I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14478

A Firm-Side Perspective on Parental Leave

Mathias Huebener
Jonas Jessen
Daniel Kuehnle
Michael Oberfichtner

JUNE 2021

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14478

A Firm-Side Perspective on Parental Leave

Mathias Huebener
DIW Berlin and IZA

Daniel Kuehnle
Universität Duisburg-Essen and IZA

Jonas Jessen
DIW Berlin and Freie Universität Berlin

Michael Oberfichtner
IAB Nuremberg

JUNE 2021

ISSN: 2365-9793

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

A Firm-Side Perspective on Parental Leave*

Motherhood and parental leave interrupt employment relationships, likely imposing costs on firms. We document that mothers who are difficult to replace internally take shorter leave and that their firms hire replacements more often. Introducing more generous parental leave benefits erases the link between mothers' internal replaceability and their leave duration. In firms with few internal substitutes this reduces employment in the short-, but not longer-term. Firms respond by hiring fewer women of childbearing age into occupations where they are difficult to replace internally. Taken together, motherhood and generous parental leave policies burden firms that have few internal substitutes available.

JEL Classification: J16, J18, J24

Keywords: parental leave, worker absences, firm-specific human capital, substitution, statistical discrimination

Corresponding author:
Jonas Jessen
DIW Berlin
Mohrenstraße 58
10117 Berlin
Germany

E-mail: jjessen@diw.de

* An updated version of this paper is available as IZA DP No. 16843 ("Parental Leave, Worker Substitutability, and Firms' Employment"). We are grateful for comments by Francine Blau, David Card, Thomas Cornelissen, Max Deter, Bernd Fitzenberger, Christina Gathmann, Rita Ginja, Ines Helm, Simon Jäger, William Jergins, Brendon McConnell, Steven Rivkin, Kjell G. Salvanes, C. Katharina Spiess, Till von Wachter, and Matthias Westphal, as well as seminar and conference participants at ASSA 2021, DIW Berlin, WEAI 2021, Hertie School of Governance, IAB, University of Erlangen–Nuremberg, Leuphana University of Lüneburg, Verein für Socialpolitik, and ifo Dresden. We thank Martin Popp for sharing data on employment by occupation at the district level.

1. Introduction

In imperfect labour markets, employment relationships generate rents for firms, making employment interruptions costly for them. One of the most important sources of employment interruptions are motherhood-related absences. While generous parental leave policies help parents to better reconcile work and family life after childbirth, firms need to handle the employment interruptions that these policies create. The costs of such interruptions may be substantial if the worker on leave cannot be easily replaced by other incumbent workers or external hirings. Such costs, in particular for small- and medium-sized firms, are a major reason brought forward in the US against a federal parental leave scheme (e.g., see Bartel et al., 2021). Yet, surprisingly little is known about the impact of parental leave absences on firms.

Our study helps fill this gap and improves our understanding of how firms deal with parental leave absences. We use the full population of administrative linked employer-employee data from Germany that allow us to uncover new stylised facts about firms' hiring behaviour as well as the relationship between parental leave absences and the availability of replacement workers inside and outside of the firm. We further examine how a paid parental leave reform granting additional parental leave benefits to medium- and high-earning mothers, incentivising them to delay their return to their employer in the first year after childbirth, affected mothers and their firms.

Our paper makes three main contributions to the literature. First, we take a firm-side perspective on paid parental leave using rich employer-employee data that includes three crucial pieces of information: employment spells at the daily level, detailed occupational codes, and information on single locations of multi-site firms. This allow us to identify *local workgroups*, i.e., workers in the same occupation, same firm and same location, and separates us from related work as we are the only study to measure the number of substitute workers inside and outside of the firm for each mother. We provide first evidence concerning the exact timing and composition of replacement hirings, and we reveal the link between mothers' parental leave-taking and the availability of internal and external substi-

tutes for two different parental leave systems. Focusing on a parental leave extension, we provide new evidence on the longer-term effects for mothers and their firms. As our analysis focuses on small firms with less than 50 employees, we contribute directly to the current debate about the implications of providing more generous parental leave schemes, as proposed by the Biden administration in April 2021 (NYT, 2021).

Second, we contribute new evidence on the substitutability of workers for temporary and agreed-upon absences. We focus on the case of parental leave absences, which rank among the most important reasons for worker absences as they directly affect the vast majority of women during their prime working lives.[1] The previous literature has so far focused on absences due to sickness and worker deaths. Parental leave absences, however, differ from absences due to sickness and worker deaths in important ways. Specifically, parental leave is typically anticipated and can thus be better planned by firms. Thus, the length of parental leave is agreed upon in advance.

Our third contribution relates to statistical discrimination against young women in the labour market and the unintended consequences of public policies. We conjecture that profit-maximising firms anticipate and internalise the potential costs of longer birth-related absences by younger female workers and in turn reduce the hiring of younger women. To identify statistical discrimination net of other potentially confounding time trends, we apply an event-study framework inspired by Dobkin et al. (2018) and examine whether firms reduce their hiring of young women into occupations that *were not* affected by a birth in the same firm following the parental leave reform. If firms hire fewer women of childbearing age for workgroups with few internal substitutes after the reform, such patterns would be evidence for statistical discrimination. Despite its theoretical appeal, identifying statistical discrimination is notoriously difficult and the existing evidence mostly stems from audit studies.

In a first step of the analysis, we examine how firms adjust their hirings and separations to birth-related worker absences and how workplace

[1] In most OECD countries, 80-90 percent of women have at least one child before age 45 (see UN World Fertility Data).

characteristics are linked to mothers' length of parental leave. We find that replacement hiring is more pronounced when few internal substitutes, i.e., workers in the same occupation, are available, whereas external substitutes, i.e., the share of workers in the same occupation in the local labour market, are less important. We find no evidence for an adjustment in separations to reduce the impact of the absent mother. We then show that mothers substantially postpone their return-to-work if more internal substitutes are available in their firm, whereas—as for replacement hiring—external substitutes are not related to the length of parental leave. These links hold when we further control for maternal characteristics, and occupational and regional heterogeneity. We then investigate how these patterns change with the introduction of a more generous paid parental leave scheme which incentivises longer absences from work during the first year after childbirth. The reform almost eradicates the link between the length of parental leave and the availability of internal substitutes during the period of benefit receipt. These results suggest that the introduction of paid parental leave may distort the coordination between employers and mothers in the benefit payment period.

In a second step, we study the effects of the parental leave expansion on mothers' return to their pre-birth employer and on firms. Our main firm outcome is the employment level: As employment generates rents for firms, lower employment implies lower profits, *ceteris paribus*. Our empirical estimation strategy employs a dynamic difference-in-differences design. In line with the economic incentives, we find that medium- and high-earning mothers substantially delay their return to their pre-birth employers when parental leave benefits are expanded, with no medium- to longer-term impact on the probability of exiting their firm. Mothers giving birth after the reform have a 20 percentage points lower probability to have returned to their pre-birth firm ten months after childbirth. We find negative effects on firms' employment which implies that firms do not fully compensate the longer absences of mothers. The effects are driven by firms in which fewer internal substitutes are available for the mother-on-leave. As the number of internal substitutes increases, the employment gap decreases and eventually disappears. We observe that firms with more internal substitutes experience a generally higher labour turnover that may help to bridge the

labour shortage. We do not find evidence that firms hire more workers from external labour markets to compensate the longer worker absence. Despite the short-term labour gap in firms with few internal substitutes, we identify no effects on firms' employment in the second to fourth year after childbirth.

As firms with fewer internal substitutes cannot fully compensate the short-run negative labour supply shock of longer absences, we conjecture that they may internalise the associated costs in their future hiring decisions and analyse whether firms' hiring under the new policy regime deviates from their pre-reform hiring history in terms of observable worker characteristics. We find that firms are less likely to hire women of childbearing age after the reform into occupations in which only few internal substitutes were available in case of a pregnancy. In contrast, the hiring of older women and men into those occupations increases.

Our paper ties into three strands of economic literature. First, our paper adds to a small literature on the effects of parental leave for firms. A recent paper by Ginja et al. (2020) studies a parental leave expansion from 12 to 15 months in Sweden and finds that the reform increased separations of mothers from their pre-birth firms and that adjustments to the longer absences are costly for firms. We complement their study by focusing on a reform that (i) substantially expanded parental leave absences *within* the first year after childbirth and (ii) allowed firms to partly anticipate the longer leave absences which could impact firms' replacement hiring decisions. Related, Gallen (2019) studies the effects of a Danish parental leave reform on firms and mothers' coworkers. The reform increased the length of fully-compensated parental leave by 22 weeks within the first year after childbirth. She finds decreases in mothers' retention probability and in firms' survival rates. For Denmark, Brenøe et al. (2020) use Danish administrative data and examine the joint effect of motherhood and the subsequent parental leave period on the mother's firm by combining a matching- and event-study-approach; their findings suggest that the costs of parental leave on firms are negligible. For the US, Bartel et al. (2021) survey small- and medium-sized firms to study the introduction of a paid parental leave policy in New York covering eight weeks of partial wage replacements. They find a short-term increase in employers' self-reported

ease of handling employee absences for firms with 50 or more employees, but not for smaller firms.[2]

Second, we contribute to the literature on the substitutability of workers, with a particular focus on firm-specific capital and firms' ability to substitute (temporary) absences of workers. Previous studies have focused on sickness absences (e.g., Hensvik and Rosenqvist, 2019) and worker deaths (e.g., Jäger and Heining, 2019).[3] Specifically, Hensvik and Rosenqvist (2019) use Swedish administrative data and show that firms keep sickness absences low for positions where workers are harder to replace. Jäger and Heining (2019) document that the unexpected death of a worker affects their coworkers' earnings and firm retention probability using German administrative data. They find that the effect depends on the degree of substitutability between workers and on the availability of external substitutes in the labour market.

Third, we add to the quasi-experimental literature on statistical discrimination against young women in the labour market (Fernández-Kranz and Rodríguez-Planas, 2021, Jessen et al., 2019). Child-related work absences require firms to find a replacement during leave, affect the accumulation of firm-specific human capital, and may deteriorate the skills of employees during leave (Adda et al., 2017). If those absences are costly for firms, theory predicts that employers would internalise the associated costs and discriminate against women of childbearing age by hiring or promoting them with a lower probability or by paying them lower wages than their male counterparts. An extension of paid parental leave that is meant to facilitate the reconciliation of work and family life, incentivises women to return to work later. Our study provides new evidence that such family policies can potentially backfire on potential mothers in the labour market.

[2]Our paper also relate to the much larger literature on the effects of parental leave policies on maternal labour market outcomes (e.g., see Lalive and Zweimüller, 2009, Schönberg and Ludsteck, 2014, Rossin-Slater, 2018, Kleven et al., 2020).

[3]One strand of the literature on the substitutability of workers focuses on how the death of key figures within firms, such as CEOs, superstar scientists, or inventors, affect the productivity and earnings of their coworkers (Azoulay et al., 2010, Jaravel et al., 2018, Bennedsen et al., 2020). These results cannot immediately be generalised about the substitutability of "regular" employees since this literature examines the loss of very specific workers characterised by exceptional productivity and/or firm-specific capital.

The remainder of this paper is structured as follows. We provide information on the background and institution details in section 2 and describe the employer-employee matched data and our sample in detail in section 3. Section 4 describes how firms' hirings and separations respond to a birth and establishes basic relationships between parental leave schemes, mothers' absences and workplace and labour market characteristics in different parental leave regimes. In section 5, we present the parental leave reform effects on workers and firms. In section 6, we evaluate the impact of the parental leave expansion on firms' longer-term hiring to assess whether parental leave expansions can result in statistical discrimination against women of childbearing age. Section 7 concludes and discusses some policy implications.

2. Background and Institutional Environment

2.1. Relevant Aspects of the German labour market

Our analysis focuses on Germany which for a long time has been characterized by low labour force participation rates of mothers with dependent children. However, a series of policy reforms starting in 1996 substantially expanded the availability of day care and strongly encouraged early maternal employment. In 2006, the day care attendance rate for 0-2 year olds, including centre-based and family day care services, was 13.6 percent (cf. OECD average 30 percent, based on OECD, 2016a,b) and 41 percent of these mothers were employed (BMFSFJ, 2018). 63 percent of women aged 25-54 with at least one child aged 0-14 participated in the labour force (cf. OECD average 66.1 percent).

One key feature of the German labour market relevant for our analysis is the important role played by small firms, on which we focus in our analysis. Small firms are defined as firms that employ less than 50 workers and earn less than 10 million euro in annual turnover. These firms represent a large share of the German labour market as they make up 96.9 percent of all enterprises and employ about 41 percent of all workers (Destatis, 2018). As the debate about possible adverse effects of parental leave on firms is centered around small and medium-sized firms, Germany provides a suitable setting to study this question.

Although the extent of temporary work agencies has increased in many countries over the past twenty years, temporary employment was of little importance for the firms we analyse (less than 50 employees) during the period we study. Specifically, between 2003 and 2010, the percentage of temporary workers among all employees in Germany was less than 0.7 percent in firms with 10-49 employees and less than 0.4 percent in firms with less than 10 employees (for more details, see Hirsch and Mueller, 2012). Workers hired from temporary work agencies therefore only play a minor role to substitute workers on parental leave in our setting.

2.2. Family Policies Supporting Women in the Labour Market

German family policy supports pregnant women and mothers in the labour market through the following key policy measures, which are also relevant for their employers: paid maternity leave, job protection, and parental leave benefits.[4]

Job Protection and Parental Leave. Parents can claim job-protected parental leave (*Elternzeit*) from their employer allowing them to return to their previous position within 36 months after childbirth. To claim job-protected parental leave subsequent to maternity leave, mothers have to 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.

Paid Maternity Leave. All mothers are entitled to paid maternity leave which lasts from six weeks before expected delivery to eight weeks after childbirth. During this period, (expecting) mothers are generally not allowed to work but they are entitled to a full (net) earnings replacement.[5] The statutory health insurance companies pay for the earnings replacements, so that firms do not incur any direct costs.[6]

[4]Additionally, day care spots are publicly subsidised and not-for-profit, as only three percent of institutions are run by private and non-charitable organisations.

[5]Mothers are allowed to work up to childbirth if they provide their explicit consent and if the work environment is considered safe. After childbirth, they are not allowed to work for the duration of maternity leave (see §3 *Mutterschutzgesetz*).

[6]Administratively, both health insurers and firms make earnings continuation payments. However, firms' expenses are fully reimbursed by health insurers. Prior to 2006, for firms with more than 30 employees, health insurers covered only 13 euro per day, and firms had to cover the excess amount of earnings continuation payments to cover

Parental Leave Benefits. Parental leave benefits are an important determinant of the length of parental leave (e.g., see Schönberg and Ludsteck, 2014). In Germany, parental leave benefits were substantially reformed in 2007 from a means-tested to an earnings-based scheme. In the following, we describe both policy regimes as we exploit the reform in our analysis.

Prior to the reform in 2007, parents with low household income were eligible to receive benefits for up to 24 months after childbirth. These publicly-funded benefits were means-tested and paid a maximum of 300 euro per month (about 370 USD in 2006), corresponding to around eleven percent of average pre-birth net household income. Families qualified for the benefit if their annual net income was below a certain threshold, which varied with the household structure, number of children, and time since giving birth.[7] About 77 percent of parents were eligible to receive benefits for up to six months after childbirth (see Huebener et al., 2019). Due to repeated means-testing and lower household 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.[8]

The 2007-Paid Parental Leave Reform. In September 2006, the German government substantially reformed the paid parental leave system which affected parents of all children born on or after January 1, 2007. The new benefit system replaced the previous means-tested benefits with an earnings-based paid parental leave system. Benefits were paid for up to 12 months to either parent and replaced 67 percent of the average net labour

women's pre-birth net earnings (Jessen et al., 2019). Changes in these regulations are not a threat to our quasi-experimental analysis where we focus on a paid parental leave reform in 2007 and analyse births after this policy change.

[7]For the first child, it was possible to receive these benefits for six months after childbirth if the net yearly income of a couple was less than 30,000 euro (23,000 euro for single parents). From the seventh month onward, this limit was 16,500 euro for couples (13,500 euro for single parents). If the expected net income exceeded these thresholds, the parental benefits were reduced rather than entirely withdrawn. The reduced child-rearing allowance was only available for families with an expected net income of up to 22,086 euro for couples (up to 19,086 euro for single parents; for details, see BMFSFJ, 2008).

[8]Part-time work of up to 30 hours per week was permitted during the benefit receipt 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 (own calculations based on SOEPv30).

income earned in the 12 months prior to childbirth.[9] The benefit had a floor of 300 euro and was capped at 1,800 euro per month.[10] Given near universal eligibility, take-up was almost 100 percent (Destatis, 2008).

The reform did not change the maternity leave period, the 36-months job protection period, or part-time employment regulations during the job-protected period. Overall, the direct costs of childbirth-related absences for employers are negligible, the mostly indirect costs relate to finding suitable replacement workers and to consequences on firm's operating processes.

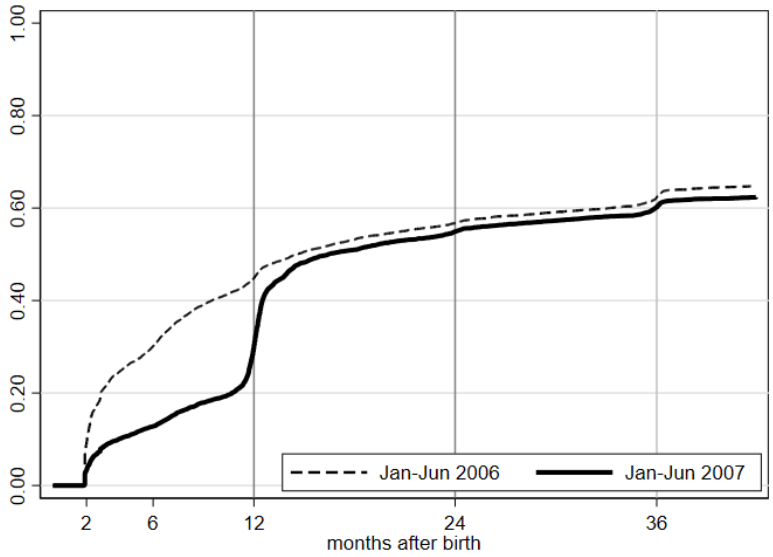
Our analysis focuses on medium- and high-earning mothers who unambiguously gained higher paid parental leave benefits following the two months of mandatory maternity leave. For example, a mother earning 2,500 euro (net) per month before childbirth could claim 1,675 euro per month, or 16,750 euro in total, after the reform. In comparison to the maximum regular benefit of 300 euro under the old benefit system, the reform clearly reduced the opportunity costs of longer workplace absences for medium- and high-earning mothers.

To illustrate the reform effect on maternal employment, Figure 1 plots the share of mothers who have returned to their pre-birth employer at different points in time, distinguishing between mothers who give birth in the same calendar months (January to June) before and after the paid parental leave expansion. With lower benefits (dashed line), mothers return gradually after the end of their maternity leave period. With extended paid parental leave (solid line), the return within the first year is substantially delayed and starts to converge to the pre-reform pattern with the expiry of the parental leave benefits after 12 months. This pattern is consistent with the changed economic incentives during the first year.[11]

[9] Two additional months were granted for single parents or if both partners take parental leave for at least two months. The maximum length of 14 months of paid parental leave could be split flexibly between both parents, with a minimum of two months per parent. Approximately 96 percent of parents assign the main benefit period (>7 months) to the mother. In our observation period, 15 percent of fathers take paid parental leave, mostly for two months. Parents could also choose to receive only half of the monthly benefits to double the benefit period, with only eight percent of parents choosing this option (Destatis, 2008).

[10] Individuals who did not work prior to giving birth, or those with low earnings, continue to receive 300 euro per month, but now only for up to 12 months instead of 24

Figure 1: Return to pre-birth firm by birth semester



Notes: Figure shows the share of mothers with pre-birth earnings of at least 1700 euro per month who have returned to their pre-birth employer by month t after childbirth. The dashed line indicates mothers giving birth between January and June 2006 (pre-reform), the solid line indicates mothers giving birth between January and June 2007 (post-reform).
Source: IEB, own calculations.

3. Data

3.1. Data Source: Social Security Records

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 V13.00.00, see Jacobebbinghaus and Seth, 2007, for a detailed description). The data is available from 1975 through 2018 and cover about 82 percent of all workers in Germany.[12] The

months.

[11] In Figure A1 we show that the pattern is similar when we examine the share of mothers who have returned to any employment.

[12] Civil servants and self-employed individuals are not included in the data. This implies that we do not have information on all workers in the public sector who are subject to social security; 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 workplace. 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.

reported earnings and job durations are used to calculate social security payments and benefits and are therefore highly reliable.

Several features of the data render it particularly suitable for our analysis. The first advantage is that the data contain information on employment spells at the daily level as employers report the precise start and end dates of any employment spell. This allows us to exploit the exact timing when new employment spells start and end. Second, the data allow us to identify single locations of multi-site firms. For simplicity, we refer to these establishments as firms throughout the paper. Third, we have detailed occupational information for workers at the 3-digit level (with 256 unique occupations of mothers in our sample). Combined with the exact location, the data allow us to identify *local workgroups*, i.e., workers in the same occupation, same firm and same location.

In addition to the above features, the data furthermore include basic socio-demographic characteristics like workers' gender, citizenship, education (imputed as described in Thomsen et al., 2018) and date of birth. As the data goes back to 1975, we can reconstruct the entire employment biographies subject to social security contributions of all individuals in our sample. The data also include a part-time/full-time indicator, but no further details on working hours. The data do not include direct information on motherhood, but we follow Müller and Strauch (2017) to identify mothers and infer the expected date of delivery by exploiting the legal requirement that employers have to notify the health insurance companies, who carry out the maternity leave payments, about the start date of this leave period.[13] We use the expected date of delivery to assign mothers to specific paid parental leave regimes. To avoid the misassignment of births around the policy cut-off, we exclude all expected births that occur two weeks before and after January 1st from the analysis.

[13]The same notification code is used in some cases of longer illnesses, but this event is very rare for women of childbearing age and such absences are often shorter than the required mandatory leave lengths for childbirth. We implement several checks to ensure that the notification reflects childbirths.

3.2. Operationalising Internal and External Substitutes

We define workers as *internal substitutes* if they work in the same firm and in the same 3-digit occupation (i.e., they perform similar tasks) ten months prior to childbirth. This coworker definition is also used in Cornelissen et al. (2017) and Hensvik and Rosenqvist (2019). Throughout the paper, we refer to mothers' coworkers as *internal substitutes* and we use the term *workgroup* when we additionally include the mother. We define three groups which correspond to terciles: mothers with 0-1, 2-5, and 6 or more internal substitutes.[14]

To measure the availability of *external substitutes*, we build on the concept of labour market thickness. From a worker's perspective, a market is thick when she receives many offers for a given amount of search effort (Lazear, 2009). 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 follow Jäger and Heining (2019) and use the relative density of 3-digit occupational groups in the local labour market of a firm.[15] For this purpose, we calculate the regional share of employment in each occupation relative to the nationwide share of employment in each occupation as of June 30, 2006 from the universe of the IEB. We also split labour market thickness as a measure for external substitutes into terciles.

3.3. Sample Selection and Treatment Assignment

In our setting, a workplace is affected by the 2007 parental leave reform if a mother employed by the firm gives birth on or after January 1, 2007. As the date of birth cut-off determines the paid parental leave eligibility, this institutional rule assigns mothers and their firms into a natural treatment group (births between January and June 2007) and control group (births between July and December 2006). To account for any seasonality in outcomes, we further consider mothers and firms with births in the preceding year (July 2005 to June 2006) within our estimation strategy.

[14] Appendix Figure A2 shows the distribution of 1-digit occupations following the classification by Blossfeld (1987).

[15] Our classification of labour market regions follows Kosfeld and Werner (2012) who define 141 regions in Germany based on commuting flows.

We focus our analysis on *private, for-profit* establishments and drop establishments 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. As we expect effects to be concentrated in smaller firms, we focus on firms with up to 50 employees before the pregnancy occurs in the firm. The mean firm size in our analysis sample is 14 employees with a median size of ten (see Figure A3).

As firms could experience multiple births – before and after the reform – we focus on firms in which exactly one birth occurs in the period between two years before and two years after childbirth. This sampling restriction allows us to cleanly identify firms as being affected by the paid parental leave reform and to contrast them with firms that were not affected. As we already focus on smaller firms, considering only one birth per firm is not a restrictive condition.

Further, we only keep firms where the mother giving birth fulfils the following three criteria. *First*, we focus on first-time mothers as these are more strongly attached to the labour market. We would therefore expect that the effects of a birth and of parental leave are more pronounced compared to mothers with higher-order births.[16] *Second*, we only keep mothers with gross monthly earnings of at least 1700 euro before giving birth (this excludes the lower 37 earning percentiles of first-time mothers). The introduction of paid parental leave unambiguously increased non-labour income for these mothers during the first year after giving birth, thus monotonically increasing mothers' financial incentives for longer absences from work.[17] *Third*, we impose a minimum tenure requirement and focus on mothers who have been at their workplace for at least ten months prior to giving birth. This restriction avoids endogenous selection into firms and

[16] We also do not focus on higher-order births as these can only be identified in the data if the mother returns to work between two births. Including mothers with higher-order births could thus yield a selective sample with respect to birth-spacing and mothers' labour force attachment, especially if the parental leave reform affects these outcomes.

[17] In contrast, the 2007 paid parental leave reform reduced some low-income mothers' family non-labour income in the second year after childbirth (see section 2.1). The reform might hence induce some low-earning mothers to return to work earlier and others to return later (Kluve and Schmitz, 2018, Huebener et al., 2019). We thus cannot exploit the 2007 paid parental leave reform to investigate the effects of longer leave on those mothers and their employers.

occupations during pregnancy.

Our final analysis sample contains 23,679 mothers and firms. We observe the entire employment history of all workers who have been employed at those firms at any time from four years before to eight years after birth. Table 1 reports pre-birth characteristics for mothers and their firms. Column (1) shows that mothers in our data are on average 30 years old at childbirth, 96 percent are German citizens. Around 39 percent of mothers have high levels of education and their average gross annual earnings amount to around 31,000 euro (38,000 USD in 2006). These mothers are strongly attached to the labour market as around 94 percent worked full-time before childbirth with an average firm tenure of just under five years. 90 percent of firms are located in West Germany. Average firm size amounts to 14 employees, and mothers are in workgroups with close to six workers (i.e., five internal substitutes), and just below two-thirds of employees in the firm are women. Most firms are in the service sector.

To assess how the selection criteria affect our sample, Appendix Table A1 compares the characteristics of mothers in our analysis sample with all excluded first-time mothers who gave birth in the two-year sample period. In particular, mothers in our sample are older at birth (30 vs. 28.6 years), have obtained higher education (39 percent vs. 31 percent), have higher monthly pre-birth earnings (2664 euro vs. 2125 euro), higher firm tenure (4.65 years vs. 3.8 years), and are more likely to work full-time pre-birth (94 percent vs. 82 percent). Consistent with the above differences, mothers in our analysis sample are more strongly attached to the labour market as reflected by the slightly higher shares of mothers who return to the labour market within one and three years after childbirth.

With respect to mothers' firms, Appendix Table A2 shows that firms included in our sample are substantially smaller compared to the excluded firms (14.5 vs. 90 employees in June 2006).[18] Firms in our sample are more likely to come from West Germany (90 percent vs. 82 percent), have slightly older employees (38.6 vs. 37.3 years), and pay higher median gross

[18]In the main table, this number is calculated ten months pre-birth, in Appendix Table A2 we get slightly different numbers as we use information from the Establishment History Panel (BHP) which is based on June 30, 2006, as our data does not cover all employees from dropped firms.

Table 1: Summary statistics and balancing

	Sample window					
	All	Jul-Dec 05	Jan-Jun 06	Jul-Dec 06	Jan-Jun 07	DD coef.
	Mean					
	(1)	(2)	(3)	(4)	(5)	(6)
Individual mother characteristics						
Age in years	29.963	29.788	30.135	29.845	30.112	-0.079
						(0.104)
German citizenship	0.958	0.960	0.954	0.962	0.957	0.001
						(0.005)
High education	0.390	0.373	0.390	0.389	0.410	0.003
						(0.013)
Annual earnings in year before birth	30539.930	31286.521	29951.461	31118.018	29674.762	-108.195
						(269.211)
Tenure at current firm in years	4.648	4.613	4.581	4.716	4.683	-0.002
						(0.098)
Full-time employed	0.940	0.943	0.942	0.938	0.937	0.000
						(0.006)
Pre-birth firm characteristics						
Location in West Germany	0.895	0.900	0.893	0.894	0.893	0.006
						(0.008)
Firm size	14.062	14.218	13.983	14.047	13.984	0.172
						(0.298)
Workgroup size	5.733	5.828	5.756	5.685	5.652	0.040
						(0.165)
Share of female employees	0.631	0.629	0.629	0.632	0.634	0.002
						(0.007)
Sector						
Agriculture, fishing and mining	0.013	0.013	0.013	0.012	0.013	0.001
						(0.003)
Manufacturing	0.125	0.127	0.121	0.125	0.126	0.006
						(0.009)
Electricity, gas, water	0.004	0.003	0.005	0.002	0.003	-0.001
						(0.002)
Construction	0.041	0.039	0.042	0.045	0.036	-0.013**
						(0.005)
Wholesale and retail	0.332	0.321	0.333	0.339	0.334	-0.017
						(0.012)
Hotels and restaurants	0.020	0.021	0.019	0.019	0.020	0.003
						(0.004)
Transport, storage, communication	0.051	0.047	0.054	0.054	0.051	-0.011*
						(0.006)
Financial intermediation	0.065	0.059	0.070	0.065	0.068	-0.008
						(0.006)
Real estate, renting, business activities	0.304	0.297	0.295	0.306	0.321	0.017
						(0.012)
Observations	23,679	6,360	5,680	6,003	5,636	23,679

Notes: Table shows pre-determined characteristics at the individual level of the mother and at her pre-birth firm. Mean values are presented in columns (1)-(5). The coefficients in column (6) are obtained from a difference-in-differences specification. Robust standard errors in parentheses. * < 10% ** < 5% *** < 1%
Source: IEB, own calculations.

wages (2564 euro vs. 2174 euro). With respect to the industry structure, we observe some small shifts, in particular that firms in our sample are less likely to come from manufacturing, and hospitality, and more likely to come from other services.

One potential concern that emerges from selecting firms with one birth

during a four-year period is that the reform effects on fertility may cause an endogenous sample selection bias. For example, if women were more likely to give births after the reform (in the medium-run), we would be more likely to exclude firms with more women of childbearing age. If this was an issue for our analysis, we would expect systematic differences in pre-determined observable characteristics between treatment firms and control firms, as treated firms were exposed longer to the new parental leave regulation during the period considered for our sample selection. However, we find no evidence for such systematic differences between treatment and control firms (see column (6) of Table 1 for balancing checks, and section 5.1 for further details).

3.4. Outcome Variables

We first study how the reform affects mothers' return to their pre-birth firm to characterise the employment gaps that an increase in parental leave causes. We take advantage of the daily level of the employment data and define binary indicators for mothers working at their pre-birth firm at the monthly level, allowing us to trace out the prolonged absence of mothers in detail. As earnings in the data are reported as a daily average over the reporting period of the employment spell (at most one calendar year), we cannot reliably calculate monthly earnings. Instead, we compute the annual earnings of mothers at their pre-birth firms and deflate earnings to a common base CPI of 2010.[19]

To measure firm outcomes, we use firms' employment level and their total wage bill, based on the following considerations: In labour markets with imperfect competition, employment generates a surplus that accrues partly to the worker and partly to the firm (for an overview, see Manning, 2011). *Ceteris paribus*, most importantly holding constant other inputs and the production technology, lower employment hence implies lower profits. We therefore use employment as our main firm-level outcome and measure it as the number of workers at a firm. We analyse employment—as for

[19]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 coworkers. Top-coded earnings are assigned the coding-threshold value, i.e., we cannot capture effects above the earnings maximum.

mothers—at the monthly level.[20] Because we do not have precise information on hours worked, we additionally examine firms' wage bill. If the number of hours worked by each worker increases in response to mothers' absences (an intensive margin response), the wage bill should respond less than total employment to the absence. The wage bill is however also affected by any wage changes necessary to increase other workers' labour supply, e.g., overtime pay, and the interpretation is therefore less clear than for employment. Analogous to mothers' earnings, we measure the wage bill of the firm at the annual level.[21] The aggregated wage bill serves as a proxy for output of the firm. To make the estimations comparable across firms of different size and across the outcomes, we consider all firm-level outcomes relative to the baseline period. We furthermore censor firm outcomes at the 99th percentile of the respective distribution to reduce the imprecision induced by outliers.

4. Firms and Parental Leave Absences

4.1. Replacement Hirings and Separations

Firms can either hire replacements or reduce separations to substitute absent mothers. We start with replacement hirings and consider hirings in the same occupation as the mother as potential replacements. To examine firm's behaviour in a stable institutional setting, we focus on births occurring prior to the reform in 2006.[22]

Panel A of Figure 2 plots the monthly number of hirings in mothers' workgroups from 24 months before birth up to 30 months after birth. Until six months prior to childbirth, firms constantly hire around 0.1 workers per month. Six months before childbirth, hirings start to gradually increase. This coincides with the end of the first trimester, at which a pregnancy is

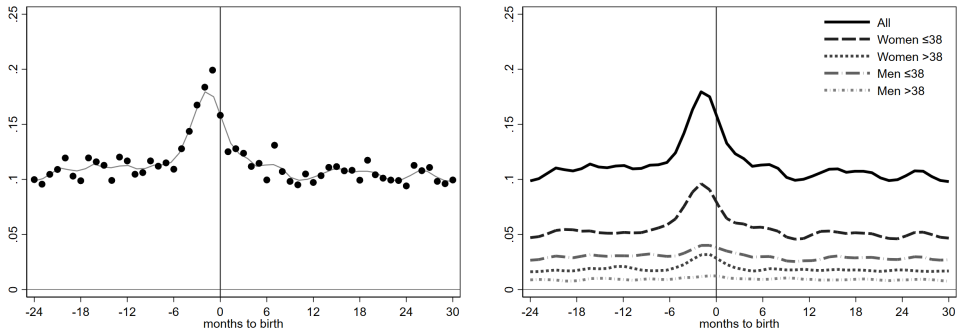
[20]The data allow for an analysis at the daily level, but we use a monthly aggregation to reduce computational demand.

[21]In contrast to the the Danish setting analysed in Brenøe et al. (2020), firms in Germany are not responsible for carrying out paid parental leave payments to mothers and this is accordingly not reflected in firms' wage bill.

[22]In Appendix Figure A4-A6 we show the same figures for the post-reform period in 2007 with very similar patterns.

typically considered safe and when mothers usually announce their pregnancy to their employers. The observed pattern shows that firms hire replacement workers from external labour markets and also allow for some transitional period before mothers go on leave, most likely to hand over tasks to ensure a smooth transition. We observe 0.28 excess hirings in the six months to birth compared to the same months one year before, i.e., not all mothers are replaced by firms (similarly, Jäger and Heining, 2019, find that less than half of deceased workers are replaced). In the period following women's childbirth, the average hiring rate returns to the pre-birth level.[23]

Figure 2: Hirings around childbirth



A: Replacement hiring around childbirth

B: Composition of replacement hirings

Notes: Figure shows hiring in the same occupation of mothers around childbirth. Mothers returning to their pre-birth workgroup are not counted as hirings. Numbers of hirings are cleansed of calendar month effects. Sample period is births from January 2006 to June 2006.
Source: IEB, own calculations.

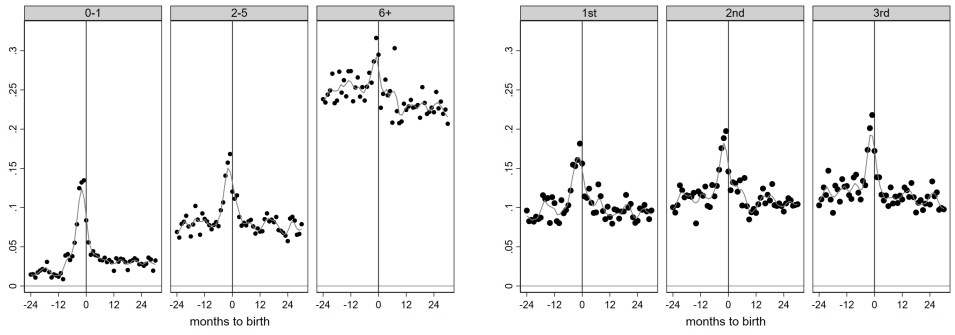
To examine the composition of the additional hirings, Panel B plots the evolution of hirings for four mutually exclusive demographic groups: men and women, above and below age 38. Almost half of replacement hiring comes from the same demographic group as mothers, i.e., young women, which corresponds to their share among hirings earlier and later. The relative uptick in hirings around childbirth is also stronger for older

[23] In Appendix Figure A7 we present hirings in all *other* occupations in the firm. While a small increase in hiring around childbirth is observed, pointing to cascade effects in the firm or some replacement hiring in other occupations, the increase is smaller in both absolute and relative terms, thus supporting our definition of internal substitutes. This is in line with Jäger and Heining (2019) who show that around three-quarters of hirings in respond to the death of a worker occur in the same occupation.

women than for men, suggesting that future mothers are more frequently replaced by women of all ages rather than by men.

Figure 3 investigates how firms' replacement hirings differ by the availability of internal and external substitutes. According to Panel A, the peak in the months leading up to childbirth is most pronounced when fewer internal substitutes are available. With more internal substitutes, the level of hirings is generally higher, in line with normal churning, and the replacement hirings are less pronounced. Excess hirings in small workgroups amount to 0.34 (0-1 substitutes) and 0.33 (2-5), but with more substitutes (6+) only 0.14 excess hirings occur. Panel B provides no evidence for substantial differences in replacement hirings between thick and thin labour markets. Thus, the availability of external substitutes does not appear to be a main hindrance for firms when trying to replace mothers.

Figure 3: Hirings by availability of substitutes



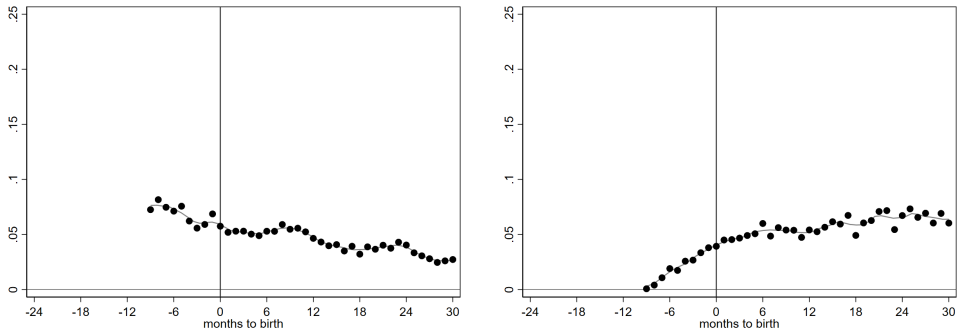
A: Number of internal substitutes (terciles) B: External substitutes (thickness terciles)

Notes: Figure shows residualised hiring by availability of internal and external substitutes. Internal substitutes are defined as the number of coworkers in the same occupation ten months pre-birth. External substitutes distinguished by terciles of labour market thickness of the occupation. Sample period is births from January 2006 to June 2006.
Source: IEB, own calculations.

Alternatively, firms may reduce separations of incumbent workers to keep the employment stock constant. We examine this in Panel A of Figure 4, where we plot the separations of incumbent workers from ten months prior to childbirth up to 30 months after childbirth. The figure shows that separations decline smoothly over time. In Panel B of Figure 4, we examine separations of non-incumbent workers, i.e., workers newly hired into the firm starting ten months prior to childbirth. Here, we observe a

steady increase in separations. Overall, the figure shows that separations are not reduced to substitute absent workers.

Figure 4: Separations by incumbents and replacements



A: Incumbents B: Replacements

Notes: Figure shows residualised separations of workers in the same occupation as mothers. Panel A shows separation events for incumbents, defines as coworkers ten months before birth. Replacements in Panel B are those hired afterwards. Sample period is births from January 2006 to June 2006.
Source: IEB, own calculations.

Given the relationship between the availability of substitutes and firms' replacement hirings, we next examine how the length of parental leave depends on the availability of internal and external substitutes in different institutional settings.

4.2. Mothers' Return Behaviour with Short Paid Parental Leave Benefits

We start in a policy environment with limited paid parental leave benefits, where all mothers receive full earnings replacements for the first two months after childbirth, but only some mothers receive a small amount of parental leave benefits three to six months after childbirth (i.e., births between January and June 2006, see section 2 for details).[24] We would expect that mothers with fewer internal substitutes return earlier to their pre-birth employer than mothers with more substitutes.

Panel A of Figure 5 provides the Kaplan–Meier plot for returning to the pre-birth employer, separately by the number of internal substitutes. The key finding that emerges is that parental leave is substantially shorter if

[24]We use births from January to June 2006 to be able to directly compare these later to births from January to June 2007 without potentially distorting calendar month effects.

only few internal substitutes are available. In particular, mothers with 0-1 substitutes return the earliest, while mothers with six or more substitutes are the slowest to return. Mothers in between these groups initially return like mothers with many substitutes, but then move towards mothers with few substitutes. All groups converge towards the child's third birthday, which coincides with the end of the job protection period. Panel B of Figure 5 shows return by the availability of external substitutes in the respective labour market region, split into terciles. In the first year after childbirth, mothers' return to the pre-birth employer is very similar across thin and thick labour markets. We then observe that mothers in thinner labour markets return slightly earlier than mothers in thicker labour markets in the second and third year after childbirth.

Given that workers' characteristics could correlate with the number of internal and external substitutes, the observed patterns could be driven by differences in worker, occupation or firm characteristics.[25]

To account for these differences, we estimate the following regression model:

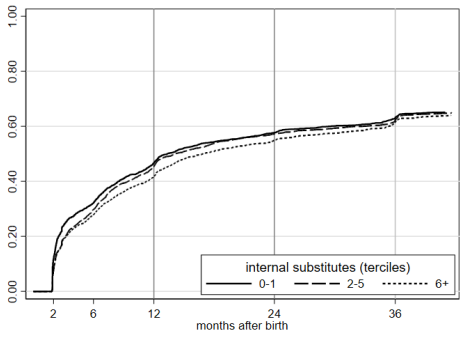
$$\begin{aligned} return_i^t = \alpha^t + \delta^t \ln\left(\text{internal substitutes}\right)_i \\ + \gamma^t \ln\left(\text{external substitutes}\right)_i + X_i'\beta^t + \epsilon_i^t \end{aligned} \tag{1}$$

where $return_i^t$ is a binary indicator for mother i to have returned to her pre-birth employer t months after giving birth where $t \in (3, ..., 42)$. We use this binary outcome rather than a continuous definition of days until return to trace out the relationship over time after birth without complications due to right-censoring. For our main specification, we use *ln(internal substitutes)* and *ln(external substitutes)* to estimate the correlation between mother's return behaviour and the availability of internal and external substitutes.[26] The vector X_i includes worker and firm characteristics determined ten months prior to childbirth, namely tenure (linear and squared), earnings (linear and squared), indicator variables for moth-

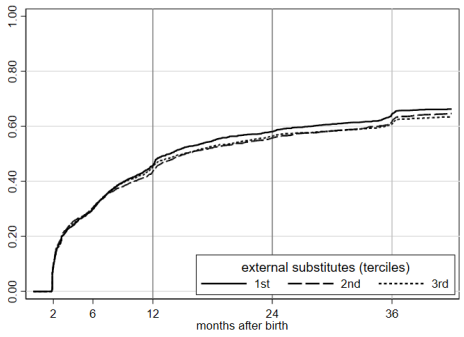
[25] See Appendix Table A3 for summary statistics which differentiate between different levels of internal substitutability and Appendix Table A4 which shows that individual and firm characteristics are balanced within those subsamples as well.

[26] The pattern is robust if we employ indicator variables for the number of internal substitutes as shown in Figure 5, see Appendix Figure A8.

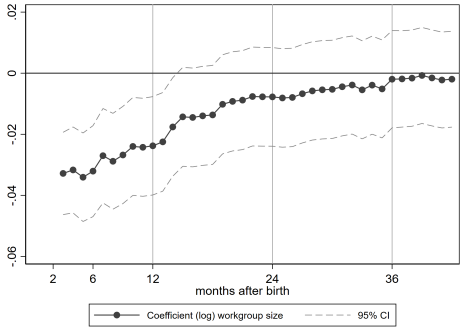
Figure 5: Return to pre-birth firm in setting with low paid leave entitlements



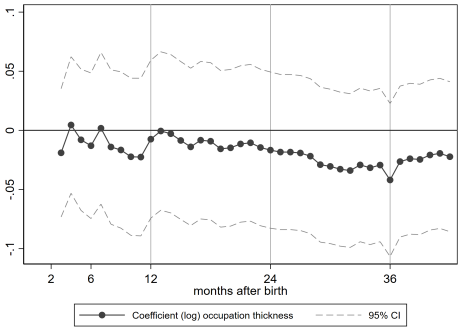
A: Kaplan-Meier by internal substitutes



B: Kaplan-Meier by external substitutes



C: Internal substitutes (estimates)



D: External substitutes (estimates)

Notes: Sample period is births from January 2006 to June 2006. Panels A and B show Kaplan-Meier failure functions of mother's return to their pre-birth firm. Panel A differentiates by the number of internal substitutes at her firm defined as coworkers in the same occupation. Panel B shows plots by external substitutes defined as the share of employees in the same occupation in the commuting zone relative to the national average (Jäger and Heining, 2019). Panels C and D show regression coefficients of binary indicators for mothers having returned to their pre-birth firm at different points in time. Each point estimate is based on a separate regression. Control variables include at the mothers' level: age dummies, education, citizenship dummy, tenure and pre-birth earnings (both linear and squared). We further include occupation FEs, log firm size, labour market region FEs (141) and the two variables plotted in the panels. 95% confidence level calculated with robust standard errors.
Source: IEB, own calculations.

ers' occupation, age at birth, education and citizenship, regional labour markets, and firm size in logs.

Panel C in Figure 5 presents the δ coefficients from eq. (1). The relationship between the availability of internal substitutes and mothers' return to their firm is strongest immediately after the expiration of maternity leave. A one percent increase in the number of internal substitutes is associated with an almost four percentage points lower probability to have returned three months after birth. Over time, the relationship gradually

weakens and becomes statistically insignificant. This relationship cannot be explained by differences in important worker, firm, and local labour market characteristics.[27] Panel D in Figure 5 presents the γ coefficients of eq. (1) and reveals no significant link between mothers' return to the firm and the availability of external substitutes in the labour market region in the first three years after childbirth.

Overall, the fewer internal substitutes are available, the shorter mothers go on leave. Additionally, firms make more replacement hirings when women with few internal substitutes become mothers.

4.3. Mothers' Return Behaviour with Extended Paid Parental Leave

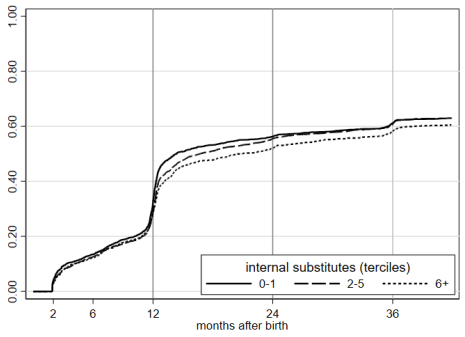
Next, we examine how the introduction of more generous paid parental leave affects the link between mothers' return and the availability of internal and external substitutes. Panels A and B of Figure 6 provide the Kaplan–Meier plots for returning to the pre-birth employer distinguishing by the availability of internal and external substitutes for mothers giving birth under the new paid parental leave regime (i.e., between January and June 2007).

The key finding that emerges from Panel A is that mothers with few internal substitutes now return at almost the same rate as mothers with more internal substitutes in the first year after childbirth. Thus, the extension of paid parental leave benefits in the first year after childbirth strongly reduced the previous differences in mothers' return-to-work behaviour by the number of internal substitutes. With respect to the availability of external substitutes, Panel B reveals no systematic differences in mothers' return related to the regional labour market thickness.

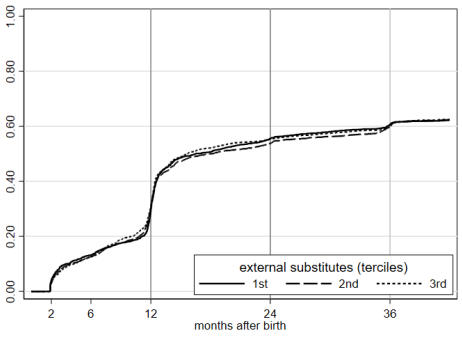
In Panels C and D of Figure 6, we condition on worker, occupation, and firm characteristics, as well as on labour market regions (see eq. (1)). Panel C shows that the link between the availability of internal substitutes and returning to work has weakened substantially and turned statistically insignificant during the first year after childbirth. Once mothers' paid leave expires after 12 months, the return-to-work pattern by the availability of internal substitutes returns to its pre-reform pattern. Thus, mothers who

[27] Appendix Table A5 shows that these relationships are robust to various choices of control variables.

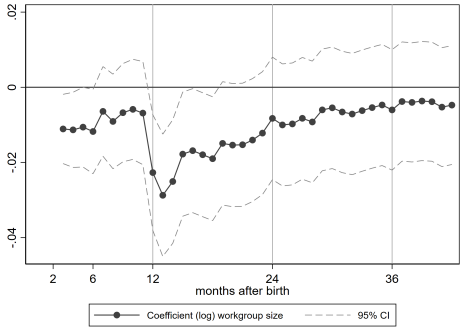
Figure 6: Return to pre-birth firm in setting with extended paid leave entitlements



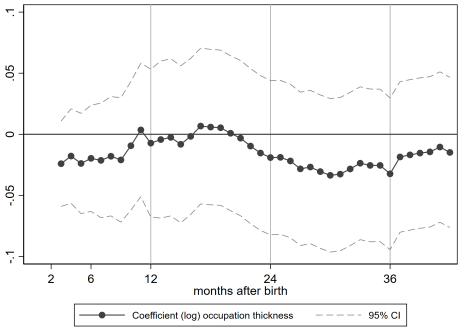
A: Kaplan-Meier by internal substitutes



B: Kaplan-Meier by external substitutes



C: Internal substitutes (estimates)



D: External substitutes (estimates)

Notes: Sample period is births from January 2007 to June 2007. See Figure 5 for other notes.
Source: IEB, own calculations.

are harder to replace internally again return more quickly to their previous employer compared to mothers with more internal replacements. Panel D shows that the availability of external substitutes is unrelated to mothers' return if additional control variables are included.

Overall, the introduction of paid parental leave distorted the coordination between employers and mothers on their return in the short-run. This may carry additional costs for firms to bridge the gap in the workforce. In the next sections, we will explore the effects of the parental leave extension on mothers' absences and firms' responses to this shock.

5. Effects of Extended Parental Leave Absences 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 dynamic difference-in-differences design (similar to Ginja et al., 2020). We describe the estimation strategy for the effects on mothers; we use the same estimation strategy for firms as we observe one birth per firm.

For the first difference, we compare outcomes between mothers giving birth up to six months before and after January 1, 2007. To account for seasonal variations and time trends in outcomes, we take a second difference using mothers giving birth one year earlier, i.e., up to six months before and after January 1, 2006. Moreover, we can use the dynamic evolution of outcomes as an additional difference. This allows us to examine the development of 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 mothers' monthly outcomes with the following regression model:

$$y_{it} = \sum_{t=-24}^{54} \gamma_t \mathbb{1}(T_t) \times reform_i \times spring_i + \sum_{t=-24}^{54} \delta_t \mathbb{1}(T_t) \times reform_i + \sum_{t=-24}^{54} \tau_t \mathbb{1}(T_t) \times spring_i + \sum_{t=-24}^{54} \beta_t \mathbb{1}(T_t) + \epsilon_{it} \tag{2}$$

where y is the outcome of mother i at event-time t; $t = 0$ corresponds to the month of birth.[28] The variable $reform_i$ takes the value of 1 if the mother gives birth between July 2006 and June 2007, and 0 otherwise. The variable $spring_i$ indicates whether a birth occurred between January and June of a year. We omit the event time dummy for $t = -10$, so that the coefficients γ_t estimate the treatment effect in each time period t relative

[28] We omit mother fixed effects from the regression equation, because we use a balanced panel and their inclusion does thus not affect our estimates.

to ten months prior to childbirth. We bin the endpoints on either side of the effect window (Schmidheiny and Siegloch, 2020). Standard errors are clustered at the mother-level. For earnings, we use annual earnings and calculate eq. (2) in calendar years and compare it to the pre-birth year.

To summarise the effect sizes, we also report the estimates for four discrete time bins. Specifically, we report effects for the *pre-birth period* (24 to 11 months before birth), as well as *short-term effects* (2 to 14 months after birth) covering the paid parental leave period, *medium-term effects* (15 to 36 months after birth) covering the remaining job protection period, and *longer-term effects* (37 to 54 months after birth). The period from ten months before birth up to the birth is the reference period.[29] We estimate the following regression:

$$y_{it} = \sum_{t=p,s,m,l} \gamma_t^d \times \mathbb{1}(D_t) \times reform_i \times spring_i + \sum_{t=p,s,m,l} \delta_t^d \times \mathbb{1}(D_t) \times reform_i + \sum_{t=p,s,m,l} \tau_t^d \times \mathbb{1}(D_t) \times spring_i + \sum_{t=p,s,m,l} \beta_t^d \times \mathbb{1}(T_t) + u_{it} \tag{3}$$

where γ_t^d denote the pre-birth (p), short- (s), medium- (m), and longer-term (l) effects.

To estimate the effects on firms, firms replace mothers as the unit of analysis and we define groups as of when the birth occurred in the firm.

Identifying assumptions. To interpret the γ coefficients as the causal effect of the 2007-reform, (i) selection into motherhood must not have changed, (ii) the timing of births around the policy cut-off needs to be as good as random, and (iii) the potential outcomes between treatment and control mothers and firms must follow common trends. The way the reform was passed and empirical findings on its fertility effects support the first assumption: Although the reform was first publicly discussed in May 2006, the final law was only passed in September 2006 (Kluve and Tamm,

[29] For the annual earnings estimates, the pre-birth period is two years before birth, short-term is the birth year and the following, medium-term is two to three years and longer-term is four years after birth. The calendar year preceding birth serves as the reference period.

2013). All births occurring until June 2007 had been conceived prior to the passing of the reform, such that parents could potentially react with conception only thereafter. In line with this, Raute (2019) observes first fertility responses only from August 2007 onward; as our sample only contains births until June 2007, differential selection into motherhood should not bias our estimates. We substantiate this point empirically in column (6) of Table 1, which reports the coefficients from difference-in-differences estimations. The coefficients reveal no systematic differences between the treatment and control groups in mothers' or firms' characteristics.

The second assumption is threatened if mothers shift the timing of births near the reform cut-off by postponing cesarean sections or labour inductions to benefit from the new regulation (Jürges, 2017, Neugart and Ohlsson, 2013, Tamm, 2013). To deal with this concern, we exclude women with expected dates of delivery in the two weeks before and after the cut-off and perform a density test for equally distributed births near the cut-off following Cattaneo et al. (2018). Our estimates reveal no evidence for any significant birth shifts (see Appendix Figure A9).

Third, a causal interpretation of our estimates requires that mothers and firms in the treatment and control groups follow a common trend in the evolution of their potential outcomes. As the potential outcomes are not observable, we assess pre-treatment trends in outcomes throughout the analysis and find no meaningful differences.

5.2. Worker Absences and Effects on Firms

We begin our quasi-experimental analysis by examining how the reform affected mothers' absences from their workplace after childbirth.[30] We estimate the reform effect on mothers' labour market outcomes using the dynamic difference-in-differences model outlined in eq. (2). Figure 7, Panel A, shows how the reform affected mothers' probability to be employed at

[30]Several other empirical studies have examined how the reform affected maternal labour market outcomes such as employment and earnings (e.g., see Kluve and Tamm, 2013, Kluve and Schmitz, 2018, Frodermann et al., 2020). For completeness, Appendix Table A6 reports comparable results for our sample of mothers where we consider employment at all firms. Our focus on the return to the pre-birth employer was also analysed in Kluve and Schmitz (2018), who find that high-earning mothers are more likely to return to their previous employers by 2 percentage points, and they are more likely to hold unlimited contracts.

their pre-birth employers. In the two years before childbirth, we estimate flat pre-trends, which supports our main identification assumption.[31] After childbirth, the parental leave reform substantially decreased mothers' probability to work for their pre-birth employers throughout the first year after birth (by a maximum of 20 percentage points ten months after birth).[32] We observe no meaningful medium- or longer-term differences in the probability to work at the same firm up to 54 months after childbirth, see also column (1) of Table 2 which summarises the estimates. These findings imply that the reform strongly increased mothers' parental leave absences in the first year after childbirth but had no effect on mothers' long-run absences, e.g., through effects on separations.

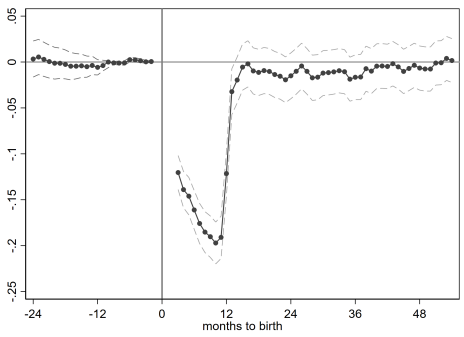
Panel B of Figure 7 presents the reform effect estimates on mothers' annual earnings at their pre-birth firms based on eq. (2). Treated mothers follow the same earnings trends within their firms prior to childbirth. Consistent with the longer absence after childbirth due to the reform, their earnings drop below those of the control group in the first two years after childbirth. In the following years, the earnings of treated mothers are above the earnings of mothers in the control group, but the difference is small (around 400 euro) and not statistically significant. Panel B of Table 2 presents the corresponding summary estimates in column (1).

Next, we examine how this negative, temporary labour supply shock affects firms' total employment and labour costs. We first examine the gap that mothers' absences create in the firm. In frictionless labour markets, we would expect that firms fully compensate the gap at the extensive margin through deferred separations or increased hirings. Panel C of Figure 7 examines employment at the firm and shows that employment is reduced after childbirth in firms exposed to longer maternal absences. Compared to ten months prior to childbirth, the parental leave expansion reduces employment within the first year after childbirth by around three percent

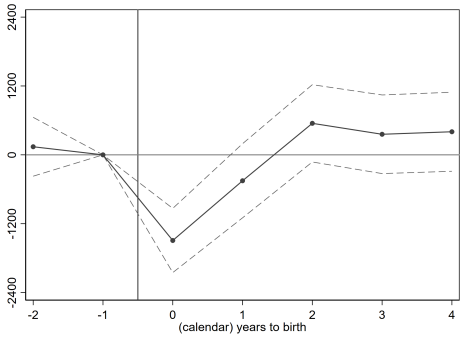
[31] As we condition our sample on mothers working in the same firm at least ten months prior to childbirth (see section 3), estimates at time $-10 \leq t \leq 0$ are deterministically close to zero.

[32] Figure 1 shows that about 34 percent of mothers giving birth prior to the reform returned to their pre-birth employer within the first six months, this share decreased by 20 percentage points, or 57 percent, for women giving birth after the reform.

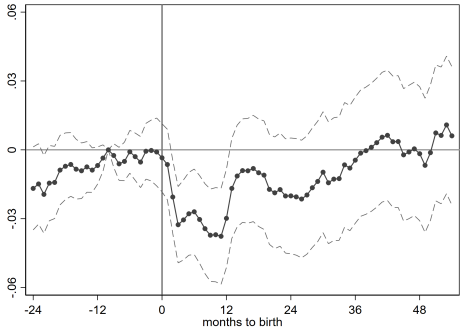
Figure 7: Event study of parental leave reform effects on mothers' and firms' outcomes



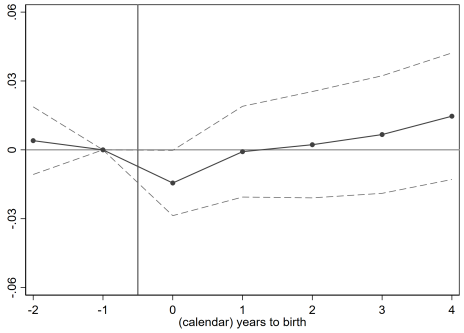
A: Mother's employment at pre-birth firm



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



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



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

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

in treated firms. The treatment effect turns insignificant 12 months after childbirth and converges to zero within three years after childbirth, that is after the expiry of the job protection period. Firms' total labour costs are not statistically significantly affected (Figure 7, Panel D), though the negative estimate in the year of childbirth suggests that mothers are not fully replaced. Column (1) of Table 3 provides corresponding short-, medium- and longer-term estimates.

Next, we examine treatment effect heterogeneities and analyse whether the effects on firms differ by the availability of internal substitutes for the mother on leave. Panel A of Figure 8 shows that the reform reduces total employment at the firm when at most one internal substitute is available.

Table 2: Summary event study estimates - mothers

		Internal substitutes		
	All	0-1	2-5	6+
Panel A: Employed at pre-birth firm				
Pre-period	-0.001	-0.003	0.015	-0.018
	(0.006)	(0.011)	(0.010)	(0.011)
Short-term effect	-0.132***	-0.148***	-0.132***	-0.109***
	(0.009)	(0.016)	(0.015)	(0.017)
Medium-term effect	-0.011	-0.006	-0.007	-0.022
	(0.011)	(0.019)	(0.018)	(0.021)
Longer-term effect	-0.002	-0.006	0.005	-0.007
	(0.011)	(0.019)	(0.019)	(0.021)
Mothers	23,679	8,624	8,504	6,551
Observations	2,415,258	879,648	867,408	668,202
Panel B: Annual earnings in calendar year at pre-birth firm				
Pre-period	140.873	73.032	556.394	-279.480
	(261.608)	(449.575)	(419.608)	(495.300)
Short-term effect	-971.978***	-1038.139**	-966.096**	-923.822
	(287.967)	(479.027)	(465.193)	(565.502)
Medium-term effect	453.893	687.969	273.447	345.682
	(334.729)	(558.677)	(540.398)	(656.514)
Longer-term effect	402.304	601.613	21.743	602.997
	(351.344)	(587.651)	(564.223)	(691.606)
Mothers	23,679	8,624	8,504	6,551
Observations	189,432	68,992	68,032	52,408

Notes: Table summarises event study estimates for the main outcomes of mothers in discrete time periods based on eq. (3). Estimates in Panel A are based on monthly information. Pre-birth is from 28 to 11 months pre-birth, the period from ten months pre- to one months post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in Panels C and D, pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to 0-1, 2-3 and 4 years after birth. Standard errors clustered at the mother level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.
Source: IEB, own calculations.

Table 3 shows that employment at these firm drops by about 3.4 percent in the first 14 months after childbirth and their wage bill drops by about 1.6 percent in the year of childbirth and the following year. The employment gap reduces over time and turns statistically insignificant in the medium- and longer-term.

With 2-5 internal substitutes (Panel C), firms' employment also drops by three percent when the mother goes on extended leave. This drop is similar to the effect on firms with fewer internal substitutes. We find no effect on the wage bill for this group (Panel D). However, in firms with 6 or more internal substitutes for the mother on leave, we do not observe any drop in employment or the wage bill (Panels E and F). Though the longer run estimates are less precise and not statistically significant, the

Table 3: Summary event study estimates - firm

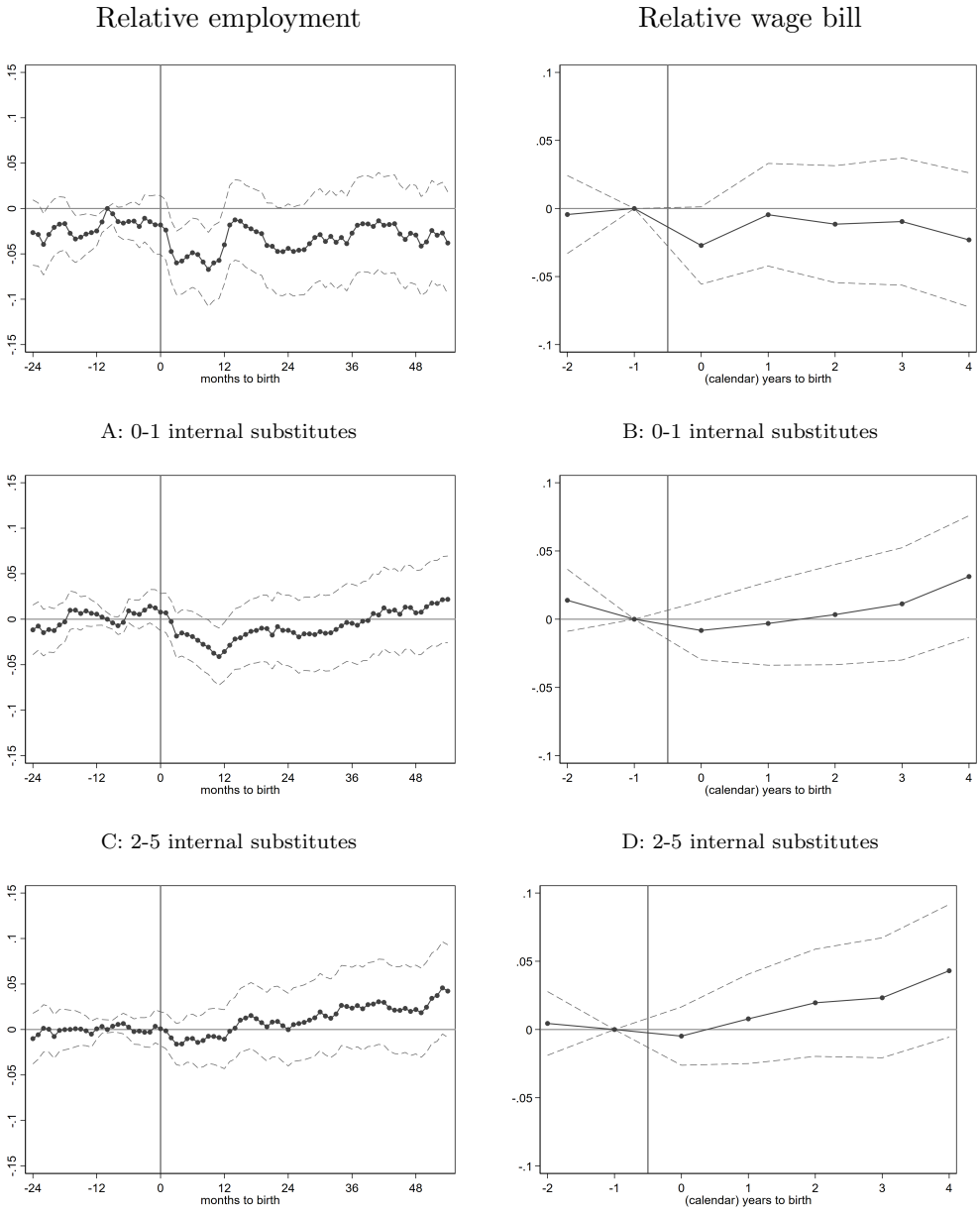
		Internal substitutes		
	All	0-1	2-5	6+
Panel A: Firm's relative employment				
Pre-period	-0.008	-0.012	-0.006	-0.003
	(0.007)	(0.014)	(0.010)	(0.010)
Short-term effect	-0.026***	-0.034**	-0.029***	-0.010
	(0.007)	(0.014)	(0.011)	(0.011)
Medium-term effect	-0.011	-0.021	-0.018	0.011
	(0.011)	(0.021)	(0.017)	(0.018)
Longer-term effect	0.007	-0.011	0.008	0.030
	(0.013)	(0.025)	(0.021)	(0.023)
Firms	23,679	8,624	8,504	6,551
Observations	2,415,258	879,648	867,408	668,202
Panel B: Firm's relative annual wage bill				
Pre-period	0.004	-0.004	0.014	0.004
	(0.008)	(0.015)	(0.012)	(0.012)
Short-term effect	-0.008	-0.016	-0.006	0.002
	(0.008)	(0.016)	(0.012)	(0.013)
Medium-term effect	0.004	-0.011	0.007	0.021
	(0.012)	(0.022)	(0.019)	(0.021)
Longer-term effect	0.015	-0.023	0.031	0.043*
	(0.014)	(0.025)	(0.023)	(0.025)
Firms	23,679	8,624	8,504	6,551
Observations	189,432	68,992	68,032	52,408

Notes: The table summarises event study estimates for the main outcomes of at the firm level in discrete time periods based on eq. (3). See Table 2 for other notes. Standard errors clustered at the firm level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.
Source: IEB, own calculations.

point estimates suggest that firms with six or more internal substitutes may have benefited from the parental leave reform.

Although the differences between the groups are not statistically significant, the point estimates indicate substantial effect heterogeneity depending on the number of internal substitutes for the mother. Overall, the point estimates support the conclusion that firms that have few internal substitutes available cannot fully close the labour shortage gap caused by longer parental parental leave absences. In our final section, we investigate whether firms internalise these costs through statistical discrimination.

Figure 8: Effect heterogeneity - event study of parental leave reform effects on firm outcomes



A: 0-1 internal substitutes

B: 0-1 internal substitutes

C: 2-5 internal substitutes

D: 2-5 internal substitutes

E: 6+ internal substitutes

F: 6+ internal substitutes

Notes: The figure plots event study estimates of the 2007 paid parental leave reform in Germany on firm outcomes based on eq. (2) (with 95% confidence interval), separately by the size of the workgroup in which the birth took place. The baseline month for employment effects is ten months prior to childbirth, and for the wage bill one calendar year prior to childbirth. Wage bill in 2010 euro.
Source: IEB, own calculations.

6. Effects of Extended Parental Leave on Hiring Decisions

We have shown that the paid parental leave expansion created a short-term gap in firms' employment when few internal substitutes are available for the mother-on-leave. We now analyse whether the paid parental leave expansion also affected the hiring composition of firms. We hypothesise that profit-maximising firms anticipate and internalise the potential costs of longer birth-related absences by younger female workers and in turn reduce the hiring of younger women.[33] Thus, finding that firms after the reform hire fewer women of childbearing age for workgroups with few internal substitutes would indicate statistical discrimination.

For this analysis, we modify the empirical approach and now study the hiring behaviour by firms in our sample into workgroups that are *not* directly affected by a birth.[34] The advantage of examining these workgroups is that they were not directly impacted by the childbirth occurring in another occupation in the same firm. Thus, their hirings should not (or to a lesser degree) be distorted by having to replace a mother going-on-leave. We distinguish between four mutually exclusive groups of hirings: By sex and by age, specifically above and below age 38. We consider women below age 38 as potential mothers as most women have completed their fertility by then.[35]

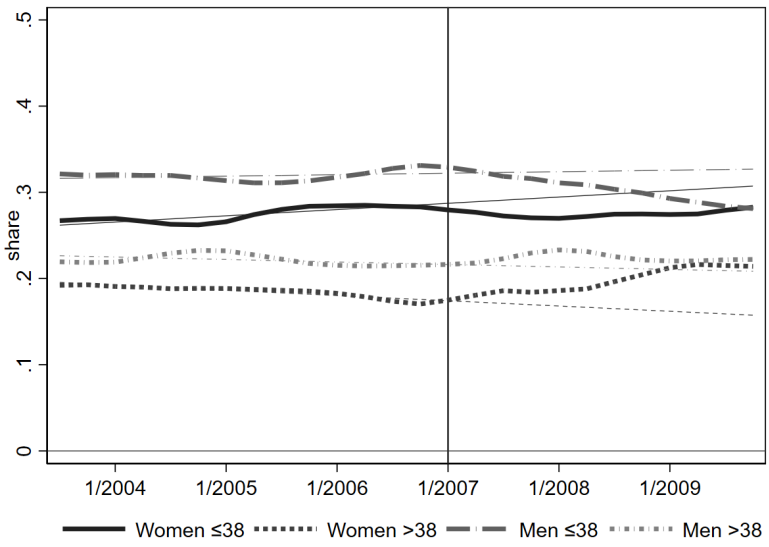
To illustrate the intuition of our approach, which follows Dobkin et al. (2018), we plot the composition of all hiring events for these groups by calendar time in Figure 9. For this figure, we net out calendar month effects and estimate a linear trend over the pre-reform period from July

[33]Statistical discrimination may not only affect actual or future mothers. Fernández-Kranz and Rodríguez-Planas (2021) find that a right for part-time work for young mothers had negative hiring effects on young childless women as well.

[34]We impose the condition that firms had at least one worker in a specific occupation on June 30, 2006, i.e., half a year before the parental leave extension. The condition that workgroups existed at a uniform reference date before the reform ensures that time-variant effects do not impact workgroups differently if the conditions is set on an existence at different time periods. In total, from July 2003 to December 2009 we observe 388,132 hiring events in 78,006 workgroups in 18,799 firms.

[35]According to the Federal Statistical Office, only 12 percent of births are from mothers above 38, most of which are higher order births. Following Müller and Strauch (2017), we use the same restriction of 38 years to identify first births in the administrative data.

Figure 9: Composition of hirings



Notes: Figure shows the residualised (calendar month effects are partialled out) demographic composition of all hirings at a quarterly level. Sample consists of all workgroups in sample firms, in which no birth is observed. Trend lines are calculated based on the period before the parental leave expansion in January 2007.
Source: IEB, own calculations.

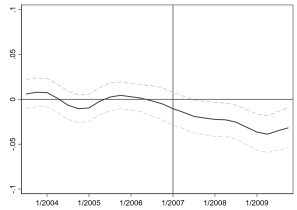
2003 to December 2006. We extrapolate this trend separately for each group over the entire sample period as the counterfactual trend in hirings.[36] To estimate the effect of the reform on the composition of hirings, we then estimate the monthly deviations from the pre-reform time trends.

Overall, Figure 9 shows that aggregate hiring shares of all demographic groups follow fairly linear trends in the pre-reform period. In the absence of the parental leave reform, we would expect hiring shares to continue along these paths. However, the aggregate figure already shows that the hiring patterns start to diverge from their pre-reform trend with the introduction of the parental leave reform. In particular, we observe that the hiring shares of young workers, both male and female, decline after the reform.

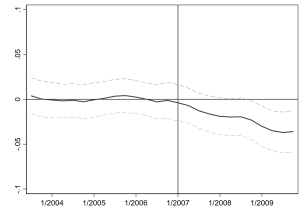
To disentangle whether these patterns depend on the availability of internal substitutes, Figure 10 plots the monthly deviations from the pre-reform time trend separately for each demographic group and by the avail-

[36]We weigh by the inverse of the number of hirings per workgroup, to give each group equal weight in this analysis (analogously to earlier analyses).

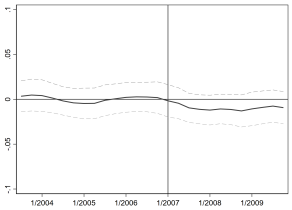
Figure 10: Trend deviations in hiring composition by demographic group and internal substitutes



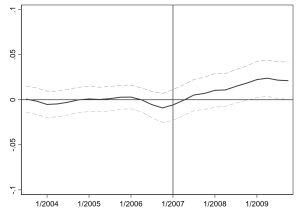
A: Women < 38, 0-1 substitutes



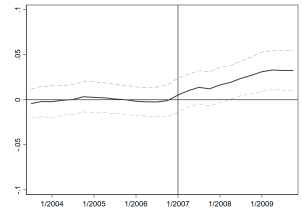
B: Women < 38, 2-5 substitutes



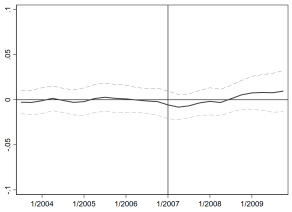
C: Women ≥ 38, 6+ substitutes



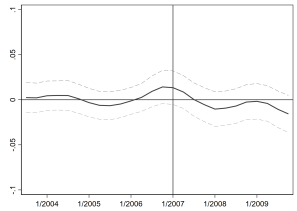
D: Women ≥ 38, 0-1 substitutes



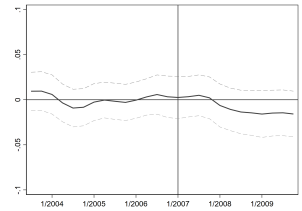
E: Women ≥ 38, 2-5 substitutes



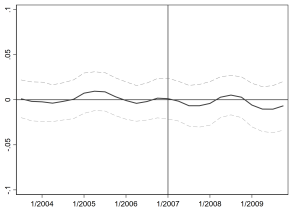
F: Women ≥ 38, 6+ substitutes



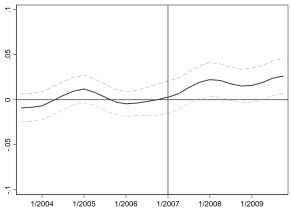
G: Men < 38, 0-1 substitutes



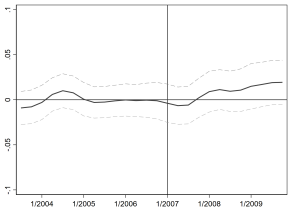
H: Men < 38, 2-5 substitutes



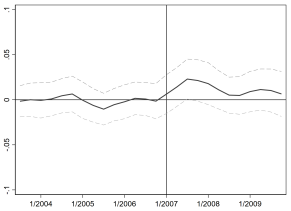
I: Men ≥ 38, 6+ substitutes



J: Men ≥ 38, 0-1 substitutes



K: Men ≥ 38, 2-5 substitutes



L: Men ≥ 38, 6+ substitutes

Notes: Figure shows quarterly deviations from a linear trend estimated over the pre-reform period (up to the 4th quarter of 2006) and extrapolated over the entire period. We include occupation FEs in the estimation for this figure.
Source: IEB, own calculations.

ability of internal substitutes. In these estimations, we control for occupation fixed effects given that men and women work in different occupations. We estimate flat pre-trends across all panels, which supports our identification strategy. Starting with Panel A, we find that the share of young women among new hirings declines in workgroups with fewer internal sub-

stitutes (Panels A and B). Conversely, we estimate an increase in the share of hirings for older women in firms with fewer internal substitutes (Panels D and E). These pattern are evidence for statistical discrimination. In contrast, we estimate no changes in the hiring shares of young or older women in firms with many internal substitutes (Panels C and F) which could reflect that these firms are better able to compensate the labour shortage. We do not find any effects for young men (Panels G-I), but observe small increases from their pre-reform time trend for older men (Panels J-L).

7. Conclusion

This paper takes a firm-side perspective on motherhood and parental leave yielding five key insights. First, firms hire more substitutes for the mother on leave when no or few internal substitutes are available (0.3 replacement hirings per mother) compared to firms where more internal substitutes are available. Second, mothers with few internal substitutes take shorter leave than mothers with more internal substitutes when parental leave benefits are limited. Third, this pattern is almost erased by a paid parental leave reform granting more generous benefits in the first year after childbirth. Fourth, the reform delays the return of mothers to their pre-birth employers and this reduces firms' employment in the short-term if they have few internal substitutes, but not in the longer-term. Fifth, firms responded to the reform by reducing the share of young women among hirings into occupations where few internal substitutes are available.

Taken together, our study shows that motherhood and parental leave policies burden firms in the short-term when few internal substitutes for the mother are available. Our analysis exploring statistical discrimination carries the important policy implication that parental leave policies, meant to improve the well-being of families, may backfire on potential mothers. To alleviate the effects of motherhood and parental leave on firms and reduce the scope for statistical discrimination, policymakers could additionally compensate firms for birth-related worker absences.

References

Adda, J., C. Dustmann, and K. Stevens (2017): "The career costs of children," *Journal of Political Economy*, 125, 293–337.

Azoulay, P., J. S. Graff Zivin, and J. Wang (2010): "Superstar extinction," *The Quarterly Journal of Economics*, 125, 549–589.

Bartel, A. P., M. Rossin-Slater, C. J. Ruhm, M. Slopen, and J. Waldfogel (2021): "The Impact of Paid Family Leave on Employers: Evidence from New York," Tech. rep., National Bureau of Economic Research.

Bennedsen, M., F. Pérez-González, and D. Wolfenzon (2020): "Do CEOs matter? Evidence from hospitalization events," *The Journal of Finance*, 75, 1877–1911.

Blossfeld, H.-P. (1987): "Labor-market entry and the sexual segregation of careers in the Federal Republic of Germany," *American Journal of Sociology*, 93, 89–118.

BMFSFJ (2008): "Elterngeld als Teil nachhaltiger Familienpolitik. Materialien aus dem Kompetenzzentrum für familienbezogene Leistungen im Bundesministerium für Familie, Senioren, Frauen und Jugend," .

——— (2018): "Family report 2017. Benefits, effects, trends." .

Brenøe, A. A., S. P. Canaan, N. A. Harmon, and H. N. Royer (2020): "Is Parental Leave Costly For Firms and Coworkers?" *NBER Working Paper Series*, w26622.

Cattaneo, M. D., M. Jansson, and X. Ma (2018): "Manipulation testing based on density discontinuity," *The Stata Journal*, 18, 234–261.

Cornelissen, T., C. Dustmann, and U. Schönberg (2017): "Peer effects in the workplace," *American Economic Review*, 107, 425–56.

Destatis (2008): "Öffentliche Sozialleistungen. Statistik zum Elterngeld. Elterngeld für Geburten 2007 Anträge von Januar 2007 bis Juni 2008." *Statistisches Bundesamt*, Wiesbaden.

——— (2018): "Unternehmensstrukturstatistiken und Statistik für kleine und mittlere Unternehmen nach dem EU-Unternehmensbegriff." *Statistisches Bundesamt*, Wiesbaden.

Dobkin, C., A. Finkelstein, R. Kluender, and M. J. Notowidigdo (2018): "The economic consequences of hospital admissions," *American Economic Review*, 108, 308–52.

Fernández-Kranz, D. and N. Rodríguez-Planas (2021): "Too family friendly? The consequences of parent part-time working rights," *Journal of Public Economics*, 197, 104407.

Frodermann, C., K. Wrohlich, and A. Zucco (2020): "Parental Leave Reform and Long-Run Earnings of Mothers," *IZA Discussion Papers*, No. 12935.

Gallen, Y. (2019): "The effect of parental leave extensions on firms and coworkers," *Working Paper*.

Ginja, R., A. Karimi, and P. Xiao (2020): "Employer Responses to Family Leave Programs," *Working Paper*.

Hensvik, L. and O. Rosenqvist (2019): "Keeping the Production Line Running - Internal Substitution and Employee Absence," *Journal of Human Resources*, 54, 200–224.

Hirsch, B. and S. Mueller (2012): "The productivity effect of temporary agency work: Evidence from German panel data," *The Economic Journal*, 122, F216–F235.

Huebener, M., D. Kuehnle, and C. K. Spiess (2019): "Parental leave policies and socio-economic gaps in child development: Evidence from a substantial benefit reform using administrative data," *Labour Economics*, 61, 101754.

Jacobebbinghaus, P. and S. Seth (2007): "The German integrated employment biographies sample IEBS," *Schmollers Jahrbuch*, 127, 335–342.

Jäger, S. and J. Heining (2019): "How Substitutable are Workers? Evidence from Worker Deaths," *Working Paper*.

Jaravel, X., N. Petkova, and A. Bell (2018): "Team-specific capital and innovation," *American Economic Review*, 108, 1034–73.

Jessen, J., R. Jessen, and J. Kluve (2019): "Punishing potential mothers? Evidence for statistical employer discrimination from a natural experiment," *Labour Economics*, 59, 164–172.

Jürges, H. (2017): "Financial incentives, timing of births, and infant health: a closer look into the delivery room," *The European Journal of Health Economics*, 18, 195–208.

Kleven, H., C. Landais, J. Posch, A. Steinhauer, and J. Zweimüller (2020): "Do family policies reduce gender inequality? Evidence from 60 years of policy experimentation," Tech. rep., National Bureau of Economic Research.

Kluve, J. and S. Schmitz (2018): "Back to work: Parental benefits and mothers' labor market outcomes in the medium run," *ILR Review*, 71, 143–173.

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

Kosfeld, R. and A. Werner (2012): "Deutsche Arbeitsmarktregionen – Neuabgrenzung nach den Kreisgebietsreformen 2007–2011," *Raumforschung und Raumordnung*, 70, 49–64.

Lalive, R. and J. Zweimüller (2009): "How does parental leave affect fertility and return to work? Evidence from two natural experiments," *The Quarterly Journal of Economics*, 124, 1363–1402.

Lazear, E. P. (2009): "Firm-Specific Human Capital: A Skill-Weights Approach," *Journal of Political Economy*, 117, 914–940.

Manning, A. (2011): "Imperfect competition in the labor market," in *Handbook of Labor Economics*, Elsevier, vol. 4, 973–1041.

Müller, D. and K. Strauch (2017): "Identifying mothers in administrative data," *FDZ-Methodenreport*.

Neugart, M. and H. Ohlsson (2013): "Economic incentives and the timing of births: Evidence from the German parental benefit reform 2007," *Journal of Population Economics*, 26, 87–108.

NYT (2021): "What's in Biden's Spending Plan: Free Preschool and National Paid Leave," 2021-04-28.

OECD (2016a): "OECD family database: Enrolment in childcare and preschool," *OECD - Social Policy Division - Directorate of Employment, Labour and Social Affairs*.

——— (2016b): "OECD family database: Maternal employment," *OECD - Social Policy Division - Directorate of Employment, Labour and Social Affairs*.

Raute, A. (2019): "Can financial incentives reduce the baby gap? Evidence from a reform in maternity leave benefits," *Journal of Public Economics*, 169, 203–222.

Rossin-Slater, M. (2018): "Maternity and Family Leave Policy," in *The Oxford Handbook of Women and the Economy*, ed. by S. L. Averett, L. M. Argys, and S. D. Hoffman, New York: Oxford University Press, 1–24.

Schmidheiny, K. and S. Siegloch (2020): "On event studies and distributed-lags in two-way fixed effects models: Identification, equivalence, and generalization," *ZEW-Centre for European Economic Research Discussion Paper*.

Schönberg, U. and J. Ludsteck (2014): "Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth," *Journal of Labor Economics*, 32, 469–505.

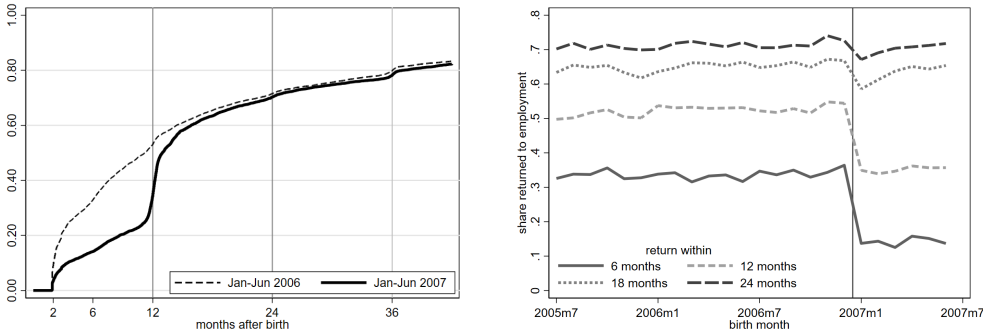
Tamm, M. (2013): "The impact of a large parental leave benefit reform on the timing of birth around the day of implementation," *Oxford Bulletin of Economics and Statistics*, 75, 585–601.

Thomsen, U., J. Ludsteck, and A. Schmucker (2018): "Skilled or unskilled-Improving the information on qualification for employee data in the IAB Employee Biography," *FDZ Methodenreport*.

Appendix (For Online Publication)

Figures

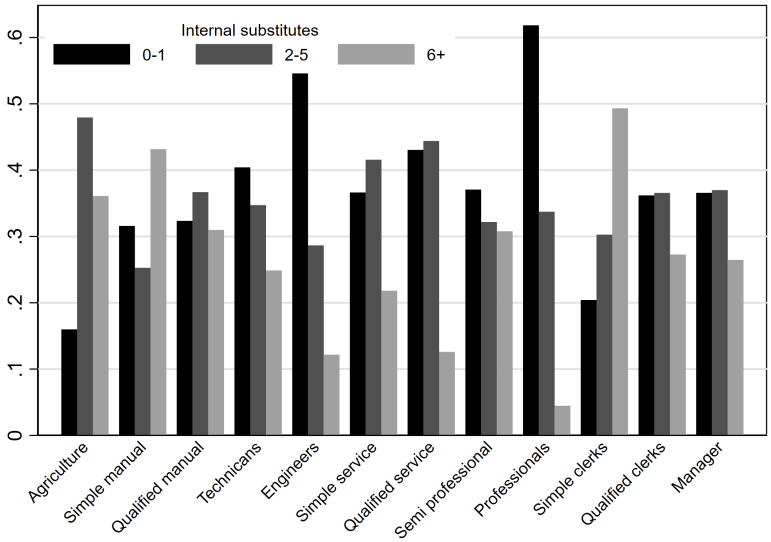
Figure A1: Return to employment before and after the 2007 parental leave reform



A: Share returned to work B: Share returned

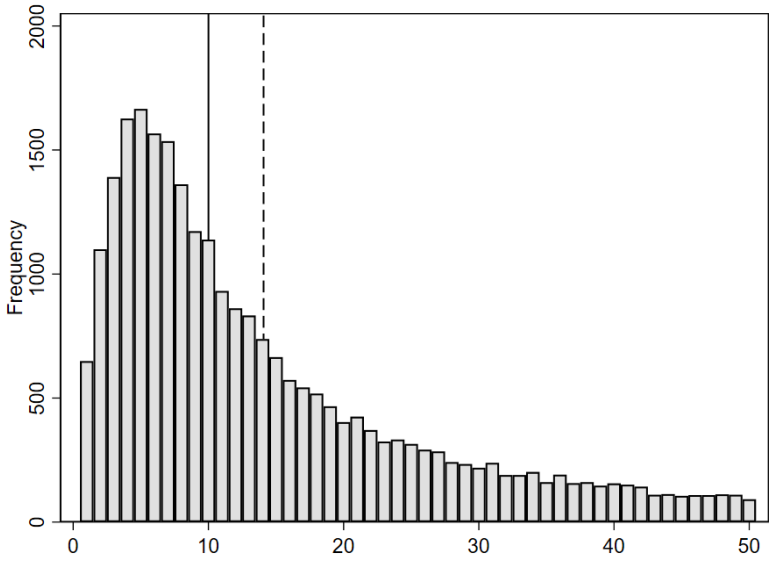
Notes: Panel A plots the share of mothers that have returned to employment by month t after childbirth. The dotted line indicates mothers giving birth between January and June 2006 (pre-reform), the solid line indicates mothers giving birth between January and June 2007 (post-reform). Panel B shows the share of mothers who have returned to employment at discrete points in time.
Source: IEB, own calculations.

Figure A2: Occupations by internal substitutes



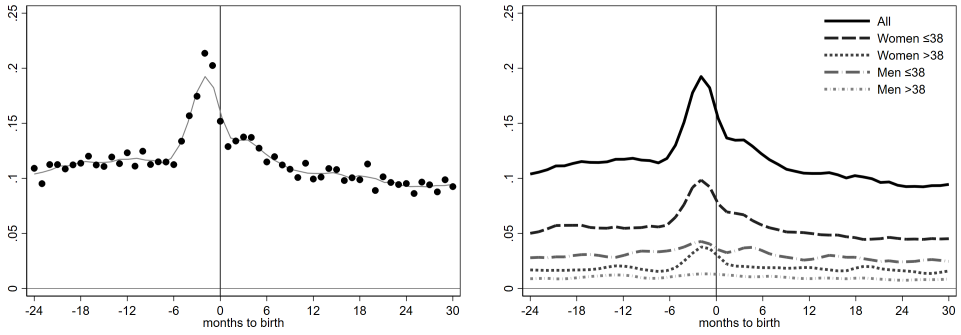
Notes: Figure shows mothers' occupations by the number of internal substitutes in her firm.
Source: IEB, own calculations.

Figure A3: Size of firms



Notes: Figure shows the distribution of pre-birth firm sizes. The solid line indicates the median firm size, the dashed line the mean size.
Source: IEB, own calculations.

Figure A4: Hirings around childbirth - 2007 births

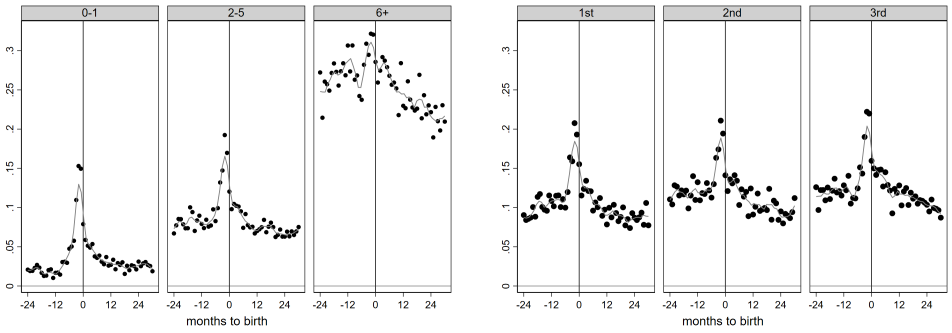


A: Replacement hiring around childbirth

B: Composition of replacement hirings

Notes: Figure shows hiring in the same occupation of mothers around childbirth. Mothers returning to their pre-birth workgroup are not counted as hirings. Numbers of hirings are cleansed of calendar month effects. Sample period is births from January 2007 to June 2007.
Source: IEB, own calculations.

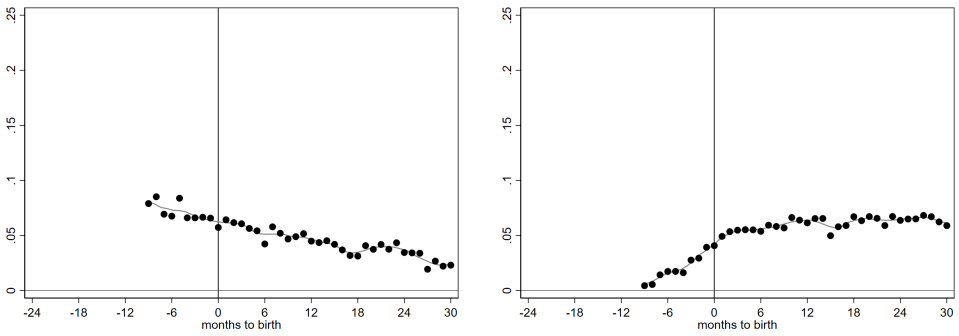
Figure A5: Hirings by availability of substitutes - 2007 births



A: Number of internal substitutes (terciles) B: External substitutes (thickness terciles)

Notes: Figure shows residualised hiring by availability of internal and external substitutes. Internal substitutes are defined as the number of coworkers in the same occupation ten months pre-birth. External substitutes distinguished by terciles of labour market thickness of the occupation. Sample period is births from January 2007 to June 2007.
Source: IEB, own calculations.

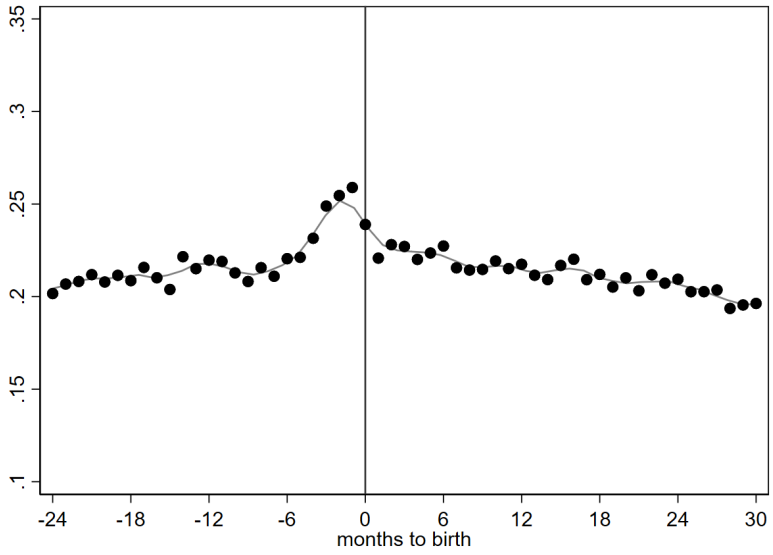
Figure A6: Separations by incumbents and replacements - 2007 births



A: Incumbents B: Replacements

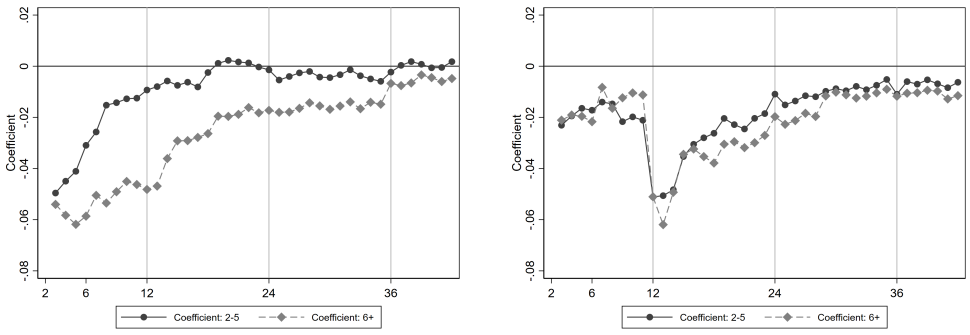
Notes: Figure shows residualised separations of workers in the same occupation as mothers. Panel A shows separation events for incumbents, defines as coworkers ten months before birth. Replacements in Panel B are those hired afterwards. Sample period is births from January 2007 to June 2007.
Source: IEB, own calculations.

Figure A7: Hirings around childbirth in other occupations



Notes: Figures shows hiring in mothers' firms in all other occupations. Number of hirings are cleansed of calendar month effects
Source: IEB, own calculations.

Figure A8: Return to pre-birth firm and availability of internal substitutes - discrete categorisation

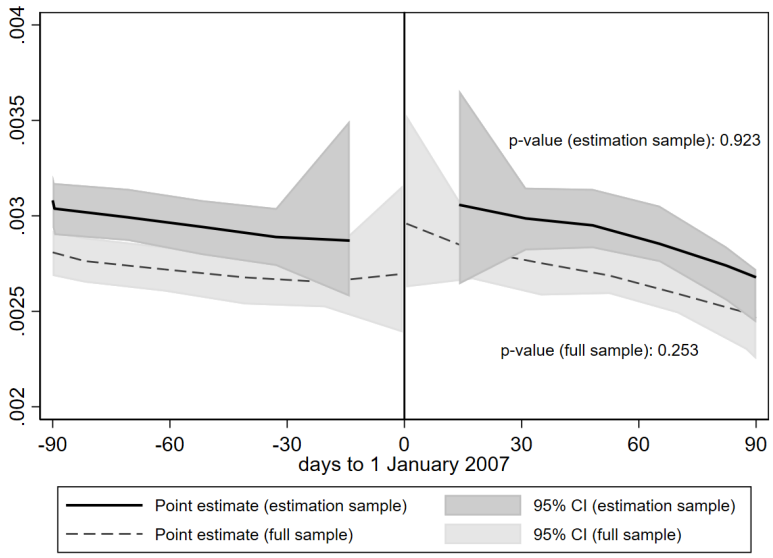


A: Low paid leave (Jan-Jun 2006)

B: Extended paid leave (Jan-Jun 2007)

Notes: Figure shows regression coefficients of binary indicators for mothers to have returned to their pre-birth employer at different points in time. In contrast to panel C of Figure 5 this figure shows estimates for the availability of internal substitutes using discrete categories of 2-5 and more than 6 (with 0-1 internal substitutes being the baseline) rather than the continuous definition in Figure 5. See Figure 5 for other notes.
Source: IEB, own calculations.

Figure A9: Density test of births



Notes: Figure plots the density of births around the introduction of the paid parental leave on 1 January 2007. The estimation sample excludes the two weeks around 1 January to avoid misassignment of births (see text). Density estimation based on Cattaneo et al. (2018).

Tables

Table A1: Comparison of mothers in analysis sample with excluded observations

	Analysis sample (1)	Dropped observations (2)
Age at birth	29.96	28.62
	(4.00)	(4.89)
German citizen	0.96	0.90
	(0.20)	(0.30)
High education	0.39	0.31
	(0.49)	(0.46)
Monthly earnings, ten months pre-birth	2,664.23	2,125.26
	(786.10)	(1367.14)
At same firm, ten months pre-birth	1.00	0.88
	(0.00)	(0.32)
Tenure at current firm in years	4.65	3.80
	(3.78)	(3.80)
Full-time employed	0.94	0.82
	(0.24)	(0.39)
Non-routine job	0.38	0.37
	(0.48)	(0.48)
Return to employment within one year	0.48	0.42
	(0.50)	(0.49)
Return to employment within three years	0.79	0.76
	(0.41)	(0.43)
Return to pre-birth firm within one year	0.41	0.34
	(0.49)	(0.47)
Return to pre-birth firm within three years	0.62	0.58
	(0.49)	(0.49)
Observations	23,679	197,995

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 in the analysis period (July 2005 - June 2007) identified in the data that were excluded. The sample restrictions leading to the exclusion are; employed at pre-birth firm ten months before birth, monthly earnings $\geq$ 1704 euro, one birth in firm in sample period, no public sector and no firms with more than 50 employees pre-birth. Dropped observations exclude mothers from public sector establishments.
Source: IEB, own calculations.

Table A2: Comparison of firms in analysis sample with excluded observations

	Analysis sample (1)	Dropped observations (2)
Characteristics		
Location in West Germany	0.90	0.82
	(0.31)	(0.38)
Number of employees	14.53	89.98
	(12.38)	(403.89)
Share of female employees	0.61	0.62
	(0.29)	(0.29)
Average age of full-time employees	38.57	37.30
	(5.94)	(6.15)
Median monthly earnings of full-time employees	2,563.39	2,174.32
	(1007.98)	(1092.55)
Sector		
Agriculture, fishing and mining	0.01	0.02
	(0.11)	(0.12)
Manufacturing	0.12	0.17
	(0.33)	(0.38)
Electricity, gas, water	0.00	0.00
	(0.06)	(0.07)
Construction	0.04	0.02
	(0.20)	(0.16)
Wholesale and retail	0.33	0.29
	(0.47)	(0.45)
Hotels and restaurants	0.02	0.09
	(0.14)	(0.29)
Transport, storage, communication	0.05	0.04
	(0.22)	(0.21)
Financial intermediation	0.07	0.04
	(0.25)	(0.20)
Real estate, renting, business activities	0.30	0.20
	(0.46)	(0.40)
Observations	23,679	109,591

Notes: Table compares firm characteristics of the analysis sample with those from observations that were dropped, see Table A1.
Source: IEB, own calculations.

Table A3: Summary statistics by internal substitutes

	Number of internal substitutes			
	0-1	2-5	6+	All
Individual characteristics				
Age in years	30.22	29.84	29.78	29.96
	(3.95)	(3.97)	(4.08)	(4.00)
German citizenship	0.96	0.96	0.95	0.96
	(0.19)	(0.20)	(0.21)	(0.20)
High education	0.44	0.37	0.35	0.39
	(0.50)	(0.48)	(0.48)	(0.49)
Annual earnings in year before birth	30,166.98	30,152.96	31,533.23	30,539.93
	(10597.15)	(9888.64)	(10499.92)	(10338.66)
Tenure at current firm in years	4.247	4.814	4.961	4.648
	(3.573)	(3.831)	(3.918)	(3.776)
Full-time employed	0.95	0.94	0.93	0.94
	(0.22)	(0.23)	(0.26)	(0.24)
Pre-birth firm characteristics				
Location in West Germany	0.88	0.90	0.91	0.90
	(0.32)	(0.30)	(0.29)	(0.31)
Firm size	9.56	12.64	21.84	14.06
	(9.59)	(10.15)	(11.44)	(11.47)
Workgroup size	1.45	4.17	13.40	5.73
	(0.50)	(1.08)	(7.53)	(6.33)
Share of female employees	0.646	0.638	0.602	0.631
	(0.312)	(0.285)	(0.254)	(0.288)
Sector				
Agriculture, fishing and mining	0.01	0.01	0.01	0.01
	(0.11)	(0.11)	(0.11)	(0.11)
Manufacturing	0.12	0.13	0.12	0.12
	(0.33)	(0.34)	(0.32)	(0.33)
Electricity, gas, water	0.00	0.00	0.01	0.00
	(0.05)	(0.05)	(0.08)	(0.06)
Construction	0.06	0.04	0.02	0.04
	(0.24)	(0.19)	(0.13)	(0.20)
Wholesale and retail	0.33	0.34	0.32	0.33
	(0.47)	(0.47)	(0.47)	(0.47)
Hotels and restaurants	0.02	0.02	0.02	0.02
	(0.13)	(0.14)	(0.14)	(0.14)
Transport, storage, communication	0.04	0.05	0.07	0.05
	(0.19)	(0.22)	(0.26)	(0.22)
Financial intermediation	0.03	0.06	0.12	0.07
	(0.17)	(0.24)	(0.32)	(0.25)
Real estate, renting, business activities	0.32	0.31	0.28	0.30
	(0.47)	(0.46)	(0.45)	(0.46)
Observations	8,624	8,504	6,551	23,679

Notes: Table shows pre-determined characteristics of the mother and her pre-birth firm by availability of internal substitutes.
Source: IEB, own calculations.

Table A4: Balancing by internal substitutes
(DD coefficients)

		Number of internal substitutes		
	All	0-1	2-5	6+
Individual characteristics				
Age in years	-0.079	-0.194	-0.092	0.083
	(0.104)	(0.170)	(0.172)	(0.202)
German citizenship	0.001	-0.001	-0.002	0.006
	(0.005)	(0.008)	(0.009)	(0.011)
High education	0.003	0.007	-0.004	0.004
	(0.013)	(0.021)	(0.021)	(0.024)
Annual earnings in year before birth	-108.195	-675.120	356.566	92.954
	(269.211)	(456.486)	(428.923)	(521.423)
Tenure at current firm in years	-0.002	0.194	-0.205	0.030
	(0.098)	(0.154)	(0.166)	(0.194)
Full-time employed	0.000	0.009	-0.022**	0.017
	(0.006)	(0.010)	(0.010)	(0.013)
Pre-birth firm characteristics				
Location in West Germany	0.006	-0.000	0.006	0.015
	(0.008)	(0.014)	(0.013)	(0.015)
Firm size	0.172	-0.118	0.268	0.584
	(0.298)	(0.414)	(0.441)	(0.566)
Workgroup size	0.040	-0.022	0.017	0.310
	(0.165)	(0.021)	(0.047)	(0.373)
Share of female employees	0.002	0.011	-0.003	-0.006
	(0.007)	(0.013)	(0.012)	(0.013)
Sector				
Agriculture, fishing and mining	0.001	0.003	0.003	-0.005
	(0.003)	(0.005)	(0.005)	(0.006)
Manufacturing	0.006	0.005	0.006	0.007
	(0.009)	(0.014)	(0.015)	(0.016)
Electricity, gas, water	-0.001	0.001	0.001	-0.006
	(0.002)	(0.002)	(0.002)	(0.004)
Construction	-0.013**	-0.025**	-0.010	0.001
	(0.005)	(0.010)	(0.008)	(0.006)
Wholesale and retail	-0.017	0.011	-0.035*	-0.030
	(0.012)	(0.020)	(0.021)	(0.023)
Hotels and restaurants	0.003	0.005	-0.002	0.007
	(0.004)	(0.006)	(0.006)	(0.007)
Transport, storage, communication	-0.011*	-0.009	-0.017*	-0.005
	(0.006)	(0.008)	(0.009)	(0.013)
Financial intermediation	-0.008	0.002	-0.010	-0.018
	(0.006)	(0.007)	(0.010)	(0.016)
Real estate, renting, business activities	0.017	0.001	0.034*	0.016
	(0.012)	(0.020)	(0.020)	(0.022)
Observations	23,679	8,624	8,504	6,551

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

Table A5: Relationship between availability of internal substitutes and parental leave length

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	January - June births							
Dep. variable: return within	2006	2007	2006	2007	2006	2007	2006	2007
3 months	-0.029***	-0.007*	-0.035***	-0.010**	-0.033***	-0.011**	-0.037***	-0.014**
	(0.006)	(0.004)	(0.006)	(0.004)	(0.007)	(0.005)	(0.009)	(0.006)
4 months	-0.024***	-0.007	-0.032***	-0.010**	-0.032***	-0.011**	-0.036***	-0.013**
	(0.006)	(0.004)	(0.006)	(0.004)	(0.007)	(0.005)	(0.009)	(0.006)
5 months	-0.026***	-0.007	-0.033***	-0.011**	-0.034***	-0.011*	-0.037***	-0.015**
	(0.006)	(0.005)	(0.006)	(0.005)	(0.007)	(0.005)	(0.009)	(0.007)
6 months	-0.024***	-0.008*	-0.032***	-0.011**	-0.032***	-0.012**	-0.034***	-0.018**
	(0.007)	(0.005)	(0.007)	(0.005)	(0.008)	(0.006)	(0.010)	(0.007)
7 months	-0.024***	-0.005	-0.031***	-0.009*	-0.027***	-0.006	-0.025**	-0.015**
	(0.007)	(0.005)	(0.007)	(0.005)	(0.008)	(0.006)	(0.010)	(0.008)
8 months	-0.025***	-0.008	-0.033***	-0.012**	-0.029***	-0.009	-0.028***	-0.015*
	(0.007)	(0.005)	(0.007)	(0.006)	(0.008)	(0.006)	(0.010)	(0.008)
9 months	-0.025***	-0.006	-0.033***	-0.010*	-0.027***	-0.007	-0.022**	-0.011
	(0.007)	(0.006)	(0.007)	(0.006)	(0.008)	(0.007)	(0.010)	(0.008)
10 months	-0.023***	-0.006	-0.030***	-0.010	-0.024***	-0.006	-0.019*	-0.011
	(0.007)	(0.006)	(0.007)	(0.006)	(0.008)	(0.007)	(0.010)	(0.009)
11 months	-0.025***	-0.008	-0.032***	-0.011*	-0.024***	-0.007	-0.018*	-0.014
	(0.007)	(0.006)	(0.007)	(0.006)	(0.008)	(0.007)	(0.010)	(0.009)
12 months	-0.025***	-0.019***	-0.033***	-0.025***	-0.024***	-0.023***	-0.017*	-0.029***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
13 months	-0.025***	-0.033***	-0.031***	-0.039***	-0.022***	-0.029***	-0.017	-0.033***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
14 months	-0.021***	-0.030***	-0.027***	-0.037***	-0.018**	-0.025***	-0.013	-0.026**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
15 months	-0.020***	-0.026***	-0.026***	-0.032***	-0.014*	-0.018**	-0.010	-0.019*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
16 months	-0.019***	-0.025***	-0.026***	-0.033***	-0.014*	-0.017**	-0.010	-0.018*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
17 months	-0.019***	-0.025***	-0.026***	-0.032***	-0.014*	-0.018**	-0.012	-0.017*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
18 months	-0.018**	-0.026***	-0.024***	-0.033***	-0.014*	-0.019**	-0.012	-0.018*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
19 months	-0.015**	-0.024***	-0.021***	-0.031***	-0.010	-0.015*	-0.011	-0.015
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
20 months	-0.014**	-0.024***	-0.020***	-0.031***	-0.009	-0.015*	-0.009	-0.015
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
21 months	-0.014**	-0.023***	-0.020***	-0.030***	-0.009	-0.015*	-0.009	-0.015
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
22 months	-0.014*	-0.022***	-0.019***	-0.029***	-0.008	-0.014*	-0.008	-0.015
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
23 months	-0.015**	-0.020***	-0.020***	-0.027***	-0.008	-0.012	-0.008	-0.013
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
24 months	-0.014*	-0.018**	-0.018***	-0.025***	-0.008	-0.008	-0.007	-0.009
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
25 months	-0.014**	-0.018**	-0.019***	-0.026***	-0.008	-0.010	-0.007	-0.013
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
26 months	-0.012*	-0.017**	-0.017**	-0.025***	-0.008	-0.010	-0.007	-0.013
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
27 months	-0.011	-0.016**	-0.016**	-0.024***	-0.007	-0.008	-0.005	-0.011
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
28 months	-0.011	-0.016**	-0.016**	-0.024***	-0.006	-0.009	-0.004	-0.012
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
29 months	-0.011	-0.014**	-0.015**	-0.021***	-0.005	-0.006	-0.004	-0.009
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
30 months	-0.010	-0.014*	-0.015**	-0.021***	-0.005	-0.005	-0.003	-0.009
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
31 months	-0.009	-0.015**	-0.013*	-0.022***	-0.004	-0.007	-0.001	-0.010
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
32 months	-0.008	-0.015**	-0.012*	-0.022***	-0.004	-0.007	-0.001	-0.010
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
33 months	-0.009	-0.014**	-0.014*	-0.021***	-0.005	-0.006	-0.003	-0.009
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
34 months	-0.007	-0.013*	-0.012*	-0.020***	-0.004	-0.005	-0.003	-0.008
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
35 months	-0.009	-0.013*	-0.014**	-0.019***	-0.005	-0.005	-0.004	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
36 months	-0.007	-0.014*	-0.012*	-0.020***	-0.002	-0.006	-0.001	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
37 months	-0.007	-0.011	-0.012*	-0.018***	-0.002	-0.004	-0.003	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
38 months	-0.007	-0.011	-0.012*	-0.018***	-0.002	-0.004	-0.003	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
39 months	-0.006	-0.011	-0.012*	-0.017**	-0.001	-0.004	-0.002	-0.006
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
40 months	-0.006	-0.010	-0.012*	-0.017**	-0.002	-0.004	-0.002	-0.006
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
41 months	-0.006	-0.012*	-0.012*	-0.019***	-0.002	-0.005	-0.003	-0.008
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
42 months	-0.006	-0.011	-0.012*	-0.018**	-0.002	-0.005	-0.003	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
Individual controls			Y	Y	Y	Y	Y	Y
Labour market thickness			Y	Y	Y	Y	Y	Y
Occupation FEs					Y	Y	Y	Y
(Log) firm size							Y	Y
Observations	5,680	5,636	5,679	5,633	5,679	5,633	4,910	4,873

Notes: Table regression coefficients of binary indicators for mothers to have returned to their pre-birth employer at different points in time. Each point estimate is based on a separate regression. Individual controls: age dummies, education, citizenship dummy, tenure and pre-birth earnings (both linear and squared), and occupation FEs. Labour market thickness: (log) occupation and sector thickness of labour market region. Estimates of columns 5-6 are plotted in Panel C of Figure 5. Robust standard errors in parentheses. * $< 10\%$ ** $< 5\%$ *** $< 1\%$.
Source: IEB, own calculations.

Table A6: Summary event study estimates - mothers - employment at any firm

		Internal substitutes		
	All	0-1	2-5	6+
Panel A: Employed				
Pre-period	-0.002	-0.004	0.003	-0.004
	(0.003)	(0.006)	(0.005)	(0.005)
Short-term effect	-0.157***	-0.175***	-0.160***	-0.129***
	(0.009)	(0.016)	(0.015)	(0.017)
Medium-term effect	-0.005	0.007	-0.006	-0.019
	(0.010)	(0.017)	(0.017)	(0.020)
Longer-term effect	0.011	0.035**	-0.014	0.011
	(0.010)	(0.017)	(0.017)	(0.020)
Mothers	23,679	8,624	8,504	6,551
Observations	2,415,258	879,648	867,408	668,202
Panel B: Annual earnings in calendar year				
Pre-period	-338.123*	-548.526*	-107.958	-331.242
	(172.954)	(307.238)	(277.925)	(309.212)
Short-term effect	-1349.159***	-1329.301***	-1303.258***	-1476.589***
	(266.316)	(448.312)	(426.642)	(519.195)
Medium-term effect	709.847**	1360.123**	290.144	353.258
	(327.543)	(555.344)	(525.388)	(632.918)
Longer-term effect	687.465**	1877.771***	-243.651	318.223
	(347.236)	(591.582)	(556.142)	(668.018)
Mothers	23,679	8,624	8,504	6,551
Observations	189,432	68,992	68,032	52,408

Notes: Table summarises event study estimates for the main outcomes of mothers in discrete time periods based on eq. (3). Estimates in Panel A re based on monthly information. Pre-birth is from 28 to 11 months pre-birth, the period from ten months pre- to one months post-birth is the omitted period. Short-, medium- and longer-term refer to 2-14, 15-36 and 37-58 months post-birth, respectively. For the annual estimation in Panels B, pre-birth is two calendar years before birth, we omit the year before and short-, medium- and longer-term refer to 0-1, 2-3 and 4 years after birth. Standard errors clustered at the mother level in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.
Source: IEB, own calculations.