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Abstract

We estimate the effect of a female employee giving birth and taking parental leave on small firms and coworkers in Denmark using a dynamic difference-in-differences design. We find little evidence that parental leave take-up has negative effects on firms and coworkers overall. This is because most firms are very effective in compensating for the worker on leave by hiring temporary workers and by increasing other employees' hours. In contrast, we do find evidence that parental leave has negative effects on a small subsample of firms which are less able to use their existing employees to compensate for absent workers.

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1 Introduction

The past few decades have been marked by a dramatic rise in female labor force participation and a narrowing of the gender gap in education, hours of work, and earnings (Goldin, 2014). Nonetheless, women still experience substantial earnings penalties due to motherhood (Bertrand *et al.*, 2010; Angelov *et al.*, 2016; Lundborg *et al.*, 2017; Kleven *et al.*, 2019). In light of these facts, policy discussions surrounding parental leave have become more prominent.[1] Nearly all high-income countries currently have generous leave entitlements with the goals of decreasing gender inequality and improving child development (Olivetti & Petrongolo, 2017). While many of these programs benefit mothers and their children (Rossin-Slater, 2018), critics argue that leave take-up could impose substantial costs on employers. These costs include both wage replacement benefits during parental leave as well as indirect expenses, such as the cost of training and recruiting replacement labor. Although one of the goals of parental leave policies is to improve mothers' well-being, these incurred costs could harm women by making employers more likely to discriminate against them in hiring and promotion decisions.

To fully understand the benefits and costs of parental leaves, it is not only essential to examine how parental leaves affect households but also how they affect firms and workplaces. Doing so is especially important for countries that are considering introducing or extending leave benefits. For example,

[1] Throughout the paper, we use the term "parental leave" to cover any period of leave that is taken in conjunction with a child's birth or in the years following. The term thus includes both periods of "pregnancy leave" taken toward the very end of a pregnancy and periods of "maternity leave" that mothers take immediately following a birth.

in the United States—the only high-income country with no national paid leave—this question is at the center of ongoing policy debates, as opponents contend that mandating parental leave would be too costly and too detrimental to businesses. Former California governor Jerry Brown signed a bill into law in 2017 that required small and medium-sized businesses to provide new parents with 12 weeks of leave. However, he rejected a similar bill just one year earlier citing concerns "about the impact of this leave particularly on small businesses and the potential liability that could result" (Barrera, 2017).

In this paper, we present some of the first evidence on how firms and coworkers are affected when female employees give birth and go on leave. Despite considerable policy relevance, direct estimates of the effects of parental leave on employers and coworkers are scarce. In contrast to the rich evidence on the effects of parental leave on women and children (Olivetti & Petrongolo, 2017; Rossin-Slater, 2018), a recent review of the literature on leave programs concludes that "we know very little about how maternity and family leave policies may impact businesses, who often worry about being burdened with extra costs resulting from dealing with employee leave-taking" (Rossin-Slater, 2018, p.337). This is largely because answering this question requires comprehensive data linking firm and worker outcomes to information on fertility and leave-taking, which is a challenging undertaking. Identifying causal effects poses an additional challenge, as leave-taking is likely correlated with unobservable factors, such as worker productivity, that may simultaneously affect firm outcomes.

We study the effects of a woman giving birth and taking leave on firms' labor demand, costs, overall performance, and coworkers' labor outcomes (hours, retention, and earnings) in a setting where firms are reimbursed for the costs associated with wage replacement benefits during parental leave. To do this, we exploit rich administrative data on the universe of firms and workers in Denmark from 2001 to 2013. We link data on individual worker fertility and leave-taking with full administrative data on their employing firm and their coworkers. We focus on small firms (those with less than 30 employees), which, due to their size, may bear the largest costs of parental leave policies. To identify the causal effects of leave-taking on firms and coworkers, we first select a sample of *treatment* women who are one year away from becoming pregnant and a sample of comparable *control* women who do not become pregnant over the next few years. We then use a dynamic difference-in-differences design to compare the outcomes of the firms and co-workers of these two groups of women.

Comparing firms based on the fertility timing of their female workers raises a few potential concerns. First, women who later give birth could choose to work at particularly family-friendly firms already before becoming pregnant, leading to differences between the groups of firms employing treatment and control women. Second, a woman could endogenously time her fertility, resulting in correlations of firm trends with the timing of her fertility. Two features of our design appease these worries. First, by conditioning on a rich set of individual and firm-level observables, we ensure that the treatment and control groups are observationally equivalent at baseline, two years before the treatment group experiences a parental leave. Second, we test for differential pre-trends across a wide range of outcomes (i.e., test for evidence of strategic timing of fertility with respect to the firm-level outcomes). Our design passes pre-trends tests.

Our empirical analysis yields several key findings. First, firms where a woman gives birth are exposed to an average of 282 extra parental leave days (about nine and a half months). In isolation, an employee going on leave thus implies a substantial loss of labor inputs for firms. We find, however, that firms are able to compensate for this lost labor supply by making adjustments both along the extensive and intensive margins. Compared to the control group, treated firms temporarily hire more workers when their employee gives birth and goes on leave. They also slightly raise the retention rates and work hours of existing employees, particularly those who are in the same occupation as the woman on leave. These adjustments appear very effective in compensating for the worker on leave. Based on an approximate measure of firm's total hours, we see no indications that firm's total labor inputs are affected by parental leave: the 95 percent confidence interval from our preferred specification excludes effect sizes such that having one percent of the workforce on leave reduces total hours by more than 0.19 percent.

Turning to the overall costs of leave, we find that Danish parental leave imposes minimal costs as best as we can measure. Consistent with the increase in work hours, we document marginal increases in existing employees' earnings, which are again driven by employees in the same occupation as the women on leave. Together with the temporary increase in hires and retention, these changes lead to an increase in the treatment firms' total wage bill. This total wage bill includes wages paid to workers on leave. However, similar to most other countries providing national paid leaves, Danish firms are compensated for the wages of employees on leave. When we exclude wages paid to workers on leave, we do not find any effect on the wage bill of having a female employee on leave. Furthermore, having an employee go on leave does not seem to affect overall firm performance. We do not find significant effects on output or on the likelihood of firm survival. The 95 percent confidence interval from our preferred specification excludes reductions in sales by more than 0.18 percent and in the likelihood of survival by more than 0.05 percentage points when one percent of the workforce goes on leave. Overall, our estimates suggest that the costs of parental leave for employers are small.

We also find no evidence of adverse impacts on coworkers overall. As noted, coworkers see increases in their hours, earnings, and likelihood of being employed and are thus compensated by their extra work effort when an employee goes on leave. Moreover, at least in terms of sick leave, workers do not seem to suffer from their coworker's absence.

While firms, on net, make labor adjustments without drastic consequences for their costs, profits and survival, some firms may face unusual adjustment costs. To delve into this further and understand the role of firm adjustments in our results, we explore heterogeneous effects across firms. First, we focus on the small subset of firms that have no other employees in the same occupation as the woman who goes on leave.[2] By construction, these firms face constraints on how they can adjust because they cannot rely on increases in hours among

[2]Firms that have no other employees in the same occupation as the woman who goes on leave make up 10 percent of our main sample.

same-occupation coworkers. Accordingly, for this subsample of firms, we do see indications of negative effects of parental leave.

Finally, we consider the potential role played by mothers' labor supply adjustments in our results. By construction, our empirical analysis identifies the joint effect of women going on parental leave and any other labor supply changes that occur around childbirth. Based on additional analyses, however, we conclude that our results most likely reflect the direct impact of parental leave absences. Over the time horizon we consider, the direct loss of labor from parental leave absences is an order of magnitude greater than any other labor supply adjustments we see following childbirth. Moreover, the timing of our estimated effects coincide sharply with the period where the woman is actually absent on leave.

The motivation for our study is to understand the ongoing costs imposed by parental leave policies. Accordingly, the aim of our analysis is to measure how employees going on parental leave affect employers and coworkers in the typical setting where employers can plan for the leave from the time the employee announces her pregnancy. This objective is distinct from other recent work on parental leave and firms. In previous work, Gallen (2019) studies a 2002 policy reform in Denmark which unexpectedly extended the length of leave from eight to ten months on average. Since the original circulation of our paper, Ginja *et al.* (2022) has added further evidence from a similar 1989 Swedish reform, which increased leave duration from 12 to 14 months on average, and Huebener *et al.* (2021) focus on a German extension of generous leave benefits from 3 to 12 months. They tend to find that extensions in the length of parental leave have negative effects on firms overall, although the exact nature of these effects vary.

These studies differ from our work in two key ways that preclude them from answering the specific research question we pose. First, they examine the intensive margin shock of experiencing longer leaves among employees who were already scheduled for a substantial leave period. Our work focuses instead on extensive margin shocks (having an employee on leave vs. not on leave). This distinction is important because extensions of long-duration parental leaves are known to have markedly different effects on women's labor market behavior, in particular on turnover (Rossin-Slater, 2018). Employee turnover, in itself, is known to have negative effects on firms (Bertheau *et al.*, 2022). Second, the extensions studied in Gallen (2019) and Ginja *et al.* (2022) were implemented while women were already on leave and thus precluded firms from planning in advance for the leave. In normal times, however, absences due to parental leave differ from most other employee absences exactly in that they are highly anticipated, giving firms more scope for planning. We expand our discussion of these differences in Section 6.10.

By focusing on the effects of worker absence due to family leave, our paper is also related to the case study of the public health care sector by Friedrich & Hackmann (2021). The authors leverage a 1994 Danish policy which made generous family leave available to all parents with children up to the age of eight and subsequently created a temporary nurse shortage, to provide causal estimates on the health effects of nurse care in hospitals and nursing homes.

Our study can also be seen as enhancing our broad understanding of labor demand as a part of firms' production process and the possible presence of labor market frictions, expanding on much theoretical work (Stole & Zwiebel, 1996a,b; Cahuc *et al.*, 2008; Acemoglu & Hawkins, 2014; Kaas & Kircher, 2015). In this vein, our paper contributes to the empirical literature on worker absences derived from sources other than parental leave including unexpected worker death or illness, labor disputes and military reserve call-ups.[3] One distinct departure from much of this literature is that absences due to parental leave are temporary with a known end date and are highly anticipated. For example, while our dynamic difference-in-differences approach resembles that of Jäger & Heining (2022) in their study of the effects of a coworker's death, the authors focus on unanticipated events in an effort to inform theory about the substitutability of different types of workers in production. In contrast, the typical effect of a worker giving birth and going on leave will involve the employer learning about the upcoming leave some months in advance. This advance notice may be one reason that parental leave absences are less costly than other absences.

2 Understanding Worker Absences Impacts

This section provides a framework to understand the impact of a worker taking parental leave on firm and coworker outcomes based on existing theory. Starting from a benchmark model with no frictions and a competitive labor

[3]References include Azoulay *et al.* (2010); Bartel *et al.* (2014); Bennedsen *et al.* (2020); Drexler & Schoar (2014); Golding *et al.* (2005); Gruber & Kleiner (2012); Herrmann & Rockoff (2012); Isen (2013); Jaravel *et al.* (2018); Jäger & Heining (2022); Bertheau *et al.* (2022); Krueger & Mas (2004); Mas (2008).

market, the temporary absence of a worker on leave has no negative effects on the employing firm and coworkers. When a worker goes on leave, the firm simply temporarily replaces them with another worker hired at the same market wage, leaving both production and profits unchanged.

If we acknowledge the possibility of costly search, training costs, or other frictions, however, the predicted effects of an absent worker are more negative (see, for example, Jäger & Heining, 2022; Oi, 1962). Under such rigidities, the firm may not be able to replace the worker perfectly by relying on external hiring or may only be able to do so with a delay or after incurring additional costs. For example, firms' production may require specific human capital, which could result in the replacement worker not being a perfect substitute for the leave-taking employee. If the firm fails to replace the lost worker immediately, coworkers' productivity at the firm will change depending on whether they are complements or substitutes in production relative to the lost worker. For coworkers who are substitutes, productivity may increase, while the opposite holds for coworkers who are complements. Depending on how wages and employment are determined, these changes in productivity would imply changes in coworkers' wages, hours, and/or unemployment risk. For the firm, output would also decrease if the lost worker is not immediately and fully replaced, while profits will tend to decrease in any case, due to either lower output or higher costs. If firms also have the option of exiting the market in response to lower profits, the absence of a worker may cause some firms to lay off all coworkers and shut down entirely.

The mechanisms at play may differ by the type of leave. In particular, parental leave-taking differs from other types of costly worker absences studied in the literature —such as unanticipated worker death or illness (Jäger & Heining, 2022; Drexler & Schoar, 2014), and policy-induced extensions in the duration of parental leave (Gallen, 2019; Ginja *et al.*, 2022). In particular, parental leave-taking is highly anticipated and temporary with a likely known end date. Relative to other types of leave, it is also a relatively more common occurrence, implying that firms may already be familiar with the details of the parental leave system and be accustomed to having employees take leave. Taken together, these particular features suggest that parental leave might impose smaller costs on employers and coworkers than other types of absences.

3 Parental Leave Policies

As with most leave policies, Danish parental leave consists of two key parts: i) wage replacement for a specified number of weeks at a specified rate, which we discuss below, and ii) job protection while on leave. Appendix B provides further details about the specific Danish parental leave policies. Eligibility is conditional on the number of work hours over the months leading up to childbirth, but requirements are low enough that virtually all employees qualify.

During our sample period, mothers are eligible for job-protected leave with wage replacement for 4 weeks before birth, 14 weeks immediately after birth, and then have 32 weeks that the parents can share. In practice, mothers take the majority of these 32 weeks, implying that a typical new mother takes close to 50 weeks of job-protected leave with wage replacement.

The employment protection offered by the leave policy means that workers who go on leave are guaranteed to be able to return to their job at the end of the parental leave, although there are certain exceptions. Employers are not allowed to terminate the employee because of the leave but can terminate her for other reasons, such as downsizing or plant closing.

The wage replacement offered during the leave depends on the details of the worker's employment contract. At a minimum, all women are eligible to receive government-provided wage replacement equal to the maximum level of Danish unemployment insurance (UI) benefits during the entire 50 weeks of leave. We refer to this as *unpaid leave*, that is, the worker receives a direct wage replacement from the government instead of from her firm. However, most employment contracts in Denmark offer some period of fully paid leave during which the employer simply continues to pay the worker her wage. We refer to these periods of employer-paid leave as *paid leave*, that is, the worker continues to receive wage payments from the firm. Typically, paid leave is offered to women during all 4 weeks of prenatal leave, all 14 weeks immediately after birth, and for some subset of the 32 weeks after that. Importantly, workers lose their right to government-provided wage replacement during periods of paid leave. Contracts offering paid leave therefore do not affect the total time that women can be on leave but instead increase the effective wage replacement for parts of the leave period. Table 1 illustrates the parental leave system.

Employment contracts offering paid leave are encouraged under the Danish parental leave policy. This is done by directly reimbursing firms for wages paid to workers on leave in two ways. First, when an employee goes on paid leave, the employing firm receives the government-provided wage replacement that the worker would have been eligible for if not on paid leave. Second, firms paying wages to workers on leave are also eligible for reimbursement from one of several semi-private "parental leave funds" to which all employers contribute. Exact rules and reimbursement amounts differ depending on the specific fund and the terms of the woman's employment contract. However, firms recoup almost all the wages paid to workers on leave in the majority of cases. To account for this in our analysis, we use data on firms' wage bill both including and excluding wages paid to workers on leave.

Appendix Table A1 compares the Danish parental leave systems to schemes in other countries. Similar to Denmark, most European countries provide mothers with between 14 and 18 weeks of maternity leave with high earnings replacement (between 80 and 100 percent up to a ceiling). Denmark compares well to the average country with a total leave duration close to one year. In all countries, the social insurance system funds the leave programs.[4]

Finally, for thinking about the external validity of our results, it is worth noting that low levels of employment protection and high turnover and mobility are important features of the Danish labor market. Turnover and job mobility rates in Denmark are more similar to the US labor market than to other European labor markets (Andersen & Svarer, 2007). Danish employers thus have much leeway for firing other employees and/or temporarily increasing their workforce when an employee goes on leave compared to other European

[4]On top of the social insurance system, the Danish source of funding for parental leave is somewhat unusual, as employers are required to be member of one of the parental leave funds. However, these funding differences are unlikely to have first-order importance because Danish employers do not bear the direct wage replacement costs for leave.

countries. Firms also frequently hire temporary workers.

4 Data

Our administrative data cover the universe of Danish firms and workers from 2001 to 2013. These data include demographic worker data, matched employer-employee data, and firm data. We describe the data sources below and Appendix C provides further details.

4.1 Demographic Worker Data

We obtain basic demographic information, such as age and gender, from the Central Person Registry. Using parent-child linkages and information on birth dates, we construct fertility histories. We use data on the payout of parental leave benefits to individuals and the payout of leave reimbursements to firms to calculate the total number of days of paid and unpaid leave for each worker. We include in our parental leave measure instances when the leave period is extended because of pregnancy-related health issues. Using data from the central education register and the Integrated Database for Labor Research (IDA), we obtain measures of workers' education and their labor market experience.

4.2 Matched Employer-Employee Data

Information on employment relationships comes from yearly administrative data on wage payments from firms to workers (the CON and RAS databases) and the IDA. We use these data to construct measures of firm-level employment, wages, and an approximate measure of work hours.

To measure the stock of employees at a firm, we use the standard IDA definition of "main November employment relationship." Under this definition, a worker is considered to be employed at a firm in a given year if his/her main job was at that firm in the last week of November. We refer to the total number of such workers as the number of *employees* at the firm. Importantly, we note that this measure of employee stock includes workers on leave.

In addition to examining the stock of employees at a given time, we are also interested in examining turnover, new hires, and changes in hours worked. We construct measures of turnover and new hires in a straightforward way based on individual workers' year-to-year employment transitions in and out of each firm. To examine hours adjustments, we use an approximate measure of how many hours each worker supplied to a firm. This measure is based on data on mandatory pension contributions from firms (ATP). We scale contributions so that hours are measured in full-time equivalent (FTE) workers. Because contributions follow a stepwise function, the measure is only an approximation of yearly hours. In particular, the measure does not allow us to capture changes in overtime hours or smaller changes in the hours of existing employees. It does capture well the drop in hours when an employee goes on leave and works zero hours for part of the year, however.

Turning to wages, we start by computing total *earnings* for each worker in a given year as the sum of all (pre-tax) payments received from their main job. We then calculate the firm-level total wage bill as the sum of all payments to workers during the year. Unlike our FTE measure, the wage bill will reflect overtime work for full-time employees to the extent that overtime work is paid. This total *wage bill* will also include any payments made to workers on paid parental leave for which firms receive reimbursements. As an alternative measure, we construct the *wage bill ex. leave* where we remove payments made to workers on leave. By examining the effects of parental leave on both the total *wage bill* and the *wage bill ex. leave*, we can shed light on how firms are affected both before and after they receive reimbursements for paid leave.

4.3 Firm Data

Firm performance comes from value-added tax (VAT) data. All firms are required to report their total sales and purchases if the revenue exceeds a defined value. We use total *sales* as our measure of firm output and use firm *purchases* for an identification check. To examine the possibility that firms may in part compensate for workers on leave temporarily by buying more services from temp agencies or other firms, we also create an approximate measure of *total variable costs* by adding firms' total purchases to the *wage bill ex. leave* measure. For a measure of firm profitability, we create a proxy for *gross profits* by subtracting purchases and the total *wage bill ex. leave* from total sales. This proxy differs from the standard accounting definition because the VAT purchase data on purchases include capital equipment, which would not normally be included.

One important feature of most firm data is that many firms enter and exit the market each year. Because leave-taking might affect firm entry and survival, we do not remove firms that are inactive and/or shut down from our sample. Instead, we consider them as having zero employees, zero hours, and zero sales. Using positive sales as a proxy for firm activity, we also examine firm shutdown directly as an outcome.

5 Research Design

The goal of our study is to identify the causal effect on firms and coworkers when a female employee gives birth and subsequently goes on leave. We do this using a dynamic difference-in-differences design. In short, we select a group of treatment women who are about to become pregnant and later give birth as well as a group of control women who do not become pregnant over the next years. We then follow the evolution of the outcomes of the firms and coworkers of the treatment and control women.

5.1 Constructing the Treatment and Control Groups

We first define what we refer to as *potential events*. These are instances where a female employee could give birth and go on leave. A potential event is defined as a woman who had her main job at some firm in some year. Events are combinations of woman-firm-year. For definitional purposes, we call the year in this combination the *baseline year* and the year two years after the baseline year the *event year*. These events are determined at the individual and not the firm level, as an individual's own behaviors trigger parental leave.

Within our sample of potential events, we then select our set of *treatment* and *control* events as follows (see Figure 1 for a summary): We classify a *treatment* event as one in which the woman gives birth in the event year but does not give birth in the year before or after the event year. In parallel, a *control event* is an event in which a woman does not give birth in the event year, the year prior, or the year after.[5] For both sets of events, the associated firm is the firm where the woman is employed in the baseline year. The association of a firm to an event occurs in the baseline year rather than the event year to allow for the possibility that job mobility between the baseline and event years may be endogenous.

5.2 Sample Restrictions

We place several restrictions on this set of potential leave events—both for the woman and the firm:[6] First, the woman must be of prime childbearing age (i.e., between 19 and 33 years of age in the baseline year). Second, the woman must have strong attachment to their employer and to the labor market (i.e., tenure at her baseline firm for at least a year and not a student). Third, the firm must employ a stock of between 3 and 30 employees in the baseline year. The cutoff of 30 employees follows other work in this ltierature (see in particular Jäger & Heining (2022)). Fourth, the firm must be in the private sector. This is to ensure meaningful measures of firm performance. Fifth, the

[5]For women of prime childbearing age, fertility exhibits a very strong negative autocorrelation pattern across adjacent years. The requirement that female employees in the control group do not give birth over any of the next three years is therefore necessary because we want to look at potential longer-run effects of a female employee giving birth without our estimates being confounded by births occurring among control group members. If we only required the control group women not to give birth in the event year, we would have large spikes in fertility in the surrounding years for this group.

[6]See Data Appendix C in Subsection C.2 for more details.

firm must be active at baseline based on sales, hours, and the total wage bill. Sixth, the firm must not be an extreme outlier in terms of growth, sales levels, or wage bill. This ensures our results are not driven by a small number of outlier firms.

Of these conditions, the restriction to small firms is most noteworthy. We restrict attention to small firms for two reasons. First, the potential disproportionate impacts on small firms often crowd public discussions of parental leave. Second, treatment in our research design is based on having a *single* additional worker who gives birth and goes on leave. This variation is most meaningful at small firms.[7]

We make no restrictions on how many times a specific woman or firm can enter the sample as part of an event. A woman who satisfies the above sample restrictions in the years 2008 to 2010 and gives birth only in the year 2012 would thus contribute one treatment event with baseline year 2010 and two different control events with baseline years 2008 and 2009. Correspondingly, if the woman is employed by the same firm between 2008 and 2010, this firm also enters the sample three times as part of these three events. In practice, a majority of firms only enter our sample once (see Appendix E).

[7] Applied to aggregate outcomes at larger firms, our design will be statistically underpowered by construction: At larger firms, the consequences of a single worker going on leave will be swamped by random firm-level variation.

5.3 Examining Firm Outcomes

Our dynamic difference-in-differences specification for firm outcomes is:

$$
\begin{aligned}
Y_{eft} =& \gamma_e + \sum_{k\in\mathcal{T}} \alpha_k \mathbb{1}_{t=k} + \sum_{k\in\mathcal{T}} \beta_k \mathbb{1}_{t=k} \cdot Treatment_e + \varepsilon_{eft} \qquad (1) \\
\mathcal{T} =& \{-4, -3, -1, 0, 1, 2\}.
\end{aligned}
$$

Here e indexes events, f indexes firms, and t measures event time (i.e., $t = 0$ is the event year and $t = -2$ is the baseline year). Y_{eft} is one of our firm outcomes for firm f at event time t, $Treatment_e$ is an indicator for whether event e is a treatment event, and $\mathbb{1}_{t=k}$ denotes the (time) dummy for event time k. γ_e is an event (i.e., woman-firm-baseline year) fixed effect that absorbs level differences in the baseline year and ensures that identification is not coming from level differences across firms. The coefficients on the time dummies, α_{-4} through α_2, reflect how the mean of Y_{eft} in control firms compares in each event year relative to the baseline year ($t = -2$). When estimating equation (1), we only include events with data available for all seven years ($t = -4$ to $t = 2$) on the relevant variables. We cluster our standard errors at the firm level since parental leave is a firm-wide event (Abadie *et al.*, 2022).

The parameters of interest are the coefficients on the interactions between treatment status and event time: β_k. These are the difference-in-differences coefficients and show how changes over time at treatment firms differ from changes over time at control firms. Under a parallel trends assumption, these coefficients identify causal effects of a worker giving birth and going on parental leave: β_0 identifies the contemporaneous effect in the year of birth, while β_1

and β_2 demonstrate the later post birth dynamics. Because many women will reveal their pregnancy to their employer in the calendar year before giving birth, we view the year before the birth as a potentially treated year. Thus, β_{-1} identifies any anticipation effects of a birth that materialize in the year prior to the birth's occurrence.

A key assumption in this approach is that of parallel trends: *two years prior* to their eventual birth, our group of treatment women should not be sorted into firms that evolve along systematically different time trends than the firms at which our group of control women are working. To make this assumption more credible, we apply matching and reweighting discussed in Section 5.6. As we later show, we find that the estimates of β_{-4} and β_{-3} are not statistically signifcant from 0 as a test of parallel trends.

5.4 Coworker Analysis

To understand the effects of a worker giving birth and taking leave on coworkers, we adopt a parallel analysis to the firm difference-in-differences. For each woman associated with a treatment or control event, we select all her male and female coworkers in the baseline year: i) whose job at the baseline firm constitutes the main attachment to the labor market in the baseline year, and ii) who had hours of at least half of a full time employee and earnings exceeding 75,000 DKK (10,000 EUR or 11,000 USD) in the baseline year. For this sample, we estimate OLS specifications akin to equation (1), but where the outcome variable is a coworker outcome with coworker-year level observations (instead of firm-year). For these analyses, the units of the outcome are the individual coworkers. We continue to cluster standard errors at the firm level.[8]

5.5 Additional Specifications to Assess the Magnitude of the Effects

5.5.1 Turnover and Imperfect Compliance

In our construction of the treatment, there will be imperfect compliance. Women employed at a firm in the baseline year may leave that firm after the baseline year. As mobility may be endogenous, we do not require women to remain at their baseline firm beyond the baseline year. The possibility of this movement across firms means that treatment firms may not experience their worker's childbirth. As is common when dealing with treatment non-compliance, we interpret the OLS estimates from equation (1) as causal intention-to-treat (ITT) estimates of how a birth and subsequent leave influences firms. But we supplement our OLS results with standard 2SLS to derive a LATE estimate of the effect of an additional birth at the firm.

For our 2SLS estimate, we first consider a differenced version of equation (1) between the baseline period and another event year (denoted by Δ):

$$\Delta Y_{ef} = \alpha_0 + \beta_0 Treatment_e + \Delta\varepsilon_{ef} \tag{2}$$

We note that estimating β_0 using OLS in this specification will give a *numerically equivalent* ITT estimate to the difference-in-differences specification (1) if

[8] Appendix F provides additional details for the coworker specification.

the sample of firms is kept the same.[9] For our LATE estimate of the effect of an additional birth at the firm, we use the functional form of (2) with $Treatment_e$ equal to the total number of births in the event year, $BirthsInEventYear_{ef}$, and instrument this using treatment status:

$$\Delta Y_{ef} = \rho_0 + \tau_0 BirthsInEventYear_{ef} + \Delta\xi_{ef} \quad (3)$$

$$BirthsInEventYear_{ef} = \delta_0 + \delta_1 Treatment_e + \epsilon_{ef} \quad \text{(3, First Stage)}$$

Under the assumptions of parallel trends and a monotonicity assumption that a birth to a female employee working at firm f at baseline increases the probability of a birth in the event year at firm f, the 2SLS estimate of τ_0 is a causal LATE estimate for the contemporaneous effect of having one additional employee give birth and go on leave in the event year. We similarly estimate equation (3) for the year after the event year.

The specifications thus far characterize the treatment in terms of number of births at a firm. Alternatively, we can consider the *share* of the total workforce on leave as a different parametrization of the treatment. To do this, we divide both the $BirthsInEventYear$ and $Treatment$ by the number of employees at the firm at baseline, $BaselineEmployees$. For ease of interpretation, we measure baseline employment in 100 baseline employees when applying this rescaling. Our resulting estimate is a LATE estimate for the contemporaneous effect of having one percent of the baseline employees give birth and take leave

[9] In practice, however, when estimating all our 2SLS specifications building on equation (2), we end up including slightly more firms than in the difference-in-differences specification because the specification does not require firms to be observed at $t = 2$ and $t = -4$.

in the event year. To address the possibility that firms of different sizes may evolve along different time trends, we always include a full set of indicators for the exact number of employees in the baseline year when estimating this alternative specification.[10]

5.6 Ensuring the Comparability of Treatment and Control Events: Matching and Reweighting

A comparison of firms experiencing a birth versus firms not experiencing a birth is unlikely to generate an unbiased estimate of the effect of a birth event on firm outcomes. Firms with low fertility rates and thus more likely to be control firms have higher rates of male representation and thus, cover different industries than firms with higher fertility rates. We want to ensure the firms and women underlying treatment and control firms are similar. This will make the critical pre-trends assumption more likely to hold.

To make the treatment firms and control firms similar, we condition on a rich set of baseline observables. In principle, this conditioning could be done in one of two ways—through matching and reweighting or through the inclusion of these observables in our regression specification. For our main specification, we choose to use matching and reweighting for two reasons: First, matching is the standard approach in previous work using similar empirical

[10] The matching and reweighting procedure ensures that the baseline number of employees is balanced across the treatment and control groups. The identification of τ_0, however, also relies on comparisons across firms of different initial sizes *within* the treatment group. Failing to control for initial firm size therefore causes a bias if firms of different sizes evolve along different time paths. In practice, firm size exhibits very clear mean reversion in our data and thus introduces this bias.

designs (Jäger & Heining, 2022; Azoulay *et al.*, 2010; Bertheau *et al.*, 2022). Second, by reweighting the control group, the matching approach uncovers the average treatment on the treated, instead of the weighted treatment effect that is uncovered by OLS with controls (Angrist, 1998). OLS specifications with controls for baseline characteristics deliver almost identical estimates in Appendix G. This is not surprising given the results of Angrist (1998).

Table 2 details the set of baseline characteristics we condition on. This includes both characteristics of the treatment/control woman and of the employing firm. In terms of the women who make up our treatment and control events, we condition on labor market experience, demographics, and fertility history to invoke comparisons of women with similar career trajectories. In terms of firms, we condition on standard measures of size and various proxies of family-friendliness. We do this specially to address that high-fertility women may sort into certain types of firms.

Our estimation procedure relies on exact matching. We use discrete versions of our matching variables and create cells based on all possible combinations of these variables. For each treatment event, we determine all of the control events that belong to the same cell (i.e., that have exactly the same values of the observables for the baseline year). This means that multiple control events will be assigned to a treatment event. Control events are therefore reweighted. When a treatment event is matched to K control units, then each of these controls receives a weight of $\frac{1}{K}$.

As usual, the matching and reweighting procedure rests on a common support assumption and the resulting estimators can be undefined or badly behaved if this assumption is not satisfied (if for some combinations of the observables there are very few treatment or control events). To deal with issues regarding common support, we apply the trimming method proposed by Crump *et al.* (2009) and discard cells where the fraction of control observations in the cell exceeds 0.9 or falls below 0.1. This effectively restricts attention to the subsample of individuals where there is "thick support" in both the control and treatment groups. The downside of this procedure is a potential loss of external validity and sample size.

6 Results

6.1 Descriptive Statistics

Panel A of Table 3 shows the selection of the population of births for our analysis. Between 2003 and 2012, prime-aged women gave birth a total of 504,810 times. About a third of these births occurred to women who were not in regular employment at an active firm two years prior (our baseline year). Among half of the women in regular employment were in the public sector. The restriction of being in a small firm whittles the sample down further. However, the imposition of the restriction of not having births in two consecutive years is less important.

Panel B of of Table 3 shows the translation of the data into potential birth events. After selecting all treatment and control events satisfying the criteria in Panel A, our sample consists of 24,829 treatment events (births) and 162,151 control events, covering a total of 45,940 unique firms. We add further restrictions, including removing a few outlier firms that experience extreme growth or declines and then finally we apply matching and reweighting. Due to the fine-grained nature of our matching and reweighting procedure, the trimming cuts the sample significantly. Of the initial 23,734 treatment events in our sample, 9,934 (41.8 percent) are left after trimming. The resulting sample covers a total of 16,080 unique firms. In Appendix H, as a robustness check, we further present results from a coarser matching and reweighting procedure that effectively trims fewer observations. The results are qualitatively similar.

In Appendix I, we compare the characteristics of our analysis sample to that for the universe of private sector firms in Denmark. This is useful for understanding the subpopulation whose LATE is identified in our design. As expected given the definition of our treatment, treatment firms experience more births and leave days per employee and employ more women than the overall sample of private sector firms. The employees at treatment firms, however, have fewer children. As such, treatment firms are not necessarily more family friendly than a typical private sector firm. Other characteristics of firms in our treatment sample—such as work hours and the wage bill—are comparable to the universe of private sector and small firms.

Table 4 shows (weighted) summary statistics for the baseline year of our analysis samples for firm and coworker outcomes.[11] In the baseline year, firms experience on average 0.79 female employees giving birth and 137 days in total of leave taken by female employees. These firms have 12.9 employees, 65 percent of whom are women, and wage bills of 3.4 million DKK (455,000

[11] Appendix Table A2 shows summary statistics for all seven years used for the analysis instead of just the baseline year.

EUR or 500,000 USD). Finally, Figure 2 shows the distribution of the length of prenatal and postnatal leave among the treatment event women. This clearly shows that women tend to take the majority of available leave. Most women take close to four weeks of prenatal leave; although the distribution of prenatal leave exhibits a long right tail due to pregnancy-related sick leave. In terms of postnatal leave, the modal woman takes the maximum 46 weeks of leave (322 days), while the median duration is 290 days.

To substantiate our identifying assumptions, we find that our reweighted sample of treatment and control firms look similar on baseline observables. Along a range of baseline observables not used in our matching and reweighting procedure, there are only small, insignificant differences between the treatment and (reweighted) control samples (see Appendix Table A3).[12] Appendix Figure A1 further compares the industry composition of our treatment and control samples. The samples are well balanced on industry as well.[13]

6.2 Estimates of our Dynamic Difference-in-Differences Design

We begin our empirical analysis by verifying that our construction of the estimation sample generates a difference in the number of births between treatment and control firms (that is, there is a treatment). Panel (a) of Figure

[12]This is also true for the timing of potential treatment and control events. Treatment and control events are not occurring in systematically different years (last row of Table A3). Our comparison of treatment and control samples is not likely confounded by aggregate time trends.

[13]Formally, the differences in the industry distribution across the two samples are not statistically significant ($p = 0.92$; see Appendix Figure A1)

3 plots OLS estimates of the β_k-coefficients from our dynamic difference-in-differences specification (1), using total births at the firm as the outcome variable. In terms of employee fertility, treatment firms appear to evolve along the same trend as control firms except in the event year, when they experience significantly more births.[14] The apparent lack of pre-trend differences in this figure is comforting for identification purposes. Panel (a) of Figure 3 reveals that treatment firms experience 0.68 *additional* births in the event year relative to control firms. For comparison purposes, Panel (c) shows the sample means over time for both treatment and control firms. Control firms experience on average 0.62 births in the event year.[15]

The patterns in Figure 3 indicate that turnover causes imperfect compliance with treatment as discussed earlier in Section 5.5.1. If all women stayed with their baseline employer through the event year, we would expect treatment firms to experience exactly one more birth in the event year relative to control firms. For our treatment events, the fraction of potential mothers who are still with the treatment firm in the event year is 0.62 in the raw data.[16] Regardless, Figure 3 confirms that our approach leads to meaningful differences in the number of births between treatment and control firms.

We next examine how the additional number of births affects leave take-up.

[14]Note that our definition of the treatment and control involves conditioning on having one female employee at baseline who either gives birth exactly in the event year or does not give birth over the next few years. We do not place any restrictions on any of the other employees at our treatment and control firms, so the pattern shown in Figure 3 is *not* a mechanical consequence of the sample definition.

[15]Appendix D shows that there is no meaningful effect on coworker fertility and leave-taking.

[16]The gap in the number of births between treatment and control firms in the difference-in-differences is not identical to potential mothers turnover rates as coworker births are subject to random variation at both treatment and control firms.

The OLS estimates in Panel (b) of Figure 3 show that the additional births cause a significant increase in the total number of parental leave days both in the event year and in the following year. Most postnatal leaves stretch partly into the calendar year after the birth—giving rise to the increase in the year after birth. In terms of magnitudes, the OLS estimates are in the order of 136 and 59 extra days of leave, respectively, for the event year and the following year. Comparing to the sample means in Panel (d), these are substantial increases in leave-taking. Because of imperfect compliance, however, the OLS estimates capture ITT effects and understate the actual number of leave days that a firm experiences when a current employee gives birth.

For total leave take-up, the 2SLS results are shown in the top row of Table 5. Columns (1) and (2) of the table show the estimated absolute effect of one additional birth, while columns (3) and (4) show the estimated relative effect of having one percent of the baseline workforce give birth. For continuous firm outcomes that are not scaled relative to baseline, the model using the number of births at the firm is the preferred specification. For binary outcomes or outcomes measured relative to baseline, our preferred treatment variable is instead the percent of the workforce on leave.

Taking these preferred specifications into account, one additional female employee giving birth in the event year leads to an increase of total leave days at the firm of 196 in the event year (column (1)) and 86 in the following year (column (2)). Adding these up, this is a total of 282 days or about nine and a half months. This aligns well with aggregate statistics indicating that the average woman in Denmark takes a little less than ten months of leave.

6.3 Labor Adjustment: Extensive Margin

We now examine whether and how firms' total labor inputs respond to the loss of labor when a worker goes on leave. Panel (a) of Figure 4 shows OLS estimates for the effect on the total employment stock (including workers on leave). We see no differences in the years prior to the event year. In the event year, however, there is a significant increase in the number of employees; this increase dissipates in the following time periods. In terms of magnitudes, the second row of Table 5 presents corresponding 2SLS estimates. Because we measure firms' employment stocks relative to the baseline, columns (3) and (4) contain our preferred estimates. When one percent of the baseline workforce gives birth and goes on leave, firms temporarily increase their employment stock by 0.63 percent in the event year. Thus, firms adjust quite strongly on the extensive margin to mitigate the implied loss of labor when an employee gives birth and goes on leave.

Next, we examine the nature of this extensive margin adjustment. An increase in the employment stock can occur in two ways: changes in the number of new hires and/or changes in the retention rates of existing workers. Panel (b) of Figure 4 shows OLS estimates for new hires. New hires indeed play a role for the increase in total employment; the number of new hires temporarily increases in the event year in response to an employee's birth and going on leave. Panel (c) of Figure 4 shows corresponding results for turnover, measured simply by the number of employees leaving the firm. Focusing on the event year only, turnover drops in response to employee giving birth. This shows that firms also adjust their employment stock through increased retention of existing workers. In terms of magnitudes, 2SLS results in Table 5 suggest that the more important adjustment channel is that of new hires but that both channels are quantitatively relevant.[17]

Looking at hiring and turnover beyond the event year, turnover increases to above the baseline level one year after the event year, while new hires drop slightly below the baseline level. This reflects that the increase in the employment stock is temporary and that firms shed the additional workers when the original employee returns from leave.

To see how firms' extensive margin adjustment affects the coworkers of a female employee giving birth and going on leave, the remaining panels of Figure 4 turn to our coworker sample. Recall that this sample follows coworkers who were at the treatment or control firms in the baseline year. Panel (d) shows OLS estimates for the effect of a birth and parental leave on coworkers' likelihood of staying with the baseline firm, while Panel (e) examines coworkers' unemployment risk. Consistent with the decrease in turnover rates, we estimate that an employee experiencing a birth and going on leave has a positive effect on the likelihood that coworkers will stay with the baseline firm in the event year and a negative effect on their unemployment risk, although only the latter effect is statistically significant. The same pattern of estimated effects emerge in the year after the event year. The temporary hires that are engaged when an employee gives birth and goes on leave thus do not replace existing employees in the longer term. Columns (3) and (4) of Table 5 quantify the

[17] Column (3) of Table 5 shows that when one percent of the workforce gives birth and goes on leave, new hires increase by 0.022 individuals, while turnover only drops by 0.012 individuals.

retention effects on coworkers. Because effects on coworkers are likely to always depend on the total size of the employment stock, we use 2SLS estimates from equation (3) as our preferred estimate throughout the coworker analysis. The 2SLS results here suggest that when one percent of the workforce has a birth and goes on leave, coworkers' likelihood of staying with the baseline firm increases by 0.12 percentage points in the event year, while their share of the event year spent unemployed decreases by 0.02 percentage points.

6.4 Labor Adjustment: Intensive Margin

Aside from hiring temporary workers and reducing turnover of existing employees, firms can compensate for labor supply losses through increasing work hours for coworkers. Panel (a) of Figure 5 presents OLS estimates for the impact of a birth and parental leave on our approximate measure of hours of work in the coworker sample. We detect a small but statistically significant increase in the event year, suggesting that when a worker experiences a birth and takes leave, firms increase the coworkers' hours. When one percent of the workforce gives birth and goes on leave, our 2SLS estimates imply that existing coworkers' hours increase by 0.10 percent in the event year (column (3) in Panel B of Table 5). We note that this estimate may be a lower bound on the true increase in hours because our measure of hours does not capture smaller increases in weekly hours or increases in overtime.

6.5 Net Effect on Labor Inputs

In response to a birth event, firms try to offset the resulting loss of labor by increasing labor inputs along both the intensive and extensive margins. In Panel (b) of Figure 5, we examine the combined net effect on labor inputs when an employee gives birth and goes on leave. The figure (and parallel 2SLS specifications in Table 5) does not show any economically or statistically significant change in our measure of hours in the year the worker goes on leave or in the following year. For the 2SLS estimates, the corresponding 95 percent confidence intervals exclude a total drop in hours exceeding 0.19 percent in the first year. Overall, firms appear to counteract very effectively the loss of labor that occurs when an employee gives birth and goes on leave.

To assess whether parental leave affects the quality of labor inputs, we examine the effect of parental leave on the characteristics of the workforce in Appendix J. We find small effects on different measures of labor quality, which go in opposite directions in the event year: when a worker experiences a birth and goes on leave, average schooling decreases slightly, while average experience increases. There is no evidence of a systematic negative effect on the quality of labor inputs. If anything, average workforce characteristics seem to improve slightly following a worker going on parental leave.

6.6 Labor Costs and Earnings

A firm may have to compensate existing workers for extending their work hours, which could raise its wage bill. But firms might pay temporary workers lower wages than that for women on leave, leading to lower costs.

The firms' total wage bill includes wages paid to workers on leave and thus reflects the costs firms face before being reimbursed for any paid leave. Panel (a) of Figure 6 shows OLS estimates for the effect of a birth event followed by parental leave on firms' total wage bills. When a worker gives birth and goes on leave, firms' total wage bills increase significantly in the event year but then return to their initial level. Our preferred 2SLS estimate in Table 6 shows that when one percent of the workforce gives a birth and goes on leave, firms' total wage bills increase by 0.27 percent in the event year (column (3)).

Next, we examine the wage bill after excluding paid leave. The wage bill excluding paid leave should be a close approximation of the actual costs firms face after receiving reimbursements. Panel (b) of Figure 6 shows the corresponding OLS estimates. We see a very different pattern here. The wage bill excluding paid leave shows no statistically significant change and the point estimate is negative. Based on our preferred 2SLS specification in Table 6, the upper bound of the 95 percent confidence interval for the impact on the wage bill excluding paid leave is 0.004 percent in the event year when one percent of the workforce has a birth and goes on leave (column (3)).

Panel (c) of Figure 6 shifts the focus to the coworker sample and provides OLS estimates on the effect of a birth and leave-taking on coworkers' earnings. Coworker earnings increase significantly in the event year, and there are some indications that this effect persists over time. The corresponding 2SLS estimate in Table 6 shows that coworker earnings increase by 0.13 percent in the event year when one percent of the baseline workforce gives birth and goes on leave. This increase mirrors the increase in coworker hours.

Finally, we examine whether firms compensate for workers on leave by buying more services from other firms, for example by bringing in temporary workers via temp firms or by paying for recruiting services. This could raise firm costs, even if wage payments do not increase. In Panel (d) of Figure 6, we examine our measure of total variable costs, which combines total wage payments and total purchases from other firms. The evidence points to minimal effects on variable costs. Based on our preferred 2SLS specification in Table 6, the upper bound of the 95 percent confidence interval for the impact on the total variable cost is 0.082 percent in the event year when one percent of the workforce gives birth and goes on leave (column (3)).

6.7 Firm Performance and Coworker Well-Being

Finally, we examine the impact on overall firm performance. Even if parental leave has negligible negative effects on total labor inputs and costs, these labor measures are insufficient alone for understanding the effects on the firm's production as the observable changes in the labor mix may have implications for productivity. Panel (a) of Figure 7 plots OLS estimates of the impact of having a worker give birth and go on parental leave on firms' output, as measured by total sales. Output does not appear to be negatively affected. Our preferred 2SLS estimate in column (3) of Table 7 is actually slightly positive, and the 95 percent confidence interval excludes drops in total sales exceeding 0.18 percent in the event year when one percent of the workforce goes on leave. Unsurprisingly given our previous results, profits also appear to be unaffected (Panel (b) of Figure 7), although estimates are less precise, likely because our measure of profits is quite noisy.

In Panel (c) of Figure 7, we look at the effects on the likelihood of firm survival as proxied by whether the firm has positive sales. No noticeable effects are apparent. Based on our preferred 2SLS estimates in Table 7, the lower bound of the 95 percent confidence interval of the effect on the probability of firm survival is -0.05 percentage points in the event year when one percent of the baseline workforce gives birth and goes on leave (column (3)). In the year after the event year, this lower bound is -0.04 percentage points (column (4)). Overall, we find no compelling evidence that worker absence due to a birth has detrimental effects on overall firm performance.

Turning to the overall effect on coworker well-being, the previous results suggest that if anything, a birth event combined with its associated leave has positive effects on coworkers' labor market outcomes: their unemployment risk falls, while their hours and earnings increase. A potential concern, however, is that the increases in work hours could reflect some coworkers in fact being overworked when a colleague goes on leave, which could have negative health effects. However, we see no impact on takeup of publicly paid sick days in Panel (d) of Figure 7.[18]

[18] Employees on sick leave become eligible for public funds once their sickness lasts longer than two weeks, so this measure captures longer sicknesses.

6.8 Parental Leave Absences vs. Labor Supply Adjustment around Childbirth?

The definition of treatment in our analysis is based on whether an employee becomes pregnant and gives birth. This treatment definition is useful for studying the effects of parental leave because a birth is always followed by a substantial leave period in our setting. Given recent results that childbirth affects women's labor supply decisions (Kleven *et al.*, 2019), this raises a question of whether our estimated results reflect the direct effects of a parental leave absence or reflect the effects of other labor supply changes around childbirth.

We observe significant differences in labor market behavior between treatment and control women going beyond leave take-up (see Appendix K). Quantitatively, however, these additional labor supply changes turn out to be an order of magnitude smaller than the direct effects of parental leave absences. For example, two years after childbirth, the drop in yearly work hours for treatment women relative to control women is less than one percent of a full-time worker. In contrast, the direct effect of an average parental leave absence is a reduction of 46 percent of a full-time worker in the birth year and 24 percent the year after. Overall, we thus expect that the estimated effects on firms and coworkers primarily reflect the direct effects of the absence of the female worker during the long parental leave period. This is corroborated by the timing of our estimated effects which all align closely with the actual leave period.

6.9 Heterogeneity and Mechanisms

In our study, the effects of a female worker giving birth and going on parental leave are small because firms are very effective in adjusting labor inputs to compensate for the worker on leave. However, given existing evidence that labor markets are characterized by frictions, at least two questions emerge from these results: How are the observed firm adjustments related to labor market frictions? And are there some firms that are unable to adjust effectively?[19]

6.9.1 Same vs. Different Occupation Coworkers

A natural first cut for heterogeneity based on possible labor market frictions is to examine the effects on co-workers who are complements and co-workers who are substitutes. When an employee goes on leave, we would expect firms to increase wages, hours, and/or retention rates for coworkers who are substitutes whereas coworkers who are complements could simultaneously see decreases in wages, hours, or retention rates.

We expect coworkers in the same occupation to be substitutes, while coworkers in different occupations to be complements. In Table 8, we therefore present 2SLS estimates separately for coworkers in the same 1-digit occupation as the woman on leave (Panel A) and for coworkers in a different occupation than the woman on leave (Panel B).[20] The positive coworker effects in the overall sample are driven entirely by same-occupation coworkers. Treated same-occupation coworkers increase their work hours and have higher earnings in the treatment

[19] Appendix L examines heterogeneity by firm size. We see no indications that the effects of parental leave vary with firm size but standard errors are fairly wide.

[20] Corresponding OLS estimates are presented graphically in Appendix M.

year and the year thereafter. Specifically, when one percent of the workforce has a birth and goes on leave, same-occupation coworkers raise their work hours at the baseline firm by 0.17 and 0.12 percent in the event year and in the following year, respectively. This is concurrent with a 0.27 and a 0.21 percent increase in their earnings for those time periods. These effect sizes are approximately twice as large as for the overall sample of coworkers. In contrast, we detect no significant changes in the work hours or earnings among different-occupation coworkers.

6.9.2 Firms with No Coworkers in the Same Occupation

The previous results suggest that the effects of a birth followed by parental leave are limited because firms compensate for the absent worker along two margins: temporary hiring of new workers and increased hours and retention for existing workers in the same occupation as the woman on leave. This suggests a relevant dimension of firm heterogeneity which we now examine: Some firms simply do not have other workers in the same occupation as the woman on leave and thus are entirely precluded from adjusting on the second margin. We refer to these firms as *no replacement* firms.[21]

For no replacement firms in a labor market with frictions, it may not be possible to seamlessly replace workers on parental leave if, for example, firm-specific human capital is important. No replacement firms may also be less able to rely on new hires. If a firm only has one worker in a specific occupation,

[21] To be precise, we say that a firm connected to some treatment or control event is a *no replacement* firm if the corresponding treatment or control woman is the only employee in her 1-digit occupation at the firm in the baseline year.

it might, for example, be more difficult to sort out qualified applicants and to attract a temporary worker to replace the leave-taking worker.

Table 9 presents 2SLS estimates for the subsample of no replacement firms.[22] These firms constitute 10 percent of the firms in our main sample and we therefore have limited statistical power when looking at these firms.

First, no replacement firms experience fewer leave days than other firms. Relative to replacement firms, no replacement firms experience 35 fewer days of leave on average. This may reflect that workers at no replacement firms internalize that they are difficult to replace

Second, despite the fact that no replacement firms experience fewer leave days than the typical firm, we see that these firms are actually less successful in replacing the lost labor input. For no replacement firms in the event year, we estimate negative effects for both total hours and the wage bill, which are at least marginally significant in all specifications unlike what we found for the overall sample. Based on the preferred specification in column (3), a no replacement firm where one percent of the workforce gives birth and goes on leave experiences a 0.33 percent drop in measured total hours and a 0.45 percent drop in the total wage bill excluding paid leave in the event year. Besides the fact that no replacement firms cannot compensate for absent workers via same-occupation coworkers, we see some evidence that this drop in labor inputs also occurs because they are less likely to hire replacement

[22] To assess whether the effects of a birth and subsequent parental leave on no replacement firms are statistically significantly different from the effects at other firms, Appendix Table A4 also presents 2SLS results from regressions on the full sample in which we interact our main effect (and instrument) with an indicator for whether the firm is a no replacement firm.

workers than other firms—although this difference is generally not statistically significant.[23]

Finally, turning to our measures of firm performance, the loss of labor inputs at no replacement firms is associated with drops in both firm sales and gross profits and an increase in shutdown but we are cautious to overintepret these due to the imprecision of the estimates.

Overall for this no replacement sample, there is suggestive evidence of negative effects. This highlights the importance of firm adjustments for our main results. It also emphasizes that parental leave may have different implications for certain vulnerable firms.

6.10 Comparison to other evidence

We document that women going on parental leave has limited costs on firms and coworkers. Our results stand in contrast to those from recent studies, which show that extensions in the duration of parental leave tend to have negative effects on firms overall. These policies have been found to increase firm shutdown (Gallen, 2019), raise firms' wage bill and decrease sales revenue (Ginja *et al.*, 2022), and reduce firms' short-term employment (Huebener *et al.*, 2021). This begs the question: which factors determine firms' ability to effectively adjust to employees' leave-taking? We attempt to shed some light on this matter by discussing how our setting and analysis differ from the rest of the literature.

[23] The estimated effects on both total employees and new hires is smaller for no replacement firms than for the overall sample but mostly statistically insignificant. In most cases, however, we cannot reject that the estimated effects for no replacement firms are the same as for other firms (see Table A4).

A distinct feature of our work is that it provides the first evidence on how firms respond to having an employee take versus not take parental leave (i.e. an extensive margin shock). Instead, all aforementioned studies examine intensive margin shocks, as they focus on extensions in the duration of leave among employees who are already eligible for a lengthy leave. This distinction is important because unlike our setting, extensions of already lengthy periods of parental leave increase women's separation from their pre-birth employer (Rossin-Slater, 2018; Gallen, 2019; Ginja *et al.*, 2022) which can in turn be highly disruptive for employers (see, for instance, Bertheau *et al.*, 2022).

A second difference is that the leave extensions studied in Gallen (2019) and Ginja *et al.* (2022) were unexpected and granted to women who had already been on leave for several months. Typically however, absences due to parental leave differ from most other employee absences exactly in that they are anticipated, as is the case in our setting. This gives firms the ability to better plan around them and avoid negative effects. This may also explain why unlike other studies, we find that firms make successful temporary adjustments to their labor inputs already in the year a parental leave starts.

Finally, the conclusions in Schmutte & Skira (2020) on hiring/firing costs and in Friedrich & Hackmann (2021) on occupational licensing, further suggest that parental leave may have negative effects if external circumstances restrict employers' ability to replace workers on leave. This dovetails our findings for *no replacement* firms.

7 Conclusion

Most governments currently offer new parents some form of parental leave. Although a large body of literature investigates the impact of leave take-up on women's careers and children's well-being, less is known about firms' responses to these programs. This paper aims to fill this gap in the existing literature by being the first to estimate how firms and coworkers are affected when an employee gives birth and goes on leave. We do this using detailed administrative data on firms and workers from Denmark—a country with generous parental leave benefits. Our main identification strategy relies on contrasting small firms where a female employee is about to give birth and observationally equivalent firms with a female employee who does not give birth in the next few years. We then compare the evolution of a multitude of firm and coworker outcomes around the time of the birth.

Our findings indicate that firms hire temporary workers and slightly increase retention of existing employees in response to a birth and subsequent leave take-up. Additionally, existing workers see temporary increases in their hours of work and earnings and reductions in their unemployment risk. On net, we therefore see no significant effects on firms' total labor inputs. Firms' total wage bills increase temporarily; however, this is completely driven by wages paid to workers on leave for which employers are eventually reimbursed. Overall, we do not find any significant effects of having an employee give birth and go on parental leave on firms' output, gross profit, and closure.

These aggregate effects conceal some important heterogeneous responses that are critical for understanding the possible disproportionate effects of worker absence, however. In particular, for a small subset of firms that cannot draw on existing same-occupation coworkers to compensate for the person on leave, we do find indications of negative effects on firms. This confirms the idea that most firms are able to very effectively compensate for a worker on parental leave. It also underscores that parental leave can have substantial negative effects on certain vulnerable firms.

Finally, our findings also have implications for understanding the effects of worker absence on firms more broadly, especially when contrasted with work on other types of worker absences and on parental leave extensions. Here our findings suggest that two factors are central to determining whether a worker absence is costly. Specifically, absences appear to be more costly if they eventually lead to higher turnover but appear less costly if firms can anticipate the absence and plan around it. Exploring these channels further is an important topic for future work.

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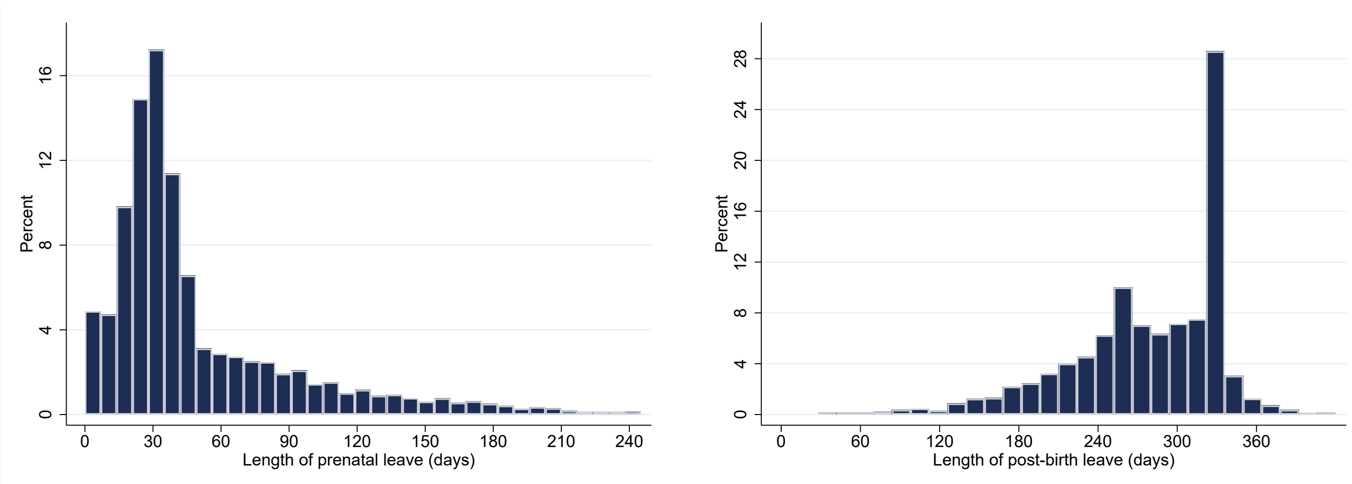
Figure 1: Definition of treatment and control samples

	Baseline year		*Event year*	
	$t = -2$	$t = -1$	$t = 0$	$t = 1$
Treatment events (firm f, woman w)	Woman w is with firm f	Woman w has *no* birth	Woman w gives birth	Woman w has *no* birth
Control events (firm f', woman w')	Woman w' is with firm f'	Woman w' has *no* birth	Woman w' has *no* birth	Woman w' has *no* birth

Notes: This figure summarizes the construction of the treatment and control samples (see Subsection 5.1).

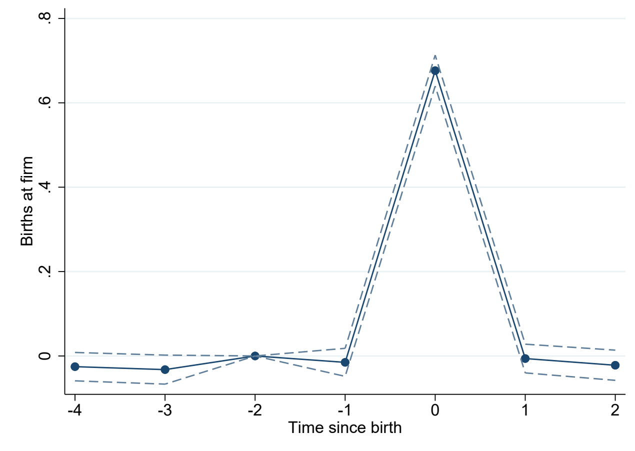
Figure 2: Histogram of the duration of women's prenatal and postnatal leave

(a) Prenatal leave (b) Postnatal leave

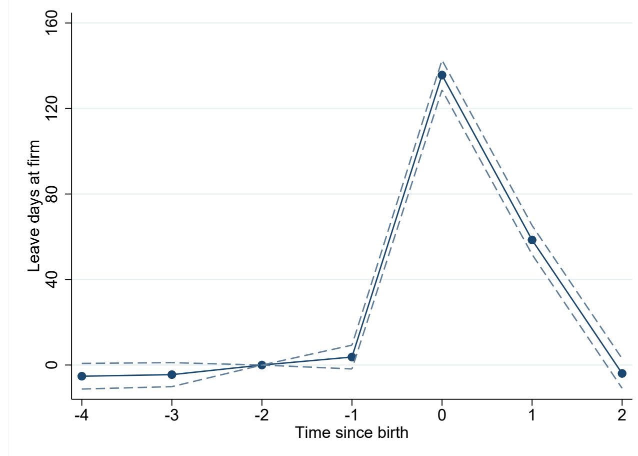


Notes: The histograms illustrate the distributions of the duration of prenatal and postnatal leave, respectively, taken among mothers in our estimation sample; it includes both paid and unpaid leave.

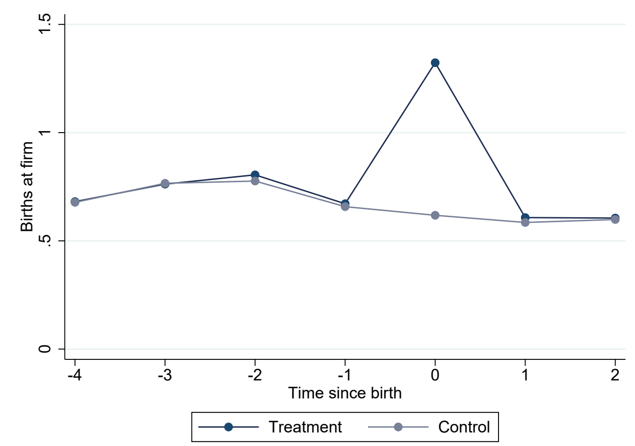
Figure 3: Estimates for firms' total births and parental leave days, OLS



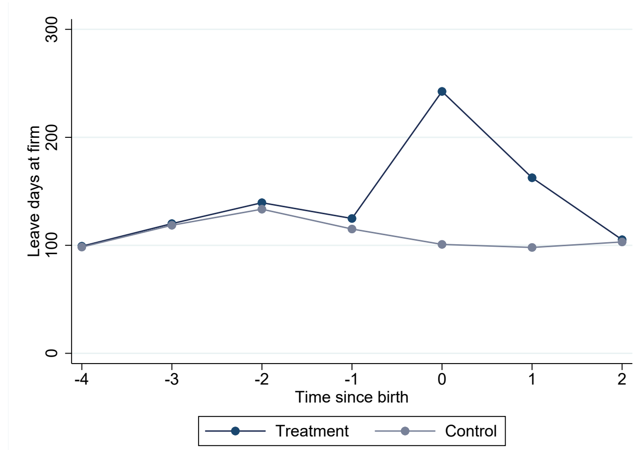
(a) Births at firm



(b) Parental leave days at firm



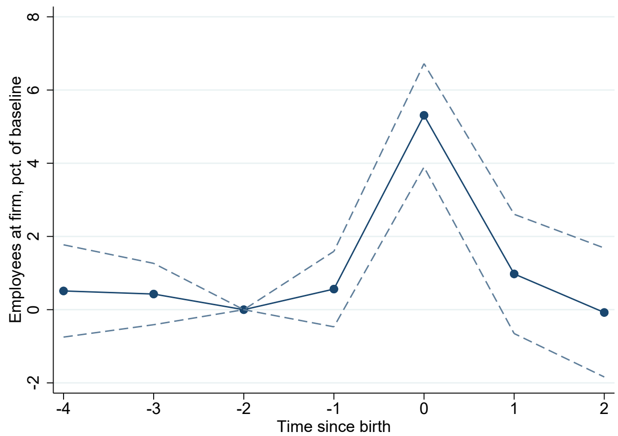
(c) Sample means, births



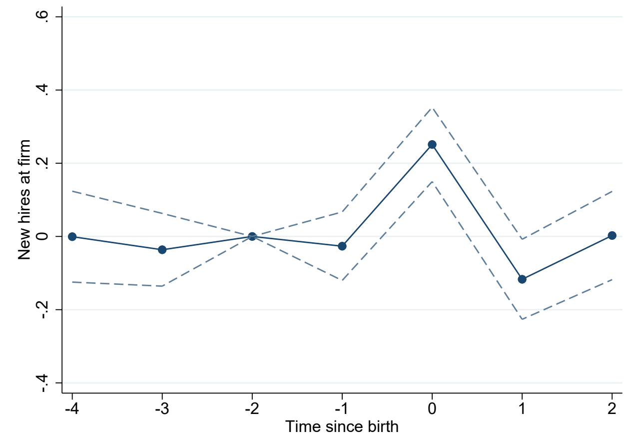
(d) Sample means, leave days

Notes: Panels a) and b) shows difference-in-difference estimates. The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, implying that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level. For comparison, Panels c) and d) shows sample means over time

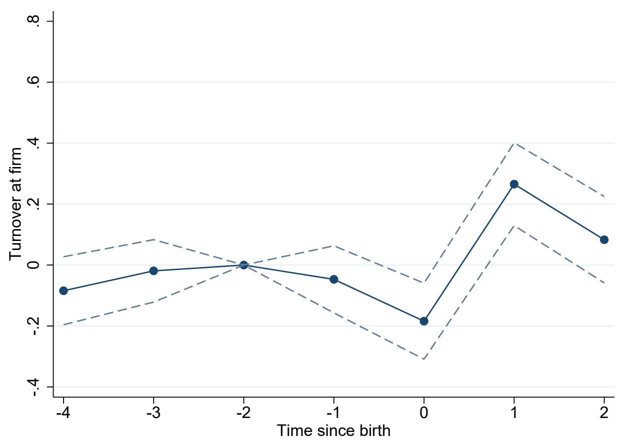
Figure 4: Effects on employment outcomes, OLS



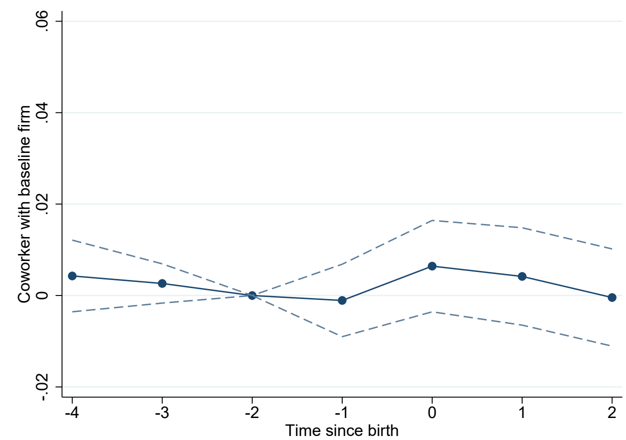
(a) Number of employees at firm



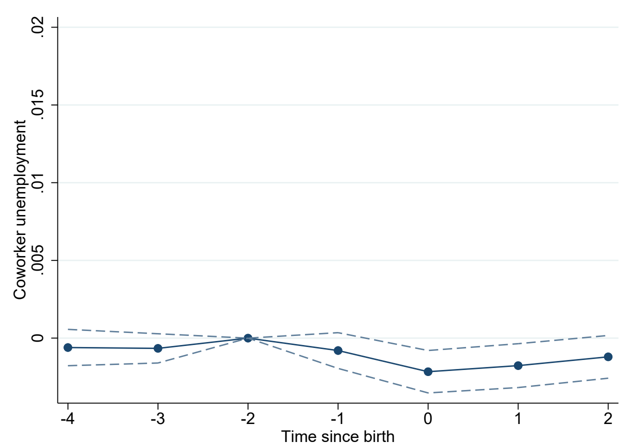
(b) New hires at firm



(c) Turnover at firm



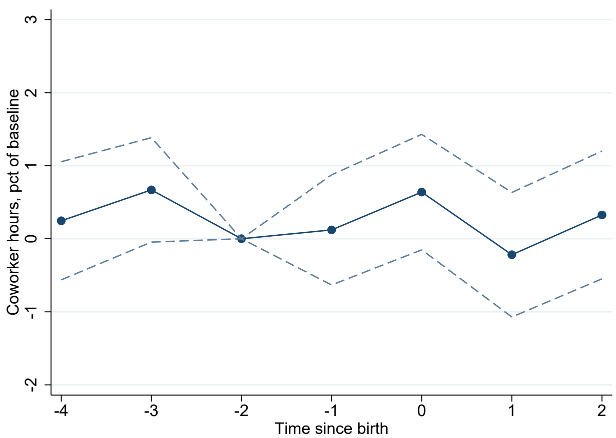
(d) Likelihood coworkers with baseline firm



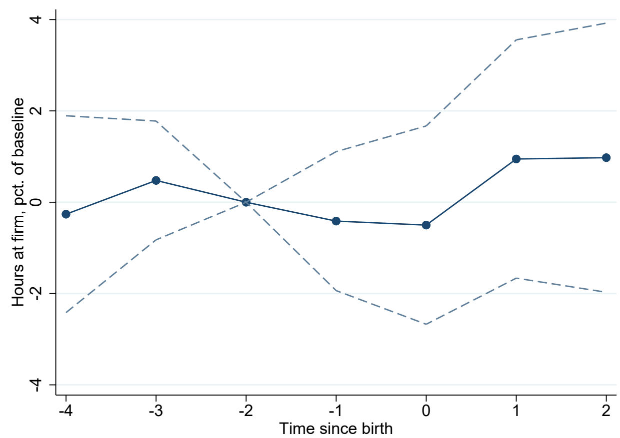
(e) Coworkers' share of year unemployed

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Figure 5: Effects on hours of work, OLS



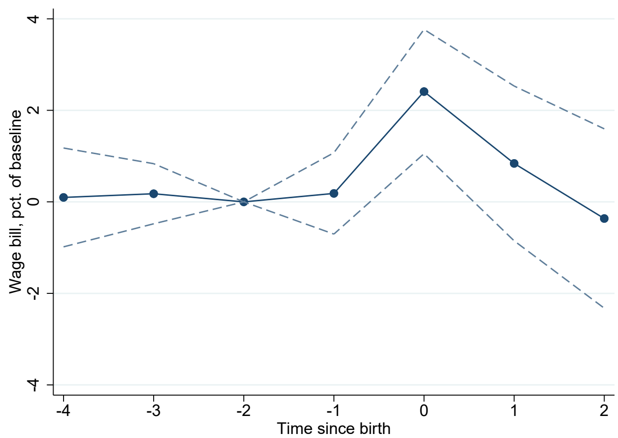
(a) Coworker hours



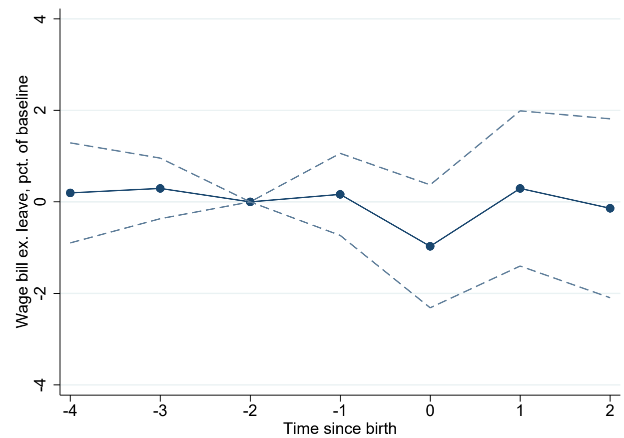
(b) Hours at firm

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

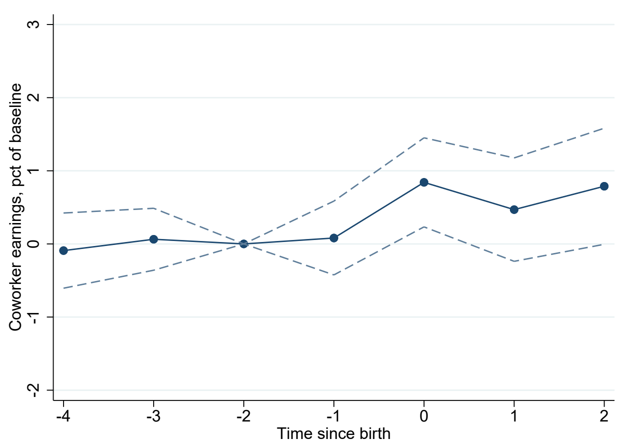
Figure 6: Effects on wage costs and earnings, OLS



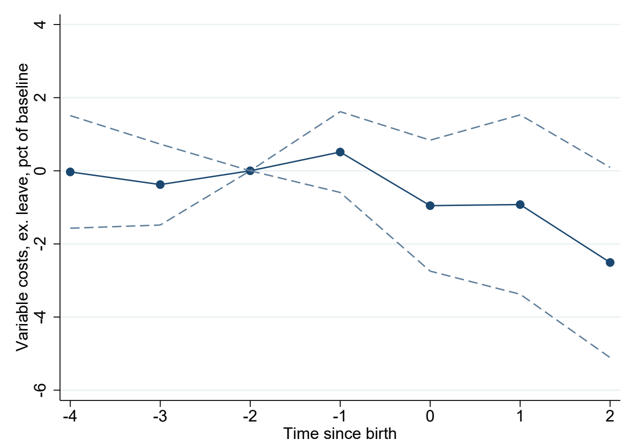
(a) Firms' wage bill



(b) Firms' wage bill (excluding paid leave)



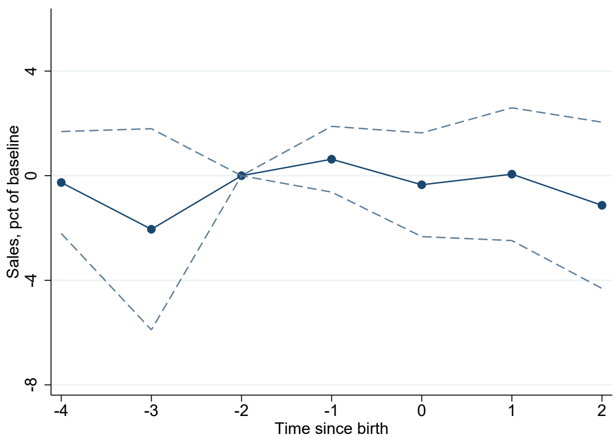
(c) Coworkers' earnings



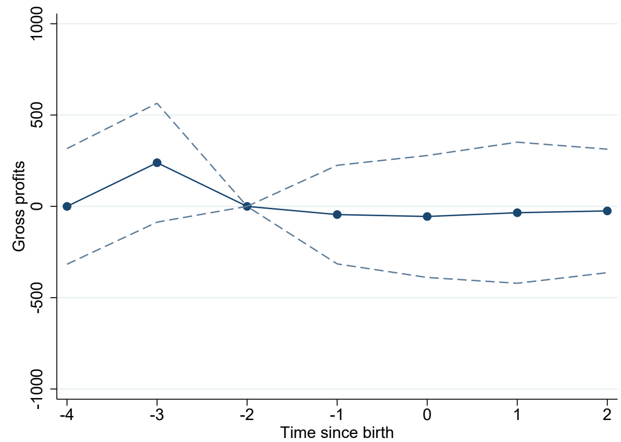
(d) Firms' total variable costs (excluding paid leave)

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

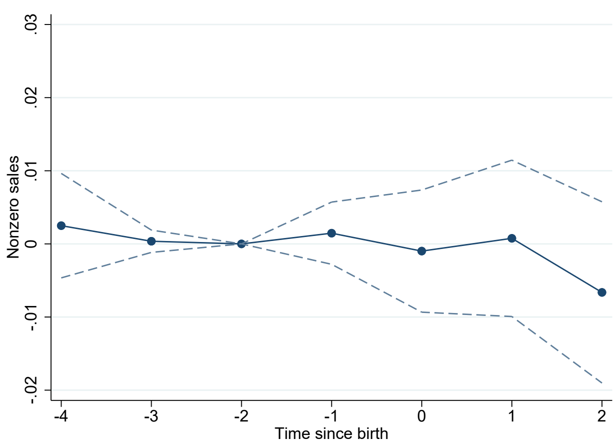
Figure 7: Effect on firms' overall performance and coworkers' sick leave, OLS



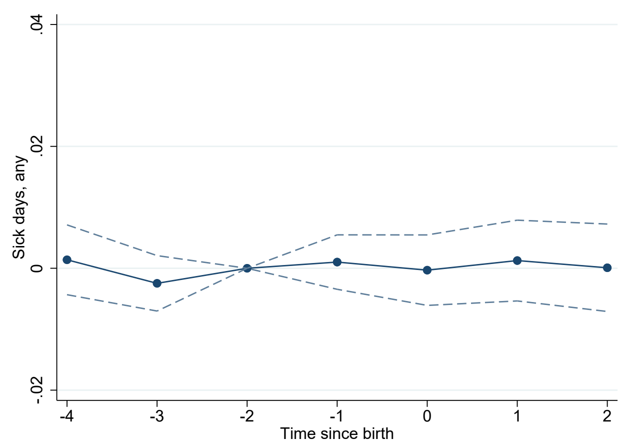
(a) Firms' total sales



(b) Firms' gross profits (1000 DKK)



(c) Likelihood of firm survival



(d) Likelihood of coworkers taking sick leave

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Table 1: Overview of the Danish parental leave system

	Prebirth 4 weeks total	Postbirth 46 weeks total[b]	
		First part	Second part
Legal minimum			
Job protection:	Yes	Yes	Yes
Wage replacement:	UI payment	UI payment	UI payment
Typical contract with leave benefits[a]			
Wage replacement:	Fully paid, firm reimbursed	Fully paid, firm reimbursed	UI payment

Notes: The table summarizes the minimum parental leave benefits for new mothers assuming that the father does not take any of the shared leave.
[a]The typical contract refers to the roughly three-quarters of firms that have a collective bargaining agreement. Mothers under this agreement are paid full wage during the first 14 weeks of leave after delivery; fathers are eligible to take 2 weeks of leave with similar compensation rules as mothers' leave during this period (and the vast majority do). In addition, parents under collective bargaining agreements have five weeks each plus three weeks with full wages that they can split as they wish.
[b]The first part of post-birth leave refers to the part where mothers are compensated their full wage (see note (a)). Regardless of being under a collective bargaining agreement, the parental leave funds reimburse 14 weeks to the mother following birth, 2 weeks to the father following birth, and 25 weeks to the parents collectively, which the parents can split as they wish. See Appendix B for more details.

Table 2: The baseline observables conditioned on in the empirical analysis

Woman's labor market characteristics	Quintiles of earnings, education group (six groups), indicator for having at least two years of tenure with the firm, quintiles of age
Woman's fertility history	Total number of children, number of two-year-old children, number of one-year-old children, number of newborns
Firm size	Quintiles of the number of employees, quintiles of sales
Additional firm characteristics	Quintiles of share of female employees, quintiles of average number of children per employee

Notes: This table lists the variables on which we do exact matching. For the education grouping, we use the standard six Danish education groups; we treat missing education information as a separate category. For all variables quintiles are computed based only on the analysis sample rather than for the population overall. Moreover, for earnings and sales, quintiles are computed separately for each year to account for inflation.

Table 3: Sample selection

Panel A—Births in Denmark

Total births to prime-age women, 2003-2012	504,810
- woman in regular employment at active firm two years prior	329,456
- woman in private sector two years prior	171,950
- woman not a student or a new hire two years prior	102,450
- woman at small firm two years prior	26,908
- woman *without* births in two consecutive years	24,829

Panel B—Analysis sample of potential birth events

	Treatment events	Control events	Total unique firms
Base sample of events:	24,829	162,151	45,940
Excluding sale and wage bill outliers:	24,543	160,480	45,558
Excluding extreme growth/decline firms:	23,734	155,625	44,165
Applying trimming:	9,934	21,974	16,080
After matching/reweighing:	9,934	9,934	16,080

Notes: The table illustrates the selection of the population of interest and the analysis sample. Panel A shows the total number of births to prime-age women (age 21-35) in Denmark over the sample period. The panel then shows how many of these births were to women who were in regular employment at an active firm two years prior. Regular employment is defined as having positive earnings and fulfilling the criteria for being a main November employee according to Statistics Denmark's standard definition. Active firms are defined as currently having a total number of workers equal to at least one full-time employee, having positive sales and wage payments and also having had either positive sales or positive wage payments in the previous year. The panel further shows how many of the births were to women who were not enrolled as students and had at least one year of tenure with their respective firm two years before the birth. Finally, the panel shows how many of the births were to women who were in private sector firms and at small firms. Small firms are defined as having a stock of employees between 3 and 30 and a total number of employment relationships during the year of 60 or less. Panel B shows the number of these births included in our analysis sample of treatment events as well as the corresponding number of control events and the total number of unique firms covered by the treatment and control events. The rest of panel B shows how our analysis sample changes as we further drop outlier firms or firms exhibiting extreme growth or declines, as well as how the sample changes when we condition on observables using trimming and matching/reweighting. Outlier firms are defined as firms with sales per employee below 10,000 DKK (1,300 EUR or 1,500 USD) or above 100 million DKK (thirteen million EUR or fifteen million USD), and firms with wages per worker must below 10,000 DKK (1,300 EUR or 1,500 USD) or above one million DKK (130,000 EUR or 150,000 USD).

Table 4: Summary statistics of the firm and coworker samples, baseline year

	Observations (unweighted)	Mean	Standard Deviation
Panel A—Firm sample			
Births at firm	31,908	0.788	1.051
Pregnancies at firm	31,908	1.394	1.559
Leave days at firm	31,908	137.4	195.7
Employees	31,908	12.94	7.933
New hires	31,908	3.714	3.271
Turnover at firm	31,908	3.674	4.054
Wage bill (1000 DKK)	31,908	3,370	2,997
Sales (1000 DKK)	31,908	18,456	40,039
Purchases (1000 DKK)	31,908	12,565	32,844
Gross profits (1000 DKK)	31,908	2,521	17,435
Workforce share women	31,908	0.647	0.278
Workforce avg. age	31,908	33.88	6.434
Workforce avg. years schooling	31,908	11.61	1.282
Workforce avg. years education	31,908	12.29	5.327
Panel B—Coworker sample			
Coworker still with baseline firm	268,403	1.000	0.0000
Coworker unemployment (yearly share)	268,403	0.0146	0.0614
Coworker hours (FTEs)	268,403	0.930	0.135
Coworker earnings (1000 DKKs)	268,403	304.0	187.1

Notes: The table shows summary statistics for the matched firm (Panel A) and coworker (Panel B) samples only for the baseline year used in the analysis. Means and standard deviations are computed with weights. The total number of observations displayed is unweighted.

Table 5: Effects on labor inputs and employment, 2SLS

	Absolute effect Effect of one additional birth		*Relative effect* Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
A) Firm outcomes				
Parental leave days at firm	195.6**	86.29**	11.08**	4.891**
	(4.785)	(4.559)	(0.257)	(0.247)
Number of employees at firm (pct. rel. to baseline)	7.244**	1.128	0.626**	0.130
	(0.926)	(1.123)	(0.0784)	(0.0940)
New hires at firm	0.351**	-0.149	0.0221**	-0.00380
	(0.0689)	(0.0766)	(0.00284)	(0.00306)
Turnover at firm	-0.261**	0.365**	-0.0115**	0.0234**
	(0.0851)	(0.0907)	(0.00332)	(0.00362)
Hours at firm (pct. rel. to baseline)	-0.321	0.611	-0.0479	0.0503
	(0.892)	(1.082)	(0.0713)	(0.0858)
F-stat	2,194	2,194	2,194	2,194
Observations	31,908	31,908	31,908	31,908
Observations (weighted)	19,868	19,868	19,868	19,868
Clusters (firms)	16,080	16,080	16,080	16,080
B) Coworker outcomes				
Coworker with baseline firm	0.00929	0.00760	0.00120*	0.000669
	(0.00631)	(0.00678)	(0.000561)	(0.000591)
Coworker share of year unemployed	-0.00221*	-0.00251**	-0.000206*	-0.000260*
	(0.000860)	(0.000893)	(9.64e-05)	(0.000103)
Coworker hours (pct. rel. to baseline)	0.846**	0.246	0.101**	0.0337
	(0.301)	(0.323)	(0.0338)	(0.0361)
F-stat	959.4	963.9	3,005	2,988
Observations	268,403	267,213	268,403	267,213
Observations (weighted)	167,522	168,416	168,281	167,522
Clusters (firms)	15,405	15,401	15,405	15,401

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the number of births at the firm in the event year is instrumented by the treatment dummy. Columns (3) and (4) show estimates from similar regressions but in which both the number of births at the firm in the event year and the treatment dummy is divided by the number of baseline employees (measured in hundreds), and where dummy variables for each possible number of baseline employees are included as controls. In Columns (1) and (3) the outcome variable is measured in the event year (Time 0). In Columns (2) and (4) the outcome variable is measured in the following year (Time 1). Panel A uses firm-level data, while Panel B uses coworker-level data. In Panel B, the number of observations changes between columns because a small number of coworkers drop out of the original administrative data after the event year. Throughout, the analysis is conducted on the matched and reweighted samples. For each panel and column, the F-stat from the first stage regression is listed. Standard errors (in parentheses) are clustered at the firm level. ** p <0.01 * p <0.05.

Table 6: Effects on labor costs and earnings, 2SLS

	Absolute effect Effect of one additional birth		*Relative effect* Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
A) Firm outcomes				
Firm's wage bill (pct. rel. to baseline)	3.468**	1.190	0.272**	0.0878
	(0.901)	(1.150)	(0.0721)	(0.0896)
Firm's wage bill excl. paid leave (pct. rel. to baseline)	-1.392	0.342	-0.139	0.0195
	(0.926)	(1.158)	(0.0730)	(0.0904)
Firm's total variable cost (pct. rel. to baseline)	-0.762	-0.698	-0.0793	-0.0255
	(1.021)	(1.395)	(0.0823)	(0.101)
F-stat	2,194	2,194	2,294	2,294
Observations	31,908	31,908	31,908	31,908
Observations (weighted)	19,868	19,868	19,868	19,868
Clusters (firms)	16,080	16,080	16,080	16,080
B) Coworker outcomes				
Coworkers' earnings (pct. rel. to baseline)	1.117**	0.624	0.134**	0.0865
	(0.387)	(0.449)	(0.0441)	(0.0505)
F-stat	959.4	963.9	3,005	2,988
Observations	268,403	267,213	268,403	267,213
Observations (weighted)	168,281	167,522	168,281	167,522
Clusters (firms)	15,405	15,401	15,406	15,401

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the number of births at the firm in the event year is instrumented by the treatment dummy. Columns (3) and (4) show estimates from similar regressions but in which both the number of births at the firm in the event year and the treatment dummy is divided by the number of baseline employees (measured in hundreds), and where dummy variables for each possible number of baseline employees are included as controls. In Columns (1) and (3) the outcome variable is measured in the event year (Time 0). In Columns (2) and (4) the outcome variable is measured in the following year (Time 1). Panel A uses firm-level data, while Panel B uses coworker-level data. In Panel B, the number of observations changes between columns because a small number of coworkers drop out of the original administrative data after the event year. Throughout, the analysis is conducted on the matched and reweighted samples. For each panel and column, the F-stat from the first stage regression is listed. Standard errors (in parentheses) are clustered at the firm level. ** p <0.01 * p <0.05.

Table 7: Effects on firms' overall performance and coworkers' sick days, 2SLS

	Absolute effect Effect of one additional birth		*Relative effect* Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
A) Firm outcomes				
Firm sales (pct. rel. to baseline)	-0.680	-0.662	0.0264	0.0401
	(1.276)	(1.559)	(0.103)	(0.115)
Gross profits (1000 DKKs)	-104.5	-249.2	0.549	-1.767
	(217.4)	(246.6)	(6.083)	(6.792)
Nonzero sales	0.00301	0.00604	0.000213	0.000476
	(0.00487)	(0.00621)	(0.000374)	(0.000468)
F-stat	2,194	2,194	2,294	2,294
Observations	31,908	31,908	31,908	31,908
Observations (weighted)	19,868	19,868	19,868	19,868
Clusters (firms)	16,080	16,080	16,080	16,080
B) Coworker outcomes				
Coworkers, any sick days	0.0619	0.0805	0.0170	0.0289
	(0.204)	(0.215)	(0.0252)	(0.0284)
F-stat	959.4	963.9	3,005	2,988
Observations	268,403	267,213	268,403	267,213
Observations (weighted)	168,281	167,522	168,416	167,522
Clusters (firms)	15,405	15,401	15,405	15,401

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the number of births at the firm in the event year is instrumented by the treatment dummy. Columns (3) and (4) show estimates from similar regressions but in which both the number of births at the firm in the event year and the treatment dummy is divided by the number of baseline employees (measured in hundreds), and where dummy variables for each possible number of baseline employees are included as controls. In Columns (1) and (3) the outcome variable is measured in the event year (Time 0). In Columns (2) and (4) the outcome variable is measured in the following year (Time 1). Panel A uses firm-level data, while Panel B uses coworker-level data. In Panel B, the number of observations changes between columns because a small number of coworkers drop out of the original administrative data after the event year. Throughout, the analysis is conducted on the matched and reweighted samples. For each panel and column, the F-stat from the first stage regression is listed. Standard errors (in parentheses) are clustered at the firm level. ** p <0.01 * p <0.05.

Table 8: Effects on outcomes of coworkers in same and different occupations as women on leave, 2SLS

	Absolute effect Effect of one additional birth		*Relative effect* Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
A) Same-occupation coworkers				
Coworker with baseline firm	0.00943	0.00841	0.00132	0.000728
	(0.00816)	(0.00854)	(0.000700)	(0.000724)
Share of year unemployed	−0.00334**	−0.00331*	-0.000252	−0.000279*
	(0.00127)	(0.00131)	(0.000134)	(0.000141)
Hours at baseline firm (pct. rel. to baseline)	1.632**	1.074*	0.168**	0.115*
	(0.444)	(0.469)	(0.0471)	(0.0507)
Earnings (pct. rel. to baseline)	2.451**	1.890**	0.270**	0.209**
	(0.563)	(0.639)	(0.0607)	(0.0700)
Any sick days	0.0968	0.142	0.0335	0.0192
	(0.308)	(0.344)	(0.0358)	(0.0434)
F-stat	640.8	644.7	1,802	1,787
Observations	121,470	120,951	121,470	120,951
Observations (weighted)	76,048	76,048	76,153	75,716
Clusters (firms)	12,526	12,509	12,526	12,509
B) Different-occupation coworkers				
Coworker with baseline firm	0.00941	0.00762	0.00120	0.000770
	(0.00723)	(0.00789)	(0.000726)	(0.000768)
Share of year unemployed	−0.00142	−0.00222	−0.000185	−0.000287
	(0.00112)	(0.00118)	(0.000136)	(0.000148)
Hours at baseline firm (pct. rel. to baseline)	0.298	−0.288	0.0510	−0.0287
	(0.369)	(0.402)	(0.0455)	(0.0488)
Earnings (pct. rel. to baseline)	0.116	−0.264	0.00702	−0.0229
	(0.487)	(0.578)	(0.0603)	(0.0698)
Any sick days	−0.00596	0.0149	−0.00881	0.0319
	(0.270)	(0.270)	(0.0353)	(0.0364)
F-stat	772.6	775.1	2,147	2,120
Observations	145,586	144,920	145,586	144,920
Observations (weighted)	91,391	90,966	91,391	90,966
Clusters (firms)	13,059	13,053	13,059	13,053

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the number of births at the firm in the event year is instrumented by the treatment dummy. Columns (3) and (4) show estimates from similar regressions but in which both the number of births at the firm in the event year and the treatment dummy is divided by the number of baseline employees (measured in hundreds), and where dummy variables for each possible number of baseline employees are included as controls. In Columns (1) and (3) the outcome variable is measured in the event year (Time 0). In Columns (2) and (4) the outcome variable is measured in the following year (Time 1). Both panels use coworker-level data. Panel A shows effects for coworkers who are in the same-(1-digit) occupation as women on leave; while Panel B shows the effects for different-occupation coworkers. The number of observations changes between columns because a small number of coworkers drop out of the original administrative data after the event year. Throughout, the analysis is conducted on the matched and reweighted samples. For each panel and column, the F-stat from the first stage regression is listed. Standard errors (in parentheses) are clustered at the firm level. ** p <0.01 * p <0.05.

Table 9: Effects on outcomes of no replacement firms, 2SLS

	Absolute effect Effect of one additional birth		*Relative effect* Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
A) Labor inputs				
Parental leave days at firm	184.8**	62.48**	8.174**	2.763**
	(12.00)	(11.36)	(0.445)	(0.472)
Number of employees at firm (pct. rel. to baseline)	2.707	-2.219	0.285	-0.0325
	(3.527)	(4.115)	(0.195)	(0.223)
New hires at firm	0.123	0.00275	0.0117*	-0.00183
	(0.176)	(0.190)	(0.00589)	(0.00588)
Turnover at firm	-0.608**	-0.406	-0.0193**	-0.00497
	(0.213)	(0.224)	(0.00702)	(0.00750)
Hours at firm (pct. rel. to baseline)	-7.178*	-5.204	-0.332	-0.195
	(3.309)	(3.899)	(0.175)	(0.202)
B) Labor costs				
Firm's wage bill (pct. rel. to baseline)	-1.532	-3.792	-0.0677	-0.191
	(3.371)	(4.247)	(0.180)	(0.218)
Firm's wage bill excl. paid leave (pct. rel. to baseline)	-8.379*	-4.806	-0.453*	-0.243
	(3.456)	(4.247)	(0.181)	(0.218)
C) Overall performance				
Firm sales (pct. rel. to baseline)	-5.085	-4.148	-0.173	-0.243
	(5.732)	(6.154)	(0.297)	(0.315)
Gross profits (1000 DKKs)	-486.9	-197.1	-8.101	-0.931
	(493.1)	(421.7)	(9.420)	(9.622)
Nonzero sales	-0.00863	-0.00396	-0.000783	-0.000647
	(0.0172)	(0.0214)	(0.000925)	(0.00114)
F-stat	306.5	306.5	306.5	306.5
Observations	3,191	3,191	3,191	3,191
Observations (weighted)	2,010	2,010	2,010	2,010
Clusters (firms)	2,718	2,718	2,718	2,718

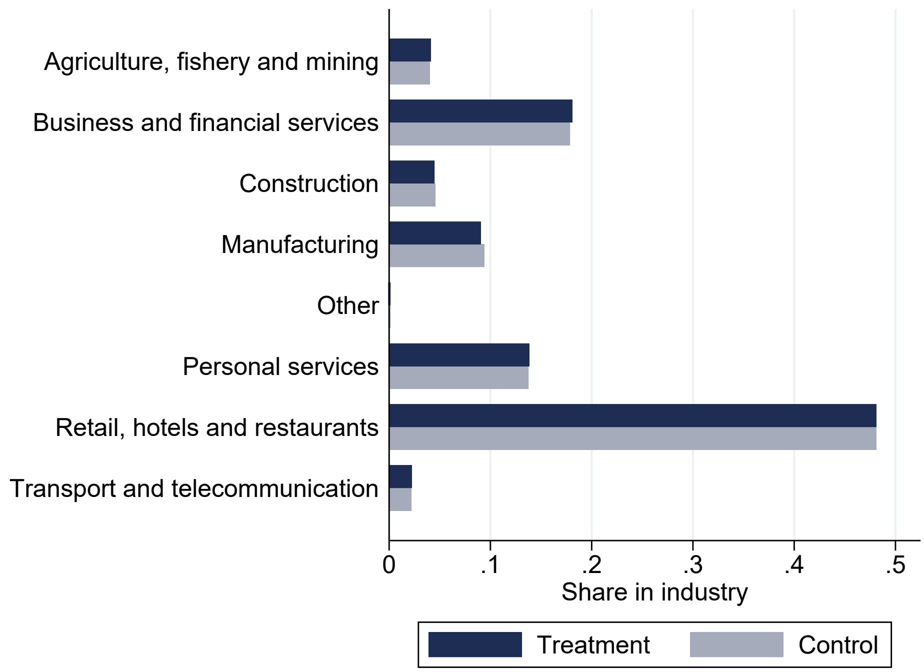
Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the number of births at the firm in the event year is instrumented by the treatment dummy. Columns (3) and (4) show estimates from similar regressions but in which both the number of births at the firm in the event year and the treatment dummy is divided by the number of baseline employees (measured in hundreds), and where dummy variables for each possible number of baseline employees are included as controls. In Columns (1) and (3) the outcome variable is measured in the event year (Time 0). In Columns (2) and (4) the outcome variable is measured in the following year (Time 1). All panels use firm-level data, and show effects for firms which do not employ other workers in the same 1-digit occupation as the woman on leave at baseline. Throughout, the analysis is conducted on the matched and reweighted samples. For each panel and column, the F-stat from the first stage regression is listed. Standard errors (in parentheses) are clustered at the firm level. ** $p < 0.01$ * $p < 0.05$.

WEB ONLY APPENDIX
Brenøe, Canaan, Harmon, Royer
"Is Parental Leave Costly for Firms and Coworkers?" *JOLE*

Appendices

A Supplementary Tables and Figures

Figure A1: Industry composition of treatment and control samples



The figure shows the industrial composition of the matched and reweighted treatment and control samples across one-digit industries. Because it contains a very small number of firms, the category "Electricity and water supply" has been lumped into the "Other" category for reasons of data confidentiality. Industries in the figure are ordered according to the number of firms in the treatment group. The differences in industry distribution across the two samples are not statistically significant ($p = 0.92$)

Table A1: Maternity and parental leaves across countries

Country	Maternity leave length (in weeks)	Amount of benefits (% of previous earnings)	Parental leave length (in weeks)	Amount of benefits (% of previous earnings)	Source of funding
Austria[(a)]	16	100%	156	flat rate	social insurance
Canada[(b)]	17 (federal)	55% for 15 weeks up to a ceiling	35	55% up to a ceiling	social insurance
Denmark(c)	18	100% up to a ceiling	32	100% up to a ceiling	social insurance + mandatory employer insurance
Finland[(d)]	17.5	between 30% and 70%	26	between 30% and 70%	social insurance
France	16	100% up to a ceiling	156	flat rate for 26 weeks for first child	social insurance
Germany	14	100% up to a ceiling	156	partially paid for 12 months	social insurance + employers
Italy	22	80	26	30	social insurance
Norway[(e)]	14		49 or 59	100% if 49 weeks, 80% if 59 weeks	social insurance
Spain	16	100%	156	unpaid	social insurance
Sweden	14	80%	80	80% for 65 weeks, flat rate for 15 weeks	social insurance
Switzerland	14	80% up to a ceiling	–	–	social security + mandatory private insurance
United Kingdom	52	90% for 6 weeks; flat rate weeks 7–39	13	unpaid	public funds reimburse employers for up to 92%
United States	–	–	12 (federal)	unpaid	–

Notes: This table shows the duration and amount of cash benefits awarded under statutory maternity and parental leave programs in 2014 across several countries. Maternity leave length includes both pre-birth and post-birth leaves. (a) Parental leave is 36 months in Austria; however, six months are earmarked to each parent and fathers typically do not take any leave; the job protection period is only 24 months (Lalive *et al.*, 2013). (b) Parents in Canada are eligible for a total of 52 weeks of maternity and parental leaves, with 50 weeks paid by social insurance at 55% of previous earnings and 2 weeks unpaid. (c) For Denmark, see Table 1 and Section 3 for further details. (d) In Finland, maternity and parental leave benefits are calculated using a sliding scale based on workers' usual salary; middle-income workers receive around 2/3 of their pre-leave wages, while other workers receive between 30 and 70%. (e) In Norway, fourteen weeks of parental leave are reserved exclusively for mothers and another fourteen weeks for fathers. Sources: (Ray *et al.*, 2008) and (Addati *et al.*, 2014).

Table A2: Summary statistics of the firm and coworker samples, all seven years

	Observations (unweighted)	Mean	Standard Deviation
Panel A - Firm sample			
Births at firm	220,879	0.725	1.073
Pregnancies at firm	220,879	1.341	1.662
Leave days at firm	220,879	127.0	198.6
Employees	220,879	11.76	9.609
New hires	217,156	3.633	4.164
Turnover at firm	217,156	3.777	4.441
Wage bill (1000 DKKs)	220,879	3,168	3,480
Sales (1000 DKKs)	209,582	17,639	41,431
Purchases (1000 DKKs)	209,582	12,060	34,176
Gross profits (1000 DKKs)	209,582	2,389	16,429
Workforce share women	200,913	0.632	0.288
Workforce avg. age	200,913	34.56	6.898
Workforce avg. years schooling	200,913	11.63	1.338
Workforce avg. years education	200,913	12.83	5.686
Panel B - Coworker sample			
Coworker still with baseline firm	1,858,327	0.691	0.462
Coworker unemployment (yearly share)	1,858,327	0.0264	0.104
Coworker hours (FTEs)	1,858,327	0.801	0.320
Coworker earnings (1000 DKKs)	1,858,327	280.8	217.7

Notes: The table shows summary statistics for the matched firm (Panel A) and coworker (Panel B) samples for all the years used in the analysis (from four years prior to the event year and until two years after the event year). Means and standard deviations are computed with weights. The total number of observations shown is unweighted.

Table A3: Covariate balance at baseline

	Treatment	Control	Difference	p-Value
Births	0.80	0.78	0.02	0.19
	(1.07)	(1.03)	(0.01)	
Leave days at firm	139.45	135.41	4.04	0.14
	(197.82)	(193.51)	(2.74)	
New hires	3.70	3.73	−0.03	0.55
	(3.30)	(3.24)	(0.04)	
Hours (FTEs)	10.61	10.59	0.02	0.84
	(7.31)	(7.28)	(0.10)	
Workforce avg. years schooling	11.62	11.62	0.00	0.84
	(1.28)	(1.28)	(0.02)	
Workforce avg. age	33.78	33.84	−0.06	0.51
	(6.34)	(6.39)	(0.09)	
Workforce avg. experience	12.24	12.26	−0.02	0.75
	(5.25)	(5.29)	(0.07)	
Wage bill (1000 DKKs)	3360.20	3379.01	−18.82	0.63
	(2991.48)	(3004.40)	(39.56)	
Purchases (1000 DKKs)	12604.45	12526.48	78.01	0.87
	(34419.62)	(31190.92)	(467.13)	
Profits (1000 DKKs)	9166.96	8830.14	336.83	0.39
	(28512.69)	(27921.00)	(394.58)	
Profits ex leave	-224132.82	-223820.91	-311.91	0.82
	(101166.61)	(100130.35)	(1394.44)	
Event year	2007.08	2007.08	−0.00	0.96
	(2.82)	(2.85)	(0.04)	

Notes: The table shows means and standard deviations for the firm- and event-specific variables in the baseline year across the matched and reweighted sample of treatment and control events. The table also shows the difference in means between the two samples along with the standard error of this difference computed based on clustering at the firm level. ** $p < 0.01$ * $p < 0.05$.

Table A4: Effects on firm outcomes based on whether women on leave have no replacement, 2SLS

	Parental leave days at firm	Number of employees at firm (pct. rel. to baseline)	New hires at firm	Turnover at firm	Hours at firm (pct. rel. to to baseline)	Firm's wage bill (pct. rel. to to baseline)	Firm's wage bill excl. paid leave (pct rel. to baseline)	Firm sales (pct. rel. to to baseline)	Gross profits (1000 DKKs)	Nonzero sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A) Absolute effect										
at t=0										
Effect of one additional birth	196.7**	7.738**	0.375**	-0.225*	0.433	4.011**	-0.632	-0.190	-70.35	0.00430
	(5.122)	(0.952)	(0.0740)	(0.0916)	(0.922)	(0.929)	(0.955)	(1.273)	(230.9)	(0.00507)
Effect of one additional birth × no replacement	-12.33	-5.098	-0.251	-0.369	-7.757*	-5.606	-7.818*	-5.047	-351.7	-0.0132
	(13.05)	(3.657)	(0.192)	(0.232)	(3.444)	(3.499)	(3.590)	(5.888)	(529.0)	(0.0180)
at t=1										
Effect of one additional birth	88.84**	1.510	-0.165*	0.445**	1.268	1.738	0.908	-0.271	-242.8	0.00717
	(4.897)	(1.159)	(0.0820)	(0.0975)	(1.122)	(1.188)	(1.197)	(1.597)	(263.7)	(0.00649)
Effect of one additional birth × no replacement	-26.48*	-3.935	0.160	-0.840***	-6.765	-5.648	-5.835	-4.044	57.69	-0.0116
	(12.40)	(4.277)	(0.207)	(0.244)	(4.068)	(4.417)	(4.419)	(6.379)	(469.1)	(0.0224)
B) Relative effect										
at t=0										
Effect of one additional birth per 100 employees	11.70**	0.700**	0.0244**	-0.0100**	0.0148	0.345**	-0.0719	0.0703	1.999	0.000441
	(0.300)	(0.0850)	(0.00321)	(0.00375)	(0.0777)	(0.0782)	(0.0794)	(0.109)	(7.034)	(0.000409)
Effect of one additional birth per 100 employees × no replacement	-3.544**	-0.415*	-0.0126	-0.00905	-0.353	-0.413*	-0.383*	-0.251	-7.177	-0.00124
	(0.541)	(0.212)	(0.00672)	(0.00798)	(0.191)	(0.196)	(0.197)	(0.317)	(11.68)	(0.00102)
at t=1										
Effect of one additional birth per 100 employees	5.352**	0.167	-0.00405	0.0295**	0.106	0.149	0.0773	0.105	-2.224	0.000737
	(0.283)	(0.103)	(0.00349)	(0.00410)	(0.0944)	(0.0979)	(0.0988)	(0.122)	(7.920)	(0.000512)
Effect of one additional birth per 100 employees ×no replacement	-2.596**	-0.208	0.00187	-0.0340**	-0.313	-0.345	-0.360	-0.315	1.696	-0.00141
	(0.553)	(0.245)	(0.00685)	(0.00856)	(0.223)	(0.239)	(0.240)	(0.339)	(12.38)	(0.00126)
Observations	31,908	31,908	31,908	31,908	31,908	31,908	31,908	31,908	31,908	31,908
Observations (weighted)	19,868	19,868	19,868	19,868	19,868	19,868	19,868	19,868	19,868	19,868
Clusters (firms)	16,080	16,080	16,080	16,080	16,080	16,080	16,080	16,080	16,080	16,080

Notes: Each column-row represents the coefficient from a separate regression. Columns refer to different outcome variables. In each regression, the outcome is the change in the relevant outcome between baseline and either the event year ($t = 0$) or the year after ($t = 1$). In Panel A, the regressors of interest are the number of births at the firm in the event year (Effect of one additional birth) and the interaction between the number of births at the firm in the event year and an indicator variable for whether the firm does not employ other workers in the same 1-digit occupation as the woman on leave at baseline (Effect of one additional birth X no replacement). In addition, an indicator for does not employ other workers in the same 1-digit occupation as the woman on leave at baseline is included in the regression as a control. The regression is estimated by 2SLS using treatment status and its interaction with the indicator variable for whether the firm does not employ other workers in the same 1-digit occupation as the woman on leave at baseline as instruments. In Panel B, the regressors of interest are the number of births at the firm in the event year divided by the number of employees at baseline (Effect of one additional birth per 100 employees), and the interaction between the number of births in the event year divided by the number of employees at baseline and an indicator variable for whether the firm does not employ other workers in the same 1-digit occupation as the woman on leave at baseline (Effect of one additional birth per 100 employees X no replacement). In addition, the indicator variable for whether the firm does not employ other workers in the same 1-digit occupation as the woman on leave at baseline is included in the regression as a control along with a full set of dummy variables for each possible number of baseline employees. The regression is estimated by 2SLS using treatment status divided by the number of employees at baseline and its interaction with the indicator variable for whether the firm does not employ other workers in the same 1-digit occupation as the woman on leave at baseline as instruments. Throughout, the analysis is conducted on the matched and reweighted samples. Standard errors (in parentheses) are clustered at the firm level. ** $p < 0.01$ * $p < 0.05$.

B Institutional Setting: Further Details

This appendix supplements Section 3 with further details about the parental leave policies in Denmark.

The exact requirements for parental leave eligibility have changed somewhat over the years but have been low throughout. Under current rules, for example, working ten hours a week for the past three months is sufficient to qualify for leave. All pregnant women are eligible for job-protected prenatal leave of four weeks with wage replacement. On top of that, women working in particular jobs (typically physically demanding labor), are eligible for an additional four weeks of leave before birth. Moreover, a woman unable to work due to her pregnancy has a right to her full salary, for which the UI system fully compensates the employer.

In addition to mothers' 14 weeks immediately after birth, fathers are eligible for two weeks of parental leave within the first 14 weeks after birth; the norm is to take these two weeks right after birth. Although, parents can freely share the following 32 weeks of parental leave, mothers typically take the majority. Fathers take only about 10 percent of the shared leave on average. The leave policy offers various possibilities for postponing part of the leave period until later in the child's life and for extending the job-protected leave without wage replacement. These possibilities are less important in practice, so in this paper, we focus on leave periods with wage replacement that occur immediately after the child is born.

The wage replacement offered during the leave depends on the details of the worker's employment contract. At a minimum, all women are eligible to receive government-provided wage replacement equal to the maximum level of UI benefits during the entire 50 weeks of leave. The wage replacement amount cannot exceed the woman's previous wage, so for the small number of women earning less than the maximum UI level, this is just equivalent to a full wage replacement. When the woman receives the government-provided UI benefit, we refer to this as *unpaid leave*, as firms do not incur any direct costs. However, most employment contracts in Denmark offer some period of fully paid leave during which the employer simply continues to pay the worker her wage. We refer to these periods of employer-paid leave as *paid leave*, as the worker continues to receive wage payments from the firm.

Employment contracts offering paid leave are encouraged under the Danish parental leave policy. This is done by directly reimbursing firms for wages paid to workers on leave in two ways. First, when an employee is on paid leave, the employing firm receives the government-provided wage replacement that the worker would otherwise have received. Second, firms paying wages to workers on leave are also eligible for reimbursement from one of several "parental leave funds" to which all employers contribute. Firms are required to pay into a parental leave fund for all employees regardless of gender and age. In 2017, there were 12 approved private parental leave funds and one public. The public parental leave fund covers the vast majority of private firms. According to our calculations, around 95 percent of all private firms are member of the public fund with the remaining firms being member of one of the private funds. Prior to 2006, employers could voluntarily join these funds to replace workers' wages. Since 2006, membership in a parental leave fund has been mandatory for all employers. As most of our analysis examines a balanced panel covering births from 2005 to 2011, the vast majority of births in our sample occur when membership of the parental leave funds was mandatory.

Roughly three-quarters of firms have a collective bargaining agreement. Mothers under this agreement are paid full wage during the first fourteen weeks of leave after delivery; fathers are eligible to take two weeks of leave with similar compensation rules as mothers' leave during this period. In addition, parents under collective bargaining agreements have five weeks each plus three weeks with full wages that they can split as they wish. Regardless of being under a collective bargaining agreement, the parental leave funds reimburse 2 weeks to the mother following birth, 2 weeks to the father following birth, and 25 weeks to the parents collectively, which the parents can split as they wish. The parental leave funds top up on the hourly wage paid by the employer from the UI level up to a maximum hourly wage in case the employer pays the employee a wage that is higher than the UI level.

Exact rules and reimbursement amounts differ depending on the specific fund and the terms of the woman's employment contract. However, firms recoup almost all the wages paid to workers on leave in the majority of cases. As an example, in 2010–2011, the maximum total hourly reimbursement that firms could receive from combining amounts from the UI system and the public parental leave fund was 149 DKK (20 EUR or 25 USD), of which the UI represented 103 DKK (14 EUR or 17

USD). Here, it is worth noting that the firm only needs to apply for reimbursement through the UI system, which automatically informs the parental leave fund, reducing the administrative burden for firms. Based on the treatment firms and women in our estimation sample, we compute that firms in our data are reimbursed for more than 90 percent of the paid leave for the average woman going on leave. In addition, firms are eligible for full reimbursement for all the paid leave for 49 percent of the women. The law requires that the reimbursement rate and duration of reimbursement serve as a minimum for private parental leave funds. In practice, the private funds reimburse at the same level or slightly higher hourly wage rates (only with a few DKK per hour) and of the same duration. Firms selecting a private parental leave fund will typically chose the one that is run by their employer union.

C Data Appendix

Subsection C.1 of this appendix supplements the Data Section 4 in the main paper with additional details on the data and how we construct our measures. Subsection C.2 adds further details on our exact sample restrictions explained in Subsections 5.2 and 5.4.

C.1 Data Construction

Worker Data Our linked administrative data yield a range of characteristics and outcomes for workers. We obtain basic demographic information, such as age and gender, from the CPR. Using parent-child linkages and information on birth dates, we further construct data on when workers give birth, as well as the number of children each worker has. We use data on the payout of parental leave benefits to individuals and the payout of leave reimbursements to firms to calculate the total number of days of paid and unpaid leave for each worker. For each birth, we calculate the number of prenatal and postnatal leave days based on the UI rate and allocate the number of leave days around childbirth, assuming that the woman takes all the prenatal leave uninterrupted right before childbirth and all the postnatal leave uninterrupted in the first year starting right after childbirth. In our measure of prenatal leave, we include instances when the leave period is extended because of health issues related to pregnancy (see Section 3). In the case of outliers, we truncate the length of any prenatal leave at 38 weeks, paid prenatal leave at 6 weeks, paid postnatal leave at 52 weeks, and any postnatal leave at 104 weeks. Finally, using data from the central education register and IDA, we obtain detailed measures of workers' education and their total labor market experience since labor market entry.

Matched Employer-Employee Data Historically, the IDA data were designed to most accurately capture employment at the end of the last week of November. The results we present are virtually identical if we do not use November employees but instead include all workers who were ever at the firm during the year. We define the main job as the job with the most hours, and in the case of any equal amounts, that with the highest earnings. Importantly, because workers on leave are not registered as leaving their current job in these data, there is no mechanical effect of leave-taking on our measures of turnover and new hires.

For the construction of our measure of total hours supplied to a firm, we rely on data on mandatory pension contributions from firms (ATP). Firms make these pension contributions for each week an employee works at the firm and the contribution per week scales approximately linearly with hours. Appropriately scaling the contribution amount therefore gives us an approximate measure of total hours supplied during the year (Lund & Vejlin, 2016), which we use in our analysis. More precisely, the ATP contribution schedule has fours steps for 0–9, 9–17, 18–26, and 27– hours per week and therefore tops out for individuals working full time (37 hours a week), so any overtime work undertaken by full-time employees will be missed in our hours measure. This implies that our analysis of yearly hours will fail to capture changes in overtime work for full-time employees as well as smaller changes in weekly hours that do not cross one of the thresholds in the step function. However, our measure will very precisely capture changes in the share of the year working (for example, due to parental leave absences) as well as changes between part-time and full-time work. In the discussion of the results, we return to the potential biases of our estimates on total hours when interpreting the results. To correct for the fact that ATP contributions continue while employees are on paid leave, we subtract the share of the year that each employee is on paid parental leave.

To the extent that parental leave causes firms to rely more on overtime work for existing full-time employees or make smaller hours adjustments for part-time employees, our analysis may thus understate the effect of parental leave on coworker hours. On the other hand, because parental leave negatively affects total hours at the firm primarily through a reduction in the share of the year the mother works—which our hours measure captures very accurately—our analysis will tend to overstate the negative effect of parental leave on total hours.

For our alternative measure of the *wage bill ex. leave*, we divide each worker's total payments from the firm by the total hours worked including paid leave (based on ATP contributions) to get wages. We then multiply their wage by their number of hours worked excluding periods of paid leave (based on ATP contributions and total days on paid leave). The gap between the workers' total payments from the firm and the earnings from labor hours is a measure of the paid leave that the firms have covered. This gap is then subtracted from the total wage bill to arrive at a measure of total wage bill excluding leave payments. Data limitations prohibit us from examining the actual reimbursements firms receive. Specifically, we do not have data on reimbursements received from parental leave funds (see Section 3).

Firm Data Given the nature of the Danish firm identifier, CVR, we can distinguish between different firms, but not between different establishments of the same firm. Nonetheless, our analysis sample includes mostly single-establishment firms since our focus is on small firms (as further discussed in Section 5). Participation in the CVR registry is required for all firms with a yearly revenue above a certain threshold. As of 2018, this value is 50,000 DKK (6,700 EUR or 7,500 USD), but it was even smaller during our sample period. With the exception of exports, the Danish VAT is almost universal. The sales and purchases data we use in the analysis have been corrected to include export data.

For our variables on total sales and firm purchases, there are a few instances of firms reporting negative sales and/or purchases (less than 0.2 percent) due to reporting errors and issues around accounting corrections. We recode these as zeros.

As mentioned in the main text, our measure of gross profits does not include purchases of capital equipment. Normally, capital purchases only affect net profits because these include capital depreciation. If firms in our sample respond to employee leave-taking by systematically increasing investments, this will understate gross profits. Accounting data that separate investments from material costs and other inputs are not available for most small firms of our analysis.

Using other definitions of firm activity does not affect the qualitative conclusions of the paper.

C.2 Sample Restrictions

Women The restriction that the women are between 19 and 33 years in the baseline year implies that they are aged 21 through 35 at the time of childbirth. These ages at childbirth account for 83 percent of all childbirths in Denmark over our sample period. We restrict the sample to women with strong attachment to their employer and to the labor market. For this restriction, we require that the woman has tenure at her baseline firm for at least a year and is not a student. Student workers in Denmark largely consist of de facto paid internships that take place simultaneously with classes. Including student jobs in the analysis would lower generalizability to other countries where this type of work arrangement is less common. Student workers and newly hired workers have a much weaker attachment to their employer than regular employees. This is problematic in our research design because turnover creates imperfect compliance. Therefore, we decide not to include these workers in the analysis.

Firms For sample restriction 4 in Subsection 5.2 (the firm is active at baseline), we require that total hours in the baseline year correspond to at least one full-time employee, that the firm had positive sales and positive wage payments in the the baseline year, and that the firm either had positive sales or positive wage payments in the year prior to the baseline year. For restriction 5 (the firm is no outlier), we more precisely exclude firms with outlier sales or wage bills relative to their employment. Specifically, sales per employee must be between 10,000 DKK (1,300 EUR or 1,500 USD) and 100 million DKK (thirteen million EUR or fifteen million USD), and wages per worker must be between 10,000 DKK (1,300 EUR or 1,500 USD) and one million DKK (130,000 EUR or 150,000 USD). For restriction 6 (small firm), we restrict the stock of employees to be between 3 and 30, which is based on the workforce in November. We further require that the total number of employment relationships observed at the firm at some point in the baseline year is less than 60. This additional restriction on total employees throughout the year deals with highly seasonal firms that only employ a smaller fraction of their work force in November. We need restriction 7 (firm is in the private sector), as the majority of public sector output will not show up in sales data. Moreover, all public sector workplaces under the same public entity (a municipality, for example) are generally assigned a single firm identifier in our data. We thus have no reliable way of looking at firm closure or identifying true coworkers.

Coworkers As described in Subsection 5.4, we restrict coworkers to have their main employment at the firm, work at least what substitutes half a full time employee, and have earnings above a minimum. We make these restrictions to confine the sample to those coworkers with a relatively strong attachment to the firm at the baseline. There are only five events, for which we do not have relevant coworkers; we drop these firms from the coworker analysis. Because this sample definition only conditions on where coworkers are employed in the baseline year, our coworker analysis will include workers who leave treatment and control firms after baseline. This is appropriate as exit from the firm is an endogenous outcome of interest. For the same reasons, the coworker analysis does not examine the outcomes of workers who join treatment and control firms after the baseline year.

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D Effects on Coworker Fertility and Leave-Taking

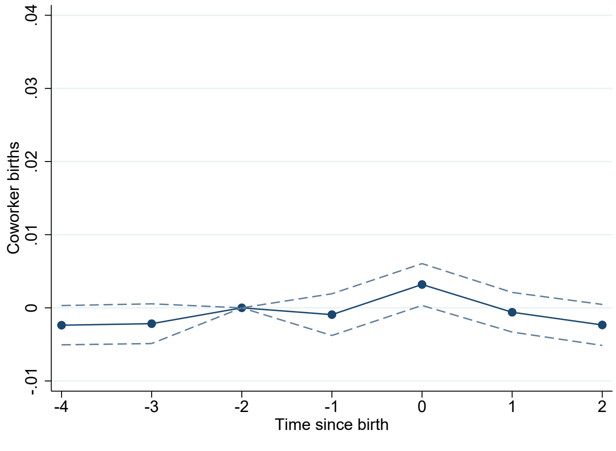
A parallel literature (e.g., Asphjell *et al.*, 2013; Ciliberto *et al.*, 2016) shows the existence of workplace peer effects in the incidence and timing of pregnancy and parental leave. For example, Asphjell *et al.* (2013) find that the likelihood that an individual in Swedish firms has a first child increases by 9 percent 13 to 24 months after a coworker's child is born. In our setting, the interpretation of our main results could potentially change if a woman's leave-taking increases the probability that another worker will take leave in the following years. Specifically, these within-firm peer effects might capture the effect of multiple workers going on leave also outside the event year in our estimates.

To investigate the extent of peer effects in our setting, we examine whether a female employee giving birth affects her coworkers' pregnancy and leave take-up. Panels (a) and (b) of Appendix Figure A2 plot OLS estimates of the differences between treated and control firms in coworkers' number of births and parental leave days, respectively. There is a very small positive effect on the number of births in the event year, but not in other years. The corresponding 2SLS estimate, reported in column (1), Panel A of Appendix Table A5, indicates that coworkers have a mere 0.005 additional births in the event year. In the following year, we find no statistically significant effects, and the upper bound of the 95 percent confidence interval is a 0.003 increase in the number of coworker childbirths. We also find no statistically significant impacts on coworkers' parental leave days,[24] and our 95 percent confidence intervals exclude increases that are larger than 1.2 days in both the event year and the following year (column (1) and (2), Panel A of Appendix Table A5). We further show OLS (Panels (c) through (f) of Appendix Figure A2) and 2SLS estimates (Panels B and C of Appendix Table A5) of the treatment effect on these outcomes for coworkers who are respectively in similar occupations and different occupations than employees on leave. These results are similar to the main estimates. Moreover, they are not different for same-occupation versus different-occupation workers.

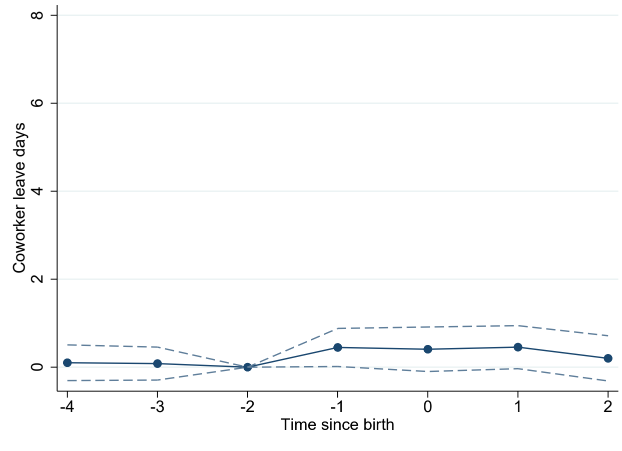
Taken together, these estimates dampen the concern that coworker peer effects could be driving our main results.

[24] The magnitude is consistent with the effect on births.

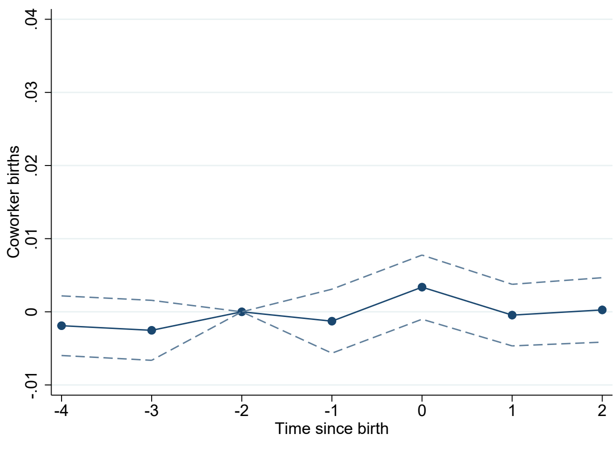
Figure A2: Effects on coworkers' fertility and parental leave days, OLS



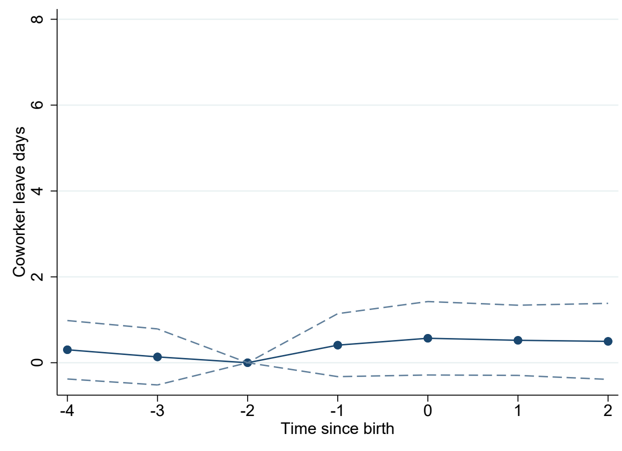
(a) Births, all coworkers



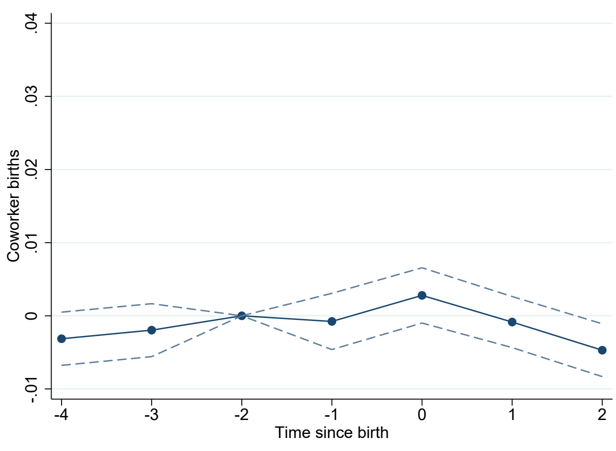
(b) Parental leave days, all coworkers



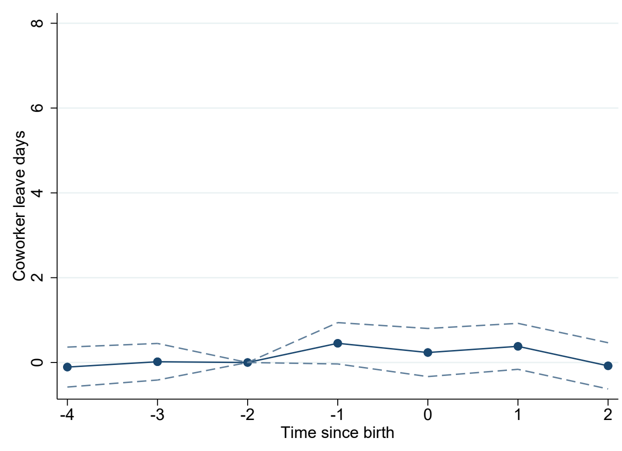
(c) Births, same-occupation coworkers



(d) Parental leave days, same-occupation coworkers



(e) Births, different-occupation coworkers



(f) Parental leave days, different-occupation coworkers

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year; which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Table A5: Effects on fertility and leave days of coworkers of women on leave, 2SLS

	Absolute effect Effect of one additional birth		*Relative effect* Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
A) All coworkers				
Number of births	0.00489**	-0.00011	0.000733**	0.000172
	(0.00178)	(0.00171)	(0.000217)	(0.000206)
Leave days	0.457	0.562	0.00184	0.0754
	(0.321)	(0.309)	(0.0435)	(0.0425)
F-stat	959.4	963.9	3,005	2,988
Observations	268,403	267,213	268,403	267,213
Observations (weighted)	168,281	167,522	168,281	167,522
Clusters (firms)	15,405	15,401	15,405	15,401
B) Same-occupation coworkers				
Number of births	0.00608*	0.160	0.000718*	0.000218
	(0.00284)	(0.00277)	(0.000328)	(0.000311)
Leave days	0.750	0.646	0.0146	0.0967
	(0.560)	(0.538)	(0.0699)	(0.0704)
F-stat	640.8	644.7	1,802	1,787
Observations	121,470	120,951	121,470	120,951
Observations (weighted)	76,048	75,716	76,048	75,716
Clusters (firms)	12,526	12,509	12,526	12,509
C) Different-occupation coworkers				
Number of births	0.00374	−0.000478	0.000691*	0.000098
	(0.00230)	(0.00213)	(0.000291)	(0.000271)
Leave days	0.179	0.438	−0.0203	0.0420
	(0.350)	(0.336)	(0.0512)	(0.0468)
F-stat	771.8	774.2	2,142	2,132
Observations	145,551	144,889	145,551	144,889
Observations (weights)	91,363	90,942	91,363	90,942
Clusters (firms)	13,049	13,043	13,049	13,043

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the number of births at the firm in the event year is instrumented by the treatment dummy. Columns (3) and (4) show estimates from similar regressions but in which both the number of births at the firm in the event year and the treatment dummy is divided by the number of baseline employees (measured in hundreds), and where dummy variables for each possible number of baseline employees are included as controls. In Columns (1) and (3) the outcome variable is measured in the event year (Time 0). In Columns (2) and (4) the outcome variable is measured in the following year (Time 1). All panels use coworker-level data. Panel A shows estimates for all coworkers. Panel B limits the sample to coworkers who are in the same occupation as the woman on leave, while Panel C shows estimates for coworkers in different occupations than the woman on leave. Throughout, the analysis is conducted on the matched and reweighted samples. For each panel and column, the F-stat from the first stage regression is listed. Standard errors (in parentheses) are clustered at the firm level. ** p <0.01 * p <0.05.

E Results Excluding Duplicate Firms

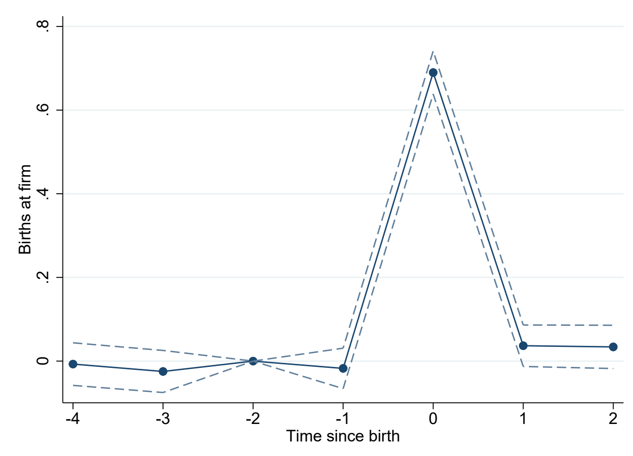
The main analysis sample is defined in terms of potential birth events. As described in the main text, this implies that a single firm may be in the sample several times as part of different treatment and/or control events. Throughout the main analysis, we correct our inference for this duplicity by clustering standard errors at the firm level. In this section, however, we further examine how the results change if we restrict the sample to have no duplicate firms in the sample of events.

After applying our sample restrictions but before matching and reweighing (see Table 3), we first drop all treatment events for which there exists no control with the same value of our conditioning variables. Similarly, we drop all control events for which there is no treatments with the same value of our conditioning variables.[25] Now, for each firm in the sample that is part of more than one event, we only keep the event that occurred first. In other words, if some firm A is part of two events in the sample, where one occurred in 2010 and the other in 2007, we only keep the event occurring in 2007. Finally, if some firm is part of more than one event in the same year, we simply randomly select one of the events. With these restrictions ensuring that each firm is only in the sample as part of one event, we then proceed to apply the same matching and reweighing procedure as in the main analysis. The resulting analysis sample consists of 4,213 treatment events and 7,384 control events.

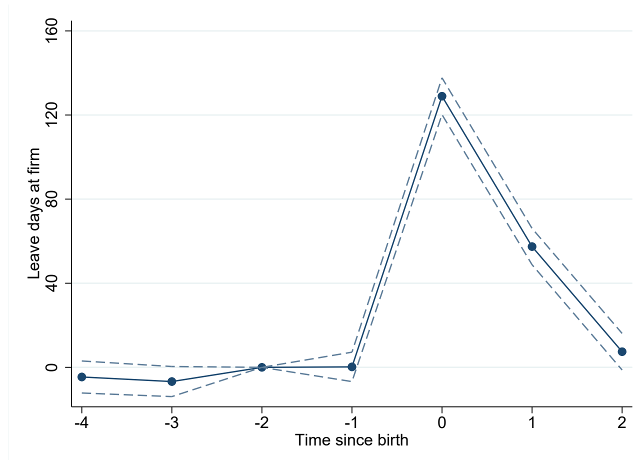
Figures A3a to A7 show OLS estimates from our main difference-in-differences specification for this alternative analysis sample. The results pattern those in the main text. Unsurprisingly, however, the substantially smaller sample results in a loss of power. As a result, confidence intervals are wider for some of the statistically insignificant estimates from the main analysis. Furthermore, a few of the previously statistically significant estimates in the main analysis (in particular coworkers' unemployment risk, work hours and earnings, as well as firms' wage bill) are no longer statistically different from zero at the five percent level in this smaller sample. Despite the standard errors being larger, the point estimates in this reduced sample are quite similar those in the main analysis.

[25] Note that the observations dropped here can never contribute to the analysis after matching and reweighing because they lie outside the common support. Dropping them explicitly here, however, avoid the possibility that when we get rid of duplicate events for the same firm, we accidentally end up keeping unusable observations outside the common support

Figure A3: Estimates for firms total births and parental leave days, excluding duplicates, OLS



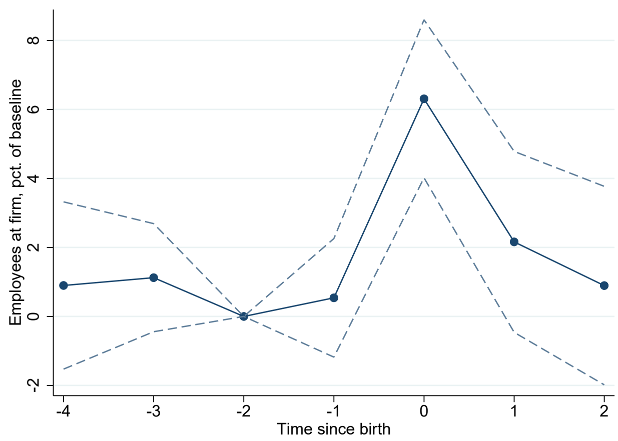
(a) Births at firm



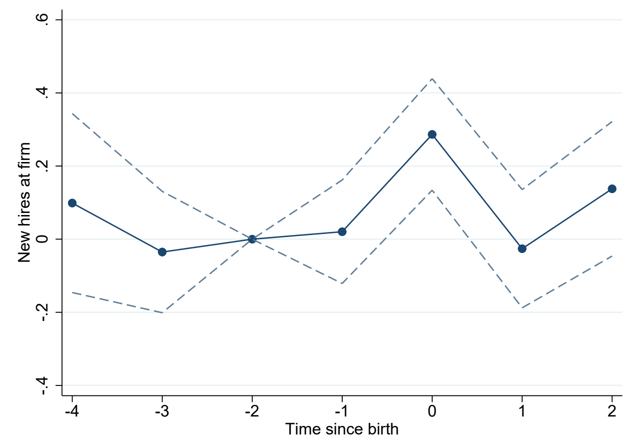
(b) Parental leave days at firm

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, implying that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

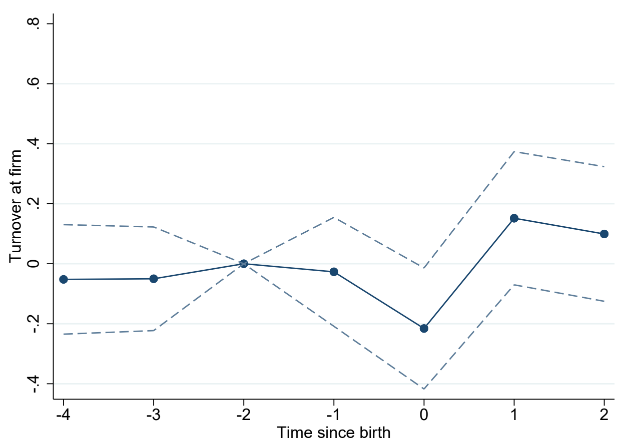
Figure A4: Effects on employment outcomes, excluding duplicates, OLS



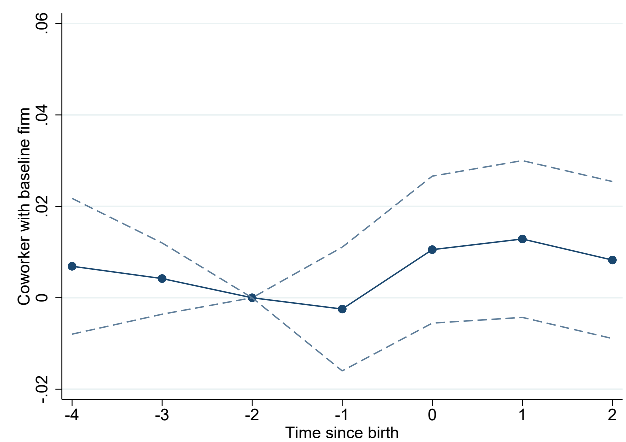
(a) Number of employees at firm



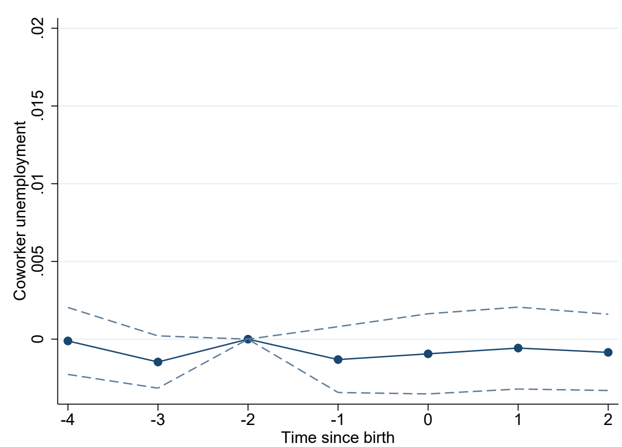
(b) New hires at firm



(c) Turnover at firm



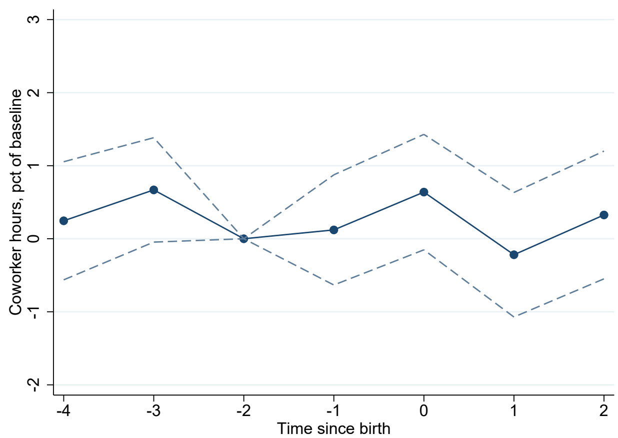
(d) Likelihood coworkers with baseline firm



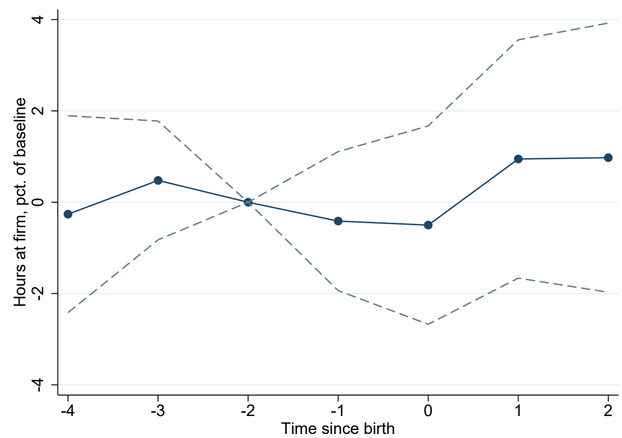
(e) Coworkers' share of year unemployed

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Figure A5: Effects on hours of work, excluding duplicates, OLS



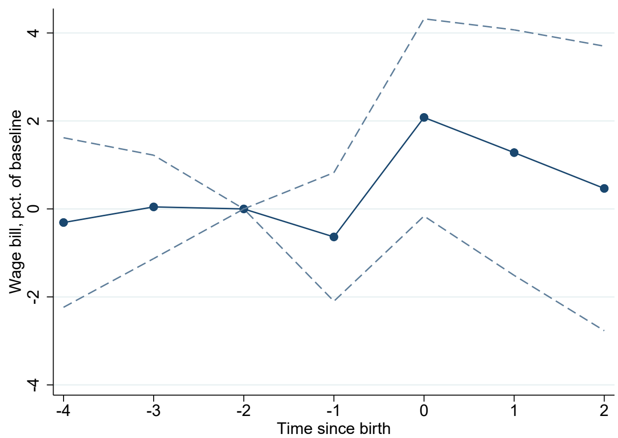
(a) Coworker hours



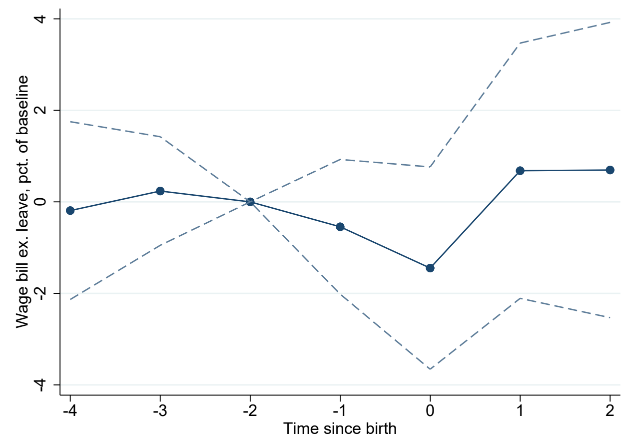
(b) Hours at firm

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

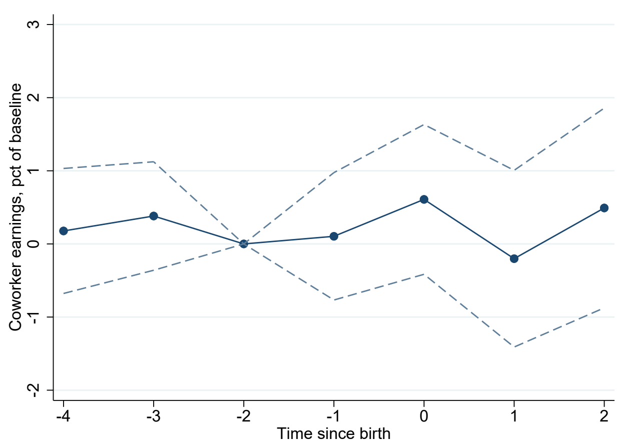
Figure A6: Effects on wage costs and earnings, excluding duplicates, OLS



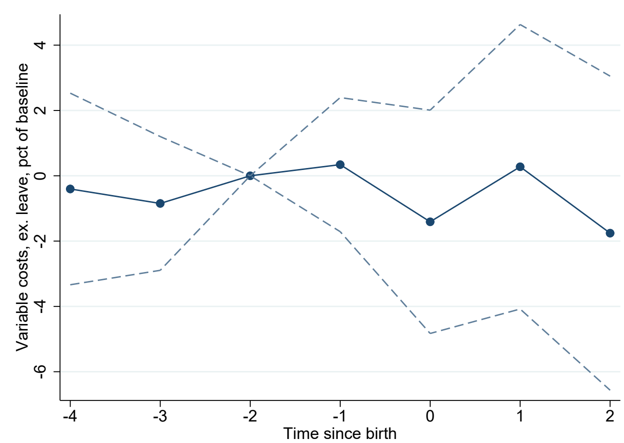
(a) Firms' wage bill



(b) Firms' wage bill (excluding paid leave)



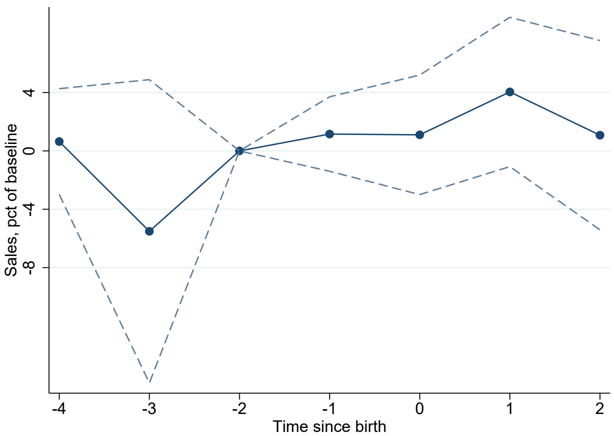
(c) Coworkers' earnings



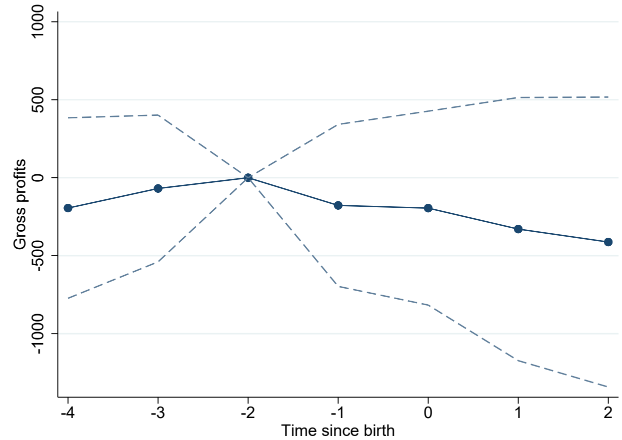
(d) Firms' total variable costs (excluding paid leave)

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

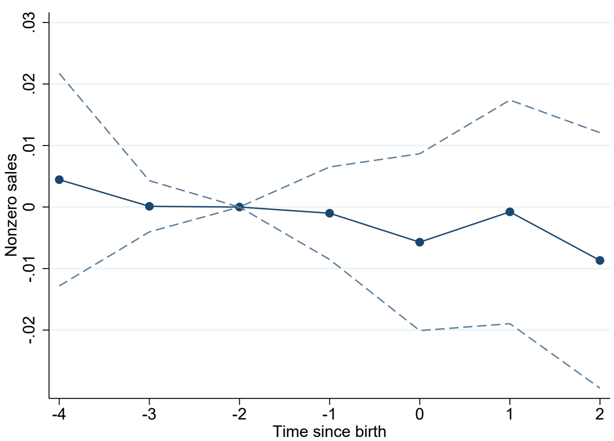
Figure A7: Effect on firms' overall performance and coworkers' sick leave, excluding duplicates, OLS



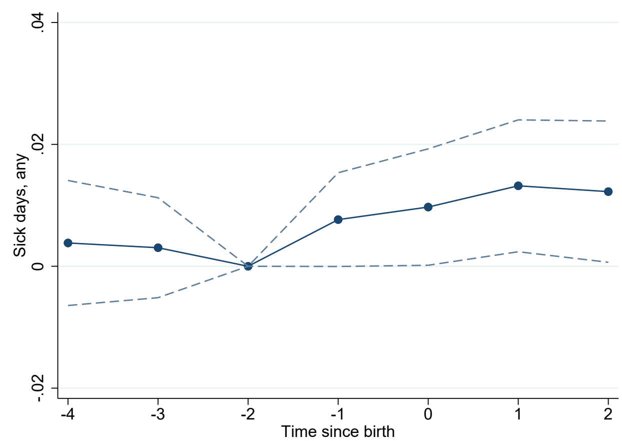
(a) Firms' total sales



(b) Firms' gross profits (1000 DKK)



(c) Likelihood of firm survival



(d) Likelihood of coworkers taking sick leave

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

F Coworker Analysis, Specifications

This section presents additional details of the specifications used in the coworker analysis. Let c index individuals in our coworker sample (see Section 5.4). In the baseline year, each coworker c is employed at some firm f that is part of a potential birth event e. Let t index event time and let y_{ecft} be some coworker outcome. Our dynamic difference-in-differences specification for coworkers is then just a natural adaptation of the firm-level OLS specification (1):

$$\begin{aligned} y_{ecft} =& \psi_e + \sum_{k \in \mathcal{T}} \omega_k \mathbb{1}_{t=k} + \sum_{k \in \mathcal{T}} \kappa_k \mathbb{1}_{t=k} \cdot Treatment_e + \nu_{ecft} \\ \mathcal{T} =& \{-4, -3, -1, 0, 1, 2\} \end{aligned} \tag{4}$$

Our 2SLS specification for estimating the (absolute) effect of an additional birth on coworkers similarly is:

$$\Delta y_{ecf} = \varrho_0 + \mu_0 BirthsInEventYear_{ef} + \Delta \nu_{ecf} \tag{5}$$

$$BirthsInEventYear_{ef} = \iota_0 + \iota_1 Treatment_e + \upsilon_{ef} \tag{5, First Stage}$$

Our 2SLS specification for estimating the (relative) effect of one percent of the workforce giving birth is a natural adaptation of specification (5):

$$\Delta y_{ecf} = \varpi_0 + \chi_0 \frac{BirthsInEventYear_{ef}}{BaselineEmployees_{ef}} + \Delta \sigma_{ecf} \tag{6}$$

$$\frac{BirthsInEventYear_{ef}}{BaselineEmployees_{ef}} = \zeta_0 + \zeta_1 \frac{Treatment_e}{BaselineEmployees_{ef}} + \sigma_{ef} \tag{6, First Stage}$$

When estimating each of the coworker specifications, we apply the reweighting described in Section 5.6. Specifically, each coworker receives the weight associated with his or her event (so coworkers at treatment firms all receive a weight of one).

G Estimates Using a Purely Regression-Based Approach

In our main analysis, we use a matching and reweighting procedure to condition on baseline observables. As is well known, matching and reweighting estimators exhibit an equivalence with linear regression using control variables modulo some issues regarding heterogeneous treatment effects and the weighting of different observations (Angrist, 1998). Accordingly, it is possible to implement our empirical strategy as a standard linear regression if one includes a particular set of control variables. We verify that this purely regression-based approach yields similar results in this appendix.

Adopting the same notation as in Section 5.3, we consider the following dynamic difference-in-differences specification:

$$\begin{aligned} Y_{eft} =& \gamma_e + \sum_{k\in\mathcal{T}} \alpha_k \mathbb{1}_{t=k} + \sum_{k\in\mathcal{T}} \gamma_k \mathbb{1}_{t=k} \cdot Treatment_e + \sum_{k\in\mathcal{T}} \beta_k \mathbb{1}_{t=k} \cdot X_e + \varepsilon_{eft} \\ \mathcal{T} =& \{-4, -3, -1, 0, 1, 2\}. \end{aligned} \tag{7}$$

This specification is identical to that used in the main text, except for the fact that a vector of event-specific baseline characteristics, X_e, has been interacted with the event time dummies and added as controls. Because these added interaction terms will absorb any differences in time trends that are related to baseline characteristics, estimating the specification above (without any reweighting) represents an alternative way to condition out baseline observables in our difference-in-differences analysis.

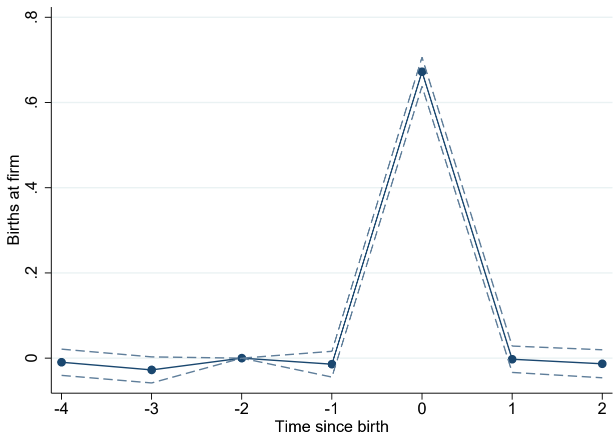
In order for this type of regression to be equivalent to the reweighting used in our main analysis, we need to choose the vector of characteristics X_e in a very specific way (see Angrist (1998) for details). In particular, we partition our sample into a very large number of cells based on all possible values of all the observables we condition on in our main analysis[26] and let X_e consist of an exhaustive set of dummies indicating which of the cells event e belongs to.

Appendix Figures A8 to A12 show OLS estimates from this alternative regression-based approach.

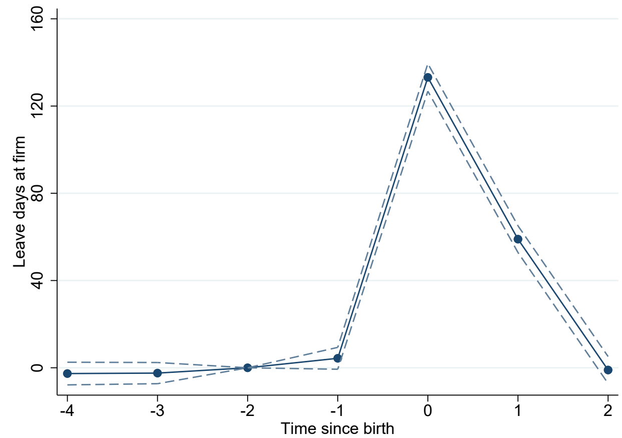
[26]For an example, assume that we only condition on women's quintile of earnings and education group, along with firm's quintile of employees. In this case, the first cell would consist of all events in which the woman is in the bottom quintile of earnings and in the bottom education group, and the firm is in the bottom quintile in terms of employees. The second cell would consist of all events in which the woman is in the bottom quintile of earnings and in the bottom education group, while the firm is in the second-to-last quintile in terms of employees, and so on.

We see that they are virtually indistinguishable from the results presented in the main text.

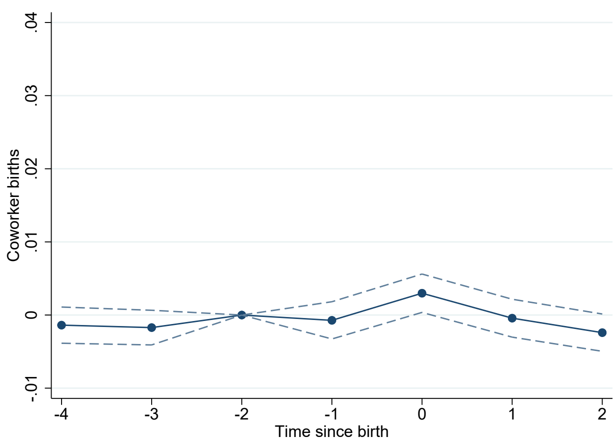
Figure A8: Effects on births and leave days, regression with controls, OLS



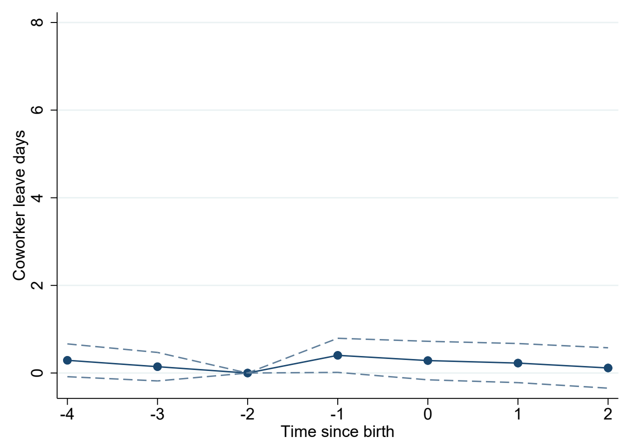
(a) Births at firm



(b) Parental leave days at firm



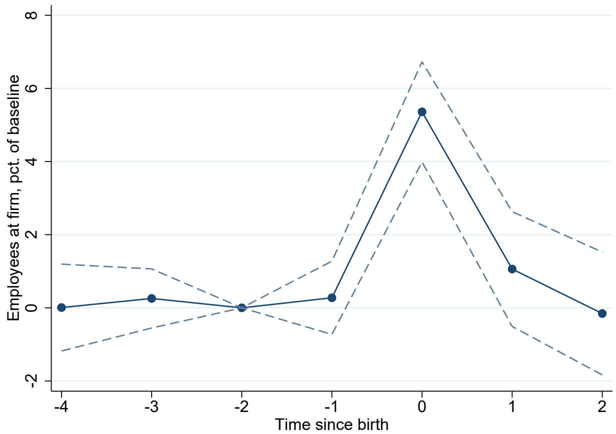
(c) Coworkers' births



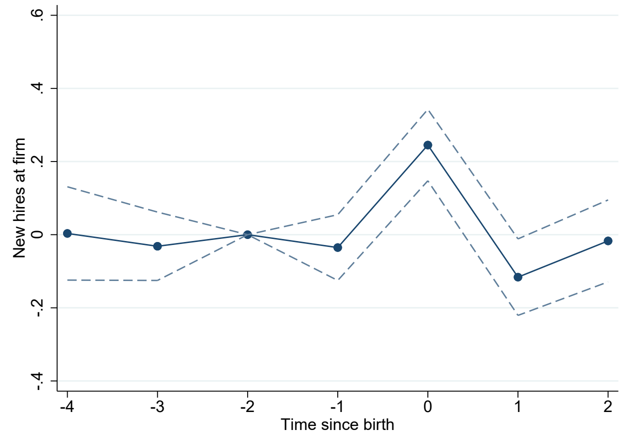
(d) Coworkers' parental leave days

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

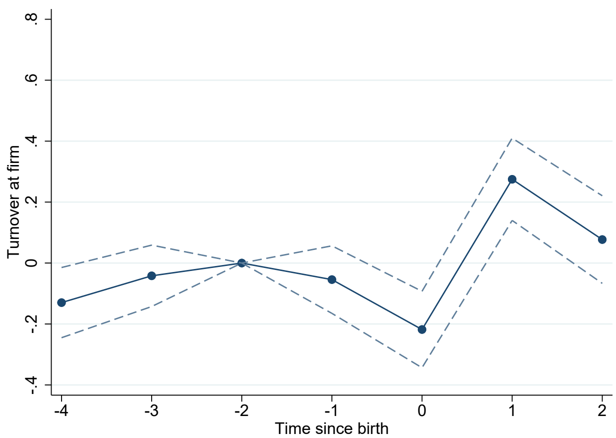
Figure A9: Effects on employment outcomes, regression with controls, OLS



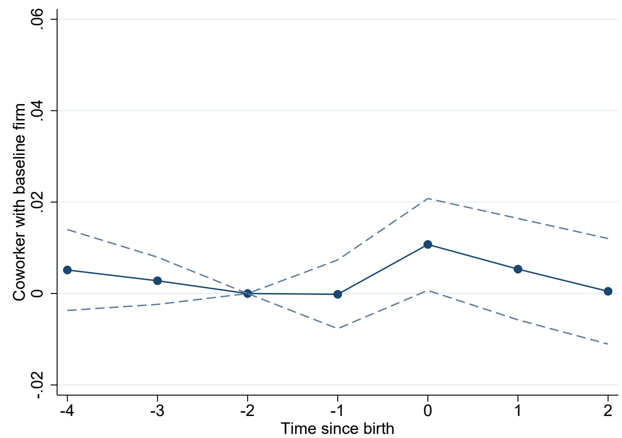
(a) Number of employees at firm



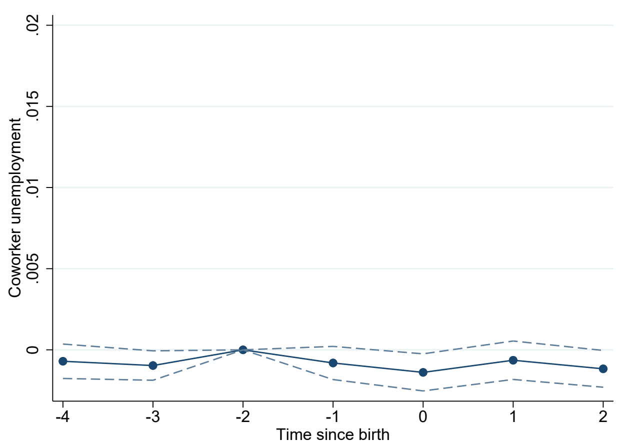
(b) New hires at firm



(c) Turnover at firm



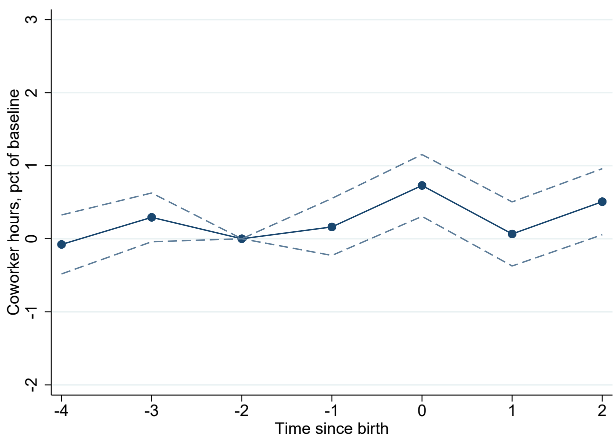
(d) Likelihood coworkers with baseline firm



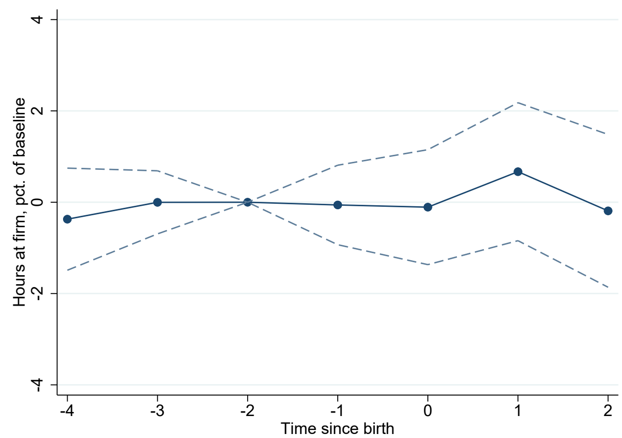
(e) Coworkers' share of year unemployed

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Figure A10: Effects on hours of work, regression with controls, OLS



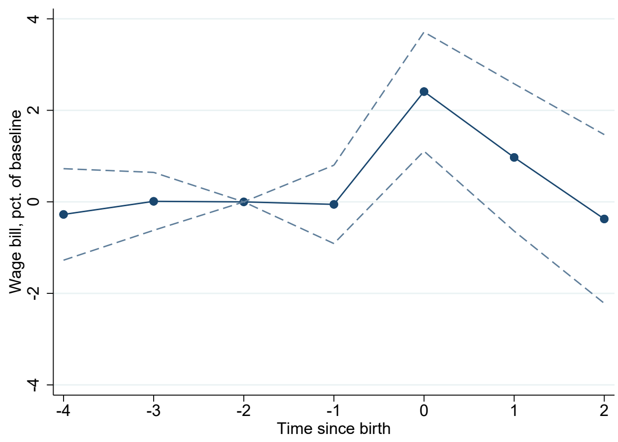
(a) Coworker hours



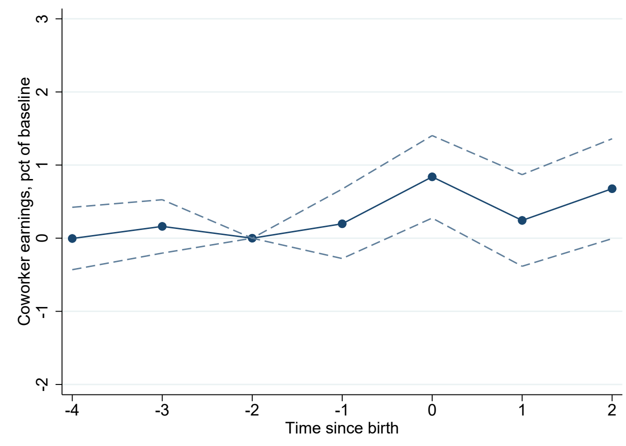
(b) Hours at firm

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

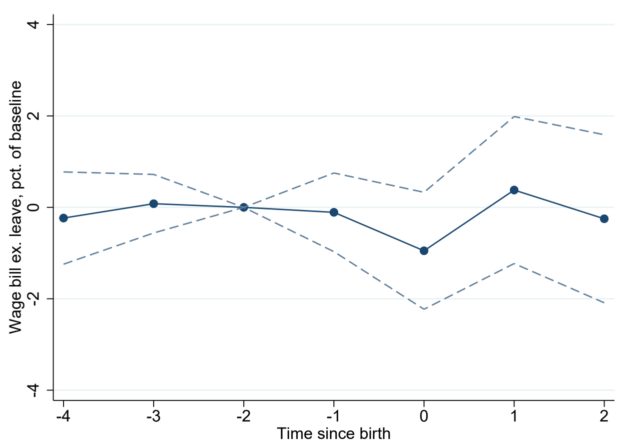
Figure A11: Effects on costs of labor supply adjustments, regression with controls, OLS



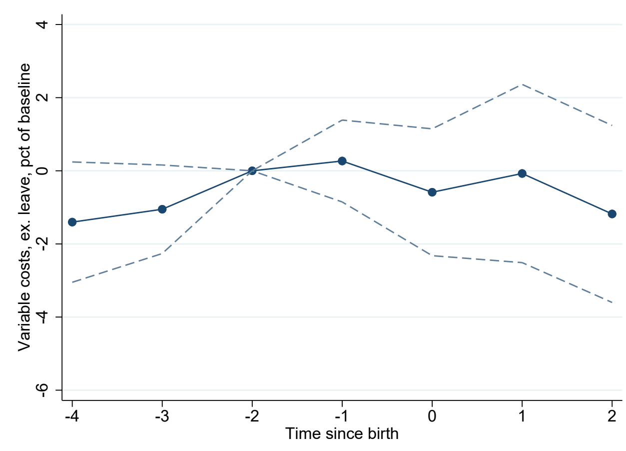
(a) Firms' wage bill



(b) Coworkers' earnings



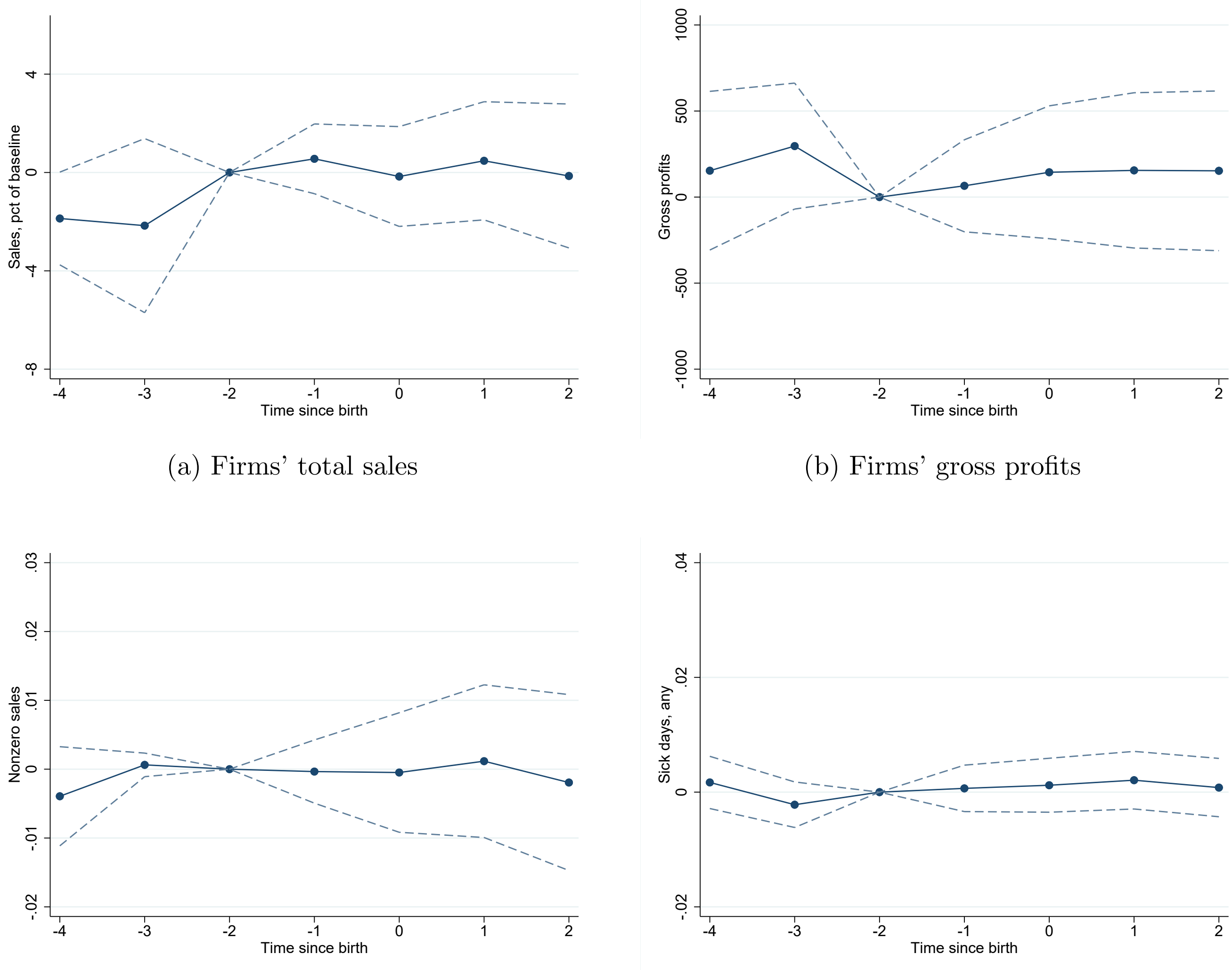
(c) Firms' wage bill (excluding paid leave)



(d) Firms' total variable costs (excluding paid leave)

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Figure A12: Effect on firms' overall performance and coworkers' sick leave, regression with controls, OLS



(a) Firms' total sales

(b) Firms' gross profits

(c) Likelihood of firm survival

(d) Likelihood of coworkers taking sick leave

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

H Results Using Coarser Set of Baseline Covariates

As discussed in Section 5.6, our main results use a very detailed matching and reweighting procedure to condition on baseline observables. This detailed procedure gives us confidence that the treatment and control firms are ex-ante similar to ensure internal validity. As we have seen, however, it also forces us to trim away some of our sample to guard against non-overlapping support issues. This raises questions about external validity and whether our sample is representative of smaller firms.

To examine how the large degree of trimming affects results, we conduct additional analyses in which we use a coarser matching and reweighting procedure. Specifically, we restrict our set of baseline observables to: (i) a set of indicators for having any children aged zero, one, two, and three or more years instead of the number of children in each age group, and (ii) quartiles instead of quintiles for all continuous variables that we match on (for example, quartiles instead of quintiles of the average number of children per employee). Using this coarser set of observables results in fewer observations' being trimmed. Of the initial 23,734 treatment events, 14,273 (60.1 percent) now remain after the trimming.[27] However, the coarser set of baseline observables implies that the treatment and control groups will be less comparable.

For all our main outcomes, Appendix Figures A13 to A17 report OLS estimates of the impact of treatment as a function of distance to the event year, using the coarsened sample. Reassuringly, the results are similar to those from our main analysis. We note, however, that some of our validity checks fail when using this alternative coarser approach. Specifically, we see in Appendix Table A6 that leave days and profits at the firm are no longer balanced across the (weighted) treatment and control samples in the baseline year. We also see some indications of pre-treatment trends in the figures. In particular, for leave days and firm total sales, these trends are statistically different from zero.

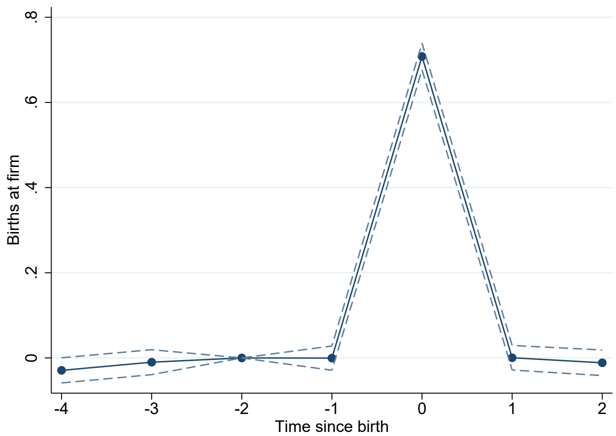
[27] Of the initial 155,625 control events, 38,533 remain after trimming when using the coarser set of baseline covariates.

Table A6: Covariate balance table conditioning on coarser set of observables

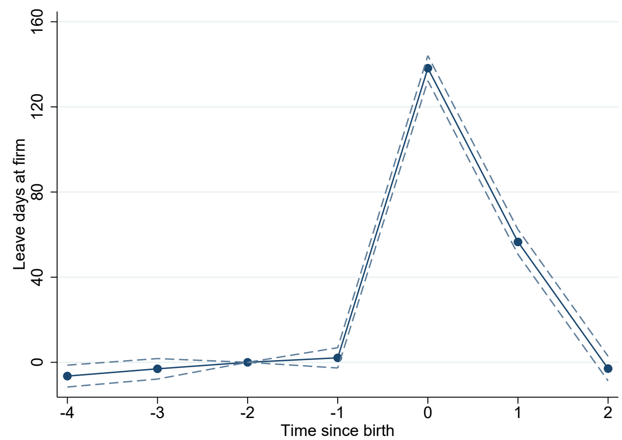
	Treatment	Control	Difference	p-Value
Births at firm	0.84	0.82	0.02	0.11
	(1.07)	(1.06)	(0.01)	
Leave days at firm	149.80	143.40	6.40	0.01
	(201.57)	(199.79)	(2.40)	
New hires	3.74	3.79	−0.05	0.14
	(3.32)	(3.31)	(0.04)	
Hours (FTEs)	10.66	10.63	0.03	0.69
	(7.16)	(7.17)	(0.08)	
Workforce avg. years schooling	11.70	11.68	0.02	0.10
	(1.34)	(1.33)	(0.02)	
Workforce avg. age	34.25	34.38	−0.12	0.09
	(6.29)	(6.43)	(0.07)	
Workforce avg. experience	12.45	12.53	−0.08	0.19
	(5.22)	(5.32)	(0.06)	
Wage bill (1000 DKKs)	3410.62	3419.82	−9.19	0.78
	(2946.29)	(2984.47)	(33.60)	
Purchases (1000 DKKs)	12306.90	12451.37	−144.48	0.68
	(32366.50)	(31620.98)	(350.94)	
Profits (1000 DKKs)	11694.68	10827.65	867.03	0.01
	(32020.50)	(29996.93)	(349.35)	
Profits ex leave (1000 DKKs)	−219113.10	−219206.04	92.94	0.94
	(105921.43)	(103166.39)	(1201.12)	
Event year	2007.12	2007.13	−0.01	0.85
	(2.82)	(2.88)	(0.03)	

Notes: The table shows means and standard deviations for the firm and event-specific variables in the baseline year across the coarsened sample of treatment and control events. The table also shows the difference in means between the two samples along with the standard error of this difference computed based on clustering at the firm level. The number of observations is 52,863. ** $p < 0.01$ * $p < 0.05$.

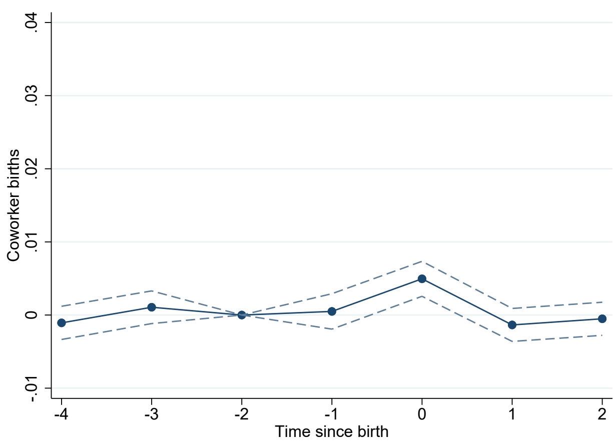
Figure A13: Effects on births and leave days, conditioning on coarser set of observables, OLS



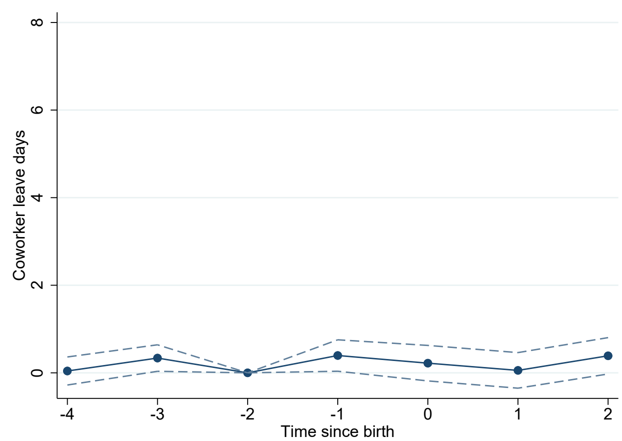
(a) Births at firm



(b) Parental leave days at firm



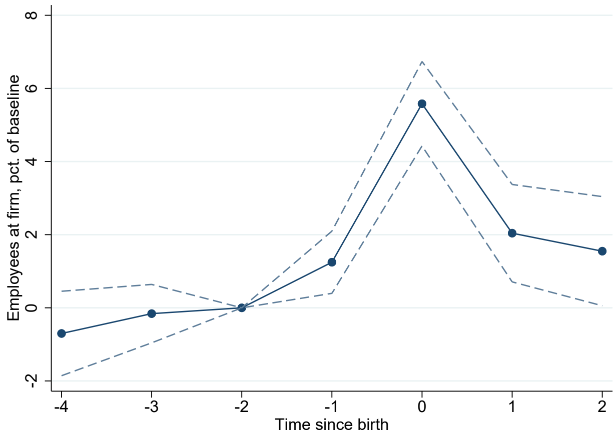
(c) Coworkers' births



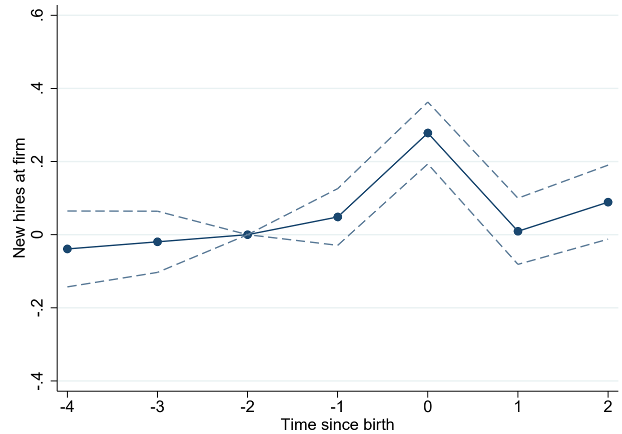
(d) Coworkers' parental leave days

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

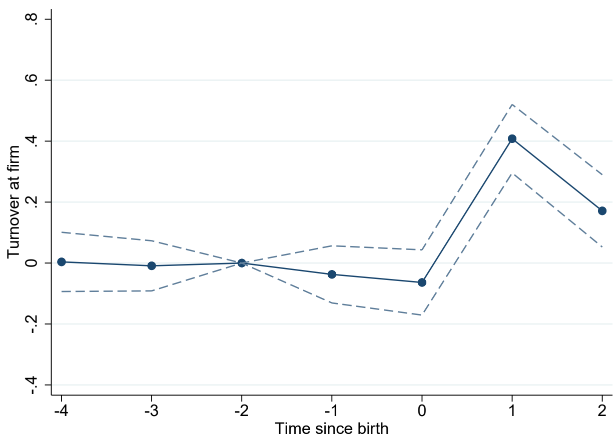
Figure A14: Effects on employment outcomes, conditioning on coarser set of observables, OLS



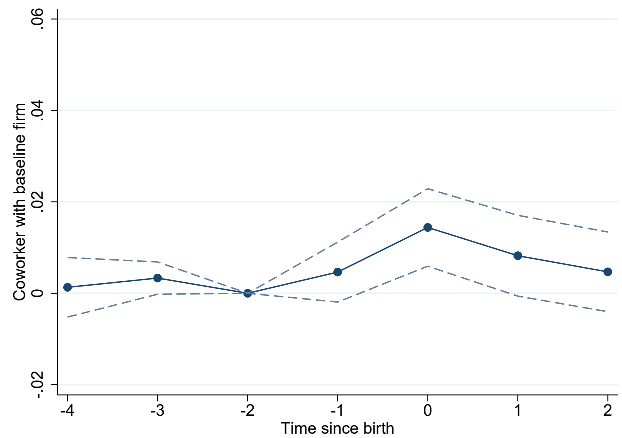
(a) Number of employees at firm



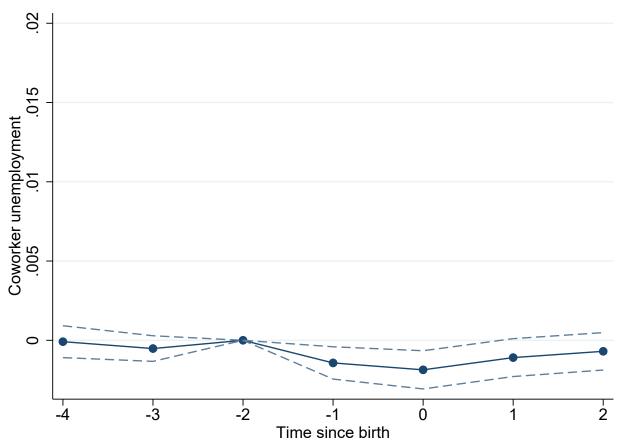
(b) New hires at firm



(c) Turnover at firm



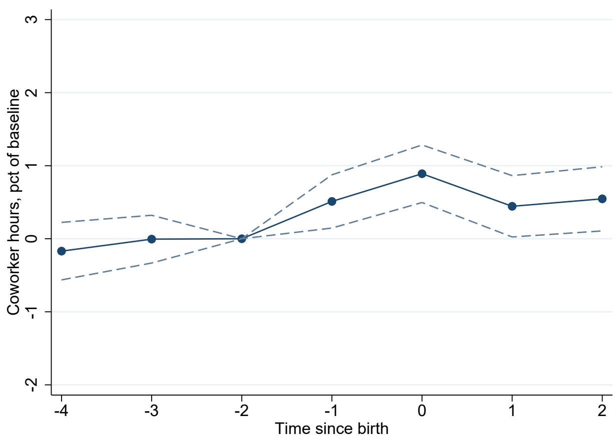
(d) Likelihood coworkers with baseline firm



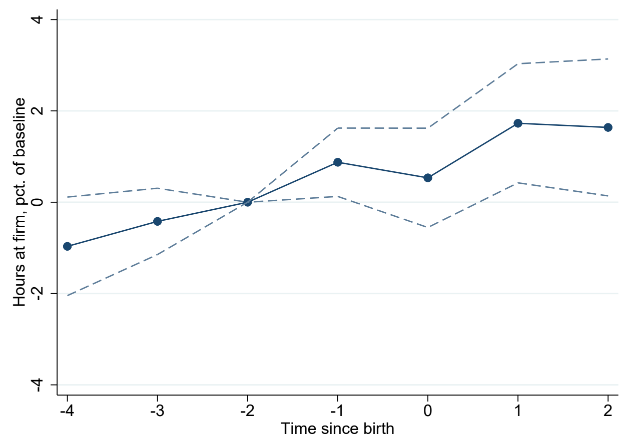
(e) Coworkers' share of year unemployed

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Figure A15: Effects on hours of work, conditioning on coarser set of observables, OLS



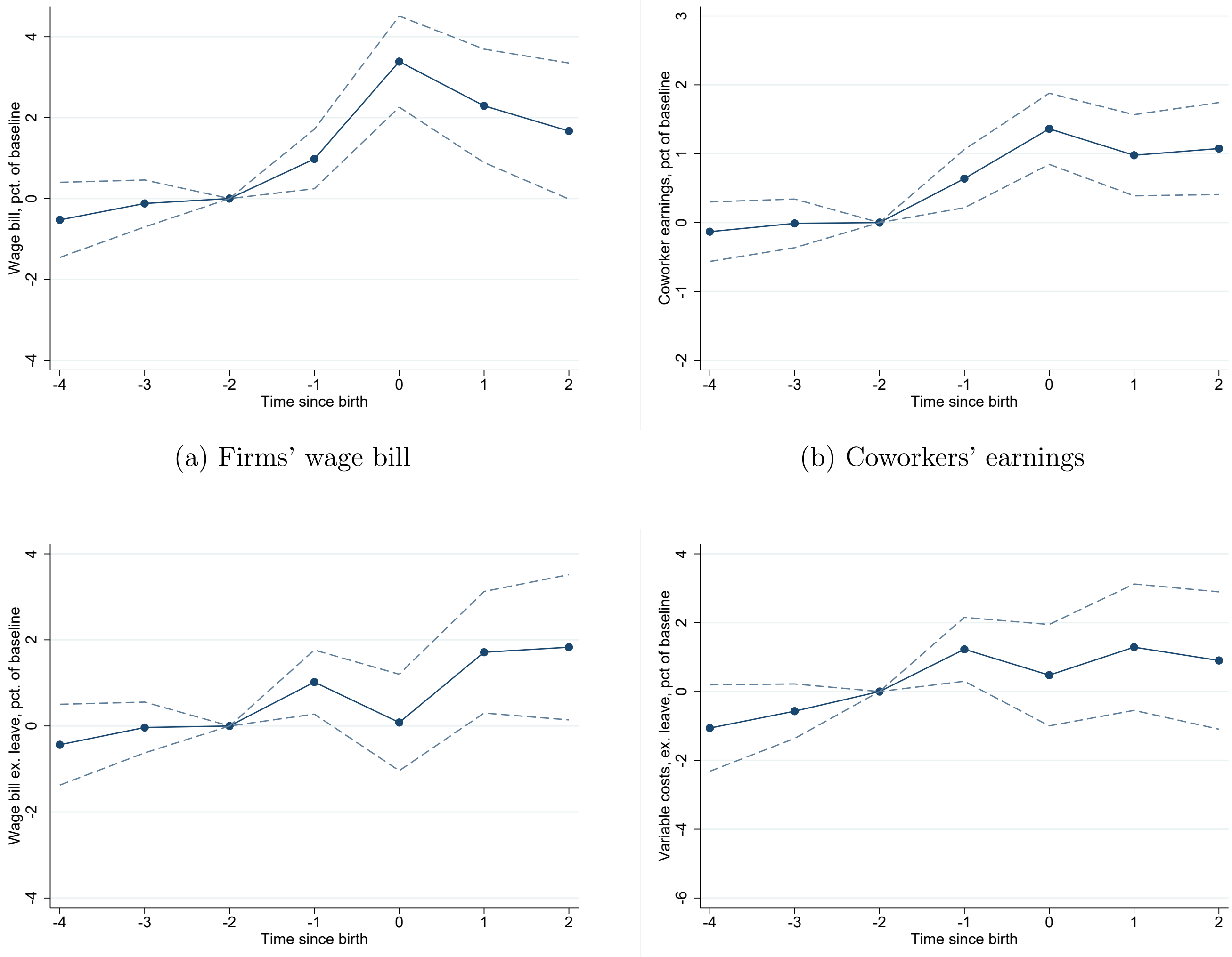
(a) Coworker hours



(b) Hours at firm

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Figure A16: Effects on costs of labor supply adjustments, conditioning on coarser set of observables, OLS



(a) Firms' wage bill

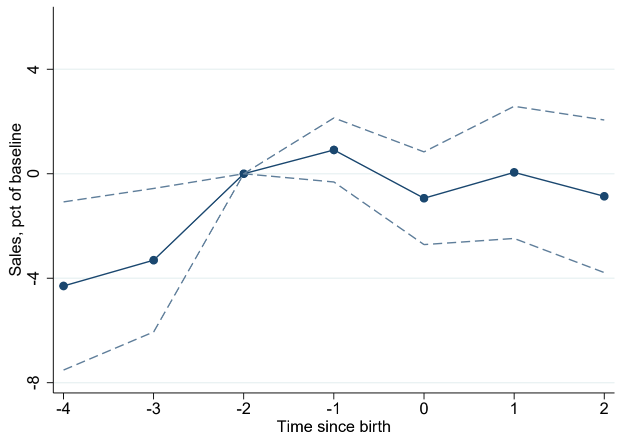
(b) Coworkers' earnings

(c) Firms' wage bill (excluding paid leave)

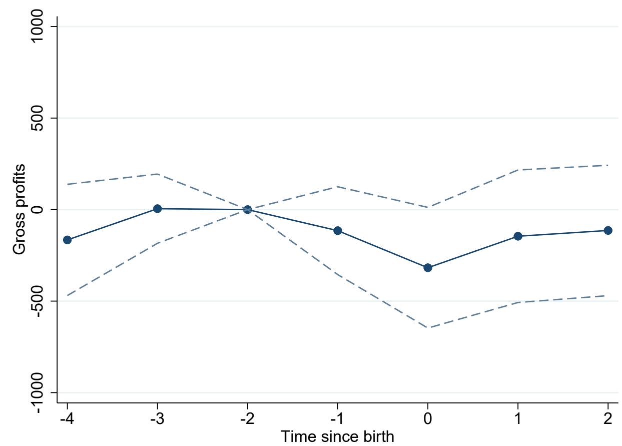
(d) Firms' total variable costs (excluding paid leave)

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

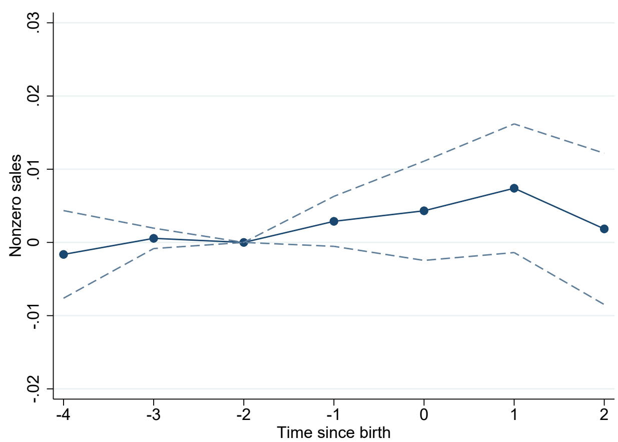
Figure A17: Effect on firms' overall performance and coworkers' sick leave, conditioning on coarser set of observables, OLS



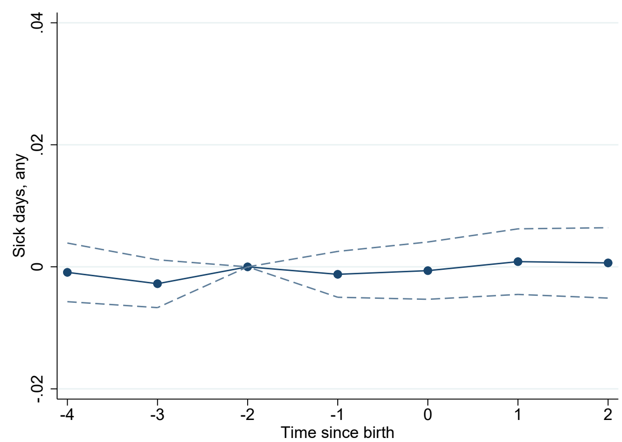
(a) Firms' total sales



(b) Firms' gross profits



(c) Likelihood of firm survival



(d) Likelihood of coworkers taking sick leave

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

I Representativeness of Firms in Analysis Sample

In constructing our main analysis sample, we apply a number of sample restrictions. Perhaps most notably, we restrict our attention to small firms, require that both treatment and control firms have at least one young female employee at baseline, and trim observations with extreme weighting values when applying our matching and reweighing procedure. To understand what types of firms we cover in our main analysis, this section compares our sample of treatment firms to both the universe of private sector firms in Denmark and to the subset of those firms that satisfy our firm size restriction. Appendix Table A7 compares baseline characteristics across the three groups of firms. The appendix table indicates that our treatment firms experience more births per employee (0.064 as opposed to 0.054 for the universe of small firms) and more leave days (12.6 as opposed to 4.8). Furthermore, the share of women at our treatment firms is higher than in other samples (at 0.647 versus 0.347). While all the treated firms naturally have at least one female employee, 27.5 and 14.8 percent of respectively all firms and the size-restricted firm sample do not employ any women (not reported in the table). Meanwhile, the number of children per employee is lower in our sample compared to the universe of private sector firms (1.3 versus 1.7). However, the characteristics of firms in our treatment sample are comparable to the universe of private and small firms in Denmark. Specifically, work hours and the wage bill per employee are comparable across the three samples, while sales and purchases per employee in the treatment sample are only slightly smaller in magnitude relative to the other samples.

We further compare the 1-digit industry composition of the three groups of firms in Appendix Figure A18. Compared to the universe of private and small firms, some industries—such as retail, hotels, and restaurants, as well as personal services—are overrepresented in our treatment sample. This is because women are more likely to work in these types of industries. Nonetheless, the figures highlight that the majority of industries are represented in our treatment sample.[28]

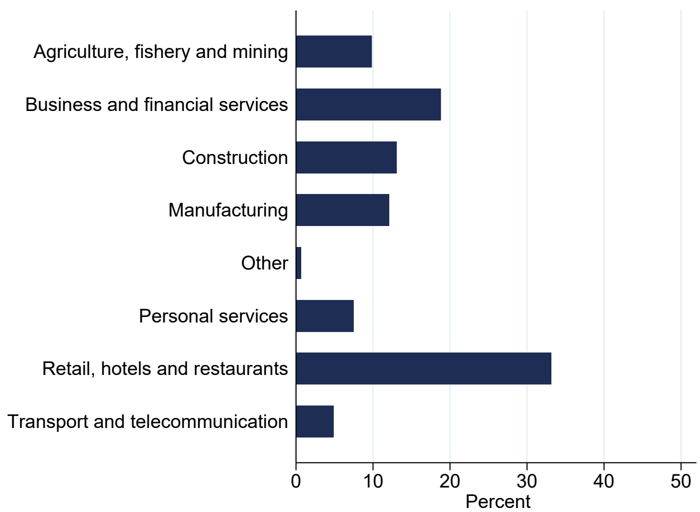
[28]The only exception is the "electricity and water supply" industry. However, even among the universe of private firms and small firms, the share of firms belonging to this industry is very small.

Table A7: Baseline characteristics compared to universe of private and small firms

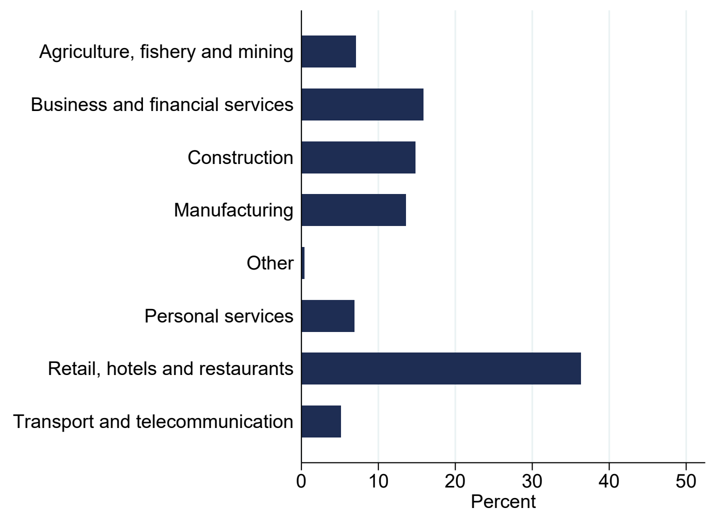
	All Firms	Size Restricted	Treatment Sample
Hours per employee (FTEs)	0.821	0.817	0.813
	(0.594)	(0.273)	(0.219)
Wage bill per employee	259.882	256.816	242.472
	(248.120)	(135.502)	(116.117)
Sales per employee	1461.935	1312.535	1220.743
	(2894.591)	(2735.783)	(1937.685)
Purchases per employee	1018.727	904.722	823.166
	(2761.920)	(2342.965)	(1681.653)
Births per employee	0.058	0.054	0.064
	(0.190)	(0.106)	(0.093)
Leave days per employee	4.952	4.759	12.636
	(25.121)	(13.847)	(21.330)
Children per employee	2.051	1.741	1.305
	(3.458)	(1.179)	(0.816)
Share women	0.337	0.347	0.647
	(0.361)	(0.322)	(0.278)
Employee avg. age	38.321	37.539	33.850
	(10.239)	(8.171)	(6.412)
Employee avg. experience (years)	15.026	15.073	12.278
	(8.277)	(6.936)	(5.307)
Employee avg. schooling (years)	11.404	11.390	11.610
	(1.760)	(1.425)	(1.283)
Observations	1,320,921	668,182	9,934

Notes: The table shows means and standard deviations for all firm-years for the firm and event-specific variables in all firms (the column *All Firms*) and size restricted firms (the column *Size Restricted*). The last column (*Treatment Sample*) shows these statistics for treated firms in the baseline year only, explaining the differences in number of observations.

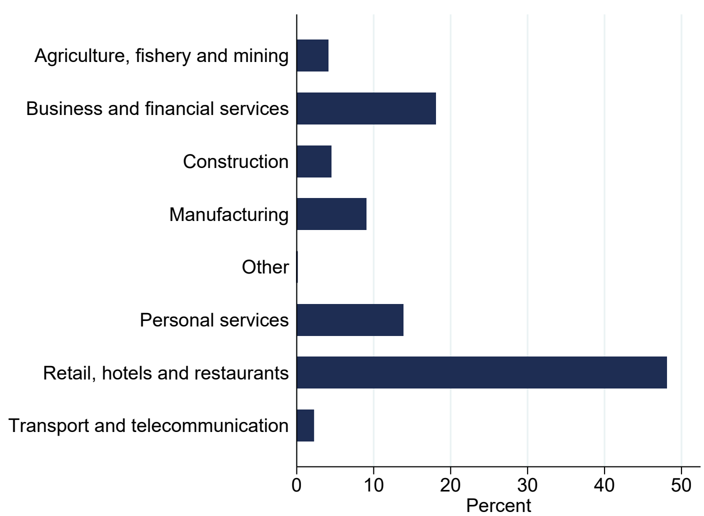
Figure A18: Industry composition by sample restrictions



(a) All firms



(b) Size Restricted



(c) Treatment Sample

Notes: The figure shows the industrial composition across 1-digit industries. Because it contains a very small number of firms, the category "Electricity and water supply" has been lumped into the "Other" category for reasons of data confidentiality.

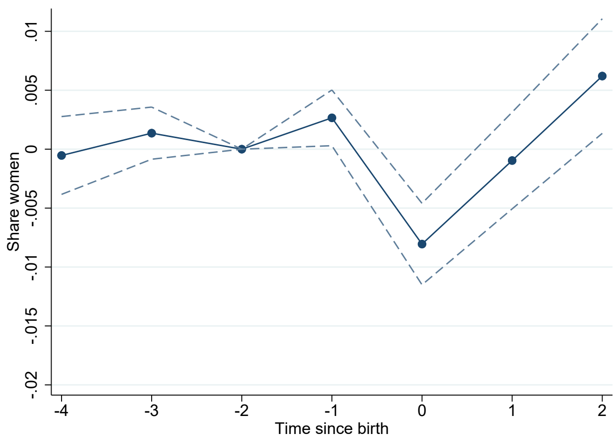
J Effects on Workforce Characteristics

Our main analysis suggests that total labor inputs are, in net, relatively unaffected when an employee goes on leave. This result is based on measuring the quantity of labor inputs (hours). In practice, there could be important losses of productivity if the quality of labor inputs changes. However, as is typical, we do not have good measures of productivity at the individual level. As the next-best alternative to characterizing the replacement worker and understanding how the quality of the workforce is affected, we look for changes in workforce characteristics (Appendix Figure A19 and Appendix Table A8).

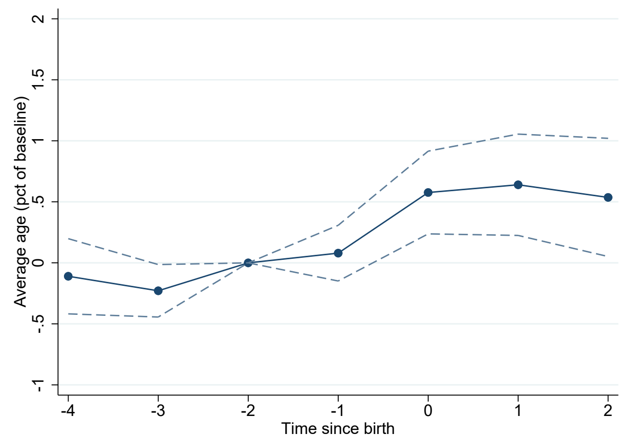
We first find that in the event year, one additional female employee giving birth at the firm lowers the share of women by one percentage point, indicating that a leave-taking woman is replaced by temporary worker of either gender.[29] We also detect small changes in other characteristics. The average age of the workforce rises by 0.719 percent in the event year when an additional employee gives birth. This is concurrent with a 0.233 percent drop in the workforce's average years of education and an increase of 0.08 years in average experience. These results indicate that temporary workers are on average older than the women who go on leave, and that older workers typically have more years of experience but fewer years of schooling. Our findings suggest that the characteristics of the firm's workforce are not substantially altered when an additional woman gives birth. Taken together, it is difficult to speculate on the expected effect on productivity as some changes in worker traits are associated with productivity gains (e.g., experience), whereas some are associated with productivity losses (e.g., education). Furthermore, given that temporary employees exit the firm after leave-takers return to their jobs, any changes appear temporary.

[29]As previously mentioned, when computing workforce shares and averages, we weight each employee by his or her hours worked at the firm. Accordingly, average workforce characteristics are undefined in years in which firms have zero work hours. However, there is no differential attrition between treatment and control groups, since leave-taking has no effect on firm shutdown (i.e., the probability of having zero employees or zero work hours).

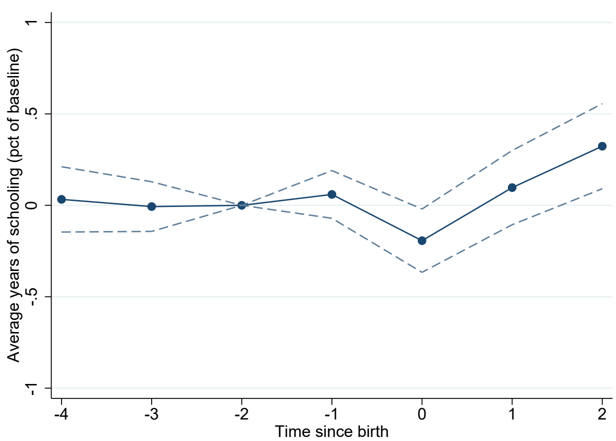
Figure A19: Effect on workforce characteristics, OLS



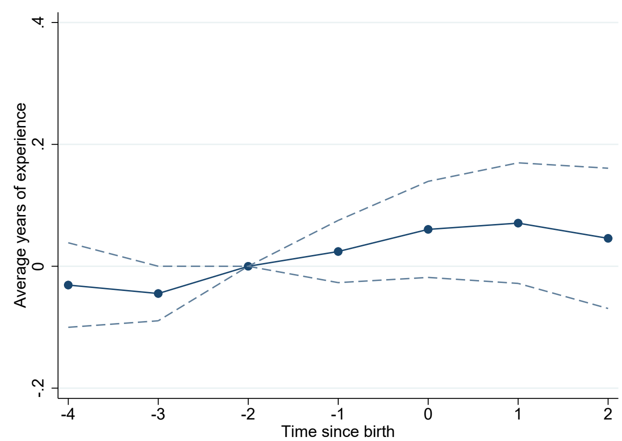
(a) Women's share of the workforce



(b) Average age of the workforce



(c) Workforce average years of schooling



(d) Workforce average years of experience

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

Table A8: Effects on workforce characteristics, 2SLS

	Absolute effect Effect of one additional birth		*Relative effect* Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
Share women at baseline firm	-0.0116^{**}	-0.239	-0.00102^{**}	-0.000401
	(0.00214)	(0.0796)	(0.000183)	(0.000212)
Average age (pct. rel. to baseline)	0.719**	0.688**	0.0535**	0.0498*
	(0.219)	(0.254)	(0.0205)	(0.0237)
Average years of education (pct. rel. to baseline)	-0.233^{*}	0.124	-0.0194	0.00852
	(0.111)	(0.127)	(0.0104)	(0.0123)
Average years of experience	0.0835	0.0773	0.00314	0.00742
	(0.0492)	(0.0600)	(0.00425)	(0.00522)
F-stat	2,409	2,293	2,676	2,538
Observations	28,263	26,231	28,263	26,231
Observations (weighted)	17,652	16,385	17,652	16,385
Clusters (firms)	14,138	13,058	14,138	13,058

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the number of births at the firm in the event year is instrumented by the treatment dummy. Columns (3) and (4) show estimates from similar regressions but in which both the number of births at the firm in the event year and the treatment dummy is divided by the number of baseline employees (measured in hundreds), and where dummy variables for each possible number of baseline employees are included as controls. In Columns (1) and (3) the outcome variable is measured in the event year (Time 0). In Columns (2) and (4) the outcome variable is measured in the following year (Time 1). Regressions use firm-level data. The number of observations changes across different columns because some firms may shut down between the event year and the following year. Throughout, the analysis is conducted on the matched and reweighted samples. For each panel and column, the F-stat from the first stage regression is listed. Standard errors (in parentheses) are clustered at the firm level. ** p <0.01 * p <0.05.

K Labor Supply Changes Among Treatment and Control Women

To understand labor supply changes around childbirth among women in our sample, this section compares the evolution of treatment and control women's own outcomes around the time of the potential birth event using a natural adaptation of our main difference-in-differences specification. We emphasize that the point of this analysis is to descriptively compare treatment and control women's behavior around the event year rather than to provide causal estimates of the effect of childbirth.

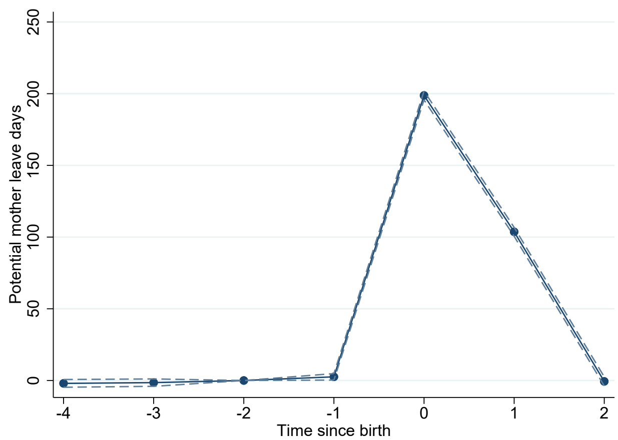
Let i index the individual woman (the potential mother), f the firm at which the woman is employed in the baseline year, e the potential birth event, t event time, and y_{eift} the individual woman's outcome. Our dynamic difference-in-differences specification for the potential mother is just a natural adaptation of the firm-level OLS specification (1):

$$\begin{aligned} y_{eift} =& \psi_e + \sum_{k \in \mathcal{T}} \omega_k \mathbb{1}_{t=k} + \sum_{k \in \mathcal{T}} \kappa_k \mathbb{1}_{t=k} \cdot Treatment_e + \nu_{eift} \\ \mathcal{T} =& \{-4, -3, -1, 0, 1, 2\}. \end{aligned} \tag{8}$$

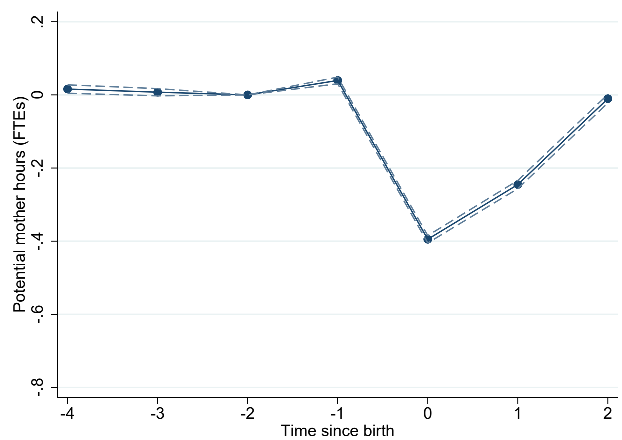
Note that we only present the OLS specification, as compliance is complete at the individual level.

Appendix Figure A20 shows no pre-trends three to four years before the event for any of the outcomes. This is consistent with previous evidence from Kleven *et al.* (2019). Meanwhile, in the year before childbirth, the event year, and the following one year (i.e. in the time up to conception, during pregnancy, and during parental leave), there are some relevant differences between treatment and control women. Women who give birth are eleven percentage points more likely to stay with the baseline firm in the event year than control women (Figure A20(c)); in levels, 59.9 percent of treatment women and 49.0 percent of control women are still with their baseline firm in the event year. This difference in the likelihood of being with the baseline firm may to some extent be mechanical, as firms typically cannot fire a woman who is pregnant or on parental leave. Alternatively, women may well be less motivated to search for a new employer immediately at the time of childbirth. The result that treatment women are more likely to stay with their employer, might seem puzzling given

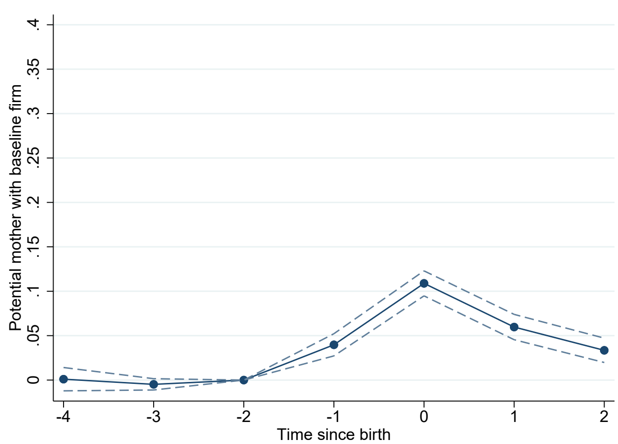
Figure A20: Effect on Potential Mothers



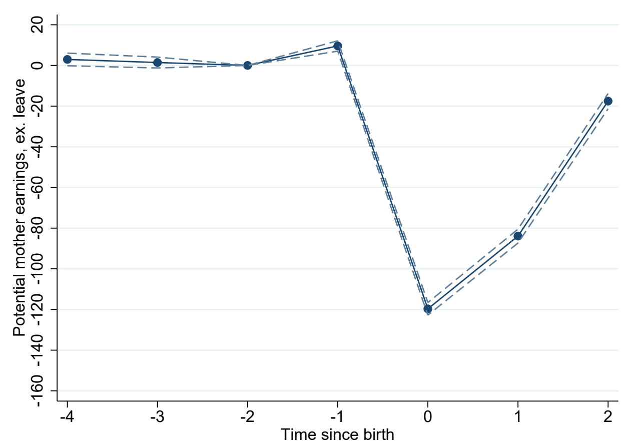
(a) Parental leave days



(b) Hours (FTE)



(c) Share remaining with baseline firm



(d) Earnings ex. paid leave (in 1,000 DKK)

Notes: The dots and solid lines show the estimated difference between the treatment and control women from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level. The graphs show the differences in the outcomes of the potential mothers (i.e. treatment and control women).

existing evidence that women start to sort into certain types of firms and jobs after the birth of their first child (see for example Kleven *et al.* (2019)). It merely reflects, however, that most of the sorting is happening through churn and not through higher separation rates for mothers.

In terms of labor supply changes after childbirth, figures A20(b) and A20(d) show that treatment women experience a large drop in hours and labor earnings in the year of childbirth and the following year while the woman is on leave. Two years after childbirth, when the parental leave has ended, our measure of hours recovers almost fully and is less than one percent below baseline. Labor earnings (excluding paid leave) also recover somewhat but remain about 20,000 DKK lower than at baseline.

The labor supply changes shown in figures A20(b) and A20(d) reflect both the direct effect of going on parental leave and being absent, as well as any other changes in labor supply that occur as a result of childbirth. To partially separate these, it is instructive to compute the direct effect that parental leave absences would have if other labor supply decisions (and wages) had otherwise remained unchanged relative to baseline. Relative to control, women in our treatment sample on average take 199 more days of parental leave in the event year and 104 the following year. With an average yearly labor supply of 0.85 full-time equivalent at baseline, these absences translate into a direct yearly hours reduction of 46 percent of a full-time worker in the event year and 24 percent the year after. With average labor earnings at 239,000 DKK in the baseline year, the absences also translate into a direct earnings loss of 120,000 DKK in the event year and 84,000 DKK the year after. Comparing these to the actual changes shown in A20(b) and A20(d), we see that these direct effect of parental leave absences can account for virtually all of the overall changes in labor supply.

L Heterogeneous Effects in Firm Size

Differences in the behavior of small and large firms have received much attention in the literature and we might well expect firms of different sizes to differ in how worker absences affect them. To shed some light on effect heterogeneity on this dimension, we therefore compare estimated effects for the smallest firms (10 or fewer baseline employees) and the rest of our analysis sample (firms with 11–30 baseline employees).

Table A9 presents 2SLS results from regressions on the full sample in which we interact our main effect (and instrument) with an indicator for whether the firm has 10 or fewer employees at baseline. We find no evidence that the effects of a birth followed by parental leave differ with firm size. Although there are some indications that smaller firms experience fewer leave days and hire more new workers, this difference is never significant in the preferred specification.[30] For firm performance, the coefficient on the relevant interaction term is insignificant throughout. At the same time, however, we note that the standard errors on the interaction terms are substantial, likely reflecting the reduced power we have when further dividing our sample by firm size. While there might be heterogeneous effects by firm size, we find little evidence of it in our data.

[30]Because the total number of parental leave days is measured in levels, Panel A is the preferred specification for this outcome. Panel B is the preferred specification for number of employees and the total wage bill because these outcomes are measured relative to baseline. Note that using a specification with a meaningful scaling is likely to be particularly important here when making comparisons across firms of different sizes (e.g., hiring and paying one additional person implies a larger increase relative to baseline if the firm is small).

Table A9: Heterogeneous effects by firm size

	Parental leave days at firm	Number of employees at firm (pct. rel. to baseline)	New hires at firm	Turnover at firm	Hours at firm (pct. rel. to to baseline)	Firm's wage bill (pct. rel. to to baseline)	Firm's wage bill excl. paid leave (pct rel. to baseline)	Firm sales (pct. rel. to to baseline)	Gross profits (1000 DKKs)	Nonzero sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A) Absolute effect										
at t=0										
Effect of one additional birth	193.3**	2.908*	0.279*	-0.347*	-0.854	1.067	-1.346	-3.077	-285.7	-0.00523
	(7.173)	(1.086)	(0.116)	(0.145)	(1.079)	(1.107)	(1.135)	(1.604)	(390.5)	(0.00609)
Effect of one additional birth × small firm	3.338	9.286**	0.145	0.193	1.181	5.174**	-0.0436	5.204*	389.8	0.0180
	(8.856)	(1.881)	(0.131)	(0.162)	(1.809)	(1.831)	(1.869)	(2.590)	(407.9)	(0.00984)
at t=1										
Effect of one additional birth	82.21**	-0.175	-0.285*	0.253	-0.317	-0.00512	-0.455	-3.373	-532.1	-0.00417
	(7.079)	(1.350)	(0.131)	(0.156)	(1.323)	(1.425)	(1.429)	(2.075)	(443.7)	(0.00798)
Effect of one additional birth × small firm	7.975	2.491	0.286	0.240	2.026	2.613	1.762	5.895	636.0	0.0223
	(8.767)	(2.276)	(0.146)	(0.173)	(2.184)	(2.317)	(2.332)	(3.129)	(459.5)	(0.0125)
B) Relative effect										
at t=0										
Effect of one additional birth per 100 employees	30.98**	0.550**	0.0456**	-0.0522*	-0.122	0.222	-0.205	-0.464	-32.86	-0.000957
	(1.145)	(0.190)	(0.0175)	(0.0216)	(0.190)	(0.194)	(0.198)	(0.305)	(55.20)	(0.00110)
Effect of one additional birth per 100 employees × small firm	-21.87**	0.0810	-0.0258	0.0447*	0.0810	0.0550	0.0724	0.539	36.92	0.00132
	(1.169)	(0.208)	(0.0177)	(0.0218)	(0.204)	(0.208)	(0.213)	(0.324)	(55.32)	(0.00117)
at t=1										
Effect of one additional birth per 100 employees	13.08**	0.0336	-0.0431*	0.0445	-0.0298	0.00883	-0.0739	-0.542	-62.01	-0.000807
	(1.131)	(0.236)	(0.0194)	(0.0232)	(0.234)	(0.251)	(0.251)	(0.389)	(60.94)	(0.00141)
Effect of one additional birth per 100 employees × small firm	-8.991**	0.107	0.0433*	-0.0232	0.0904	0.0895	0.105	0.612	66.46	0.00147
	(1.157)	(0.256)	(0.0196)	(0.0234)	(0.251)	(0.268)	(0.268)	(0.406)	(61.02)	(0.00149)
Observations	31,908	31,908	31,908	31,908	31,908	31,908	31,908	31,908	31,908	31,908
Observations (weighted)	19,868	19,868	19,868	19,868	19,868	19,868	19,868	19,868	19,868	19,868
Clusters (firms)	16,080	16,080	16,080	16,080	16,080	16,080	16,080	16,080	16,080	16,080

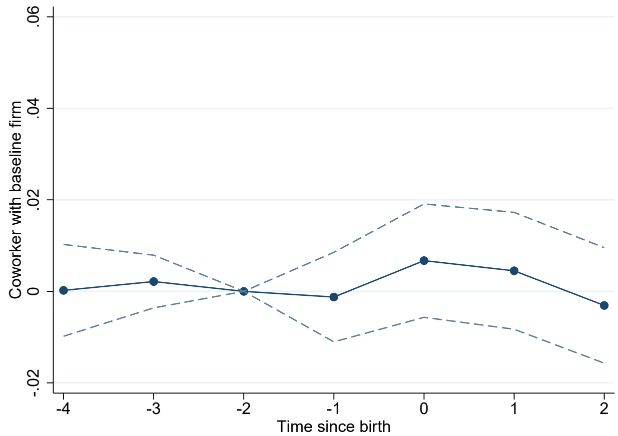
Notes: Each column-row represents the coefficient from a separate regression. Columns refer to different outcome variables. In each regression, the outcome is the change in the relevant outcome between baseline and either the event year ($t = 0$) or the year after ($t = 1$). In Panel A, the regressors of interest are the number of births at the firm in the event year (Effect of one additional birth) and the interaction between the number of births at the firm in the event year and an indicator variable for whether the firm had ten or fewer employees in the baseline year (Effect of one additional birth X small firm). In addition, the indicator variable for whether the firm had ten or fewer employees is included in the regression as a control. The regression is estimated by 2SLS using treatment status and its interaction with the indicator variable for whether the firm had ten or fewer employees in the baseline year as instruments. In Panel B, the regressors of interest are the number of births at the firm in the event year divided by the number of employees at baseline (Effect of one additional birth per 100 employees), and the interaction between the number of births in the event year divided by the number of employees at baseline and an indicator variable for whether the firm had ten or fewer employees in the baseline year (Effect of one additional birth per 100 employees X small firm). In addition, the indicator variable for whether the firm had ten or fewer employees is included in the regression as a control along with a full set of dummy variables for each possible number of baseline employees. The regression is estimated by 2SLS using treatment status divided by the number of employees at baseline and its interaction with the indicator variable for whether the firm had ten or fewer employees in the baseline year as instruments. Throughout, the analysis is conducted on the matched and reweighted samples. Standard errors (in parentheses) are clustered at the firm level. ** $p < 0.01$ * $p < 0.05$.

M Effects on Coworkers in Same vs. Different Occupation

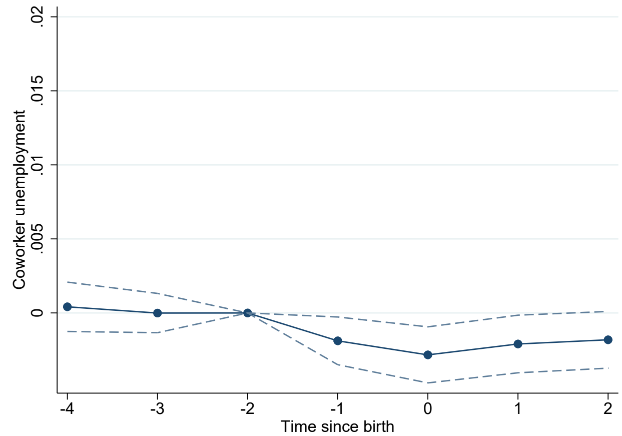
To examine whether the effects of parental leave are different for coworkers who are more likely complements or substitutes for the worker on leave, we split our coworker sample by occupation. For each treatment and control event, we determine the 1-digit occupation of the woman defining the event and then restrict attention either to coworkers who are in this same occupation or to coworkers who are not in this same occupation. The expectation is that same-occupation coworkers are likely substitutes to the worker on leave, while other coworkers are likely to be complements to the worker on leave.

Appendix Figures A21 and A22 show OLS estimates for the resulting two coworker samples. We consistently see that the estimated effects for all coworkers found in the main text are driven almost exclusively by same-occupation coworkers. In contrast, there is very limited evidence of effects for coworkers not in the same occupation.

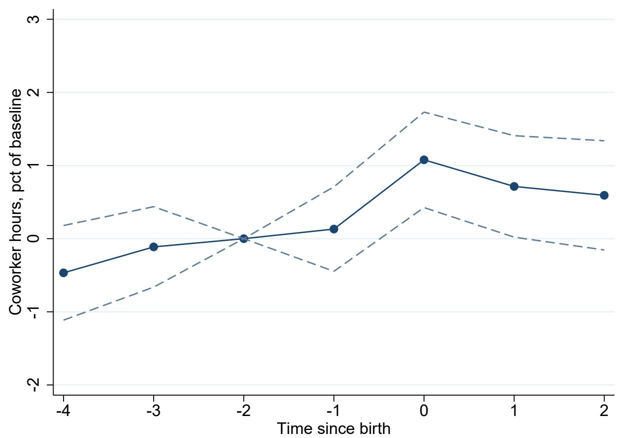
Figure A21: Effects on outcomes of coworkers in same occupations as women on leave, OLS



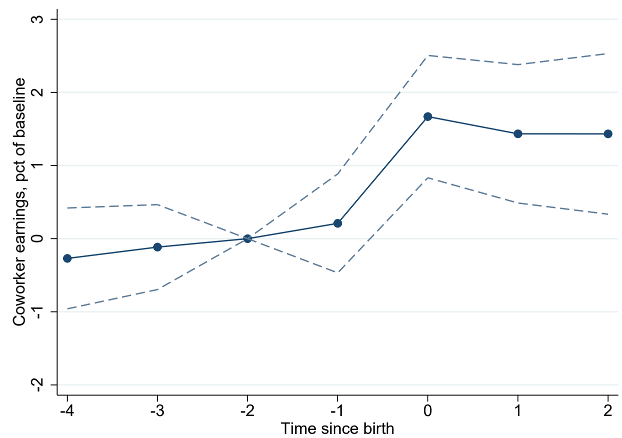
(a) Likelihood of coworker at baseline firm



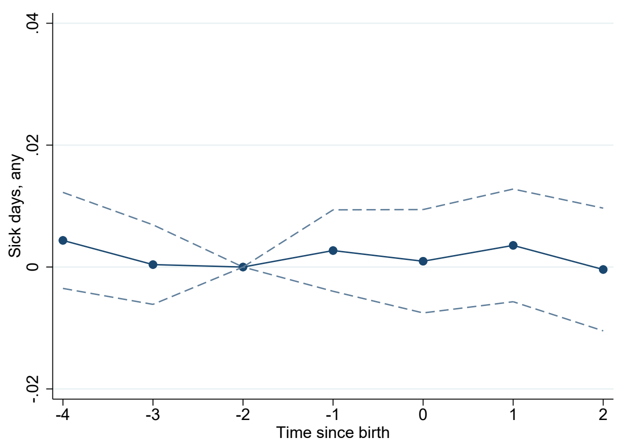
(b) Coworker's share of year unemployed



(c) Coworkers' hours at baseline firm



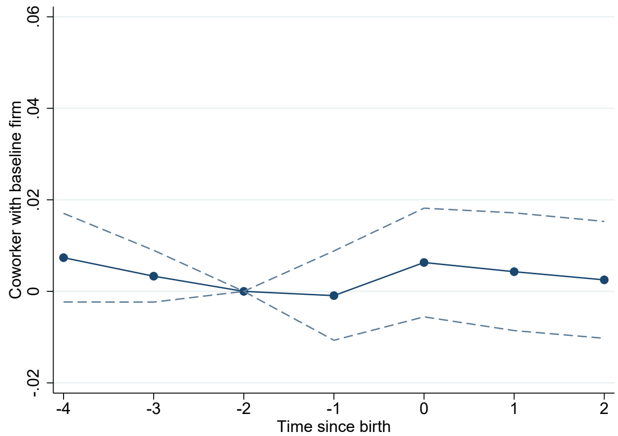
(d) Coworkers' earnings



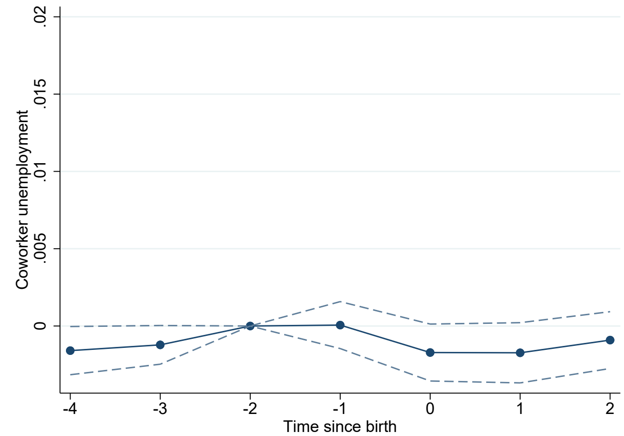
(e) Likelihood of coworker taking a sick day

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.

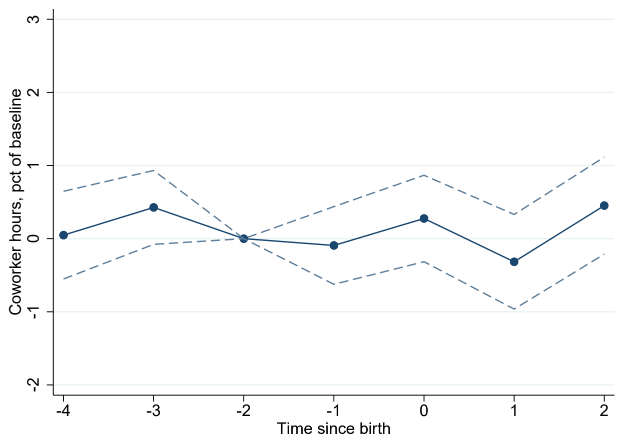
Figure A22: Effects on outcomes of coworkers in different occupations than women on leave, OLS



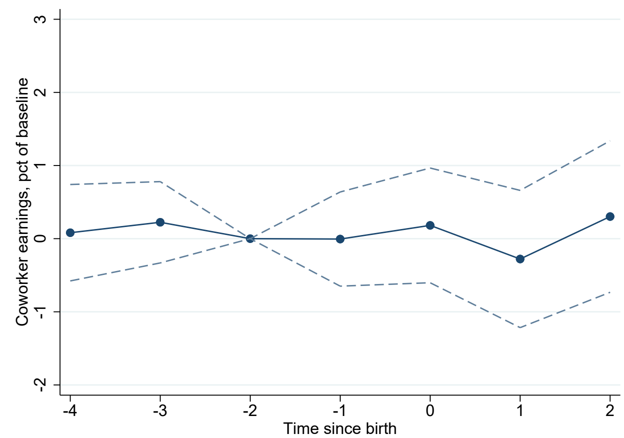
(a) Likelihood of coworker at baseline firm



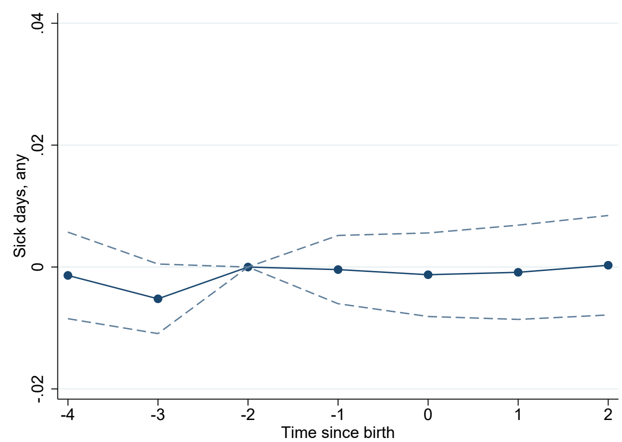
(b) Coworker's share of year unemployed



(c) Coworkers' hours at baseline firm



(d) Coworkers' earnings



(e) Likelihood of coworker taking a sick day

Notes: The dots and solid lines show the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year, which implies that the difference is identically zero here. The dashed lines show the 95% confidence interval based on standard errors clustered at the firm level.