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DISCUSSION PAPER SERIES

IZA DP No. 16843

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Mathias Huebener
BiB and IZA

Daniel Kuehnle
University of Duisburg-Essen and IZA

Jonas Jessen
IZA and IAB

Michael Oberfichtner
IAB, University of Erlangen-Nuremberg and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Parental Leave, Worker Substitutability, and Firms' Employment*

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 parental leave absences affect small- and medium-sized firms. We show that they anticipate the absence with replacement hirings in the six months before childbirth. A 2007 parental leave reform extending leave absences reduces firm-level employment and total wages in the first year after childbirth, driven by firms with few internal substitutes for the absent mother. However, we do not find longer-term effects on firms' employment, wage-bill, or likelihood to shut down. We find that the reform increases replacement hirings, but firms directly affected do not respond to longer expected absences of mothers by subsequently hiring fewer young women. Overall, our findings show that extended parental leave does not have a lasting impact on firms when these can anticipate the absences.

JEL Classification: J16, J18, J24
Keywords: parental leave, worker absences, worker substitutability

Corresponding author:
Jonas Jessen
IZA
Schaumburg-Lippe-Straße 5-9
53113 Bonn
Germany
E-mail: Jessen@iza.org

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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 they entail longer employment interruptions that firms need to handle. Although this may be costly for firms if they cannot easily replace the absent worker, longer leave may also help firms to retain more productive workers or to find more suitable replacements.

Our paper examines how parental leave absences affect firms using administrative linked employer-employee data that cover the universe of firms in Germany. We study effects on firms' hiring and separations, employment, wages and shutdown. Our data allow us to zoom into the adjustment processes around childbirth and analyse (i) the exact timing of replacement processes based on exceptionally detailed high-frequency data, and (ii) how firms use internal and external substitutes to cope with parental leave absences. We then examine how firms respond to a reform that extended parental leave for medium- and high-earning mothers in the first year after childbirth. We also investigate the longer-term implications for hiring, especially the likelihood of hiring young women who might be more affected by longer leave absences. Our analysis focuses on firms with up to 100 employees, thereby addressing the debate on adverse effects of parental leave on small and medium-sized firms (Rossin-Slater, 2018). In our period of analysis, these firms constitute 98.4 percent of all enterprises and employ more than every second worker (57.3 percent) in Germany.

Despite a large literature on the short- and long-term effects of parental leave on mothers' employment and earnings (e.g., Lalive and Zweimüller, 2009, Schönberg and Ludsteck, 2014, Olivetti and Petrongolo, 2017), the economic literature on the effects on firms is still in its infancy. Brenøe et al. (forthcoming) examine the joint effect of pregnancy and subsequent parental leave on firms with a matching- and event-study-approach, finding negligible costs for firms, unless they have few internal substitutes for the mother. Only two studies analyse extensions of paid parental leave schemes: Ginja et al. (2023) study the effects of a parental leave extension from 12 to 15 months in Sweden in 1989 and find that the reform increased mothers' parental leave and that firms

with greater exposure incurred larger additional wage costs. Gallen (2019) studies effects of a parental leave extension from 8 to 10 months in Denmark in 2002, finding negative effects on firm survival. However, both evaluated reforms were applied retroactively. Thus, mothers had already begun their parental leave when it was unexpectedly extended, forcing firms to adjust to the longer absences. As firms typically anticipate the timing and length of leave, the adjustment costs could thus be particularly high in such a setting. According to List (2020), these settings may not be the most "natural" ones to learn about the costs of anticipated absences. In contrast, we contribute new evidence for a setting where firms can anticipate and account for the extended absences when deciding on replacements before mothers take leave.[1]

We begin with a descriptive analysis on firms' hirings and separations around childbirth. We document a pronounced hiring peak in the six months prior to childbirth, corresponding to 0.387 additional workers in the firm per birth, but no adjustments in separations. These "excess hirings" imply that firms incur some additional costs from births in their workforce, as hiring costs for skilled workers in Germany amount to roughly two months' wages (Muehlemann and Pfeifer, 2016). We further show that replacement hiring is most pronounced when few internal substitutes are available for the mother on leave, but does not differ by the availability of external substitutes. Our results provide the novel insight that firms adjust to expected worker absences largely before childbirth, when mothers are still at the firm.

We then study the effects of extended parental leave on firms' employment, wages and likelihood of shutdown. A 2007 parental leave reform in Germany affected all births from January 1, 2007 onward and substantially extended paid parental leave entitlements for medium- and high-earning mothers in the first year after childbirth. The reform allows us to cleanly identify causal effects as the reform was announced late enough to rule out selection effects, but before mothers went on leave. Firms were thus able to anticipate and account for longer absences in their initial replacement decision before mothers left the workplace. Our estimation strategy employs an event-study design, which compares outcomes (i) between births occurring in January to June and July to December, (ii)

[1]Further, Schmutte and Skira (2023) provide descriptive analyses for Brazil on the link between parental leave absences and firms' employment, hiring, and separations. Focusing on the quality of firms' output, Friedrich and Hackmann (2021) study the effects of extended parental leave of nurses in Denmark.

between births occurring in July 2005 to June 2006 and July 2006 to June 2007, (iii) at several points in time before and after childbirth.

Our results show that the reform significantly delayed the return of mothers to their previous workplaces if they gained additional parental leave benefits. However, the reform did not have any impact on the retention of these mothers at these firms in the medium- to longer-term. We find small negative effects on firms' employment and wage bill during the extended parental leave period, but not in the longer-term. Firms' likelihood of shutting down is not affected. The short-term gap in firms' employment is driven by firms with few internal substitutes for the absent mother. We find no differences by the availability of external substitutes. However, the reform affected the hiring in the replacement period. Firms tend to hire more workers, especially more workers with higher education levels, workers on full-time contracts, and more workers remain in the firm for at least six or twelve months after childbirth. Our findings indicate that the effects of anticipated, extended leave on firms are negligible in the longer-term despite negative short-term effects.

The final section examines whether the reform affected firms' longer-term hiring decisions. Given the negative effects on employment in the short-term, firms might hire fewer younger women to keep expected absences low if these are too costly. To identify such effects empirically, we apply the same treatment and control group assignment as before within a difference-in-differences setting. This allows us to examine the effect of being directly exposed to the reform. The intuition is that firms which already experienced prolonged absences may react faster or more strongly to the reform compared to firms without prior experience under the new parental leave policy. Our results provide no evidence that firms directly exposed to the reform are less likely to hire women of childbearing age compared to firms not directly exposed to the reform. Even when we differentiate by the availability of internal substitutes, we do not observe that firms hire fewer younger women to keep absences low.

Overall, we conclude that firms' additional costs of extended, but anticipated, parental leave absences are sufficiently small in our setting to not affect firms' outcomes and longer-term hiring. Thus, our findings draw a more optimistic picture of the costs of parental leave absences for firms compared to the setting analysed in Ginja et al. (2023) and Gallen (2019). Their analysed reforms increased women's probability of changing jobs

after childbirth, thereby creating a lasting employment gap for firms. We only find a short-term increase in absences, but no longer-term effects on women's probability to return to their firms. Another important reason for the different results may lie in the retroactive implementation of the reforms, as firms typically make initial adjustments before mothers go on leave. The idea that this anticipation could help reduce absence costs for firms is further supported by Brenøe et al. (forthcoming). They also find only minimal costs of childbirth and subsequent parental leave when businesses can also anticipate the duration of leave.

Our paper also contributes new evidence on how firms use internal and external substitutes to manage employment interruptions. 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 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 new workers. We focus on employment interruptions due to motherhood and parental leave, which rank among the most important reasons for women's absences during their prime working age. Understanding how firms handle employment interruptions due to birth-related absences is important because they differ fundamentally from sickness absences and worker deaths: First, they are typically longer than sickness absences, but mostly 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. We contribute 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 the unintended consequences of family policies for women's careers (Blau and Kahn, 2017). Theoretically, generous parental leave policies can contribute to gender gaps and glass ceilings in the labour market when they are costly for firms. However, it is empirically difficult to identify potential mother-

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

hood as the source of hiring discrimination against women in the labour market. Previous cross-country comparisons that do not allow a causal interpretation 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.[3] We add to this literature by examining whether extended parental leave reduces firms' hiring of younger women, as our setting isolates the effect of expected longer absences in case of childbirth.

2. Institutional Background

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.

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, parents 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.

[3]Few previous studies examine unintended consequences of other family-related policies, such as 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).

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 parliament passed a law that substantially reformed the paid parental leave system, affecting all parents of 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] 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). Appendix Figure A.1 illustrates the minimum and maximum benefits before and after the reform. The reform did not change the regulations concerning maternity leave, job-protected leave, or part-time employment rules during the job-protected period.

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

[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 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).

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 throughout the paper. Fourth, we have detailed occupational information for workers at the 3-digit level according to the 1988 classification of occupations (with 309 unique occupations of mothers in our sample). This allows us to identify internal and external substitutes for each worker (see below).

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.

Our dataset lacks direct information about motherhood. We identify mothers in the data by exploiting the legal requirement that employers have to notify health insurance companies about the start date of the leave period (see Müller and Strauch, 2017).[7] This method reliably identifies first births, on which we focus in our analysis, but can only identify higher-order births in the data if a mother returns to work between two births.

[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.

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

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 based on terciles of the distribution of internal substitutes ten months prior to childbirth: 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 receiving 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 the availability external substitutes into terciles.

3.3. Outcome Variables

For mothers, we consider two main outcomes. First, we analyse mothers' return to their pre-birth firm to quantify the employment gap caused by longer parental leave absences. Using exact daily 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. Second, we consider maternal earnings at their pre-birth firm which also captures changes in hourly wages, contractual

working hours, or overtime. In the data, earnings are reported as a daily average over the administrative reporting period (at most one calendar year), including bonuses and overtime pay of mothers. We deflate earnings to a common base CPI of 2010 and calculate monthly earnings.[8]

For firms, we focus on their employment level, their wage bill, and shutdown. In the absence of direct measures of firms' profits or productivity, these outcomes have been used to proxy firm performance in imperfect labour markets (Dustmann et al., 2022). The use of employment levels stems from the idea that employment creates a surplus that accrues at least partly to the firm in labour markets with imperfect competition (Manning, 2011). Empirical evidence supports this by demonstrating that firms receive a large share of the surplus from employment relationships (Jäger et al., 2020). 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' wage bill, which includes changes at the intensive margin, wage changes, bonus payments, and overtime pay. As firms are not responsible for providing parental leave payments to mothers, these payments are not reflected in firms' wage bill. Analogous to mothers' earnings, we measure the wage bill of the firm at the monthly level.

We also analyse effects on firms' likelihood to shut down. Economic theory suggests that firms shut down when it is no longer profitable to sustain its operations. We define a shutdown as the moment in which firms enter zero employment without any subsequent positive employment until 2019 (e.g., as defined by U.S. Census Bureau, 2023).

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

[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.

3.4. Sample Selection and Descriptive Statistics

We focus on the population of all firms with first-time births between July 2005 to June 2007. Mothers and firms are treated by the reform if women in the firm give birth between January and June 2007. Firms with births between July and December 2006 serve as a control group. To account for seasonality in outcomes in our analysis, we further include mothers and firms with births in the preceding year (July 2005 to June 2006) in our main sample.[9] Children born before June 2007 were conceived before the parental leave reform passed the parliament in September 2006; however, firms could still plan for the prolonged absences in the new parental leave regime for at least three months in advance.

Our analysis focuses on first-time mothers for two main reasons. First, first-time motherhood has been shown to cause large and persistent employment and earnings gaps (e.g., see Kleven et al., 2019). We therefore expect the potential effects to be larger compared to mothers with higher-order births. Second, we can only identify higher-order births in the data if a mother returns to the labour market 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.

We impose the following sample restrictions to construct our analysis sample from the population of all firms with first-time births between July 2005 to June 2007; we illustrate in Appendix Figure A.2 how many observations are dropped with each step from the starting sample. First, we only consider firms in the *private* sector and drop firms that are part of the government, military, churches, and other non-profits as their substitution and wage setting processes substantially differ from private sector firms (Gregory and Borland, 1999, Oberfichtner and Schnabel, 2019). This reduces the sample by 31.2 percent. We focus on firms with up to 100 employees before the pregnancy occurs in the firm (-10.6 percent). To avoid endogenous selection into firms and occupations during pregnancy, we focus on firms where mothers have worked for at least ten months prior to giving birth, excluding 6.1 percent of firms.

To cleanly identify whether a firm was affected by the parental leave reform, we focus on firms with a first-time birth in only one of the four semesters between June 2005

[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).

and July 2007. This restriction avoids that the parental leave reform could spill over from mothers with post-reform to mothers with pre-reform births, e.g., by encouraging their earlier return if post-reform mothers' return later. Therefore, we focus on firms that experience first-time births in only one of the four semesters (-7.8 percent). In case a firm experiences multiple births in a semester, we use the first birth occurring in that period to identify when firms were first affected by a birth.[10] We drop firms that experience another first-time birth within a symmetric four year window around the birth (-12 percent). We impose this symmetric four year window on all firms in our analysis sample—affecting treatment and control firms identically—and do not place any further restrictions on mother's subsequent fertility. This restriction allows us to assign the treatment status of firms unambiguously and to trace effects independent of pre-reform births.

Our final analysis sample contains 62,959 mothers and 61,026 firms.[11] Column (1) of Table 1 provides the main descriptive statistics for our analysis sample. Mothers in our sample are on average 28.5 years old, with monthly earnings of around 1,800 euro, and firm tenure of around 4.6 years. 27 percent have higher education level, 92 percent have German citizenship, and 84 percent work full-time before childbirth. On average, firms have 15 employees, and the average workgroup size is six workers. Appendix Figure A.3 plots the distributions for firm and workgroup sizes of our analysis. Furthermore, the share of women in the firms is on average 68 percent, 82 percent of firms are based in West Germany. Appendix Table A.1 compares our analysis sample to the observations that were excluded due to the sampling restrictions. Apart from firm size, the analysis sample appears sufficiently similar to the excluded observations and, in particular, mothers exhibit similar return-to-work behaviour.

4. Firms' Hiring and Separation Responses to Motherhood

Firms have at least two options to address the employment gap due to motherhood: hiring replacements from the external labour market, or managing the gap internally

[10] To ensure that we can define the number of internal substitutes for a mother consistently at the firm-level, we restrict all analyses with internal substitutes to firms experiencing exactly one birth. Similarly, in our analysis of replacement hiring, we only look at one-birth firms, as we are interested in the precise timing of hiring events relative to the birth in the firm.

[11] To ensure that firms can reliably anticipate the applicable parental leave regime and to avoid assigning births to the wrong side of the cut-off, we additionally exclude births expected to occur two weeks before and after January 1 from the analysis.

Table 1: Summary statistics and balancing

		Sample with births between ...				
		Control Cohort		PPL Reform Cohort		
	All	Jul-Dec 05	Jan-Jun 06	Jul-Dec 06 (before PPL reform)	Jan-Jun 07 (after PPL reform)	DD coef.
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-birth characteristics: mother						
Age in years	28.484	28.335	28.678	28.315	28.637	-0.008
						(0.078)
Monthly earnings before birth	1805.310	1848.661	1809.727	1777.020	1785.348	44.170***
						(16.688)
Tenure at current firm in years	4.584	4.575	4.568	4.589	4.601	0.024
						(0.057)
Higher education level	0.268	0.260	0.269	0.265	0.279	0.004
						(0.007)
German citizenship	0.922	0.928	0.921	0.921	0.916	0.001
						(0.004)
Full-time employed	0.838	0.845	0.843	0.832	0.833	0.003
						(0.006)
Mothers	62,959	16,283	14,877	16,428	15,371	62,959
Pre-birth characteristics: firm						
Firm size	15.408	15.691	15.455	15.323	15.152	0.043
						(0.281)
Workgroup size	6.243	6.350	6.257	6.175	6.190	0.109
						(0.141)
Share of women in firm	0.675	0.675	0.675	0.676	0.676	-0.000
						(0.005)
Location in West Germany	0.820	0.818	0.822	0.819	0.822	-0.001
						(0.006)
Firms	61,026	15,750	14,435	15,948	14,893	61,026
Joint F-test that all coefficients in column (6) equal 0:						$p = 0.6229$
Joint F-test additionally including sectors:						$p = 0.5234$

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), where columns (2)-(5) report the means for mothers and firms with births in the calendar period before and after the paid parental leave (PPL) reform. The coefficients in column (6) are obtained from a difference-in-differences specification outlined in eq. (4). The p-value stems from a joint estimation using the routine of Oberfichtner and Tauchmann (2021). The first p-value is based only on the variables shown in the table, the second p-value additionally includes 1-digit sectors. Robust standard errors in parentheses, * < 10% ** < 5% *** < 1%. Source: IEB, own calculations.

by reducing separations. We explore these adjustment mechanisms leveraging the high-frequency nature of our data, focusing on monthly hirings and separations. We start with a descriptive analysis for all mothers who meet our sampling criteria. Figure 1 illustrates the average number of hirings (Panel A) and separations (Panel B) in mothers' firms (black circles) and their workgroups (grey triangles), ranging from 24 months before to 24 months after childbirth.[12]

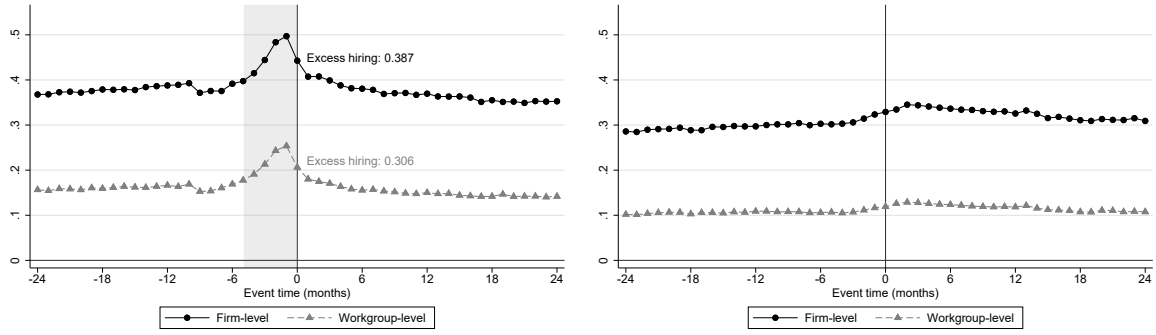
Panel A documents that hiring at the firm and workgroup level develops in parallel, with a level shift explained by hiring in occupations other than mothers' occupations. Firms hire around 0.4 workers per month on average until six months prior to childbirth.

[12]Appendix Figure A.4 presents the same figures separating between the four birth semesters.

Hiring increases at the end of the first trimester when pregnancies are commonly disclosed to employers. The peak suggests that firms partially replace absent mothers from the external labour market and allow for some transition period before workers go on leave, most likely to allow for a handover and to share job- or firm-specific knowledge.

Put into perspective, Jäger and Heining (2022) find that firms hire 0.7 additional workers over the subsequent three years after the death of a worker. Compared to these permanent worker exits, 60 percent of women return to their pre-birth employer within three years after childbirth, on average working 50 percent of their previous working time. Back-of-the-envelope, mothers' return can account for the 0.3 worker difference in external hiring compared to Jäger and Heining (2022).

Figure 1: Firms' hiring and separations around childbirth



A: Hirings B: Separations

Notes: Black circles show monthly hirings or separations at the firm-level, grey triangles are at the workgroup-level of the mother (firm-occupation cell). Event time indicates the time to the first birth in a firm and calendar months effects are partialled out. Baseline hirings are the number of hirings from 18 to 12 months pre-birth. We define excess hirings as the difference between the total number of hirings in a workgroup/firm during the six months before childbirth and the baseline hirings in the same workgroup/firm, see eq. (1). Births between July 2005 and June 2007 are in the sample and the number of firms is 57,603 (firms with multiple births in one semester are excluded to guarantee an unambiguous workgroup assignment at the firm-level). Source: IEB, own calculations.

Panel B of Figure 1 displays the average number of separations at the firm- and workgroup-level over the same period. The figure shows that separations remain relatively stable before childbirth, followed by a slight increase after childbirth. This small increase in separations post-birth can be explained by the higher excess hiring rate before childbirth, as many of the new hires leave the firm in subsequent months, especially in the workgroups directly affected by additional hirings due to childbirth.

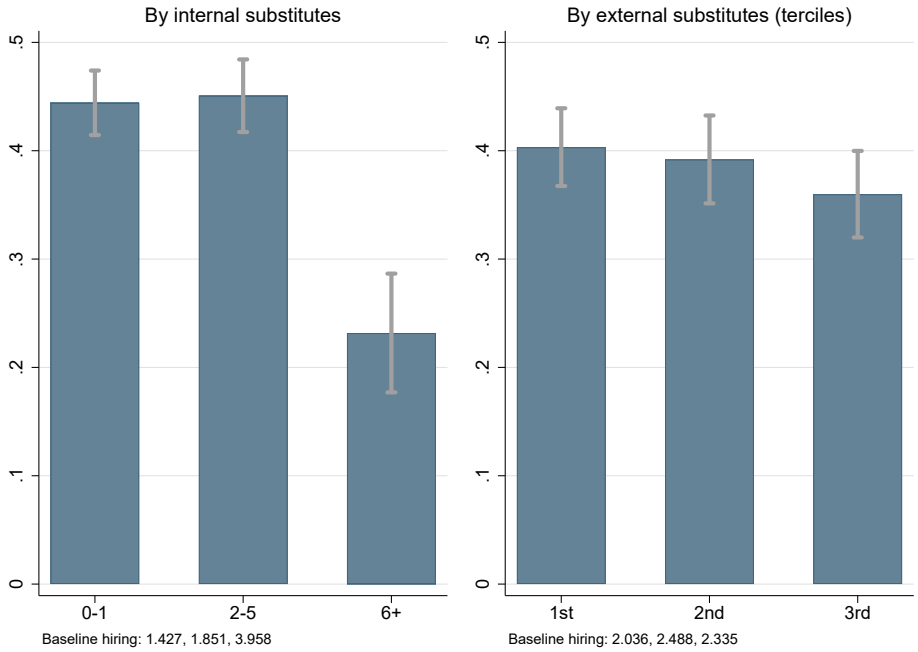
Given the importance of hiring as a key adjustment strategy, we explore these in more detail. We first compute the additional hirings due to childbirth. We define these "excess hirings" as the difference between the total number of hirings in a firm (or workgroup) i during the six months before childbirth and the number of hirings in the same calendar

months of the previous year in the same firm (or workgroup):

$$\text{excess hiring}_i = \underbrace{\sum_{t=-5}^{0} \text{hiring}_i^t}_{\substack{\text{hiring in six months} \\ \text{before childbirth}}} - \underbrace{\sum_{t=-17}^{-12} \text{hiring}_i^t}_{\substack{\text{hiring in same months} \\ \text{of the previous year}}} \tag{1}$$

On average, we observe 0.387 excess hirings in the firm, i.e., firms replace around a third of mothers through external hirings before childbirth. In the period following childbirth, hiring returns to the pre-pregnancy level. Excess hiring in mothers' workgroups amounts to 0.306, which implies that around 80 percent of excess hirings occur in mothers' workgroups. This finding indicates that mothers on leave are predominantly replaced by workers within the same 3-digit occupation, supporting our definition of substitute workers.[13]

Figure 2: Firms' excess hiring by internal and external substitutes



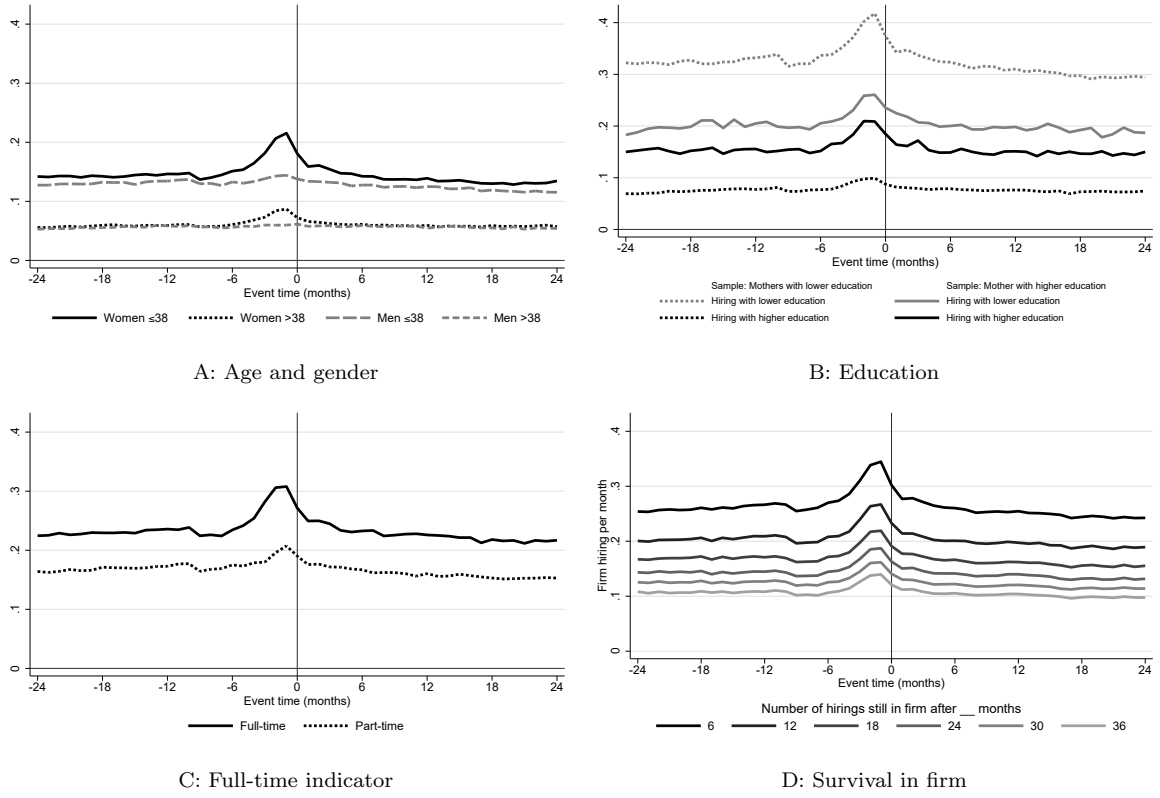
Notes: Figure shows firms' excess hirings as defined in eq. (1) by availability of internal and external substitutes for the mother going on leave. Baseline hiring denotes the second term on the right-hand side of the equation. Internal substitutes are defined as the number of co-workers in the same workgroup ten months prior to birth. External substitutes are defined as the number of employees in the same occupation as the mother, per square kilometre in the same local labour market region. See Figure 1 for other notes. Source: IEB, own calculations.

Next, we investigate how firms' replacement hiring differs by the availability of internal

[13]Unlike excess hiring, the period during which incumbent workers are retained may span the entire period of the mother's absence. Hence, we do not calculate a similar excess measure for separations.

and external substitutes. The left panel of Figure 2 reveals that excess hiring before childbirth is more pronounced in workgroups with fewer internal substitutes. For mothers with up to five substitutes, firms' excess hiring averages around 0.45, while in groups with six or more substitutes, firms' excess hiring drops to 0.23. The right panel indicates that firms' excess hiring barely differs between thinner and thicker labour markets. This suggests that the decision of firms to replace a mother on leave through external hiring is not significantly influenced by the availability of external substitutes in the labour market.

Figure 3: Firms' hiring composition around childbirth



A: Age and gender

B: Education

C: Full-time indicator

D: Survival in firm

Notes: Figures show the characteristics of new hirings by event time. Panel A distinguishes by age and gender, Panel B by education (separately by mothers' education), Panel C by whether the new hirings work full- or part-time and Panel D by the minimum survival of new hirings in the firm. Source: IEB, own calculations.

To learn more about the traits of the new hires during the replacement period, we examine their characteristics in Figure 3. Panel A shows that mothers on leave are more frequently replaced by other women, particularly younger ones. In Panel B, we examine educational differences between the mothers on leave and their replacements. Dashed lines represent mothers with lower education, while solid lines denote those with higher education; grey lines indicate replacements with lower education, and black lines denote

those with higher education. We see that mother with lower education are more likely to be substituted by individuals with similarly lower education levels. Better educated mothers on leave are externally replaced with workers of both lower and higher levels of education. Panel C focuses on the full-/part-time dimension of these replacements, revealing that most additional workers are hired for full-time positions. Panel D examines the subsequent tenure of these external replacements in the firm. Generally, workers hired as replacements before childbirth tend to leave the firms at similar rates as workers hired in other periods (see also the shares in Appendix Figure A.5).

5. Effects of Extending Parental Leave Benefits on Mothers and Firms

5.1. Empirical Strategy

To estimate the effects of the 2007 parental leave reform on mothers and firms, we implement a difference-in-differences/event-study design. We use the same estimation strategy for mothers and firms. 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. For the event-study, we use the 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 event-study model:

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

where y is the outcome of mother or firm i at event-time t; $t = 0$ corresponds to the

month of birth. The variable $reform_i$ takes the value of 1 if a mother gives birth between July 2006 and June 2007, and 0 for births between July 2005 and June 2006. 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 γ_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, 2023) and cluster the standard errors at the mother- or firm-level.

To summarise our 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 12 months after birth, s), medium-term effects covering the remaining job protection period (13 to 36 months after birth, m), and longer-term effects (37 to 54 months after birth, l). We estimate the following regression:

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

where γ_t denotes the period-specific effects.

Identifying assumptions. To interpret the γ_t coefficients as the effects of the parental leave reform, our empirical strategy relies on the parallel trends assumption, i.e., the outcomes between treatment and control mothers and firms would have followed common trends in absence of the reform. Our identification strategy could be threatened if the reform affects fertility, 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 (Kluve and Tamm, 2013), the final law was only passed in September 2006. 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 law, such that differential selection into motherhood should not bias our estimates. Selective birth timing around the cut-off (as shown by Tamm, 2013, Neugart and Ohlsson, 2013, Jürges, 2017) is also not an issue in our setting, as we exclude mothers giving birth

two weeks before and after the reform date.

To assess any systematic differences between mothers and firms in the treatment and control groups, we check the covariate balancing of maternal and firm characteristics within our difference-in-differences framework. Specifically, we estimate the following regression model:

$$x_i = \beta_0 + \beta_1 reform_i + \beta_2 spring_i + \beta_3 reform_i \times spring_i + \epsilon_i \tag{4}$$

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 coefficient on the interaction term identifies potential covariate imbalances for mothers/firms with births under the new parental leave regime.

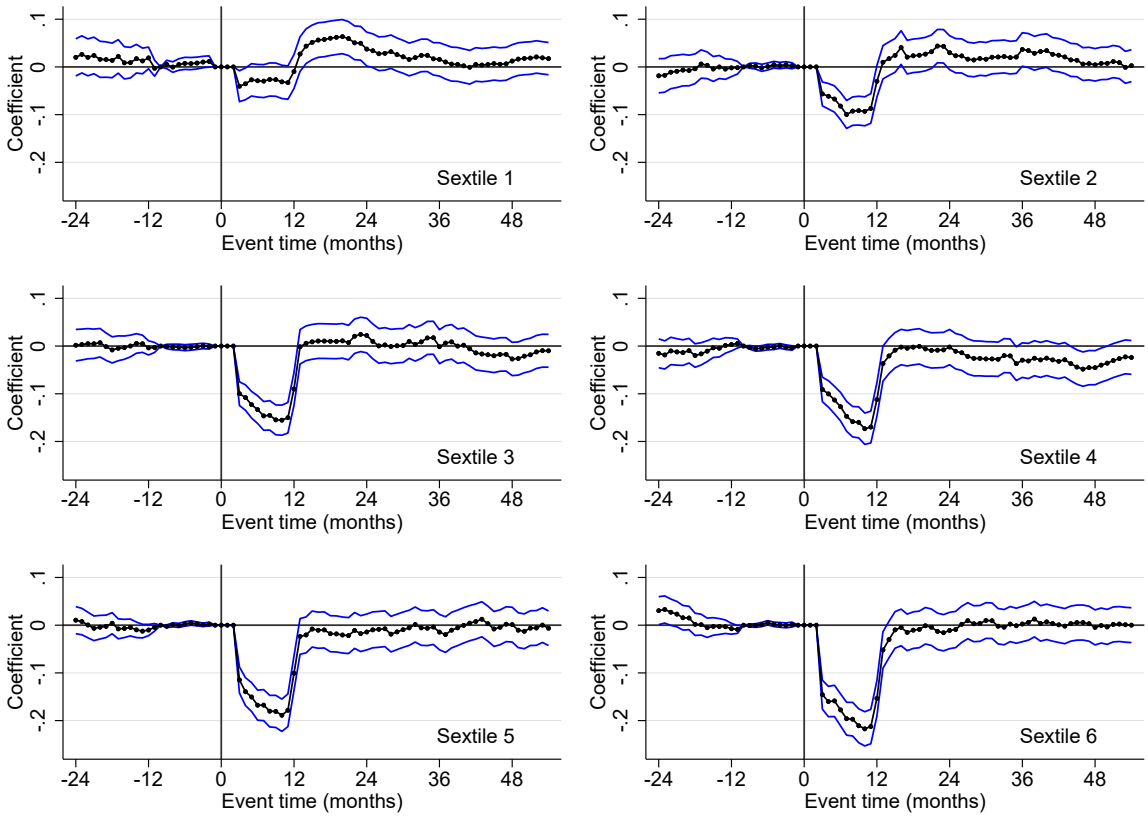
Columns (2)-(5) of Table 1 provide the means for each of the four groups, and column (6) reports the β_3 coefficient estimates from eq. (4). Overall, the balancing checks alleviate concerns about endogenous sample selection as we find no evidence for any systematic differences between treatment and control firms. Only the coefficient on earnings is significant individually, though small economically, but once we take into account multiple hypothesis testing, the joint F-tests do not reveal statistically significant differences between the groups.

5.2. Effect of Extended Parental Leave Effects on Mothers and Firms

We begin our analysis by examining mothers' absences from their pre-birth firms. Given the incentive structure of the paid parental leave reform, we expect different effects depending on mothers' pre-birth earnings. For a substantial share of mothers with low earnings, parental leave benefits did not increase in the first year after childbirth and the reform potentially withdrew leave benefits in the second year after childbirth; thus, we expect a positive labour supply effect for the second year. For mothers with medium to high earnings with a new leave entitlement in the first year, we expect a decrease in labour supply in the first year after childbirth, and no effect in the second year after childbirth.

We document the effect separately by mothers' pre-birth earnings in Figure 4, which plots the event study coefficients of the reform according to eq. (2) for the different sex-

Figure 4: Effects on mothers' employment at their pre-birth firm by pre-birth earnings sextile



Notes: The figure plots event study estimates of the 2007 paid parental leave reform in Germany on a binary indicator whether the mother works at her pre-birth firm based on eq. (2). Blue lines indicate the 95% confidence interval; standard errors clustered at the mother-level. The six panels are based on pre-birth earnings terciles. The thresholds for the sextiles are at monthly pre-birth earnings (ten months pre-birth) of 730, 1295, 1688, 2129 and 2733 euro. *Source:* IEB, own calculations.

tiles of their pre-birth earnings distribution. Supporting the parallel trends assumption, we observe flat pre-trends in maternal employment in the two years before childbirth across all panels. As expected, mothers in the lowest earnings sextile display a small but insignificant reduction in employment in the first year after childbirth, followed by a significant increase in employment in the second year after childbirth. Mothers in the second sextile display a significant decrease in the first year after childbirth, and a small, not statistically significant, increase in employment in the second year after childbirth. For the remaining groups, we observe large employment reductions in the first year, but no increases in the second year after childbirth.

Figure 4 shows that only women in the medium- and high-earnings brackets exhibit a monotonic effect of the treatment, indicating a clear reduction in their workplace presence post-reform. In contrast, the effects for mothers with low earnings are mixed in the first

and second year after childbirth, making it uncertain whether their absences from work increased or decreased following the reform. To focus on a group with a clear response to the treatment, we will concentrate on mothers in the medium to high earnings brackets in the following. Due to the lack of a distinct earnings cut-off and based on the results from Figure 4, we set the earnings threshold at the upper end of the second sextile, which is 1,295 euro in monthly pre-birth earnings.[14]

Next, we examine in more detail how the reform affected the outcomes of medium and high earning mothers, and their firms.[15] In Figure 5, Panel A, we observe that 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 18 percentage points six to ten months after birth, or 39 percent relative to the pre-reform average). After the first 13 months, we observe no meaningful differences in the probability to work at the same firm up to 54 months after childbirth. Hence, the reform strongly increased mothers' absences in the first year after childbirth but had no effect on their medium- and longer-term absences, e.g., through increased separations as observed by Ginja et al. (2023) for a parental leave extension in Sweden.[16]

Panel B of Figure 5 presents estimates on mothers' monthly earnings at their pre-birth firms. Consistent with the longer absence after childbirth due to the reform, treated mothers earn about 240 euro less per month than control group mothers, who have average monthly earnings of 359 euro in that time period, in the year after childbirth. We observe zero-effects from the second year after childbirth onwards, which suggests that the reform did not affect the working hours of mothers in the medium- and long-run.[17] In Table 2, we provide the corresponding short-, medium- and longer-term estimates in columns 1 and 2.

How does this negative, temporary labour supply shock affect firms? In frictionless labour markets, firms would be expected to fully compensate the gap. Panel C of Figure 5 examines total employment at the firm and shows that the parental leave extension

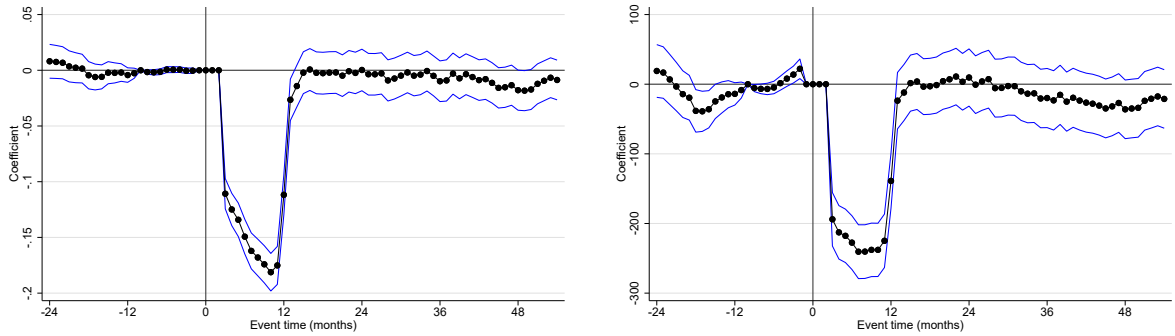
[14]We show summary statistics and the balancing of characteristics within this sample in Appendix Table A.2.

[15]In Appendix Table A.3 we also provide summary estimates for the sample including lower earning mothers and their firms.

[16]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, Huebener et al., 2019, Frodermann et al., 2023).

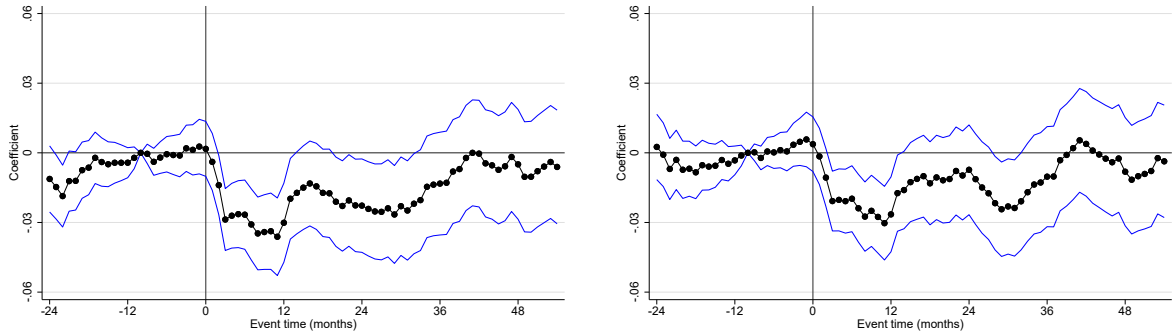
[17]As mentioned in section 3, the data does not provide information on working hours.

Figure 5: Event study of parental leave reform effects on mothers' and firms' outcomes



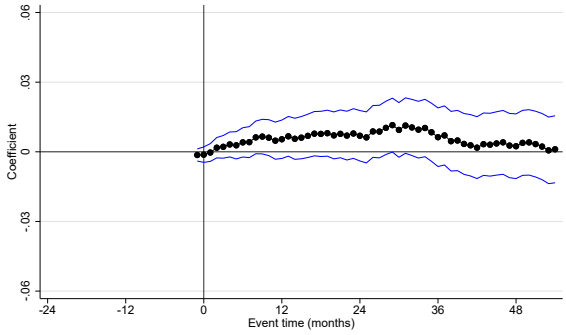
A: Mother's employment at pre-birth firm

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



C: Firm's employment (relative to baseline)

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



E: Firm shutdown

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.

reduces employment within the first year after childbirth by up to 3 percent (see column 3, Table 2).[18] The treatment effect remains negative and converges to zero three years after childbirth, which is after the expiry of the job-protected period.

To capture internal adjustments like increased working hours of internal substitutes,

[18]Note that we cannot directly compare the point estimates between Panels A and C in Figure 5, as Panel A is interpreted in percentage points and Panel C in percent.

Table 2: Summary of event study estimates

Outcome:	Mothers		Firms		
	Employment at pre-birth firm	Earnings at pre-birth firm	Relative employment	Relative wage bill	Firm shutdown
	(1)	(2)	(3)	(4)	(5)
Pre-period	0.005	8.268	-0.006	-0.005	.
	(0.005)	(12.559)	(0.006)	(0.005)	(.)
Short term effect	-0.145***	-195.892***	-0.029***	-0.025***	0.005
	(0.007)	(15.712)	(0.005)	(0.005)	(0.003)
Medium term effect	-0.002	16.163	-0.019**	-0.015*	0.008
	(0.008)	(17.088)	(0.008)	(0.008)	(0.005)
Longer term effect	-0.005	-0.583	-0.003	-0.003	0.001
	(0.008)	(18.104)	(0.011)	(0.011)	(0.007)
Mothers / firms	42,053	42,053	40,776	40,776	40,776
Observations	4,289,406	4,289,406	4,159,152	4,159,152	4,159,152

Notes: Table summarises event study estimates in discrete time periods based on eq. (3). 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 3-12, 13-36 and 37-58 months post-birth, respectively. Standard errors clustered at the mother / firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

we examine firms' wage bill in Panel D of Figure 5. The reform slightly reduced total labour costs in the 12 months following childbirth by about 2.5 percent (see column 4, Table 2). This finding suggests that firms do not completely fill the gap created by mothers' longer leave. The negative effects on employment and the wage bill diminish over time, coming close to zero and becoming statistically insignificant in the longer-term, i.e., 37 to 58 months after childbirth.

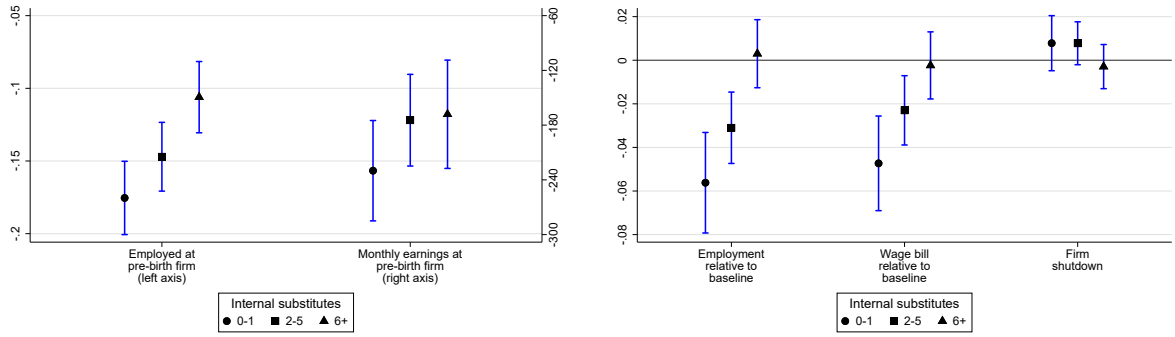
Panel E of Figure 5 reports the estimated effects on firms' permanent shutdown. For this outcome, we cannot examine the pre-trends as firms, by our definition, must exist before childbirth. The estimates suggest very small and insignificant effects until 54 months after childbirth. The DiD summary estimates are precisely estimated and provide no evidence that the reform affected firm shutdown (see column 5, Table 2).

Our main results are robust to alternative specifications that we summarise in Appendix Figure A.6. We report the baseline estimates in the left panels. In the panels in the middle we narrow the sample to births from a six- to a two-months window before and after the reform. The panels at the right show an intermediate window of four months. While our preferred six-months window has the tightest confidence intervals, we reach the same inclusion using narrower windows around the reform.

5.3. Effect Heterogeneity by Availability of Internal and External Substitutes

The effects of mothers' prolonged absences might well depend on the availability of suitable replacements as firms' replacement hirings are higher when they have few internal substitutes available. To explore this aspect further, we differentiate the analysis by the availability of internal and external substitutes and re-estimate the previous models by the availability of both types of substitutes. Our discussion focuses on the short-term effects of the reform on both mothers' and firms' outcomes as we estimate null-effects for the medium- and longer-term (see Appendix Tables A.4 and A.5 for the full estimation results by internal and external substitutability, respectively).

Figure 6: Short-term effects by internal substitutes



A: Mothers B: Firms

Notes: Figure shows the short-term estimates as in Table 2 by internal substitutes of mothers. Internal substitutes are defined as the number of workers in the same workgroup (occupation-firm cell) as mothers ten months before birth. *Source:* IEB, own calculations.

Panel A of Figure 6 presents the short-term coefficients of mothers' outcomes, differentiated by the availability of internal substitutes. Notably, the additional short-term employment gap is substantially larger when only few internal substitutes are available at the firm (-17.5pp vs -10.6pp).[19,20] This finding suggests that firms do not compel women to go back to work earlier while they are eligible for parental leave benefits, despite firms facing more constraints with fewer internal substitutes. Thus, parental leave

[19]This pattern can also be seen when comparing the Kaplan-Meier failure graphs for the pre- and post-reform periods, see Appendix Figure A.7. The figure furthermore shows descriptively that mothers with few internal substitutes return earlier to their employer than those with more internal substitutes, when they are not eligible for parental leave benefits.

[20]The differences by workgroup size are also statistically significant, see Appendix Table A.6, which uses a less demanding specification. We also examine different operationalisations of workgroup size differences in Appendix Figure A.8 and consider interactions with ln(workgroup size), 3rd vs. 1st tercile of workgroup size distribution, and a median split of workgroup size distribution. We reach the same conclusions using these different approaches.

choices appear to lie more with the women than the firms. Concerning mothers' earnings, the negative effects are slightly more pronounced in workgroups with one or no internal substitute. However, these differences are not statistically significant compared to those in larger workgroups (refer to Appendix Table A.6).

Panel B of Figure 6 reports the results for firm outcomes. The first set of coefficients shows that the reform reduced relative employment particularly for firms with few internal substitutes for the mother on leave. For instance, in workgroups with at most one substitute, employment reduces by 5.6 percent in the 14 months after childbirth, compared to a 3.1 percent decline in workgroups with 2-5 substitutes. We do not observe any economically or statistically relevant reduction in firms' employment when absent mothers have at least six internal substitutes. Firms' wage bill show a similar pattern. The probability that firms shut down is not affected by the extended parental leave absences, irrespective of the availability of internal substitutes.[21]

Smaller firms may generally find it more challenging to handle longer worker absences. Previous evidence from Sweden by Ginja et al. (2023) shows that wage bills increase mainly in firms with less than 50 employees. Gallen (2019) finds that the parental leave extension in Denmark mainly lowered the survival probability of firms with less than 30 employees.[22] When we differentiate our analysis by firm size, we find larger short-term employment gaps in smaller firms with up to 30 employees, though no longer-term effects on firms' employment, wage bill and shutdown (Appendix Table A.7). However, firm size and workgroup size are correlated. To better understand whether differences in the availability of internal substitutes or firm size determine the short-term impact on firms, we also restrict our analysis to firms with at least 11 employees (above median), ensuring that all workgroup sizes are represented (see Appendix Figure A.9). We find that the reform effects are larger in firms with few internal substitutes for the mother on leave (see Appendix Table A.8). This analysis confirms that our findings regarding internal substitutability do not reflect differences in firm size.[23]

[21] In Appendix Table A.9 we report, analogous to mothers, estimates for firms where the treatment indicators are interacted with the workgroup size.

[22] Beyond the negative effects on the survival probability of small firms, Gallen (2019) finds economically small and insignificant effects on surviving firms' size, their fraction of female workers, and sickness leave taking or parental leave taking of co-workers.

[23] As one may worry that these heterogeneities reflect differences in baseline levels, we report similar findings on absolute effects on firms' employment and their wage bill in Appendix Table A.10.

As firms might partially substitute the mother on leave through the external labour market, we further investigate whether the reform effects on mothers and firms differ by the thickness of the local labour market. We find that short-term effects are not statistically different depending on the availability of external substitutes, see Appendix Table A.5.

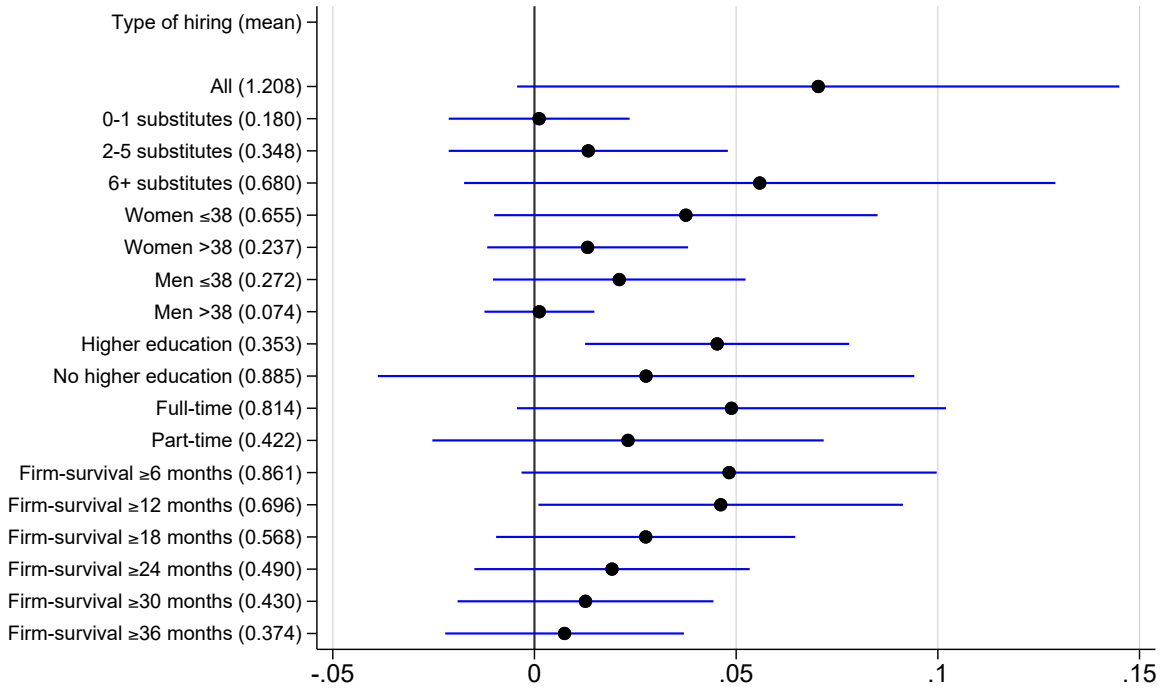
Overall, this section shows that firms, on average, do not fully close the short-run employment gap caused by longer parental leave absences. This effect is driven by firms that have only few internal substitutes available for the mother on leave. However, we do not find that extended parental leave absences impact firms' employment or wage bill in the long-run.

5.4. Effects on Replacement Hiring

We now shift our focus to the individuals hired during the replacement period. We focus on workers hired within mothers' workgroups where, as shown earlier, 80 percent of the replacement hiring occurs. Using the same empirical framework as in eq. (4), we estimate the effects of the parental leave reform on the number of hirings, and their characteristics, in the six months prior to childbirth. We investigate the total hirings, separately by the number of internal substitutes. Furthermore, we examine their characteristics, particularly their gender, age, education level, employment status (full-time or part-time), and tenure within the firm.

Figure 7 shows that the number of hirings increased by 0.07 workers (around 6 percent relative to the mean) in the replacement period, though the coefficient is not statistically significant at the 5 percent level. We do not observe statistically significant differences by internal substitutability of mothers, though larger but also imprecisely estimated coefficients for workgroups with 6 or more internal substitutes. The remaining coefficients show that under the extended parental leave benefits, firms hire more workers with higher education levels, workers on full-time contracts, and more workers remain in the firm for at least six or twelve months after childbirth. Yet in sum, the effect of the reform on replacement hiring is relatively small due to which the employment gap induced by mothers' longer absences is not fully filled.

Figure 7: Effect of the parental leave reform on hiring in the replacement period



Notes: Figure shows the difference-in-differences coefficient based on eq. (4) on the number of and composition of hirings in the replacement period, i.e. the six months prior to childbirth. The means reported in parentheses are calculated over the entire sample. Horizontal lines are 95% confidence intervals based on robust standard errors. *Source:* IEB, own calculations.

6. Effects on Hiring Discrimination Against Women

This section investigates the impact of extended paid parental leave on firms' longer-term hiring decisions, focusing on whether firms affected by the reform reduce the hiring of young women who are statistically more likely to extend parental leave absences in the event of childbirth following the reform. To identify the reform effects on firms' hiring behaviour, we build on the same treatment and control group assignment as before, but now concentrate on longer-term hiring outcomes. The reasoning behind this approach is that firms that have previously dealt with longer parental leave absences may respond more promptly or decisively compared to firms without prior experience under the new parental leave policy, especially if longer absences result in significant costs.

We slightly adjust the empirical approach and now focus on hirings in calendar time–instead of event-time as before–as we want to ensure that new hirings occur entirely in the post-reform period. Our data includes all hirings between July 2007 through December 2009. In total, we observe 508,951 hiring events. We focus on two main groups for this analysis: childless women below age 30, and women up to the age of 38. As outcomes,

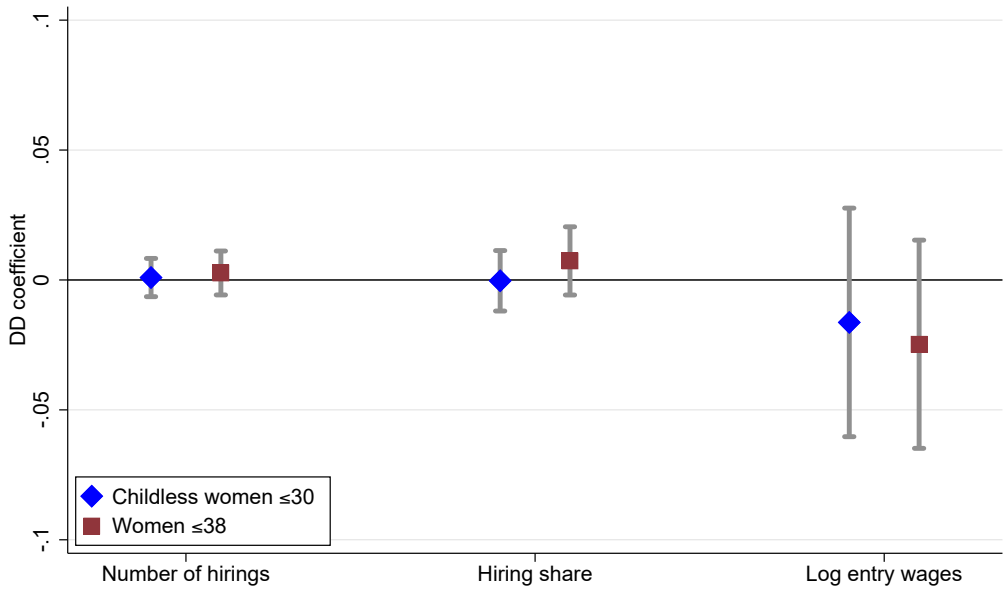
we examine i) their absolute number of hirings, ii) their share of all hirings, and iii) their log entry wages.

We study the hiring outcomes within the following difference-in-differences model:

$$y_{ijt} = \gamma_0 + \gamma_1 reform_i + \gamma_2 spring_i + \gamma_3 reform_i \times spring_i + \theta_t + \epsilon_{ijt} \tag{5}$$

where y_{ijt} is an indicator whether worker i hired into firm j at time t is a childless women up to the age of 30 (or a woman up the age of 38). θ_t denotes fixed effects for calendar time (year-by-month). The coefficient of interest is γ_3, which identifies the effect of firms' exposure to a birth event shortly after the reform on longer-term hiring outcomes of younger women. As firms may differ in the number of hirings, we weight the regressions so that all firms receive equal weight in the analysis of hirings as in the previous analyses.

Figure 8: Longer-term effects on hiring and wages of young women



Notes: Figure plots difference-in-differences coefficients for the number of hirings, the hiring share and log entry wages of young women at the firm-level. The sample period is July 2007 to December 2009. $N = 36,128$ firms with $508,951$ hirings. 95% confidence intervals based on robust standard errors. *Source:* IEB, own calculations.

Figure 8 reports the γ_3 coefficients based on eq. (5) for our main sample. The first two coefficients show that the reform had no effect on the decision to hire younger female workers by firms that previously experienced extended absences. The coefficients are precisely estimated such that we can rule out reductions of more than 0.006 female workers, which corresponds to 6 percent relative to the monthly mean hiring of 0.1 in that

period. Conditional on hiring any workers, we then examine the composition of hirings and find that the reform did not affect the propensity to hire younger women. Again, the coefficients are precisely estimated so that we can rule out reductions in the hiring share greater than one percentage point. Finally, examining the log entry wages of young women, we find no significant negative effects, although these estimates lack precision to rule out small negative wage effects. Our analysis reveals no economically substantial or statistically significant differences when we differentiate by the availability of internal substitutes, see Appendix Figures A.10.

One concern with these estimates is that the zero-effects on longer-term hiring of younger women might stem from two opposing mechanisms. On the one hand, the 'discrimination channel' suggests that firms concerned about costly extended leave absences might discriminate against younger women in their hiring. On the other hand, as shown in section 4, the reform led to more replacement hirings after births, with firms often hiring similar workers as replacements. This may induce additional demand for young women, which we label the 'replacement channel'. Our empirical set-up so far identifies the overall effect of these opposing mechanisms. To shut down the replacement channel, we conduct a robustness check focusing only on firms with no additional births until the end of 2009. Reassuringly, we find almost identical results (see Appendix Figure A.11).

Another concern is that the reform's public discussion might have prompted both affected and unaffected firms to reduce hiring young women. We address this by examining the hiring patterns of younger and older women in the raw data, see Appendix Figure A.12. We do not observe a general decline in the hiring shares of younger women, and no evidence of younger women being substituted by older women. These findings help alleviate concerns about substantial general equilibrium effects that might also lead to finding zero effects in our analysis.

In sum, our analysis does not find any evidence of hiring discrimination against younger women in the aftermath of the expansion of paid parental leave.

7. Discussion and Conclusion

This paper examines the impact of parental leave absences on firms and how they deal with them. We first show that firms hire replacement workers with similar characteristics mainly in the six months prior to childbirth. We then analyse the effects of a substantial

paid parental leave reform that significantly delayed mothers' return to their firms in the first year after childbirth. We find a short-term employment gap in firms together with a lower wage bill. The effects are more pronounced when few internal substitutes are available for the mother on leave. In the longer-term, we do not find effects on firms' employment or shutdown, independent of the availability of internal substitutes. To understand firms' response to the reform, we investigate whether firms affected by the reform hired differently during the replacement period, and show that firms indeed hire more workers in mothers' workgroups. Asking whether firms may try to avoid anticipated longer absences, we finally analyse the longer-term effects on firms hiring of younger women, but do not find any effects pointing at hiring discrimination. Overall, our findings show that extended parental leave does not have a lasting impact on firms.

What might explain the small effects on firms in our setting in comparison to evidence for the Scandinavian countries provided by Ginja et al. (2023) and Gallen (2019)? First, firms may already anticipate to lose a significant portion of their workforce after childbirth, because German mothers typically return to part-time employment after childbirth.[24] Compared to the longer-term loss in mothers' working hours that firms have to deal with anyways, the additional costs of extended parental leave might be comparably small. Our setting allows us to more directly explore the role of maternal labour market attachment after childbirth. We exploit the fact that mothers in East Germany return earlier and work more hours after childbirth than mothers in West Germany (e.g., see Jessen, 2022). We do not find longer-term effects on firms in either region (Appendix Table A.11). Hence, the high share of women returning to work part-time post-birth is not the main explanation for the small effects on firms in our study.

Second, can the way the reform was implemented explain the small effects? The German parental leave extension was announced when affected women were still working at the firm. Thus, the timing of the exogenous shock in our setting accounts for a key characteristic of parental leave absences, which is that they are anticipated. Consistent with this anticipation, we find evidence for adjustments in the replacement period before women go on leave, suggesting that firms account for the longer absence of mothers in their initial replacement strategy. In contrast, in the settings analysed by (Ginja et al.,

[24] As of 2009, employed mothers with children aged 0-14 have a full-time share of 39 percent in Germany compared to more than 80 percent in Sweden and Denmark (OECD, 2020).

2023) and Gallen (2019), the parental leave extensions were announced when women were already on leave and expected to return soon. Such unexpected and retroactively applied reforms might exacerbate negative effects for firms, which might have been avoided had firms been able to plan for extended absences. Moreover, these reforms also created permanent employment gaps for firms, as more mothers were more likely to change jobs after childbirth. Our results align with those of Brenøe et al. (forthcoming) who find that anticipated birth-related absences have little impact on firm outcomes—provided they can internally replace the mother. Put into perspective, our results draw a more optimistic picture than Ginja et al. (2023) and Gallen (2019) on the costs of parental leave extensions for firms.

To sum up, our results add a firm-side perspective on the effects of parental leave. Our findings fit well with other studies showing that such policies typically do not have, on average, long-term effects on mothers' careers (e.g., Kleven et al., forthcoming). The empirical evidence supports the conclusion that such policies help reconcile work and family life without further widening average gender gaps in the labour market. Even absent effects of parental leave policies on average gender gaps, mother's individual parental leave choice could still act as a signal about commitment to work, potentially resulting in consequences for their individual careers (Tô, 2018). Going forward, to narrow average gender gaps in the labour market, further attention should be paid to policies that support parents in returning to the labour market such as early child care spending and in-work benefits (Olivetti and Petrongolo, 2017).

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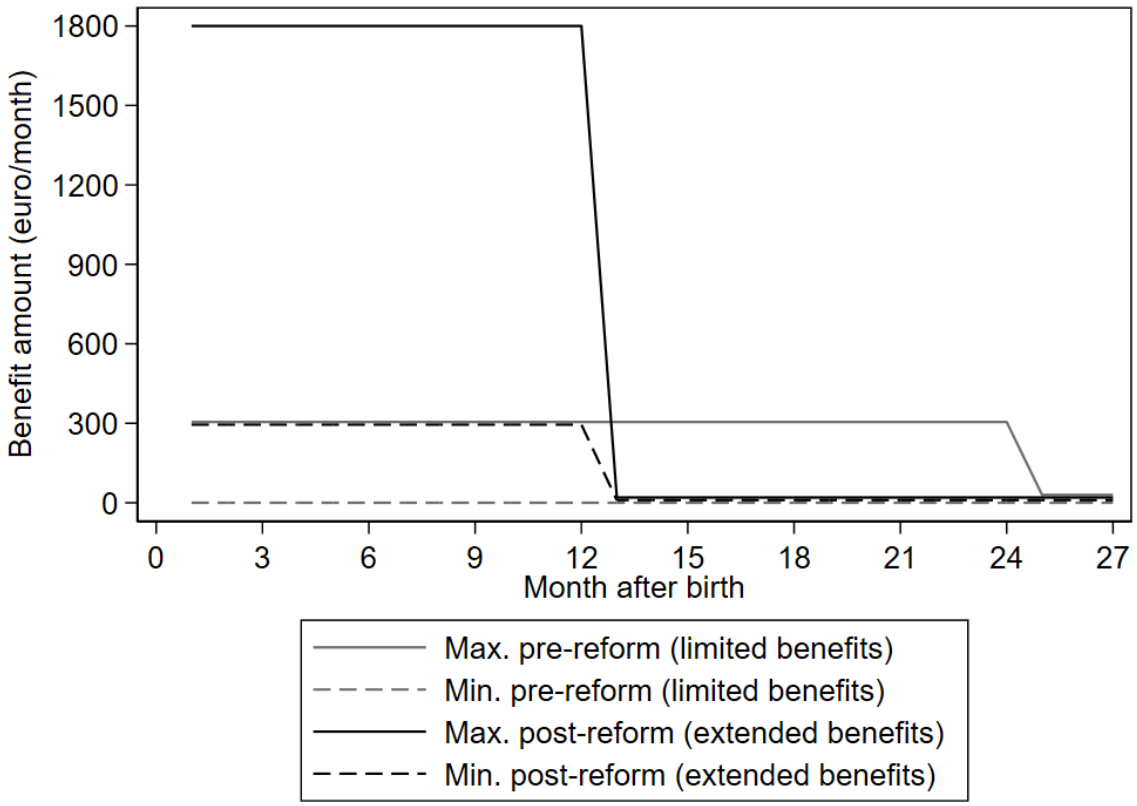
Appendix (For Online Publication)

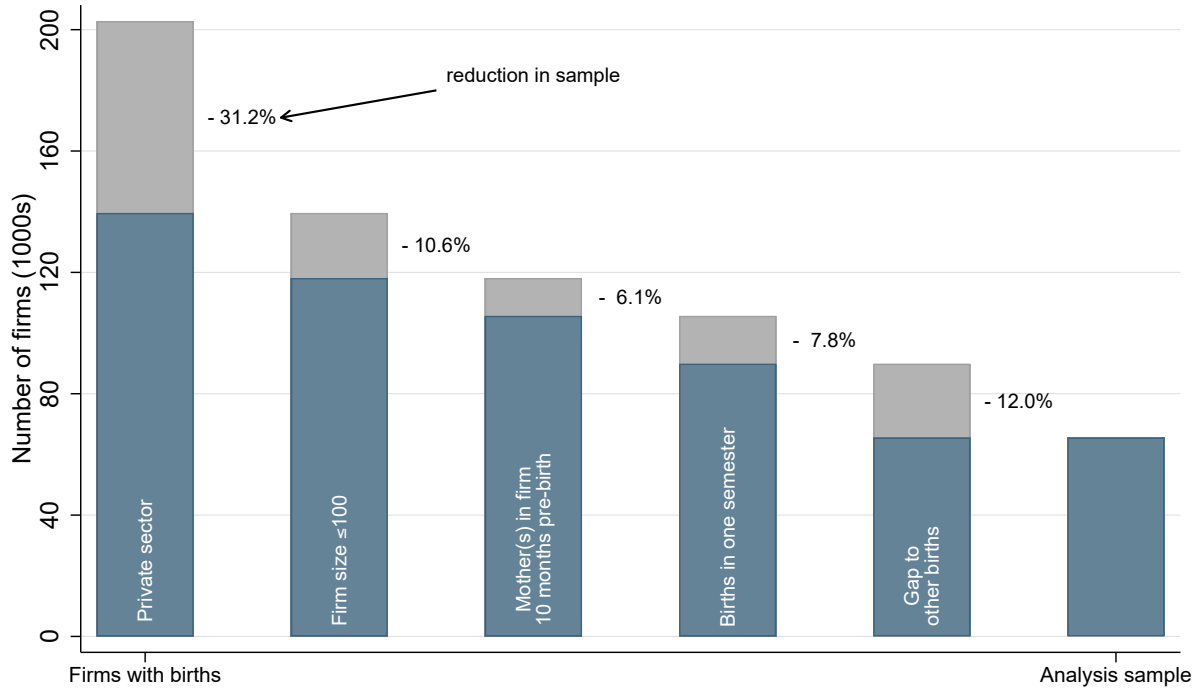
Appendix Figures

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



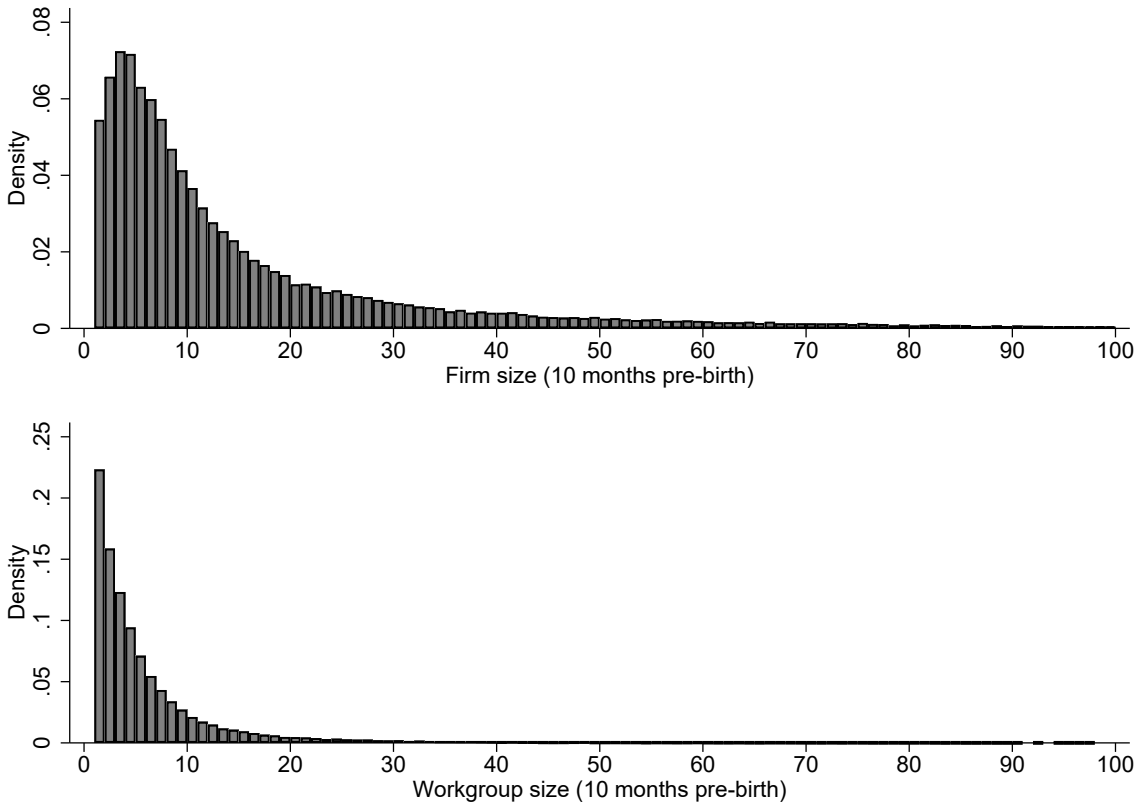
Notes: The figure illustrates the minimum and maximum benefit amounts before and after the 2007 parental leave reform. The two partner months introduced with the reform are omitted from the stylized benefit scheme.

Figure A.2: Sample selection process



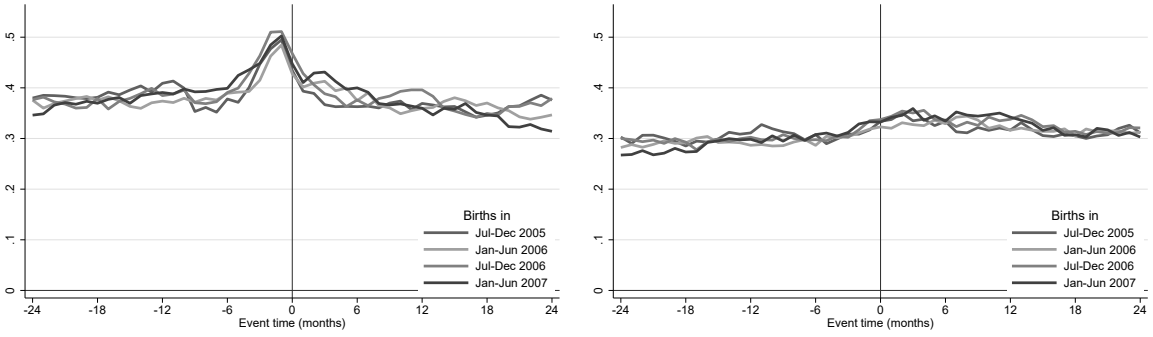
Notes: The figure illustrates the sample selection process and the share of firms dropped in each sampling restriction step described in subsection 3.4. In the final analysis sample we additionally exclude births in the two weeks around January 1 of each year. *Source:* IEB, own calculations.

Figure A.3: Firm and workgroup size of analysis sample



Notes: Figure shows the distribution of firm and workgroup (firm-occupation cell of mother) calculated ten months before birth.

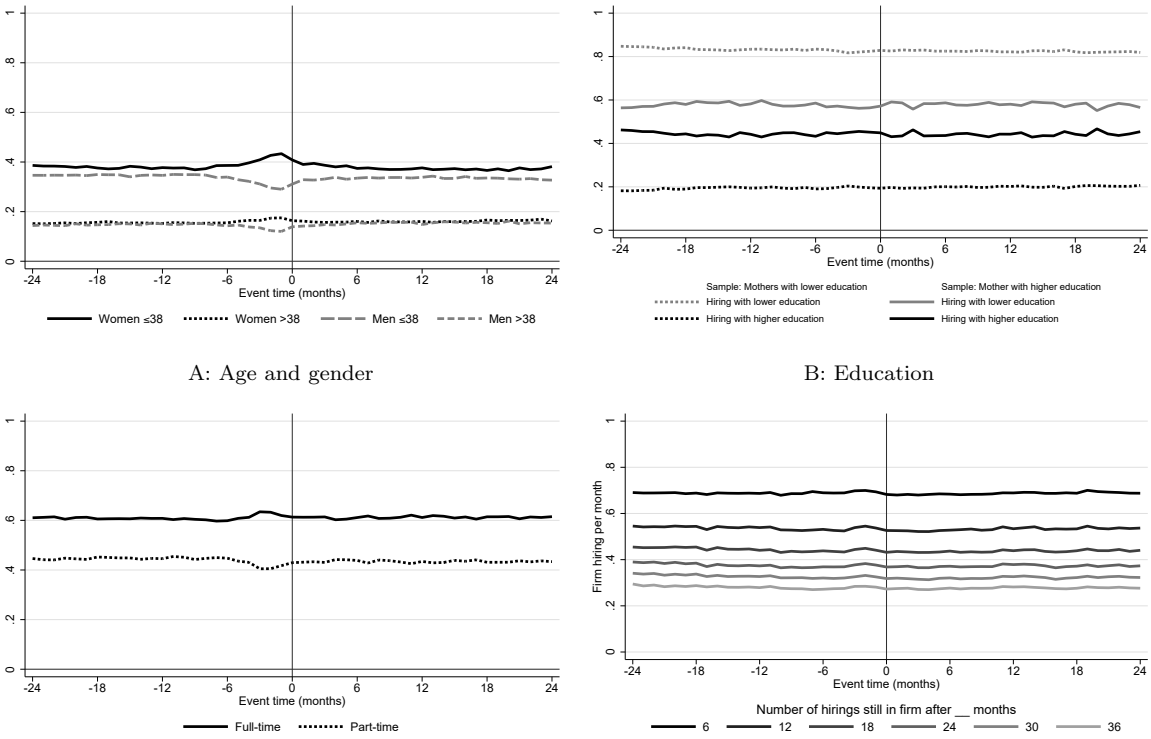
Figure A.4: Firms' hirings and separations by birth semester



A: Hirings B: Separations

Notes: Figure shows monthly hirings and separations as in Figure 1 separately by birth semester. See Figure 1 for other notes. *Source:* IEB, own calculations.

Figure A.5: Firms' hiring shares around childbirth



A: Age and gender

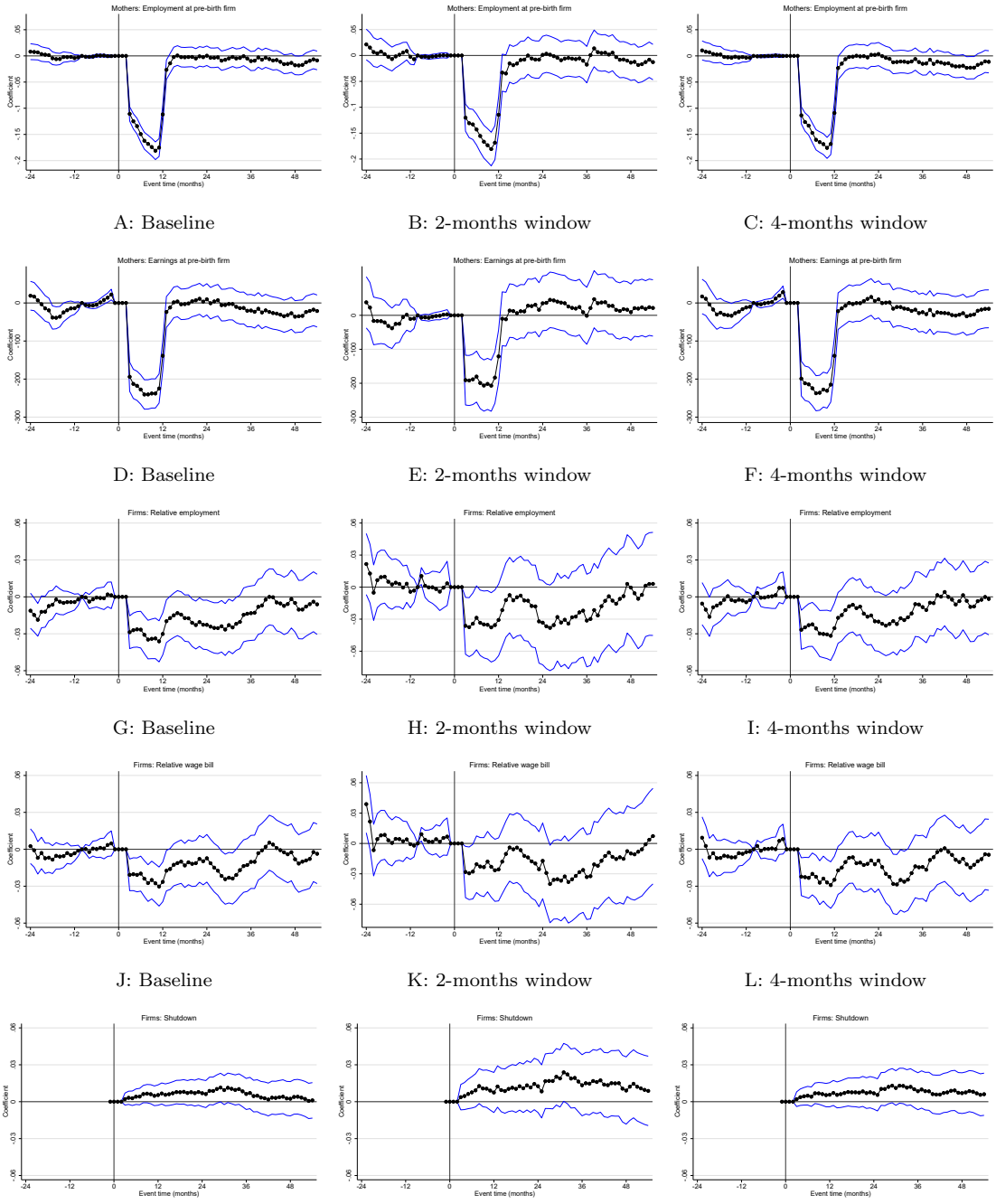
B: Education

C: Full-time indicator

D: Survival in firm

Notes: Figures show the characteristics of new hirings by event time calculated as shares of total hirings. In contrast, Figure 3 shows characteristics of absolute hirings. Panel A distinguishes by age and gender, Panel B by education (separately by mothers' education), Panel C by whether the new hirings work full- or part-time and Panel D by the minimum survival of new hirings in the firm. Source: IEB, own calculations.

Figure A.6: Specification checks



A: Baseline B: 2-months window C: 4-months window

D: Baseline E: 2-months window F: 4-months window

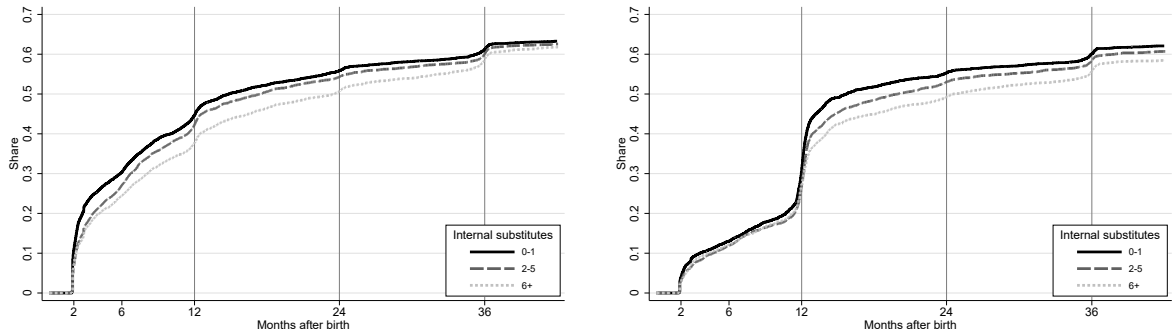
G: Baseline H: 2-months window I: 4-months window

J: Baseline K: 2-months window L: 4-months window

M: Baseline N: 2-months window O: 4-months window

Notes: The figure plots event study estimates of the 2007 paid parental leave reform in based in different specifications. Blue lines indicate 95% confidence interval, standard errors clustered at the mother-level. The left column reports our baseline estimates based on eq. (2) shown in Figure 5. The middle column shows coefficients using a window of only 2 months around the cut-off, the right column uses a bandwidth of 4 months. *Source:* IEB, own calculations.

Figure A.7: Return to pre-birth firm, by availability of internal substitutes

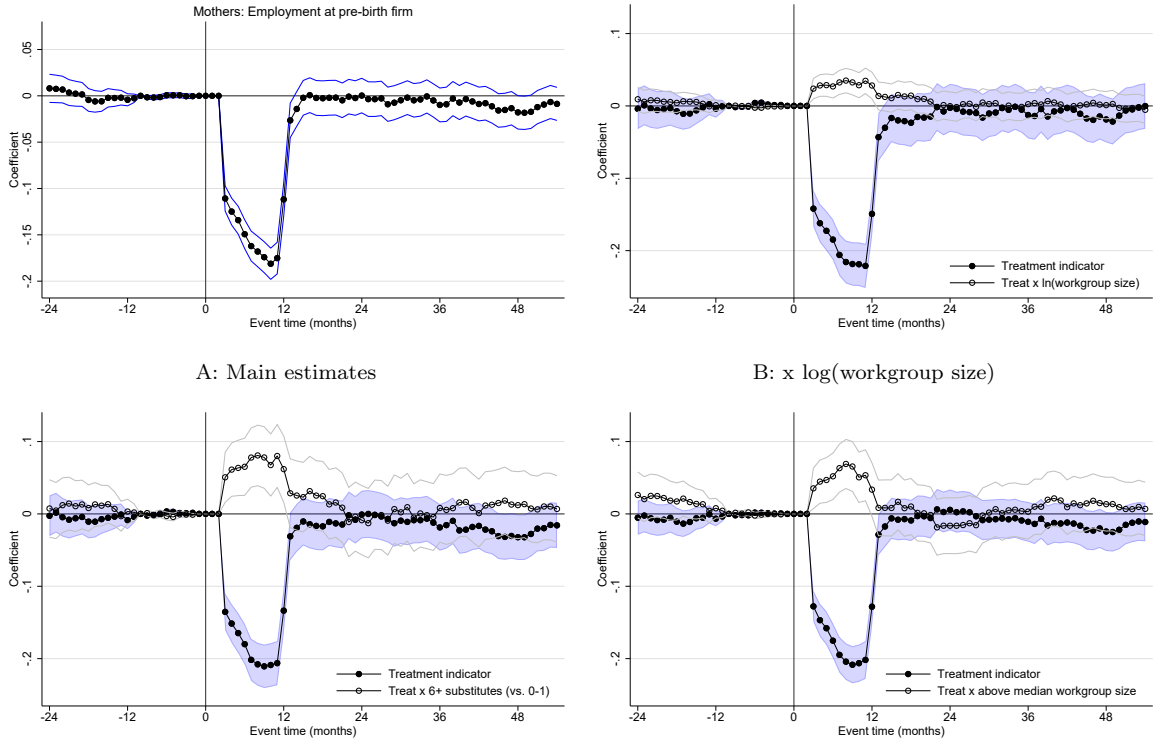


A: Pre-reform

B: Post-reform

Notes: Panels A and B show Kaplan-Meier failure functions of mother's return to their pre-birth firm for different parental leave regimes. Internal substitutes are defined as co-workers in the same occupation in the same firm and workplace. Panel A uses births from January 2006 to June 2006, Panel B uses births from January 2007 to June 2007. Source: IEB, own calculations.

Figure A.8: Event study of parental leave reform effects on mothers', different workgroup size definitions



A: Main estimates

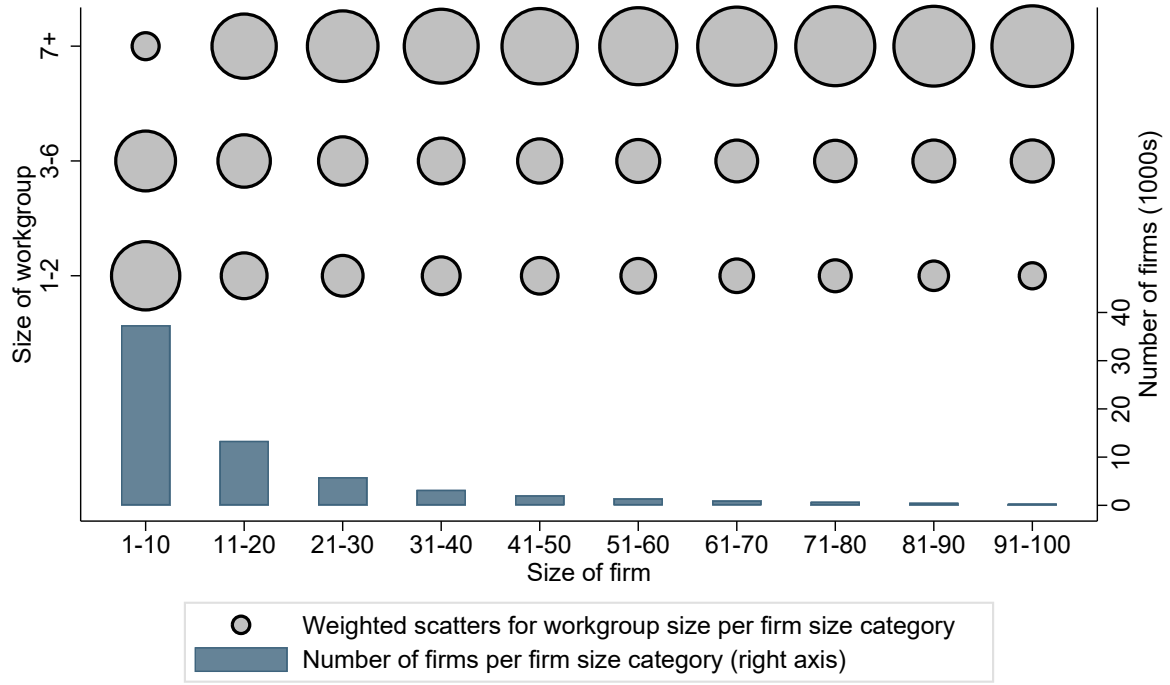
B: x log(workgroup size)

C: x 3rd vs 1st tercile

D: x above median

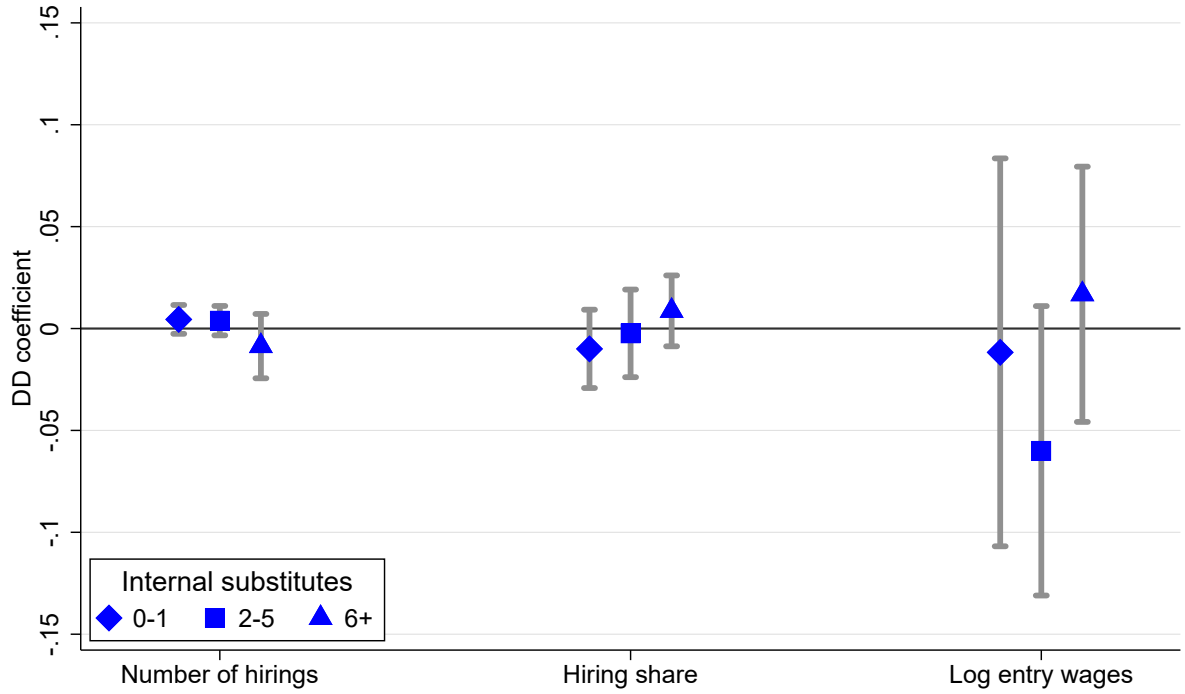
Notes: The figure plots event study estimates of the 2007 paid parental leave reform in Germany on maternal labour supply and firm outcomes based on eq. (2), additionally including and showing interaction terms for different definitions of the workgroup size. The estimates shown in Panel A correspond to Panel A in Figure 5. Lines indicate 95% confidence interval, standard errors clustered at the mother level. *Source:* IEB, own calculations.

Figure A.9: Combination of firm and workgroup size



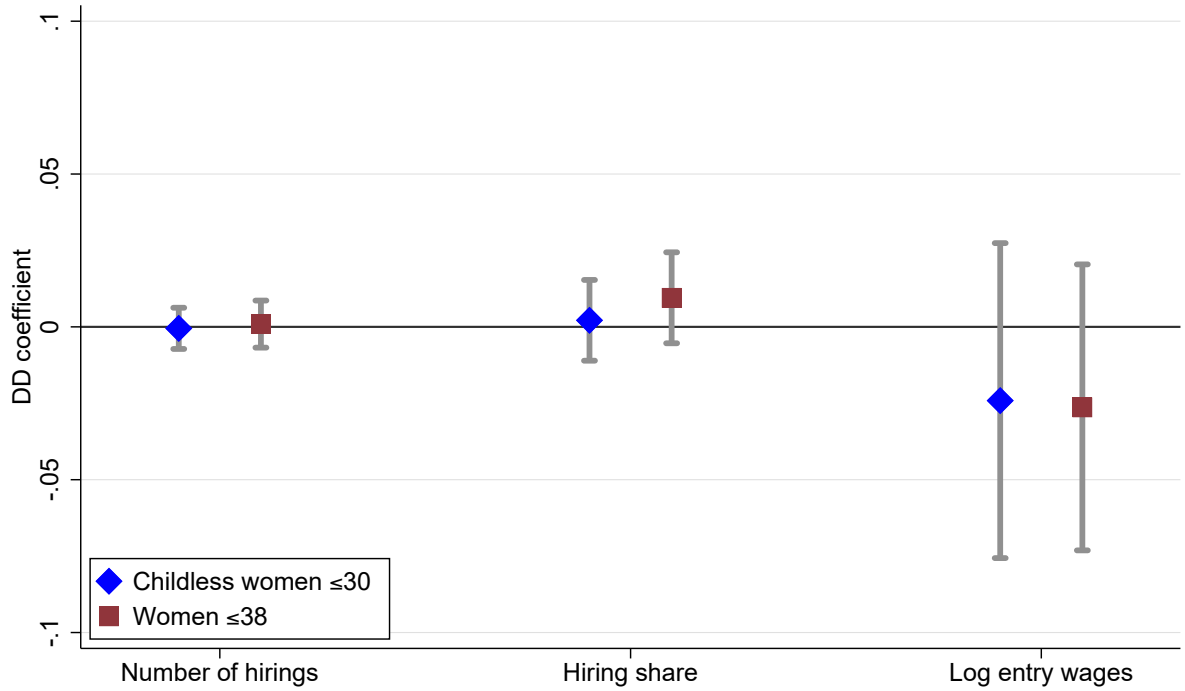
Notes: Horizontal blue bars show the number of firms per size category. The size of the grey circles indicate the distribution of workgroup sizes within those firm size groups. *Source:* IEB, own calculations.

Figure A.10: Longer-term effects of hiring childless women under 30 by internal substitutes



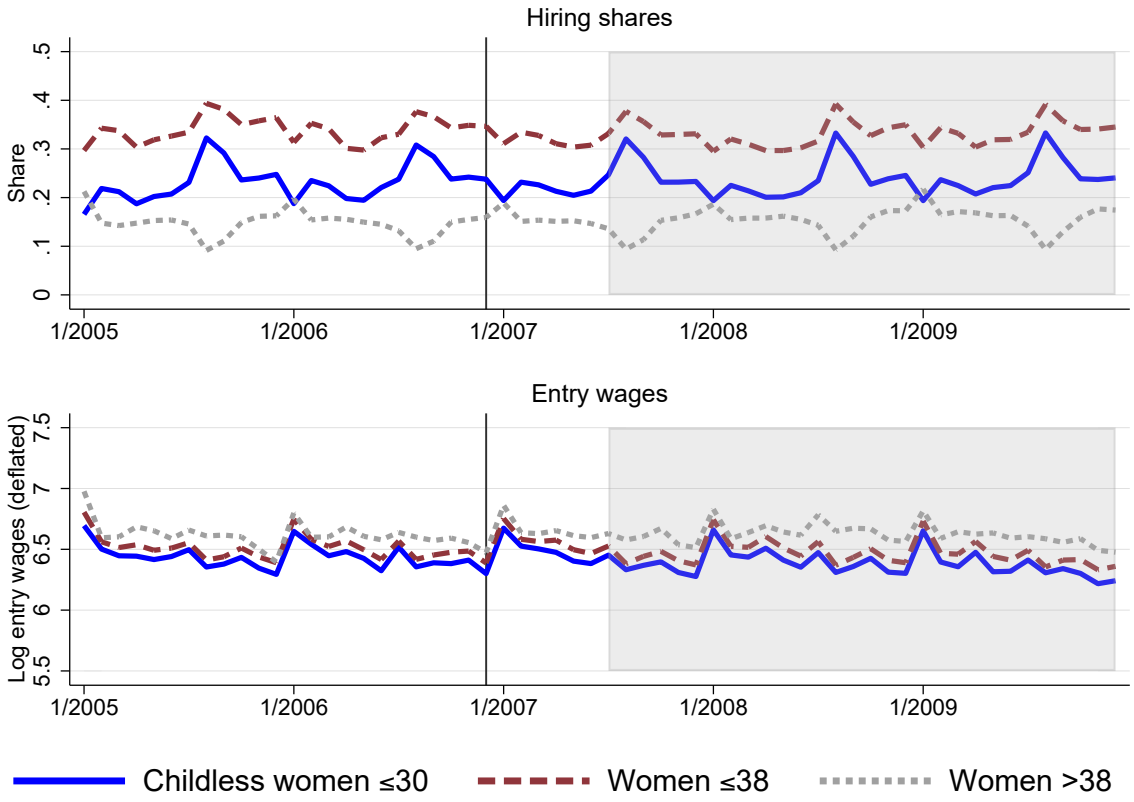
Notes: Figure plots difference-in-differences coefficients for the number of hirings, the hiring share and log entry wages of young women at the firm-level. The sample period is July 2007 to December 2009. Internal substitutes are based on the number of workers in the same occupation of a hired person as of January 1, 2007. 95% confidence intervals based on robust standard errors. *Source:* IEB, own calculations.

Figure A.11: Longer-term effects of hiring of young women—firms without births



Notes: Figure plots difference-in-differences coefficients for the number of hirings, the hiring share and log entry wages of young women at the firm-level. The sample period is July 2007 to December 2009. Firms with a birth in this time period are omitted. $N = 29,658$ firms with $356,465$ hirings. 95% confidence intervals based on robust standard errors. *Source:* IEB, own calculations.

Figure A.12: Hiring patterns of women over time



Notes: The upper panel plots the hiring share of young women in the sample firms over time. The lower panel shows deflated log entry wages. Number of firms is 41,831. *Source*: IEB, own calculations.

Appendix Tables

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

Monthly pre-birth earnings of mothers	All		≥ 1,295 euro (upper two terciles)	
	Analysis sample	Dropped observations	Analysis sample	Dropped observations
	(1)	(2)	(3)	(4)
Panel A: Mother's pre-birth characteristics				
Age at birth	28.48	28.95	29.32	29.76
	(4.95)	(4.89)	(4.47)	(4.37)
Higher education level	0.27	0.34	0.32	0.40
	(0.44)	(0.48)	(0.47)	(0.49)
Monthly wage, 10 months pre-birth (1,000 Euros)	1.81	2.35	2.33	2.90
	(1.04)	(1.41)	(0.85)	(1.13)
At same firm, 10 months pre-birth	1.00	0.85	1.00	0.93
	(0.05)	(0.35)	(0.03)	(0.25)
Tenure at current firm in years	3.87	3.91	4.50	4.65
	(3.58)	(3.91)	(3.75)	(3.98)
Full-time employed	0.84	0.82	0.92	0.89
	(0.37)	(0.38)	(0.28)	(0.31)
German citizen	0.92	0.90	0.95	0.92
	(0.27)	(0.30)	(0.22)	(0.27)
Return to employment within 1 year	0.45	0.42	0.47	0.43
	(0.50)	(0.49)	(0.50)	(0.50)
Return to employment within 3 years	0.79	0.78	0.80	0.80
	(0.41)	(0.41)	(0.40)	(0.40)
Return to pre-birth firm within 1 year	0.36	0.34	0.38	0.35
	(0.48)	(0.47)	(0.49)	(0.48)
Return to pre-birth firm within 3 years	0.57	0.58	0.59	0.62
	(0.50)	(0.49)	(0.49)	(0.48)
Mothers	62,959	159,485	42,053	121,865
Panel B: Firm's pre-birth characteristics				
Firm size	16.45	133.05	18.55	158.41
	(19.02)	(502.15)	(19.94)	(586.06)
Share of female employees	0.65	0.59	0.62	0.56
	(0.30)	(0.28)	(0.30)	(0.27)
Average age of full-time employees	37.53	37.55	38.24	38.12
	(6.71)	(5.59)	(6.17)	(4.99)
Median monthly wage of full-time employees (1,000 euros)	2,142.31	2,586.54	2,429.47	2,897.90
	(1108.91)	(1279.02)	(1107.91)	(1272.40)
Location in West Germany	0.82	0.85	0.86	0.88
	(0.38)	(0.36)	(0.35)	(0.33)
Firms	61,026	72,255	40,622	49,404

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; private sector firms, firm size up to 100 employees, mothers' were employed in the same firm from ten months before birth up the start of motherhood protection, and firms have births in only one semester for an unambiguous treatment assignment. Columns (3) and (4) additionally restrict the sample to mothers with pre-birth earnings of at least 1295 euro per month. 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.

Table A.2: Summary statistics and balancing—upper two pre-birth earnings terciles

		Sample with births between ...				
		Control Cohort		PPL Reform Cohort		
	All	Jul-Dec 05	Jan-Jun 06	Jul-Dec 06 (before PPL reform)	Jan-Jun 07 (after PPL reform)	DD coef.
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-birth characteristics: mother						
Age in years	29.316	29.127	29.492	29.175	29.502	-0.038
						(0.087)
Monthly earnings before birth	2333.368	2354.819	2326.969	2316.345	2334.216	45.721***
						(16.669)
Tenure at current firm in years	5.217	5.163	5.166	5.268	5.273	0.003
						(0.073)
Higher education level	0.321	0.311	0.319	0.318	0.337	0.011
						(0.009)
German citizenship	0.949	0.951	0.946	0.951	0.946	0.000
						(0.004)
Full-time employed	0.917	0.919	0.917	0.917	0.917	0.002
						(0.005)
Mothers	42,053	11,170	10,000	10,817	10,066	42,053
Pre-birth characteristics: firm						
Firm size	17.637	17.818	17.653	17.576	17.487	0.075
						(0.371)
Workgroup size	6.613	6.704	6.662	6.494	6.593	0.142
						(0.175)
Share of women in firm	0.639	0.641	0.640	0.639	0.638	0.000
						(0.006)
Location in West Germany	0.860	0.858	0.860	0.857	0.865	0.005
						(0.007)
Firms	40,622	10,756	9,685	10,453	9,728	40,622
Joint F-test that all coefficients in column (6) equal 0:						$p = 0.6119$
Joint F-test additionally including sectors:						$p = 0.3559$

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), where columns (2)-(5) report the means for mothers and firms with births in the calendar period before and after the paid parental leave (PPL) reform. The coefficients in column (6) are obtained from a difference-in-differences specification outlined in eq. (4). The p-value stems from a joint estimation using the routine of Oberfichtner and Tauchmann (2021). The first p-value is based only on the variables shown in the table, the second p-value additionally includes 1-digit sectors. Robust standard errors in parentheses, * < 10% ** < 5% *** < 1%. Source: IEB, own calculations.

Table A.3: Summary of event study estimates (including lower earning mothers)

Outcome:	Mothers		Firms		
	Employment at pre-birth firm	Earnings at pre-birth firm	Relative employment	Relative wage bill	Firm shutdown
	(1)	(2)	(3)	(4)	(5)
Pre-period	0.005	8.138	-0.005	-0.006	.
	(0.005)	(9.260)	(0.005)	(0.005)	(.)
Short term effect	-0.115***	-150.669***	-0.029***	-0.030***	0.002
	(0.006)	(12.929)	(0.005)	(0.005)	(0.003)
Medium term effect	0.007	5.805	-0.011	-0.013*	0.004
	(0.007)	(13.218)	(0.007)	(0.007)	(0.005)
Longer term effect	-0.001	-13.092	-0.001	-0.006	-0.004
	(0.007)	(13.980)	(0.009)	(0.009)	(0.006)
Mothers / firms	62,959	62,959	60,884	60,884	60,884
Observations	6,421,818	6,421,818	6,210,168	6,210,168	6,210,168

Notes: Table summarises event study estimates in discrete time periods based on eq. (3). In contrast to Table 2, this table also includes lower earning mothers and their firms. 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 3-12, 13-36 and 37-58 months post-birth, respectively. Standard errors clustered at the mother / firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

Table A.4: Event study estimates by internal substitutes

	Number of 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	-0.001	0.010	0.008	2.572	23.074	3.800
	(0.009)	(0.008)	(0.009)	(23.095)	(20.791)	(23.082)
Short term effect	-0.175***	-0.147***	-0.106***	-229.956***	-174.602***	-168.100***
	(0.013)	(0.012)	(0.013)	(28.065)	(25.627)	(30.282)
Medium term effect	-0.006	-0.003	0.001	27.784	18.610	3.815
	(0.014)	(0.014)	(0.015)	(30.715)	(28.143)	(32.483)
Longer term effect	-0.016	0.008	-0.004	5.738	10.609	-3.370
	(0.015)	(0.014)	(0.016)	(32.217)	(29.712)	(35.051)
Mothers	13,567	13,793	11,738	13,567	13,793	11,738
Observations	1,383,834	1,406,886	1,197,276	1,383,834	1,406,886	1,197,276
Panel B: Firms						
	Employment relative to baseline			Wage bill relative to baseline		
	0-1	2-5	6+	0-1	2-5	6+
Pre-period	-0.024**	0.000	0.002	-0.018	-0.001	0.002
	(0.012)	(0.009)	(0.008)	(0.012)	(0.009)	(0.008)
Short term effect	-0.056***	-0.031***	0.003	-0.047***	-0.023***	-0.002
	(0.012)	(0.008)	(0.008)	(0.011)	(0.008)	(0.008)
Medium term effect	-0.034**	-0.019	0.008	-0.026	-0.018	0.008
	(0.017)	(0.013)	(0.013)	(0.016)	(0.013)	(0.013)
Longer term effect	-0.035	0.013	0.023	-0.032	0.013	0.021
	(0.022)	(0.018)	(0.018)	(0.021)	(0.018)	(0.018)
Firms	13,567	13,793	11,738	13,567	13,793	11,738
Observations	1,383,834	1,406,886	1,197,276	1,383,834	1,406,886	1,197,276
	Firm shutdown					
Pre-period	.	.	.			
	(.)	(.)	(.)			
Short term effect	0.008	0.008	-0.003			
	(0.006)	(0.005)	(0.005)			
Medium term effect	0.014	0.015*	-0.010			
	(0.010)	(0.008)	(0.009)			
Longer term effect	0.016	0.000	-0.021*			
	(0.013)	(0.011)	(0.012)			
Firms	13,567	13,793	11,738			
Observations	1,383,834	1,406,886	1,197,276			

Notes: Table summarises event study estimates as in Table 2 by internal substitutes of the mother. 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.

Table A.5: Event study estimates by external substitutes

	Tercile of external substitutes					
	1st	2nd	3rd	1st	2nd	3rd
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mothers						
	Employed at pre-birth firm			Earnings at pre-birth firm		
Pre-period	0.007	-0.002	0.012	2.160	0.295	25.572
	(0.008)	(0.009)	(0.009)	(19.973)	(23.282)	(22.453)
Short term effect	-0.148***	-0.143***	-0.144***	-176.895***	-191.293***	-214.069***
	(0.012)	(0.013)	(0.012)	(24.937)	(29.303)	(27.865)
Medium term effect	0.006	-0.019	0.006	41.734	-5.641	17.602
	(0.014)	(0.014)	(0.014)	(28.361)	(31.404)	(29.443)
Longer term effect	0.002	-0.021	0.003	25.474	-16.349	-7.793
	(0.015)	(0.015)	(0.014)	(29.966)	(33.539)	(31.129)
Mothers	13,987	13,329	14,165	13,987	13,329	14,165
Observations	1,426,674	1,359,558	1,444,830	1,426,674	1,359,558	1,444,830
Panel B: Firms						
	Employment relative to baseline			Wage bill relative to baseline		
	0-1	2-5	6+	0-1	2-5	6+
Pre-period	-0.009	-0.015	0.003	-0.003	-0.011	-0.002
	(0.009)	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)
Short term effect	-0.024***	-0.033***	-0.030***	-0.015*	-0.034***	-0.025***
	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
Medium term effect	-0.019	-0.019	-0.016	-0.005	-0.023	-0.017
	(0.014)	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)
Longer term effect	0.002	-0.007	-0.000	0.014	-0.006	-0.015
	(0.018)	(0.019)	(0.019)	(0.018)	(0.019)	(0.019)
Firms	13,585	12,929	13,706	13,585	12,929	13,706
Observations	1,385,670	1,318,758	1,398,012	1,385,670	1,318,758	1,398,012
	Firm shutdown					
Pre-period	.	.	.			
	(.)	(.)	(.)			
Short term effect	0.004	0.008	0.002			
	(0.005)	(0.006)	(0.006)			
Medium term effect	0.008	0.019**	-0.002			
	(0.009)	(0.009)	(0.009)			
Longer term effect	-0.003	0.017	-0.011			
	(0.012)	(0.012)	(0.012)			
Firms	13,585	12,929	13,706			
Observations	1,385,670	1,318,758	1,398,012			

Notes: Table summarises event study estimates as in Table 2 by external substitutes of the mother. 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.6: 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	0.005	-0.004	19.150	49.363
	(0.006)	(0.016)	(15.681)	(50.897)
Short term effect	-0.134***	0.059***	-170.723***	111.871**
	(0.008)	(0.020)	(17.964)	(45.848)
Medium term effect	-0.001	0.010	25.357	43.379
	(0.009)	(0.022)	(19.434)	(48.726)
Longer term effect	-0.004	0.003	5.168	3.643
	(0.010)	(0.022)	(20.478)	(49.091)
Mothers	39,098		39,098	
Observations	3,987,996		3,987,996	

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. See Table 2 for other notes. Significance levels: * < 10% ** < 5% *** < 1%.
Source: IEB, own calculations.

Table A.7: Summary estimates by pre-birth firm size

	Firms' number of employees 10 months prior to childbirth					
	1-30	31-50	51-100	1-30	31-50	51-100
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mothers						
	Employed at pre-birth firm			Earnings at pre-birth firm		
Pre-period	0.005	-0.001	0.016	10.247	-39.808	51.269
	(0.005)	(0.015)	(0.016)	(13.756)	(41.149)	(45.662)
Short term effect	-0.150***	-0.133***	-0.107***	-188.958***	-219.267***	-244.356***
	(0.008)	(0.022)	(0.024)	(16.916)	(53.845)	(62.815)
Medium term effect	-0.002	0.006	-0.007	26.755	-5.312	-74.425
	(0.009)	(0.026)	(0.030)	(18.342)	(59.124)	(70.771)
Longer term effect	-0.006	0.021	-0.027	6.937	18.016	-106.473
	(0.009)	(0.028)	(0.031)	(19.371)	(63.226)	(76.260)
Mothers	34,477	4,168	3,408	34,477	4,168	3,408
Observations	3,516,654	425,136	347,616	3,516,654	425,136	347,616
Panel B: Firms						
	Employment relative to baseline			Wage bill relative to baseline		
Pre-period	-0.007	-0.007	0.000	-0.005	-0.011	0.009
	(0.006)	(0.014)	(0.015)	(0.006)	(0.014)	(0.015)
Short term effect	-0.031***	-0.022	-0.015	-0.026***	-0.023*	-0.015
	(0.006)	(0.014)	(0.015)	(0.006)	(0.013)	(0.014)
Medium term effect	-0.014	-0.067***	-0.012	-0.010	-0.065***	-0.005
	(0.009)	(0.023)	(0.024)	(0.009)	(0.023)	(0.024)
Longer term effect	-0.003	-0.046	0.053*	-0.003	-0.049	0.054*
	(0.012)	(0.030)	(0.031)	(0.012)	(0.030)	(0.031)
Firms	33,719	3,923	3,134	33,719	3,923	3,134
Observations	3,439,338	400,146	319,668	3,439,338	400,146	319,668
	Firm shutdown					
Pre-period	.	.	.			
	(.)	(.)	(.)			
Short term effect	0.005	0.001	0.003			
	(0.004)	(0.009)	(0.009)			
Medium term effect	0.007	0.013	0.018			
	(0.006)	(0.015)	(0.017)			
Longer term effect	0.004	0.000	-0.026			
	(0.008)	(0.021)	(0.022)			
Firms	33,719	3,923	3,134			
Observations	3,439,338	400,146	319,668			

Notes: Table summarises event study estimates as in Table 2 of the manuscript by firm size ten months pre-birth. Standard errors clustered at the mother- / firm-level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

Table A.8: Summary event study estimates for larger firms

		Internal substitutes				Internal substitutes		
	All	0-1	2-5	6+	All	0-1	2-5	6+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Mothers								
	Employed at pre-birth firm				Earnings at pre-birth firm			
Pre-period	0.004	0.005	-0.008	0.011	-8.315	1.151	-37.808	5.445
	(0.007)	(0.017)	(0.013)	(0.010)	(18.058)	(44.477)	(34.339)	(25.408)
Short term effect	-0.118***	-0.156***	-0.131***	-0.097***	-185.887***	-244.601***	-170.889***	-168.507***
	(0.010)	(0.025)	(0.020)	(0.015)	(24.949)	(60.924)	(46.649)	(35.576)
Medium term effect	-0.006	0.002	-0.026	-0.001	2.938	60.293	-8.903	-14.052
	(0.012)	(0.028)	(0.023)	(0.018)	(27.984)	(68.899)	(52.957)	(39.418)
Longer term effect	-0.013	-0.020	-0.026	-0.003	-14.181	51.300	-39.357	-14.714
	(0.013)	(0.030)	(0.025)	(0.019)	(29.559)	(72.540)	(55.060)	(42.116)
Mothers	18,572	3,554	4,909	8,619	18,572	3,554	4,909	8,619
Observations	1,894,344	362,508	500,718	879,138	1,894,344	362,508	500,718	879,138
Panel B: Firms								
	Employment relative to baseline				Wage bill relative to baseline			
Pre-period	-0.001	-0.019	0.006	0.000	0.001	-0.020	0.009	0.002
	(0.006)	(0.015)	(0.012)	(0.009)	(0.006)	(0.015)	(0.012)	(0.009)
Short term effect	-0.015**	-0.041***	-0.021*	-0.002	-0.018***	-0.036**	-0.025**	-0.007
	(0.006)	(0.014)	(0.012)	(0.008)	(0.006)	(0.014)	(0.011)	(0.008)
Medium term effect	-0.015	-0.016	-0.038*	-0.001	-0.011	-0.000	-0.036*	0.002
	(0.011)	(0.025)	(0.021)	(0.015)	(0.011)	(0.025)	(0.021)	(0.015)
Longer term effect	0.013	0.008	0.005	0.021	0.012	0.016	-0.000	0.021
	(0.014)	(0.034)	(0.028)	(0.020)	(0.014)	(0.034)	(0.028)	(0.020)
Firms	17,926	3,555	4,909	8,622	17,926	3,555	4,909	8,622
Observations	1,828,452	362,610	500,718	879,444	1,828,452	362,610	500,718	879,444
	Firm shutdown							
Pre-period	.	.	.	.				
	(.)	(.)	(.)	(.)				
Short term effect	0.004	0.015**	0.001	0.002				
	(0.003)	(0.007)	(0.006)	(0.005)				
Medium term effect	0.006	0.014	0.017	-0.008				
	(0.007)	(0.015)	(0.012)	(0.009)				
Longer term effect	-0.006	-0.003	0.001	-0.016				
	(0.009)	(0.021)	(0.018)	(0.013)				
Firms	17,926	3,555	4,909	8,622				
Observations	1,828,452	362,610	500,718	879,444				

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 (above median firm size, i.e. at least 11 employees). See Table 2 for other notes. Standard errors clustered at the mother / firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.
Source: IEB, own calculations.

Table A.9: Summary event study estimates for firms - interaction with internal substitutes

	Relative employment		Relative wage sum		Firm shut-down	
	Treat	Treat × ln(workgroup size)	Treat	Treat × ln(workgroup size)	Treat	Treat × ln(workgroup size)
Pre-period	-0.003	0.028	0.001	0.039**	.	.
	(0.006)	(0.019)	(0.006)	(0.018)	(.)	(.)
Short term effect	-0.035***	-0.026	-0.028***	-0.014	0.005	0.003
	(0.007)	(0.025)	(0.006)	(0.024)	(0.003)	(0.011)
Medium term effect	-0.031***	-0.076**	-0.023**	-0.053*	0.010*	0.015
	(0.010)	(0.030)	(0.009)	(0.028)	(0.006)	(0.015)
Longer term effect	-0.016	-0.080**	-0.011	-0.056*	0.003	0.017
	(0.012)	(0.035)	(0.012)	(0.033)	(0.007)	(0.018)
Firms	39,098		39,098		39,098	
Observations	3,987,996		3,987,996		3,987,996	

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. See Table 2 for other notes. Significance levels: * < 10% ** < 5% *** < 1%.
Source: IEB, own calculations.

Table A.10: 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	-0.049	-0.197	0.038	-0.051	-0.239	-0.061	0.138
	(0.104)	(0.129)	(0.143)	(0.264)	(0.169)	(0.196)	(0.178)
Short term effect	-0.311***	-0.420***	-0.406***	-0.065	-0.213	-0.359**	-0.372**
	(0.099)	(0.124)	(0.136)	(0.255)	(0.167)	(0.182)	(0.171)
Medium term effect	-0.328*	-0.392*	-0.428*	-0.003	-0.369	-0.144	-0.477
	(0.178)	(0.216)	(0.251)	(0.459)	(0.290)	(0.336)	(0.307)
Longer term effect	0.248	-0.152	0.106	0.947	0.444	0.457	-0.200
	(0.236)	(0.287)	(0.333)	(0.605)	(0.389)	(0.440)	(0.409)
Firms	34,047	11,714	11,606	9,631	11,228	10,921	11,428
Observations	3,472,794	1,194,828	1,183,812	982,362	1,145,256	1,113,942	1,165,656
Panel B: Wage bill							
Pre-period	-128.768	-509.663	-147.336	56.326	-483.148	146.634	-54.913
	(249.961)	(312.469)	(333.827)	(647.158)	(392.820)	(480.792)	(436.722)
Short term effect	-639.078***	-842.871***	-816.153**	-154.002	-420.041	-643.960	-841.765*
	(246.248)	(287.838)	(336.978)	(652.143)	(411.645)	(442.740)	(438.169)
Medium term effect	-594.120	-634.535	-1192.096*	572.541	-589.608	233.786	-1398.367*
	(444.982)	(541.296)	(620.957)	(1160.813)	(721.690)	(825.202)	(787.471)
Longer term effect	665.577	0.316	-421.823	2920.966*	1057.908	1781.311	-808.925
	(589.720)	(721.107)	(828.841)	(1520.227)	(969.095)	(1088.383)	(1036.061)
Firms	34,047	11,714	11,606	9,631	11,228	10,921	11,428
Observations	3,472,794	1,194,828	1,183,812	982,362	1,145,256	1,113,942	1,165,656

Notes: Table summarises event study estimates for the main outcomes of firms in absolute values (rather than relative outcomes as in Table 2. Standard errors clustered at the firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%. *Source:* IEB, own calculations.

Table A.11: Summary event study estimates for West and East Germany

	Mothers				Firms					
	Employed at pre-birth firm		Earnings at pre-birth firm		Relative employment		Relative wage bill		Firm shut-down	
Location:	West	East	West	East	West	East	West	East	West	East
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-period	0.006	0.002	9.536	-2.050	-0.005	-0.013	-0.005	-0.001	.	.
	(0.005)	(0.013)	(13.801)	(29.097)	(0.006)	(0.015)	(0.006)	(0.015)	(.)	(.)
Short term effect	-0.144***	-0.154***	-194.927***	-194.260***	-0.023***	-0.070***	-0.018***	-0.067***	0.003	0.011
	(0.008)	(0.018)	(17.261)	(35.262)	(0.006)	(0.015)	(0.006)	(0.014)	(0.003)	(0.009)
Medium term effect	0.003	-0.024	24.359	-6.979	-0.014	-0.047**	-0.010	-0.046**	0.005	0.027*
	(0.009)	(0.022)	(17.553)	(45.385)	(0.009)	(0.023)	(0.009)	(0.022)	(0.006)	(0.014)
Longer term effect	-0.001	-0.026	9.301	-40.638	0.004	-0.048	0.004	-0.045	-0.002	0.019
	(0.009)	(0.024)	(18.832)	(50.400)	(0.012)	(0.030)	(0.011)	(0.029)	(0.007)	(0.019)
Mothers / firms	36,180	5,873	36,180	5,873	35,061	5,721	35,061	5,721	35,061	5,721
Observations	3,690,360	599,046	3,690,360	599,046	3,576,016	583,136	3,576,016	583,136	3,576,016	583,136

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. Standard errors clustered at the mother / firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.
Source: IEB, own calculations.