How Do Firms Respond to Parental Leave Absences?*

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Abstract

How do firms adjust their labor demand when a female employee takes temporary leave after childbirth? Using Austrian administrative data, we compare firms with and without a birth event and exploit policy reforms that significantly altered leave durations. We find three main results. First, firms respond primarily through anticipatory hiring, while medium-run effects on total employment and the wage bill are small. Second, these aggregate adjustments differ sharply by gender: replacement hiring is overwhelmingly female, while the more persistent internal adjustment is concentrated among incumbent men. Third, while longer leave entitlements substantially extend actual leave absences, we do not find systematic differences in firms' labor demand responses across leave regimes.

JEL classification: J13, J16, J18, J23

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1 Introduction

Childbirth typically creates a temporary workplace absence for mothers. From the firm's perspective, this raises important questions about labor demand adjustments. First, how do firms adjust to such a temporary reduction in labor supply? They could replace the absent worker with a new hire, or they could reallocate the workload to existing workers. The chosen adjustment depends crucially on the costs and frictions firms face when searching for a suitable replacement. A second question is who absorbs that adjustment inside the firm: since the temporary absence is female by construction, do firms' responses lead to a gendered reallocation of work? Third, do firms' labor demand responses change when temporary leaves last longer? To the extent that long leaves imply greater disruptions, longer leaves may lead firms to adjust labor demand more strongly.

This paper addresses these questions. In a perfect labor market, a temporary worker absence does not impose costs on firms: firms can hire a temporary replacement and dismiss the replacement when the absent worker returns. In a more realistic setting, hiring, training, and firing costs (Oi, 1962) may make temporary leaves costly, especially when leaves are longer. Consistent with this argument, employer representatives often strongly oppose extensions of government-mandated parental leaves, even when income replacement benefits are government funded, and leave extensions therefore do not directly increase firms' labor costs. At the same time, muted average effects on total employment or the wage bill do not imply a gender-neutral response. A firm can absorb a temporary absence without large medium-run changes while still shifting hiring and retention in different ways across women and men.

Our analysis focuses on the Austrian labor market, which is particularly interesting for two reasons. First, due to generous parental leave policies, leave absences after childbirth are very long compared to other countries; if extended leaves impose significant costs on firms, Austria may offer an upper bound for firms' labor demand responses to temporary leave. Second, Austria implemented drastic parental leave policy changes during our sample period, providing an ideal setting to study how leave duration affects firms' labor demand responses. Using the Austrian social security register database (ASSD), a matched employer-employee dataset covering the entire population of private-sector workers, we compare firms with and without a birth event and study reform-induced changes in expected leave duration.

We find three main results. First, firms with a parental leave absence respond primarily through anticipatory hiring. Consistent with this, headcount employment and the wage bill rise before the birth, decline in the short run, and show only limited medium-run effects.

Second, the response is sharply gendered. Firms rely mainly on female new hires for immediate replacement, whereas the more persistent internal adjustment is concentrated among incumbent men. Thus, the aggregate firm response masks a gendered reallocation within firms. This pattern does not simply mirror baseline firm composition, but neither does it fit a simple story in which firms avoid hiring women after a leave spell, since immediate replacement hiring is itself heavily female.

Third, we find that firms' labor demand responses do not systematically change with the duration of government-mandated parental leave. While the reforms of 1990 and 1996 substantially extended or shortened actual leave-taking, we do not find systematic differences in the main firm-level outcomes across longer and shorter leave entitlements.

Our paper relates to two strands of the literature. First, it relates to work on how firms adjust labor demand in response to changes in their economic environment. Hamermesh and Pfann (1996) provide an early review of this literature. Azoulay et al. (2010), Jaravel et al. (2018), Becker and Hvide (2022), Jäger et al. (2024), and Sauvagnat and

Schivardi (2024) investigate the effects of unexpected worker exits on firms and coworkers. Hamermesh and Biddle (2024) examine the relative importance of labor demand adjustments via new hires compared to changes in existing workers' hours. Silva and Toledo (2009), Gavazza et al. (2018), and Muehlemann and Strupler Leiser (2018) examine hiring costs and how they vary across the business cycle. These studies typically find substantial costs associated with recruiting or replacing workers.[1] Our evidence differs from much of this literature because the leave spell is anticipated and because muted average effects coexist with a persistent gendered reallocation within the firm. The results also speak to work on gender segregation, firm pay-setting, and job substitutability within firms (Sorensen, 1990; Blau and Kahn, 2017; Carrington and Troske, 1995; Card et al., 2016; Azmat et al., 2025).

Second, our paper contributes to the growing firm-side literature on parental leave. Existing studies show that firms adjust to parental-leave absences through replacement hiring and internal reallocation, with effects depending on whether reforms are anticipated and on worker substitutability (Ginja et al., 2023; Gallen, 2019; Brenøe et al., 2024; Huebener et al., 2025; Schmutte and Skira, 2025; Machado et al., 2024). Related evidence also points to gender-related shifts in workforce composition (Corekcioglu et al., 2025). We add evidence on who within the firm absorbs the adjustment: replacement hiring is overwhelmingly female, whereas the more persistent incumbent response tilts toward men. This contribution links the firm-side parental-leave literature to gender segregation and within-firm allocation. We also show that even large reform-induced changes in leave-taking do not materially alter firms' observed medium-run adjustment margins in Austria.

[1] Recent literature also examines how the need for replacement hires affects firms' vacancy posting behavior (Mercan and Schoefer, 2020; Elsby and Gottfries, 2022; Elsby et al., 2024; Mueller et al., 2024).

2 Institutional Background and Data

Institutional background. Austria provides generous support for mothers around the birth of a child. Obligatory maternity leave starts 8 weeks before and ends 8 weeks after the birth, during which mothers are prohibited from working and generally receive a full wage replacement.[2] Following maternity leave, parental leave begins. Throughout the paper, we use parental-leave absences to refer to mothers' combined maternity leave and subsequent parental leave unless stated otherwise. While parental leave is formally available to either parent, take-up among fathers was almost zero during our sample period; accordingly, we focus on mothers' parental leave absences throughout the paper. During this period, mothers' jobs are protected, and they are entitled to a fixed cash benefit funded by the government (the benefit corresponds to about 40% of median net earnings). Firms do not bear any direct costs when an employee takes parental leave. During our sample period, mothers on leave could work only below the earnings threshold for "minor employment."[3] Firms looking to replace them faced few legal barriers: hiring on temporary contracts was established practice and legally unproblematic as long as the contract was explicitly tied to a specific worker's leave.

The parental leave reforms of 1990 and 1996. In Section 4.4, we exploit two reforms to the Austrian parental leave system that led to significant changes in leave duration and thus define three distinct parental-leave regimes.[4] Before July 1990, job protection and cash benefits were granted until the child's first birthday (1-year regime). Between July

[2] Maternity leave payment (Wochengeld) is partly funded by employee contributions, and partly by taxes and public health insurance.

[3] Earnings limits under "geringfügige Beschäftigung" ranged from €164 to €289 per month between 1985 and 2000.

[4] Neither of the two reforms could be anticipated by mothers who gave birth around the reform dates, because affected mothers were already pregnant when the reforms were passed. For firms, however, the reforms were known before the affected births occurred, and therefore before firms made the relevant replacement decisions.

1990 and June 1996, job protection and cash benefits were extended to the child's second birthday, increasing the maximum duration by 12 months (2-year regime). Between July 1996 and June 2000, benefit duration for mothers was effectively reduced to 18 months because 6 of the 24 benefit months were reserved for fathers, while job protection remained at 24 months; with almost zero take-up among fathers, we refer to this period as the 1.5-year regime. [5] Lalive and Zweimüller (2009), Lalive et al. (2014) and Kleven et al. (2024) show that these reforms had a significant impact on mothers' employment and parental leave take-up, but no lasting effect on child penalties.

Data and Outcome Variables. We use Austrian social security data (ASSD).[6] The ASSD is a matched employer-employee dataset that covers the earnings and employment history of all private-sector workers and their employers in Austria. A key advantage is that the ASSD provides information on employment and parental leave take-up at the daily level, allowing us to identify the precise timing and length of temporary leaves within firms.

We aggregate our main firm-level variables to the quarterly level. *Days of parental leave*: the sum of all female employees' days of maternity and parental leave at the firm. *Quarterly Hiring*: the number of workers who join the firm.[7] *Employees*: the number of workers employed by the firm, i.e., headcount employment (excluding workers on parental leave). *Wage bill*: the sum of all employees' earnings from the firm (in euro, 2000

[5] We intentionally omit the 2000 reform—which effectively extended parental leave eligibility from 1.5 to 2.5 years—because it does not provide comparable, clean duration variation. It was implemented retrospectively (enacted January 1, 2002, but affecting births from July 1, 2000), so firms could not anticipate it and had already made replacement decisions, raising selection concerns. It also coincided with changes to part-time work rules and earnings limits.

[6] See Zweimüller et al. (2009). The Data Appendix C provides more details on the data and variable construction.

[7] A worker is counted as a quarterly hire if he/she is not employed at the firm in quarter $t-1$ and is employed in quarter t. Women on parental leave are treated as continuously attached to the firm for this purpose, so returning mothers are not counted as quarterly hires here. This differs from the decomposition in Section 4.3, where new workers are defined relative to presence in the baseline quarter.

CPI). *Average daily wage*: Total wage bill divided by total days of employment (excluding workers on parental leave) at the firm. If an outcome is not observed because of firm closure, we set it to zero.

3 Sample and Research Design

Our goal is to study how firms respond when a worker gives birth and takes leave, creating a temporary reduction in labor supply at the firm.[8] Simply comparing firms experiencing a birth to those that do not potentially leads to biased effects from endogenous mobility. This bias would occur if women systematically switched to certain types of firms after they learned about their pregnancy. Because of this concern, we select women-firm pairs one year before the birth and treat these baseline firms as treated even if some women move before birth. This anchors the design to the firm that first faces the pregnancy shock. This is an intention-to-treat effect, and we scale our estimates by the first stage so that the coefficients can be interpreted as the effect of an additional birth in the baseline firm. The first stage is the effect of the pregnancy shock on the number of births in the baseline firm. In practice there is little mobility in the year before birth, so the scaled-up estimates are close to the reduced form. Because the difference is small, we omit the distinction between shock and treatment for brevity.

Control and treatment groups. The timing of the pregnancy shock is arguably random, but firms whose workers are more likely to give birth are not on average the same as firms where this is less likely. To ensure the treatment and control woman-firm pairs are comparable, we implement a matched sampling procedure following Azoulay et al. (2010) and Jäger et al. (2024). We first restrict the sample to women who are childless

[8] We use "a birth" and "taking leave" interchangeably because live births trigger at least 16 weeks of maternity leave by law. For details see Appendix C.

before quarter $k = b$ and select the treatment group from women who give birth in quarter $k = b$. The control group is sampled from women observed in $k = b$ who do not give birth between $k = b$ and $k = b + 4$, meaning they remain childless for at least one year after the "placebo birth" quarter. The firm subject to the pregnancy shock is the one where the woman works in the baseline quarter $k = b - 4$ (one year before the birth), and the same is true for the control group. For treated pairs that have multiple potential matches, we randomly select up to three control pairs and assign equal weights such that the total weight of these control units sums to one. Our treatment and control "events" therefore comprise woman-firm-baseline quarter combinations.

Sample restrictions. In both the treatment and the control groups, we restrict our sample to private-sector firms with 3–30 employees and Austrian nationals aged 18–40 who are childless at baseline and observed between 1985 and 2000. Additional restrictions are described in Appendix C.

Final sample. Our analysis sample comprises 33,779 first births in 24,032 unique firms. Appendix Table B.1 reports baseline-quarter summary statistics for treated and control groups, for women and their firms, overall and across parental-leave periods. Overall, mothers are on average 23 years old, earn €3,574 quarterly, and have been with the firm for 3.8 years. Their firms are 12.6 years old and have 8.8 employees, of whom 82% are women.

Empirical strategy. To quantify how firms respond when a female employee gives birth and takes leave, we estimate the impact of one additional birth in the baseline firm, together with the associated leave spell, using the following dynamic difference-in-differences equation:

$$Y_{ek} = \gamma_e + \boldsymbol{\alpha} \boldsymbol{D}_{ek}^{Event} + \boldsymbol{\beta} \boldsymbol{D}_{ek}^{Event} \times Birth_e + \epsilon_{ek}, \tag{1}$$

where Y_{ek} is outcome Y for event e (woman-firm-baseline quarter) in quarter k. γ_e represents event-specific fixed effects, and $\boldsymbol{D}_{ek}^{Event}$ indicate quarterly event-time dummies from 10 quarters before through 20 quarters after the birth event. We cluster standard errors at the firm level to address potential serial correlation in outcomes over time. Following Brenøe et al. (2024), we instrument an additional birth in the event quarter in the firm ($\boldsymbol{D}_{ek}^{Event} \times Birth_e$) with the interaction of the pregnancy event and the event quarter (see Appendix C.2 for details). Under IV estimation, the β-coefficients capture the difference in how outcomes change when there is an additional birth in treated firms compared to control firms, relative to the baseline quarter (the last quarter before the female employee becomes pregnant).

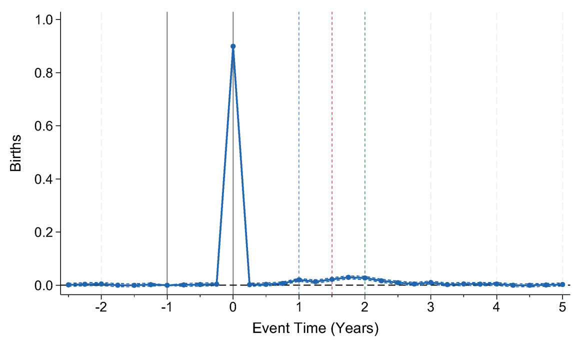
For interpretation, we refer to the three quarters before birth ($-3 \leq k \leq -1$) as the anticipation period, the two years after birth ($0 \leq k \leq 8$) as the short run, and the period thereafter through five years after birth ($9 \leq k \leq 20$) as the medium run. The employer typically learns about the pregnancy during the anticipation period. There is no law governing when mothers have to announce the pregnancy, but maternity protection legislation only applies from the time when the employer knows. The Arbeiterkammer, the organization representing workers' interests in Austria, recommends informing the employer as soon as the pregnancy is known.

The identifying assumption is that the timing of when the employer learns about the pregnancy is unrelated to expected future firm performance (parallel trends). We assess this by examining trends in outcomes before the employer knew about the pregnancy.

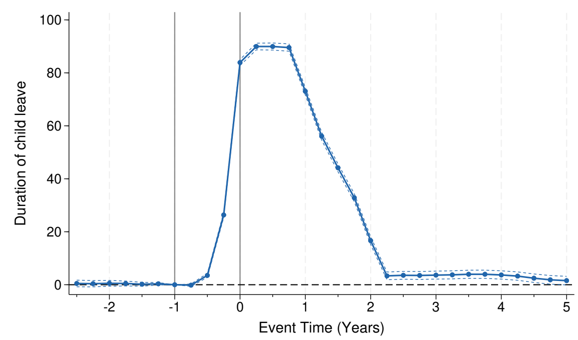
First-stage dynamics and parental leave take-up. Figure 1 presents the first stage in terms of births and the associated parental-leave take-up dynamics.[9] Panel (a) shows the first stage for the event-study design: a sharp increase in births at event time 0 (around 0.9), while estimated differences at later event times are very small, suggesting limited differential follow-up fertility between treated and control firms. Panel (b) plots the corresponding event-time response in parental-leave take-up, measured by total days of parental leave at the firm-quarter level and pooled across regimes. Because parental-leave days enter as an outcome in equation (1), these estimates are already scaled to one additional birth. This panel shows a pronounced increase in parental leave days around the birth quarter and persistent post-event differences that reflect actual leave-taking.

Figure 1
First-Stage Dynamics: Births and Leave Uptake

(a) Births in Baseline Firm



(b) Days of Parental Leave



Notes: Each panel plots the β-coefficients from equation (1), capturing differential changes in treated firms relative to control firms, compared to the baseline quarter. Panel (a) uses the number of births in the baseline firm as the outcome. Panel (b) reports total days of parental leave at the firm-quarter level, scaled to one additional birth and pooled across regimes. Event time is measured in quarters relative to birth. Dashed lines indicate 95% confidence intervals.

[9] Appendix Figure A.5 documents focal mothers' return to employment at the baseline firm.

4 Firm Responses and Gendered Reallocation

4.1 Aggregate Firm Responses

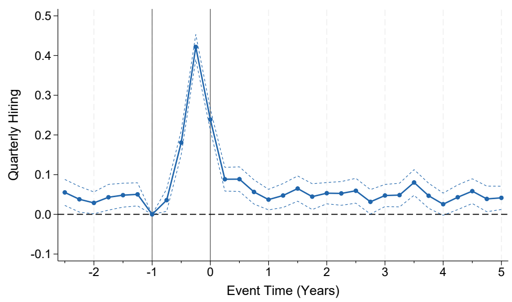
Firms begin adjusting their hiring as soon as they learn that a pregnant employee will soon go on leave. As shown in panel a) of Figure 2, the number of quarterly hires in treated firms rises already two quarters before the birth and peaks in the quarter right before birth. After the birth, excess hiring declines again and returns toward its baseline level. Taken together, the coefficients from $k = -3$ to $k = 0$ imply 0.88 excess hires in treated firms relative to control firms, pointing to substantial anticipatory replacement hiring.

Panel b) of Figure 2 shows the effect of an additional birth on headcount employment. Before the baseline quarter, we observe no effect on firm size, which supports the validity of our empirical design. Employment increases substantially during the anticipation period, consistent with anticipatory replacement hiring. In the quarter of birth (when all expectant women are on leave), headcount employment in treated firms falls below that in control firms before rebounding in the subsequent quarters and stabilizing approximately a year after birth. Thereafter, employment remains slightly above the level in control firms, with a statistically significant difference of 0.13 employees. However, higher headcount does not necessarily imply higher labor input, because former full-time workers may be replaced by part-time workers and mothers often return part-time. Consistent with this, panel c) shows a decline in daily wages. Appendix E provides additional evidence supporting the interpretation that the persistent increase in headcount is driven by a higher prevalence of part-time employment.

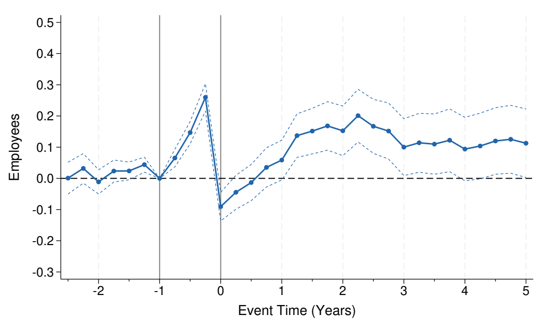
Panel d) of Figure 2 shows a similar pattern for firms' total wage bill. The wage bill in treated firms already starts to exceed that of control firms in the anticipation period. In the quarter before birth, the difference is about €960, corresponding to about 27% of

Figure 2
Dynamic Effects of an Additional Birth and Leave-Taking on Firm Outcomes

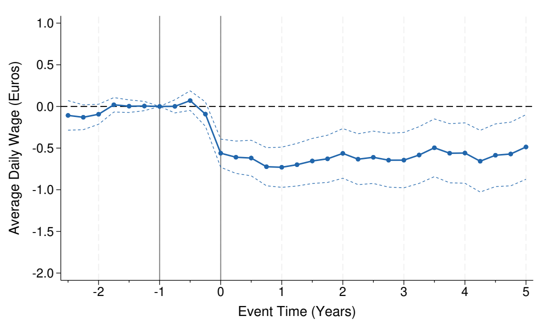
(a) Quarterly Hiring



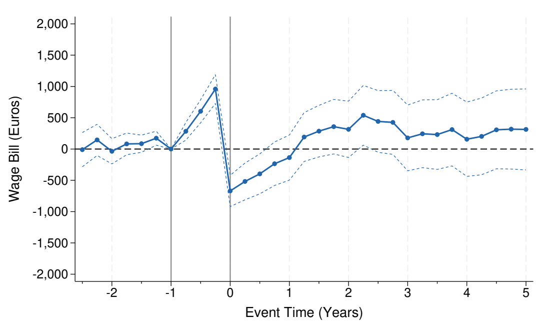
(b) Employees



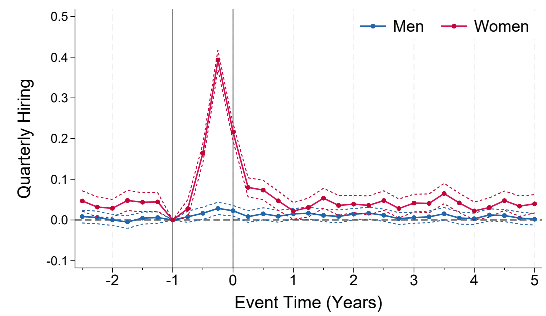
(c) Average Daily Wage



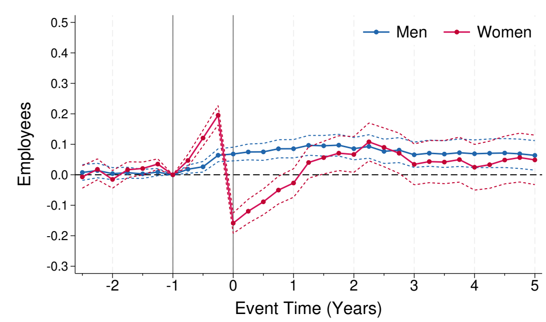
(d) Wage Bill



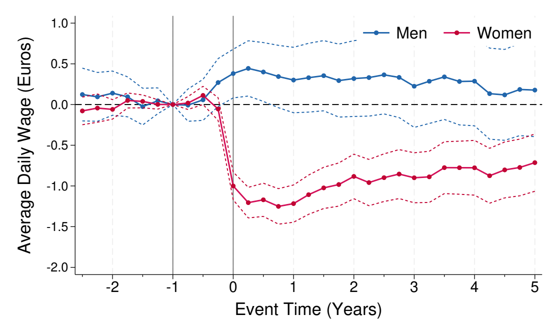
(e) Quarterly Hiring by Gender



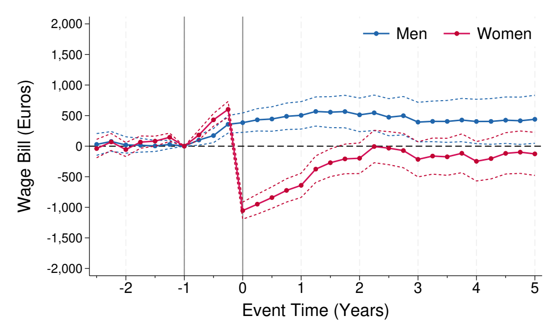
(f) Employees by Gender



(g) Average Daily Wage by Gender



(h) Wage Bill by Gender



Notes: Each panel plots the β-coefficients from equation 1, capturing the differential changes in treated firms' outcomes compared to control firms, relative to the baseline quarter. Coefficients are scaled to one additional birth in the baseline firm. Left-hand panels show outcomes for all employees, and right-hand panels show outcomes by gender (blue for men and red for women). Outcomes are described in Section 2. Dashed lines indicate 95% confidence intervals.

the average quarterly earnings of a future mother at baseline. In the birth quarter and the following two quarters, treated firms have a lower wage bill compared to control firms. Afterwards, the total wage bill does not differ significantly between treated and control firms over the remaining event window. Aggregate effects on labor costs are therefore concentrated in the short run. We also do not find effects on the likelihood of firm closure (Appendix F provides the details).

4.2 Gendered Reallocation Patterns

The gender-specific panels of Figure 2 show that these aggregate patterns mask gender differences in the observed response pattern. Panel e) separates quarterly hires by gender. If the additional hiring were not strongly gendered, the additional hires would be more evenly split between women and men. However, panel e) reveals a pronounced gendered pattern: nearly all of the additional hires in treated firms are female. Only a small minority are male (9% in the anticipation period and 17% in the short run), a pattern consistent with gender segregation within the firm in the allocation of workers to specific jobs or tasks.

Panel f) shows how an additional birth changes the gender composition of the firms' headcount employment. Female employment in treated firms rises in the anticipation period, then declines sharply around the birth and takes around a year to recover. The temporary decrease in female employment is partially offset by a persistent small increase in male employment. In the medium run, men account for around half of the remaining increase in headcount caused by the additional birth at the firm.

Panel g) shows a decrease in the average daily wages of women after the birth event and mostly unchanged daily wages of men. The former is likely the result of more part-time work at treated firms, both by mothers and their replacement workers (see Appendix E for a more detailed discussion). The decrease in the average daily wages of women persists, suggesting a shift in the structure of labor inputs in treated firms. This is also evident from the effect on the wage bill, shown in panel h). In line with the results for employment and wages, we see a strong short-run decrease in the female wage bill and a slight increase in the male wage bill. In relative terms compared to gender-specific baseline wage bill levels, the female wage bill decreases by 2.5% and the male wage bill increases by 3.6% in the short run. In the medium run, the remaining wage-bill response is tilted toward men, even though the total wage bill for all employees is not affected. Appendix Figures A.1 to A.4 show qualitatively similar patterns when we split the sample by the temporarily absent employee's previous wage, tenure, or wage rank within the firm. The same basic gendered pattern appears across these subsamples.

Overall, Figure 2 shows that firm adjustment of total labor input operates through both employment and wages, but it does not separate their contributions to the wage-bill response. In Appendix D, we formally decompose the wage bill effects into changes in daily wages and employment to understand whether one is more important than the other. We find that the increase in the wage bill in anticipation of the birth is driven mainly by the employment component, consistent with firms preparing for the leave spell by hiring workers to be trained as replacements. In the short and medium run, the aggregate wage bill remains close to zero because firms operate with more employment days at lower daily wages. This compositional shift is also gendered: the short-run decline in the wage bill of female workers is driven mainly by the wage component, whereas the positive wage-bill response of male workers is driven mainly by the employment component.

4.3 Decomposition of Firm Responses

Figure 3 decomposes the overall employment headcount and wage-bill responses into focal workers (mothers and matched control women), incumbents, and new workers.[10] Panel (a) shows that the change in employment in response to the leave of the focal worker is mainly driven by new workers. The firm hires in anticipation of the birth and those new workers stay while the mother is on leave, with some remaining in the medium run. We fix incumbents at baseline, which means the small positive effect on incumbent employees visible in panel (a) reflects slightly increased retention of existing workers. As panels (b) and (c) show, the small retention effect is driven by male incumbents, while female incumbent retention is almost zero. The wage bill response in Panel (d) shows a similar pattern but higher wage payments to incumbents are quantitatively more important, especially in the first two years after birth with a significant increase of about 1.3% relative to the baseline wage bill (see Appendix Tables B.2 and B.4 for effects by period). New workers almost fully replace the mother on leave and this persists into the medium run. The overall insignificance of the wage bill effect is due to a highly significant decrease among focal workers and a persistent compensating increase in payments to new workers.

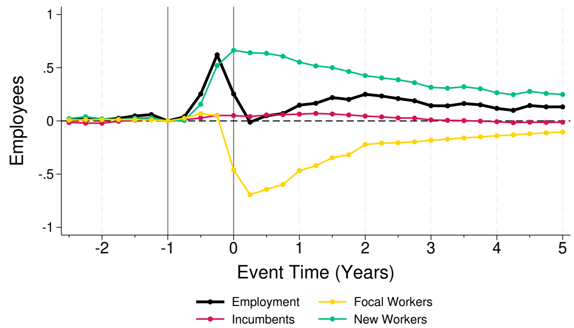
The gendered decomposition in panels (b)–(f) reveals an asymmetry that evolves over time. During the anticipation and short-run periods, the aggregate wage-bill response to the birth is dominated by a large influx of female new workers. As Appendix Table B.4 shows, the short-run wage bill of female new workers increases by around €1,470, heavily outweighing smaller but statistically significant increases in the wage bill of female incumbents (€236), male new workers (€250), and male incumbents (€251). In the medium run, virtually all of the remaining adjustment among female workers comes

[10] Incumbents and new workers are defined relative to the baseline quarter, excluding the focal worker. New workers are therefore a stock measure of workers not present at baseline, rather than the quarterly hiring flow shown in Figure 2.

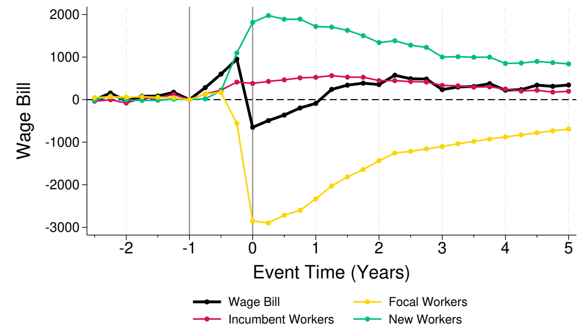
Figure 3

Decomposition of Firm Responses: Focal Workers, Incumbents, and New Workers

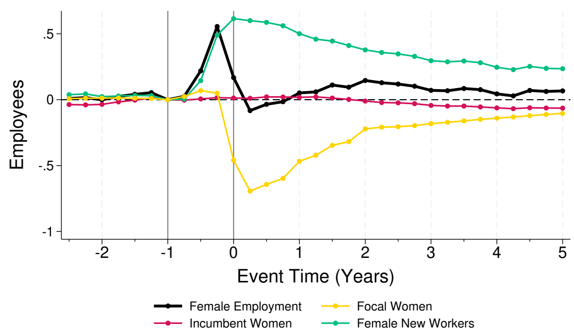
(a) Employees Overall



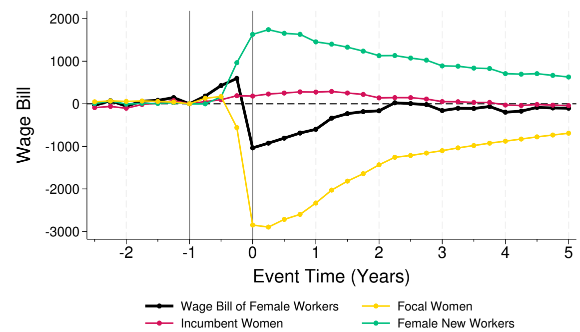
(d) Wage Bill Overall



(b) Female Employees

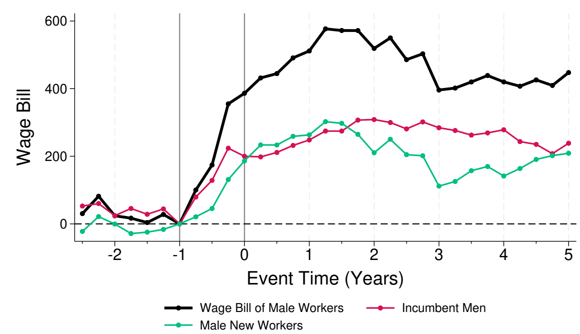


(e) Wage Bill of Female Workers



(c) Male Employees

(f) Wage Bill of Male Workers



Notes: Each panel plots the β-coefficients from equation 1, capturing the differential changes in treated firms' outcomes relative to control firms and to the baseline quarter. Coefficients are scaled to one additional birth in the baseline firm. We split total headcount employment and wage bill at the baseline firms in any given quarter into constituent parts. Focal workers are mothers and their matched female control workers, incumbents are all workers who work at the baseline firm in the baseline quarter (co-workers of the focal worker at baseline), new workers are all other workers at the baseline firm (none in the baseline quarter). Due to the linearity of OLS, the effects on focal workers, incumbents, and new workers sum to the total effect (thick black line).

from new workers; incumbent women headcount even shows a small negative effect (indicating no increase in retention), and the incumbent wage bill shows no effect. Thus, we do not see persistent gains for women already at the firm.

Among male workers, however, the adjustment pattern is reversed. Roughly 60% of their medium-run wage bill increase (and even more, 68%, for headcount) reflects adjustments among incumbent men rather than male new workers. Although this internal adjustment is smaller than the short-run hiring spike in Figure 3, it is persistent and statistically significant (Appendix Table B.2). In the medium run, the wage-bill increase for male incumbents is about 2% of their baseline wage bill (€265, $p < 0.1$), whereas the increase for new male workers is smaller and statistically indistinguishable from zero (Appendix Table B.4). The persistent response of male headcount employment and wage bill in the medium run is therefore due mostly to the retention of incumbent men instead of hiring.[11]

This highly gendered response raises the question to what extent it reflects the gender composition of the market these firms operate in. Focusing on the wage bill as a measure of total labor input cost, we calculate the female share at baseline to be 62.4%. The new-worker wage-bill increase is strongly female-skewed: around 80-85 percent of the increase goes to female new workers.[12] This is consistent with a female-skewed local labor supply, where women may be more likely to apply for the roles vacated by mothers or for the time-limited contracts used to cover parental leave. It is also consistent with a preference for female workers for these kinds of positions among firms. We ultimately cannot distinguish supply and demand in this market. By contrast, women account for only about 45-50% of the incumbent wage-bill response during anticipation and in the

[11] For estimates of employment days, which react in line with headcount, see Appendix Table B.3.

[12] €375/€441=0.85, €1,468/€1,718=0.85 and €840/€1,018=0.83 in the anticipation, short-run and medium-run periods, respectively; see Appendix Table B.4. In terms of employment headcount the share is around 90%: 0.21/0.23=0.91, 0.51/0.56=0.91 and 0.28/0.31=0.90 in the anticipation, short-run and medium-run periods, respectively; see Appendix Table B.2.

first two years after the birth. In the medium run, it is only significant for male workers. The new-worker margin is therefore much more female than the baseline female share of 62.4% would indicate, whereas the internal adjustment through incumbents is more male.

This structural shift with a strongly dominant female hiring effect and more balanced retention represents the average response. Does this highly gendered behavior meaningfully alter the firm's overall gender composition? Ultimately, the effect is small. The female share of the total wage bill decreases only slightly, from 62.4% at baseline to 61.4% in the medium run (Figure 2h) and the share of female employees similarly decreases by 0.4 percentage points. This raises the question: are these outcomes merely mechanical because the focal worker is always female? The answer is nuanced. The active margins, i.e. the specific mix of who is hired and retained, are genuine firm choices and not mechanical. However, a slight overall decline in the female share can be viewed as mechanical. Because the focal worker is female by construction, any replacement strategy that is not exclusively female will inherently reduce the firm's overall female share.

4.4 The Effects of Parental Leave by Duration

The results above show how firms respond to parental leave absences on average. We next ask whether the expected length of the absence changes these responses. The relevant margin is the choice between covering the absence internally and hiring a replacement. For shorter absences, firms may rely more heavily on incumbent workers. For longer absences, replacement hiring may be more attractive because the fixed costs of hiring and training can be spread over a longer period.

To explore whether changes in leave duration eligibility actually changed these firm-side response margins, we use the three parental-leave regimes introduced in Section 2.

We employ two complementary empirical designs. First, we use a simple-difference specification. This approach expands the baseline dynamic difference-in-differences model in equation (1) by allowing the effect of an additional birth to vary across regimes. In the full sample, this shows whether firm responses differ systematically with leave duration. To simplify the exposition, we report averages for the anticipation, short-run, and medium-run periods.

To isolate the effect of leave duration more cleanly, we also use a reform-based difference-in-differences design around the July 1 reforms of 1990 and 1996. This design compares births just before and just after the policy changes, net of the same within-year contrast in the preceding year. We restrict the sample to firms that experienced a birth and compare female-worker/firm pairs with births in the first half of the year (January 1 to June 30) to those with births in the second half (July 1 to December 31) in the reform year (1990 or 1996), relative to the same comparison in the preceding year (1989 or 1995). This identifies the effect on firm outcomes of a one-year increase in maximum leave duration in 1990 and a 0.5-year decrease in 1996. We estimate this design separately for the 1989–1990 and 1995–1996 reform episodes using the following regression:

$$Y_{ep} = \gamma_e + \boldsymbol{\alpha} \boldsymbol{D}_{ep}^{Event} + \boldsymbol{\alpha}^F \boldsymbol{D}_{ep}^{Event} \cdot Fall_e + \boldsymbol{\alpha}^R \boldsymbol{D}_{ep}^{Event} \cdot Reform_e + \boldsymbol{\delta} \boldsymbol{D}_{ep}^{Event} \cdot Reform_e \cdot Fall_e + \epsilon_{ep}$$

where Y_{ep} is the outcome for event e in period p. The interaction terms indicate whether the birth occurred in the second half of the year ($Fall$, i.e., after July 1) and whether it occurred in the reform year ($Reform$). Because this specification is restricted to firms with a birth event, no additional control group of non-birth firms is required.

Taken together, the two designs combine a broader dynamic comparison across regimes with a cleaner reform-based contrast around the policy changes. They show whether longer leave entitlements change firms' adjustment patterns in a systematic way.

Table 1 starts with the first-stage effect on actual leave-taking in Panel A. Panels B through D then report the wage bill overall and by gender, our preferred proxy for total labor input because it captures both headcount and daily-wage adjustments.

Panel A confirms that actual leave-taking is highly responsive to the policy regimes. In both designs, the reforms substantially changed parental leave days in the expected direction; for example, the reform-based design implies that the 1990 reform increased leave take-up by more than 35 days per quarter (318 days over the first 9 quarters). Yet Panel B shows that these large changes in actual leave-taking do not translate into corresponding changes in the aggregate wage bill. Across both designs, the estimated effects on overall labor costs remain small and unsystematic.

The same conclusion extends to the other firm-side margins. Appendix Tables B.5 and B.6 show similar patterns for headcount employment, quarterly hiring, and average daily wages. A few individual coefficients are statistically significant, but they do not point to a systematic shift in firm adjustment.

By gender, the duration estimates likewise do not reveal a systematic pattern across the full set of specifications. The clearest indication appears for the large extension from 1 year to 2 years. There, the simple-difference estimates suggest a lower female wage bill and a higher male wage bill than in the baseline 1-year regime, consistent with the gendered response documented above: when the focal worker is absent for longer, the remaining wage-bill adjustment may tilt somewhat more toward men. In the reform-based estimates for the same extension, the male wage bill points in a similar direction, whereas the female wage-bill response is less clear. The corresponding estimates for the later reform are more mixed.

Taken together, the duration results indicate that longer entitlements substantially change actual leave-taking, but they do not fundamentally change firms' observed ad-

Table 1
Leave-Taking and Wage Bills by Parental Leave Duration

	Baseline PL 1 year	1990 Reform PL 1y → 2y		1996 Reform PL 2y → 1.5y	
	Effect of Additional Birth	Simple Difference	Diff-in-Diff	Simple Difference	Diff-in-Diff
	(1)	(2)	(3)	(4)	(5)
Panel A: Parental Leave (PL) Take-Up Duration (Days per Quarter)					
Anticipation (-3q to -1q)	8.68***	2.31***	-2.86	-2.03**	-3.93
	(0.64)	(0.86)	(2.71)	(0.96)	(3.05)
Short run (0 to 8q)	45.25***	35.28***	35.09***	-19.41***	-18.86***
	(0.90)	(1.37)	(3.99)	(1.63)	(5.24)
Medium run (9q to 20q)	4.19***	-0.85	-3.23	-2.52	1.81
	(1.07)	(1.54)	(4.98)	(1.83)	(5.56)
Panel B: Overall Wage Bill					
Anticipation (-3q to -1q)	559.0***	-59.2	208.4	36.8	33.5
	(114.8)	(172.8)	(539.5)	(261.2)	(632.7)
Short run (0 to 8q)	-78.0	-197.1	680.1	132.8	525.5
	(233.4)	(354.5)	(994.0)	(516.0)	(1,307.1)
Medium run (9q to 20q)	462.8	-262.5	901.4	-304.2	1,420.2
	(393.8)	(570.7)	(1,580.8)	(786.9)	(2,272.4)
Panel C: Female Workers' Wage Bill					
Anticipation (-3q to -1q)	390.70***	-65.64	137.15	-64.25	185.09
	(69.05)	(100.16)	(345.43)	(143.37)	(359.61)
Short run (0 to 8q)	-369.43***	-432.74**	334.81	-16.10	165.45
	(131.13)	(195.22)	(586.35)	(278.36)	(720.45)
Medium run (9q to 20q)	133.79	-384.28	302.92	-362.29	754.25
	(214.77)	(310.59)	(896.32)	(429.83)	(1199.44)
Panel D: Male Workers' Wage Bill					
Anticipation (-3q to -1q)	168.33**	6.43	71.22	101.04	-151.63
	(68.51)	(106.33)	(299.42)	(164.15)	(413.94)
Short run (0 to 8q)	291.42**	235.63	345.26	148.86	360.00
	(143.19)	(219.06)	(586.68)	(320.73)	(847.61)
Medium run (9q to 20q)	329.01	121.80	598.52	58.11	665.92
	(240.88)	(347.86)	(916.91)	(477.94)	(1450.36)
Observations	1,059,394	2,264,209	158,968	1,857,768	133,269
Events	34,174	73,039	5,128	59,928	4,299
Firms	21,898	37,216	4,714	32,567	4,007

Notes: This table summarizes the main duration results across our two empirical designs. Column (1) shows the effect of an additional birth on firm outcomes in the baseline 1-year regime (1985–1990), using the same specification as equation (1). Columns (2)–(5) compare the 2-year regime to the 1-year and 1.5-year regimes. Columns (2) and (4) report the simple-difference specification, which captures heterogeneity across leave regimes, while columns (3) and (5) report the reform-based difference-in-differences design. Standard errors are clustered by firm identifier, and the simple difference results are based on cell-weighted fixed effects regressions. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

justment margins. The clearest heterogeneity appears for the move from 1 year to 2 years and points in the same direction as the decomposition evidence, but these differences remain modest relative to the large first-stage changes in leave duration.

5 Discussion

Our results show that firms adjust to temporary leave following childbirth primarily through anticipatory hiring. Despite sizable short-run adjustments, we do not find large medium-run effects on the main firm-level outcomes. These estimates concern labor-demand adjustment rather than broader firm performance such as output, productivity, or profits, which we do not observe. At the same time, the muted aggregate effects mask the paper's central result: adjustment within firms is strongly gendered, with female-dominated replacement hiring and a more male-skewed persistent adjustment in employment and the wage bill. Our reform evidence likewise shows that even large differences in actual leave-taking do not materially change these observed firm-side response margins. These findings may seem surprising, given that temporary leaves in Austria tend to be unusually long under the country's generous parental leave policies. They also contrast with much of the literature on unexpected worker exits, which finds substantial hiring and replacement costs for firms. For instance, Jäger et al. (2024) find that replacing an unexpectedly deceased worker costs firms as much as 65 percent of an incumbent's annual salary.

A likely reason for this contrast is that a temporary leave after childbirth is typically communicated to the employer several months in advance, and our results show that firms begin hiring before the birth event. Informing the employer early on is incentivized by labor regulations that protect the health of the mother and child and provide enhanced employment protection. This interpretation is consistent with Schmutte and

Skira (2025), who show that firms in Brazil begin adjusting before maternity leave starts, whereas comparable pre-absence adjustment is much weaker for sickness absence. Related evidence emphasizes that firms can limit disruption through internal substitution when workers are more substitutable (Hensvik and Rosenqvist, 2019).

The distinction between anticipated and unanticipated absence also helps organize the recent firm-side parental-leave literature. Ginja et al. (2023) study a Swedish reform that was implemented retrospectively and document employer adjustment costs under a retroactive extension. Gallen (2019) likewise points to more adverse effects in Denmark when firms have little time to adjust, especially smaller firms. By contrast, Huebener et al. (2025) and the Austrian reform evidence in this paper point to more muted average firm responses when the policy environment is known in advance. Kleven et al. (2024) show that the Austrian reforms shifted leave-taking and short-run labor supply, but did not generate lasting changes in mothers' long-run employment and earnings. That worker-level convergence does not pin down firm-side effects a priori, because longer absences can still raise staffing and coordination needs; our reform evidence shows that, despite large differences in leave-taking, firms do not exhibit systematic medium-run differences in labor demand responses.

A second reason why a temporary leave after childbirth may be more easily accommodated relates to the position of the exiting worker. Compared to the deceased workers in Jäger et al. (2024), women giving birth are younger, have lower tenure, and occupy lower job ranks. They may therefore be easier to replace. Recent long-run evidence from Norway likewise points to gendered workforce-composition responses rather than a simple contraction in firms' demand for women (Corekcioglu et al., 2025).

The gendered pattern we document is neither purely mechanical nor well described by a simple story in which firms stop hiring women after a leave spell, because the new-hire margin is itself overwhelmingly female. Huebener et al. (2025) likewise do not find that directly exposed firms subsequently hire fewer women of childbearing age. The evidence instead points to gendered reallocation within firms, where the external replacement margin is female-dominated while the more persistent incumbent adjustment tilts toward men. This interpretation is also consistent with evidence that after childbirth women become less likely to remain in low-substitutability jobs (Azmat et al., 2025). Statistical discrimination may still be part of the pattern if women of childbearing age are sorted into jobs that are easier to replace or are treated differently in promotion and retention decisions (Xiao, 2024; Peterson Gloor et al., 2022). The same pattern could also reflect occupational choices, job design, or labor supply (Adda et al., 2017; Haegele, 2024; Hotz et al., 2017). Our design does not distinguish among these channels, but the results show that even muted aggregate responses can mask gendered reallocation within the firm.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process. During the preparation of this work the authors used ChatGPT and Codex by OpenAI, Claude by Anthropic, and Gemini by Google to assist with language editing and text revision, and to help write, review, and debug code used for the data analysis. After using these tools/services, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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“How Do Firms Respond to Parental Leave Absences?”

Online Appendix

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Urša Krenk

Andreas Steinhauer

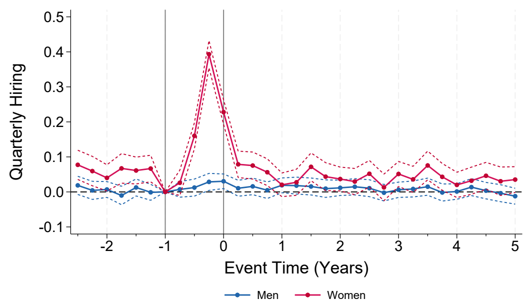
Josef Zweimüller

Table of Contents

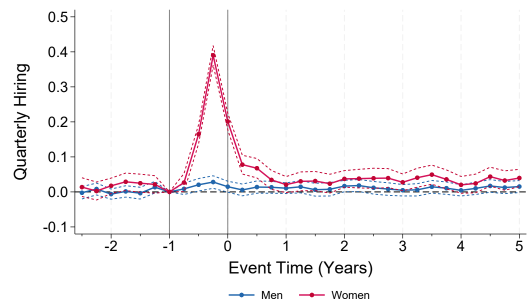
A Appendix Figures

Figure A.1
Dynamic Effects of an Additional Birth and Leave-Taking on Quarterly Hiring, by Gender and Focal Worker Characteristics

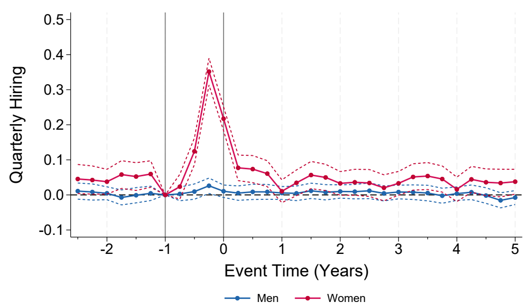
(a) Below Median Tenure



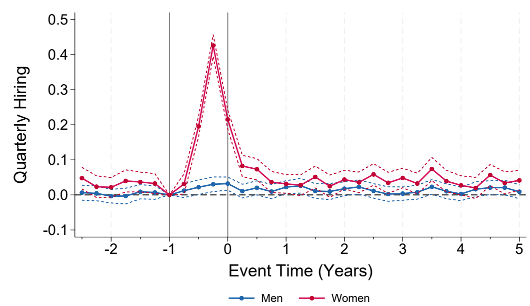
(b) Above Median Tenure



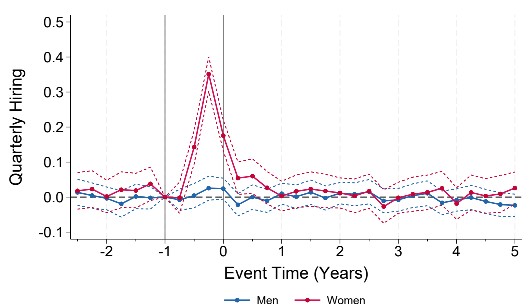
(c) Below Median Earnings



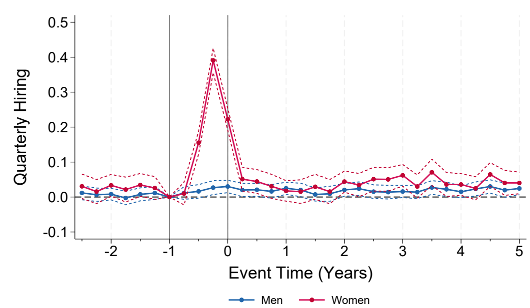
(d) Above Median Earnings



(e) Bottom Quintiles Firm Rank



(f) Top Quintiles Firm Rank

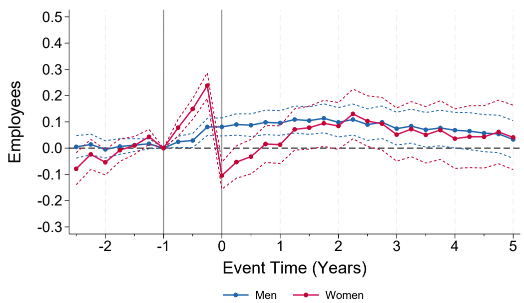


Notes: The figure plots the coefficients from equation (1), estimated separately for women and men within subsamples defined by the focal worker's baseline tenure, baseline earnings, and within-firm earnings rank. Panels (a) and (b) split the sample by overall tenure quintiles, panels (c) and (d) by overall earnings quintiles, and panels (e) and (f) by within-firm earnings quintiles. In each case, "below median" corresponds to quintiles 1 and 2, and "above median" corresponds to quintiles 4 and 5. Dashed lines indicate 95% confidence intervals.

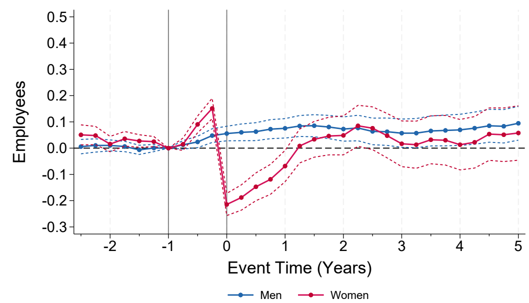
Figure A.2

Dynamic Effects of an Additional Birth and Leave-Taking on Employment, by Gender and Focal Worker Characteristics

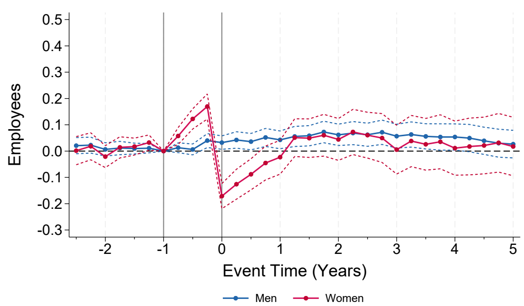
(a) Below Median Tenure



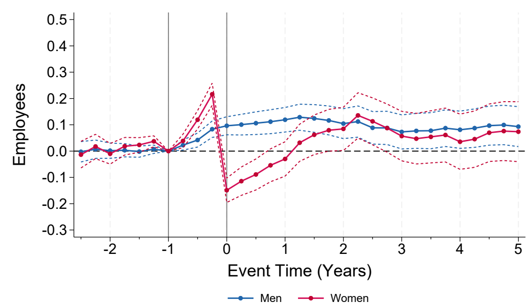
(b) Above Median Tenure



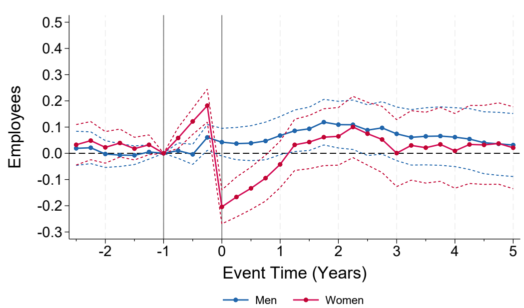
(c) Below Median Earnings



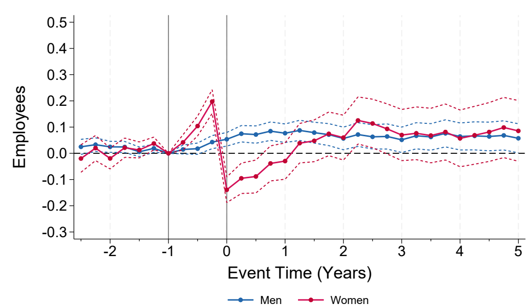
(d) Above Median Earnings



(e) Bottom Quintiles Firm Rank



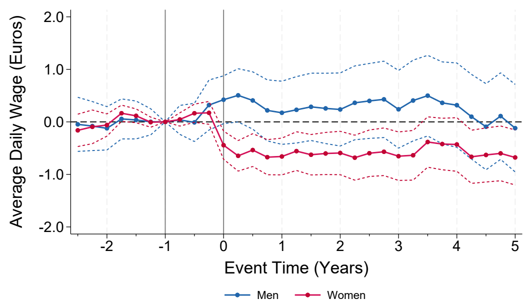
(f) Top Quintiles Firm Rank



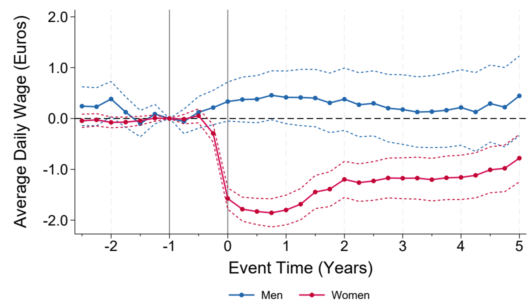
Notes: The figure plots the coefficients from equation (1), estimated separately for women and men within subsamples defined by the focal worker's baseline tenure, baseline earnings, and within-firm earnings rank. Panels (a) and (b) split the sample by overall tenure quintiles, panels (c) and (d) by overall earnings quintiles, and panels (e) and (f) by within-firm earnings quintiles. In each case, "below median" corresponds to quintiles 1 and 2, and "above median" corresponds to quintiles 4 and 5. Dashed lines indicate 95% confidence intervals.

Figure A.3
Dynamic Effects of an Additional Birth and Leave-Taking on Average Daily Wages, by Gender and Focal Worker Characteristics

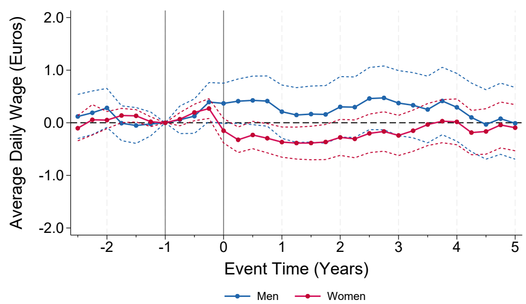
(a) Below Median Tenure



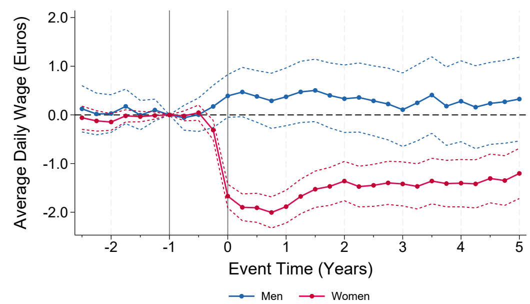
(b) Above Median Tenure



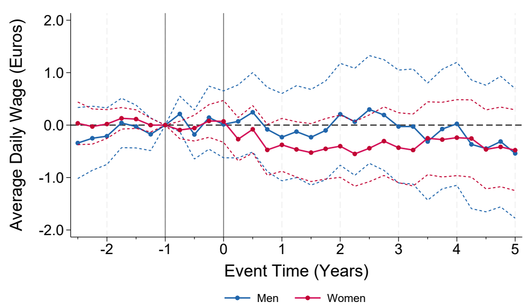
(c) Below Median Earnings



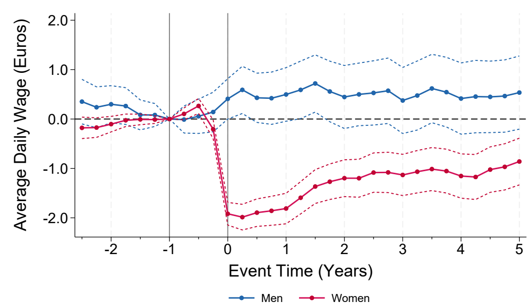
(d) Above Median Earnings



(e) Bottom Quintiles Firm Rank



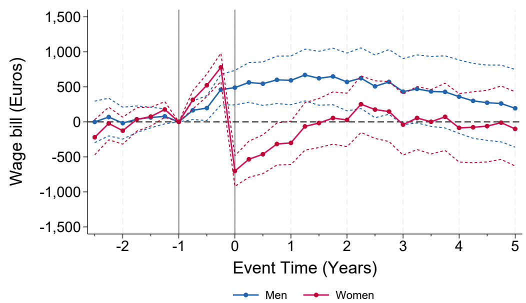
(f) Top Quintiles Firm Rank



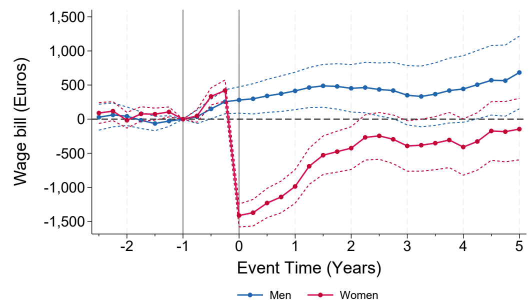
Notes: The figure plots the coefficients from equation (1), estimated separately for women and men within subsamples defined by the focal worker's baseline tenure, baseline earnings, and within-firm earnings rank. Panels (a) and (b) split the sample by overall tenure quintiles, panels (c) and (d) by overall earnings quintiles, and panels (e) and (f) by within-firm earnings quintiles. In each case, "below median" corresponds to quintiles 1 and 2, and "above median" corresponds to quintiles 4 and 5. Dashed lines indicate 95% confidence intervals.

Figure A.4
Dynamic Effects of an Additional Birth and Leave-Taking on Wage Bills, by Gender and Focal Worker Characteristics

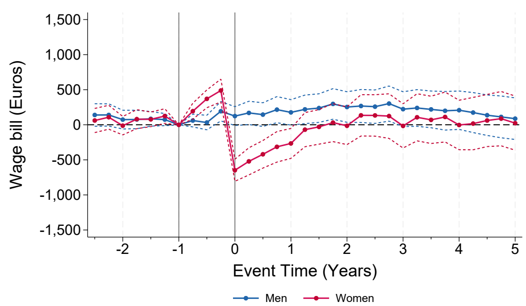
(a) Below Median Tenure



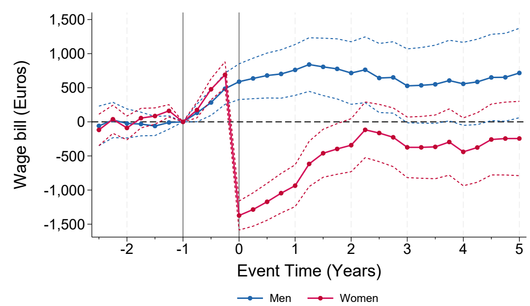
(b) Above Median Tenure



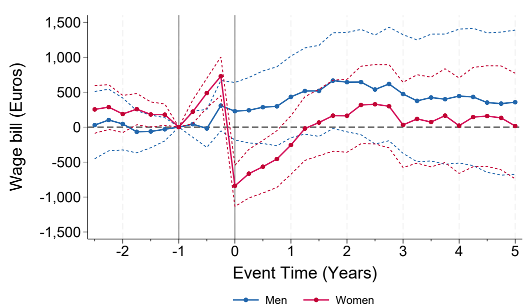
(c) Below Median Earnings



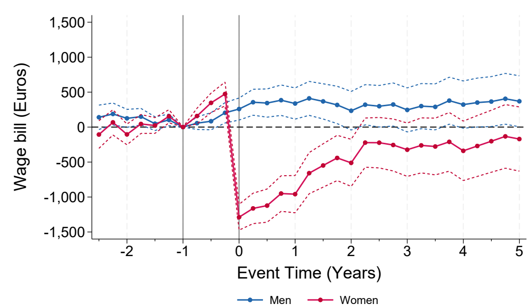
(d) Above Median Earnings



(e) Bottom Quintiles Firm Rank

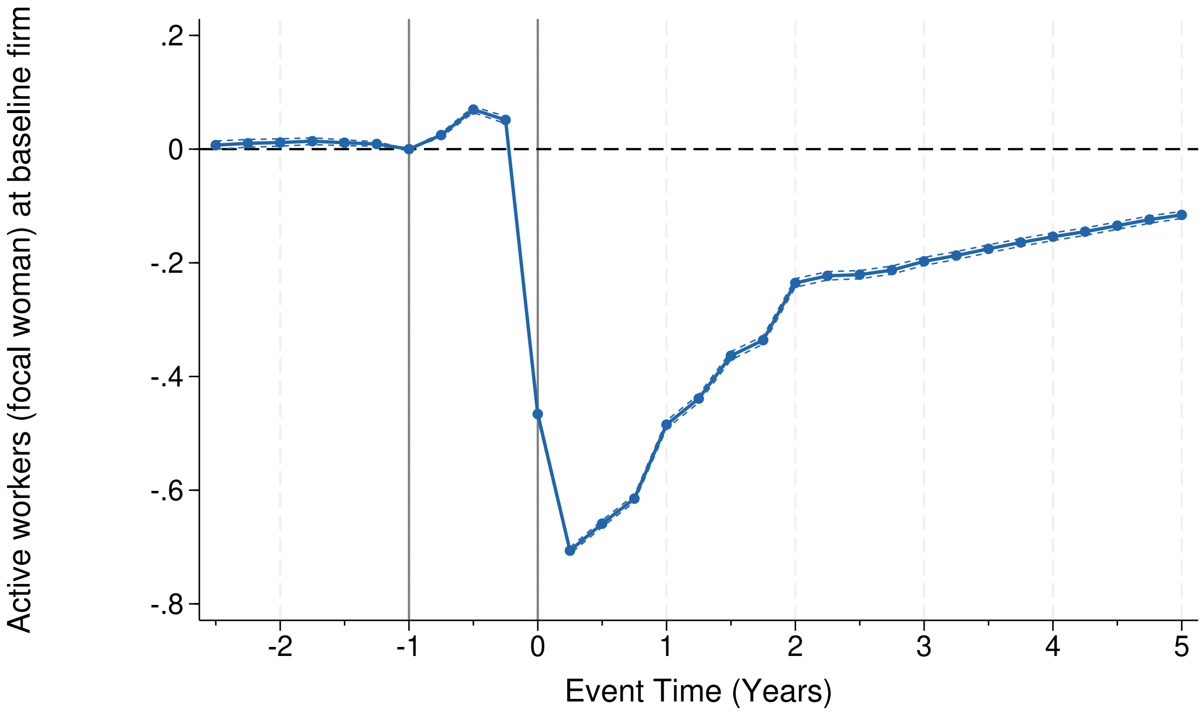


(f) Top Quintiles Firm Rank



Notes: The figure plots the coefficients from equation (1), estimated separately for women and men within subsamples defined by the focal worker's baseline tenure, baseline earnings, and within-firm earnings rank. Panels (a) and (b) split the sample by overall tenure quintiles, panels (c) and (d) by overall earnings quintiles, and panels (e) and (f) by within-firm earnings quintiles. In each case, "below median" corresponds to quintiles 1 and 2, and "above median" corresponds to quintiles 4 and 5. Dashed lines indicate 95% confidence intervals.

Figure A.5
Return of the Mothers to the Baseline Firm



Notes: The figure plots event-time estimates for an indicator equal to one if the focal mother is employed at the baseline firm in quarter *k*. Dashed lines indicate 95% confidence intervals.

B Appendix Tables

Table B.1
Baseline Characteristics of Women and Firms

	Pooled		1y Regime		2y Regime		1.5y Regime	
	Control	Treat	Control	Treat	Control	Treat	Control	Treat
Woman								
Age	23.12	23.17	21.99	22.07	23.21	23.25	24.70	24.67
	(4.56)	(4.33)	(4.14)	(3.92)	(4.38)	(4.17)	(4.99)	(4.72)
Wage	3,548	3,574	2,979	3,003	3,653	3,672	4,225	4,264
	(1,678)	(1,686)	(1,279)	(1,264)	(1,648)	(1,644)	(1,956)	(2,000)
Tenure	3.73	3.79	3.28	3.35	3.73	3.83	4.41	4.39
	(2.95)	(2.96)	(2.28)	(2.32)	(3.03)	(3.07)	(3.54)	(3.48)
Firm								
Firm age	12.68	12.64	11.04	11.01	12.94	12.90	14.70	14.66
	(7.03)	(7.04)	(5.02)	(5.08)	(7.07)	(7.09)	(8.79)	(8.73)
Employees	8.76	8.78	8.35	8.37	8.87	8.91	9.17	9.20
	(6.87)	(6.89)	(6.61)	(6.61)	(6.99)	(7.00)	(7.03)	(7.04)
Quarterly hiring	1	1	1	1	0.93	0.90	0.83	0.80
	(2)	(2)	(2)	(1)	(1.57)	(1.55)	(1.55)	(1.50)
Average daily wage	41.85	41.45	37.38	37.00	43.24	42.76	46.12	45.83
	(18.89)	(18.77)	(15.30)	(15.13)	(18.79)	(18.73)	(22.31)	(22.16)
Wage Bill	38,154	37,861	32,085	31,615	39,803	39,503	44,399	44,384
	(43,252)	(43,092)	(36,392)	(35,393)	(44,332)	(44,422)	(49,304)	(49,587)
Female Wage Bill	23,718	23,747	20,358	20,311	24,596	24,629	27,237	27,375
	(22,044)	(22,080)	(18,660)	(18,375)	(22,632)	(22,656)	(24,838)	(25,228)
Male Wage Bill	14,437	14,114	11,727	11,304	15,207	14,875	17,162	17,009
	(29,268)	(29,138)	(24,254)	(23,586)	(30,081)	(30,123)	(34,028)	(34,166)
Turnover	2	2	2	2	1.78	1.77	1.70	1.68
	(2)	(2)	(2)	(2)	(2.30)	(2.27)	(2.27)	(2.17)
Trade & services sector	1	1	1	1	0.67	0.67	0.64	0.64
	(0)	(0)	(0)	(0)	(0.47)	(0.47)	(0.48)	(0.48)
state: Vienna	0.24	0.24	0.23	0.23	0.24	0.24	0.24	0.24
	(0.43)	(0.43)	(0.42)	(0.42)	(0.43)	(0.43)	(0.43)	(0.43)
Observations: events	60,323	33,779	22,318	11,856	24,735	14,130	13,270	7,793
Observations: unique individuals	47,034	33,779	18,716	11,856	20,643	14,130	11,598	7,793
Observations: unique firms	33,325	24,032	15,548	9,978	16,756	11,664	10,179	6,923

Notes: The table reports baseline-quarter summary statistics for women and firms in the treatment and control samples, both for the pooled sample and separately by parental-leave regime. Standard deviations are reported in parentheses. For the control group, observations are weighted inversely by the number of control cells per treatment cell. Tenure measures years of employment at the firm. Wages and wage bill are real annual earnings in euros (2000 CPI). Turnover is hirings plus separations over the past year.

Table B.2
Effects on Number of Employees by Worker Group and Period

	Total	Decomposition of Total: Focal Worker	Incumbents	New Workers
Panel A: Employment of all Workers				
Anticipation (-3q to -1q)	0.304***	0.048***	0.030	0.227***
	(0.019)	(0.002)	(0.018)	(0.017)
Short run (0 to 8q)	0.150***	-0.463***	0.056*	0.556***
	(0.034)	(0.003)	(0.033)	(0.034)
Medium run (9q to 20q)	0.155***	-0.157***	0.004	0.308***
	(0.052)	(0.003)	(0.045)	(0.051)
Panel B: Employment of Female Workers				
Anticipation (-3q to -1q)	0.267***	0.048***	0.006	0.213***
	(0.014)	(0.002)	(0.013)	(0.013)
Short run (0 to 8q)	0.055**	-0.463***	0.012	0.506***
	(0.025)	(0.003)	(0.024)	(0.026)
Medium run (9q to 20q)	0.077**	-0.157***	-0.049	0.283***
	(0.038)	(0.003)	(0.033)	(0.038)
Panel C: Employment of Male Workers				
Anticipation (-3q to -1q)	0.037***		0.024***	0.014
	(0.009)		(0.009)	(0.008)
Short run (0 to 8q)	0.094***		0.044***	0.050***
	(0.016)		(0.017)	(0.016)
Medium run (9q to 20q)	0.078***		0.053**	0.025
	(0.023)		(0.024)	(0.023)
Observations	2,917,162	2,917,162	2,917,162	2,917,162
Events	94,102	94,102	94,102	94,102
Firms	44,847	44,847	44,847	44,847

Notes: Rows report period averages of coefficients from the baseline event-study specification. Anticipation covers quarters $k = -3$ to $k = -1$, short run covers quarters $k = 0$ to $k = 8$, and medium run covers quarters $k = 9$ to $k = 20$. The focal worker is the treated mother and her matched control worker. Incumbents are workers present at the baseline firm in the baseline quarter, excluding the focal worker. New workers are workers not present at the baseline firm in the baseline quarter. Headcount employment counts all workers with an employment spell in a given quarter.

Table B.3
Effects on Employment Days by Worker Group and Period

		Decomposition of Total		
	Total	Focal Worker	Incumbents	New Workers
Panel A: Employment Days of all Workers				
Anticipation (-3q to -1q)	0.156***	-0.013***	0.032**	0.137***
	(0.016)	(0.002)	(0.015)	(0.012)
Short run (0 to 8q)	0.068**	-0.499***	0.053*	0.513***
	(0.030)	(0.003)	(0.029)	(0.028)
Medium run (9q to 20q)	0.131***	-0.161***	-0.002	0.294***
	(0.047)	(0.003)	(0.040)	(0.044)
Panel B: Employment Days of Female Workers				
Anticipation (-3q to -1q)	0.120***	-0.013***	0.008	0.124***
	(0.012)	(0.002)	(0.011)	(0.009)
Short run (0 to 8q)	-0.019	-0.499***	0.011	0.469***
	(0.022)	(0.003)	(0.021)	(0.022)
Medium run (9q to 20q)	0.057*	-0.161***	-0.052*	0.270***
	(0.035)	(0.003)	(0.030)	(0.033)
Panel C: Employment Days of Male Workers				
Anticipation (-3q to -1q)	0.036***		0.024***	0.012**
	(0.008)		(0.007)	(0.006)
Short run (0 to 8q)	0.086***		0.042***	0.044***
	(0.014)		(0.015)	(0.013)
Medium run (9q to 20q)	0.074***		0.049**	0.025
	(0.021)		(0.021)	(0.020)
Observations	2,917,162	2,917,162	2,917,162	2,917,162
Events	94,102	94,102	94,102	94,102
Firms	44,847	44,847	44,847	44,847

Notes: Rows report period averages of coefficients from the baseline event-study specification. Anticipation covers quarters $k = -3$ to $k = -1$, short run covers quarters $k = 0$ to $k = 8$, and medium run covers quarters $k = 9$ to $k = 20$. The worker-group definitions follow Appendix Table B.2. Employment days count all days of employment of all workers at a firm within a given quarter.

Table B.4
Wage Bill Effects by Worker Group and Period

		Decomposition of Total		
	Total	Focal Worker	Incumbents	New Workers
Panel A: Wage Bill of all Workers				
Anticipation (-3q to -1q)	612.1***	-85.4***	256.4***	441.1***
	(85.0)	(9.1)	(76.0)	(53.4)
Short run (0 to 8q)	-51.7	-2,256.4***	486.5***	1,718.3***
	(169.6)	(15.5)	(153.5)	(135.6)
Medium run (9q to 20q)	351.7	-964.6***	298.8	1,017.6***
	(274.2)	(16.8)	(225.9)	(226.5)
Panel B: Wage Bill of Female Workers				
Anticipation (-3q to -1q)	402.3***	-85.4***	112.4***	375.3***
	(48.1)	(9.1)	(43.3)	(31.1)
Short run (0 to 8q)	-552.2***	-2,256.4***	236.0***	1,468.2***
	(93.0)	(15.5)	(84.6)	(80.2)
Medium run (9q to 20q)	-90.4	-964.6***	33.9	840.3***
	(148.8)	(16.8)	(122.7)	(128.9)
Panel C: Wage Bill of Male Workers				
Anticipation (-3q to -1q)	209.8***		144.0***	65.8**
	(52.1)		(47.3)	(33.1)
Short run (0 to 8q)	500.6***		250.5**	250.0***
	(105.1)		(98.5)	(80.3)
Medium run (9q to 20q)	442.1***		264.8*	177.3
	(167.7)		(147.5)	(134.4)
Observations	2,917,162	2,917,162	2,917,162	2,917,162
Events	94,102	94,102	94,102	94,102
Firms	44,847	44,847	44,847	44,847

Notes: Rows report period averages of coefficients from the baseline event-study specification. Anticipation covers quarters $k = -3$ to $k = -1$, short run covers quarters $k = 0$ to $k = 8$, and medium run covers quarters $k = 9$ to $k = 20$. The worker-group definitions follow Appendix Table B.2. Within each panel, the coefficients on focal workers, incumbents, and new workers sum to the total effect by linearity of OLS. The wage bill is the total quarterly wage payments to all workers employed at the firm. Based on these estimates we calculate the female shares of the effects, mentioned in the main text. We divide the female effect by the sum of the female and male effect. For new workers during the anticipation period, $0.85 = 375.3/(375.3 + 65.8)$ shows that a dominant majority of the wage bill increase during anticipation is driven by increased payments for new female hires. Similarly, we can calculate the female share for new workers in the short and medium run as 0.85 and 0.83, respectively. For incumbents, the female shares are 0.44 (anticipation), 0.49 (short run), and 0.11 (medium run). In the baseline quarter, the female share of the wage bill is 0.62.

Table B.5
Firm Outcomes by Parental Leave Duration

	Baseline PL 1 year	1990 Reform PL 1y → 2y		1996 Reform (reversed) PL 2y → 1.5y	
	Effect of Additional Birth	Simple Difference	Diff-in-Diff	Simple Difference	Diff-in-Diff
	(1)	(2)	(3)	(4)	(5)
Panel A: Actual Leave-Taking (Days per Quarter)					
Anticipation (-3q to -1q)	8.68***	2.31***	-2.86	-2.03**	-3.93
	(0.64)	(0.86)	(2.71)	(0.96)	(3.05)
Short run (0 to 8q)	45.25***	35.28***	35.09***	-19.41***	-18.86***
	(0.90)	(1.37)	(3.99)	(1.63)	(5.24)
Medium run (9q to 20q)	4.19***	-0.85	-3.23	-2.52	1.81
	(1.07)	(1.54)	(4.98)	(1.83)	(5.56)
Panel B: Quartely Hiring					
Anticipation (-3q to -1q)	0.18***	-0.00	0.11	0.05*	-0.13*
	(0.02)	(0.02)	(0.07)	(0.03)	(0.08)
Short run (0 to 8q)	0.06***	-0.01	0.13**	0.02	-0.15**
	(0.02)	(0.02)	(0.07)	(0.02)	(0.07)
Medium run (9q to 20q)	0.03	-0.01	0.13*	0.02	-0.13
	(0.02)	(0.02)	(0.07)	(0.03)	(0.08)
Panel C: Employment					
Anticipation (-3q to -1q)	0.14***	-0.02	0.09	0.00	0.20*
	(0.02)	(0.04)	(0.12)	(0.04)	(0.11)
Short run (0 to 8q)	0.05	-0.02	0.20	0.04	0.14
	(0.05)	(0.07)	(0.21)	(0.08)	(0.22)
Medium run (9q to 20q)	0.13*	-0.00	0.33	-0.10	0.18
	(0.08)	(0.10)	(0.30)	(0.13)	(0.36)
Panel D: Average Daily Wages					
Anticipation (-3q to -1q)	-0.01	0.15	0.47	-0.40***	-0.65
	(0.07)	(0.11)	(0.34)	(0.15)	(0.40)
Short run (0 to 8q)	-0.58***	0.08	0.48	-0.48	0.28
	(0.16)	(0.23)	(0.68)	(0.31)	(0.84)
Medium run (9q to 20q)	-0.45*	-0.03	-0.08	-0.51	0.19
	(0.25)	(0.35)	(1.01)	(0.46)	(1.32)
Panel E: Wage Bill					
Anticipation (-3q to -1q)	559.0***	-59.2	208.4	36.8	33.5
	(114.8)	(172.8)	(539.5)	(261.2)	(632.7)
Short run (0 to 8q)	-78.0	-197.1	680.1	132.8	525.5
	(233.4)	(354.5)	(994.0)	(516.0)	(1,307.1)
Medium run (9q to 20q)	462.8	-262.5	901.4	-304.2	1,420.2
	(393.8)	(570.7)	(1,580.8)	(786.9)	(2,272.4)
Observations	1,059,394	2,264,209	158,968	1,857,768	133,269
Events	34,174	73,039	5,128	59,928	4,299
Firms	21,898	37,216	4,714	32,567	4,007

Notes: This table reports duration estimates for actual leave-taking and the additional firm outcomes discussed in the text. Column (1) shows the effect of an additional birth on firm outcomes in the baseline 1-year regime (1985–1990), using the same specification as equation (1). Columns (2)–(5) compare the 2-year regime to the 1-year and 1.5-year regimes. For the 1996 reform, which reduced benefit duration from 2 years to 1.5 years, we reverse the sign so that both reform episodes can be read as increases in leave duration. Columns (2) and (4) report the simple-difference specification, which captures heterogeneity across leave regimes, while columns (3) and (5) report the reform-based difference-in-differences design. Standard errors are clustered by firm identifier, and the simple difference results are based on cell-weighted fixed effects regressions. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table B.6
Gender-Specific Firm Outcomes by Parental Leave Duration

	Baseline PL 1 year	1990 Reform PL 1y → 2y		1996 Reform (reversed) PL 2y → 1.5y	
	Effect of Additional Birth	Simple Difference	Diff-in-Diff	Simple Difference	Diff-in-Diff
	(1)	(2)	(3)	(4)	(5)
Panel A1: *Quarterly Hiring of Female Workers*					
Anticipation (-3q to -1q)	0.16***	0.01	0.05	0.04**	-0.20***
	(0.01)	(0.02)	(0.06)	(0.02)	(0.06)
Short run (0 to 8q)	0.05***	-0.01	0.07	0.01	-0.15***
	(0.01)	(0.02)	(0.05)	(0.02)	(0.05)
Medium run (9q to 20q)	0.01	-0.00	0.07	0.01	-0.12**
	(0.01)	(0.02)	(0.06)	(0.02)	(0.06)
Panel A2: *Quarterly Hiring of Male Workers*					
Anticipation (-3q to -1q)	0.02***	-0.01	0.06*	0.01	0.07*
	(0.01)	(0.01)	(0.03)	(0.01)	(0.04)
Short run (0 to 8q)	0.01	-0.00	0.07**	0.01	-0.00
	(0.01)	(0.01)	(0.03)	(0.01)	(0.03)
Medium run (9q to 20q)	0.01	-0.01	0.06*	0.01	-0.00
	(0.01)	(0.01)	(0.03)	(0.01)	(0.04)
Panel B1: *Employment of Female Workers*					
Anticipation (-3q to -1q)	0.11***	-0.01	0.07	-0.01	0.18**
	(0.02)	(0.03)	(0.10)	(0.03)	(0.09)
Short run (0 to 8q)	-0.02	-0.05	0.14	0.04	0.11
	(0.03)	(0.05)	(0.16)	(0.06)	(0.16)
Medium run (9q to 20q)	0.06	-0.01	0.27	-0.08	0.12
	(0.06)	(0.08)	(0.22)	(0.09)	(0.26)
Panel B2: *Employment of Male Workers*					
Anticipation (-3q to -1q)	0.03***	-0.00	0.02	0.01	0.01
	(0.01)	(0.02)	(0.05)	(0.02)	(0.06)
Short run (0 to 8q)	0.06***	0.02	0.06	0.00	0.03
	(0.02)	(0.03)	(0.09)	(0.04)	(0.11)
Medium run (9q to 20q)	0.07*	0.01	0.07	-0.01	0.06
	(0.03)	(0.05)	(0.13)	(0.06)	(0.16)
Panel C1: *Average Daily Wages of Female Workers*					
Anticipation (-3q to -1q)	0.07	0.09	0.43	-0.46***	-0.56
	(0.07)	(0.10)	(0.33)	(0.14)	(0.38)
Short run (0 to 8q)	-0.82***	-0.18	0.60	-0.67**	0.34
	(0.15)	(0.22)	(0.65)	(0.29)	(0.80)
Medium run (9q to 20q)	-0.59**	-0.10	-0.42	-0.77*	0.46
	(0.23)	(0.32)	(0.93)	(0.42)	(1.20)
Panel C2: *Average Daily Wages of Male Workers*					
Anticipation (-3q to -1q)	0.16	0.09	0.22	-0.56*	-0.11
	(0.15)	(0.22)	(0.66)	(0.33)	(0.78)
Short run (0 to 8q)	-0.03	0.60	0.48	-0.18	0.70
	(0.27)	(0.39)	(1.15)	(0.49)	(1.38)
Medium run (9q to 20q)	-0.16	0.57	4.04***	0.09	0.73
	(0.39)	(0.54)	(1.55)	(0.69)	(1.95)
Observations	1,059,394	2,264,209	158,968	1,857,768	133,269
Events	34,174	73,039	5,128	59,928	4,299
Firms	21,898	37,216	4,714	32,567	4,007

Notes: This table reports the corresponding gender-specific duration estimates for new hires, employment, and average daily wages. Column (1) shows the effect of an additional birth on firm outcomes in the baseline 1-year regime (1985–1990), using the same specification as equation (1). Columns (2)–(5) compare the 2-year regime to the 1-year and 1.5-year regimes. For the 1996 reform, which reduced benefit duration from 2 years to 1.5 years, we reverse the sign so that both reform episodes can be read as increases in leave duration. Columns (2) and (4) report the simple-difference specification, which captures heterogeneity across leave regimes, while columns (3) and (5) report the reform-based difference-in-differences design. Standard errors are clustered by firm identifier, and the simple difference results are based on cell-weighted fixed effects regressions. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

C Data Appendix

C.1 Outcome Variables

Employees are defined as the number of workers employed by the firm, i.e., headcount employment (excluding workers on parental leave). Firm employment per quarter is calculated using daily information on start and end dates of employment for a given worker. For example, a person who works only one month in a quarter contributes 1/3 to employment. *Quarterly hiring* is the number of workers who join the firm. A worker is counted as a new hire if the worker is not employed at the firm in quarter $t-1$ and is employed in quarter t. Women on maternity or parental leave are treated as continuously attached to the firm for this purpose, so returning mothers are not counted as new hires in the main hiring series. *Wage bill* is the sum of all employees' earnings from the firm, measured in euros and adjusted for inflation to the year 2000. ASSD earnings are top-coded and available on a yearly basis. To calculate the quarterly wage bill, we first calculate the daily wage and multiply by employment days in the quarter. We adjust top-coded earnings with values derived from Pareto distributions (separately for men and women). In the total wage bill, we also include mini-job employment, which occurs when workers earn less than a defined monthly threshold that is updated with inflation. The quarterly wage for a mini job (which does not enter the earnings data) is imputed by taking the monthly contribution threshold and scaling it up to obtain the quarterly wage. *Average daily wage* is the total quarterly wage bill divided by the total quarterly number of employment days at the firm.

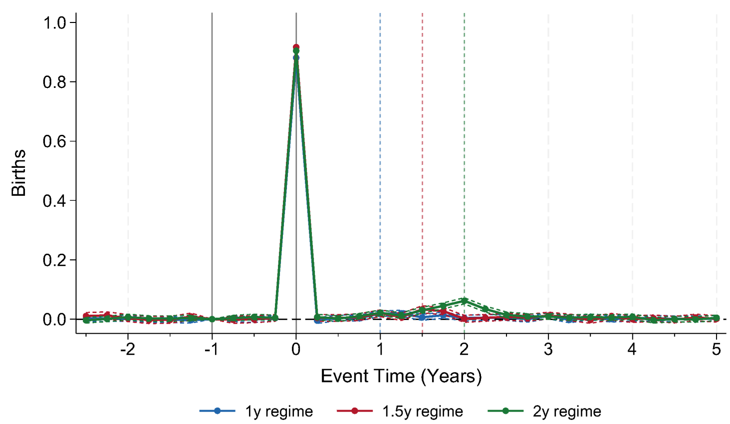
C.2 Detailed Matching Procedure and Sample Restrictions

We identify events from live births, which are recorded in the ASSD as they generate (mandatory) maternity leave of at least 16 weeks. In cases of pregnancy loss or still births the ASSD does not contain a birth spell so these events are not covered by our definition of event. The matching procedure is based on characteristics observed in the baseline quarter, four quarters before the birth ($k = -4$). This is the last calendar quarter before the beginning of treated women's pregnancy. For each treated female-worker/firm pair, we select a control female-worker/firm pair that exactly matches the following worker characteristics in the baseline quarter: (i) age (8 groups), (ii) earnings quintile, and (iii) tenure at the firm (< 2 and $2+$ years); and the following firm characteristics: (i) firm size (deciles), (ii) average earnings per worker (quartiles), (iii) firm age (< 3, $3 - 10$, $10+$ years), (iv) female employment share ($< 1/3$, $1/3 - 2/3$, $> 2/3$), (v) 1-digit industry, (vi) firm location (state), and (vii) turnover quartile within the past year. We also match on the quarterly date. For each treated female-worker/firm pair, we look for a control female-worker/firm pair in the same cell. We exclude potential control workers employed at the same firm as the treated worker from the control pool. Using this procedure, we find an exact match for 98% of female-worker/firm pairs in the treatment group. We exclude treated pairs without matches. For treated pairs with multiple control matches, we randomly select up to three control pairs and apply weights. Each treatment unit has the weight of one, and the weights of its control units sum to one.

Sample Restrictions We focus on firms that employ a woman at baseline $k = -4$ four quarters before she gives birth (or placebo birth), considering births that occurred between January 1986 and June 2000. We impose restrictions based on characteristics observed in the baseline quarter for both the women and their employing firms. The sample is restricted to Austrian nationals aged 18–40 years (98 percent of births), who are childless (as first births have the largest effects on women's labor market outcomes), and who have their primary employment in the firm at $k = -4$ (thereby restricting our sample to women with high attachment to the labor market). Firms are restricted to those in the private sector that employ 3–30 employees at baseline (this size class accounts for more than 30% of private sector employment and more than 90% of private sector firms) and that from $k = -4$ exist for at least one year. The main reason for focusing on smaller establishments is to obtain a sharp research design. We also exclude firms that have large seasonal employment fluctuations (changes of more than 20 workers between subsequent quarters), firms that experience extremely fast growth (to more than 100 employees within 6 years after baseline quarter), as well as firms in the tourism sector in three regions where tourism is the primary industry (Kärnten, Voralberg, and Tirol).

Pregnancy Shock vs Treatment Because we are worried about endogenous mobility after learning about the pregnancy, we allow women to move between the baseline quarter and the quarter of birth (or placebo birth). This means that the firm where the woman works when she learns about the pregnancy (the pregnancy shock from the firm's perspective) is not necessarily "treated" with a birth. Similarly, firms selected as controls, where the focal woman does not give birth for at least a year and there is thus no pregnancy shock from that woman, can still have workers who give birth, from other female workers employed there in the baseline quarter or who move to that firm, at any point in time. Combined, this means that the difference in the average number of births between the firms with and without a pregnancy shock will not be one. Or, in other words, the correlation between pregnancy shock (used to select treatment and control group) and birth (the treatment) is not perfect. However, in practice, the correlation is very high, visible in Figure C.6 (around 0.9 in all regimes). The first stage of regressing the probability of a birth on the pregnancy shock indicator has an F-stat in the 10,000s and the scaled-up estimates we report are close to the reduced form. Because of this, we do not pay much attention to the distinction between treatment (a birth) and the shock used for identification (the pregnancy). Importantly for interpreting longer-run dynamics, Figure C.6 also shows that post-event differences in births are close to zero, so differential follow-up fertility is limited.

Figure C.6
Effect of a Pregnancy Shock on number of Births in Firm



Notes: The figure plots the coefficients from equation (1) using births in the baseline firm as the outcome, estimated separately by parental-leave regime. Dashed lines indicate 95% confidence intervals.

D Wage-Bill Decomposition

This appendix reports the result of formally decomposing the overall change in the wage bill into wage and employment components in response to a birth event. This formalizes the visual evidence in Figure 2 which separately shows employment and daily wage effects, but is silent on the relative importance of the two margins in the overall firm response.

For any group of workers $g \in \{\text{Overall, Female, Male}\}$, the total wage bill is the product of employment days and the daily wage rate: $Y^g_{ek} = E^g_{ek} \times W^g_{ek}$. Because logarithmic transformations are sensitive to near-zero values and composition changes over time, we employ an exact additive decomposition of the wage bill (essentially a three-fold Oaxaca-Blinder decomposition. See e.g. Daymont and Andrisani, 1984; Jann, 2008.) The absolute change in the wage bill for any firm (event) relative to the baseline can be expressed as:

$$\Delta Y^{g}_{ek} = (E^{g}_{e,-4} \times \Delta W^{g}_{ek}) + (W^{g}_{e,-4} \times \Delta E^{g}_{ek}) + (\Delta E^{g}_{ek} \times \Delta W^{g}_{ek}), \tag{2}$$

where $\Delta W^{g}_{ek} = W^{g}_{ek} - W^{g}_{e,-4}$ and $\Delta E^{g}_{ek} = E^{g}_{ek} - E^{g}_{e,-4}$. Based on this identity, we construct three new dependent variables for each observation (event-by-k) to capture the partial effects:

1. **Daily Wage Component ($C1^{g}_{ek}$):** $E^{g}_{e,-4} \times \Delta W^{g}_{ek}$. This isolates the effect of changing daily wages, holding employment days fixed at baseline levels.
2. **Employment Component ($C2^{g}_{ek}$):** $W^{g}_{e,-4} \times \Delta E^{g}_{ek}$. This isolates the effect of hiring or reducing employment days, holding the daily wage fixed at baseline levels.
3. **Interaction Component ($C3^{g}_{ek}$):** $\Delta E^{g}_{ek} \times \Delta W^{g}_{ek}$. This absorbs the covariance between simultaneous adjustments in employment and wages.

Because ordinary least squares is linear, running our baseline dynamic difference-in-differences specification on these three components guarantees that their estimated coefficients will sum exactly to the total wage bill effect: $\boldsymbol{\beta}^{Y,g} = \boldsymbol{\beta}^{C1,g} + \boldsymbol{\beta}^{C2,g} + \boldsymbol{\beta}^{C3,g}$.

To allow us to formally test for statistical differences between these margins (e.g., whether the employment response is significantly larger than the wage response), we estimate the components simultaneously by stacking the three constructed components into a single dependent variable $C^{g,c}_{ek}$, where $c \in \{1, 2, 3\}$ indexes the component type.

We then estimate the following fully interacted equation:

$$C_{ek}^{g,c} = \gamma_e^c + \boldsymbol{\alpha}^c \boldsymbol{D}_{ek}^{Event} + \boldsymbol{\beta}^c \boldsymbol{D}_{ek}^{Event} \times Birth_e + \epsilon_{ek}^{g,c} \tag{3}$$

In this stacked specification, γ_e^c allows for component-specific event fixed effects. Standard errors are clustered at the event level to account for the mechanical correlation introduced by stacking multiple components for the same firm-quarter. This framework yields identical point estimates to running the equations separately, while computing the correct cross-equation covariance matrix required to test hypotheses of the form $H_0 : \beta_k^{C1,g} = \beta_k^{C2,g}$. We estimate this stacked model separately for the total wage bill, as well as for male and female sums of wages independently.

Table D.7 presents the results of the level-based decomposition. The 'Overall' column reproduces the aggregate wage bill effects we already discussed in the paper. To recap, Panel A shows a significant increase in the total wage bill during the anticipation period, followed by a return to baseline in the short and medium run. Panels B and C highlight a distinct gender divergence underlying this aggregate trend: while the wage bill paid to female workers drops significantly in the short run, this is covered almost entirely by paying more to male workers. Consequently, when a female worker takes leave, a significantly larger share of the firm's total wage bill is paid to men.

Decomposing these aggregate effects into daily wages, employment days, and their interaction reveals the margins of firm adjustment. During the anticipation period, the initial spike in the wage bill is driven almost entirely by the employment component. Across both male and female workers, firms expand their labor pool by hiring on the external labor market or increasing the retention of existing workers ahead of the focal worker's leave, with negligible changes to daily wages.[13]

[13]Note on Additivity: Due to the linearity of the estimator, the total wage bill effect (Column 1) in Panel A is exactly equal to the sum of the wage bill effects in Panels B and C. However, the individual

Table D.7
Decomposition of Wage Bill Effects into Wage and Employment Margins

	Overall	Wage Component	Employment Component	Interaction Component
Panel A: Total Wage Bill				
Anticipation (-3q to -1q)	615.5***	11.0	704.5***	-100.1*
	(85.0)	(58.8)	(85.0)	(52.7)
Short run (0 to 8q)	-89.5	-175.8	410.3**	-324.0***
	(169.5)	(124.2)	(166.6)	(116.2)
Medium run (9q to 20q)	305.7	-340.0*	747.1***	-101.4
	(274.7)	(200.2)	(262.6)	(189.1)
Panel B: Wage Bill of Female Workers				
Anticipation (-3q to -1q)	405.2***	-2.9	455.2***	-47.1
	(48.0)	(33.4)	(48.4)	(29.3)
Short run (0 to 8q)	-584.4***	-413.9***	-55.6	-114.8*
	(92.7)	(69.1)	(92.4)	(63.4)
Medium run (9q to 20q)	-130.8	-490.7***	259.3*	100.6
	(149.2)	(111.9)	(145.4)	(104.7)
Panel C: Wage Bill of Male Workers				
Anticipation (-3q to -1q)	210.2***	57.2	228.3***	-75.3**
	(52.1)	(35.5)	(53.1)	(31.2)
Short run (0 to 8q)	494.9***	135.0*	511.7***	-151.8**
	(105.1)	(75.6)	(104.5)	(70.9)
Medium run (9q to 20q)	436.5***	118.9	435.5***	-117.9
	(167.8)	(120.1)	(160.9)	(114.1)

Notes: Rows report period averages of coefficients from the baseline event-study specification applied to the total wage bill and to its three constructed components. Anticipation covers quarters $k = -3$ to $k = -1$, short run covers quarters $k = 0$ to $k = 8$, and medium run covers quarters $k = 9$ to $k = 20$. The wage component holds employment days fixed at the baseline level, the employment component holds the daily wage fixed at the baseline level, and the interaction component captures simultaneous changes in both margins.

In the short and medium run, the overall wage bill returns to baseline levels (Panel A). However, this net-zero aggregate effect masks a distinct compositional shift. In the short run, while the pure wage effect is insignificant, a highly significant negative interaction

decomposition components (Columns 2-4) are not strictly additive across groups. The overall components absorb between-group compositional shifts (e.g., substituting female hours for male hours), whereas the group-specific components only capture within-group adjustments.

term reveals that the firms increasing employment days the most are actively substituting toward lower daily-wage (part-time) workers. In the medium run, firms permanently maintain more employment days at lower average daily wages, replacing the single full-time worker with part-time equivalents.

Importantly, the compositional shift is highly gendered. The sharp decrease in the short-run female wage bill (Panel B) is driven mostly by the wage component, while the female employment component remains small and statistically insignificant. This indicates the firm fully replaces the woman on leave with other female workers, but relies on part-time or lower-paid substitutes. In the medium run, this shift expands: firms permanently maintain more female employment days at lower daily wages.

The picture is different for male workers. In both the short and medium run, the male wage bill increases, driven by more employment days of male workers. The wage component is positive but small and insignificant for male workers.[14]

E Part-time Employment

In this appendix, we investigate the role played by part-time employment in explaining the increase in headcount employment and the decrease in daily wages. We do not directly observe hours of work for our main sample, but we have two proxies available in sub-periods or after our main sample period. The first proxy we look at is mini-job employment, which applies when workers are earning less than a defined amount per month (the amount changes with inflation). Because the cut-off is applied monthly, it is more likely to capture part-time employment than cut-offs defined yearly, the frequency at which our main earnings variable is recorded (seasonal employment is common in

[14]Note that the stacked regression allows us to do a Wald test comparing coefficients on components and we reject the equality of wage and employment component in all specifications at least at the 5% level of statistical significance.

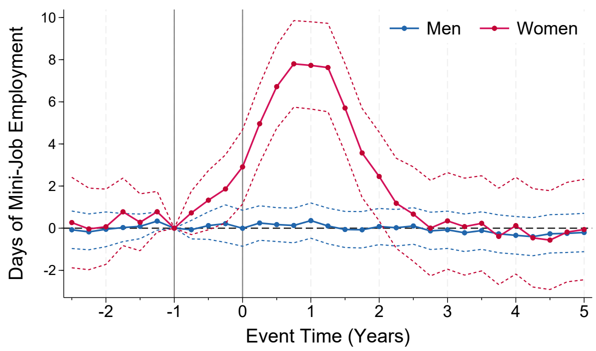
Austria). The ASSD only records mini-job employment from 1990 onward; thus, we can only use it for the 2y and 1.5y policy regimes. We apply our event study methodology according to equation (1) using days of mini-job employment as the dependent variable. Panel (a) of Figure E.7 plots the β-coefficients from this specification by gender. There is a strong increase in days of mini-job employment by women in treated firms after a birth event, in line with a shift towards more part-time employment at the firm. We do not find any change in mini-job employment among male workers.

Our second proxy for part-time employment comes from income tax data, which can be merged to the main ASSD sample and contain an indicator for part-time employment from 2002 to 2012. The income tax data are based on information annually supplied by firms to the tax authorities and define part-time work as contractual hours below 40 per week (or the number of hours defined as full-time employment in the collective bargaining agreement that applies to a particular worker). We again apply our main event study methodology according to equation (1) using as a dependent variable how many workers who were with the firm in a given quarter were classified as part-time in their annual tax receipt. Panel (b) of Figure E.7 shows that there is a strong increase in part-time employment among female workers before and after the birth. There is no corresponding change among male workers.

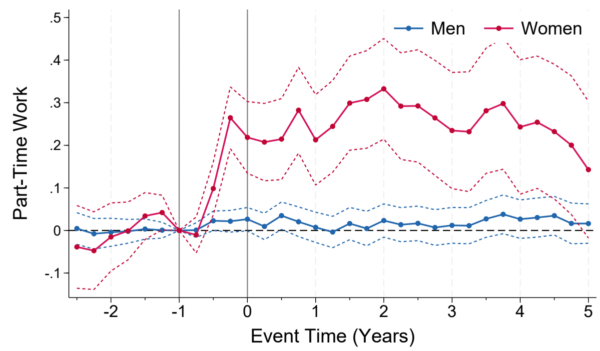
Third, we directly investigate the change in hours of work among mothers following a birth using data from the Austrian Microcensus. We focus on the period 1994-2015, which contains a harmonized measure for weekly hours of work (the measure was defined differently before 1994, as the survey was not yet based on the ILO concept of employment). The microcensus is a rotating panel that interviews households quarterly and follows households for two years. Based on this, we can observe future mothers a maximum of two years before birth but observe many women each year after first birth based on the age of the oldest child. To investigate the change in hours of work,

Figure E.7
Part-Time Employment

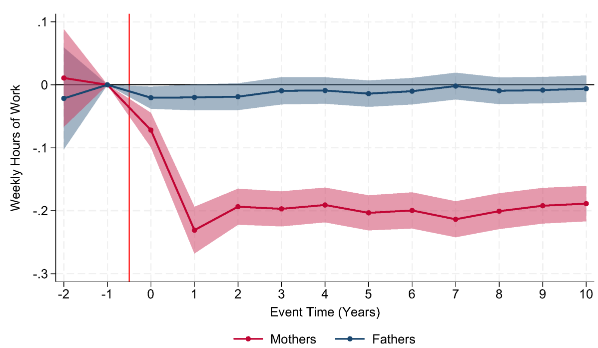
(a) Days of Mini-Job Employment



(b) Number of Part-Time Workers



(c) Child Penalty in Weekly Hours of Work



Notes: Panel (a) plots β-coefficients from specification (1) using days of mini-job employment at the firm as the dependent variable, available from 1990-2000. A mini-job is defined as a job with monthly earnings below a frequently changing cut-off and is generally counted for pension accumulation. Panel (b) plots β-coefficients from specification (1) using the number of part-time workers at the firm as the dependent variable, available from 2002-2008. We calculate the number of part-time workers from income tax receipts linked to the ASSD where firms declare workers that work less than full time (40 hours or what is stipulated by collective bargaining at the firm). Panel (c) plots child penalty estimates using weekly hours of work as the dependent variable including only employed mothers and fathers. The data come from the Austrian Microcensus. We select men and women currently employed and calculate event time based on when their oldest child was born (dropping non-employed/zero hours). Sample period: 1994-2015. Sample size: 23,190 men and 25,948 women. Estimates shown are event time coefficients divided by counterfactual hours of work. Dashed lines and shaded area around the graph indicate 95% confidence intervals.

we follow Kleven et al. (2019) and estimate "child-penalty" regressions, which allow us to estimate the percent reduction in weekly hours of work of mothers relative to the counterfactual of not having had a child. The baseline equation is

$$hours^{g}_{ist} = \boldsymbol{\alpha}^{g}\boldsymbol{D}^{Event}_{ist} + \boldsymbol{\beta}^{g}\boldsymbol{D}^{Age}_{ist} + \boldsymbol{\gamma}^{g}\boldsymbol{D}^{Year}_{ist} + \nu^{g}_{ist} \tag{4}$$

with weekly hours of work of individual i of gender g in year s who is observed at event time t. $\boldsymbol{D}^{Event}$ captures dummies for event time, defined as zero in the year the first child is born, and we also include dummies for age and calendar year. The event time dummy for $t = -1$ is omitted and the child penalty is defined as the percentage drop in hours of work relative to the counterfactual of not having a child:

$$P^{g}_{t} = \frac{\hat{\alpha}^{g}_{t}}{E\left[\tilde{Y}_{ist}\,|t\right]}$$

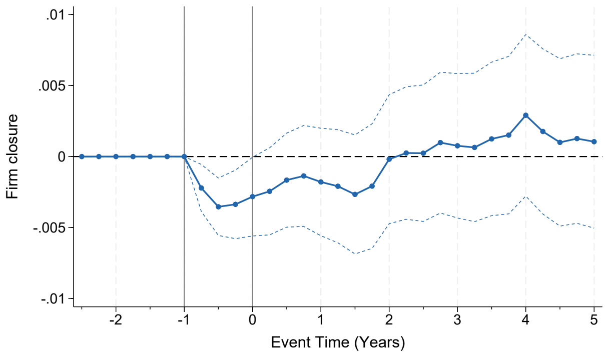
where $\tilde{Y}_{ist}$ are predicted hours of work after subtracting the predicted impact of event time $\hat{\boldsymbol{\alpha}}^{g}\boldsymbol{D}^{Event}_{ist}$.

For this sample, we only keep employed women and men to not confound the analysis of hours with the analysis of extensive margin employment (for child penalties in earnings and extensive margin employment among Austrian women, see Kleven et al. (2024)). Panel (c) of Figure E.7 plots the child penalties in weekly hours of work. We see about a 20% drop in hours of work among mothers that is persistent up to 10 years after first birth. There is no corresponding drop among fathers. This finding confirms that many mothers return to work part-time, leading firms to increase headcount with a corresponding drop in daily wages.

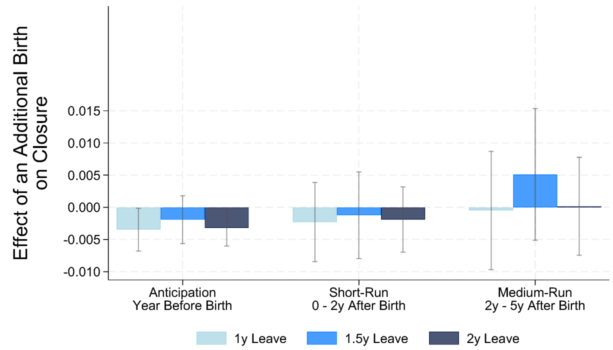
F Firm Closure

Figure F.8
Firm Closures

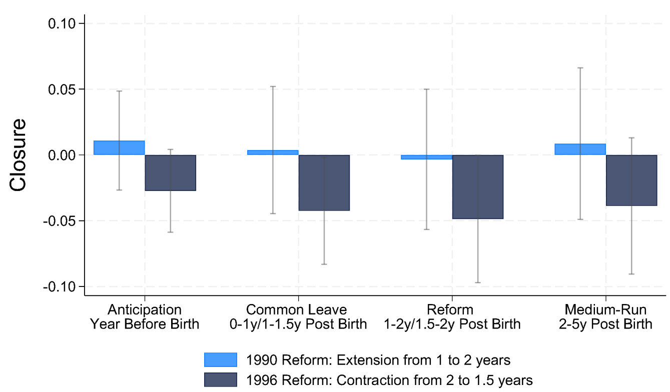
(a) Effects of an Additional Birth



(b) Effects of an Additional Birth by Regime



(c) Reform Effects



Notes: Closure is a binary indicator equal to one if the firm has permanently shut down and zero otherwise. Panel (a) shows pooled event-study estimates from the baseline dynamic difference-in-differences specification for all regimes pooled). Panel (b) shows a summary of regime-specific estimates. Panel (c) reports reform-based difference-in-differences estimates around the July 1 policy cutoffs (1990 and 1996), comparing births just before and just after the cutoff relative to the same within-year contrast in the preceding year.

This appendix examines whether childbirth-related leave absences affect the probability that the baseline firm permanently closes. *Closure* is a dummy variable that takes the value of one if a firm has shut down, using the permanent closure measure from Fink et al. (2010). Appendix Figure F.8 reports three complementary designs.

Panel (a) shows pooled event-study estimates from our baseline dynamic difference-in-differences design. Across event time, the closure estimates are close to zero. In the last observed quarter (five years after birth), the estimated effect of an additional birth on closure is 0.1 percentage points with a 95% confidence interval of $[-0.504, 0.713]$ percentage points, relative to a control-group closure risk of 7.0%.

Panel (b) reports a regime-specific analysis that allows the effect of an additional birth to vary by the parental leave regime in place at the time of birth. The point estimates are small and statistically indistinguishable from zero across regimes. The confidence intervals are also relatively tight in this design; for example, in the 1y-regime we can rule out increases in closure risk above about 0.01, which corresponds to an increase of roughly 11% relative to the control-group mean of 8.7% five years after birth.

Panel (c) reports reform-based estimates using our difference-in-differences design around the July 1 cutoff in the 1990 and 1996 reform years. These reform-based closure estimates are statistically noisy, reflecting the limited number of births used in the within-year reform comparison. For the 1990 extension, the estimated closure effect for the medium-run period is 0.009 (s.e. 0.029), which implies a 95% confidence interval of approximately $[-0.049, 0.066]$; For the 1996 contraction, the estimated medium-run effect is -0.039 (s.e. 0.026), implying a 95% confidence interval of approximately $[-0.090, 0.013]$.

Taken together, we do not find robust evidence that longer mandated leave durations increase firm closure. At the same time, the reform-based confidence intervals are wide enough that they do not rule out moderate effects (in either direction) on closure risk.

Fink et al. (2010) use a worker-flow approach to identify "real" bankruptcies as opposed to mergers and spin-offs. One limitation of their measure, however, is that their approach works for firms with five or more employees, so this measure is only available for a subset of our firms. As a robustness check, we also use an alternative measure of firm closure: an indicator variable that equals 1 if the firm has 0 employees. Results are shown in Figure F.9. They mirror the results presented above.

Figure F.9
Closure (zero employment)