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 that (i) firms adjust hiring, employment, and wages around leave periods, but these effects fade quickly; (ii) adjustments differ sharply by gender, reflecting strong gender segregation within firms; (iii) longer leave entitlements extend actual leave absences but have only short-term effects; and (iv) there is no impact on firm closure up to five years after birth.

JEL classification: H2, H5, J2, J08, J13
Keywords: family leave, firms, labor supply, labor demand, gender, absence duration

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1 Introduction

Childbirth is typically associated with a temporary workplace absence by new mothers. From the firm's perspective, this raises two questions. First, should the absent worker be replaced with a new hire or should the workload be reallocated to existing workers? The answer to this question depends crucially on the costs and frictions firms face when searching for a suitable replacement. The second question arises due to the temporary nature of the worker absence: Should firms' labor demand responses be stronger when temporary leaves last longer? To the extent that long leaves imply greater disruptions of workplace procedures, longer leaves may require stronger labor-demand adjustments.

This paper aims to address both questions. In a perfect labor market, a temporary worker absence does not impose costs on firms. Firms can hire a temporary replacement worker and dismiss her as soon as the absent worker returns. In a more realistic setting, with labor market frictions due to hiring, training, and firing costs, (Oi, 1962), temporary leaves may become costly for firms and these costs may be larger with longer leave durations. Consistent with this argument, employer representatives often strongly oppose extensions of government-mandated parental leaves. This is the case even in systems where income replacement benefits are government funded, and leave extensions therefore do not directly increase firms' labor costs.

Our analysis focuses on the Austrian labor market. There are two main reasons why the Austrian context is particularly interesting. 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, the Austrian context may offer an upper bound for firms' labor demand responses to a temporary leave absence. A second reason why the Austrian case is of particular interest is its drastic parental leave policy changes that have been implemented over the last decades. This provides us with an ideal setting to study how the duration of leave absences affect the labor demand responses of firms.

Using the Austrian social security register database (ASSD), a matched employer-employee dataset covering the entire population of private-sector workers, we document four main findings. First, firms with a parental leave absence ("treated" firms) maintain medium-run employment levels and total wage bills that are comparable to those of similar firms without a parental leave absence ("control" firms). Short-run employment adjustments primarily occur through the hiring margin, with the size and timing of additional hires indicating that firms anticipate temporary absences and effectively secure replacement workers in advance.

Second, there are significant gender differences in firms' employment and earnings responses. Our findings show that replacement workers are almost entirely female; that there is a significant, albeit temporary, reduction in the female wage bill of treated firms, accompanied by an increase in the male wage bill. This latter effect persists over time: in response to a parental leave absence, firms' labor input shifts toward a greater reliance on male workers. These responses are inconsistent with a gender-blind labor market where female and male employees are perfectly substitutable and treated equally by employers. Instead, the evidence suggests there is strong within-firm gender segregation.

Third, we find that firms' labor demand responses do not systematically change with the duration of government-mandated parental leave. Female replacement hires are equally important, and male hires are hardly affected by the generosity of the parental leave regime. Firm responses are purely "mechanical" in the sense that parental leaves last longer and the short-run effects are more spread out, but there are no differential effects in the medium run: five years after the childbirth, headcount employment and the total wage bill are not differentially affected by the leave duration.

Fourth, we find no statistically or economically significant effects on firm closure through five years after the birth event. Neither an additional birth at the firm nor a longer or shorter duration of parental leave affect the probability that the firm goes bankrupt.

Our paper relates to various strands of the literature. First, it relates to the literature examining 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 co-workers. 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 the importance of hiring costs and their variation across the business cycle. These studies typically find substantial costs associated with recruiting or replacing workers.[1]

Second, our paper contributes to the literature on gender segregation in the labor

[1]Recent literature 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).

market (Sorensen, 1990; Blau and Kahn, 2017), complementing evidence that gender segregation across firms is widespread, particularly among small employers (Carrington and Troske, 1995). Recent evidence, consistent with gender-segregated workplaces, reveals substantial within-firm pay disparities between women and men (Card et al., 2016).

Third, our paper contributes to a better understanding of how parental leave policies impact firms. Recent studies have examined how the generosity of parental leave programs influences firms' labor demand responses, yielding inconclusive empirical evidence.[2] Ginja et al. (2022) and Gallen (2019) investigate parental leave reforms in Sweden and Denmark, respectively, and report substantial negative effects on firm performance. In contrast, Brenøe et al. (2024) report no adverse impact of parental leave absences on Danish firms. Huebener et al. (2024) find that the impact of a German parental leave reform was very weak. Their findings align closely with ours. This is noteworthy and surprising, given the distinct nature of the German and Austrian reforms. The German reform increased parental leave benefits during the first year for mothers with sufficiently high earnings. The Austrian reforms modified parental leave durations—either extending or shortening them—while keeping benefits unchanged and uniformly affecting all mothers.

The rest of this paper is structured as follows. Section 2 introduces the institutional background and the data. Section 3 describes our empirical design. Section 4 presents the results, followed by a discussion in Section 5.

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.[3] Following maternity leave, parental leave begins. During this period, the mothers' job is protected, and they are entitled to a fixed cash benefit funded

[2]For a discussion on how the introduction of paid leaves in U.S. states affected firms, see Rossin-Slater (2018). Appelbaum and Milkman (2011) and Bartel et al. (2016) show that employers were not adversely affected by paid leaves in California and Rhode Island. Goldin et al. (2020) discusses potential benefits of parental leaves for firms and what may induce them to provide paid leaves voluntarily.

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

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.

The parental leave reforms of 1990 and 1996 Our analysis exploits two reforms to the Austrian parental leave system that led to significant changes in leave durations and thus provides us with three distinct regimes in terms of mothers' entitlements after giving birth. Before July 1990, job protection and cash benefits were granted until the child's first birthday (1y-regime). Between July 1990 and June 1996, job protection and cash benefits were extended to the child's second birthday, increasing the maximum duration by 12 months (2y-regime). Between July 1996 and June 2000, the benefit duration was effectively reduced to 18 months, while job protection remained at 24 months (1.5y-regime).[4] Lalive and Zweimüller (2009), Lalive et al. (2013) 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.[5]

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. With firm identifiers, we can identify the precise timing and length of temporary leaves.

We aggregate our main firm variables to the quarterly level. *Days of parental leave*: the sum of all female employees' days of maternity and parental leave at the firm. *New hires*: the number of workers who join the firm. *Employees*: the number of workers employed by the firm, i.e., headcount employment (excluding workers on maternity or parental leave). *Wage bill*: the sum of all employees' earnings from the firm (in euros, 2000 CPI). *Average daily wage*: Total wage bill divided by total days of employment (excluding workers on maternity or parental leave) at the firm. *Closure*: a dummy variable that takes the value of one if a firm shuts down during that quarter or has shut down in a previous

[4] Both parents were still entitled to 24 months of benefits, but 6 months were earmarked for fathers. However, uptake among fathers was almost zero.

[5] Neither of the two reforms could be anticipated by mothers who gave birth around the reform dates, as affected mothers were already pregnant when the reforms were decided in parliament. This quasi-random assignment of mothers to treated and control groups motivates our second empirical design to estimate the effect of leave durations (Section 4.3).

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

quarter, using the permanent closure measure from Fink et al. (2010). If an outcome is not observed due to firm closure, we set its value to zero.

3 Sample and Research Design

Our goal is to study the labor demand responses of firms experiencing a negative labor supply shock, in our case resulting from a worker giving birth. Simply comparing firms experiencing a birth to those that do not potentially leads to biased effects from endogenous mobility. This mobility 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 think of these firms as treated even though the women in some cases move to other firms by the time of the birth. This is a classical intention-to-treat effect, and we scale our estimates by the first stage to get the effect of an additional birth instead of a pregnancy. The first stage is the effect of the pregnancy shock on the number of births in the same firm. In practice there is not much mobility in the year before birth so the scaled-up estimates are close to the reduced form. Because the difference is small, we will mostly omit the distinction between shock and treatment for the sake of brevity. For details see Appendix D.

Control and treatment groups The timing of the pregnancy shock is arguably random but firms whose workers are likely to become pregnant are on average not 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 select the treatment group from women who give birth in quarter $k = b$. The control group is sampled from a set of women 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. We require that both sets of women are childless before $k = b$. 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, for the period between 1985 and 2000. We make other minor restrictions on women and their respective firms; see Appendix D for details.

Final sample Our analysis sample comprises 33,618 first births in 23,837 unique firms. Appendix Table A.1 presents summary statistics for treated and control groups in the baseline quarter, both for women and their respective firms overall, as well as for different parental leave periods. Overall, mothers are on average 23 years old, earn 3,580 euros (CPI 2000) quarterly, and have been with the firm for 3.8 years. Their respective firms are 12.7 years old and have 8.8 employees, of which 82% are female.

4 Empirical Results

4.1 How Do Firms Respond to an Upcoming Temporary Leave?

Empirical strategy To quantify how firms adjust their labor demand upon learning that a female employee is pregnant and will soon take temporary leave, we estimate the impact of one additional birth within the firm with the following dynamic difference-in-differences equation:

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

where Y_{ek} is outcome Y for event e (woman-firm-baseline quarter) in quarter k. γ_e represents event-specific fixed effects, and $\boldsymbol{D}_{ek}$ 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 ($\boldsymbol{D}^{Event}_{ek} \times Birth_e$) with the interaction of the pregnancy event and the event quarter (see Appendix D.2 for details). Due to 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). We refer to the three quarters before birth ($-3 \leq k \leq -1$) as the anticipation period, in the two years after birth ($0 \leq k \leq 8$) as the short-run and the period thereafter through five years after the birth ($9 \leq k \leq 20$) as the medium-run. The employer will be informed 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 to inform the employer as soon as the pregnancy is known.

The identifying assumption is that the timing when the employer is informed about the pregnancy is not related to the expected future firm performance (parallel trends). We can check the plausibility of this assumption by examining trends in outcomes before the employer knew about the pregnancy.

The effects of an additional birth and leave-taking Panel a) of Figure 1 shows that treated firms adjust their hiring right after learning that a pregnant employee will soon go on leave. Learning that an additional employee will give birth and take parental leave increases the number of new hires already two quarters before the birth and soars in the quarter right before the birth. After the birth, the number of new hires returns to its baseline level. The cumulative hiring in $k = -3$ to $k = 0$ shows almost complete replacement: the sum of these coefficients yields 0.89 excess hires of treated firms relative to control firms.

Panel b) of Figure 1 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, suggesting that treated firms begin hiring replacement workers even while the expectant woman is still at her workplace. 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 permanently above the level in control firms, with a statistically significant difference of 0.15 employees. However, an increase in headcount does not necessarily translate into higher labor input, as former full-time workers may be replaced by part-time workers, and mothers often return to part-time employment. Consistent with this, panel c) shows a decline in daily wages. Appendix C provides additional evidence supporting the interpretation that the persistent increase in headcount is driven by a higher prevalence of part-time employment.

A dynamic pattern similar to that of headcount employment emerges for firms' total wage bill, shown in panel d) of Figure 1. The wage bill in treated firms already starts

Figure 1
Dynamic Effects of an Additional Birth and Leave-Taking on Firm Outcomes

Notes: Each graph plots the β-coefficients from equation 1, capturing the differential changes in treated firms' outcomes compared to control firms, relative to the baseline quarter. Left-hand-side panels represent the outcome variables for all employees, right-hand-side panels represent outcomes by gender (blue for men and red for women). Outcomes are described in detail in Section 2. Dashed lines indicate 95% CIs.

to exceed that of control firms in the anticipation period. In the quarter before birth, the difference is about 1,000 euros, corresponding to about 28% of 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, up to five years after the event, having an additional birth in the firm does not affect the total wage bill significantly. Overall, we conclude that temporary leave after childbirth does not have a lasting impact on firms' medium-run labor costs.

4.2 Differential labor demand responses by gender

In panels e)–h) of Figure 1, we examine the presence of gender bias in firms' labor demand responses. Panel e) disaggregates the results on excess new hires by examining female and male hires separately. In a gender-neutral labor market, excess hires would equally likely be female or male. However, panel e) reveals a pronounced gender bias: nearly all the excess new hires in treated firms are female. A minority of the excess hires in treated firms are male (9% in the anticipation period and 17% in the short-run), a pattern consistent with significant gender segregation within the firm in the allocation of workers to specific jobs or tasks.

Panel f) shows how the dynamics of female and male hiring translate into a change in the gender composition of the firms' headcount employment. Right after the baseline quarter, female employment in treated firms initially increases, then sharply declines in the event quarter when the treatment woman gives birth, and subsequently takes around a year to recover. The temporary decrease in female employment is partially offset by a persistent small increase in male employment. This increase in male employment is mainly driven by new male employees, as we only find evidence of a small effect on the retention of incumbent male employees (Appendix Figure B.1). In line with this, as shown in panel g), average daily wages for men are hardly affected, whereas we would expect an increase if firms were trying to increase retention. In the medium run, men constitute around half of the permanent increase in the 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 C for a more detailed discussion). The decrease in the average daily wages of women persists, suggesting a permanent change in the structure of labor inputs in the 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, a significant difference remains, with more of the wage bill going to men and less to women, even though the total wage bill for all employees is not affected. Overall, the evidence suggests that the workforce becomes more "male" when a female worker gives birth and takes parental leave, but far less "male" than we would expect if the labor market were gender neutral in terms of the gender of new hires.

Robustness In Appendix Figures B.2 to B.5, we show that labor demand responses are qualitatively similar when we split the sample by the temporarily absent employee's previous wage, tenure, or wage rank within the firm. We conclude that the gender differences in firms' labor demand responses are highly robust and pervasive across many subgroups.

4.3 The Effects of Government-Mandated Parental Leave by Duration

The analysis in Figure 1 pools births across three different parental leave regimes that were in place on the date the mother delivered the child. Hence, the estimated effects on the employment dynamics and costs correspond to the average treatment effect across the respective parental leave regimes. We now ask whether the duration of government-mandated parental leave makes a difference for firms' labor demand responses. To explore this, we employ two distinct designs.

Treatment-intensity design Our first approach allows the effect of an additional birth to vary across parental leave regimes by interacting event time with regime dummies ($\boldsymbol{Regime_e}$), indicating the regime in place at the time of birth, for the 1y-regime (pre-1990), the 2y-regime (1990-1996) and the 1.5y-regime (1996-2000):

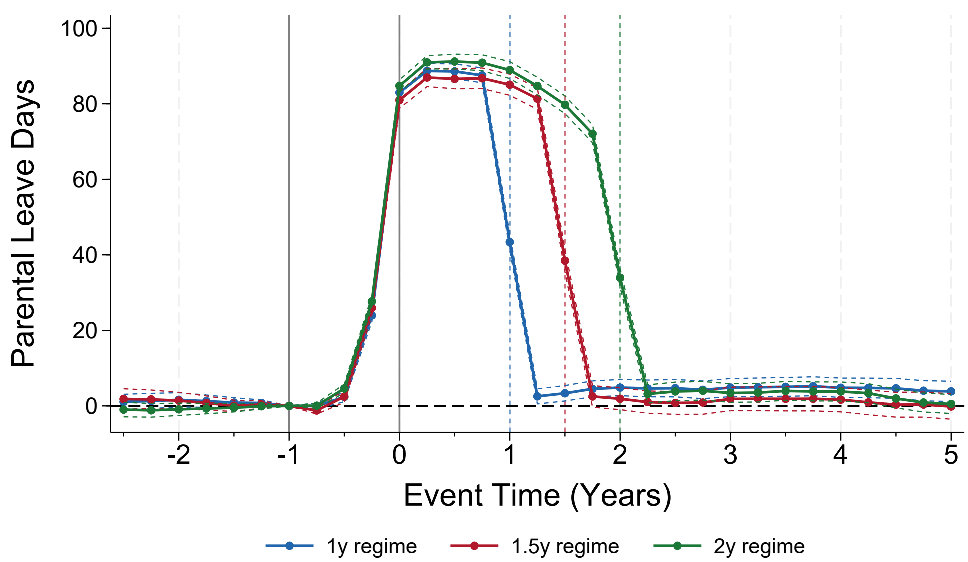
$$Y_{ep} = \gamma_e + \boldsymbol{\alpha} \boldsymbol{D}_{ep}^{Event} + \boldsymbol{\alpha}^r \boldsymbol{D}_{ep}^{Event} \cdot \boldsymbol{Regime}_e + \boldsymbol{\beta}^r \boldsymbol{D}_{ep}^{Event} \cdot Birth_e \cdot \boldsymbol{Regime}_e + \epsilon_{ep}. \quad (2)$$

The notation is as before but we now focus on three distinct sub-periods relative to the birth and aggregate event time k to periods p (so α and β are vectors over p instead of

k). (i) the *anticipation* period $p = -1$, when the employee becomes pregnant and informs the employer ($-3 \leq k \leq -1$), (ii) the *short-run* period $p = 1$ during which the employee is either entirely (1990-1996 regime) or partly (pre-1990 and post-1996 regimes) entitled to parental leave benefits ($0 \leq k \leq 8$), and (iii) the *medium-run* period $p = 2$, when parental leave eligibility has expired even in the most generous regime ($9 \leq k \leq 20$). We now estimate regime-specific effects of treatment $\boldsymbol{\beta}^r$ relative to flexible regime-specific baseline event time effects $\boldsymbol{\alpha}^r$. As before, we normalize relative to the baseline period.

Figure 2 shows that treated firms experience a large increase in parental leave days compared to control firms, starting the quarter before the birth event. The duration of this treatment effect is closely linked to the parental leave regime in place at time of birth. Parental leave absences are relatively short under the 1y-regime (before 1990), very long under the 2y-regime (1990–1996), and intermediate under the 1.5y-regime (post-1996). The 1990 and 1996 reforms generate substantial variation in actual worker absences after childbirth, allowing us to investigate how the expected duration of temporary leave affects firms' labor demand responses.

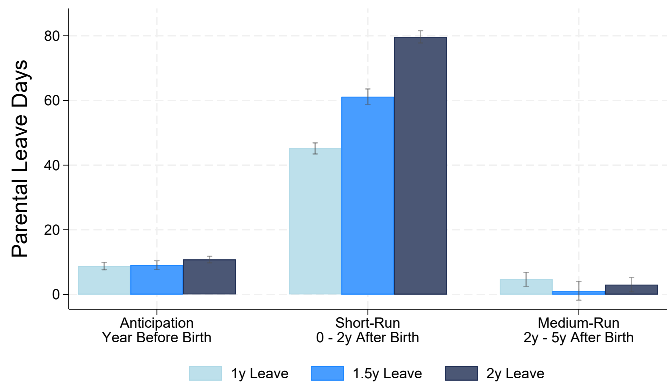
Figure 2
Difference in Parental Leave Days between Treated and Control Firms, by Regime



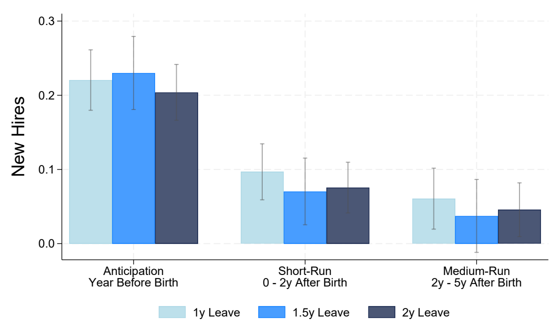
Notes: Each line represents the β-coefficients from equation (1) separately for each of the regimes. Therefore, each line captures the differential changes in the number of parental leave days in treated firms compared to control firms by regime. Dashed lines indicate 95% CIs.

Figure 3
Effects of an Additional Birth and Leave-Taking on Firm Outcomes by Regime

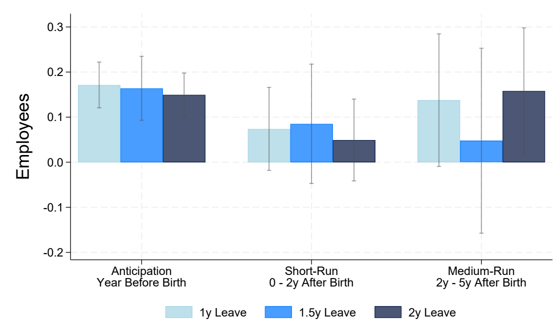
(a) Parental Leave Days



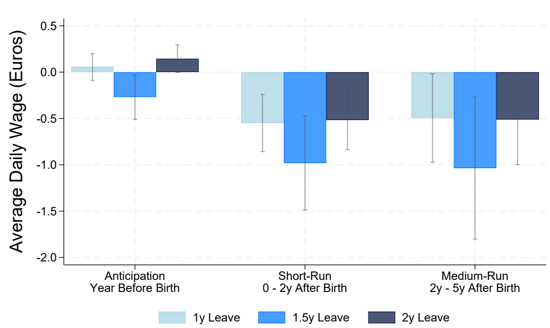
(b) New Hires



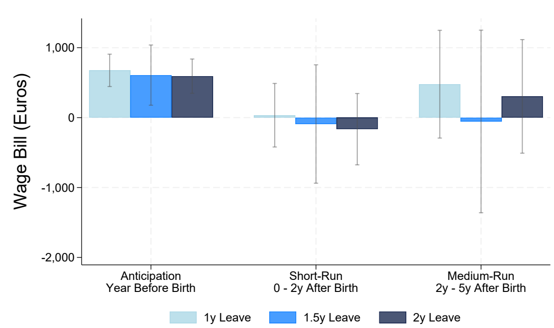
(c) Employees



(d) Average Daily Wage



(e) Wage Bill



Notes: Each bar displays β^r from equation (2) for a specific regime r and period relative to the birth event. Each bar therefore captures the effects on treated firms' respective average outcomes relative to control firms in the anticipation, short-run and medium-run period. Appendix Table A.2 shows the estimates in table format. For results by quarter, see Appendix Figure B.6, which instead plots the quarterly βs (as in equation (1)) separately for each regime.

Figure 3 shows the effect of an additional birth in the firm on firm outcomes by policy regime. Panel a) shows that more generous parental leave regimes lead to longer leave durations in the short-run period. In the short run (the quarter of the birth event and the following eight quarters), there are strong differences between regimes, consistent with the evidence presented in Figure 2. Panel a) also shows that, for all regimes, treated firms experience more parental leave days during the anticipation period than control firms. This is attributed to the maternity protection period and possibly to prenatal sick leave. The small effects in the medium-run are likely driven by second births.

Panel b) shows the results for new hires. The number of excess hires per quarter in treated firms compared to control firms is large during the anticipation period and remain positive, though substantially lower, during the short- and medium-run periods. However, the key finding is that hiring during the anticipation, short-, and medium-run periods is of similar magnitude across the regimes, despite the substantial differences in the duration of parental leave absences. Moreover, similar to the results by gender in panel e) of Figure 1, the excess hires are predominantly female across all policy regimes (Appendix Figures B.7 and B.8).

Panels c), d) and e) display the results for headcount employment and wages. The dynamics across sub-periods mirror our findings in the pooled analysis (Figure 1) and we do not see substantial differences across regimes. Results for gender-specific firm outcomes are also similar across regimes with the strongest adjustments for female employment and wage bill, compared to their male counterparts (Appendix Figures B.7 and B.8). Despite the fact that the actual duration of parental leave varies by one year between the shortest and longest regimes, this does not materialize in systematic differences in treated firms' labor demand responses. We conclude that firms cope surprisingly well with parental leave absences, even when government-provided parental leave mandates allow employees to take leaves of very long durations.

Discontinuity design An important caveat of the above analysis comparing regimes is that causal effects of changes in parental leave duration may be confounded by business cycle effects or time trends affecting firms during the different regime periods. To address this concern, we now go one step further and zoom in on the 1990 and 1996 reforms. Specifically, we compare female-worker/firm pairs where the mother delivered a child shortly before and shortly after July 1^{st} 1990 and July 1^{st} 1996, the dates when the reforms were implemented, relative to comparable pairs from a year before the reforms. With this difference-in-differences design, we estimate the causal effect on firm outcomes of, respectively, a 1-year increase (1990 reform) and a 0.5-year decrease (1996 reform) in the maximum duration of government-mandated parental leave. Put differently, we compare the outcomes of firms that all had a worker give birth and take parental leave, but the duration of parental leave absence varied. To capture this, we run the following regression separately for births in 1989-1990 and 1995-1996:

$$\begin{aligned} Y_{ep} =& \gamma_e + \boldsymbol{\alpha} \boldsymbol{D}_{ep}^{Event} + \boldsymbol{\alpha}^F \boldsymbol{D}_{ep}^{Event} \cdot Fall_e + \boldsymbol{\alpha}^R \boldsymbol{D}_{ep}^{Event} \cdot Reform_e \\ &+ \boldsymbol{\delta} \boldsymbol{D}_{ep}^{Event} \cdot Reform_e \cdot Fall_e + \epsilon_{ep}, \end{aligned} \tag{3}$$

where the notation is as in equation (2), and we include interaction terms with a dummy for whether the birth was in the fall (*Fall*, i.e., after 1^{st} July) or spring and in the reform year (*Reform*, i.e., 1990 and 1996) or the control year (1989 and 1995).[7]

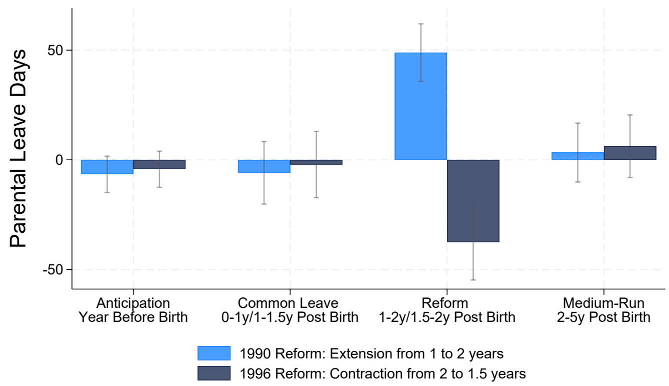
Figure 4 presents estimates of δ, the difference-in-differences effect comparing firms with a birth before and after the reform relative to the previous year, in five panels. Slightly different from Figure 3, we now divide the observation window into four subperiods by splitting the short-run into a "common leave period" and a "reform period". The "common leave period" is defined as quarters $k = 0$ to $k = 3$ for the 1990 reform, and as $k = 0$ to $k = 5$ for the 1996 reform. The "reform period" is defined as quarters $k = 4$ to $k = 8$ (extended leave period) for the 1990 reform, and $k = 6$ to $k = 8$ (shortened leave period) for the 1996 reform. The bars in Figure 4 show the effect on the respective outcome of treated firms of a 1-year increase (light blue) and of a 0.5-year decrease (dark blue) in the maximum duration of parental leave.

Panel a) shows the impacts of the two reforms on the actual parental leave take-up. As expected, the reforms only affected parental leave take-up during the reform period. Average parental leave take-up increased by nearly 50 days per quarter in response to the 1-year increase in maximum parental leave duration, and it decreased by more than 30 days in response to the 0.5-year parental-leave decrease. Thus, the reforms generated

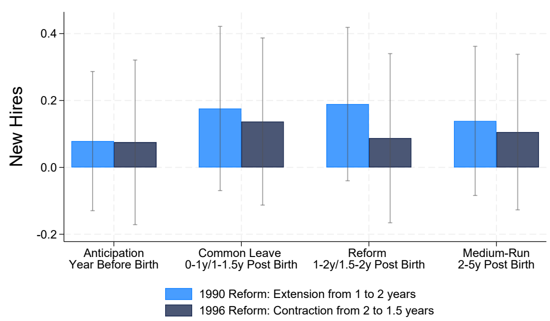
[7] The identifying assumption for this design to be valid is that mothers cannot manipulate the exact timing of births. As explained in section 2, in our context this assumption is likely to hold because the reform was announced after the mothers got pregnant. To address this concern, we check the robustness of our results to excluding births two weeks around the cutoff and find similar results (Appendix Figure B.9).

Figure 4
Reform Effects of Extended and Reduced Parental Leave on Firm Outcomes

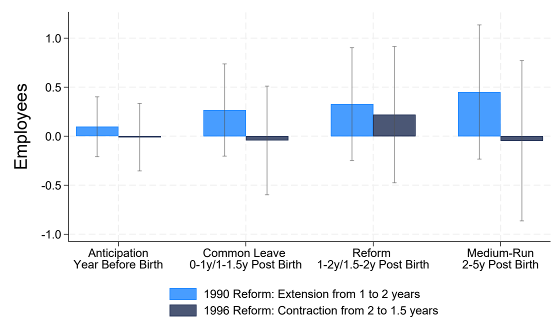
(a) Parental Leave Days



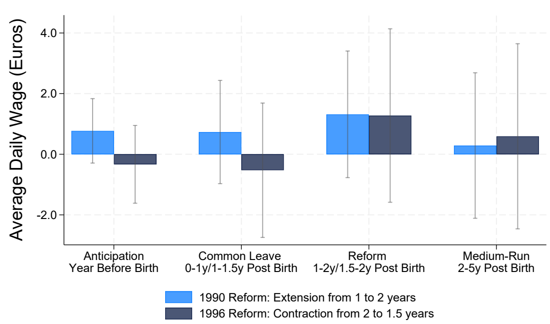
(b) New Hires



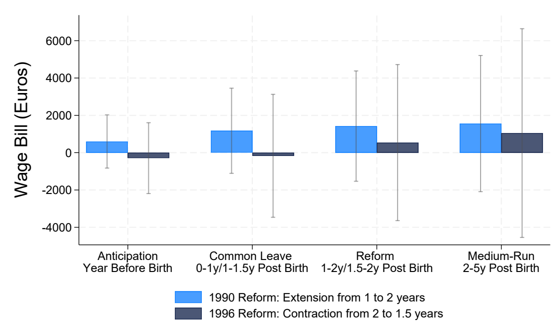
(c) Employees



(d) Average Daily Wage



(e) Wage Bill



Notes: Each bar displays δs from equation (3). For both reforms, the graph plots the average δ_ks across the period in question. Each bar therefore captures the effect on treated firms' respective outcomes of a 1-year increase in maximum parental leave duration (first bar, in blue, from the 1990 reform) and a 0.5-year decrease (second bar, in dark blue, from the 1996 reform). For a table representation of these estimates see Appendix Table A.3.

the intended changes in actual take-up of parental leave and show that leave absences are significantly affected by mandated leave duration.

We find no systematic or statistically significant effects of the 1-year increase or 0.5-year decrease in maximum parental leave duration on treated firms' hiring, headcount employment, average daily wage, or total wage bill [panels b)–e)]. Notably, the 1990 and 1996 reform coefficients point in the same direction despite the asymmetric policy changes, possibly due to increased uncertainty—a common challenge in policy evaluation. This contrasts with our treatment intensity analysis based on an additional birth, which occur in a general equilibrium setting. Overall, results from the discontinuity design closely mirror those of the treatment intensity approach.

In sum, while the 1990 and 1996 reforms substantially changed the maximum duration of government-mandated parental leave—and actual leave durations adjusted accordingly—these shifts did not translate into meaningful changes in labor demand among treated firms. Although firm outcomes respond when a female employee goes on leave, these responses do not vary systematically with the mandated leave duration.

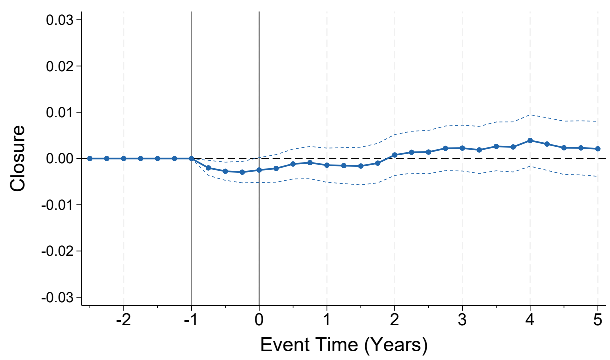
4.4 Do Long Parental Leaves Threaten Firm Survival?

Finally, we explore whether the long leave granted to employees after giving birth may increase the risk that the firm goes bankrupt. In Figure 5, we explore whether firms are more likely to go bust when a female worker gives birth and takes leave, and whether this depends on the duration of the leave. Panel a) compares treated and control firms, irrespective of the particular parental leave regime under which the new mother is eligible. From this graph, it is clear that there is no statistically or economically significant effect of an additional birth at the firm on the probability of firm closure. In the medium-run, we see no statistically significant or meaningful effect on firm closure. For instance, in the last observed quarter, the effect of an additional birth in the firm on closure is 0.2 percentage points (CI: -0.00384, 0.00804), compared to an average risk of firm closure of 7.0% among control firms.

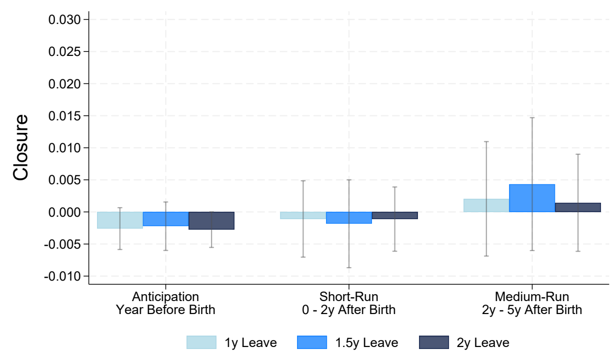
Panel b) splits the effects of an additional birth in the firm by regime. All coefficients in panel b) are small and statistically insignificant, suggesting that even very long durations of government-mandated parental leave do not provide an existential threat to firms when a female employee goes on leave after childbirth. Based on the 95%

Figure 5
Firm Closures

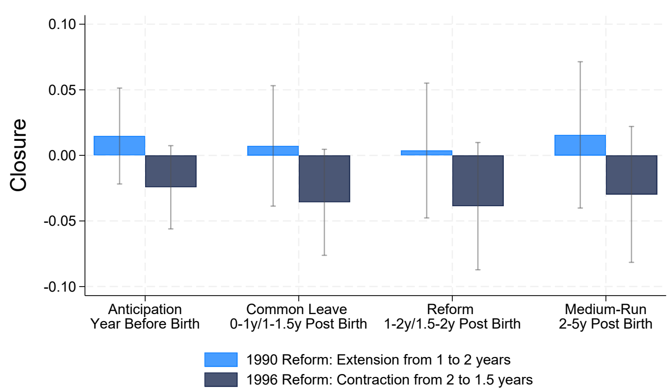
(a) Effects of an Additional Birth



(b) Effects of an Additional Birth by Regime



(c) Reform Effects



Notes: Closure is a binary indicator taking the value one if the firm has permanently shut down and zero otherwise. Panel a) shows estimates of β according to equation (1), see notes to Figure 1. Panel b) plots $\boldsymbol{\beta}^r$ from equation (2) for a specific regime r and period relative to the birth event, see notes to Figure 3. Panel c) shows difference-in-differences estimates from the discontinuity design, see notes to Figure 4 for details.

confidence intervals, we can rule out effects above around 0.01 in the 1y regime, corresponding to an 11% increase in the probability of closure over the control group average, which is 8.7% five years after birth.

Panel c) applies the discontinuity design. In line with panel b), we do not find statistically significant evidence for a change in the probability of closure at any point after the birth, whether the maximum leave duration is extended from one to two years or reduced from two to 1.5 years. Overall, the evidence suggests that longer durations of parental leave are not associated with a higher risk of firm closure.

5 Discussion

Our analysis shows that employers adapt well to temporary leaves following childbirth. This finding may be surprising, given that temporary leaves in Austria tend to be unusually long due to the country's generous parental leave policies. This result contrasts with much of the existing literature, which finds that hiring and replacement costs are substantial and impose significant burdens on 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 first and obvious reason for this discrepancy is that a temporary leave after childbirth is typically communicated to the employer several months in advance. Informing the employer early on is incentivized by labor regulations that protect the health of the mother and child and provide enhanced employment protection. By contrast, an unexpected worker exit, such as the sudden death of a worker, may cause significant disruption to workplace procedures and/or necessitate substantial recruitment efforts. These costs can often be mitigated when a worker's absence is anticipated and can be properly managed. The notion that anticipating worker absence reduces costs to firms is further supported by the fact that the labor laws of many countries require advance notice of a predetermined length of time before an employee can leave the firm.

Anticipation (or non-anticipation) of a temporary leave after childbirth also reconciles the seemingly contradictory findings of recent papers addressing how firms respond to changes in government-provided parental leave mandates. Ginja et al. (2022) for Sweden and Gallen (2019) for Denmark find adverse effects on firms of parental-leave extensions which were implemented retrospectively and caught firms by surprise. In contrast, no such negative responses are found for the German parental leave benefit increases (which resulted in higher parental leave take-up) studied in Huebener et al. (2024), and the Austrian parental leave reforms studied in this paper. These reforms were anticipated by firms and could be accommodated by forward-looking labor demand responses.

A second reason why a temporary leave after childbirth may be more easily accommodated by the firm relates to the specific skills of the exiting worker. Compared to the deceased workers in Jäger et al. (2024), women giving birth to a child are younger, have lower levels of tenure, and occupy lower job ranks within the firm. As a result, they have accumulated less firm-specific human capital and may be easier to replace. Our finding that the temporarily absent worker is almost fully replaced through new hires aligns with this argument.

Statistical discrimination is another reason why young women may end up in jobs that are easily substitutable. The standard argument is that employers statistically discriminate against women of childbearing age due to their higher probability of leaving the firm after childbirth (Xiao, 2023; Peterson Gloor et al., 2022). Several recent papers show that women are typically allocated to lower ranks within the firm (Bayer and Kuhn, 2023; Benson et al., 2024; Bronson and Thoursie, 2021). For instance, Benson et al. (2024) demonstrate that young women are often denied promotions. The allocation to more easily substitutable jobs within firms may not only be rooted in discrimination, but also shaped by gender differences in occupational choices (Adda et al., 2017) or by women being less likely to seek promotions associated with leadership roles (Haegele, 2024). Azmat et al. (2022) emphasize that the higher substitutability of women originates from self-selection into jobs requiring less presenteeism. Similarly, Hotz et al. (2017) find that workers in more family-friendly jobs are more easily substitutable for one another, facilitating mothers' ability to balance work and family responsibilities.

While the empirical evidence we present is consistent with these explanations, it is beyond the scope of this paper to explore their relative importance. We think this is a fruitful avenue for future research.

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“How Do Firms Respond to Parental Leave Absences?”

Online Appendix

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A Appendix Tables

Table A.1
Summary Statistics of Baseline Characteristics

	Pooled		1y Regime		2y Regime		1.5y Regime	
	Control	Treat	Control	Treat	Control	Treat	Control	Treat
Woman								
Age	23.14	23.18	22.01	22.10	23.21	23.26	24.69	24.66
	(4.57)	(4.33)	(4.15)	(3.94)	(4.39)	(4.16)	(4.99)	(4.72)
Wage	3,556	3,580	2,992	3,014	3,651	3,671	4,222	4,259
	(1,680)	(1,688)	(1,282)	(1,267)	(1,645)	(1,646)	(1,962)	(2,003)
Tenure	3.73	3.80	3.30	3.37	3.72	3.83	4.39	4.39
	(2.96)	(2.97)	(2.28)	(2.32)	(3.03)	(3.08)	(3.54)	(3.48)
Firm								
Firm age	12.69	12.66	11.08	11.05	12.93	12.90	14.67	14.64
	(7.04)	(7.04)	(5.01)	(5.06)	(7.07)	(7.08)	(8.79)	(8.73)
Employees	8.78	8.81	8.41	8.43	8.87	8.91	9.17	9.20
	(6.88)	(6.90)	(6.63)	(6.63)	(6.98)	(7.00)	(7.03)	(7.05)
New hires	0.88	0.85	0.86	0.82	0.93	0.90	0.83	0.81
	(1.58)	(1.51)	(1.59)	(1.47)	(1.58)	(1.55)	(1.55)	(1.51)
Average daily wage	41.92	41.54	37.52	37.13	43.22	42.78	46.12	45.82
	(18.90)	(18.80)	(15.37)	(15.17)	(18.76)	(18.74)	(22.29)	(22.15)
Wage Bill	38,309	38,020	32,408	31,908	39,795	39,526	44,383	44,374
	(43,367)	(43,168)	(36,633)	(35,555)	(44,356)	(44,412)	(49,314)	(49,573)
Employee female share	0.82	0.82	0.82	0.82	0.82	0.82	0.82	0.82
	(0.22)	(0.22)	(0.22)	(0.22)	(0.23)	(0.22)	(0.22)	(0.22)
Turnover	1.72	1.72	1.67	1.69	1.78	1.77	1.70	1.68
	(2.25)	(2.27)	(2.19)	(2.33)	(2.29)	(2.27)	(2.26)	(2.19)
Trade & services sector	0.65	0.65	0.62	0.62	0.67	0.67	0.64	0.64
	(0.48)	(0.48)	(0.48)	(0.48)	(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
	(0.42)	(0.42)	(0.42)	(0.42)	(0.43)	(0.43)	(0.43)	(0.43)
Observations: events	60,114	33,618	21,853	11,603	24,980	14,223	13,281	7,792
Observations: unique individuals	46,738	33,618	18,263	11,603	20,822	14,223	11,607	7,792
Observations: unique firms	32,971	23,837	15,119	9,754	16,793	11,715	10,219	6,921

Notes: The table reports summary statistics for women and firms in our treatment and control samples in the baseline quarter (1 year prior to the birth event), both for the pooled sample, as well as for the samples of different regimes. Standard deviations are reported in parentheses. For the control group sample, the observations are weighted inversely by the number of control cells for each treatment cell. Tenure measures the years of the woman's employment at the firm, wages and wage bill are real annual earnings in euros (2000 CPI). Turnover is hirings + separations in the past year. We exclude closure from the table, since it is, by definition, zero at baseline.

Table A.2

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

	Days Leave	Hires	Employment	Daily wage	Wage bill	Closure
	(1)	(2)	(3)	(4)	(5)	(6)
Anticipation eff.	8.771***	0.221***	0.171***	0.0550	676.293***	-0.003
	(0.580)	(0.021)	(0.026)	(0.0736)	(117.942)	(0.002)
Anticipation eff. x 1.5y	0.299	-0.007	0.010	-0.325*	-67.935	0.000
	(0.909)	(0.044)	(0.033)	(0.143)	(249.576)	(0.003)
Anticipation eff. x 2y	2.013***	-0.022	-0.017	0.0901	-82.783	-0.000
	(0.776)	(0.036)	(0.028)	(0.106)	(171.611)	(0.002)
Short-run eff.	45.164***	0.097***	0.074	-0.549***	34.654	-0.001
	(0.874)	(0.019)	(0.047)	(0.157)	(231.868)	(0.003)
Short-run eff. x 1.5y	16.011***	-0.026	0.011	–0.430	-125.675	-0.001
	(1.502)	(0.030)	(0.082)	(0.303)	(488.624)	(0.005)
Short-run eff. x 2y	34.521***	-0.021	-0.025	0.0342	-199.898	-0.000
	(1.310)	(0.026)	(0.066)	(0.228)	(349.691)	(0.004)
Medium-run eff.	4.647***	0.061***	0.138*	-0.495*	478.426	0.002
	(1.108)	(0.021)	(0.075)	(0.244)	(393.099)	(0.005)
Medium-run eff. x 1.5y	-3.532*	-0.023	-0.090	-0.541	-533.137	0.002
	(1.857)	(0.033)	(0.129)	(0.460)	(770.056)	(0.007)
Medium-run eff. x 2y	-1.616	-0.015	0.020	-0.0180	-174.584	-0.001
	(1.599)	(0.028)	(0.103)	(0.348)	(568.454)	(0.006)
p-values						
Anticipation eff. (1.5y reg - 2y reg)	0.05	0.41	0.74	0.00	0.95	0.83
Short-term eff. (1.5y reg - 2y reg)	0.00	0.86	0.66	0.13	0.88	0.87
Long-term eff. (1.5y reg - 2y reg)	0.30	0.79	0.38	0.26	0.65	0.66
Observations						
Events	93,732	93,732	93,732	93,732	93,732	53,599
Individuals	75,639	75,639	75,639	75,639	75,639	45,639
Firms	44,390	44,390	44,390	44,390	44,390	24,462

Notes: Columns 1-6 report the OLS estimates from equation (3) for different outcome variables at quarterly-level with additional interaction terms for the 1.5 and 2 years regimes. Anticipation effect: average effect for $-3 \leq k \leq -1$; Short-run effect: average effect for $0 \leq k \leq 8$; Medium-run effect: average effect for $9 \leq k \leq 20$. Column 1: Days of parental leave (including the maternity protection period) taken by the mother; Column 2: New hires, i.e. number of workers who joined the firm this quarter. Column 3: Number of employees at the firm (excluding the women on leave) weighted by the number of days they work in that quarter. Column 4: Average daily wage in euros. Column 5: Total wage bill in euros. Column 6: Closure is a dummy variable equalling 1 if a firm closed. By definition cannot be one before baseline. p-values are reported for test of equality of 1.5y vs 2y interaction coefficients. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3

Reform Effects of Extended and Reduced Parental Leave on Firm Outcomes

Panel A: Reform 1990 (Extension from 1 to 2 years)

	Days Leave	Hires	Employees	Daily Wage	Wage bill	Closure
	(1)	(2)	(3)	(4)	(5)	(6)
Anticipation eff.	-6.607	0.079	0.097	0.771	599.981	0.015
	(4.226)	(0.106)	(0.156)	(0.542)	(727.551)	(0.019)
Common leave per.	-5.915	0.176	0.267	0.732	1173.050	0.007
	(7.275)	(0.125)	(0.240)	(0.869)	(1164.246)	(0.023)
Extended leave per.	48.922***	0.189	0.327	1.317	1423.300	0.004
	(6.681)	(0.117)	(0.294)	(1.065)	(1508.131)	(0.026)
Medium-run eff.	3.332	0.139	0.451	0.288	1559.105	0.016
	(6.853)	(0.114)	(0.349)	(1.225)	(1861.397)	(0.028)
Observations						
Events	5070	5070	5070	5070	5070	3080
Individuals	5070	5070	5070	5070	5070	3080
Firms	4658	4658	4658	4658	4658	2779

Panel B: Reform 1996 (Contraction from 2 to 1.5 years)

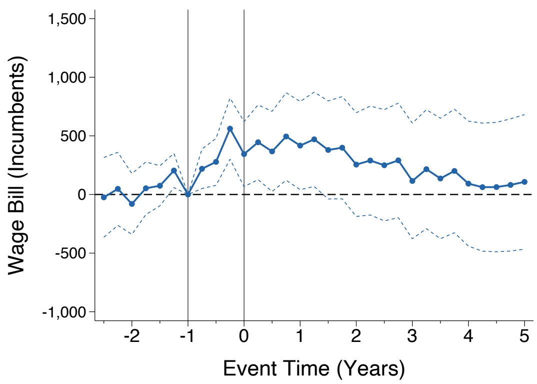
	Days Leave	Hires	Employees	Daily Wage	Wage bill	Closure
	(1)	(2)	(3)	(4)	(5)	(6)
Anticipation eff.	-4.278	0.075	-0.010	-0.333	-293.597	-0.024
	(4.204)	(0.126)	(0.175)	(0.654)	(968.722)	(0.016)
Common leave per.	-2.189	0.137	-0.043	-0.528	-169.308	-0.036*
	(7.695)	(0.128)	(0.283)	(1.131)	(1680.936)	(0.021)
Shortened leave per.	-37.628***	0.087	0.220	1.277	539.268	-0.039
	(8.776)	(0.129)	(0.354)	(1.460)	(2133.417)	(0.025)
Medium-run eff.	6.234	0.106	-0.046	0.593	1050.332	-0.030
	(7.269)	(0.119)	(0.417)	(1.558)	(2853.370)	(0.026)
Observations						
Events	4326	4326	4326	4326	4326	2713
Individuals	4326	4326	4326	4326	4326	2713
Firms	4036	4036	4036	4036	4036	2485

Notes: Columns 1-5 report the OLS estimates from equation (3) for different outcome variables at the quarterly-level. Anticipation effect: average effect for $-3 \leq k \leq -1$; Common leave period: average effect for $0 \leq k \leq 3$ (for reform 1y extension) and for $0 \leq k \leq 5$ (for reform 0.5y reduction); Extended/shortened leave period: average effect for $4 \leq k \leq 8$ (for 1y extension) and $6 \leq k \leq 8$ (for 0.5y reduction); Medium-run: average effect for $9 \leq k \leq 20$. Column 1: Days of parental leave (including the maternity protection period); Column 2: New hires; Column 3: Number of employees (excluding the women on leave), weighted by the number of days they work in that quarter; Column 4: Average daily wage; Column 5: Total wage bill; Column 6: Closure is a dummy variable equaling 1 if a firm closed (by definition cannot be one before baseline). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

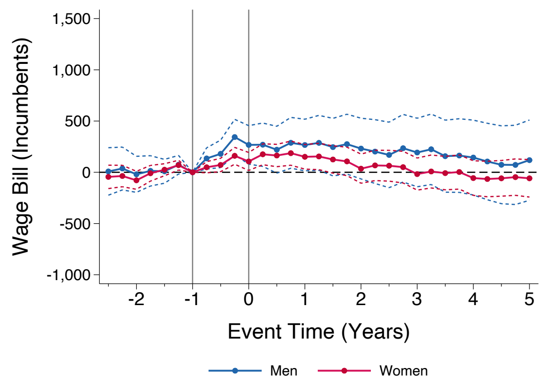
B Appendix Figures

Figure B.1
Effects of an Additional Birth and Leave-Taking on Incumbent Workers

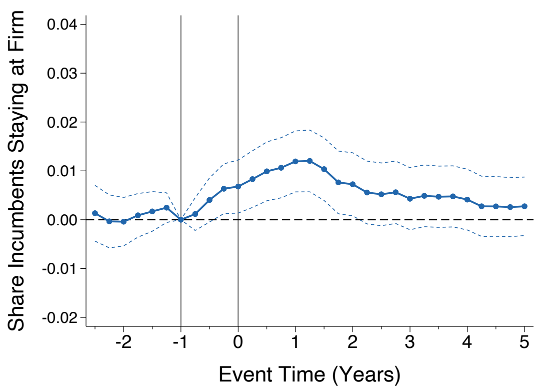
(a) Wage Bill



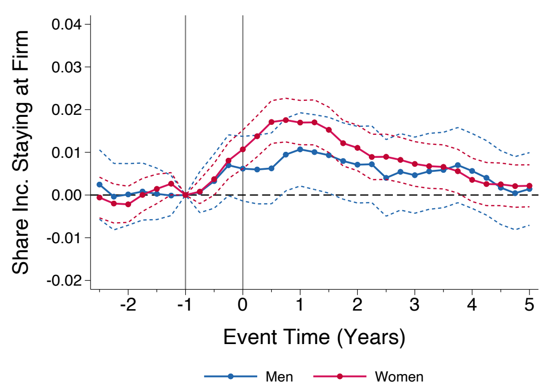
(b) Wage Bill by Gender



(c) Retention



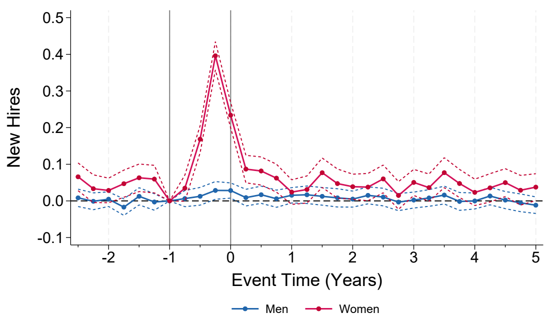
(d) Retention by Gender



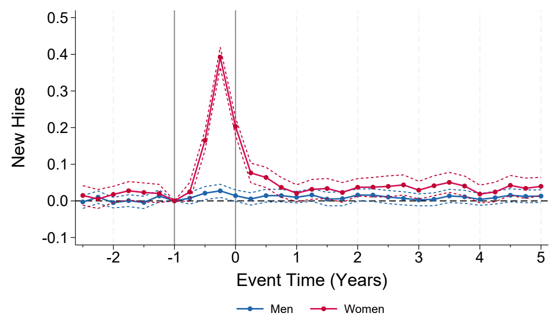
Notes: The figure plots regression coefficients from the dynamic differences-in-differences equation (1) which captures the differential changes in the treatment group compared to the control group for incumbent workers (i.e. workers who have been at the firm at the baseline quarter $b-4$). Panel a): Wage bill to incumbent workers. Panel b): Wage bill split by gender of incumbent workers. Panel c): Retention of incumbent workers. Panel d): Retention of incumbent workers by gender. Dashed lines indicate 95% CIs.

Figure B.2
Sample Splits: Effects of an Additional Birth and Leave-Taking on New Hires by Gender

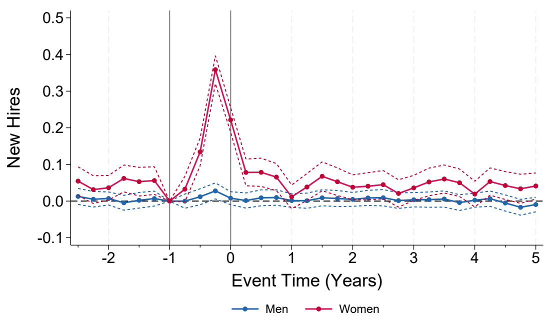
(a) Below Median Tenure



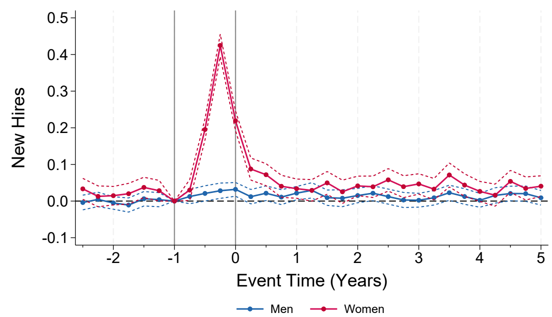
(b) Above Median Tenure



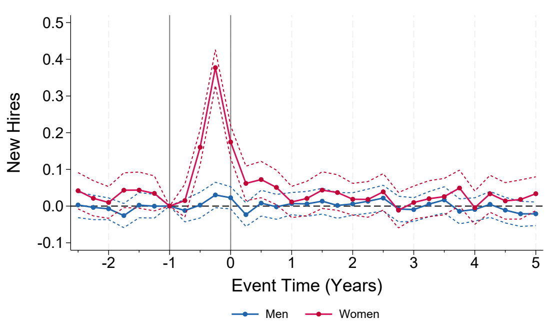
(c) Below Median Earnings



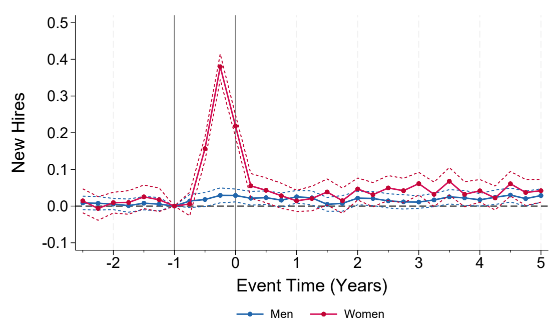
(d) Above Median Earnings



(e) Bottom Quintiles Firm Rank



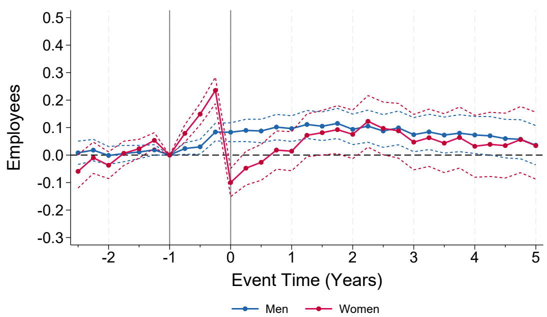
(f) Top Quintiles Firm Rank



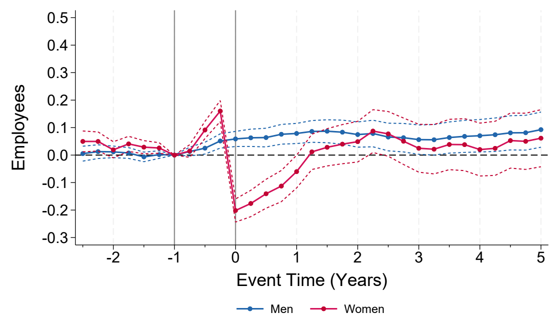
Notes: The figure displays regression coefficients from the dynamic differences-in-differences equation (1), estimated separately for men and women across different subsample splits. Panels (a) and (b) show results split by overall sample tenure quintiles, panels (c) and (d) by overall earnings quintiles, and panels (e) and (f) by within firm earnings quintiles. In each case, "below median" refers to quintiles 1 and 2, while "above median" refers to quintiles 4 and 5. Dashed lines indicate 95% CIs.

Figure B.3
Sample Splits: Effects of an Additional Birth and Leave-Taking on Number of Employees by Gender

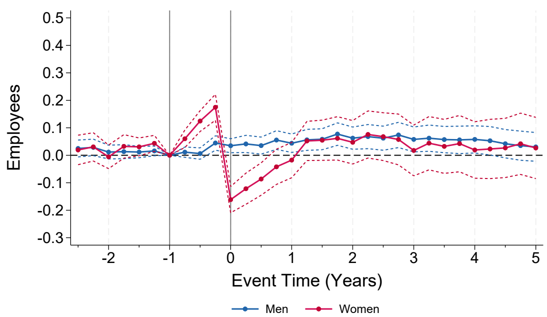
(a) Below Median Tenure



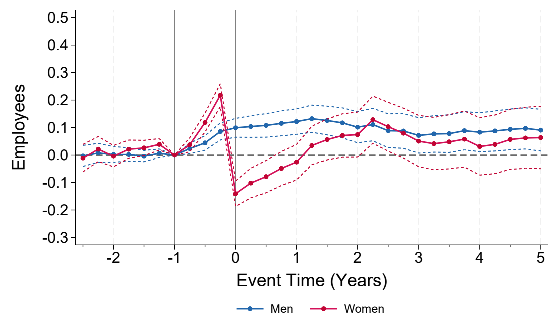
(b) Above Median Tenure



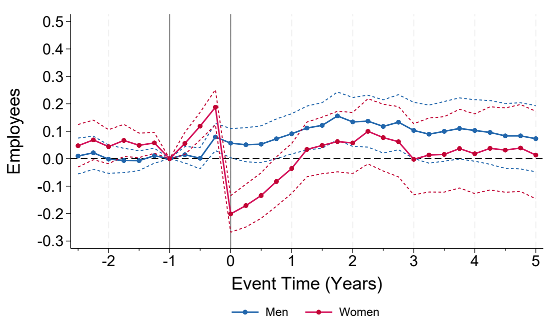
(c) Below Median Earnings



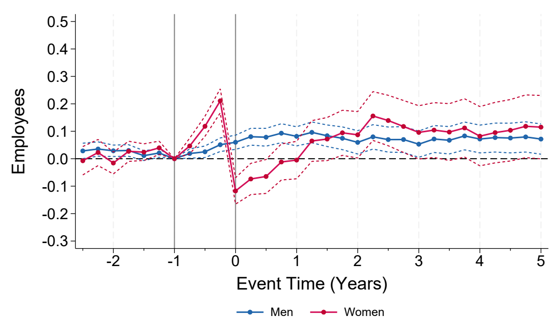
(d) Above Median Earnings



(e) Bottom Quintiles Firm Rank



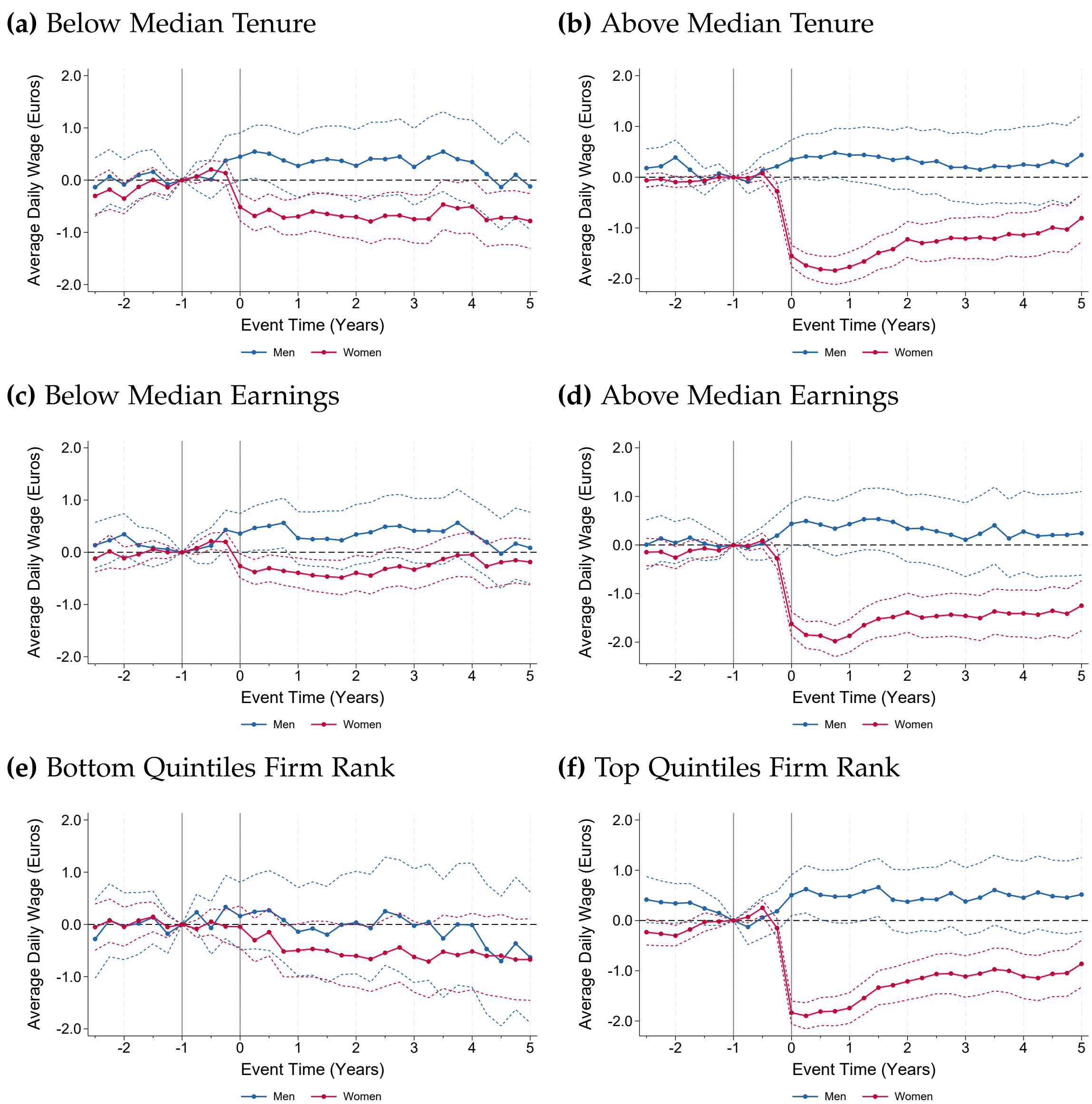
(f) Top Quintiles Firm Rank



Notes: The figure displays regression coefficients from the dynamic differences-in-differences equation (1), estimated separately for men and women across different subsample splits. Panels (a) and (b) show results split by overall sample 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" refers to quintiles 1 and 2, while "above median" refers to quintiles 4 and 5. Dashed lines indicate 95% CIs.

Figure B.4

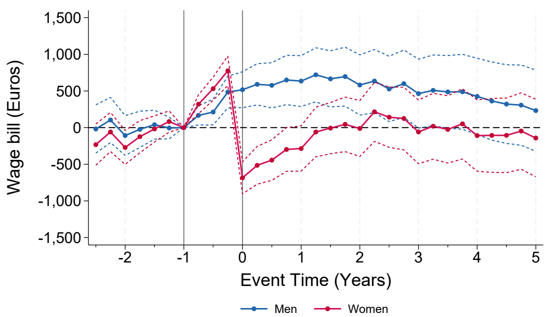
Sample Splits: Effects of an Additional Birth and Leave-Taking on Average Daily Wage by Gender



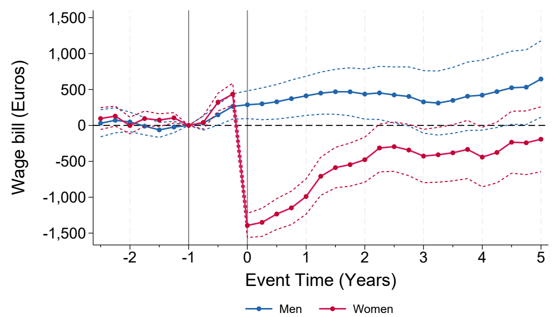
Notes: The figure displays regression coefficients from the dynamic differences-in-differences equation (1), estimated separately for men and women across different subsample splits. Panels (a) and (b) show results split by overall tenure quintiles, panels (c) and (d) by overall earnings quintiles, and panels (e) and (f) by within firm earnings quintiles. In each case, "below median" refers to quintiles 1 and 2, while "above median" refers to quintiles 4 and 5. Dashed lines indicate 95% CIs.

Figure B.5
Sample Splits: Effects of an Additional Birth and Leave-Taking on Wage Bill by Gender

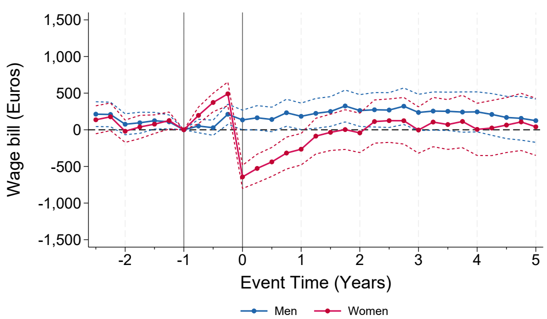
(a) Below Median Tenure



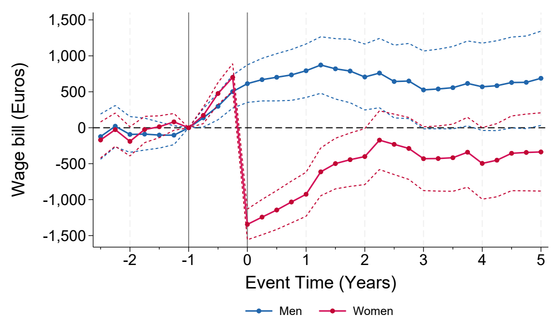
(b) Above Median Tenure



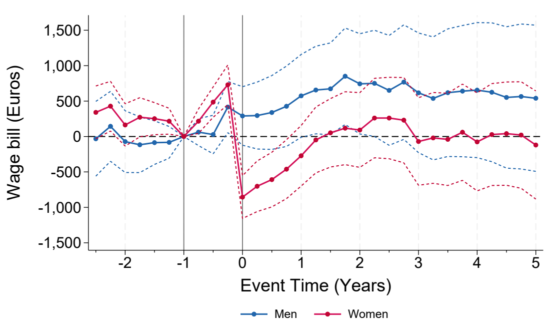
(c) Below Median Earnings



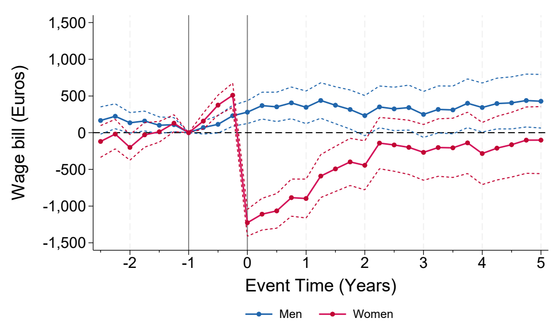
(d) Above Median Earnings



(e) Bottom Quintiles Firm Rank

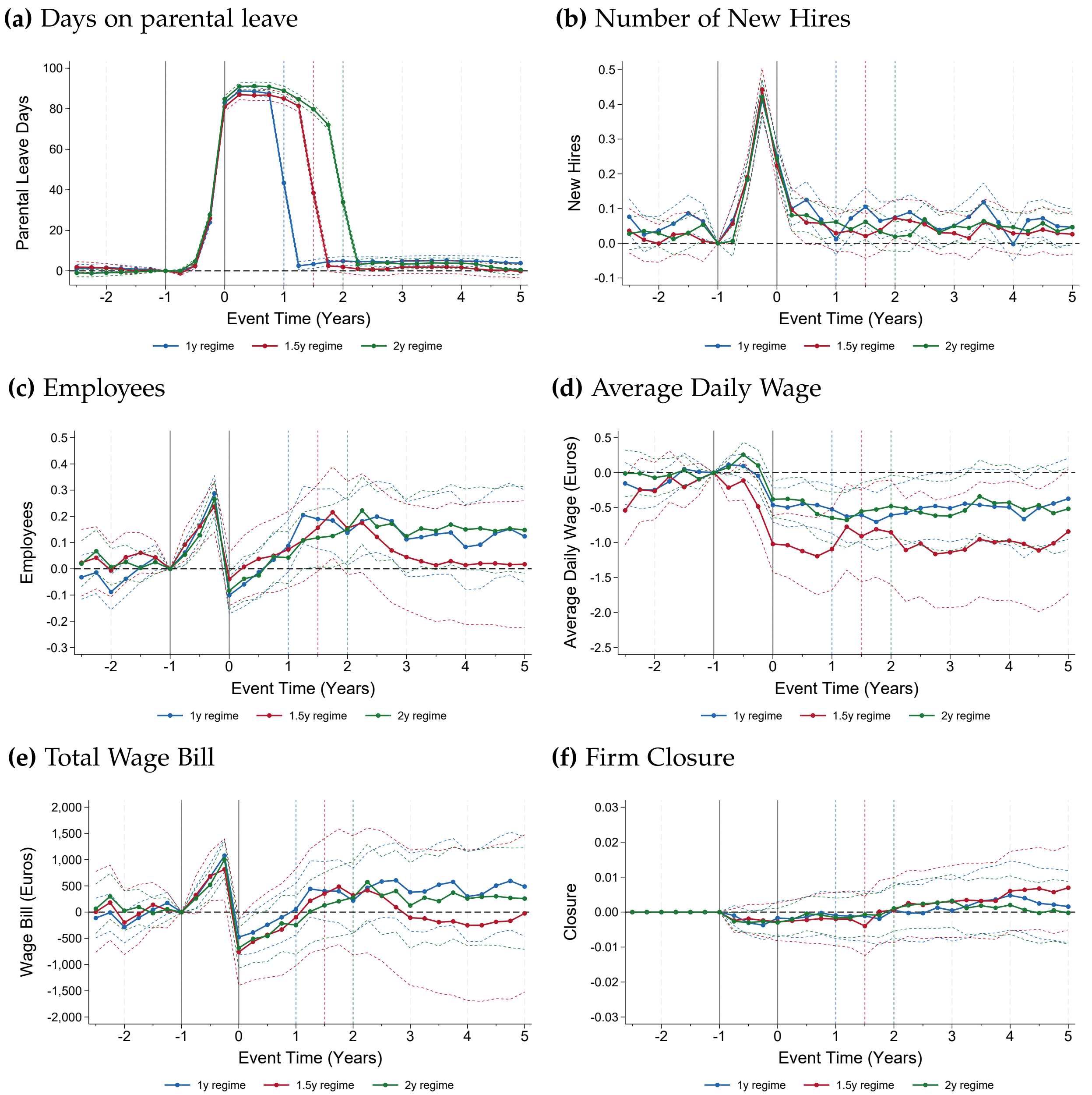


(f) Top Quintiles Firm Rank



Notes: The figure displays regression coefficients from the dynamic differences-in-differences equation (1), estimated separately for men and women across different subsample splits. Panels (a) and (b) show results split by overall sample 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" refers to quintiles 1 and 2, while "above median" refers to quintiles 4 and 5. Dashed lines indicate 95% CIs.

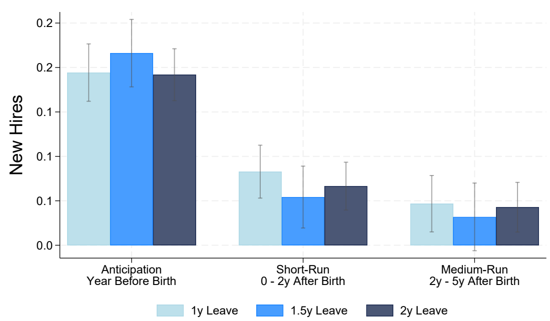
Figure B.6
Robustness Check: Graphical Evidence on the Dynamic Effects of an Additional Birth and Leave-Taking on Firm Outcomes by Regime



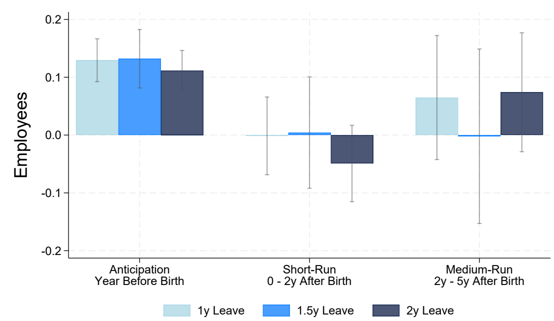
Notes: Each graph plots the β-regression coefficients from equation (1) run separately for each regime, capturing the differential changes in treated firms' outcomes compared to control firms. The outcomes of the graphs are described in Section 2. Dashed lines indicate 95% CIs.

Figure B.7
Effects of an Additional Birth and Leave-Taking on Firm Outcomes for Women by Regime

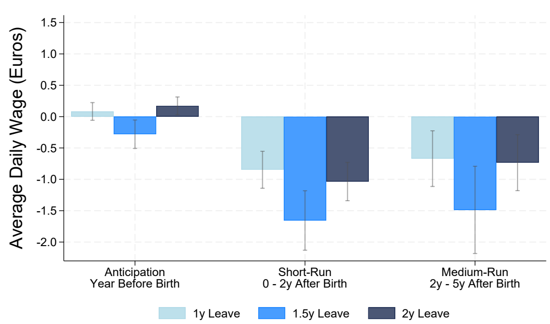
(a) Female New Hires



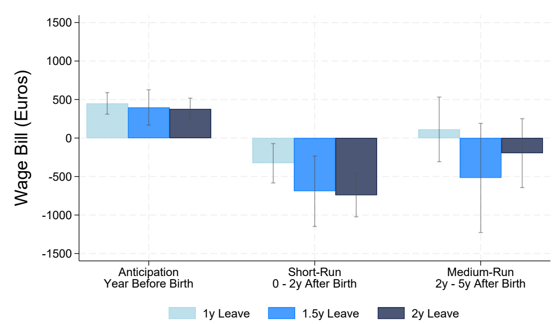
(b) Female Employees



(c) Female Average Daily Wage



(d) Female Wage Bill

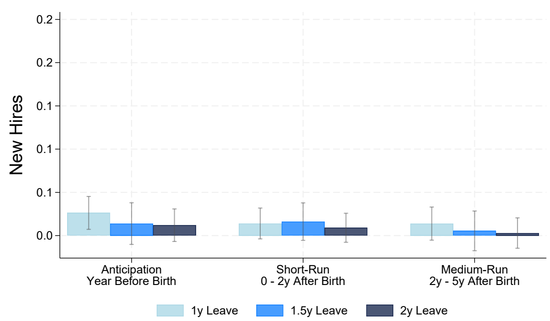


Notes: Each bar displays β^r from equation (2) for a specific regime r and period relative to the birth event. Each bar therefore captures the effects on treated firms' respective average outcomes for women relative to control firms in the anticipation, short-run and medium-run period.

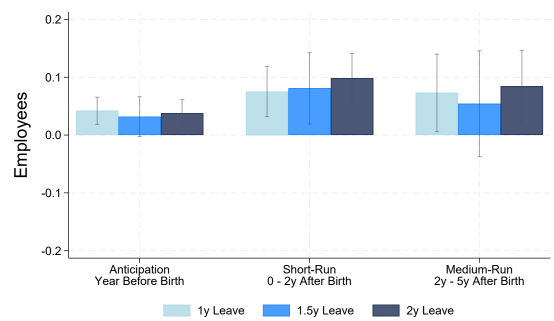
Figure B.8

Effects of an Additional Birth and Leave-Taking on Firm Outcomes for Men by Regime

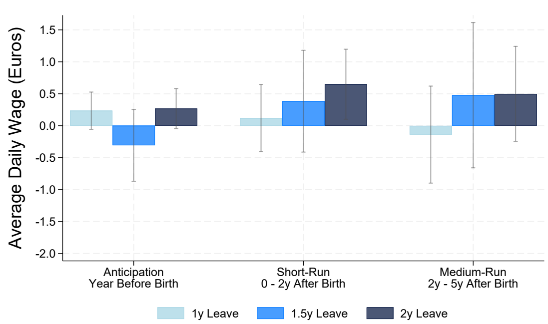
(a) Male New Hires



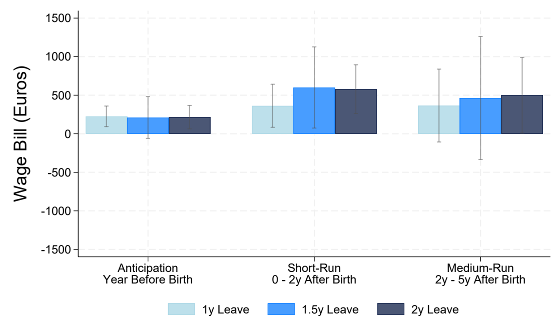
(b) Male Employees



(c) Male Average Daily Wage



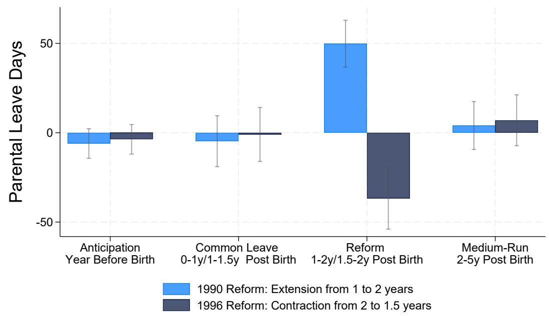
(d) Male Wage Bill



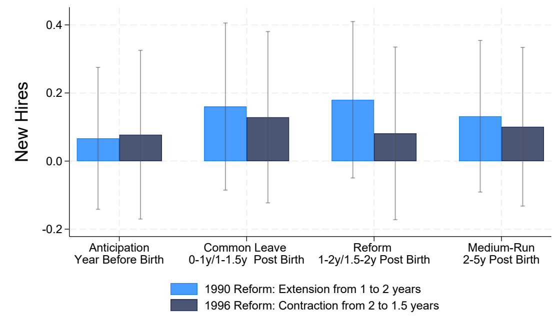
Notes: Each bar displays β^r from equation (2) for a specific regime r and period relative to the birth event. Each bar therefore captures the effects on treated firms' respective average outcomes for men relative to control firms in the anticipation, short-run and medium-run period.

Figure B.9
Reform Effects of Extended and Reduced Parental Leave on Firm Outcomes:
Robustness Check – Excluding Births Within Two Weeks of the Cutoff Date

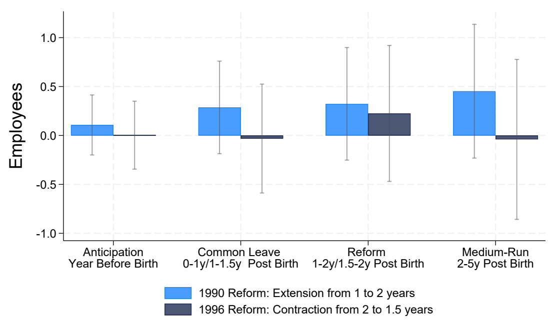
(a) Parental Leave Days



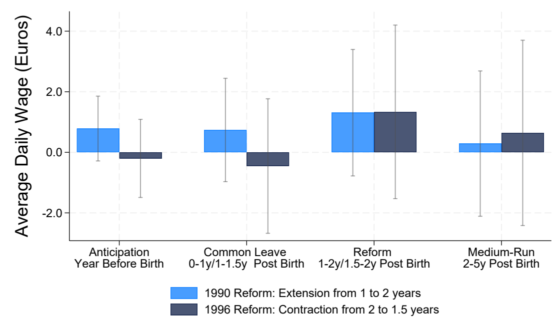
(b) New Hires



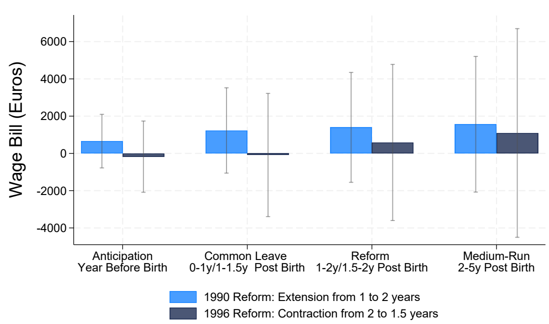
(c) Number of Employees



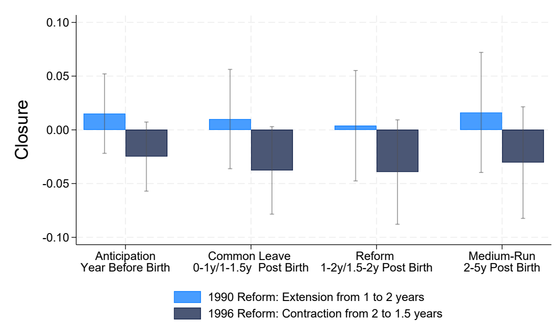
(d) Average Daily Wage



(e) Wage Bill



(f) Closure



Notes: Each bar displays βs from equation (3). For both reforms, the graph plots the average β_ks across the periods in question. Each bar therefore captures the effect on treated firms' respective outcomes of a 1-year increase in maximum parental leave duration and a 0.5-year decrease.

C 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 Austria). The ASSD only record mini-job employment from 1990 onward, which means we can only study this outcome 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 C.10 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 the 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 C.10 shows the β-coefficients from this specification. We see 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, 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:

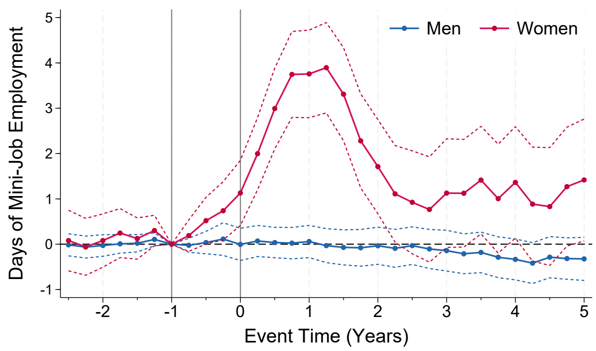
$$P^{g}_{t} = \frac{\hat{\alpha}^{g}_{t}}{E\left[\tilde{Y}_{ist}\,|t\right]}$$

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

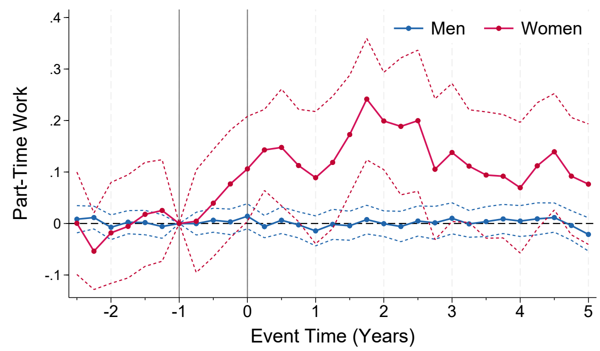
For this sample, we only keep employed women and men to not confound the analysis of hours with the analysis of extensive margin employment (for child penalties in earnings and extensive margin employment among Austrian women, see Kleven et al. (2024)). Panel (c) of Figure C.10 plots the child penalties 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.

Figure C.10
Part-Time Employment

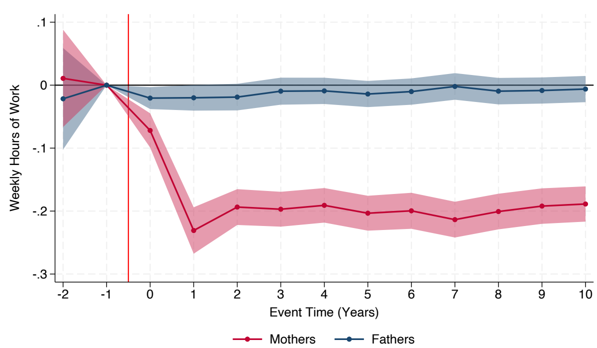
(a) Days of Mini-Job Employment



(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% CI.

D Data Appendix

D.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.

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.

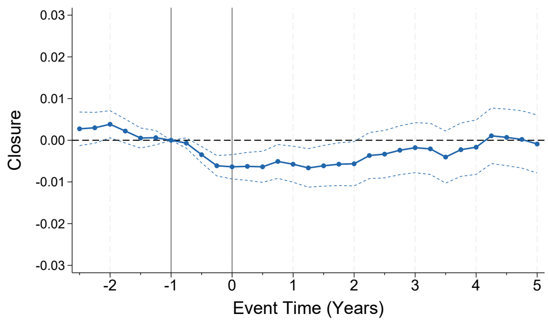
Closure is a dummy variable that takes the value of one if a firm has shut down, using the permanent closure measure from Fink et al. (2010). Fink et al. (2010) use a worker-flow approach to identify "real" bankruptcies as opposed to mergers and spin-offs. One limitation of their measure, however, is 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 two alternative measures of firm closure— a) an indicator variable that equals 1 if the firm has 0 employees, and b) an indicator variable that equals 1 if the firm has 0 employees in every period $k > x$. Results are shown in Figure D.11.

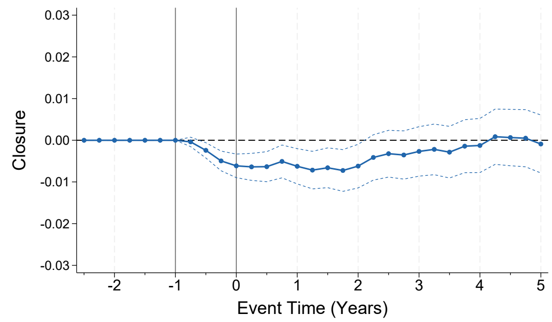
D.2 Detailed Matching Procedure and Sample Restrictions

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 characteris-

Figure D.11
Alternative Measures of Firm Closure

(a) Closure (zero employment) **(b)** Closure (zero empl. in each $k > x$)





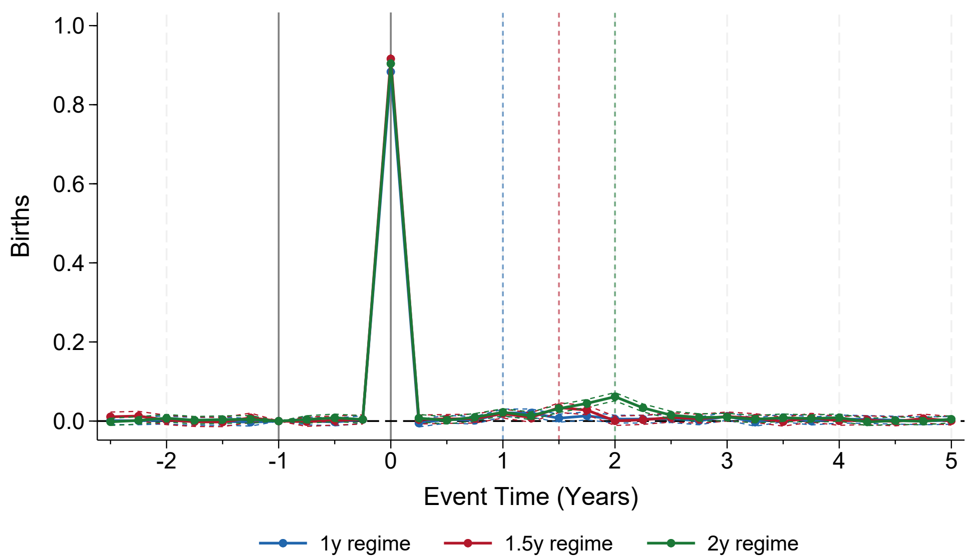
Notes: Each graph plots the β-regression coefficients from equation (1), capturing the differential changes in treated firms' outcomes compared to control firms. Dashed lines indicate 95% CI.

tics 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 D.12 (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).

Figure D.12
Effect of a Pregnancy Shock on number of Births in Firm



Notes: The figure plots regression coefficients from the dynamic differences-in-differences equation (1) of the treatment status on births within the firm by regime. Dashed lines indicate 95% CI.