with a high expected

[17] We remove 1-digit occupations with an actual birth event from the analysis sample to rule out that reform effects on the general hiring composition are mixed with replacement hiring effects. We condition on workgroups that existed half a year before the parental leave extension.

[18] As a robustness check, we assign high- and low-wage workgroups based on men's entry wages. Appendix Figures A.7 summarise the results. We reach the same conclusions using this alternative assignment.

Figure 3: Effect of the parental leave reform on return to pre-birth firm, by female entry wages in occupation




*Notes:* The figure plots event study estimates of the 2007 paid parental leave reform in Germany on mothers' probability to be employed at their pre-birth firm based on eq. (2). Wage terciles are defined at the 3-digit occupation level according to entry wages of childless women up to age 38 in the occupation between July 2003 and December 2006. *Source:* IEB, own calculations.

propensity to take longer parental leave, i.e., women of childbearing age. Younger women without children could be even more affected because they typically enter the firm with a full-time position, creating a larger gap in case of a birth. We study the hiring of childless women up to the age of 30, corresponding to the mean age at first birth in our sample of high-earning mothers. In robustness checks, we also use alternative group definitions based on different age cut-offs (Appendix Figure A.8).

We study the hiring of these groups into *low*-wage and *high*-wage workgroups *before* and *after* the parental leave extension with the following difference-in-differences (DiD) model:

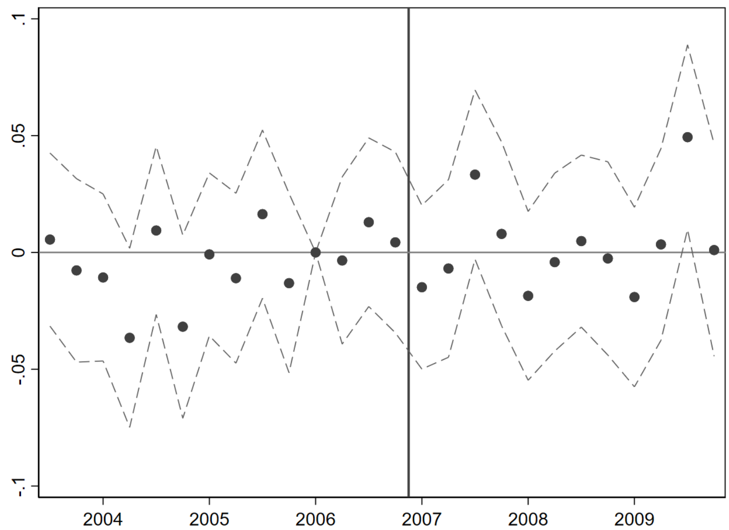
$$\begin{aligned} y_{iwt} = \alpha_w &+ \sum_{\substack{t=2003q3,\\ t\neq 2006q1}}^{2009q4} \gamma_t \times quarter_t \times high_w + \sum_{t=2003q3}^{2009q4} \tau_t \times quarter_t \\ &+ \sum_{s=q1}^{q4} \delta_s \times season_s \times high_w \\ &+ \sum_{b=-1}^{1} \beta_b \times birth_{fb} + \sum_{b=-1}^{1} \theta_b \times birth_{fb} \times high_w + \epsilon_{iwt} \end{aligned} \tag{4}$$

where $y_{iwt}$ is an indicator whether the hired worker $i$ into workgroup $w$ in quarter $t$ is a childless women up to the age of 30; $\alpha_w$ captures workgroup fixed effects. The model flexibly controls for the calendar time of new hirings including dummy variables for each quarter between July 2003 and December 2009. The coefficients of main interest are $\gamma_t$, identifying the difference in the hiring composition in high-wage workgroups relative to low-wage workgroups. The baseline quarter is 2006q1, allowing us to identify whether firms anticipate the reform in their hiring. Quarter dummies, interacted with the high-wage-group, absorb general and wage group-specific seasonal patterns. To account for potential replacement hiring into other workgroups, we include three indicators marking the quarter of birth in the firm as well as the quarters before and after ($birth_{fb}$) and allow them to differ by the wage level. As our earlier analysis weighted each firm equally, we now weigh the regressions by the inverse of the number of hirings per workgroup.

The main identification assumption is that the hiring shares in high-wage groups would have followed the same trends as in low-wage groups without the parental leave reform. To check the plausibility of this assumption, we investigate the pre-reform evolution of outcomes from our event study approach outlined in eq. (4). Our approach assumes that the parental leave extension had no effect on the decision to hire any worker.

### 5.2. *Effects on Hiring Composition*

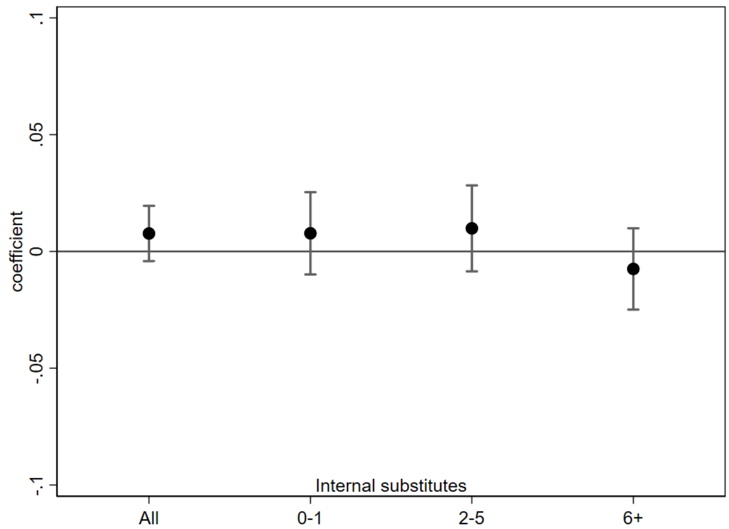
Figure 4: Event study on hiring of childless women $\leq 30$




*Notes:* Figures show the $\gamma^g$ coefficients of eq. (4), which indicate how hiring of women $\leq 30$ without children differs between high- and low-wage workgroups relative to baseline (first quarter of 2006). High- and low-wage workgroups are defined as the 3rd and 1st tercile of entry wages of childless women up to age 38 before the reform in that occupation. Dashed lines indicate the 95% confidence interval, standard errors are clustered at the workgroup level. *Source:* IEB, own calculations.

Figure 4 reports the estimates based on eq. (4). First, we note that aggregate hiring shares follow similar trends in the pre-reform period, providing support for the common trend assumption.[19] We observe no differential changes in the hiring composition of high-wage workgroups after the implementation of the reform in 2007 (nor in 2006, when the reform was publicly debated and passed). Relative to low-wage workgroups, firms are not less likely to hire childless, younger women into high-wage workgroups after the reform despite their longer absence in case of a birth.

Figure 5: Hiring effects of childless women $\leq 30$, by availability of internal substitutes




*Notes:* The figure shows the estimated effects of the parental leave extension on the hiring of women $\leq 30$ without children into high-wage workgroups separately by internal substitutes. See Figure 4 for other notes. *Source:* IEB, own calculations.

As firms with few internal substitutes experience the largest employment gap from longer parental leave absences, firms could be more reluctant to hire young women into positions with few internal substitutes. Therefore, we next consider effect heterogeneity by the availability of internal substitutes. Figure 5 reports the summary estimates, for which we substitute the variable $quarter_t$ in eq. (4) with a post-reform indicator. We find no evidence for a reduced hiring of younger or childless women after the parental leave reform into workgroups with few internal substitutes. The summary estimates are fairly precise, allowing us to rule out negative effects larger than -1pp.

[19] For an inspection of common trends in the raw data, see Appendix Figure A.9.

## 6. Firms' hiring and separations

To get a better understanding of how firms handle absences, we next explore firms' adjustment mechanisms before comparing our findings to the literature in the following section. Firms have at least two options to counteract the employment gap due to motherhood and parental leave: They can hire replacements from the external labour market or replace the absent mother internally by reducing separations. The feasibility and attractiveness of these options will depend on the availability of internal and external substitutes.

To shed light on firms' reaction, we examine their replacement hiring and separations in the same occupation as the women going on leave. For this analysis, we leverage the high frequency of our data and examine hiring at the monthly level. Panel A of Figure 6 plots the number of hirings in mothers' workgroups from 24 months before birth up to 30 months after birth occurring before (2006) and after the reform (2007).[20] We document the same pattern for both birth cohorts: Until six months prior to childbirth, firms hire around 0.1 workers per month. Then, hiring gradually increases, which coincides with the end of the first trimester when pregnancies are typically announced to employers. It shows that firms hire replacement workers from external labour markets and also allow for some transition period before workers go on leave, most likely to share job- or firm-specific knowledge.[21]

To compute additional hirings due to childbirth, we calculate excess hirings per workgroup as the difference between the sum of hirings in a workgroup $i$ throughout the six months prior to birth and hirings in the same calendar months in the previous calendar year:

$$\text{excess hiring}_i = \underbrace{\sum_{t=-6}^{0} \text{hiring}_i^t}_{\text{hiring before childbirth}} - \underbrace{\sum_{t=-18}^{-12} \text{hiring}_i^t}_{\text{hiring in previous year}} \tag{5}$$

On average, we observe 0.278 excess hirings across the two birth cohorts. Firms

[20]We use births from January to June in 2006/2007 to avoid seasonality effects.

[21]Hiring into other 1-digit occupations and into the same 1-digit, but different 3-digit occupation do not show a similarly pronounced hiring peak (see Appendix Figure A.10). The results validate our definition of substitute workers and shows that mothers-on-leave are mainly replaced with workers in the same 3-digit occupation.

replace a little more than a quarter of mothers through external hirings. Similarly, Jäger and Heining (2022) find that firms replace less than half of deceased workers externally. That the replacement rate is smaller for parental leave absences is plausible because they are, in most cases, not permanent. In the period following women's childbirth, hiring returns to the pre-birth level.

Figure 6: Hirings and separations around childbirth


A: Hirings

B: Separations

*Notes:* Dots/triangles are average number of hirings/separations per event month net of calendar month effects, the solid line is a local polynomial. Panel A shows hiring in the same workgroup (firm-occupation cell) of mothers around childbirth; mothers returning to their pre-birth workgroup are not counted as hirings. Panel B shows separations in mothers' workgroups, again with mothers excluded. Baseline hirings are the number of hirings from 18 to 12 months pre-birth, and excess hirings are defined as in eq. (5). Source: IEB, own calculations.

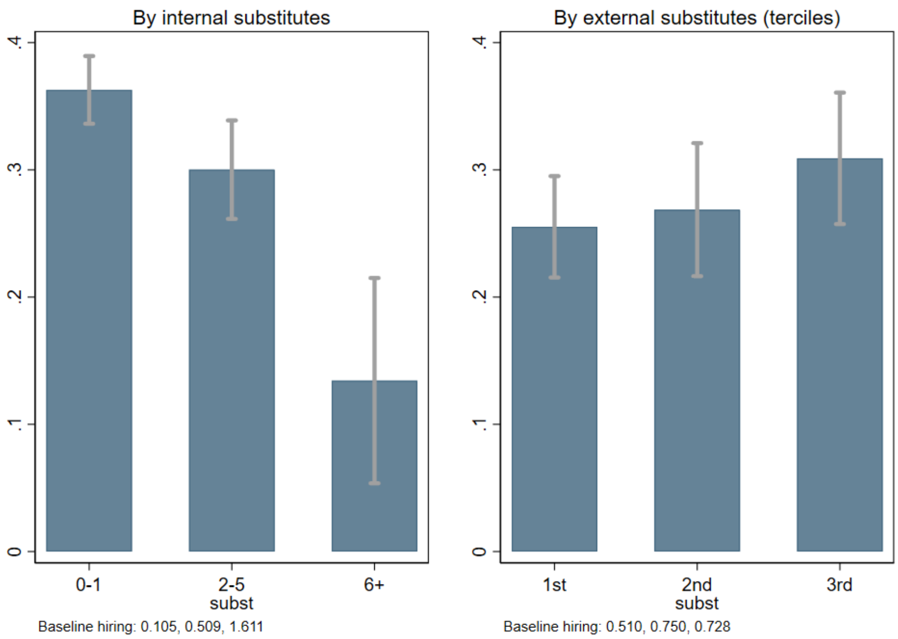
To mitigate leave-related employment gaps, firms could also retain more incumbent workers by reducing separations. In Panel B of Figure 6, we plot average separations in the workgroup over the same period. The figure shows that separations are fairly stable before and after childbirth in both parental leave regimes.[22] However, we observe no substantial differences between births events pre- and post-reform.

As we documented larger negative employment effects for firms with few internal substitutes, Figure 7 investigates how firms' replacement hiring differs by the availability of internal and external substitutes. The left panel shows that excess hiring prior to childbirth is most pronounced when few internal substitutes are available. Specifically, excess hiring in smaller workgroups amounts to around 0.363 (up to one substitute) and 0.300 (for two to five substitutes), but with six and more substitutes only 0.134 excess

[22]In contrast to excess hiring, the period during which incumbent workers are retained could be spread over the entire period of mothers' absence. Therefore, we do not calculate an analogous measure to excess hiring for separations.

hirings occur. The right panel shows that excess hiring is only slightly larger when labour markets are thicker, though here, none of these differences are statistically significant. Thus, the ability of firms to substitute one mother on leave with an external hiring is not tied to the density of the labour market. With respect to separations, we do not observe meaningful adjustments along this margin by firms (see Appendix Figure A.11).

Figure 7: Excess hiring by internal and external substitutes




*Notes:* Figure shows excess hirings as defined in eq. 5 by availability of internal and external substitutes for the mother going on leave. Internal substitutes are defined as the number of co-workers in the same occupation ten months prior to birth. External substitutes are defined as the number of employees in a commuting zone in the same occupation as the mother, per square kilometre. See Figure 6 for other notes. Source: IEB, own calculations.

## 7. Discussion

Our finding that longer parental leave absences have only small effects on firms' employment and wage bill during the extended absence period, but no effect beyond, stands in contrast to Ginja et al. (forthcoming) who find much higher adjustment costs for firms. In this section, we explore two major differences between the studies that may cause the diverging findings. First, German mothers are more likely to return into part-time employment after childbirth compared to Swedish mothers, implying that the Swedish reform was more intense for firms compared to the German reform. Second, the role of firms being able to anticipate the length of absence.

Regarding maternal employment, mothers with children aged 0-14 have an employment rate of 79% in Sweden, but only 66% in Germany. Moreover, 86% of the employed mothers work full-time in Sweden, compared to 39% in Germany (as of 2009 for both countries, see OECD, 2020). Thus, the effects of longer parental leave absences could be smaller in our setting because differences in the intensive margin of maternal employment after childbirth imply different treatment intensities for firms.

We use our estimates for the effects on wages to quantify, back-of-the-envelope, the specific full-time equivalent employment gap for firms. Our effect estimate of 1,500 euro corresponds to around five percent of mothers' pre-birth annual incomes. Assuming that the hourly wage is constant, as required by German law, this corresponds to an additional full-time equivalent employment gap of less than one month of full-time work. In Ginja et al. (forthcoming), the treatment effect corresponds to about 10 percent of pre-birth annual income, or about two months of full-time work. Thus, the full-time employment gap in our setting is smaller which could partially explain diverging findings.

Our setting allows us to more directly explore the role of maternal labour market attachment. We exploit the feature that mothers in East Germany work more hours after childbirth than mothers in West Germany (e.g., see Jessen, 2022). Thus, we can estimate reform effects separately for East and West Germany, holding constant many of the underlying, but time-constant, differences between East and West Germany. If we find similar results for both regions, it indicates that differences in maternal employment intensity after childbirth is not the main explanation for our negligible employment effects.

Appendix Table A.9 presents the reform effects on mothers and firms separately for East and West Germany. In general, the substantive patterns are similar between East and West Germany. Comparing the effect on mothers' earnings to their pre-birth earnings, we find that the treatment is 36 percent stronger for firms in East than in West Germany. Yet, also in East Germany, we find only small effects on firms and only in the short-term. Overall, these results support the conclusion that the treatment intensity is not the major explanation for smaller effects on firms in our study.

Second, can the way the reforms were implemented explain the diverging findings? The Swedish (Ginja et al., forthcoming) parental leave extensions was announced when women were already on leave and expected to return soon. Such an unexpected and retroactively applied reform might exacerbate negative effects for firms that these may not have incurred if they had been able to anticipate longer worker absences. In contrast, the German parental leave extension we examine was announced when women were still at the firm, allowing firms to plan for and anticipate the longer absence of mothers.

Section 6 has shown that firms' replacement hiring mainly occurs in the six months prior to childbirth. Thus, the timing of the exogenous shock in our setting allowed most firms to account for the longer absence in their initial replacement strategy. Similarly, Brenøe et al. (2021) find that anticipated birth-related absences at the extensive margin have little impact on firm outcomes, unless few internal substitutes are available. These arguments support the interpretation that the retroactive implementation of the reform in the setting of Ginja et al. (forthcoming) explains the diverging findings.

## 8. Conclusion

This paper examines how firms' employment reacts to longer parental leave absences by mothers when firm can anticipate the longer absences. We show that more generous parental leave benefits delay the return of mothers to their pre-birth firms and that the employment gap is more pronounced when few internal substitutes are available. However, firms do not respond to longer parental leave absences of mothers by hiring fewer young, childless women, even when few internal substitutes are available. Our findings indicate that extended leave does not harm firms when they can plan for the longer worker absences. The results thus imply that policy reforms such as introducing or extending parental leave by modest amounts do not harm firms when they can anticipate these absences.

Put into perspective, our results draw a more optimistic picture than Ginja et al. (forthcoming) on the costs of parental leave extensions for firms. Rather, our results fit those of Brenøe et al. (2021) who find that anticipated birth-related absences have little impact on firm outcomes—unless firms cannot replace the mother internally. The negative effect for these firms aligns well with our finding that firms more often make costly replacement hirings when they have few internal substitutes available. Frequent replacement hirings raise the question whether public policy should reimburse firms for their hiring and other adjustments costs associated with motherhood.

Our focus on firms with up to 50 employees has advantages for a clean identification of reform effects. But what general lessons can we draw from our study? We find zero-effects on the longer-term outcomes and hiring of smaller firms—which are at the centre of the debate about adverse effects of longer parental leave absences—and for a reform which strongly increased the expected length of absence of medium- and high-earning mothers in the first year after childbirth. As bigger firms are likely to handle employment interruptions more easily, especially those that can be anticipated, and as it should be easier for firms to substitute less qualified workers, we would expect our study to provide upper bound estimates for the employment effects of longer parental leave absences generally. The effects of anticipated parental leave extensions should hence be limited also for firms more broadly.

For policy-makers, our results add a new perspective on the effects of parental leave. Taken together, they imply that such policies help reconcile work and family life without further widening gender gaps in the labour market. Our findings fit well with other studies showing that such policies typically do not have long-term effects on mothers' careers (e.g., Kleven et al., 2022). Yet, to narrow gender gaps in the labour market, more attention should be paid to policies that support parents *after* returning to the labour market.

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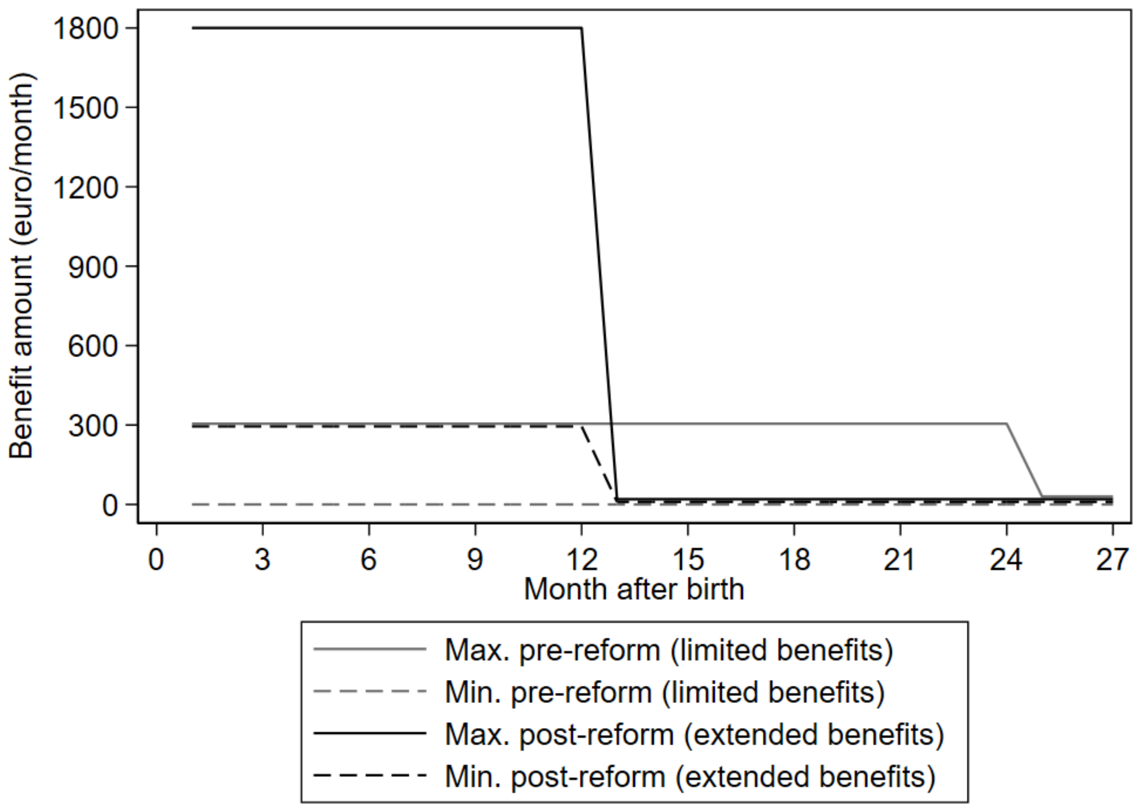
# Appendix (For Online Publication)

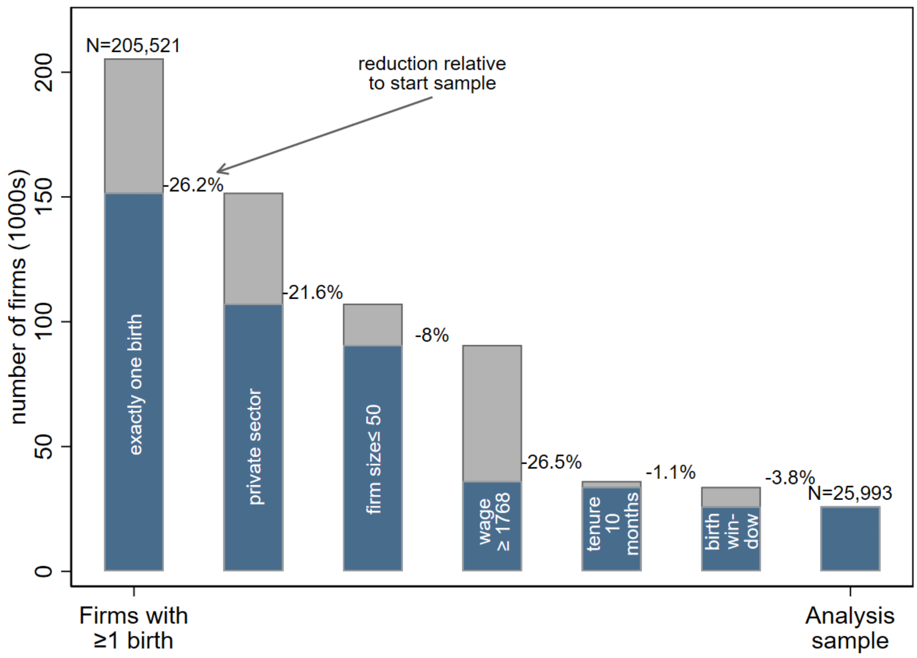
## Figures

Figure A.1: Illustration of benefits pre-reform and post-reform



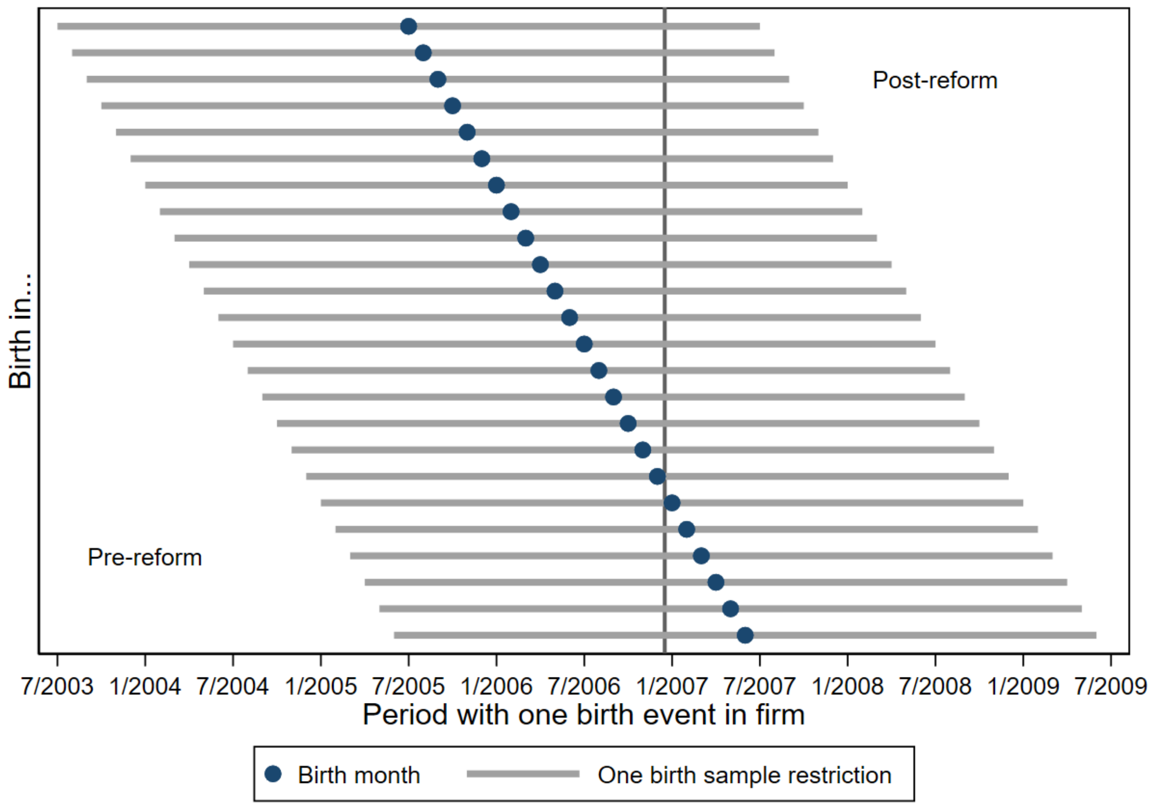

*Notes:* The figure illustrates the minimum and maximum benefit amounts before and after the 2007 parental leave reform. The two partner months introduced with the reform are omitted.

Figure A.2: Sample selection process



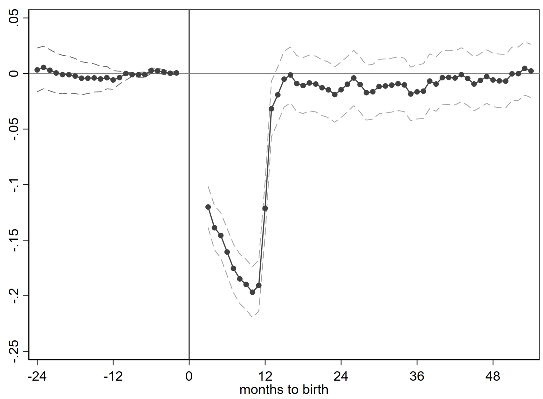

*Notes:* The figure shows the share of firms dropped in each sampling restriction step described in sub-Section 3.4. In the final analysis sample we additionally exclude births in the two weeks around January 1 of each year.

Figure A.3: Illustration of sample selection window

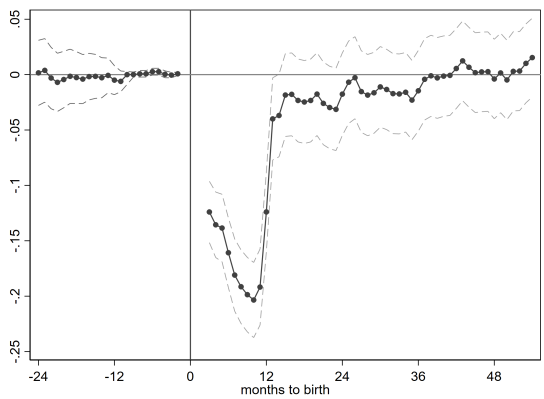



*Notes:* The figure illustrates the time window for which we require firms to experience no additional first-time births, separately by the month of the first-time birth in the firm.

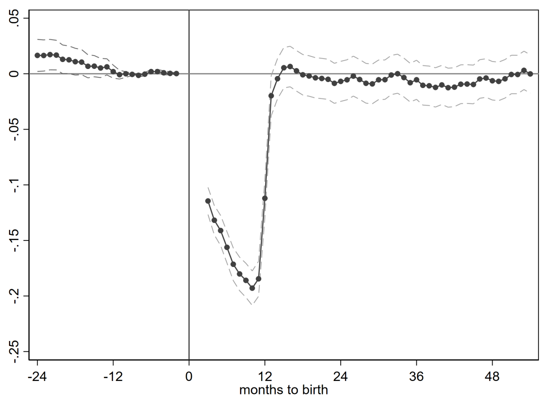
Figure A.4: Specification checks: Mother return to same firm




A: Baseline specification



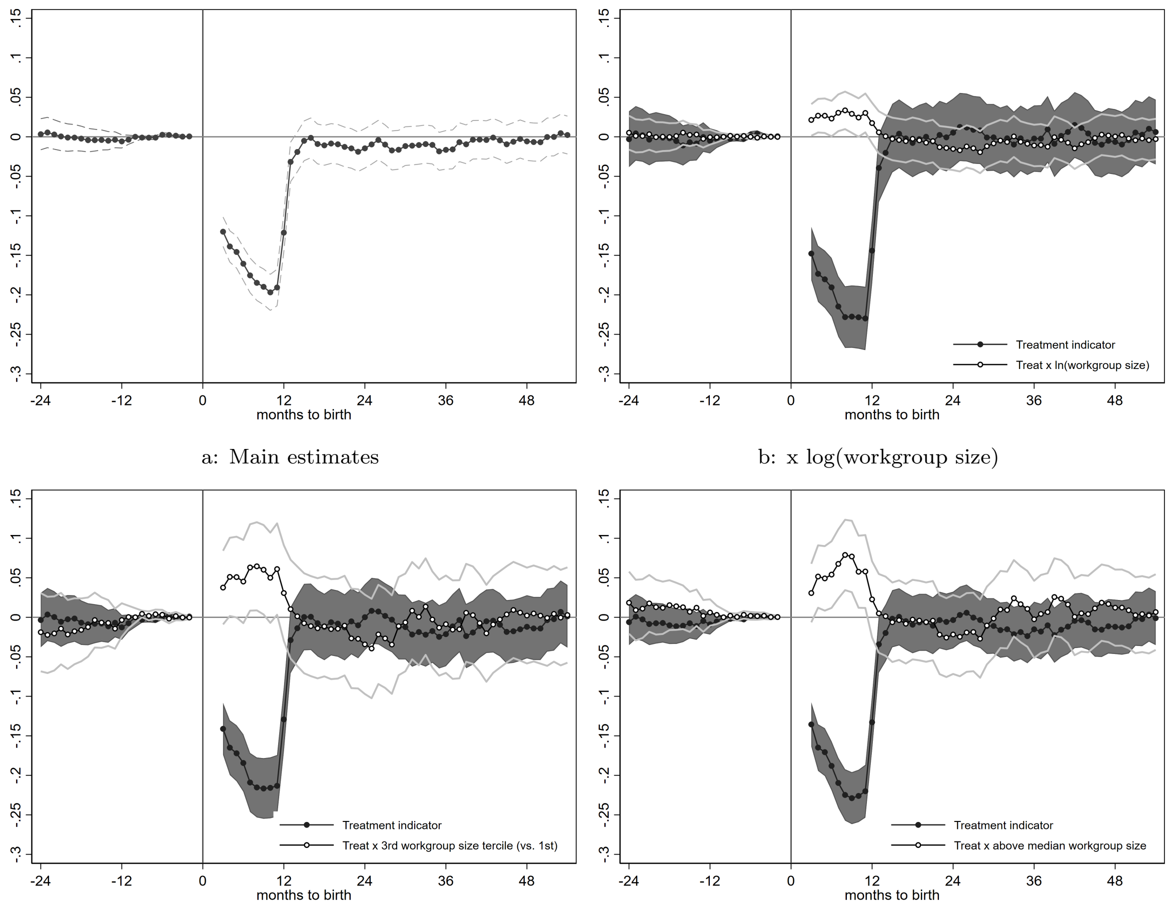

B: Baseline specification, 3-months window




C: Difference-in-differences with spring births

*Notes:* The figure plots event study estimates of the 2007 paid parental leave reform in Germany on maternal labour supply. Dashed lines indicate 95% confidence interval, standard errors clustered at the mother / firm level. Panel A reports our baseline estimates based on eq. (2) shown in Figure 2. Panel B shows coefficients using a narrower window of 3 months around the cut-off. Panel C reports the estimates for a simple difference-in-difference specification, using only spring birth (i.e., births between January 2006 to June 2006 vs. January 2007 to June 2007). *Source:* IEB, own calculations.

Figure A.5: Event study of parental leave reform effects on mothers', different workgroup size definitions




a: Main estimates

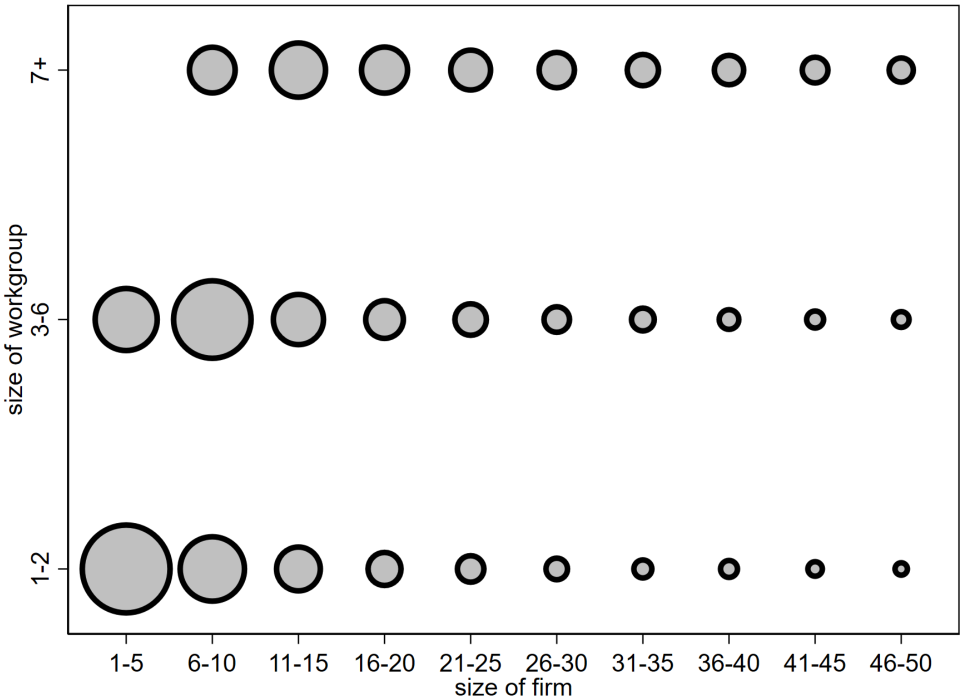
b: x log(workgroup size)

c: x 3rd vs 1st tercile

d: x above median

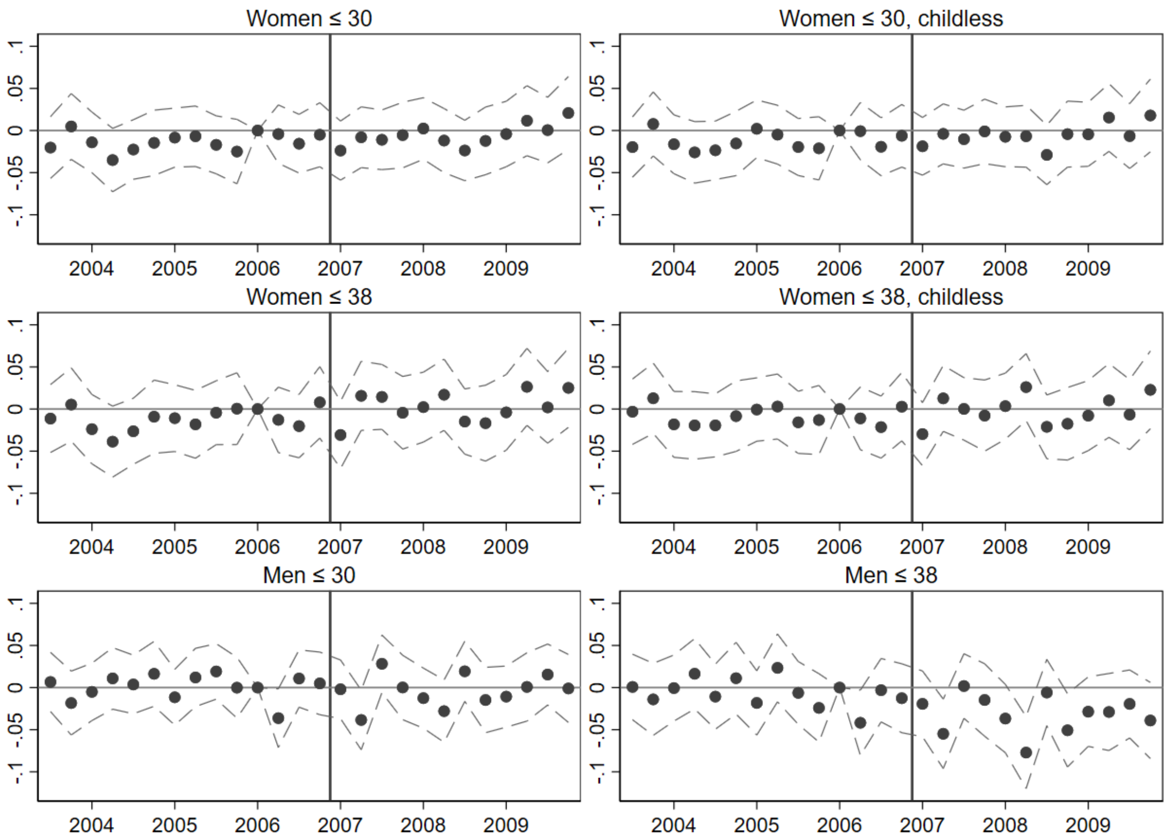
*Notes:* The figure plots event study estimates of the 2007 paid parental leave reform in Germany on maternal labour supply and firm outcomes based on eq. (2), additionally including and showing interaction terms for different definitions of the workgroup size. Dashed lines indicate 95% confidence interval, standard errors clustered at the mother level. Information on earnings in Panels C and D are reported annually; earnings in 2010 euro. *Source:* IEB, own calculations.

Figure A.6: Scatterplot of workgroup size and firm size



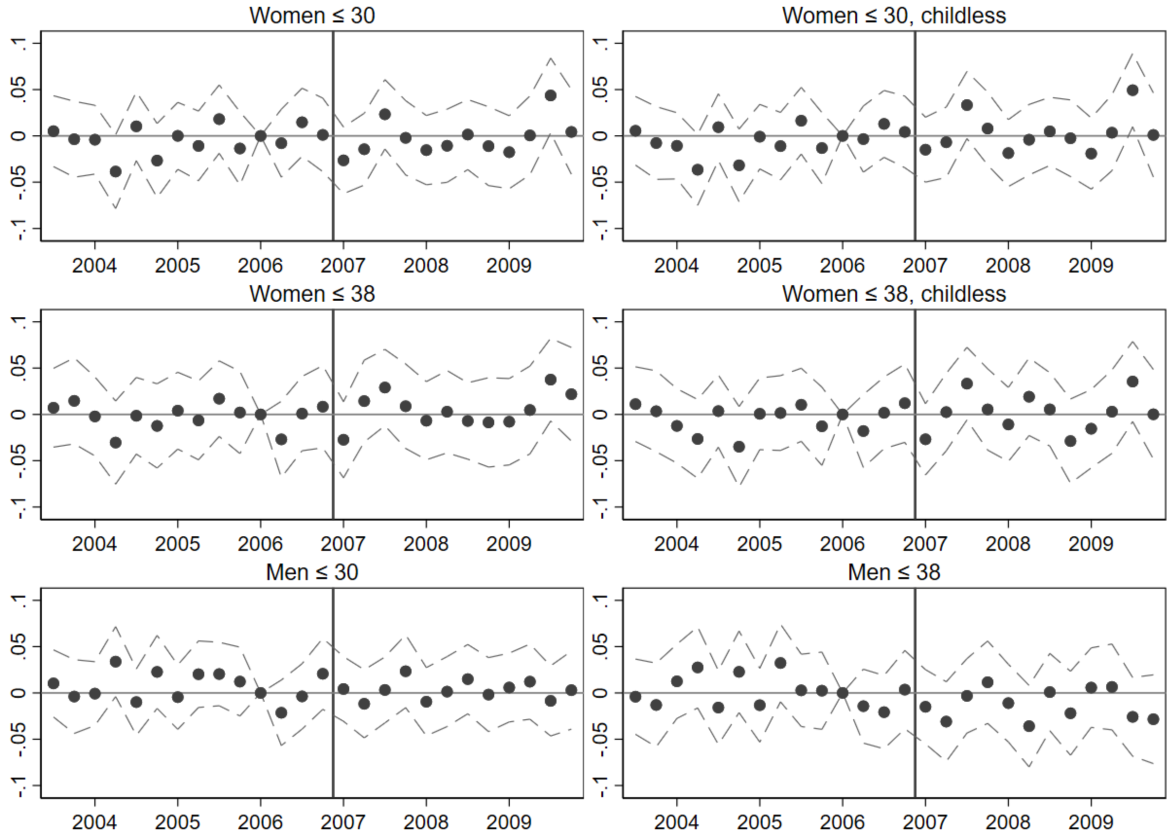

*Notes:* Figure shows a scatterplot of pre-birth workgroup size against firm size, weighing each dot by the number of observations. *Source:* IEB, own calculations.

Figure A.7: Event study on hiring composition - by entry wages of men



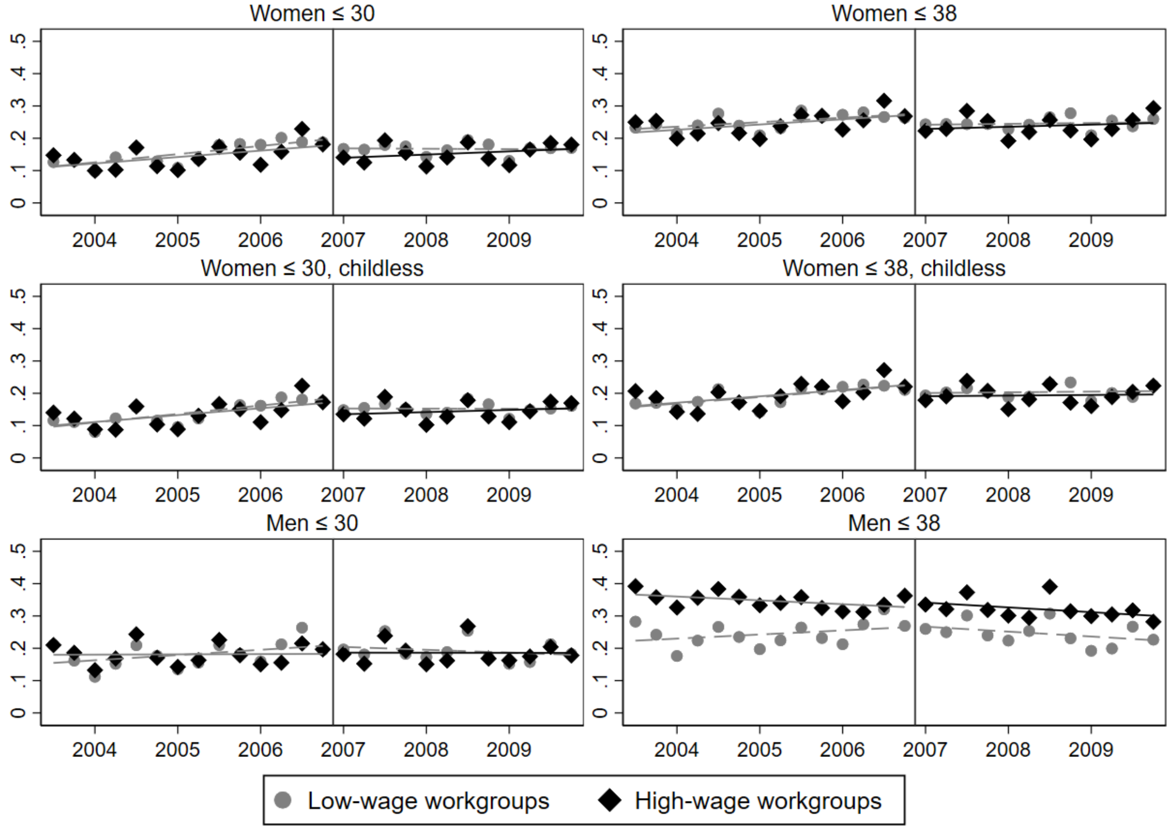

*Notes:* Figures show the $\gamma^g$ coefficients of eq. (4), which indicate how hiring of demographic groups differs between high- and low-wage workgroups relative to baseline (first quarter of 2006). High- and low-wage workgroups are defined as the 3rd and 1st tercile of entry wages of men before the reform in that occupation. Dashed lines indicate 95% confidence interval, standard errors clustered at the workgroup level. *Source:* IEB, own calculations.

Figure A.8: Event study on hiring composition



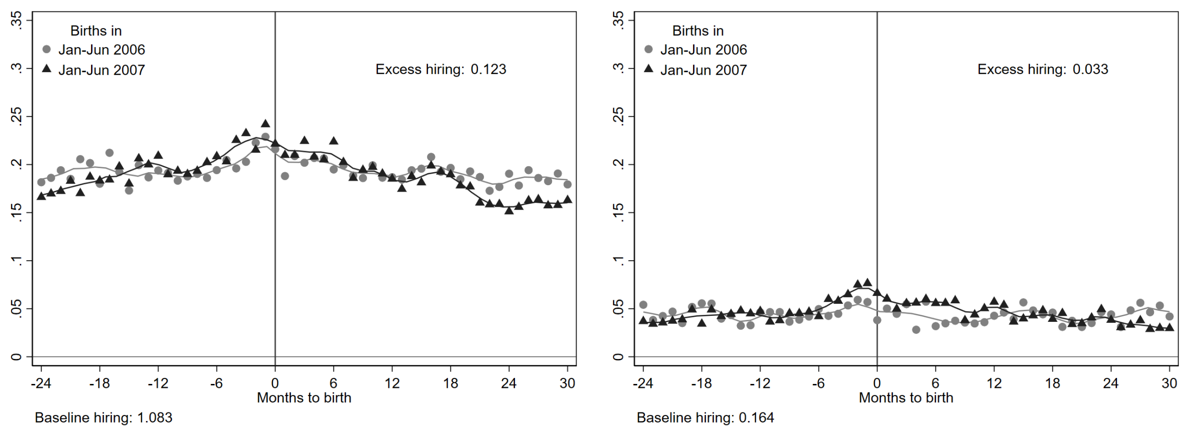

*Notes:* Figures show the $\gamma^g$ coefficients of eq. (4), which indicate how hiring of demographic groups differs between high- and low-wage workgroups relative to baseline (first quarter of 2006). High- and low-wage workgroups are defined as the 3rd and 1st tercile of entry wages of childless women up to age 38 before the reform in that occupation. Dashed lines indicate the 95% confidence interval, standard errors are clustered at the workgroup level. *Source:* IEB, own calculations.

Figure A.9: Descriptive hiring composition in low-wage and high-wage workgroups




*Notes:* The figure shows the demographic composition of all hirings at a quarterly level. High- and low wage workgroups are defined at the 3-digit occupation level according to entry wages of childless women up to age 38 in the occupation prior to 2007 (1st and 4th quartile). The sample consists of all workgroups in sample firms in which no birth event occurred. *Source:* IEB, own calculations.

Figure A.10: Hirings around childbirth in other occupations

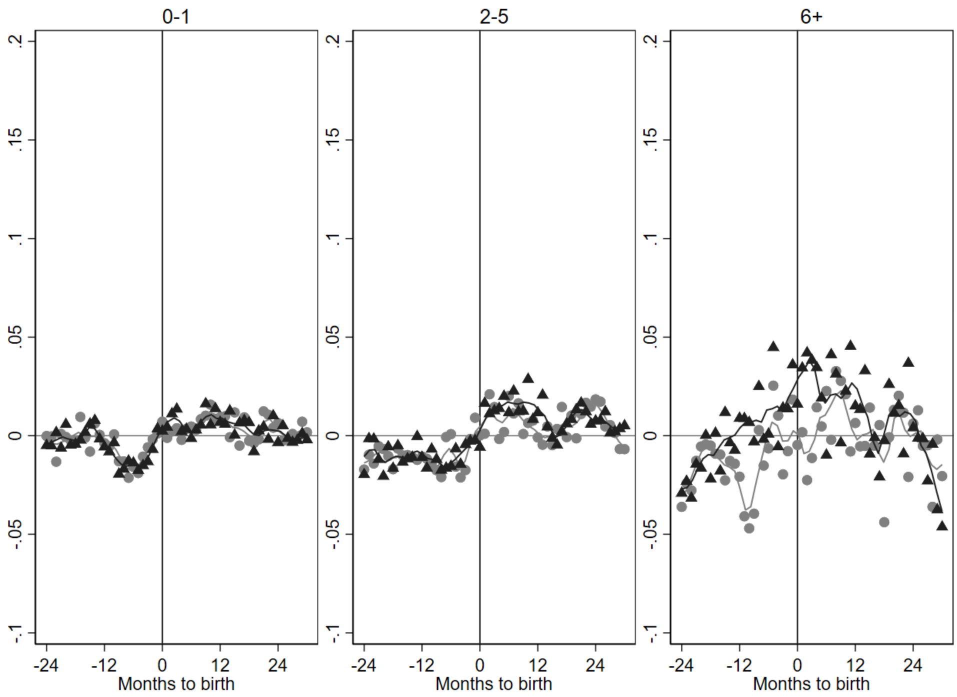



A: Different 1-digit occupation

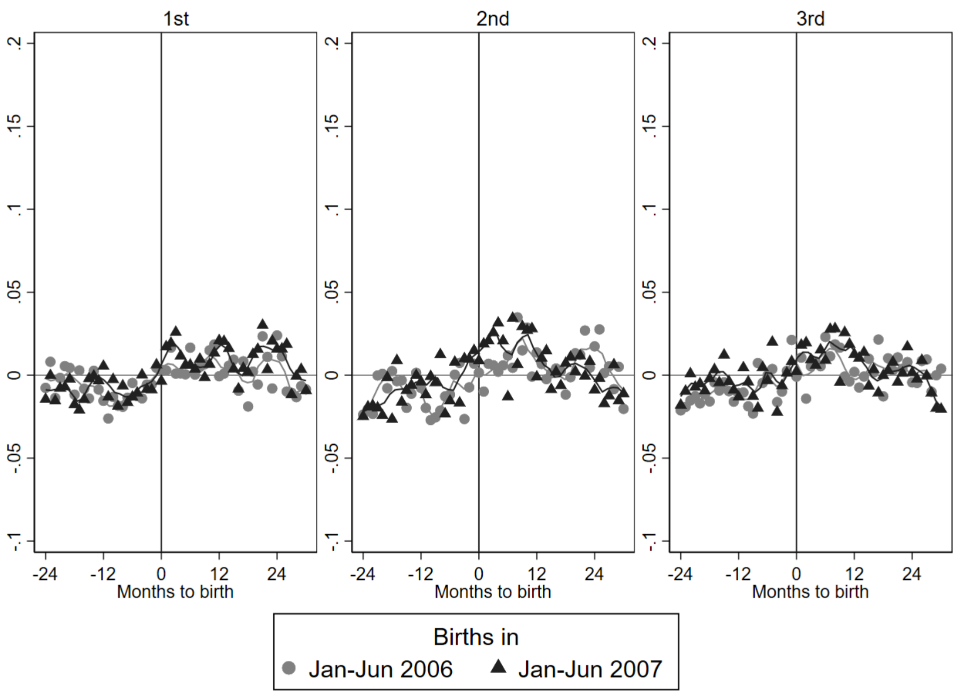
B: Same 1-digit, different 3-digit occupation

*Notes:* Figures show hiring into other occupations. Whereas Figure 6 considered hiring into the same 3-digit occupation as the mother, Panel A shows hiring into all other 1-digit occupations and Panel B shows hiring into the same 1-digit but different 3-digit occupation. See Figure 6 for other notes. Source: IEB, own calculations.

Figure A.11: Separations by availability of internal and external substitutes




A: Separations by internal substitutes




B: Separations by external substitutes (thickness terciles)

*Notes:* Figure shows separations by the availability of internal and external substitutes. Internal substitutes are defined as the number of co-workers in the same occupation ten months prior to birth. External substitutes are defined as the number of employees in a commuting zone in the same occupation as the mother, per square kilometre. The sample includes workgroups with births from January 2006 to June 2006 and January 2007 to June 2007. See Figure 6 for other notes. Source: IEB, own calculations.

# Appendix Tables

Table A.1: Summary event study estimates for mothers: other outcomes

| | | Internal substitutes | | | External substitutes (terciles) | | |
|---|---|---|---|---|---|---|---|
| | All | 0-1 | 2-5 | 6+ | 1st | 2nd | 3rd |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: Employed (any firm) | | | | | | | |
| Pre-period | -0.002 | -0.004 | 0.003 | -0.004 | -0.001 | 0.000 | -0.004 |
| | (0.003) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Short term effect | -0.156*** | -0.175*** | -0.159*** | -0.128*** | -0.144*** | -0.172*** | -0.155*** |
| | (0.009) | (0.016) | (0.015) | (0.017) | (0.016) | (0.016) | (0.016) |
| Medium term effect | -0.004 | 0.007 | -0.004 | -0.019 | -0.005 | -0.016 | 0.005 |
| | (0.010) | (0.017) | (0.017) | (0.020) | (0.018) | (0.018) | (0.019) |
| Longer term effect | 0.011 | 0.036** | -0.013 | 0.011 | 0.024 | -0.033* | 0.043** |
| | (0.010) | (0.017) | (0.017) | (0.020) | (0.018) | (0.018) | (0.018) |
| Mothers | 23,617 | 8,573 | 8,495 | 6,549 | 7,866 | 7,866 | 7,885 |
| Observations | 2,408,934 | 874,446 | 866,490 | 667,998 | 802,332 | 802,332 | 804,270 |
| Panel B: Annual earnings in calendar year (any firm) | | | | | | | |
| Pre-period | -338.601* | -553.711* | -103.707 | -334.698 | -702.981** | -240.609 | -76.594 |
| | (173.114) | (307.970) | (278.097) | (309.238) | (289.897) | (309.816) | (299.374) |
| Short term effect | -1880.666*** | -2106.795*** | -1850.639*** | -1645.491*** | -1607.612*** | -1916.752*** | -2117.175*** |
| | (262.612) | (441.988) | (418.329) | (515.458) | (423.116) | (485.382) | (452.659) |
| Medium term effect | -80.176 | 289.409 | -138.931 | -540.365 | 127.210 | -155.265 | -262.216 |
| | (314.174) | (533.258) | (502.709) | (607.724) | (521.170) | (566.708) | (540.696) |
| Longer term effect | 697.572** | 1762.186*** | -108.268 | 327.827 | 803.038 | 147.269 | 1133.266** |
| | (332.682) | (566.698) | (533.766) | (638.974) | (559.239) | (599.637) | (567.690) |
| Mothers | 23,617 | 8,573 | 8,495 | 6,549 | 7,866 | 7,866 | 7,885 |
| Observations | 188,936 | 68,584 | 67,960 | 52,392 | 62,928 | 62,928 | 63,080 |

*Notes:* Table summarises event study estimates for the main outcomes of mothers in discrete time periods based on eq. (3). Estimates in Panel A are based on monthly information. Pre-period is from 28 to 11 months pre-birth, the period from ten months pre- to one months post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in Panels B, pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to the birth year, 1-2 and 3-4 years after birth. Standard errors clustered at the mother level in parentheses. Significance levels: * $< 10\%$ ** $< 5\%$ *** $< 1\%$.
*Source:* IEB, own calculations.

Table A.2: Summary event study estimates for mothers - interaction with internal substitutes

| | Employed at pre-birth firm | | Annual earnings | |
|---|---|---|---|---|
| | Treat | Treat × ln(workgroup size) | Treat | Treat × ln(workgroup size) |
| Pre-period | -0.001 | 0.002 | -404.216 | 57.424 |
| | (0.010) | (0.003) | (318.409) | (189.9142) |
| Short term effect | -0.187*** | 0.024** | -1366.957*** | -4.196 |
| | (0.016) | (0.010) | (469.507) | (298.507) |
| Medium term effect | 0.018 | -0.017 | 1352.83** | -508.003 |
| | (0.018) | (0.011) | (579.236) | (366.688) |
| Longer term effect | 0.034* | -0.018 | 1591.017*** | -695.728* |
| | (0.018) | (0.011) | (615.998) | (388.247) |
| Clusters | 23,617 | | 23,617 | |
| N | 2,408,934 | | 188,936 | |

*Notes:* Table summarises event study estimates for the main outcomes of mothers in discrete time periods based on eq. (3), where we additionally include ln(workgroup size) and the interaction ln(workgroup size)× treat as regressors. Estimates in columns (1) and (2) are based on monthly information. Pre-period is from 28 to 11 months pre-birth, the period from ten months pre- to one months post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in columns (3) and (4), pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to the birth year, 1-2 and 3-4 years after birth. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.
*Source:* IEB, own calculations.

Table A.3: Balancing by internal and external substitutes
(DD coefficients)

| | | Internal substitutes | | | External substitutes (terciles) | | |
|---|---|---|---|---|---|---|---|
| | All | 0-1 | 2-5 | 6+ | 1st | 2nd | 3rd |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Individual characteristics* | | | | | | | |
| Age in years at childbirth | -0.083 | -0.212 | -0.081 | 0.079 | -0.096 | -0.057 | -0.088 |
| | (0.104) | (0.171) | (0.172) | (0.202) | (0.175) | (0.181) | (0.184) |
| German citizenship | 0.000 | -0.002 | -0.003 | 0.007 | -0.009 | 0.002 | 0.006 |
| | (0.005) | (0.008) | (0.009) | (0.011) | (0.007) | (0.009) | (0.010) |
| High education | 0.003 | 0.006 | -0.003 | 0.004 | 0.007 | 0.000 | -0.005 |
| | (0.013) | (0.021) | (0.021) | (0.024) | (0.022) | (0.022) | (0.021) |
| Annual earnings in year before birth (1,000 euros) | -0.082 | -0.645 | 0.388 | 0.098 | -0.245 | -0.156 | 0.144 |
| | (0.269) | (0.458) | (0.429) | (0.522) | (0.425) | (0.507) | (0.462) |
| Tenure at pre-birth firm in years | -0.000 | 0.202 | -0.205 | 0.026 | -0.034 | 0.217 | -0.194 |
| | (0.099) | (0.155) | (0.166) | (0.194) | (0.171) | (0.170) | (0.170) |
| Full-time employed | 0.000 | 0.008 | -0.021** | 0.017 | -0.012 | 0.010 | 0.003 |
| | (0.006) | (0.010) | (0.010) | (0.013) | (0.011) | (0.011) | (0.010) |
| *Pre-birth firm characteristics* | | | | | | | |
| Location in West Germany | 0.006 | -0.001 | 0.006 | 0.015 | 0.018 | -0.005 | 0.011 |
| | (0.008) | (0.014) | (0.013) | (0.015) | (0.017) | (0.013) | (0.011) |
| Firm size | 0.177 | -0.116 | 0.273 | 0.572 | 0.552 | 0.209 | -0.046 |
| | (0.299) | (0.415) | (0.441) | (0.567) | (0.463) | (0.523) | (0.550) |
| Workgroup size | 0.044 | -0.022 | 0.016 | 0.306 | -0.203 | 0.231 | 0.127 |
| | (0.165) | (0.022) | (0.047) | (0.373) | (0.260) | (0.319) | (0.271) |
| Share of female employees | 0.002 | 0.011 | -0.003 | -0.006 | -0.015 | 0.002 | 0.009 |
| | (0.007) | (0.014) | (0.012) | (0.013) | (0.012) | (0.013) | (0.013) |
| *Industry Sector* | | | | | | | |
| Agriculture, fishing and mining | 0.000 | 0.001 | 0.003 | -0.006 | 0.002 | -0.001 | -0.001 |
| | (0.003) | (0.005) | (0.005) | (0.006) | (0.006) | (0.005) | (0.004) |
| Manufacturing | 0.007 | 0.003 | 0.009 | 0.009 | 0.023 | -0.006 | 0.005 |
| | (0.009) | (0.014) | (0.015) | (0.016) | (0.016) | (0.013) | (0.016) |
| Electricity, gas, water | -0.001 | 0.001 | 0.001 | -0.006 | -0.003 | -0.003 | 0.003 |
| | (0.002) | (0.002) | (0.002) | (0.004) | (0.002) | (0.003) | (0.003) |
| Construction | -0.011** | -0.021** | -0.010 | 0.000 | -0.008 | 0.004 | -0.026** |
| | (0.005) | (0.011) | (0.008) | (0.006) | (0.008) | (0.008) | (0.011) |
| Wholesale and retail | -0.009 | 0.016 | -0.024 | -0.022 | -0.004 | -0.017 | -0.005 |
| | (0.012) | (0.020) | (0.021) | (0.023) | (0.021) | (0.021) | (0.021) |
| Hotels and restaurants | 0.004 | 0.006 | -0.002 | 0.008 | 0.007 | -0.001 | 0.004 |
| | (0.004) | (0.006) | (0.006) | (0.007) | (0.006) | (0.008) | (0.004) |
| Transport, storage, communication | -0.008 | -0.004 | -0.015 | -0.003 | -0.023** | 0.007 | -0.009 |
| | (0.006) | (0.009) | (0.009) | (0.013) | (0.011) | (0.010) | (0.009) |
| Financial intermediation | -0.008 | 0.002 | -0.010 | -0.017 | -0.011 | -0.010 | -0.007 |
| | (0.006) | (0.007) | (0.010) | (0.016) | (0.009) | (0.015) | (0.008) |
| Real estate, renting and business activities | 0.029** | 0.010 | 0.048** | 0.029 | 0.007 | 0.039* | 0.045** |
| | (0.012) | (0.020) | (0.020) | (0.022) | (0.021) | (0.021) | (0.021) |
| Joint F-test that all coefficients equal 0: | $p = 0.614$ | $p = 0.464$ | $p = 0.359$ | $p = 0.849$ | $p = 0.329$ | $p = 0.879$ | $p = 0.551$ |
| Observations | 23,617 | 8,573 | 8,495 | 6,549 | 7,866 | 7,866 | 7,885 |

*Notes:* The table shows difference-in-differences coefficients for pre-determined characteristics by size of the workgroup and terciles of labour market thickness. Column (1) corresponds to column (6) of Table 2. Robust standard errors in parentheses. * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

Table A.4: Summary event study estimates for larger firms

| | | Internal substitutes | | |
|---|---|---|---|---|
| | All | 0-1 | 2-5 | 6+ |
| | (1) | (2) | (3) | (4) |
| *Panel A: Mothers* | | | | |
| Employed at pre-birth firm | | | | |
| Pre-period | -0.002 | 0.013 | 0.010 | -0.016 |
| | (0.009) | (0.019) | (0.015) | (0.012) |
| Short term effect | -0.114*** | -0.167*** | -0.088*** | -0.106*** |
| | (0.013) | (0.029) | (0.024) | (0.018) |
| Medium term effect | -0.010 | 0.016 | 0.007 | -0.032 |
| | (0.016) | (0.035) | (0.029) | (0.023) |
| Longer term effect | 0.002 | 0.023 | 0.002 | -0.009 |
| | (0.016) | (0.035) | (0.030) | (0.023) |
| N Mothers | 11,539 | 2,546 | 3,495 | 5,498 |
| Observations | 1,176,978 | 259,692 | 356,490 | 560,796 |
| Wage sum at pre-birth firm | | | | |
| Pre-period | -552.739 | -501.211 | -636.442 | -535.419 |
| | (368.474) | (815.549) | (635.246) | (539.819) |
| Short term effect | -1536.093*** | -2343.615** | -986.860 | -1502.658** |
| | (419.993) | (918.434) | (727.309) | (618.850) |
| Medium term effect | -82.279 | 394.224 | 256.798 | -529.866 |
| | (478.408) | (1049.767) | (840.655) | (698.098) |
| Longer term effect | 551.201 | 1582.761 | 160.521 | 292.549 |
| | (506.624) | (1119.551) | (884.811) | (739.125) |
| N mothers | 11,539 | 2,546 | 3,495 | 5,498 |
| Observations | 92,312 | 20,368 | 27,960 | 43,984 |
| *Panel B: Firms* | | | | |
| Relative employment | | | | |
| Pre-period | 0.000 | -0.004 | 0.012 | -0.005 |
| | (0.008) | (0.018) | (0.015) | (0.011) |
| Short term effect | -0.017** | -0.036* | -0.030** | -0.002 |
| | (0.008) | (0.018) | (0.015) | (0.012) |
| Medium term effect | -0.010 | 0.005 | -0.060** | 0.015 |
| | (0.014) | (0.030) | (0.025) | (0.019) |
| Longer term effect | 0.024 | 0.040 | -0.021 | 0.043* |
| | (0.018) | (0.038) | (0.032) | (0.025) |
| N firms | 11,539 | 2,546 | 3,495 | 5,498 |
| Observations | 1,172,605 | 258,456 | 354,984 | 559,165 |
| Relative wage sum | | | | |
| Pre-period | 0.006 | 0.009 | 0.026 | -0.007 |
| | (0.009) | (0.020) | (0.017) | (0.013) |
| Short term effect | -0.011 | -0.017 | -0.025 | -0.000 |
| | (0.008) | (0.019) | (0.016) | (0.012) |
| Medium term effect | -0.003 | 0.008 | -0.049** | 0.020 |
| | (0.013) | (0.030) | (0.024) | (0.019) |
| Longer term effect | 0.019 | 0.014 | -0.023 | 0.045* |
| | (0.018) | (0.039) | (0.033) | (0.025) |
| N firms | 11,539 | 2,546 | 3,495 | 5,498 |
| Observations | 91,813 | 20,228 | 27,810 | 43,775 |

*Notes:* Table shows summary event study estimates for the main outcomes of mothers and firms in discrete time periods based on eq. (3) separately for larger firms (3rd tercile). See Table 3 for other notes. Standard errors clustered at the mother / firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.
*Source:* IEB, own calculations.

Table A.5: Summary event study estimates for firms - interaction with internal substitutes

| | Relative employment | | Relative wage sum | |
|---|---|---|---|---|
| | Treat | Treat × ln(workgroup size) | Treat | Treat × ln(workgroup size) |
| Pre-period | -0.0183 | 0.0084 | -0.0055 | 0.0072 |
| | (0.0139) | (0.0077) | (0.0147) | (0.0083) |
| Short term effect | -0.0380*** | 0.0086 | -0.0316** | 0.0127 |
| | (0.0144) | (0.0080) | (0.0146) | (0.0081) |
| Medium term effect | -0.0194 | 0.0063 | -0.0102 | 0.0078 |
| | (0.0210) | (0.0120) | (0.0197) | (0.0114) |
| Longer term effect | -0.0077 | 0.0116 | -0.0122 | 0.01830 |
| | (0.0256) | (0.0148) | (0.0242) | (0.0143) |
| Clusters | 23,616 | | 23,617 | |
| Observations | 2,389,986 | | 187,047 | |

*Notes:* Table summarises event study estimates for the main outcomes of firms in discrete time periods based on eq. (3), where we additionally include ln(workgroup size) and the interaction ln(workgroup size)× treat as regressors. Estimates in columns (1) and (2) are based on monthly information. Pre-period is from 28 to 11 months pre-birth, the period from ten months pre- to one months post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in columns (3) and (4), pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to the birth year, 1-2 and 3-4 years after birth. Significance levels: * $< 10\%$ ** $< 5\%$ *** $< 1\%$.
*Source:* IEB, own calculations.

Table A.6: Summary event study estimates for firms: absolute outcomes firm

| | | Internal substitutes | | | External substitutes (terciles) | | |
|---|---|---|---|---|---|---|---|
| | All | 0-1 | 2-5 | 6+ | 1st | 2nd | 3rd |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: Total employment | | | | | | | |
| Pre-period | -0.071 | -0.042 | -0.043 | -0.139 | -0.209 | -0.074 | 0.055 |
| | (0.106) | (0.140) | (0.169) | (0.256) | (0.152) | (0.189) | (0.207) |
| Short term effect | -0.357*** | -0.388** | -0.454*** | -0.199 | -0.134 | -0.671*** | -0.265 |
| | (0.109) | (0.155) | (0.160) | (0.263) | (0.164) | (0.190) | (0.207) |
| Medium term effect | -0.364* | -0.205 | -0.637** | -0.236 | 0.014 | -0.802** | -0.296 |
| | (0.187) | (0.249) | (0.282) | (0.463) | (0.267) | (0.333) | (0.365) |
| Longer term effect | 0.023 | 0.073 | -0.224 | 0.259 | 0.138 | -0.495 | 0.422 |
| | (0.249) | (0.333) | (0.379) | (0.608) | (0.358) | (0.437) | (0.486) |
| Pre mean | 14.1219 | 9.631867 | 12.67934 | 21.87082 | 9.631867 | 12.67934 | 21.87082 |
| Firms | 23,617 | 8,573 | 8,495 | 6,549 | 7,866 | 7,866 | 7,885 |
| Observations | 2,408,934 | 874,446 | 866,490 | 667,998 | 802,332 | 802,332 | 804,270 |
| Panel B: Total wage bill | | | | | | | |
| Pre-period | 1433.671 | 176.687 | 2683.424 | 1028.946 | -5602.461 | 3853.503 | 5450.278 |
| | (3966.631) | (4621.486) | (6241.846) | (10153.803) | (5013.782) | (6915.657) | (8294.177) |
| Short term effect | -4002.819 | -3174.901 | -4835.445 | -4229.812 | 2797.723 | -1.43e+04** | -549.141 |
| | (3394.798) | (4460.999) | (4821.600) | (8749.941) | (4683.139) | (5972.950) | (6793.562) |
| Medium term effect | -5410.378 | 1302.377 | -1.46e+04 | -2632.104 | 7776.096 | -2.25e+04** | -1332.720 |
| | (6065.854) | (7628.474) | (9004.641) | (15564.911) | (7949.605) | (10850.124) | (12244.304) |
| Longer term effect | 1520.114 | 6729.766 | -1.67e+04 | 17211.994 | 4150.005 | -2.07e+04 | 20966.005 |
| | (8697.970) | (10952.000) | (12670.779) | (22450.318) | (11228.496) | (15245.665) | (17928.121) |
| Pre mean | 395,570 | 262,854 | 346,508 | 632,944 | 262,854 | 346,508 | 632,944 |
| Firms | 23,617 | 8,573 | 8,495 | 6,549 | 7,866 | 7,866 | 7,885 |
| Observations | 188,936 | 68,584 | 67,960 | 52,392 | 62,928 | 62,928 | 63,080 |

*Notes:* Table summarises event study estimates for the main outcomes of firms in absolute values (rather than relative outcomes as in Table 3. Estimates in Panel A are based on monthly information. Pre-period is from 28 to 11 months pre-birth, the period from ten months pre- to one months post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in Panels B, pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to the birth year, 1-2 and 3-4 years after birth. Standard errors clustered at the firm level in parentheses. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$. *Source:* IEB, own calculations.

Table A.7: Summary event study estimates - mother outcomes by external substitutes

| | | External substitutes (terciles) | | |
|---|---|---|---|---|
| | All | 1st | 2nd | 3rd |
| | (1) | (2) | (3) | (4) |
| Panel A: Employed at pre-birth firm | | | | |
| Employed at pre-birth firm | | | | |
| Pre-period | -0.001 | -0.007 | 0.003 | 0.002 |
| | (0.006) | (0.010) | (0.011) | (0.011) |
| Short term effect | -0.131*** | -0.124*** | -0.132*** | -0.139*** |
| | (0.009) | (0.016) | (0.016) | (0.016) |
| Medium term effect | -0.010 | -0.011 | -0.012 | -0.009 |
| | (0.011) | (0.019) | (0.019) | (0.019) |
| Longer term effect | -0.001 | 0.002 | -0.024 | 0.018 |
| | (0.011) | (0.019) | (0.019) | (0.020) |
| Mothers | 23,617 | 7,866 | 7,866 | 7,885 |
| Observations | 2408934 | 802,332 | 802,332 | 804,270 |
| Panel B: Annual earnings in calendar year at pre-birth firm | | | | |
| Pre-period | 144.100 | -594.397 | 629.192 | 372.355 |
| | (261.969) | (425.831) | (472.759) | (460.997) |
| Short term effect | -1508.969*** | -1678.065*** | -1512.051*** | -1349.200*** |
| | (285.881) | (465.258) | (525.526) | (491.590) |
| Medium term effect | 40.278 | -189.586 | -117.322 | 387.940 |
| | (324.003) | (536.139) | (587.253) | (556.382) |
| Longer term effect | 392.273 | 308.390 | -217.814 | 1089.295* |
| | (341.554) | (565.293) | (619.238) | (586.750) |
| Pre mean | 23,776.2 | 22,764 | 24,610.1 | 23,954.2 |
| Mothers | 23,617 | 7,866 | 7,866 | 7,885 |
| Observations | 188,936 | 62,928 | 62,928 | 63,080 |

*Notes:* Table summarises event study estimates for the main outcomes of mothers in discrete time periods based on eq. (3). Estimates in Panel A are based on monthly information. Pre-period is from 28 to 11 months pre-birth, the period from ten months pre-birth to one month post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in Panel B, pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to the birth year, 1-2 and 3-4 years after birth. Internal substitutes are defined as the number of co-workers in the same occupation ten months prior to birth. External substitutes are defined as the number of employees in a commuting zone in the same occupation as the mother, per square kilometre. Standard errors clustered at the mother level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

Table A.8: Summary event study estimates - firm outcomes by external substitutes

| | | External substitutes (terciles) | | |
|---|---|---|---|---|
| | All | 1st | 2nd | 3rd |
| | (1) | (2) | (3) | (4) |
| Panel A: Firm's relative employment | | | | |
| Pre-period | -0.008 | -0.020* | 0.014 | -0.018 |
| | (0.007) | (0.011) | (0.012) | (0.013) |
| Short term effect | -0.027*** | -0.026** | -0.032*** | -0.022* |
| | (0.007) | (0.012) | (0.012) | (0.013) |
| Medium term effect | -0.011 | -0.024 | -0.002 | -0.007 |
| | (0.011) | (0.018) | (0.019) | (0.020) |
| Longer term effect | 0.008 | -0.011 | 0.014 | 0.019 |
| | (0.013) | (0.022) | (0.023) | (0.024) |
| Firms | 23,616 | 7,866 | 7,865 | 7,885 |
| Observations | 2389986 | 797,994 | 795,559 | 796,433 |
| Panel B: Firm's relative annual wage bill | | | | |
| Pre-period | 0.003 | -0.009 | 0.016 | 0.002 |
| | (0.008) | (0.013) | (0.013) | (0.014) |
| Short term effect | -0.015** | -0.012 | -0.028** | -0.005 |
| | (0.007) | (0.012) | (0.012) | (0.013) |
| Medium term effect | -0.000 | 0.000 | -0.012 | 0.011 |
| | (0.010) | (0.017) | (0.018) | (0.019) |
| Longer term effect | 0.011 | 0.002 | 0.002 | 0.030 |
| | (0.013) | (0.022) | (0.023) | (0.024) |
| Firms | 23,617 | 7,866 | 7,866 | 7,885 |
| Observations | 187,047 | 62,463 | 62,289 | 62,295 |

*Notes:* The table summarises event study estimates for the main outcomes of at the firm level in discrete time periods based on eq. (3). Standard errors clustered at the firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

Table A.9: Summary event study estimates for West and East Germany

| | Mothers | | | | Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | Employed at pre-birth firm | | Annual earnings | | Relative employment | | Relative wage bill | |
| Location: | West | East | West | East | West | East | West | East |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Pre-period | -0.002 | 0.007 | 176.830 | -178.254 | -0.006 | -0.021 | 0.005 | -0.007 |
| | (0.007) | (0.019) | (278.257) | (772.989) | (0.007) | (0.022) | (0.008) | (0.024) |
| Short term effect | -0.135*** | -0.097*** | -1458.515*** | -1782.821** | -0.026*** | -0.030 | -0.014* | -0.025 |
| | (0.010) | (0.026) | (303.684) | (825.640) | (0.007) | (0.022) | (0.008) | (0.023) |
| Medium term effect | -0.011 | 0.009 | 99.278 | -3.016 | -0.013 | 0.007 | -0.003 | 0.020 |
| | (0.012) | (0.035) | (334.590) | (1009.905) | (0.011) | (0.034) | (0.011) | (0.033) |
| Longer term effect | -0.001 | 0.004 | 457.491 | 240.314 | 0.008 | 0.002 | 0.012 | 0.007 |
| | (0.012) | (0.036) | (353.472) | (1116.130) | (0.014) | (0.042) | (0.014) | (0.042) |
| Mothers / firms | 21,134 | 2,483 | 25,067.2 | 22,647.2 | 21,133 | 2,483 | 21,134 | 2,483 |
| Observations | 2,155,668 | 253,266 | 21,134 | 2,483 | 2,138,695 | 251,291 | 167,393 | 19,654 |

*Notes:* Table shows summary event study estimates for the main outcomes of mothers and firms in discrete time periods based on eq. (3) separately for East and West Germany. The location is determined by the pre-birth firm of mothers. See Table 3 for other notes. Standard errors clustered at the mother / firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.
*Source:* IEB, own calculations.