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PARENTAL LEAVE, WORKER SUBSTITUTABILITY AND FIRMS' EMPLOYMENT*

Mathias Huebener, Jonas Jessen, Daniel Kuehnle and Michael Oberfichtner

Motherhood and parental leave are frequent causes of worker absences and employment interruptions, yet little is known 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 up to three years 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 bills or likelihood to shut down. The reform led to an increase in replacement hirings, but firms did not respond to longer expected absences of mothers by subsequently hiring fewer young women. Overall, our findings show that anticipated, extended parental leave does not have a lasting impact on firms.

JEL codes: J16, J18, J24

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, posing challenges particularly for firms with few workers who can fill in for the absent mother. Although this may make longer leave 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

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The data and codes for this paper are available on the Journal repository. They were checked for their ability to reproduce the results presented in the paper. The authors were granted an exemption to publish parts of their data because access to these data is restricted. However, the authors provided the Journal with temporary access to the data, which enabled the Journal to run their codes. The codes for the parts subject to exemption are also available on the Journal repository. The restricted access data and these codes were also checked for their ability to reproduce the results presented in the paper. The replication package for this paper is available at the following address: <https://doi.org/10.5281/zenodo.14277729>.

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based on exceptionally detailed high-frequency data, and (ii) how firms use internal substitutes (i.e., workers in the same firm, location and occupation) and external substitutes (i.e., workers in the same occupation and local labour market) to cope with parental leave absences. We 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. In line with the public debate on adverse effects of longer parental leave on small- and medium-sized firms, our analysis focuses on firms with up to a hundred employees. In our period of analysis, these firms constitute 98.4% of all enterprises and employ more than every second worker (57.3%) 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 (Rossin-Slater, 2018). Only two studies exploit exogenous policy changes to examine the effects of extended paid parental leave schemes on firms. Gallen (2019) studied the effects of a parental leave extension from eight to ten months in Denmark in 2002, finding negative effects on firm survival, which are concentrated on firms with less than thirty workers. Ginja *et al.* (2023) studied the effects of a parental leave extension from twelve to fifteen months in Sweden in 1989 and found that the reform increased mothers' parental leave and that firms with greater exposure incurred larger additional wage costs, which appears to be driven by firms with less than fifty employees. However, both evaluated reforms were applied retroactively. Thus, mothers had already begun their parental leave when it was extended, forcing firms to adjust unexpectedly 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. Seen through the lens of List (2020), these settings may not be the most 'natural' ones to learn about the costs of anticipated absences. Complementing these two studies, Brenøe *et al.* (2024) examined the joint effect of pregnancy and subsequent parental leave on firms in a non-reform setting. They combined matching with an event-study approach, finding negligible costs for firms unless they have few internal substitutes for the mother. We contribute new evidence from a quasi-experimental setting in which firms can anticipate and account for extended absences when deciding on replacements before mothers take leave. Our setting is thus more natural than those previously examined quasi-experimentally, while still yielding exogenous variation in the length of mothers' leave.¹

We begin with a descriptive analysis of firms' hirings and separations around childbirth. We document a pronounced hiring peak in the six months prior to childbirth, corresponding to 0.359 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 (Muehleemann and Pfeifer, 2016). We further show that replacement hiring is most pronounced when few internal substitutes are available for the mother on leave, but barely differs by the availability of external substitutes. Our results clearly document that firms adjust to expected worker absences mainly before childbirth, when mothers are still at the firm.

¹ Furthermore, Schmutte and Skira (2023) provided 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) studied the effects of extended parental leave of nurses in Denmark. Relatedly, Carta *et al.* (2024) examined the impact on firms if mothers permanently quit employment after giving birth, with exogenous variation derived from an unemployment benefit reform in Italy. It is important to note that parental leave policies typically intend to extend temporary absences, and that firm responses to permanent exits likely differ.

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 1 January 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 the causal effects of a substantial parental leave extension as it 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 empirical estimation strategy employs a difference-in-differences event-study design, which compares outcomes (i) between births occurring from January to June and July to December, (ii) between births occurring from July 2005 to June 2006 and July 2006 to June 2007, and (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 longer term. We find small negative effects on firms' employment and wage bills mainly during the extended parental leave period. 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 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 remaining 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.

In the final section, we examine whether the reform affected firms' later 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 one might expect firms that had already experienced prolonged absences to 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 their longer-term hiring and outcomes. Short-term effects mainly arise in firms with few internal substitutes for the mother on leave, while the zero long-term effects are independent of the availability of internal substitutes and are applicable to both smaller and larger firms. As a result, our findings draw a more optimistic picture of the costs of parental leave absences for firms compared to the setting analysed by Gallen (2019) and Ginja *et al.* (2023). As mentioned above, Ginja *et al.* (2023) found sizeable adjustment costs, mainly driven by smaller firms. Gallen (2019) found a lowered survival probability of firms with fewer than thirty employees. One reason for the different results could be that the previously analysed reforms increased women's probability of changing jobs after childbirth, thereby creating a lasting employment gap for firms. In our study, we only find a short-term increase in absences, but no longer-term effects on women's probability to return to their firms. Another significant reason for the different results may lie in the retroactive implementation of

the reforms. We show that firms typically make initial adjustments before mothers go on leave. The ability to anticipate longer absences in our arguably more natural setting allows firms to incorporate this knowledge into their initial replacement decisions, thereby helping to reduce adjustment costs for firms.

More generally, our paper 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) showed that firms keep sickness absences low for positions where workers are harder to replace, and Jäger and Heining (2022) documented 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 therefore 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 motherhood 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) showed that longer parental leave reduced employer-provided training for young women in Germany, and Thomas (2020) documented that mandated maternity leave benefits reduced women's promotions in the United States.³ Moreover, women's compliance with or deviation from the policy norm could signal their preferences for family and work to their employers (Tô, 2018), potentially resulting in consequences for their careers. 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 the case of childbirth.

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).

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-hour restrictions and maternity benefits (Zveglic and Rodgers, 2003), as well as mandated employer-provided childcare (Prada *et al.*, 2015).

1. Institutional Background

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

1.1. Paid Maternity Leave

All expecting mothers are entitled to paid maternity leave that lasts from six weeks before expected delivery to eight weeks after childbirth. Mothers receive a full replacement of net earnings during this period. They are legally not allowed to work in the eight weeks 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).

1.2. 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 thirty-six 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.

1.3. 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.

Prior to 2007, parents with low household income were eligible to receive benefits for up to twenty-four months after childbirth. Families qualified for benefits of 300 euros per month (about 370 USD in 2006, around 11% of average pre-birth net household income) if their annual net income was below a certain threshold, which varied with household structure, the number of children and time since giving birth. About 77% of parents were eligible to receive benefits for up to six months after childbirth (for details, see Huebener *et al.*, 2019). Because of gradually lowered income thresholds for eligibility, the share of eligible parents fell to 47% for seven to twelve months after childbirth and to 40% for twelve to twenty-four 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 1 January 2007. The reform replaced the previous means-tested benefits with an earnings-based benefit scheme that was paid for up to twelve months to either parent. The new benefit replaced 67% of the average net labour income earned in the twelve months prior to childbirth.⁵ The benefit had a floor of 300 euros and was capped at 1,800 euros per month. Labour income earned during the benefit period reduced the

⁴ Part-time work of up to thirty hours per week was permitted during the benefit payment period. Parents eligible for benefits for up to twenty-four months could also choose higher benefits (450 euros) for up to twelve months. For children born in 2005 and 2006, only 10% 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 fourteen months of paid parental leave could be split flexibly between both parents. Approximately 96% of parents assigned the main benefit period (> seven months) to the mother. In our observation period, 15% of fathers took paid parental leave, mostly for two months (Destatis, 2008).

benefits by 67 cents per euro of labour income, making part-time work particularly unattractive financially due to this high benefit withdrawal rate. Take-up among mothers was almost 100% (Destatis, 2008). [Online 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.

2. Data

2.1. 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 V13.01.01-190111; IAB, 2017). Data are available from 1975 onward and cover about 82% of all workers in Germany.⁶ As the information on earnings and job duration are used to calculate social security payments and benefits, they are highly reliable. Jacobebbinghaus and Seth (2007) provided 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. Furthermore, 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 three-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 for details).

In addition to the above features, the data include basic socio-demographic characteristics such as workers' gender, citizenship, education (imputed as described in Thomsen *et al.*, 2018) and dates 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 follow Müller and Strauch (2017) to identify mothers in the data and infer their expected date of delivery by exploiting the legal requirement that employers have to notify health insurance companies about the start date of the leave period.⁷ 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. We use the expected date of delivery to assign mothers to specific paid parental leave regimes.

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 therefore 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.

2.2. *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 three-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 three-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 defined 141 regions in Germany based on commuting flows. We also split labour market thickness as a measure for the availability of external substitutes into terciles.

2.3. *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. Leveraging detailed information about the employment spells, we define binary indicators for mothers working at their pre-birth firm at the monthly level, allowing us to trace out the prolonged absence of mothers in detail. Second, we consider maternal earnings at their pre-birth firm. Firms could offer mothers higher wages to counteract the reform incentive for longer absences. Moreover, earnings would also capture changes in 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 consumer price index (CPI) of 2010 and calculate monthly earnings.⁸

For firms, we focus on their employment levels, their wage bills 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 joint 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

⁸ Earnings are top coded at the social security contribution ceiling, which affects less than 1% of mothers in our analysis sample and less than 2.5% 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.

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 bills, which include changes at the intensive margin, wage changes and bonus payments necessary to increase other workers' labour supply, and overtime pay. As firms are not responsible for providing parental leave payments to mothers, the payments are not reflected in firms' wage bills. 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 follow the US Census Bureau (2023) and define a shutdown as the moment in which firms enter zero employment without any subsequent positive employment, until 2019.

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.

2.4. Sample Selection and Descriptive Statistics

In our setting, the date of birth determines the paid parental leave eligibility. 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 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 (see, e.g., 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 Online Appendix Figure A.2 how many observations are dropped from the original sample with each step. 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%. We focus on firms with up to a hundred employees before the pregnancy occurs in the firm (−10.6%); see Online Appendix Figure A.3 for the distribution of the firm size in our analysis sample. 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% 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 and July 2007 (−7.8%).

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.*, 2024).

This restriction avoids the possibility 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. 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%); see [Online Appendix Figure A.2](#). 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 descriptive statistics for our analysis sample. Mothers in our sample are on average 28.5 years old, with monthly earnings of around 1,800 euros, and firm tenure of around 4.6 years. Of these, 27% have high education (*Abitur*), 92% are German citizens and 84% worked full time before childbirth. On average, firms have fifteen employees, and the average workgroup size is six workers. [Online Appendix Figure A.3](#) plots the distributions of firm and workgroup sizes in our analysis sample. Furthermore, the share of women in the firms is on average 68%, and 82% of firms are based in West Germany. [Online 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 to be sufficiently similar to the excluded observations, and, in particular, mothers exhibit similar return-to-work behaviour.

3. 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 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, as presented in panels (a) and (b) of Figure 1. These panels illustrate the average number of hirings and separations in mothers' firms (black circles) and their workgroups (grey triangles), ranging from twenty-four months before to twenty-four months after childbirth.¹²

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.

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

¹² This analysis includes all mothers who meet our sampling criteria. [Online Appendix Figure A.4](#) presents the same figures separating between the four birth semesters.

Table 1. *Summary Statistics and Balancing.*

	Sample with births between...					DD coefficient (6)
	Control cohort		PPL reform cohort			
	All (1)	Jul-Dec 05 (2)	Jan-Jun 06 (3)	Jul-Dec 06 (before PPL reform) (4)	Jan-Jun 07 (after PPL reform) (5)	
<i>Panel A. Pre-birth characteristics: mother</i>						
Age in years	28.484	28.335	28.678	28.315	28.637	−0.021 (0.079)
Monthly earnings before birth	1,805.310	1,848.661	1,809.727	1,777.020	1,785.348	47.263*** (16.620)
Tenure at current firm in years	4.584	4.575	4.568	4.589	4.601	0.020 (0.057)
High education	0.268	0.260	0.269	0.265	0.279	0.005 (0.007)
German citizenship	0.922	0.928	0.921	0.921	0.916	0.002 (0.004)
Full-time employed	0.838	0.845	0.843	0.832	0.833	0.002 (0.006)
Mothers	62,959	16,283	14,877	16,428	15,371	62,959
<i>Panel B. Pre-birth characteristics: firm</i>						
Firm size	15.408	15.679	15.461	15.329	15.152	0.041 (0.284)
Workgroup size	6.243	6.350	6.257	6.175	6.190	0.109 (0.141)
Share of women in firm	0.675	0.674	0.675	0.676	0.676	−0.001 (0.005)
Location in West Germany	0.820	0.818	0.823	0.819	0.822	−0.001 (0.006)
Firms	61,026	15,746	14,429	15,961	14,890	61,026
Joint <i>F</i> -test that all coefficients in column (6) equal 0:						<i>p</i> = .6345
Joint <i>F</i> -test additionally including sectors:						<i>p</i> = .5256

Notes: The table shows pre-determined characteristics at the individual level of the mother and at her pre-birth firm measured ten 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 (DD) specification outlined in (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 one-digit sectors. Robust SEs are reported in parentheses; *** < 1%.

Source: IEB, own calculations.

Figure 1(a) documents that hiring at the firm and workgroup levels 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. 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.

Figure 1(b) displays the average number of separations at the firm and workgroup levels over the same period. The plot shows that separations remain relatively stable before childbirth, followed by a slight increase after childbirth. This small increase in separations post-birth can partially 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.

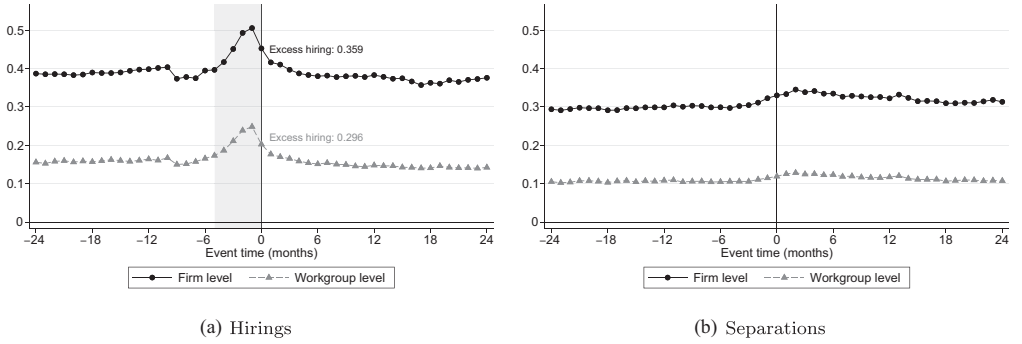


Fig. 1. *Firms' Hiring and Separations around Childbirth.*

Notes: Black circles show monthly hirings (panel a) or separations (panel b) 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 month effects are partialled out. Baseline hirings are the number of hirings from eighteen to twelve 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 (1). Births between July 2005 and December 2006 are in the sample and the number of firms is 38,555 (firms with multiple births in one semester are excluded to guarantee an unambiguous workgroup assignment at the firm level).

Source: IEB, own calculations.

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):

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

On average, we observe 0.359 excess hirings in the firm, i.e., firms replace around a third of mothers through external hirings before childbirth. In the period following women's childbirth, hiring returns to the pre-pregnancy level. Excess hiring in mothers' workgroups amounts to 0.296, which implies that around 80% of excess hirings occur in mothers' workgroups. This finding indicates that mothers on leave are predominantly replaced by workers within the same three-digit occupation, supporting our definition of substitute workers.¹³

In comparison, Jäger and Heining (2022) found 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% of women return to their pre-birth employer within three years after childbirth, on average working 50% of their previous working time. Back-of-the-envelope, according to this calculation, mothers' return can ac-

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.

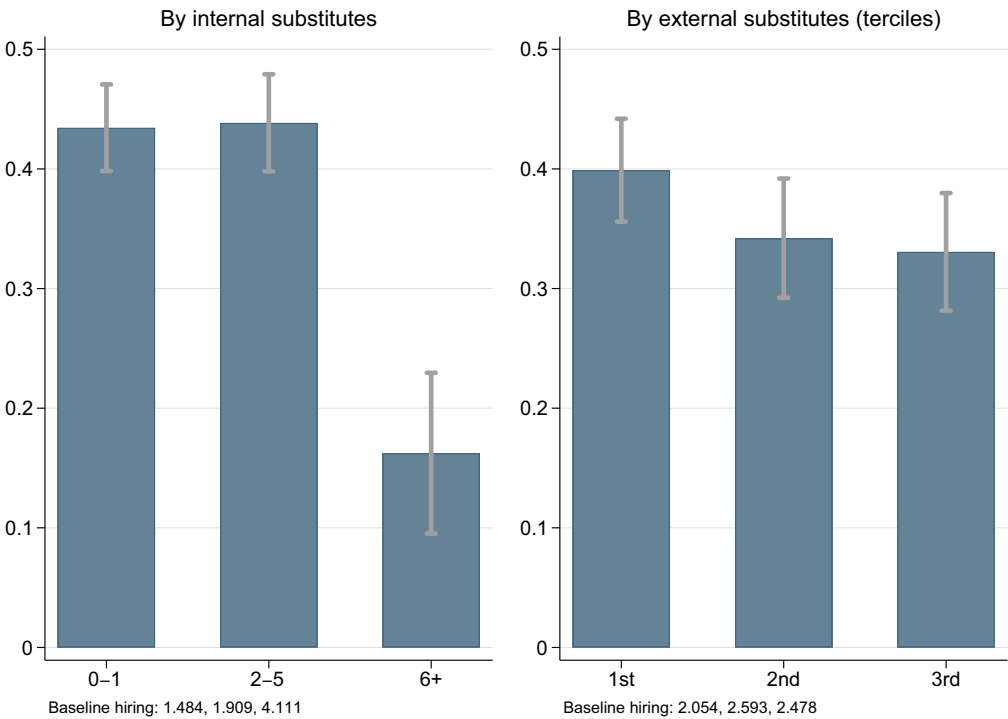


Fig. 2. Firms’ Excess Hiring by Internal and External Substitutes.

Notes: The figure shows firms’ excess hirings as defined in (1) by availability of internal and external substitutes for the mother going on leave. Internal substitutes are defined as the number of co-workers in the same 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 local labour market region. Births between July 2005 and December 2006 are in the sample and the number of firms is 38,555. See Figure 1 for other notes.

Source: IEB, own calculations.

count for the 0.3 worker difference in external hiring compared to Jäger and Heining (2022).

Next, we investigate how firms’ replacement hiring differs based on the availability of internal and external substitutes. The left panel of Figure 2 reveals that excess hiring before childbirth is more pronounced in workgroups with fewer internal substitutes. Specifically, for mothers with up to five substitutes, firms’ excess hiring averages around 0.44, while in groups with six or more substitutes, firms’ excess hiring drops to 0.16. The right panel indicates that firms’ excess hiring does not strongly differ 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.

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

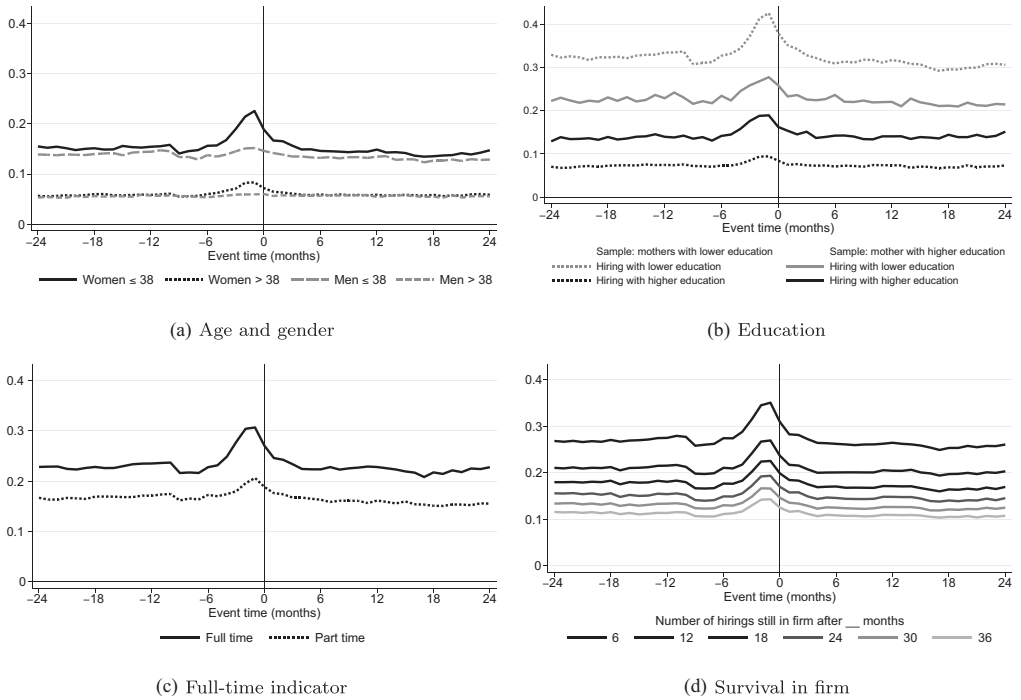


Fig. 3. *Firms' Hiring Composition around Childbirth.*

Notes: The figure shows 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. Births between July 2005 and December 2006 are in the sample and the number of firms is 38,555.

Source: IEB, own calculations.

between the mothers on leave and their replacements. Dashed lines represent firms' hiring of lower-educated mothers, while solid lines denote the hiring of higher-educated mothers. Grey lines indicate replacements with lower education, and black lines denote those with higher education. We see that mothers 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 future tenure, i.e., how long these workers stay in the firm, of external replacements. Generally, workers hired as replacements before childbirth tend to leave the firms at similar rates as workers hired in other periods (see also the share of hirings with certain characteristics depicted in [Online Appendix Figure A.5](#)).

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

4.1. Empirical Strategy

To estimate the effects of the 2007 parental leave reform on mothers and firms, we implement a 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 1 January 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 1 January 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 event-study model

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

where y is the outcome of the 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 SEs at the mother or firm level.

To summarise our estimates, we also report estimates for four discrete time bins. Specifically, we use the pregnancy (ten months before birth until childbirth) as the reference period and then estimate pre-pregnancy effects (twenty-four to eleven months before birth, p), short-term effects covering the paid parental leave period (two to twelve months after birth, s), medium-term effects covering the remaining job protection period (thirteen to thirty-six months after birth, m) and longer-term effects (thirty-seven to fifty-four months after birth, l). We estimate the regression

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

where γ_t denotes the period-specific effects.

4.1.1. Identifying assumptions

To interpret the γ_t coefficients as the effects of the parental leave reform, our empirical strategy relies on the parallel-trend assumption, i.e., the potential outcomes between treatment and control mothers and firms would have followed common trends in the 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 cutoff. Although the reform was first publicly discussed in May 2006, the final law was only passed in September 2006 (Kluve and Tamm, 2013). Raute (2019) observed 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 cutoff (as shown by Neugart and Ohlsson, 2013; Tamm, 2013; Jürges, 2017) is also not an issue in our estimations, as we exclude mothers giving birth two weeks before and after the reform date.

To assess any systematic differences between mothers 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 regression model

$$x_i = \beta_0 + \beta_1 \text{reform}_i + \beta_2 \text{spring}_i + \beta_3 \text{reform}_i \times \text{spring}_i + \epsilon_i, \quad (4)$$

where x_i represents pre-birth characteristics of the 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 (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, and small economically, but once we take into account multiple hypothesis testing, the joint F -tests do not reveal statistically significant differences between the groups.

4.2. Effects of Extended Parental Leave on Mothers and Firms

We begin our analysis by examining effects on 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 graphs of the reform according to (2) for the different sextiles of their pre-birth earnings distribution. Supporting the parallel-trend 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.

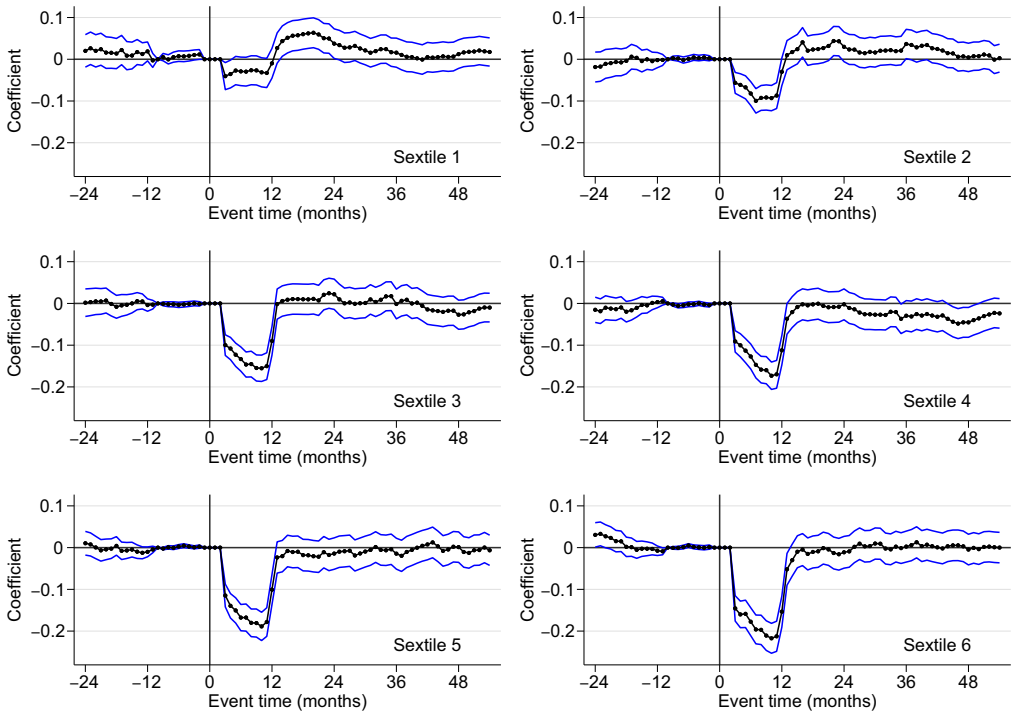


Fig. 4. *Effects on Mothers' Employment at Their Pre-Birth Firm by Pre-Birth Earnings Sextile.*

Notes: The figure plots event-study estimates of the 2007 paid parental leave reform in Germany on a binary indicator of whether the mother works at her pre-birth firm based on (2). Blue lines indicate the 95% confidence interval; SEs are 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 731, 1,295, 1,688, 2,129 and 2,733 euros.

Source: IEB, own calculations.

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 years 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 concentrate on mothers in the medium- to high-earnings bracket in the following. Because of the lack of a distinct earnings cutoff and based on the results from Figure 4, we set the earnings threshold at the high end of the second sextile, which is 1,295 euros in monthly pre-birth earnings.¹⁴

Next, we examine in more detail how the reform affected the outcomes of mothers with medium and high earnings, and their firms.¹⁵ In Figure 5(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% relative to the pre-reform average). After the first thirteen months, we observe

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

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

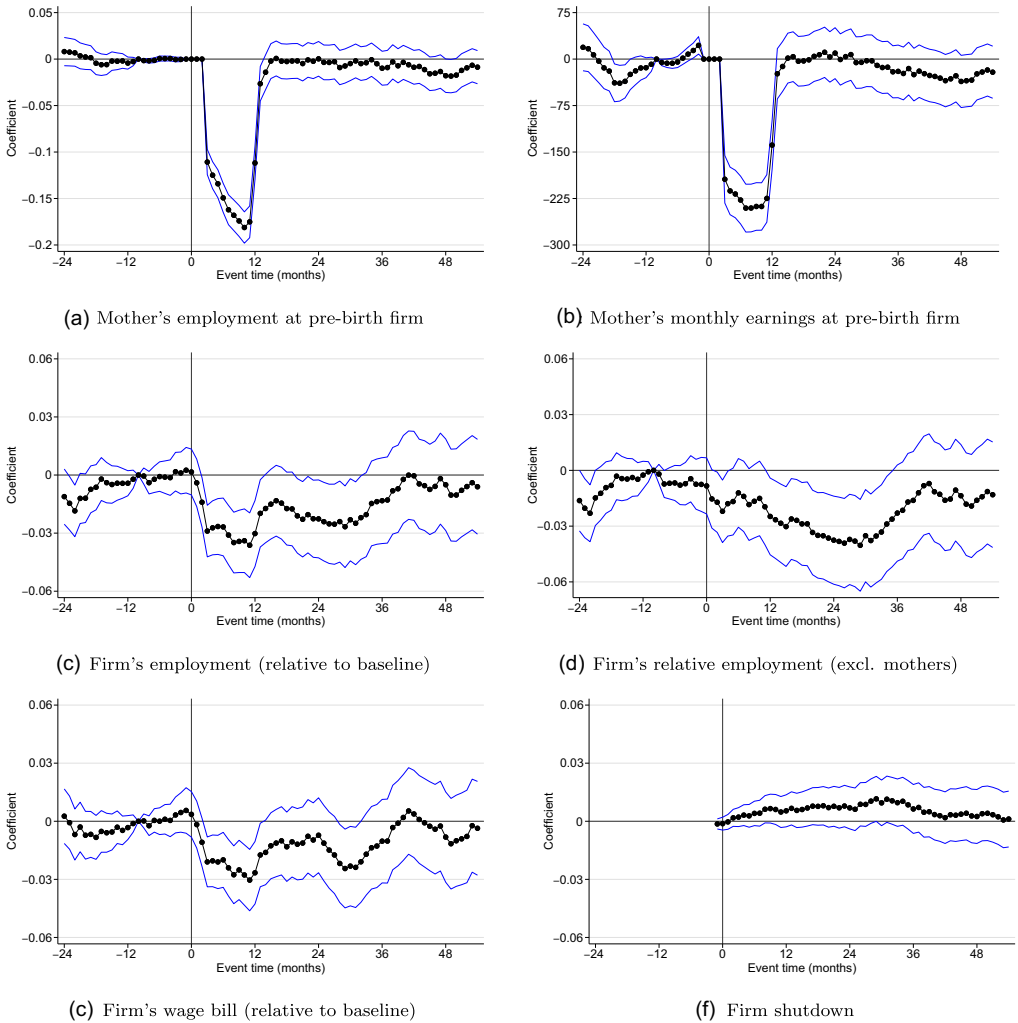


Fig. 5. Event Study of Parental Leave Reform Effects on Mothers' and Firms' Outcomes.

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 (2). The outcomes studied in panels (a)–(f) are listed below each graph. Dashed lines indicate the 95% confidence interval; SEs are clustered at the mother or firm level. Earnings in panels (b) and (e) are reported annually and converted to 2010 euros.

Source: IEB, own calculations.

no meaningful differences in the probability to work at the same firm up to fifty-four 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.

Figure 5(b) presents effect estimates on mothers' monthly earnings at their pre-birth firms. Consistent with the longer absence after childbirth due to the reform, treated mothers earn up to 240 euros less per month than control group mothers, who have average monthly earnings

Table 2. *Summary of Event-Study Estimates.*

Outcome:	Mothers		Firms		
	Employment at pre-birth firm (1)	Earnings at pre-birth firm (2)	Relative employment (3)	Relative wage bill (4)	Firm shutdown (5)
Pre-period	0.005 (0.005)	8.268 (12.559)	−0.006 (0.006)	−0.005 (0.005)	
Short-term effect	−0.145*** (0.007)	−195.892*** (15.712)	−0.029*** (0.005)	−0.025*** (0.005)	0.005 (0.003)
Medium-term effect	−0.002 (0.008)	16.163 (17.088)	−0.018** (0.008)	−0.015* (0.008)	0.008 (0.005)
Longer-term effect	−0.005 (0.008)	−0.583 (18.104)	−0.003 (0.011)	−0.003 (0.011)	0.001 (0.007)
Mothers/firms	42,053	42,053	40,768	40,768	40,768
Observations	4,289,406	4,289,406	4,158,336	4,158,336	4,158,336

Notes: The table summarises event-study estimates in discrete time periods based on (3). Pre-period is from twenty-eight to eleven 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 37–54 months post-birth, respectively. SEs clustered at the mother of firm level are reported in parentheses. Significance levels: * < 10%, ** < 5%, *** < 1%.

Source: IEB, own calculations.

of 355 euros in the year after childbirth (1,289 euros conditional on working). 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.¹⁶ In Table 2, we provide the corresponding short-, medium- and longer-term estimates in columns (1) and (2).¹⁷

How does this negative, temporary labour supply shock affect firms? In frictionless labour markets, firms would be expected to fully compensate for the gap. Figure 5(c) examines total employment at the firm and shows that the parental leave extension reduces employment within the first year after childbirth by up to 3% (see column (3) of Table 2).¹⁸ The treatment effect remains significantly negative for most of the second and third years after childbirth, meaning that the negative effects on firms reach beyond mothers' extended leave. The effect becomes statistically insignificant around thirty months after birth and converges to zero three years after childbirth, which is after the expiry of the job-protected period. Extended parental leave thus has negative effects on firms beyond mothers' leave duration, but not in the longer term.

To further explore the negative effects on firms' employment beyond mothers' longer leave, we next examine the effect of the reform on firms' employment excluding the mothers (panel (d)). In the first year after childbirth, we observe small and statistically insignificant employment effects. Thus, the negative impact of extended leave on firm-level employment during this period is primarily driven by the longer absence of mothers, who are not fully replaced. In years two and three after childbirth, the negative employment effects are attributable to reduced employment of co-workers. Further analyses of separations and hirings suggest that the remaining employment gap, excluding mothers, is due to a reduction in hirings rather than an increase in separations (see Online Appendix Figure A.6).

¹⁶ As mentioned in Section 2, the data do not provide information on working hours.

¹⁷ Several other empirical studies examine how the reform affected maternal labour market outcomes such as employment and earnings (see, e.g., Kluve and Tamm, 2013; Kluve and Schmitz, 2018; Huebener *et al.*, 2019; Frodermann *et al.*, 2023).

¹⁸ Note that we cannot directly compare the point estimates between panels (a) and (c) in Figure 5 to infer what happens to hirings and separations, as panel (a) is interpreted in percentage points and panel (c) in percent.

To capture internal adjustments such as increased working hours of internal substitutes, we examine firms' wage bills in Figure 5(e). The reform slightly reduced total labour costs in the twelve months following childbirth by about 2.5% (see column (4) of 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, approaching zero and becoming statistically insignificant in the longer term, i.e., thirty-seven to fifty-four months after childbirth.

Figure 5(f) 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 following estimates suggest very small and insignificant effects until fifty-four months after childbirth. The difference-in-differences summary estimates are precisely estimated and provide no evidence that the reform affected firm shutdown (see column (5) of Table 2).

Our main results are robust to alternative specifications that we summarise in [Online Appendix Figure A.7](#). We report the baseline results for maternal employment in the left panels. In the panels in the middle we narrow the sample to births from a six- to a two-month window before and after the reform. The panels at the right show an intermediate window of four months. While our preferred six-month window has the tightest confidence intervals, we reach the same inclusion using narrower windows around the reform.

4.3. *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 substitute. Our discussion focuses on the short-term effects of the reform on both mothers' and firms' outcomes as we estimate null effects for the longer term (see [Online Appendix Tables A.4](#) and [A.5](#) for the full estimation results).

Figure 6(a) presents the short-term coefficients related to 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 (−17.5 percentage points versus −10.6 percentage points).¹⁹ 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 (see [Online Appendix Table A.6](#)).

To better understand why the employment effects are stronger for mothers with few internal substitutes, we present the Kaplan–Meier failure functions of a mother's return to their pre-birth firm before and after the reform, differentiated by the availability of internal substitutes, in [Online Appendix Figure A.9](#). Prior to the reform (panel A), mothers started to return to work two months after giving birth, i.e., when paid maternity leave expired, and women with few (0–1) substitutes returned to work earlier than those with more substitutes. After the reform (panel B), mothers' return bunches at twelve months after birth, which is sensible given their eligibility for

¹⁹ The differences by workgroup size are also statistically significant; see [Online Appendix Table A.6](#), which uses a less demanding specification. We also examine different operationalisations of workgroup size differences in [Online Appendix Figure A.8](#) and consider interactions with $\ln(\text{workgroup size})$, third versus first tercile of workgroup size distribution and a median split of workgroup size distribution. We reach the same conclusions using these different approaches.

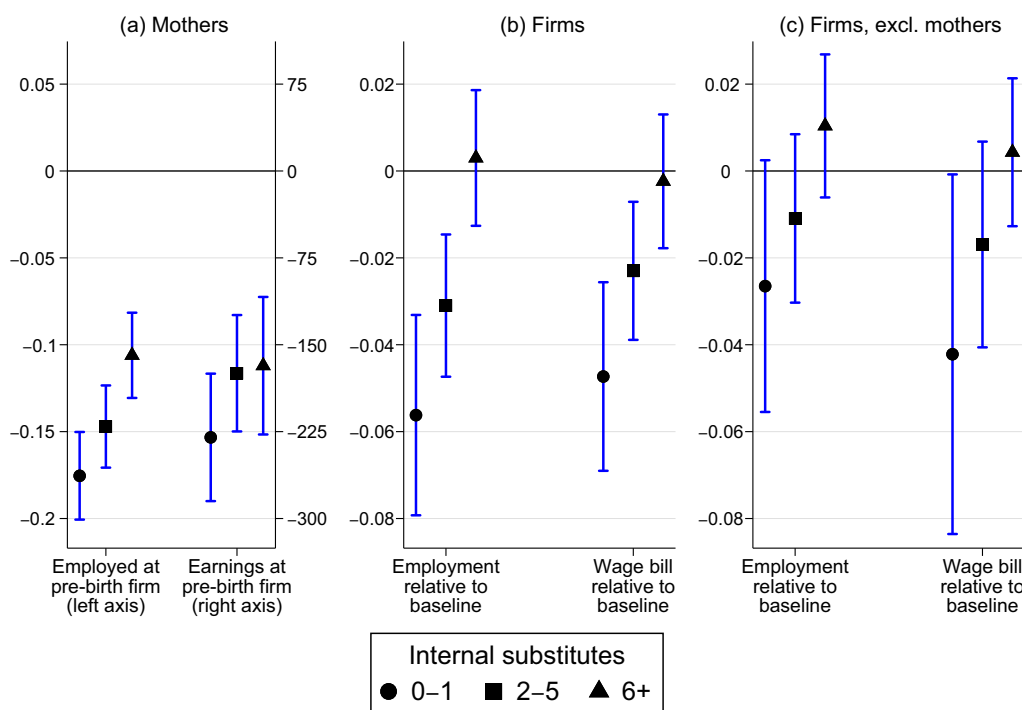


Fig. 6. *Short-Term Effects by Internal Substitutes.*

Notes: The figure shows the short-term estimates as in Table 2 by internal substitutes of mothers. Panel (a) contains estimates for mothers, panel (b) estimates for firms, and panel (c) estimates for firms, but excluding mothers in the calculation. 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.

up to twelve months of benefits and the high benefit withdrawal rate. One explanation for this change is that the parental leave reform introduced a strong economic incentive and provided a new social anchor for mothers not to return to work during the first twelve months. The differences by substitutability vanish in this period, but re-emerge afterwards. Consequently, women with fewer substitutes experienced the largest increase in leave duration.

Given the larger initial gap in workgroups with fewer internal substitutes, we turn to the effect heterogeneity for firms in Figure 6(b). 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% in the fourteen months after childbirth, compared to a 3.1% 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 bills 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 (Online Appendix Figure A.10).²⁰ The

²⁰ In Online Appendix Table A.7 we report, analogous to mothers, estimates for firms where the treatment indicators are interacted with the workgroup size.

results indicate that firms' reactions do not compensate for the experienced prolonged absences in smaller workgroups.

To explore whether these differences go beyond the direct effects from mothers' reduced employment, Figure 6(c) shows the effects on firms' employment when excluding mothers. As before, these effects are much smaller, confirming that the negative employment effect stems mostly from the mothers. That said, the results suggest small negative effects in the short run for the employment of other workers in firms with few internal substitutes, although these estimates are not statistically significant.

One might suspect that the observed differences in the availability of internal substitutes might reflect differences in firm size due to the correlation between firm size and workgroup size. As mentioned above, smaller firms may generally find it more challenging to handle longer worker absences. When we differentiate our analysis by firm size, we find larger short-term employment gaps in smaller firms with up to thirty employees, though no longer-term effects on firms' employment, wage bills and shutdown (Online Appendix Table A.8). 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 eleven employees (above median), ensuring that all workgroup sizes are represented (see Online Appendix Figure A.11). We find that the reform effects are larger in firms with few internal substitutes for the mother on leave (see Online Appendix Table A.9). This analysis confirms that our findings regarding internal substitutability do not merely reflect differences in firm size.²¹

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 Online Appendix Table A.5.

4.4. *Effects on Replacement Hiring*

To better understand how firms adjust to prolonged absences, we now shift our focus to the reform's effect on replacement hiring. We focus on workers hired within mothers' workgroups where, as shown earlier, roughly 80% of the replacement hiring occurs.²² Using the same empirical framework as in (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%) in the replacement period, though the coefficient is not statistically significant at the 5% level. As mothers' employment decreased by 15 percentage points in the first year after childbirth (see Table 2), back-of-the-envelope reasoning suggests that hires account for around half of the increased employment gap, which aligns with firms' typical adjustments to parental leave absences of around 0.4 additional hires per birth (see Section 3).

²¹ As one may worry that these heterogeneities reflect differences in baseline levels, we report similar findings on absolute effects on employment and the wage bill in Online Appendix Table A.10.

²² When we consider replacement hirings at the firm level, our estimates are less precisely estimated and mainly insignificant, which we attribute to more noise in the data.

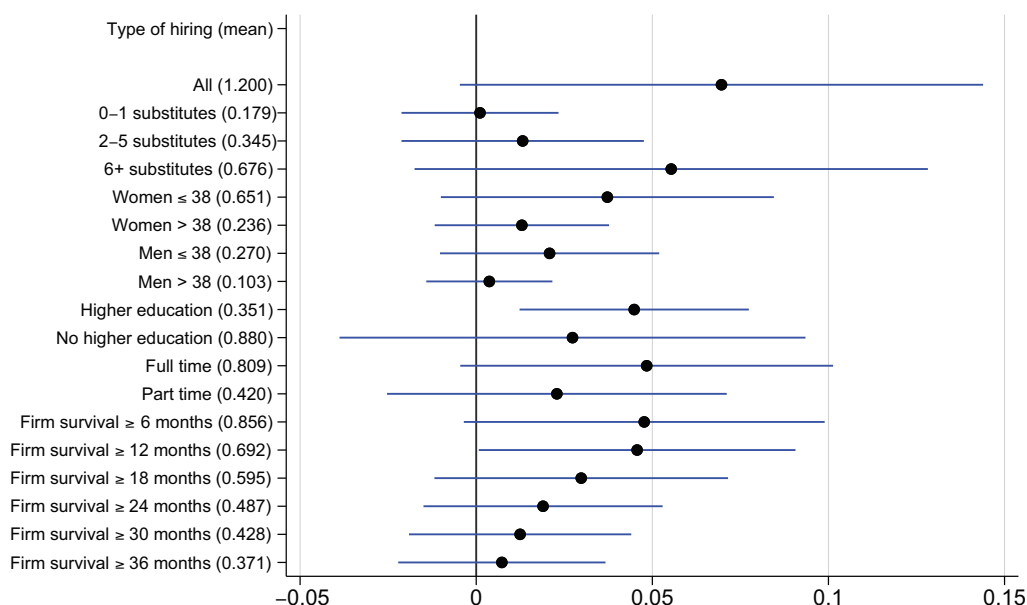


Fig. 7. Effect of the Parental Leave Reform on Hiring in the Replacement Period.

Notes: The figure shows the difference-in-differences coefficient based on (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 SEs.

Source: IEB, own calculations.

If we differentiate by the availability of internal substitutes, we find no large or statistically significant differences. If anything, the estimates seem to suggest that firms with more substitutes tend to increase their external hiring more.²³ There are three potential explanations for the small and statistically insignificant differences in the effect on replacement hiring by the availability of internal substitutes. First, firms might not have been aware of the difference in mothers' responses to the reform. While this cannot be ruled out, we consider it unlikely, given the setting of the reform, and mothers typically announcing their intended return early. Second, the loss in actual working hours due to mothers' delayed return could be relatively similar across workgroup sizes. As mothers typically would have returned part time, it is plausible that the differences in the effect on actual working hours are not large enough to induce substantial heterogeneities in firms' replacement hiring. Third, despite the large sample size, our statistical power to detect minor heterogeneities in effects on replacement hiring is limited, as can be seen from the large confidence intervals for workgroups of six or more internal substitutes. Taken together, the relatively similar effects on actual working hours leading to small heterogeneities in replacement hiring and limited power to detect minor heterogeneities can plausibly explain why the effects on replacement hiring do not differ more strongly by the availability of internal substitutes.

²³ To better understand these patterns, one can compare the number of replacement hirings by workgroup size before the reform (Figure 2(a)) and after the reform (Online Appendix Figure A.12). Descriptively, firms with few internal substitutes barely increased replacement hirings and the replacement hiring in larger workgroups catches up. The means for the subgroups in the last semester are however estimated less precisely.

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. These estimates tentatively suggest that firms also adjust to longer absences at the intensive margin by choosing workers with different characteristics.

Turning to the sex composition of replacement hirings, two channels can be at work. First, as we show in Section 3, firms tend to hire similar workers as replacements, and the reform increased replacement hirings. This combination may induce additional demand for young woman, which we label the 'replacement channel'. Second, there could be a 'discrimination channel', which describes that firms concerned about costly extended leave absences discriminate against younger women in their hiring. As replacement hirings can reflect both, we cannot disentangle these two channels in this analysis, which does not imply that there is no discrimination in the short run.

To examine effects on hiring discrimination, we thus turn to hirings from July 2007 onwards in the next section. As these are no replacement hirings, this allows us to isolate the discrimination channel.

5. Medium- and Longer-Term Effects on Hiring Discrimination against Younger Women

The preceding analysis revealed moderate short-term effects of longer leave on firms' employment and no longer-term effects on employment or firm survival. However, our set of outcomes does not fully capture all of the firms' relevant outcomes, e.g., we lack information on profits and sales. If firms face costs in these dimensions, they may try to mitigate these costs through altered future hiring of young women.

In our final section, we therefore investigate the impact of extended paid parental leave on firms' hiring decisions. We focus on whether firms affected by the reform hire fewer young women, who, following the reform, are statistically more likely to extend parental leave absences in the event of childbirth. Our rationale is two-fold. First, it sheds light on whether this family policy has unintended consequences. Second, hiring patterns can indicate whether longer absences are perceived to be costly for firms: if this was the case, we would expect firms to respond more promptly or decisively by hiring fewer young women or offering them lower wages. Conversely, if hiring decision practices remain unchanged, this would suggest that extended parental leave does not impose significant economic costs on firms that may have gone unnoticed in our analysis due to data limitations.

To identify the reform effects on firms' hiring behaviour, we use the same treatment and control group assignment as before, but now for later hirings. We argue that firms that have previously dealt with longer parental leave absences could react more strongly compared to firms without experience under the new parental leave policy. We slightly adjust the empirical approach and now focus on hirings in calendar time instead of event time relative to birth. As discussed at the end of the previous section, hirings until June 2007 can be directly affected by the reform via an increased need for replacements for the mother, as well as changes in discrimination. We therefore focus on hirings from July 2007 onwards, which helps us to isolate the medium- to longer-term discrimination channel.²⁴

²⁴ To alleviate concerns about the influence of the replacement channel due to subsequent births by co-workers, we conduct a robustness check focusing only on firms with no additional births until the end of 2009. Reassuringly, the results remain nearly identical (see [Online Appendix Figure A.13](#)).

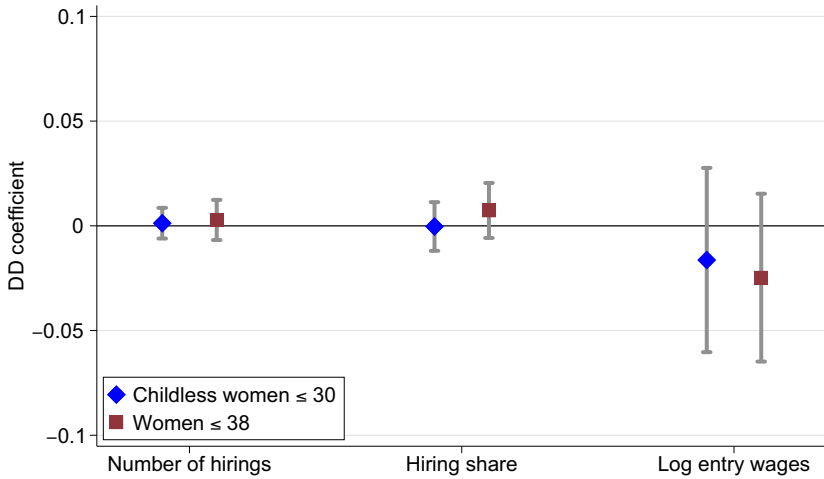


Fig. 8. *Longer-Term Effects on Hiring and Wages of Young Women.*

Notes: The 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. The number of firms $N = 38,678$ with 508,951 hirings. The 95% confidence intervals are based on robust SEs.

Source: IEB, own calculations.

Our data include 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 thirty, and women up to the age of thirty-eight. 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 difference-in-differences model

$$y_{ijt} = \gamma_0 + \gamma_1 \text{reform}_j + \gamma_2 \text{spring}_j + \gamma_3 \text{reform}_j \times \text{spring}_j + \theta_t + \epsilon_{ijt}, \quad (5)$$

where y_{ijt} is an outcome of worker i in firm j at time t ; 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 reports the γ_3 coefficients based on (5) for our main sample. The first two coefficients show that the reform had no longer-term 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 young female workers, which corresponds to a 6% reduction relative to the mean. 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, when 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 [Online Appendix Figures A.14](#).

One concern with these estimates is that the public discussion of the reform might have prompted both affected and unaffected firms to reduce their hiring of young women. While unaffected firms might hire fewer women due to expected higher costs, affected firms might hire fewer women due to experienced higher costs. As our approach uncovers potential discrimination only through *differential* effects on hiring between affected and unaffected firms, such general equilibrium effects could result in a null finding. We address this concerns about general equilibrium effects on female hiring by examining the hiring patterns of younger and older women in the raw data; see [Online Appendix Figure A.15](#). We do not observe a general decline in the hiring shares of younger women, and there is no evidence of younger women being substituted by older women. These findings help alleviate some of the concerns about substantial general equilibrium effects that might also result in zero effects in our analysis.

Another concern is that statistical discrimination might occur in all firms and not only in firms with first-time births. Moreover, the treatment-control group assignment based on prior experience with extended absences may not adequately capture the intensity with which firms are affected by the reform. To address both concerns, we draw an entirely new sample and pursue an alternative identification strategy that exploits the economic incentives of the reform. The new data include a random subset of all firms and not just firms that had a birth between 2005 to 2007, increasing our sample to around 3.8 million hiring events between July 2007 and December 2009. Methodologically, we use high-wage occupations as the treatment group and low-wage occupations as the control group (see [Figure 4](#)). To assign occupations to one of these groups, we focus on full-time entry wages by occupation before the reform. We assign occupations with entry wages in the third tercile of the earnings distribution to the treatment group and occupations with entry wages in the first tercile to the control group.²⁵

To disentangle the replacement and discrimination channels in this analysis, we further distinguish between all firms and those that did not experience a birth. The intuition is that firms without births can only be affected by the discrimination channel, whereas firms with births could be affected by both channels. [Online Appendix Figure A.16](#) shows the results for the hiring probability of young women. As a direct robustness check, we also report the results for several samples of firms, where we vary the time frames for when births were not allowed to take place in the firm. Overall, we observe consistent and precisely estimated null effects on the longer-term hiring composition of firms, allowing us to rule out negative effects on the hiring probability of younger women of one percentage point. This alleviates concerns that the previous results depend on the sample of firms considered, or the chosen identification strategy.

In sum, our analysis does not find any evidence of medium- to longer-term hiring discrimination against younger women in the aftermath of the paid parental leave expansion. Since firms typically adjust their hiring decisions to account for additional costs, these results further suggest that extended parental leave imposes no substantial costs on firms.

6. 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

²⁵ As a robustness check, we assign high- and low-wage occupations based on men's entry wages. [Online Appendix Figure A.17](#) summarises the results. We reach the same conclusions using this alternative assignment.

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. This finding suggests that parental leave choices lie more with the women than the firms and further supports our conclusion that extended leave does not impose substantial costs on firms and workers. In the longer term, we do not find effects on firms' employment or shutdown, independent of the availability of internal substitutes. To understand firms' responses 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 increased 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 Gallen (2019) and Ginja *et al.* (2023)? First, firms may already anticipate losing a significant portion of their workforce after childbirth, because German mothers typically return to part-time employment after childbirth.²⁶ Compared to the longer-term loss in working hours that firms have to deal with due to motherhood, the additional costs of extended parental leave might be comparatively 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 (see, e.g., Jessen, 2022). We do not find longer-term effects on firms in either region (Online 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 of 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. This finding underlines the importance of how the reform is implemented in determining its effects. In the settings analysed by Gallen (2019) and Ginja *et al.* (2023), 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 changed jobs after childbirth. Our results are more similar to those of Brenøe *et al.* (2024), whose findings from a non-reform setting also suggest that anticipated absences related to childbirth have little impact on firm outcomes—provided they can internally replace the mother. Put into perspective, our results draw a more optimistic picture than Gallen (2019) and Ginja *et al.* (2023) 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.*, 2024). Taken together, the empirical evidence supports the conclusion that such policies help reconcile work and family life without further

²⁶ As of 2009, employed mothers with children aged 0–14 have a full-time share of 39% in Germany compared to more than 80% in Sweden and Denmark (OECD, 2020).

widening gender gaps in the labour market. Going forward, to narrow 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 childcare spending and in-work benefits (Olivetti and Petrongolo, [2017](#)).

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Additional Supporting Information may be found in the online version of this article:

Online Appendix Replication Package

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