Is Parental Leave Costly for Firms and Coworkers?*

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Abstract

Most existing evidence on the effectiveness of family leave policies comes from studies focusing on their impacts on affected families—mothers, fathers, and their children—without a clear understanding of the costs and effects on firms and coworkers. 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 compare small firms in which a female employee is about to give birth to an observationally equivalent sample of small firms with female employees who are not close to giving birth. Identification rests on a parallel trends assumption, which we substantiate through a set of natural validity checks. We find little evidence that parental leave take-up has negative effects on firms and coworkers overall. Specifically, after accounting for wage reimbursements received by firms offering paid leave, there are no measurable effects on firm output, labor costs, profitability or survival. Coworkers of the woman going on leave see temporary increases in their hours, earnings, and likelihood of being employed but experience no significant changes in well-being at work as proxied by sick days. These limited effects of parental leave reflect that 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 that are less able to use their existing employees to compensate for an absent worker.

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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, 2019), 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, 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" (The San Diego Union-Tribune, 2017).

In this paper, we present some of the first evidence on the impact of parental leave on firms and coworkers. Despite considerable policy relevance, direct estimates of the effects of leave on employers and coworkers are scarce. In contrast to the rich evidence on the effects of parental leave on women and children,[2] a recent review of the literature on leave programs concludes that "we know very little about

[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.

[2] The evidence on the effects of leave programs on women and children is mixed. Previous studies find that short

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 (2019, p.337).[3] 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's 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 compare treatment firms where a female employee becomes pregnant and gives birth and control firms with a female employee who does not give birth over the same time period. We estimate these effects via dynamic difference-in-differences regressions. Recognizing that high-fertility firms may be fundamentally different from low-fertility firms, we match treatment firms with comparable control firms based a rich set of baseline characteristics and follow firm-level outcomes before and after a birth at treatment firms relative to

periods of leave can raise women's likelihood of employment and return to work, but that leaves that are longer than one year can have negative effects on their labor market opportunities (Ruhm, 1998; Baum, 2003; Baker & Milligan, 2008; Lalive & Zweimüller, 2009; Lequien, 2012; Blau & Kahn, 2013; Schönberg & Ludsteck, 2014). Furthermore, the introduction of parental leave improves children's health, education and earnings (Carneiro *et al.* , 2015; Rossin, 2011) but further expansions in the duration of leave have no significant effects on a range of child outcomes (Baker & Milligan, 2010; Rasmussen, 2010; Dustmann & Schönberg, 2012; Dahl *et al.* , 2016; Danzer & Lavy, 2018). Olivetti & Petrongolo (2017) and Rossin-Slater (2019) provide detailed reviews of the literature.

[3]Besides the two papers by Gallen (2019) and Ginja *et al.* (2020) on parental leave extensions that are discussed further below, we are only aware of a handful of policy reports dealing with parental leave and firms. Notable policy reports with a focus on causality include Bedard & Rossin-Slater (2016) and Bartel *et al.* (2016). Bedard & Rossin-Slater (2016) use panel data from California and employer fixed effects to compare firms with varying fractions of workers on leave. They find that an increase in the share of workers is associated with a lower wage bill and slightly higher turnover. In contrast, other work has shown that leave take-up rates are endogenous across firms in California (Bana *et al.* , 2018), making causal conclusions is somewhat challenging. Bartel *et al.* (2016) survey 414 small and medium-sized firms in the manufacturing and food services sectors to study the introduction of a four-week paid leave in Rhode Island. They use a difference-in-differences approach and compare employers in the state to those in neighboring Massachusetts and Connecticut before and after the policy. They find no significant impact on turnover rates, employee productivity, or morale but warn that their small sample size precludes them from drawing definitive conclusions. Other policy reports include Appelbaum & Milkman (2011) and Lerner & Appelbaum (2014) who provide descriptive analyses of in-depth interviews and survey data collected after the introduction of paid family leave programs in California and New Jersey.

control firms. Relying on a parallel trends assumption for identification, this allows us to estimate both the contemporaneous effects of having an employee take leave and any delayed effects that persist or appear over the following few years. This difference-in-differences empirical strategy lends itself to several natural validity checks, such as the testing of pre-trends before birth.

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 at 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 0.18 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.[4] By construction, these firms face constraints on how they can adjust because they cannot rely on increases in hours among same-occupation coworkers. Accordingly, for this subsample of firms, we do see indications of negative effects of parental leave. In particular, despite experiencing slightly shorter parental leaves on average, firms without same-occupation coworkers do not fully compensate for the worker on leave but instead experience drops in total labor inputs; we estimate that in the year the leave starts, total hours drop by 0.33 percent when one percent of the workforce goes on leave. In addition, we see signs that this drop in labor input translates into worse firm performance, although in this smaller subsample we have limited statistical power. These negative effects underscore that firm's labor adjustments play an important role in mitigating the potential costs of parental leave in our overall sample. It also highlights that even if the costs of parental leave are negligible overall, parental leave can be costly for certain vulnerable firms. Finally, we explore heterogeneity by the initial size of the firm. We find little evidence that the costs of parental leave varies with firm size in our sample, however.

The goal of our work is to understand how employees going on parental leave affects 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 work in the nascent research area on the effect of parental leave on firms. Gallen (2019) studies a 2002 policy reform in Denmark that caused mothers already on parental leave to unexpectedly extend their leave from 8 to 10 months on average. Since the original circulation of our paper, Ginja *et al.* (2020) has added further evidence from a similar reform in Sweden in 1989, which extended leaves from 12 to 14 months on average.

These studies differ from our work in two key ways. First, they examine the intensive margin shock

[4]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.

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, 2019). Employee turnover, in itself, is known to have negative effects on firms (Bertheau *et al.* , 2019; Jäger & Heining, 2019). Second, the extensions studied in Gallen (2019) and Ginja *et al.* (2020) were retroactive (i.e., implemented while these 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 7.7.

By focusing on the effects of worker absence due to family leave, our study is also related to the case study of the public health care sector by Friedrich & Hackmann (2019). Friedrich & Hackmann (2019) study a Danish policy reform in 1994, which made generous family leave available to all parents with children up to the age of eight. Because of occupational licensing and high take-up among female nurses, this reform created a temporary nurse shortage. Friedrich & Hackmann (2019) leverage this 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 (Azoulay *et al.* , 2010; Bartel *et al.* , 2014; Bennedsen *et al.* , 2019; Drexler & Schoar, 2014; Golding *et al.* , 2005; Gruber & Kleiner, 2012; Herrmann & Rockoff, 2012; Isen, 2013; Jaravel *et al.* , 2018; Jäger & Heining, 2019; Bertheau *et al.* , 2019; Krueger & Mas, 2004; Mas, 2008). As we discuss later, however, absences due to parental leave differ markedly from other types of worker absences by being temporary with a known end date and by being highly anticipated.

This paper also has connections to other related literatures. By directly estimating the cost of mandated parental leave policies on firms, the paper is related to a large body of literature on how firms may pass on the costs of mandated benefits to workers (e.g., Summers, 1989; Gruber, 1994; Buchmueller *et al.* , 2011; Clemens & Cutler, 2014; Kolstad & Kowalski, 2016; Pichler & Ziebarth, 2018). Lastly, our paper's focus on firm outcomes can be seen as part of a growing focus in labor economics on bringing a firm perspective to the analysis of the labor market (see e.g., Card *et al.* , 2013; Song *et al.* , 2018).

2 Understanding the Impacts of Worker Absences

This section provides a framework to understand the impact of a worker taking parental leave on firm and coworker outcomes based on existing theory and evidence. From a theoretical perspective, if labor markets operate as frictionless and competitive labor markets, the only effect of a worker on leave should be that the firm exactly replaces the lost labor input by hiring a replacement worker. In this case, as labor is replaced at the market wage, there would be no effect on coworkers or firm output. Assuming that firms do not bear any costs related to paid leave, firm costs and thus profits would also be unaffected by workers taking leave.

However, in the presence of costly search or other frictions, the predicted effects of an absent worker are no longer this simple (see, for example, Jäger & Heining, 2019). Under such rigidities, the firm may not be able to replace the worker perfectly or may only be able to do so with a delay or after incurring additional costs. If the firm fails to replace the lost worker immediately, the 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 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 loss of a worker may cause some firms to lay off all coworkers and shut down entirely. In sum, if the labor market is characterized by frictions, the absence of one worker can have important negative effects throughout the firm—on the number of workers, productivity, wages, firm output, profits, and ultimately, firm survival.

Empirically, a large existing literature has examined whether these effects of worker absence exist and how large they are, focusing on a range of different sources of worker absence, including worker deaths (Azoulay *et al.* , 2010; Isen, 2013; Bennedsen *et al.* , 2019; Jaravel *et al.* , 2018; Jäger & Heining, 2019; Bertheau *et al.* , 2019), labor disputes (Krueger & Mas, 2004; Mas, 2008; Gruber & Kleiner, 2012), illness (Herrmann & Rockoff, 2012; Drexler & Schoar, 2014), military reserve call-ups (Golding *et al.* , 2005), and the departure of experienced nurses (Bartel *et al.* , 2014). Broadly speaking, the results of this work are consistent with the presence of significant labor market frictions and with worker absences having important negative effects on firm and/or coworker outcomes.

The key motivation for our empirical analysis of parental leave absence, however, is that the sources of worker absence studied in the previous literature differ conceptually from parental leave in a number of ways—having a worker go on parental leave is very different from having a worker die, suddenly fall ill, or be absent for some other reason.

First, relative to other sources of worker absence, parental leave is highly anticipated and firms can thus plan around absences due to parental leave. The Danish parental leave policy requires mothers to announce their pregnancy to employers at least three months before giving birth, but it is common for women to announce a pregnancy at their workplace up to six months before the due date. This may make it substantially easier to compensate for the worker on leave than for a worker who dies unexpectedly. For example, knowing about the upcoming leave several months in advance allows the firm to start looking for potential replacement workers early and also makes it possible to involve the worker going on leave in the potential recruitment and training process.

Second, parental leave tends to be a temporary and not permanent absence that ends at a specific, known time. The majority of women in Denmark return to their employer at the end of their parental leave period. In fact, in the analysis sample we present later, the turnover for female employees who give birth and take leave is actually slightly *lower* over the next few years than for comparable women who do not give birth. Not having to deal with an employee's permanent departure from the firm implies that some firms may be able to conclude or postpone tasks where the worker going on leave is harder to replace. Meanwhile, the temporary nature of the leave may also impose constraints on the types of adjustments firms can do; for example, downward wage-rigidity makes temporary wage changes impractical.[5]

Third, relative to some other types of worker absence, parental leave is a relatively common occurrence. This implies 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 properties of parental

[5] If nominal wages are downwardly rigid, potentially due to moral costs, firms would find it difficult to raise coworker wages during the absence and then decrease them when the absent worker returns.

leave imply that the effects of parental leave absences may be very different from the types of worker absences studied in the previous literature. This motivates our goal of providing empirical evidence on the effects of parental leave on firms.

3 Institutional Setting: Parental Leave Policies

Danish parental leave, as is typical of most leave policies, 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. 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.[6]

Mothers giving birth during our sample period are eligible for job-protected leave with wage replacement for 4 weeks before birth,[7] 14 weeks immediately after birth, and then have 32 weeks that the parents can share.[8] 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.[9] In addition, women with medical difficulties in pregnancy are entitled to extended prenatal leave.[10]

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.[11] We refer to this as *unpaid leave*, that is, the worker receives a direct wage replacement from

[6]The exact requirements 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.

[7]Women working in particular jobs (typically physically demanding labor), are eligible for an additional four weeks of leave before birth.

[8]Fathers are additionally eligible for two weeks of parental leave immediately after the birth.

[9]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 we focus on leave periods with wage replacement that occur immediately after the child is born in this paper.

[10]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.

[11]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.

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.[12] 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.[13] 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). In addition to maternity leave, most countries provide parental leave that both parents can share. However, the duration and the amount of benefits received under parental leave programs vary substantially across countries. Relative to other European countries, Denmark offers a shorter period of parental leave (32 weeks) but provides higher earnings replacement during that period. The Danish system of encouraging firms to offer paid

[12]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 we return to later, most of our analysis examines a balanced panel covering births from 2005 to 2011 so the vast majority of births in our sample occur when membership of the parental leave funds was mandatory. Firms are required to pay into a parental leave fund for all employees regardless of gender and age.

[13]Based on the treatment firms and women in our estimation sample (see Section 5), 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.

leave but then reimbursing them for these expenses is somewhat unusual; most countries offer wage replacements that are directly funded and paid out via the social insurance system. But these funding differences are unlikely to matter, because employers never bear the direct costs of replacing the wages of women on leave.

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 countries. Firms also frequently hire temporary workers.

4 Data

Our administrative data were collected from several sources and cover the universe of Danish firms and workers from 2001 to 2013. Data on workers are linked across the different sources using unique person identifiers from the central person registry (CPR). For firms, we link the data using firm identifiers from the central firm registry (CVR). These identifiers are required for tax purposes for nearly all active firms and public workplaces and enable us to merge our employer-employee data with firm-level outcomes, such as output and profitability.[14] 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).

4.1 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

[14] Participation in the CVR registry is required for all firms with a yearly revenue above 50,000 DKK (about 6,700 EUR or 7,500 USD).

leave for each worker.[15] 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). Finally, using data from the central education register and the Integrated Database for Labor Research (IDA), we obtain detailed measures of workers' education and their total labor market experience since labor market entry.

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."[16] 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.[17] We refer to the total number of such workers as the number of *employees* at the firm.[18] 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 changes in hours worked. To do this, we construct an approximate measure of how many hours each worker supplied to a firm by using 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. When constructing the measure of hours, we scale contributions so that hours are measured in full-time equivalent (FTE) workers. 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.

[15] 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. Prenatal leave includes pregnancy-related sick leave. 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.

[16] Historically, the IDA data were designed to most accurately capture employment at the end of the last week of November.

[17] The main job is defined as the job with the most hours, and in the case of any equal amounts, that with the highest earnings.

[18] The results we present later are virtually identical if we instead include all workers who were ever at the firm in any capacity during the year.

When analyzing the resulting measure of yearly hours, it is important to note that the assumed linear relationship between weekly hours and pension contributions is only an approximation. The true relationship is in fact a stepwise function with four steps that tops out for full-time employees.[19] 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.

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. We return to this later when discussing the results.

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.[20] 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.[21]

[19] 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. For this reason, we also cannot calculate a reliable measure of hourly wages. An alternative data source that better captures overtime hours is available from 2008 and onward. Relying on this would leave us with too few observations for the analysis however.

[20] 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.

[21] 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).

4.3 Firm Data

Information on firm performance is taken from value-added tax (VAT) data. As part of administering the Danish VAT, all firms are required to report their total sales and purchases if the revenue exceeds a defined value.[22] We use total *sales* as our measure of firm output and use firm *purchases* for an identification check.[23] To examine the possibility that firms may in part compensate for workers on leave by buying more services from other firms, we also create an approximate measure of *total variable costs* by adding firms' total purchases to the wage bill ex. leave.

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. We note that this proxy differs from the standard accounting definition because the VAT data on purchases also include purchases of capital equipment, which would not normally be included when calculating gross profits.[24]

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. In other words, when estimating the effects of parental leave on these outcomes, we allow firm shutdown to be one reason why employees, hours, or sales may change. Using positive sales as a proxy for firm activity, we also examine firm shutdown directly as an outcome.[25]

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 with a setup that is similar to Jäger & Heining (2019). In essence, this research design leverages a comparison of treatment firms that employ a female worker who gives birth with control firms that employ a female worker who does not give birth over the next few years.

[22] 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.

[23] Due to reporting errors and issues around accounting corrections, there are a few instances of firms reporting negative sales and/or purchases (less than 0.2 percent). We recode these as zeros.

[24] 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.

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

In this section, we begin by describing the construction of our treatment and control firms and then our firm-level and coworker difference-in-differences empirical specifications. We address several considerations of the analysis, including the intent-to-treat nature of our research design, the comparability of the treatment and control firms, and the possible threats to identification.

5.1 Constructing the Treatment and Control Groups

To start our discussion of the construction of treatment and control groups, we first define what we refer to as *potential events*. A potential event is defined as a woman who had her main job at some firm in some year. In other words, events are combinations of woman-firm-year. For definitional purposes, the year in this combination is called the *baseline year* and the year two years after the baseline year is the *event year*. These events are determined at the individual and not the firm level because 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.[26] 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. Hotz *et al.* (2017), for example, have found evidence that Swedish women may sort into certain types of firms in the year just prior to giving birth, although Kleven *et al.* (2019) and Pertold-Gebicka *et al.* (2016) find no evidence of this behavior in the Danish setting we focus on.[27] Importantly, none of these papers find evidence of systematic sorting two years prior to childbirth, which is why we choose this as the baseline year for our analysis.

[26] 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 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.

[27] Based on event studies around first birth, both Hotz *et al.* (2017), Kleven *et al.* (2019) and Pertold-Gebicka *et al.* (2016) find that women's labor supply and propensity to work at certain firms evolve along a stable trend until the year immediately before their first birth. For Sweden, Hotz *et al.* (2017) then find a sharp change occurring in the year immediately before childbirth. For Denmark, however, both Kleven *et al.* (2019) and Pertold-Gebicka *et al.* (2016) find that the stable trend continues until the year of first birth. See also Appendix B.

As described previously, the full data set we use spans the period 2001 to 2013. Because our definition of treatment and control events requires observing women two years before and one year after the birth event, the base sample of treatment and control events span the years 2003 to 2012.

5.2 Sample Restrictions

We place several restrictions on this set of potential leave events—both at the individual and the firm level. Of these restrictions, the most important are a focus on women in their prime childbearing years and a restriction to small private firms with less than 30 employees.

We restrict attention to small firms for two reasons. First, the potential disproportionate impacts on small firms often crowd public discussions of parental leave, and thus justify a particular focus on them. Second, our research design is based on comparing firms that differ in whether one particular employee is on parental leave. At medium and large firms, differences in the behavior of a single employee will not generate meaningful variation. The restriction to small firms is therefore also invoked in previous studies on worker absence that use related research designs (Jäger & Heining, 2019).

In terms of other restrictions, we impose the following restrictions on the women who make up our treatment and control events:

1. The woman must be between 19 and 33 years of age in the baseline year.

2. At the baseline year, the woman must have been with the firm for more than one year.

3. The woman must not be a student in the baseline year.

Restriction 1 ensures a focus on prime-childbearing-age women.[28] Restrictions 2 and 3 ensure a focus on women with reasonably strong labor market attachment. Second, we impose the following restrictions on firms:

4. Based on sales, hours, and the total wage bill, the firm must be active at baseline.[29]

5. The firm must not be an extreme outlier in terms of growth, sales levels, or wage bill.[30]

[28]As described above, we examine the effect of women's fertility two years after the baseline year. Women aged 21 through 35 account for 83 percent of all childbirths in Denmark over our sample period.

[29]Specifically, 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.

[30]Firms with outlier sales or wage bills relative to their employment are excluded. 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).

6. We restrict our sample to small firms in which our measure of the stock of employees is between 3 and 30 in the baseline year, and where the total number of employment relationships observed at the firm at some point in the baseline year is less than 60.[31]
7. The firm must be in the private sector.

Restrictions 4 and 5 ensure that our results are not driven by a small number of outlier firms or by firms with very little activity in the baseline year. As noted previously, Restriction 6 implies that we are looking at small firms. Restriction 7 is necessary because our measures of firm performance (sales, firm closure, and profits) are not relevant outcomes for the public sector.[32]

5.3 Firm-Level Difference-in-Differences

Using data on the treatment and control events (along with data preceding, during, and following the events), our dynamic difference-in-differences specification has the following basic form:

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

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 event years $t=-4$ through $t=2$ relative to the baseline year, i.e. $t=-2$.

Note that the issues with heterogeneous treatment effects in difference-in-differences designs discussed recently (e.g., Abraham & Sun (Forthcoming); De Chaisemartin & d'Haultfoeuille (2020); Goodman-Bacon (2020)) do not apply here. All of those papers focus on two-way fixed effect re-

[31] Recall that our main measure of the stock of employees is based on the workforce in November. The 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.

[32] 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.

gressions with calendar time fixed effects. In contrast, our regression specification does not rely on calendar time fixed effects.[33]

The parameters of interest are the coefficients on the interactions between treatment status and event time: β_{-4}, β_{-3}, β_{-1}, β_0, β_1, and β_2. 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 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. β_{-1} identifies any anticipation effects of a birth that materialize in the year prior to the birth's occurrence. For example, at the firm level, management may make adjustments in this year in anticipation of the leave. An advantageous feature of the dynamic difference-in-differences research design is the ability to test one of the crucial identifying assumptions—the parallel trends of the treatment and control events. The coefficients β_{-4} and β_{-3} serve this purpose and ideally should hover around zero—signifying that outcomes among treatment and control firms evolve along parallel trends in the periods before the birth occurs. We return to our pretrend estimates in Subsection 5.6.3 after discussing the rest of our empirical strategy, including the baseline covariates we condition on to make treatment and control events comparable.

For each event e, we use data ranging from four years prior to the event to two years following the event in the estimation.[34] The main reason for this time span is to ensure a clean comparison between the treatment and control groups. This is because our design mainly leverages the timing rather than the incidence of childbearing at a firm. Although women who contribute to control events are restricted to having no births from event year -1 through 1, they are still likely to give birth at some later date. If we expand the event window to include years that lie further into the future, the parental leave taken by these women at control firms would make it difficult to interpret differences between the treatment and control samples.

In our analysis, we estimate equation (1) via OLS and compute standard errors clustered at the firm level. This is appropriate as the level of treatment is at the firm level (Abadie *et al.* , 2017). The

[33] As shown in Table A3, our setting and our matching and reweighting procedure implies that the calendar year in which events occur does not differ systematically between the treatment and control samples (i.e., event year is balanced across the treatment and control samples). Our approach is similar to the stacked regression approach in the study of minimum wages by Cengiz *et al.* (2019).

[34] We could, in principle, extend the time frame to include more years prior to the baseline year and/or more years after the event year. Requiring data on more than these seven years causes us to lose a significant number of observations at the beginning and end of our sample window.

clustering also corrects for the fact that the same firm may be part of more than one event in our data. To avoid any issues with composition effects, we always estimate equation (1) on a balanced sample that includes only firms for which we have data for all seven years.[35] Whenever possible, we scale the outcome variable in equation (1) relative to its baseline value so that our estimated effects can be interpreted as percentage changes relative to the baseline year.[36]

5.4 Coworker Analysis

To understand the effects of 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.[37,38]. For this sample, we estimate OLS specifications that are completely analogous to (1), but where the outcome variable is some coworker outcome (earnings, hours, etc.) and where observations are at the coworker-year level (instead of firm-year). For this set of analyses, the units of the outcome are the individual coworkers. For inference, we continue to cluster standard errors at the firm level. Appendix D provides additional details for the coworker specification.

5.5 Discussion of the Treatment Leveraged in the Research Design

Although our focus is on the effects of parental leave, we note that our definition of treatment is based on whether an employee gives birth or not. In our setting, a birth is always followed by a parental

[35] Recall that firms, even if they shut down, are still included in our sample. The balanced sample restriction simply implies that we do not include firms at the beginning or end of our sample window where we have missing data on event time $t = -4$ or $t = 2$. All our main conclusions hold if we instead consider an unbalanced panel.

[36] Despite our restriction to small firms, the firms in our analysis do differ substantially in size in the baseline year. When measured in levels, year-to-year changes in outcomes therefore exhibit considerable skewness. Because our outcome variables contain zeros, however, we cannot apply the usual log-transformation to mitigate this. Instead we scale outcomes relative to baseline whenever possible. We cannot do this for all outcomes, however, because some of our outcome variables can be zero or negative in the baseline year. Except where noted, none of our qualitative conclusions are sensitive to the scaling.

[37] We make this restriction to confine the sample to those 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.

[38] 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.

leave period and the duration of this leave is very substantial in most cases.[39] Formally, our design thus estimates the joint effect of an employee giving birth and of her subsequent parental leave. Given our interest in the effects of parental leave, this raises a concern that our estimates will conflate the effects of parental leave with the potential negative effects of women lowering their working hours or effort after giving birth. Indeed, much previous work has found evidence that women's labor supply changes after giving birth (Kleven *et al.* , 2019).

In Appendix B, we examine how the labor supply of our sample of treatment women changes around the time they give birth relative to our sample of comparable control women.[40] Besides the differences caused by the parental leave itself, treatment women change their labor supply very little. Two years after the event year—the year when our treatment women should be fully back to work—the drop in yearly work hours for treatment women is less than one percent of a full-time worker. Moreover, because women are less likely to quit their job immediately around childbirth, treatment women are in fact slightly *more* likely to still be with their baseline employer two years after the event year.[41] The fact that women's labor supply changes so little after childbirth likely reflects that we are focusing on women with reasonably high labor market attachment in the baseline year.[42] Given these very limited changes in labor supply, we expect that any negative effects on firms stemming from changes in the labor supply of new mothers will be dominated by the direct effects of the parental leave period. During the parental leave period, firms have to go *completely* without the women's labor input during the almost 10 months the average woman is on parental leave.

[39]As required by European Union law, women must take at least two weeks of parental leave following childbirth and families are only guaranteed publicly provided childcare after the child is 26 weeks old. Moreover, the leave length is substantial for most women; 93 percent of the women in our sample take at least six months of postnatal leave and 34 percent take the maximum duration of 322 days (46 weeks).

[40]We use a specification analogous to the firm-level difference-in-differences. To ensure that the sample of treatment and control women are comparable, we condition on observables in the same way as in our main analysis; see Subsection 5.6.2.

[41]This might seem puzzling given 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)). Part of the explanation is that much of this sorting pattern does not occur immediately after birth. More substantially, however, note that the sorting of new mothers into certain jobs and firms does not require them to have higher quit rates than other women. If new mothers are systematically more likely to switch to certain firms and jobs when they do quit, sorting will occur even if new mothers quit at exactly the same rate as other women. This is indeed what we see in our sample: women do not become more likely to quit following a birth but target different jobs and employers if they do quit.

[42]Note also that we are not restricting the analysis to only first births as has been done in much previous work.

5.6 Important Considerations for the Research Design

5.6.1 Turnover and Imperfect Compliance: Obtaining LATE Estimates via 2SLS

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 we recognize that 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, the OLS estimates from the difference-in-differences specification in equation (1) can still be interpreted as causal intention-to-treat (ITT) estimates of how parental leave influences firms. Because we are also interested in quantifying the size of these effects for affected firms, however, we supplement our OLS results with standard 2SLS estimators. In our 2SLS estimates, we use treatment status as an instrument and recover local average treatment effects (LATE) estimates, even under imperfect compliance.

To set the stage for our specific 2SLS specifications, consider first a differenced version of equation (1) that looks at differences across only the baseline and event years (denoted by Δ):

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

This regression relates changes in the outcomes between the baseline and the event years to treatment status. 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 the sample of firms is kept the same.[43] To obtain a LATE estimate for the effect of an additional birth at the firm, we instead apply 2SLS. To do this, we replace $Treatment_e$ in equation (2) with the total number of births in the event year, $BirthsInEventYear_{ef}$, and instrument this using treatment status:

$$\Delta Y_{ef} = \rho_0 + \tau_0 BirthsInEventYear_{ef} + \Delta\xi_{ef} \tag{3}$$

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

[43] 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 $t = -4$.

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. To obtain LATE estimates of any non-contemporaneous effects in the year after the event year as well, we simply modify the outcome equation (3), so that it involves changes in the outcome variable between the baseline and the year of interest.

5.6.2 Ensuring the Comparability of Treatment and Control Events: Conditioning on Observables via Matching and Reweighting

The difference-in-differences design employed in our firm and coworker analyses requires that our treatment and control firms—in the absence of a female worker giving birth—exhibit parallel pre-trends. In the raw data, however, the firms and women underlying treatment and control events are already quite distinct in the baseline year. One of the obvious differences is the composition of the firms—i.e., firms experiencing treatment events are more likely to have employed more women.

These baseline differences are not surprising. It is well-established that births and fertility timing are highly correlated with a range of individual characteristics. Moreover, women may sort systematically into certain types of firms based on these characteristics or directly on expected future fertility. Similar baseline differences also exist in other work using related research designs. In their study on the effect of unexpected worker deaths on firms, for example, Jäger & Heining (2019) face the issue that sources of unexpected mortality correlate strongly with a number of individual characteristics. The existence of such baseline differences raise obvious concerns about the validity of the parallel trends assumption. As in Jäger & Heining (2019), we address these issues by conditioning on a rich set of baseline observables through a matching and reweighing procedure. The point of this covariate balancing is to ensure that in the baseline year, firms in the treatment sample look similar to our control sample.

Before estimating the regression specifications discussed above, we first apply a matching and reweighting procedure to our sample. As we show in Appendix E, however, we can obtain similar results using a purely regression-based approach using baseline covariates as control variables.[44]

[44] This is a reflection of the well-known equivalence between matching and reweighting estimators and linear regression (Angrist, 1998). See Appendix E for details.

Because movement across firms may be related to childbearing our matching procedure uses baseline characteristics (i.e., characteristics two years prior to the event) rather than characteristics at the time of the event. Table 2 details the set of baseline characteristics we condition on. 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 particular, female fertility behavior might be related to labor market returns and is therefore important to include. In terms of firms, we condition on standard measures of size and various proxies of family-friendliness. We do this especially to address that high-fertility women may sort into certain types of firms.

Our estimation procedure relies on exact matching. As our baseline matching variables are discrete, we create cells based on all possible combinations of these variables. For each treatment event, we determine all of the control events 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 accordingly. When a treatment event is matched to K control units, then each of these controls receives a weight of $\frac{1}{K}$. These weights are applied throughout when estimating our regression specifications.

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 trim away 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, thereby improving the estimator's performance. The downside of this procedure is a potential loss of external validity and sample size, as we restrict attention to a particular subsample of the data.

5.6.3 Assessing the Parallel Trends Assumption

A useful feature of our research design is that it offers some natural validity checks to assess the identifying parallel trends assumption. First, we can conduct treatment-control balance tests of pre-determined characteristics excluded from the matching and reweighting procedure. This allows us to assess whether treatment and control firms indeed look similar at baseline once we condition on the observables included in our matching and reweighting procedure. Appendix Table A3 displays this comparison. Across all variables there are only small, insignificant differences between the treatment and (reweighted) control sample. We note that this is also true in terms of when the potential treatment and control events occur: treatment and control events are not occurring in systematically different years (see last row of A3). This ensures that our comparison of treatment and control samples is not confounded by aggregate time trends.

Appendix Figure A1 further compares the industry composition of our treatment and control samples. The samples are well balanced on industry as well. Formally, the differences in the industry distribution across the two samples are not statistically significant ($p = 0.92$; see Appendix Figure A1). At least in terms of these baseline characteristics, the women and firms underlying the treatment and control events are quite comparable.

Second, we can assess the validity of our parallel trends assumption by looking at pre-trends in outcomes. One specific potential economic-driven violation of the parallel trends assumption is the endogeneity of birth timing. Firm-level events may influence employees' birth timing. A positive demand shock at the firm, for example, could translate into promotions or pay raises for employees and lead these employees to start or postpone having children, thereby introducing reverse causality between firm outcomes and births. The test of these pre-trends is critical for dispelling that worry. Reassuringly, we see no evidence of differential pre-trends in any of the firm-level outcomes we consider (see Figures 3 to A8d).

Aside from testing for trends in firm-level outcomes, we also look at trends in the evolution of outcomes for treatment and control women. We again see no evidence of differential pre-trends here (as we show in Appendix B). This is consistent with the event-study results for Danish women presented by Kleven *et al.* (2019) and the work of Pertold-Gebicka *et al.* (2016) studying transitions from private to public sector employment in Denmark.[45] The lack of pre-trends may reflect the combination of several

[45]Broadly speaking, the event studies in Kleven *et al.* (2019) have the following flavor: Women's labor market outcomes evolve along fairly stable trends in the years leading up to their first birth. Then follows a few years with potentially very distinct patterns in the years the women become pregnant, give birth and take parental leave. Finally, in the post-leave period labor market outcomes settle on a new stable trend. As our matching and reweighting procedure conditions on past fertility, our research design here tends to leverage two types of comparisons: Some comparisons will be among pairs of women where both are on the stable trend occurring after the first birth but where one (treatment) women gives birth again over the next few years and the other (control) woman does not. Other comparisons will be among pairs of women who are both on the stable trend occurring before first birth. Of course, because we do not condition on future fertility in the control group, our research design will also leverage some comparisons where the control woman never gives birth. As shown in the Appendix to Kleven *et al.* (2019), however, these comparisons also tend to show parallel pre-trends.

factors: the fact that strategically planning childbearing is challenging for a woman, the unplanned nature of many births, women not anticipating the employment effects of motherhood (Kuziemko *et al.* , 2018), and the refined nature of our contrast of treatment events with similar control events.

It is worth noting that even in the presence of some endogeneity bias for the treated individual, propagation of that endogeneity to the firm level would be less than that at the individual level. In the language of a regression equation, this is because the correlation of the regressors with the error term, which drives the endogeneity bias, would only be generated by the treated individual and would be zero for the non-treated individuals in the firm. Of course, this logic hinges on a lack of peer effects in fertility decisions at the firm; if fertility peer effects across coworkers are strong, endogenous fertility decisions by one employee will spill over to the entire workforce and lead to severe bias. In our setting, fertility peer effects appear negligible, however. In Appendix C, we show that the estimated effect of a female worker's birth on her coworkers' leave taking is less than one day per coworker in the event year and the following year, and it is statistically insignificant.[46]

5.6.4 Alternative Treatment Definitions

Our main regression equations characterize the treatment in terms of number of births at a firm (i.e., equation (3)). One could well imagine, however, that what matters for the outcomes is not the total number of employees on leave but instead the *share* of the total workforce on leave. We therefore consider an alternative parameterization of the treatment effect that takes the firm's number of employees into consideration. Specifically, we consider a relative version of equation (3), in which births in the event year are divided by the number of employees at the firm at the baseline and in which the same scaling is applied to the first stage:

$$\Delta Y_{ef} = \pi_0 + \theta_0 \frac{BirthsInEventYear_{ef}}{BaselineEmployees_{ef}} + \Delta u_{ef} \tag{4}$$

$$\frac{BirthsInEventYear_{ef}}{BaselineEmployees_{ef}} = \delta_0 + \delta_1 \frac{Treatment_e}{BaselineEmployees_{ef}} + v_{ef} \tag{4, First Stage}$$

[46] Asphjell *et al.* (2014), using Swedish data, uncover workplace peer effects in fertility behavior but the peer effects occur with delay—affecting the probability of a birth 1 to 3 years later. Because of the time frame of these peer effects and given we only focus on four years prior and two years following a birth, these type of peer effects, if they existed in our setting, would be less problematic for us. Also, in this vein of the literature, Ciliberto *et al.* (2016) develop a game theoretic peer effects model to understand the propensity in having a birth but not the timing of that birth.

For ease of interpretation, we measure baseline employment in 100 baseline employees when estimating (4). This scaling implies that our estimate of θ_0 is a LATE estimate for the contemporaneous effect of having one percent of the baseline employees give birth and take leave in the event year.[47] 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 (4).[48] As above, we can modify outcome equation (4) so that it involves changes in the outcome variable between the baseline and some year other than the event year in order to estimate the effect in later years.

We take 2SLS estimates from (4) as our preferred estimate for all our firm outcomes that are binary or are measured relative to baseline. For transparency, however, we always present results from both specifications (3) and (4) throughout (i.e., considering the number of births as the treatment variable as in (3) or the percent of workers on leave as in (4)). For continuous firm outcomes that are not scaled relative to the baseline, we use 2SLS estimates from equation (3) as our preferred estimate, as they measure the absolute effect of having one additional employee take leave. Finally, 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.

5.6.5 Additional Comparisons with Previous Work

As noted, the dynamic difference-in-difference approach that we use here is in many ways very similar to that employed by Jäger & Heining (2019) in their study of the effects of a coworker's death. We therefore finish this section with some additional discussion of differences and similarities.

Like us, Jäger & Heining (2019) define a sample of potential (death) events each involving a firm, a worker and a baseline year. Treatment and control events are then defined by whether the worker later dies in the event year. Similar to us, Jäger & Heining (2019) then estimate a dynamic difference-in-difference specification on this sample of events. The identifying assumption requires parallel trends between treatment and control in the absence of the worker dying. Finally, as discussed in Section 5.6.2, both our analysis and Jäger & Heining (2019) condition on a set of baseline observables to address the

[47] The average firm in our sample has approximately ten employees, so a typical complier firm actually experiences ten percentage points more of their employees go on leave in the event year.

[48] The matching and reweighting procedure ensures that the baseline number of employees is balanced across the treatment and control groups. The identification of θ_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.

fact that both fertility and unexpected mortality are strongly correlated with individual characteristics.

There are however also some notable distinctions between our setting and that in Jäger & Heining (2019). A key distinction is the anticipated nature of the birth events we study. In their study, Jäger & Heining (2019), deliberately focus only on *unexpected* death events. While all employers are obviously aware that their workers may die - and may also have expectations about which employees are more at risk than others—Jäger & Heining (2019) are very careful in selecting only death events in which the firm does not receive any concrete forewarnings that the worker is about to die. This is important because a major aim of Jäger & Heining (2019) is to inform theory by testing for the substitutability of different types of workers in production. The ideal experiment for this test involves situations where firms lose a worker with no advance warning. The aim of our study is fundamentally different. To inform policy, we are interested in measuring the typical effect on firms when a worker gives birth and goes on leave. Such an event will always involve the employer learning about the upcoming leave some months in advance and as discussed in Section 7.7 , this advance notice may be one reason that parental leave absences are less costly than other types of leave. By construction, our estimates will include these policy relevant effects.

A final difference between our setting and that of Jäger & Heining (2019) is the frequency with which the event of interest occurs. While worker deaths happen quite rarely, workers giving birth and going on leave is a common occurrence. Indeed, in our analysis sample, both treatment and control firms will likely to all experience a worker giving birth at some point over the course of their existence. As noted, firms' previous experience with leave-taking could be one reason that parental leave only incurs a relatively small adjustment cost. The frequency of birth events, however, also has two implications for the setup of our analysis. First, it necessitates our restriction to only estimating the effects of a birth up to two years after the birth occurs. At longer time horizons, control firms may be likely to have more births because of our sample restrictions (i.e., a control event is one where a woman does not have a birth for three consecutive years). Second, while the comparison in Jäger & Heining (2019) is based on the fact that most treatment firms experience a death in the event year while control firms never do,[49] our analysis allows for control firms to also experience worker births in the event year. Indeed, control firms in our analysis experience on average 0.62 births in the event year. Accordingly,

[49]Note that Jäger & Heining (2019) face the same issue of turnover and imperfect compliance that we do. Some treatment workers who later die, leave their baseline firm before the event year. This implies that not all treatment firms actually experience a worker death in their analysis.

our analysis is based on the fact that our treatment firms experience *more* births than our control in the event year. This is why our 2SLS specification includes the number of workers giving birth as the variable of interest rather than a dummy for a single birth occurring. In the absence of imperfect compliance—if no women left their baseline firm before giving birth—we would expect treatment firms to experience exactly one more birth in the event year relative to control firms. As discussed later, in our first stage results, treatment firms in fact experience 0.68 *additional* births in the event year relative to control firms.

6 Descriptive Statistics

Before discussing our main difference-in-differences results, we present descriptive statistics. The first row of Table 3 shows our baseline sample of events, after imposing restrictions on the woman involved in each event. This baseline sample consists of 1,147,108 control events and 199,229 treatment events. These treatment events cover 16 percent of all births occuring in Denmark 2003–2012.[50]

The next rows of Table 3 show how our working sample size changes after we impose restrictions on firms. Not surprisingly, the most limiting sample restriction here is the restriction to small firms. This large reduction in sample size reflects that by definition, most workers work at firms with many employees.[51] The fact that we lose observations due to trimming reflects the fine-grained nature of our matching and reweighting procedure. Of the initial 23,734 treatment events in our sample, 9,934 (41.8 percent) are left after trimming. In Appendix F, as a robustness check, we further present results from a coarser matching and reweighing procedure that effectively trims fewer observations.

While the final sample consists of 9,934 treatment and 21,974 control events, we note that only 16,080 unique firms appear in the sample. This reflects that our sample construction is based around potential birth events as described in Subsection 5.1. Because some firms employ more women (potential mothers), some firms naturally appear more often in the base population of potential events as well as in our sample.[52] Throughout our main analysis, our statistical inference is automatically corrected for

[50]The reason that the treatment sample only covers a fraction of all births is the set of restrictions we impose on the women giving birth, in particular the restriction on birth timing and the focus on non-students with a strong labor market attachment (see Subsection 5.2).

[51]Given that women are disproportionately more likely to work in the public sector, it might seem surprising that the restriction to the private sector only cuts our sample by 2.0 percent. The reason for this is that public sector firms tend to be very large in our data. Most of the public sector had already been dropped before we restricted our attention to small firms.

[52]Of the firms in our sample, 58.2 percent show up once, 19.8 percent show up twice, while 22.0 percent show up three

this firm duplicity because we cluster standard errors at the firm level. In Appendix G, we additionally examine how results change if duplicate firms are excluded in the sample construction. Removing duplicate firms leads to the same pattern of results as in the main text but because of the smaller sample size, estimates are noisier.

In Appendix H, we examine the characteristics of our analysis sample to understand how representative our final analysis sample is. Compared to the universe of private sector firms in Denmark, firms in our treatment sample experience more births and leave days per employee. Moreover, treated firms employ twice as many women than the average small firm. This is not surprising; the more women a firm employs, the more likely it is to have an employee that gives birth. Other characteristics of firms in our treatment sample—such as work hours and the wage bill—are mostly comparable to the universe of private sector and small firms.

Table 4 shows (weighted) summary statistics for the baseline year of the firm and coworker samples that we use to estimate equation (1).[53] 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 EUR or 500,000 USD). Finally, Figure 2 shows the distribution of the respective length of prenatal and postnatal leave among the women in our sample of treatment events. 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.

7 Results

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 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.[54] In terms of

or more times.

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

[54]As described in Subsection 5.3, this specification is estimated by OLS on the reweighted sample of treatment and control events.

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.[55] The apparent lack of pre-trend differences in this figure is comforting for identification purposes.

The magnitude of the increase in the event year reveals that there is imperfect compliance with treatment. In the event year, the relative increase in births at treatment firms is only 0.68—significantly less than one. As discussed in Subsection 5.6.1, this imperfect compliance reflects that some baseline employees at treatment firms leave their firms before giving birth. 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.[56]

Having established that treatment firms experience more births in the event year, we next examine how this affects leave take-up. 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. However, because of imperfect compliance, these OLS estimates capture ITT effects and understate the actual number of leave days that a firm experiences when a current employee gives birth.

As described in Subsection 5.6.1, we use a set of 2SLS specifications, equations (3) and (4), to correct for this and obtain LATE estimates. For total leave take-up, these 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. Because total leave is measured in levels, columns (1) and (2) are our preferred specifications. When one additional female employee gives birth in the event year, total leave days at the firm increase by 196 in the event year (column (1)) and 86 in the following year (column (2)). Adding these up, we thus see that treated (complier) firms in our 2SLS specification are experiencing an additional employee going on leave and being absent for 282 days or about nine and a half months in total. This aligns well with aggregate statistics indicating that the average woman in

[55] 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.

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

Denmark takes a little less than ten months of leave in connection with childbirth.[57]

7.1 Labor Adjustment: Extensive Margin

The results in the previous subsection show that when an employee gives birth and goes on leave, the employing firm loses her labor for an extended period. We now examine whether and how firms' total labor inputs respond to this loss of labor. We start by examining extensive margin responses. 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 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 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. We see that 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 going on leave. Panel (c) of Figure 4 shows corresponding results for turnover, defined as the number of employees leaving the firm relative to the previous year. Focusing on the event year only, we see that turnover drops when an employee goes on leave. 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.[58]

Looking at hiring and turnover beyond the event year, we see that turnover increases to above the baseline level one year after the event year, while new hires drop slightly below the baseline level. This

[57] This also stresses the fact that women take the majority of parental leave in Danish families; recall that mothers and fathers together have a total of 46 weeks of postbirth leave. Thus, while women can take less than the maximum amount of leave (and some do as shown in Figure 2), the magnitude of the effect on parental leave is consistent with women at the complier firms taking close to the maximum amount of leave.

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

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 someone 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 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 seen previously, we estimate that an employee 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 goes on leave thus do not replace existing employees in the longer term. Columns (3) and (4) of Table 5 quantify the retention effects on coworkers. The 2SLS results here suggest that when one percent of the workforce 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. Both effects are significant in the 2SLS specification.

7.2 Labor Adjustment: Intensive Margin

Aside from hiring temporary workers and reducing turnover of existing employees, firms can compensate for labor supply losses by making changes at their intensive margin. Specifically, treated firms might increase work hours for coworkers of women who take parental leave. Panel (a) of Figure 5 presents OLS estimates for the impact of parental leave on our approximate measure of hours of work in the coworker sample.[59] We detect a small but statistically significant increase in the event year, suggesting that when a worker takes leave, firms increase the coworkers' hours. The 2SLS estimates in Panel B of Table 5 quantify this effect. When one percent of the workforce goes on leave, existing coworkers' hours increase by 0.10 percent in the event year (column (3)). As discussed in Section 4, 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.

[59]Recall the measure of hours does not cover overtime hours for full-time employees. Therefore, any increase in hours worked at the firm comes from workers going from reduced time to less reduced time (e.g. full-time).

7.3 Net Effect on Labor Inputs

The previous results show that when an employee goes on leave, 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 goes on leave. The figure 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. Our 2SLS specifications in Table 5 also show no statistically significant effects on hours. Point estimates suggest that when one percent of the workforce goes on leave, total hours decrease by only 0.048 percent in the event year (column (3)) and increase by 0.050 percent in the following year. Our corresponding 95 percent confidence intervals exclude a total drop in hours exceeding 0.19 percent in the first year. Overall, firms appear to counteract the loss of labor that occurs when an employee goes on leave very effectively. Indeed, due to the nature of our approximate hours measures, this slightly negative estimate may be an upper bound on the actual number of lost hours.[60]

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 I. We find small effects on different measures of labor quality, which go in opposite directions in the event year: when a worker 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.

7.4 Labor Costs and Earnings

We next examine how an employee going on parental leave affects firms' labor costs. A firm may have to compensate existing workers for extending their work hours, which can subsequently raise its wage bill. On the other hand, firms might pay temporary workers lower wages than women on leave, leading to lower costs.

We start our analysis by examining firms' total wage bills. As discussed in Sections 3 and 4, this includes wages paid to workers on leave and thus reflects the costs firms face before being reimbursed

[60]As discussed in Section 4 our hours measure will miss overtime hours for full-time workers and smaller hours adjustments for part time employees. It very accurately captures if employees are not working during some part of the year as is the case when on parental leave. When looking at the net effects on firms' total hours we will thus capture all of the lost hours due to parental leave but may miss some extra hours put forth by existing employees to compensate.

for any paid leave. Panel (a) of Figure 6 shows OLS estimates for the effect of parental leave on firms' total wage bills. When a worker 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 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. As discussed in Section 3, Danish firms are almost fully reimbursed for the costs of paid leave, so 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. Instead of an increase in labor costs, the wage bills excluding paid leave shows no statistically significant change and the point estimate is actually 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 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 leave-taking on coworkers' earnings. Coworker earnings increase significantly in the event year, and there are some indications that this effect persists over time. In terms of magnitudes, 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 goes on leave. This increase mirrors the increase in coworker hours documented earlier.

Finally, we examine whether there is evidence that firms use outsourcing to compensate for workers on leave by buying more services from other firms. 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. We see no indications of an increase in 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 goes on leave (column (3)).

7.5 Firm Performance and Coworker Well-Being

Finally, we examine the effect of parental leave take-up 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 on parental leave on firms' output, as measured by total sales. We see no indication that output is negatively affected by leave take-up. 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, we also see no indication that leave take-up affects profits (Panel (b) of Figure 7), although we note here that estimates are less precise, likely because our measure of profits is quite noisy.

In Panel (c) of Figure 7, we look at the impact of leave 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 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 parental leave has detrimental effects on overall firm performance.

Turning to the overall effect on coworker well-being, the previous results suggest that if anything, an employee's going on parental leave has positive effects on coworkers' labor market outcomes: their unemployment risk falls, while their hours and earnings increase. A potential concern here, 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 effects on health and/or welfare. To test for this possibility, Panel (d) of Figure 7 provides OLS estimates for the effect of leave take-up on coworkers' receipt of publicly paid sick days.[61] We see no evidence that parental leave take-up affects coworkers' sick days.

7.6 Heterogeneity and Mechanisms

The overall results presented above suggest that the effects of 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

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

results: How are the observed firm adjustments related to labor market frictions? And are there some firms that are unable to adjust effectively? As discussed in Section 2, parental leave should not affect firm costs and profits in a frictionless and competitive labor market. Moreover, this should be true for all types of firms because all firms will be able to increase coworkers' hours and/or hire replacement workers at the market wage. If, however, there are labor market frictions—for example, stemming from firm- or occupation-specific human capital—the effects of parental leave might be heterogeneous with respect to both worker and firm characteristics.

7.6.1 Same vs. Different Occupation Coworkers

As mentioned in Section 2, if there are labor market frictions and workers are heterogeneous, the effects of parental leave on coworkers could be very different for different coworkers, depending on whether they are substitutes to the worker on leave as opposed to complements. When an employee goes on leave, we would expect firms in this case 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 therefore examine whether the net effects of parental leave for coworkers mask some heterogeneous effects.

Following previous work, we expect coworkers in the same occupation to be substitutes, while coworkers in different occupations to be complements (Jäger & Heining, 2019). 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). Corresponding OLS estimates are presented graphically in Appendix J. In line with the theoretical predictions, 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 year and the year thereafter. Specifically, when one percent of the workforce is 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.[62] These effect sizes are approximately twice

[62] Although the estimates for the increase in earnings are larger in magnitude than for hours (not statistically significantly, however), we do not believe that firms compensate increased hours at a higher rate than the base salary. As discussed in Section 4, our hours measure does not include changes in overtime work, so we expect the estimates for hours to be attenuated.

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. Finally, we find no significant effects on both same-occupation and different-occupation coworkers' well-being as measured by their receipt of paid sick leave.

7.6.2 Firms with No Coworkers in the Same Occupation

The previous results suggest that the effects of 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. To shed light on the role of these margins of adjustment and to examine firm heterogeneity, we now focus on a subsample of firms that by definition cannot adjust along the second margin. That is, we examine firms that do not have any existing coworkers in the same occupation as the woman on leave. We refer to these firms as *no replacement* firms.[63]

In a labor market without any frictions, we would simply expect no replacement firms to respond to parental leave more strongly on the hiring margin and fully replace the worker on leave through new hires. However, this may not be possible if workers on parental leave cannot be seamlessly replaced by new hires—if, for example, firm-specific human capital is important. Indeed, it is also possible that no replacement firms are *less* able to rely on new hires. If a firm only has one worker in a specific occupation, it might, for example, be more difficult to sort out qualified applicants and to attract a temporary worker to replace this worker when on leave. Overall, we expect no replacement firms to be less able to compensate for the worker on leave due to frictions.

Table 9 presents 2SLS estimates for the subsample of no replacement firms.[64] These firms constitute 10 percent of the firms in our main sample and we therefore have limited statistical power when looking at these firms. Nonetheless, some interesting patterns still emerge from this analysis.

First, no replacement firms experience fewer leave days than other firms. Over the event year and the following year, an additional birth leads to only 247 additional leave days at no replacement firms compared to 282 days for the main sample. This difference of 35 days is statistically significant and

[63] 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.

[64] To assess whether the effects of 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.

may reflect the fact that workers at no replacement firms internalize that they are harder to replace.[65]

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 the overall sample in Tables 5 and 6, we saw no changes in total hours or the wage bill excluding paid leave when an employee takes leave. For no replacement firms in the event year, however, we estimate negative effects for both outcomes, which are at least marginally significant in all specifications.[66] Based on the preferred specification in column (3), a no replacement firm where one percent of the workforce 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.[67] 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 workers than other firms—although this difference is generally not statistically significant.[68]

Finally, turning to our measures of firm performance, we estimate that 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. However, because of the noisy nature of these measures and the significantly smaller sample size for this subgroup, these estimates are less precise and not statistically significant at standard levels. However, they are consistent with sizable negative effects.[69] For example, the lower bound of the 95 percent confidence interval for the effect on firm sales is a 0.75 percent drop when one percent of the workforce is on leave in the event year. In contrast, the overall point estimate in Table 7 is positive with a standard error roughly one-third the size but is also statistically insignificant.

Despite this limited statistical power, we view the overall results as suggesting that there are indeed

[65]Ginja *et al.* (2020) uncover similar findings. Because they lack data on occupation, however, their measure of whether coworkers can replace each other is different and is based on education and field of study.

[66]In the preferred specification in column (3), the estimated negative effect on total hours is only significant at the 10 percent level ($p = 0.06$). The estimated effects on hours and the wage bill excluding paid leave in the event year is statistically significant at least at the 5 percent level in all other specifications. These estimated effects for no replacement firms are also statistically significant from the effects on other firms at least at the 10 percent level in all specifications (see Table A4).

[67]The difference between these two estimated effects is not statistically significant. The estimated effect on hours may be attenuated and therefore, the true difference between the two estimates is likely smaller than estimated (see Section 4).

[68]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).

[69]In unreported results, we do in fact find that the estimated negative effect on sales for no replacement firms is statistically significant when sales is measured in absolute changes instead of scaling relative to baseline. As discussed in footnote 36, however, we view scaling relative to baseline as the preferred approach.

negative effects of parental leave for firms that cannot use same-occupation coworkers to compensate for the absent worker. 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.

7.6.3 Firm Size

Finally, we examine heterogeneous effects across firms of different sizes. 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). Appendix Table A5 summarizes the results of this analysis.[70] We see little evidence that the effects of 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.[71] For firm performance, the coefficient on the relevant interaction term is insignificant throughout and exhibits no systematic pattern. To the extent that we can measure it, the effects of parental leave does not seem to vary systematically with firm size.

7.7 Contrasts with Effects of Parental Leave Extensions

We close the discussion of our results by contrasting them with the results of Gallen (2019) and Ginja *et al.* (2020) on the effect of parental leave extensions on firms.

Gallen (2019) studies a parental leave reform in Denmark passed in late March 2002 but applicable for women with births in January 2002 and later. For these women already on leave in March 2002, the reform led to an extension of their leave period from eight to ten months on average. Gallen (2019) studies how these leave extensions affected the women's coworkers and employers. Ginja *et al.* (2020) examines a very similar Swedish policy reform in July 1989. The introduction of this reform was also retroactive—affecting parents with children born in August 1988 and later. This reform caused women

[70] Appendix Table A5 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.

[71] Because 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).

already on leave to extend their leave duration from twelve to fourteen months on average. Ginja *et al.* (2020) analyze how these leave extensions affected the employing firms.

Both Gallen (2019) and Ginja *et al.* (2020) find that parental leave extensions impose significant costs on firms and coworkers. For non-manufacturing firms, Ginja *et al.* (2020) document a two-year increase labor costs after the extended leave periods ended. For manufacturing firms—the only firms for which they have revenue data—Ginja *et al.* (2020) find no effect on labor costs but document a one-year drop in sales revenue in the year after the conclusion of the extended leave. Gallen (2019) in turn finds an increase in firm shutdown a year following the end of the extended leave. Furthermore, Gallen (2019) finds a one-year increase in coworker sick days two years after the extended parental leave finished and a one-year decrease two years thereafter. These findings stand in stark contrast to our results. For the vast majority of firms, we see no indications of negative effects either during the parental leave period or after the leave ends. Moreover, the firm adjustments we observe dissipate completely in the first year after the worker returns from parental leave.

In interpreting these different sets of findings, it is important to note two key distinctions in our focus relative to this other work. First, whereas our study estimates the effects of having a worker go on parental leave, Gallen (2019) and Ginja *et al.* (2020) examines the effects of experiencing longer leaves among employees who were already eligible for a lengthy leave. This is important because extensions of parental leave of already long durations have been shown to lead to separation from the pre-birth employer (Rossin-Slater, 2019). In contrast, the introduction of some amount of parental leave tends to increase the likelihood that female employees will return to their old employer after giving birth. Consistent with this, in both Gallen (2019) and Ginja *et al.* (2020), the studied parental leave extensions made the leave-eligible mothers significantly more likely to separate from their pre-birth employers. In light of previous studies demonstrating that turnover and permanent employee absences can be highly disruptive for firms (see, for instance, Bertheau *et al.* , 2019; Jäger & Heining, 2019), this may explain why Gallen (2019) and Ginja *et al.* (2020) find negative effects on firms after the conclusion of the parental leave period but see no negative effects at the time of the extended leave. In contrast, women going on parental leave in the Danish context we study are not more likely to leave their employer than other comparable employees.

Second, because Gallen (2019) and Ginja *et al.* (2020) study unexpected extensions of already ongoing leaves, firms in their settings could not plan for the extended absence before the leave period started. Normally, however, absences due to parental leave differ from most other employee absences exactly in that they are highly anticipated and give firms more scope for planning and adjusting while the pregnant employee is still working. This may also contribute to difference in results—in particular, the fact that we see firms making successful temporary adjustments to their labor inputs already in the year a parental leave starts.

Overall, the comparison of our results with the estimated effects of parental leave extensions in Gallen (2019) and Ginja *et al.* (2020) highlight two factors that may be key for understanding the effects of parental leave on firms. First, particular parental leave reforms that affect the turnover rates of new mothers can have effects on firms that are separate from the actual parental leave. Second, ensuring that firms are able to anticipate and plan for upcoming leaves in advance may be particularly important for limiting the detrimental effects of parental leave on firms.

8 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 subsequent to the birth.

Our findings indicate that firms hire temporary workers and slightly increase retention of existing employees in response to leave take-up. Additionally, existing workers see temporary increases in their hours of work and earnings, as well as reductions in their unemployment risk. On net, we therefore see no significant effects on firms' total labor inputs. Firms' total wage bills do increase temporarily in response to the leave; 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 on parental leave on firms' output, gross profit, and closure or on existing employees' sick days.

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 the limited effects of parental leave overall reflect 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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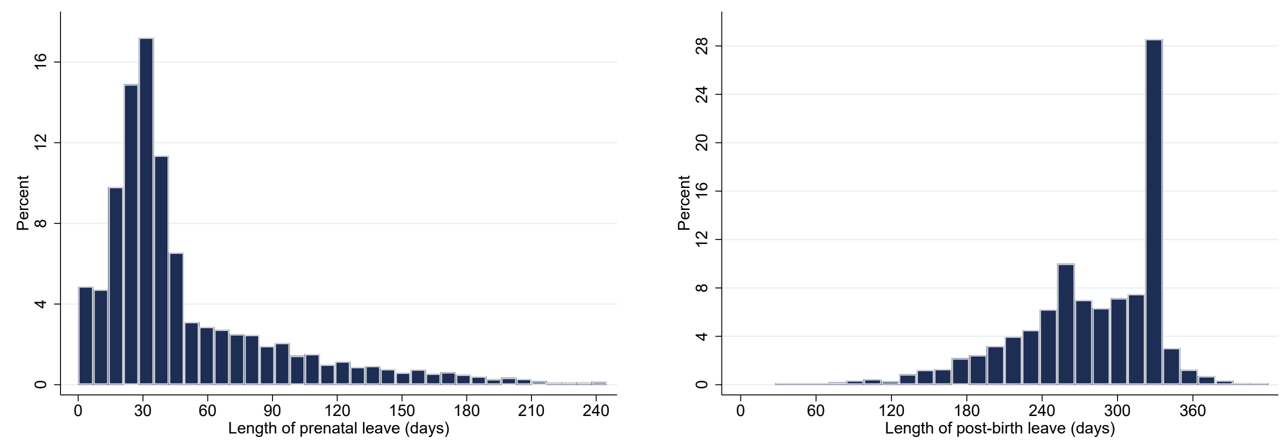
Figure 1: Definition of treatment and control samples

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 as explained in Subsection 5.1.

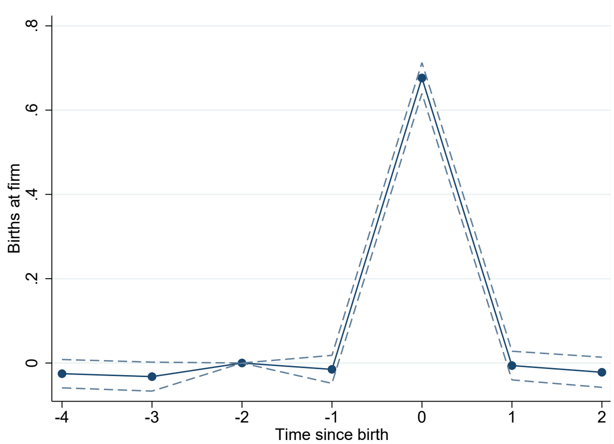
Figure 2: Histogram of the duration of women's prenatal and postnatal leave

(a) Prenatal leave (b) Postnatal leave

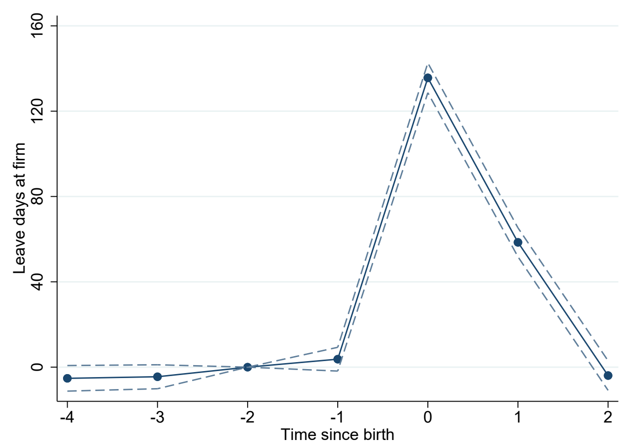


Notes: The histograms illustrate the distributions of the duration of prenatal and postnatal leave, respectively, taken among mothers in our estimation sample; it includes both paid and unpaid leave.

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



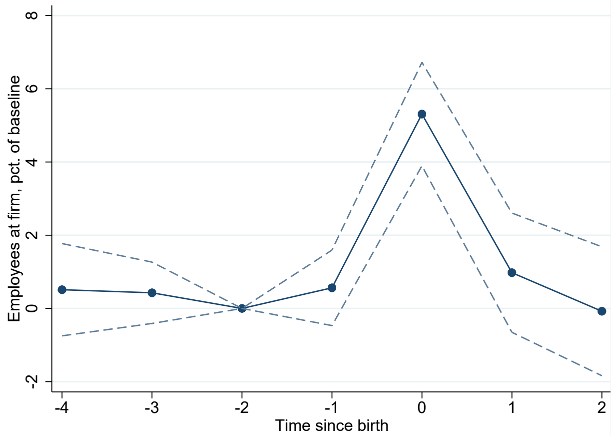
(a) Births at firm



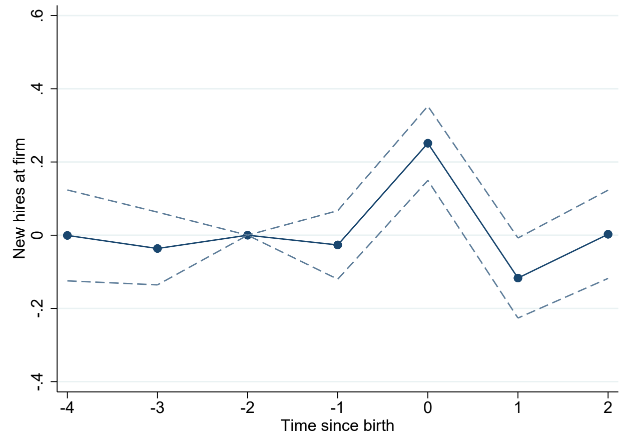
(b) Parental leave days at firm

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.

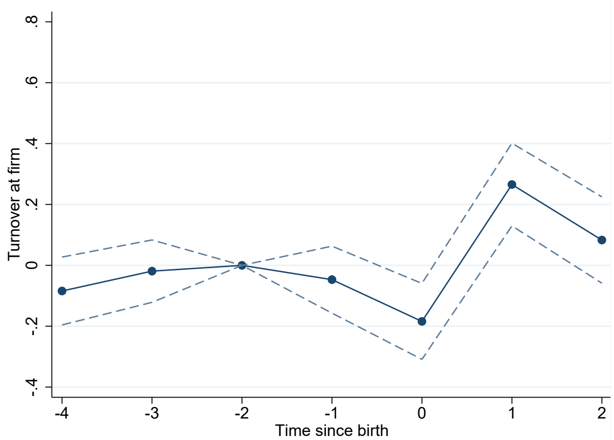
Figure 4: Effects on employment outcomes, OLS



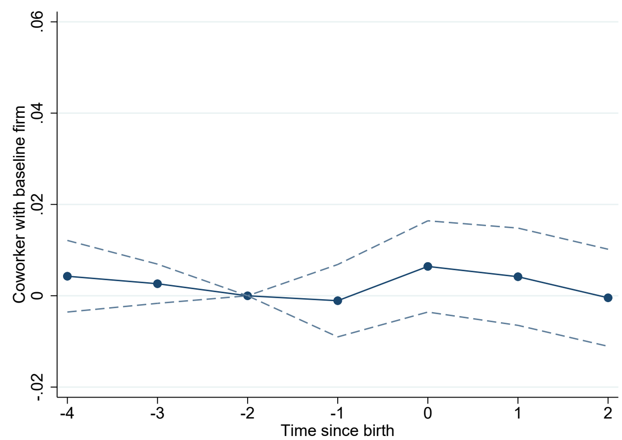
(a) Number of employees at firm



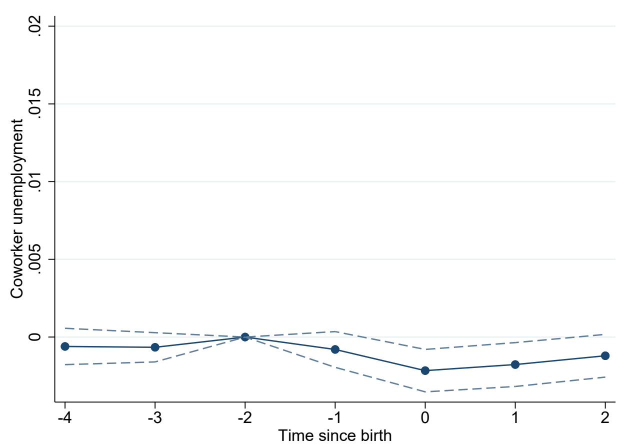
(b) New hires at firm



(c) Turnover at firm



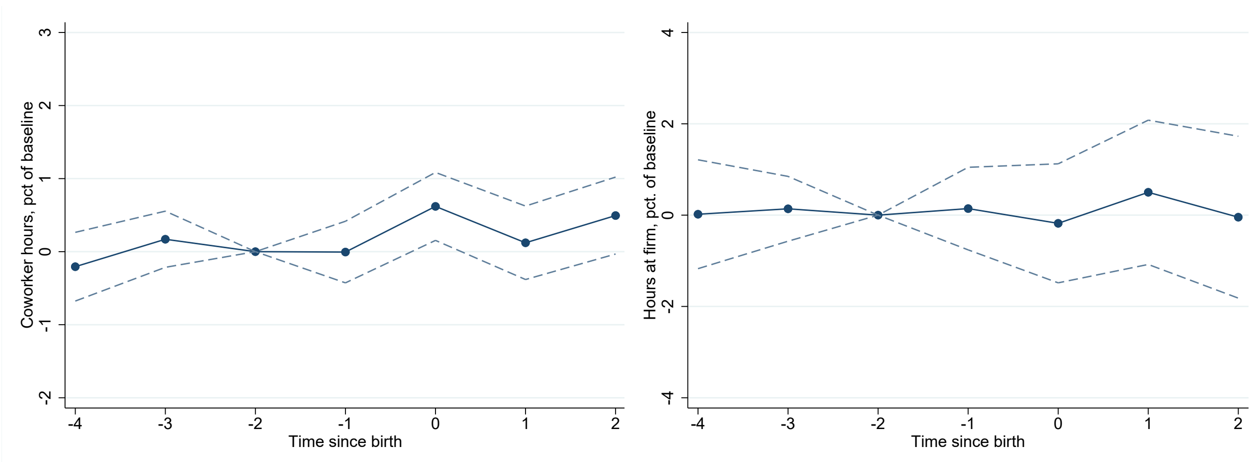
(d) Likelihood coworkers with baseline firm



(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

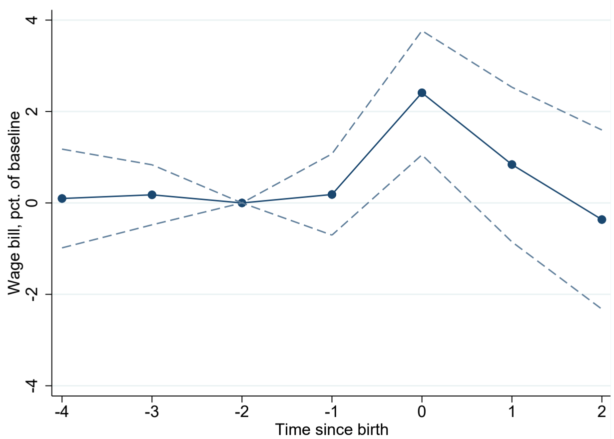


(a) Coworker hours

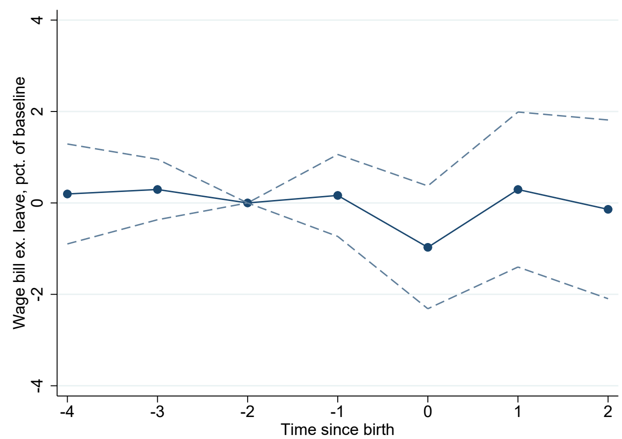
(b) Hours at firm

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.

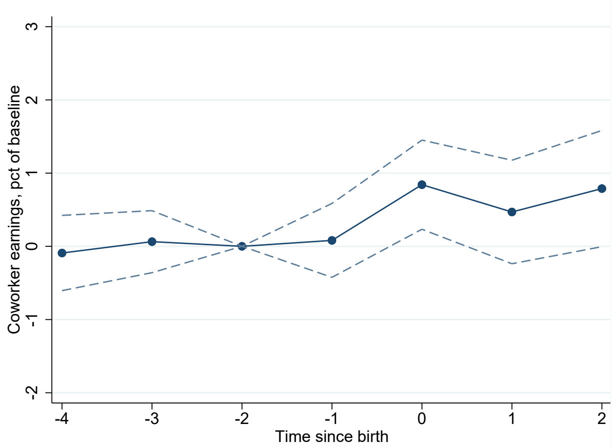
Figure 6: Effects on wage costs and earnings, OLS



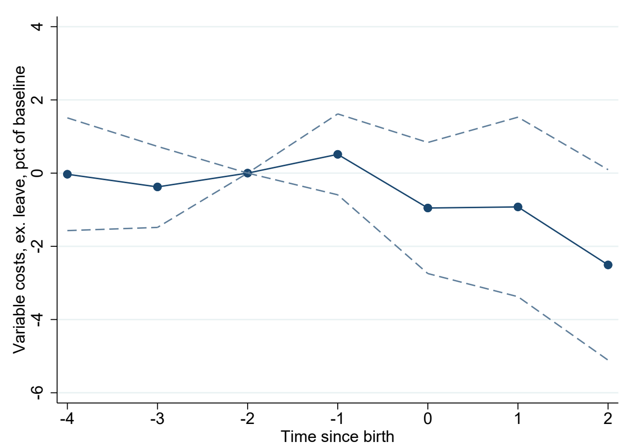
(a) Firms' wage bill



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



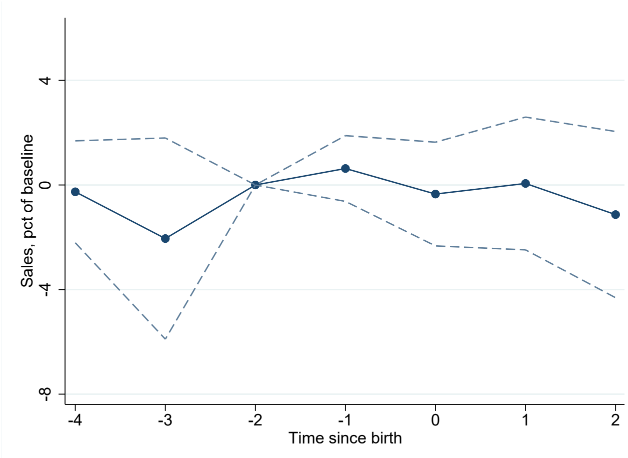
(c) Coworkers' earnings



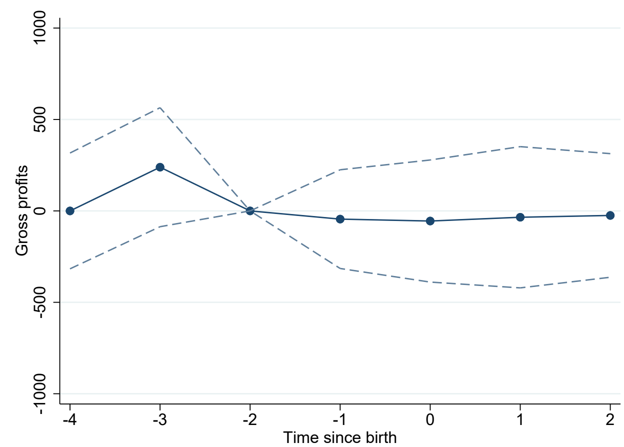
(d) Firms' total variable costs (excluding paid leave)

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.

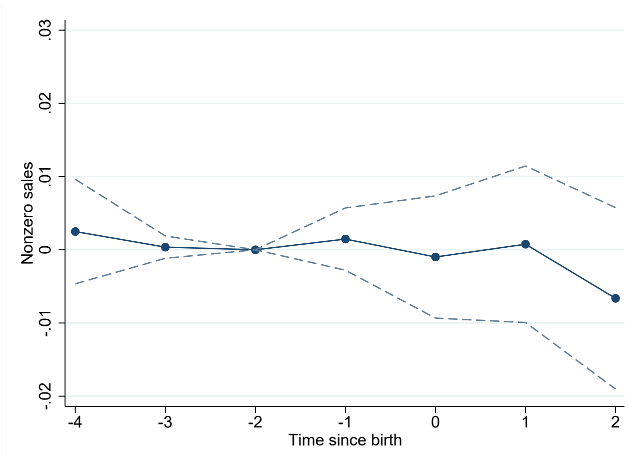
Figure 7: Effect on firms' overall performance and coworkers' sick leave, OLS



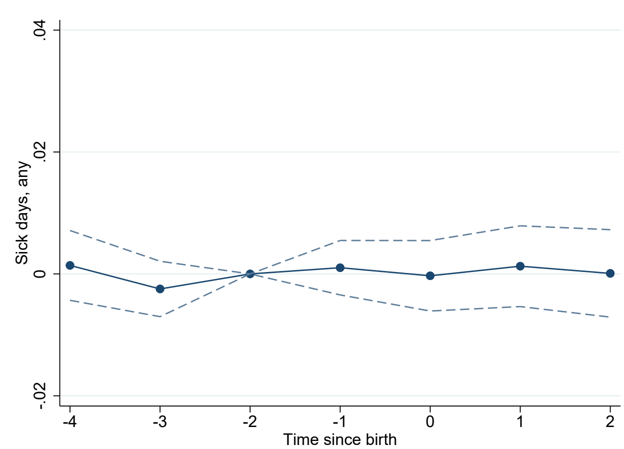
(a) Firms' total sales



(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]			
Job protection:	Yes	Yes	Yes
Wage replacement:	Fully paid, firm reimbursed	Fully paid, firm reimbursed	UI payment

Notes: The table summarizes the minimum parental leave benefits available to all new mothers as well as the benefits available to new mothers on a typical employment contract. The table shows available benefits assuming that the father does not take any of the shared leave (on average fathers only take around three weeks 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 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 (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 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.

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.

Table 3: Sample selection

	Treatment events	Control events	Total unique firms
Baseline sample:	199,229	1,147,108	74,818
Restricted to active firms:	181,295	1,045,434	61,211
Restricted to small firms:	25,369	165,424	46,928
Restricted to private firms:	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 final sample of matched treatment and control events. *Baseline sample:* restricting woman-firm-year observations to eligible women according to our potential event definition. *Sample restricted to active firms:* based on sales, hours and total wage bill, the firm must be active in the baseline year. Specifically, we require that total hours in the baseline year correspond to at least one full time employee and that the firm either had positive sales or positive wage payments in the year prior to the baseline year. *Restricted to small firms:* the stock of employees is between three and 30 in the baseline year, and the total number of employment relationships is less than 60 in the baseline year. *Restricted to private firms:* the firm must be in the private sector. *Excluding sale and wage bill outliers:* the firm must not be an extreme outlier in terms of sales levels or wage bills—firms with outlier sales or wage bills relative to their employment are excluded. 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). *Excluding extreme growth/decline firms:* the firm must not be an extreme outlier in terms of growth.

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

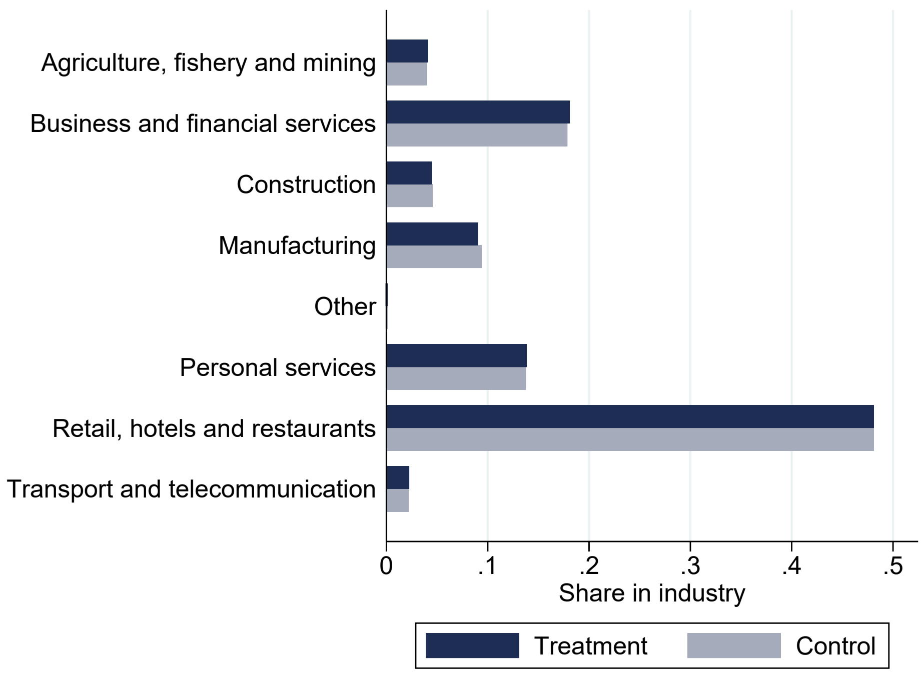
	Absolute effect Effect of one additional birth		*Relative effect* Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
A) Labor inputs				
Parental leave days at firm	184.8**	62.48**	8.174**	2.763**
	(12.00)	(11.36)	(0.445)	(0.472)
Number of 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.

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	16	100	104	flat rate	social insurance
Canada	17 (federal)	55% for 15 weeks	37	55	social insurance
Denmark	18	100	32	100	public funds + employers
Finland	18	70	26	70	social insurance
France	16	100	156	flat rate for 26 weeks for first child	social insurance
Germany	14	100	156	67	social insurance + employers
Italy	22	80	26	30	social insurance
Norway*			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 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 2013 across several countries. Maternity leave length includes both prebirth and postbirth leaves; *In Norway, fourteen weeks of parental leave are reserved exclusively for mothers and another fourteen weeks for fathers. Source: ILO (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. ** $p < 0.01$ * $p < 0.05$.

Table A5: 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. ** $p < 0.01$ * $p < 0.05$.

B Comparing Outcomes for Treatment and Control Women

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-difference 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 child birth.

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):

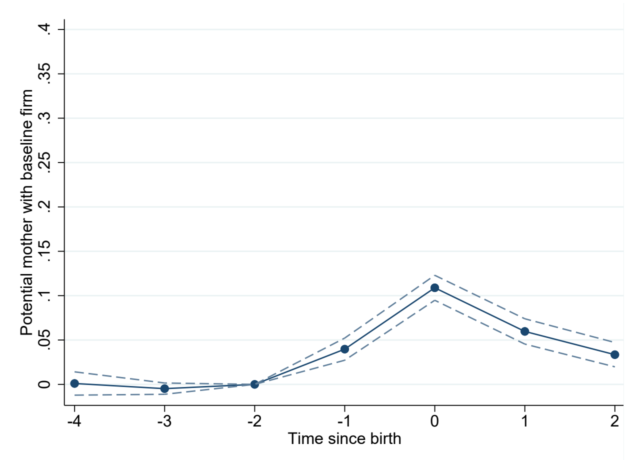
$$\begin{aligned} y_{eift} = &\psi_e + \sum_{k \in \mathcal{T}} \omega_k \mathbb{1}_{t=k} + \sum_{k \in \mathcal{T}} \kappa_k \mathbb{1}_{t=k} \cdot Treatment_e + \nu_{eift} \\ &\mathcal{T} = \{-4, -3, -1, 0, 1, 2\}. \end{aligned} \tag{5}$$

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

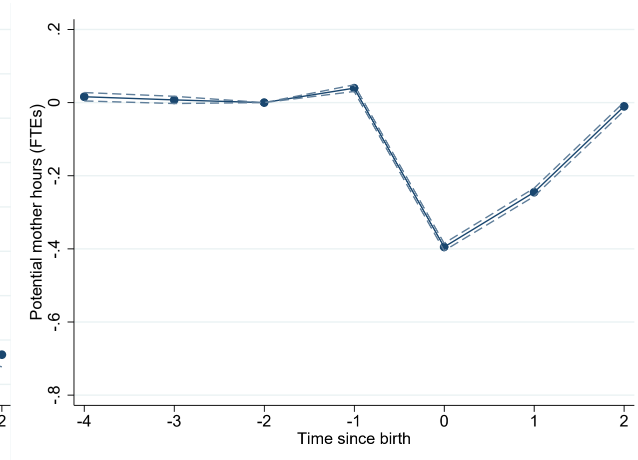
Appendix Figure A2 shows no pre-trends three to four years before the event for any of the outcomes. This is consistent with previous evidence from Kleven *et al.* (2019) (see Footnote 45). Meanwhile, in the year before childbirth, the event year, and the following one year (i.e. in the time up to conception, 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 A2(a)); 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 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 (see Footnote 41).

Figure A2(c) and A2(d) show that treatment women experience a large drop in their earnings in

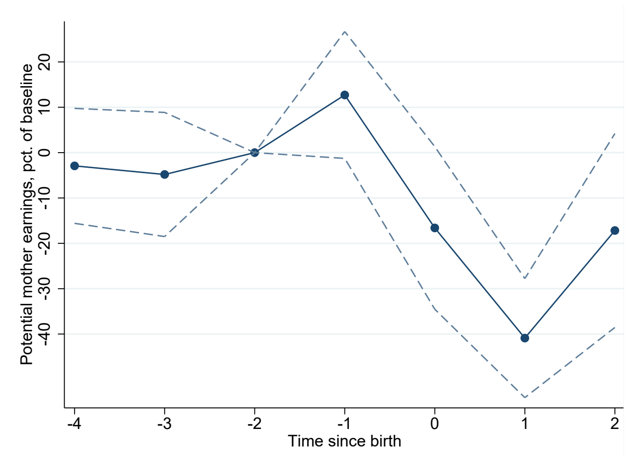
Figure A2: Effect on Potential Mothers



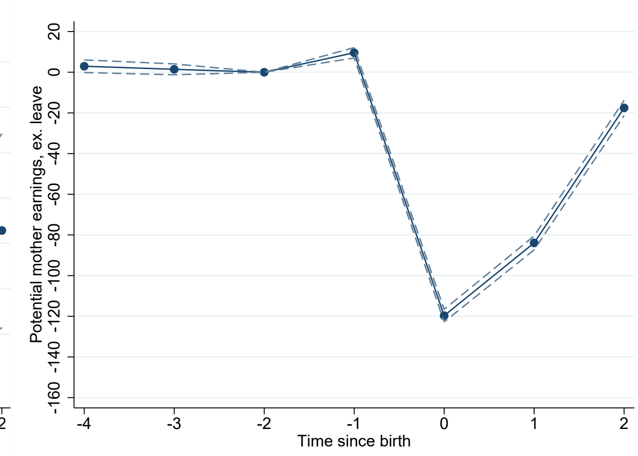
(a) With baseline firm



(b) Hours (FTE)



(c) Earnings (incl. leave) rel. to baseline



(d) Earnings ex. 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).

the year of childbirth and the following year. Two years after childbirth, earnings recover somewhat and are 20 percent lower than at baseline which is of similar magnitude as found by Kleven *et al.* (2019). Finally, Figure A2(b) shows that treatment women tend to work slightly more hours in the year preceding childbirth and much fewer hours during the event year and the following year due to parental leave take-up. The difference between treatment and control women seems to almost disappear two years after childbirth.

C Effects on Coworker Fertility and Leave-Taking

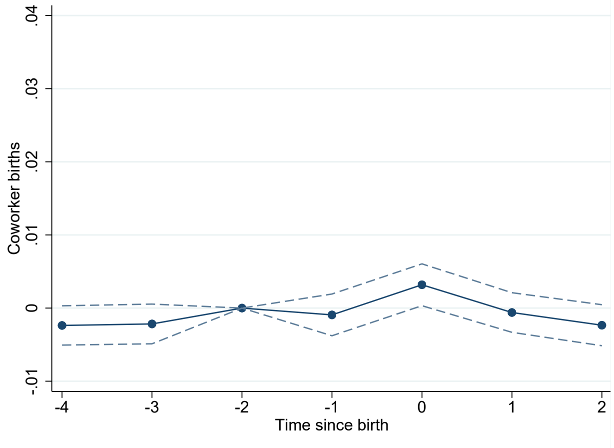
A parallel literature (e.g., Asphjell *et al.* , 2014; Ciliberto *et al.* , 2016) shows the existence of workplace peer effects in the incidence and timing of pregnancy and parental leave. For example, Asphjell *et al.* (2014) find that the likelihood that an individual in Swedish firms has a first child 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 A3 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 A6, 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,[72] 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 A6). We further show OLS (Panels (c) through (f) of Appendix Figure A3) and 2SLS estimates (Panels B and C of Appendix Table A6) 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.

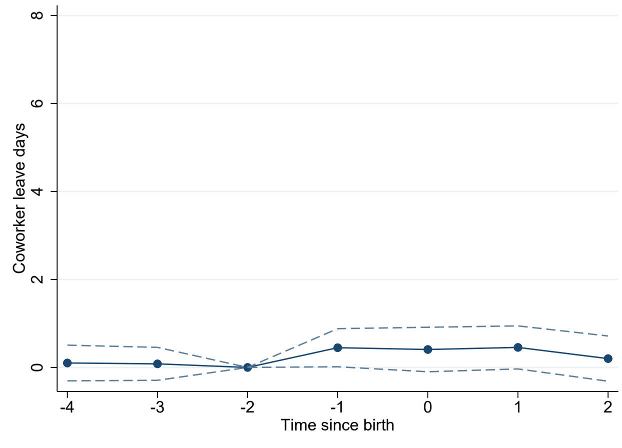
Taken together, these estimates dampen the concern that coworker peer effects could be driving our main results.

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

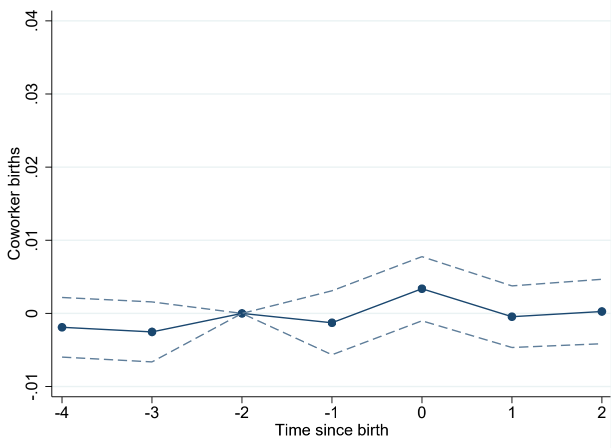
Figure A3: Effects on coworkers' fertility and parental leave days, OLS



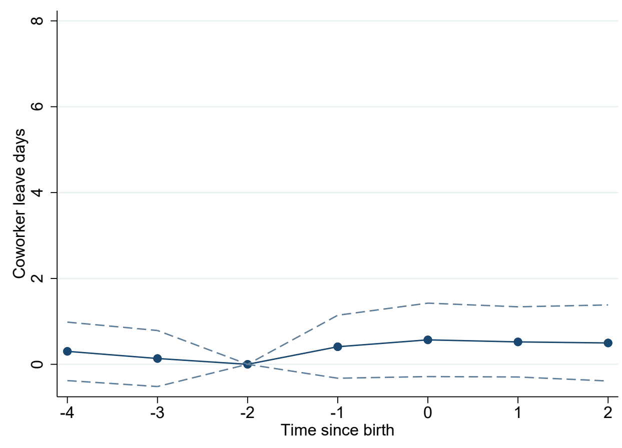
(a) Births, all coworkers



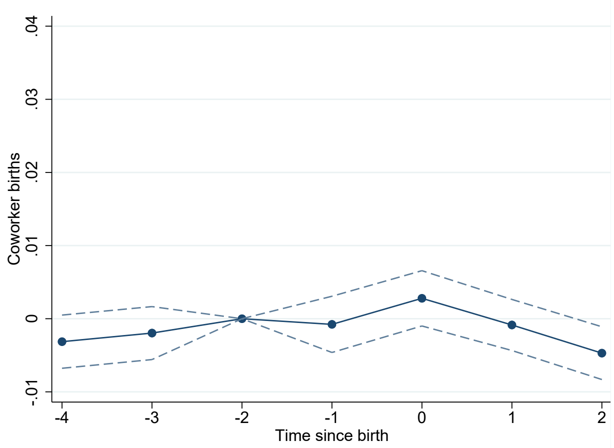
(b) Parental leave days, all coworkers



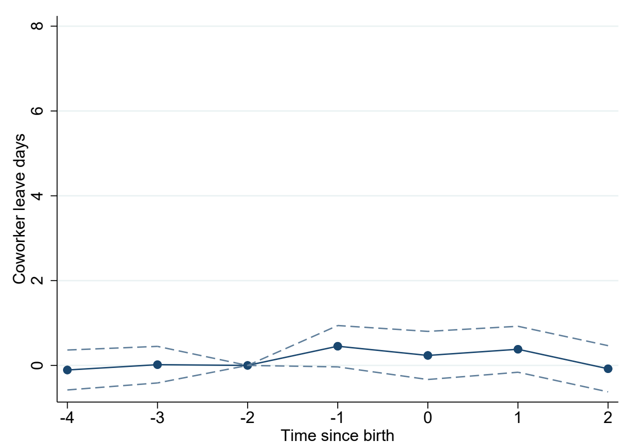
(c) Births, same-occupation coworkers



(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 A6: 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.

D 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):

$$\begin{aligned} y_{ecft} =& \psi_e + \sum_{k \in \mathcal{T}} \omega_k \mathbb{1}_{t=k} + \sum_{k \in \mathcal{T}} \kappa_k \mathbb{1}_{t=k} \cdot Treatment_e + \nu_{eft} \\ \mathcal{T} =& \{-4, -3, -1, 0, 1, 2\} \end{aligned} \tag{6}$$

Our 2SLS specification for estimating the (absolute) effect of an additional birth on coworkers is a natural adaptation of specification (3):

$$\Delta y_{ecf} = \varrho_0 + \mu_0 BirthsInEventYear_{ef} + \Delta\nu_{ecf} \tag{7}$$

$$BirthsInEventYear_{ef} = \iota_0 + \iota_1 Treatment_e + \upsilon_{ecf} \tag{7, First Stage}$$

Our 2SLS specification for estimating the (relative) effect of one percent of the workforce giving birth is a natural adaptation of specification (4):

$$\Delta y_{ecf} = \varpi_0 + \chi_0 \frac{BirthsInEventYear_{ef}}{BaselineEmployees_{ef}} + \Delta\sigma_{ecf} \tag{8}$$

$$\frac{BirthsInEventYear_{ef}}{BaselineEmployees_{ef}} = \zeta_0 + \zeta_1 \frac{Treatment_e}{BaselineEmployees_{ef}} + \sigma_{ecf} \tag{8, First Stage}$$

When estimating each of the coworker specifications, we apply the reweighting described in Section 5.6.2. Specifically, each coworker receives the weight associated with his or her event (so coworkers at treatment firms all receive a weight of one).

E 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:

$$\begin{aligned} Y_{eft} =& \gamma_e + \sum_{k \in \mathcal{T}} \alpha_k \mathbb{1}_{t=k} + \sum_{k \in \mathcal{T}} \gamma_k \mathbb{1}_{t=k} \cdot Treatment_e + \sum_{k \in \mathcal{T}} \beta_k \mathbb{1}_{t=k} \cdot X_e + \varepsilon_{eft} \\ & \mathcal{T} = \{-4, -3, -1, 0, 1, 2\}. \end{aligned} \tag{9}$$

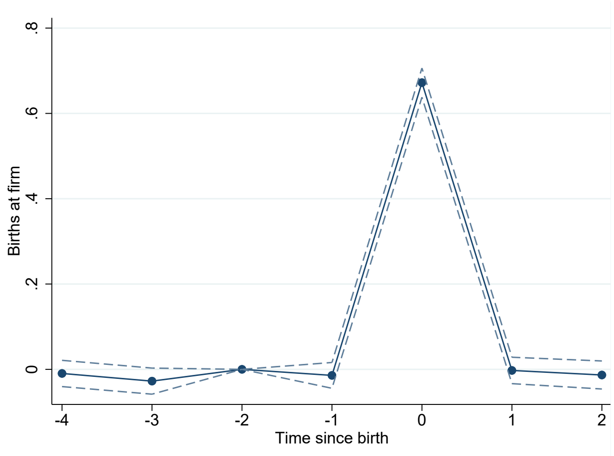
This specification is identical to that used in the main text, except for the fact that a vector of event-specific baseline characteristics, X_e, has been interacted with the event time dummies and added as controls. Because these added interaction terms will absorb any differences in time trends that 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[73] and let X_e consist of an exhaustive set of dummies indicating which of the cells event e belongs to.

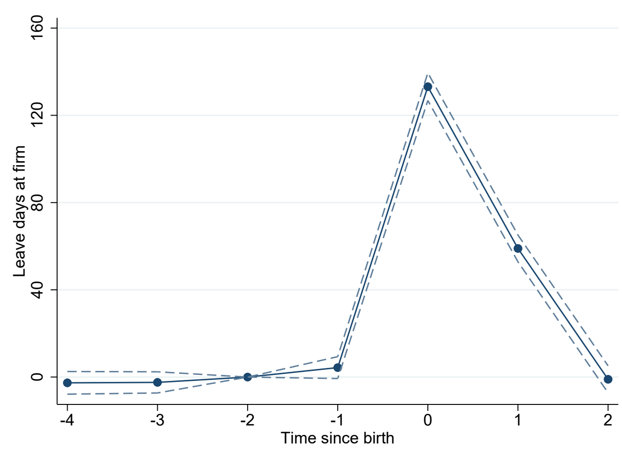
Appendix Figures A4 to A8 show OLS estimates from this alternative regression-based approach. We see that they are virtually indistinguishable from the results presented in the main text.

[73]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.

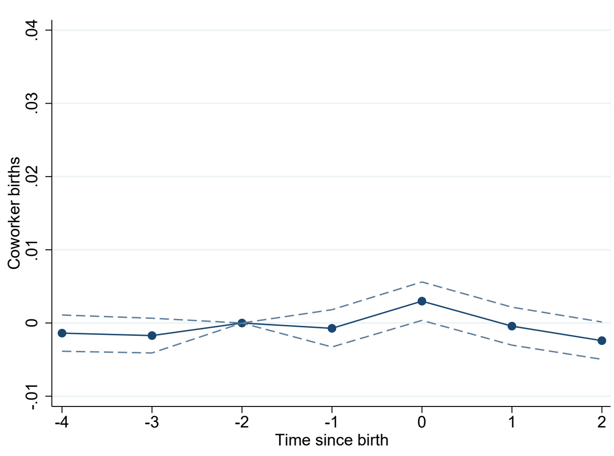
Figure A4: Effects on births and leave days, regression with controls, OLS



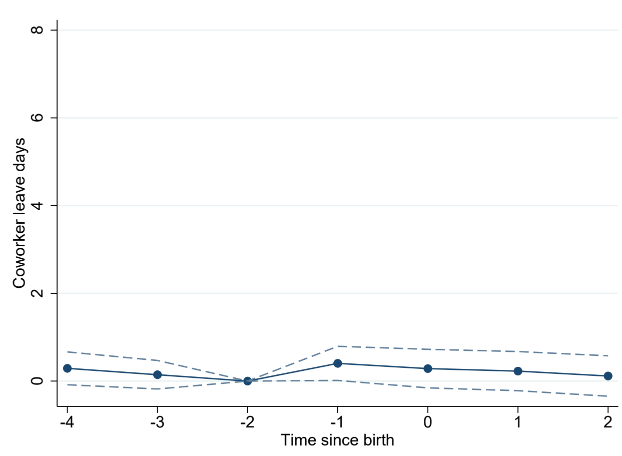
(a) Births at firm



(b) Parental leave days at firm



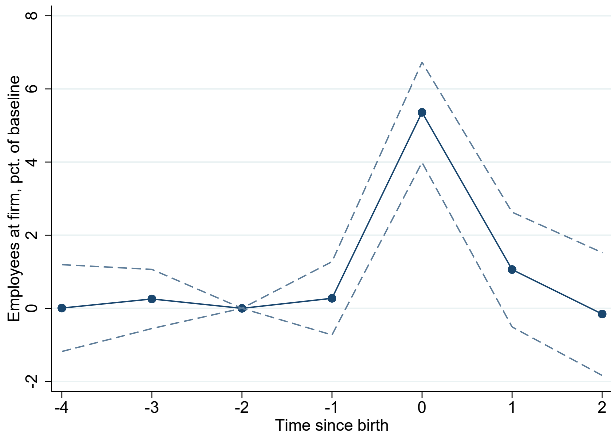
(c) Coworkers' births



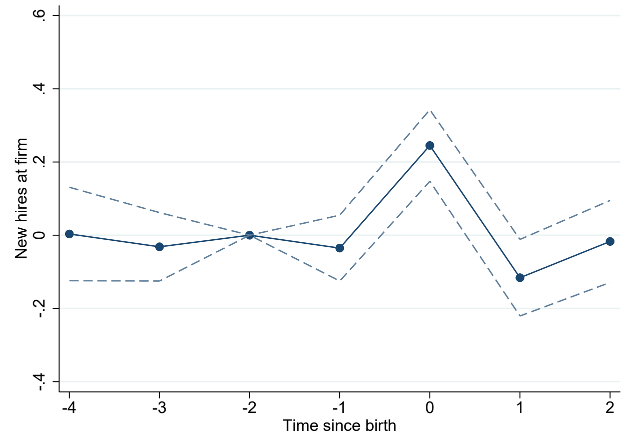
(d) Coworkers' parental leave days

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.

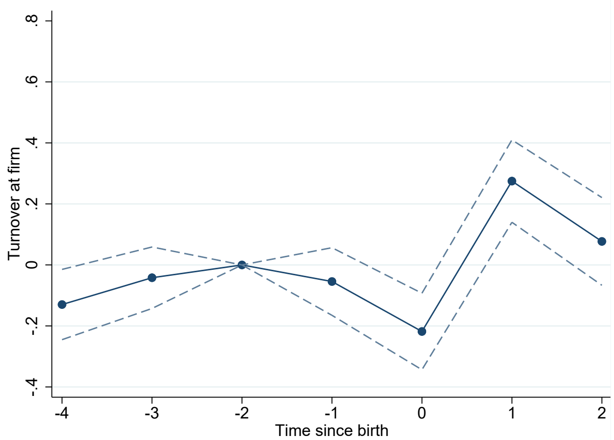
Figure A5: Effects on employment outcomes, regression with controls, OLS



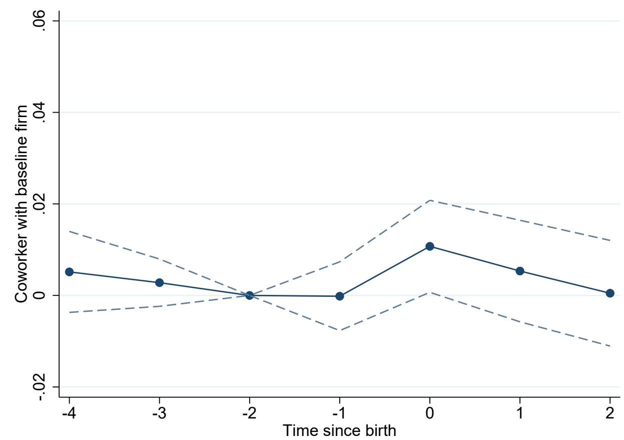
(a) Number of employees at firm



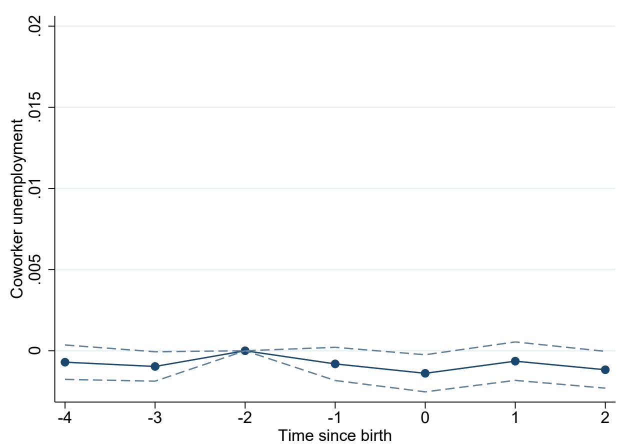
(b) New hires at firm



(c) Turnover at firm



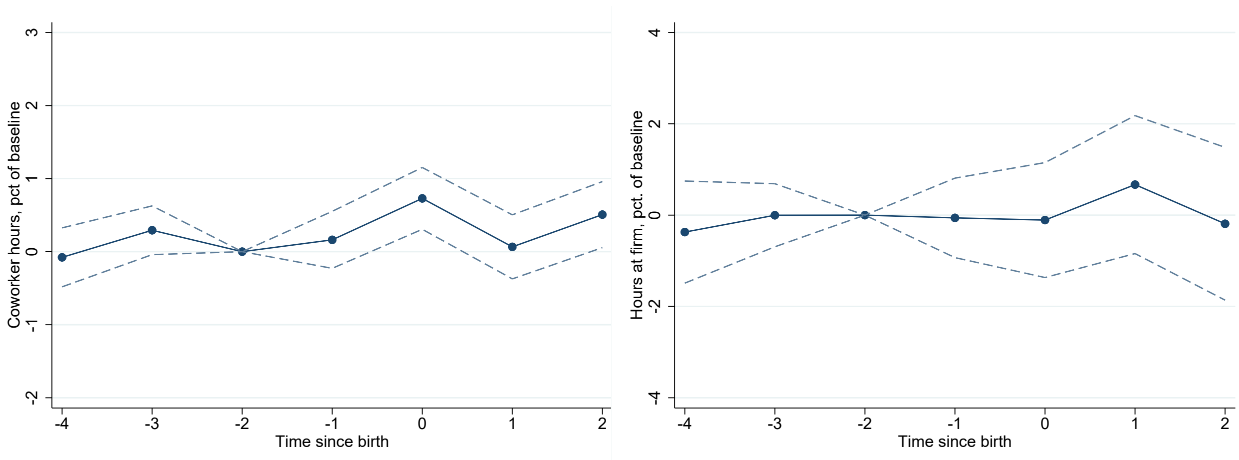
(d) Likelihood coworkers with baseline firm



(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 A6: Effects on hours of work, regression with controls, OLS

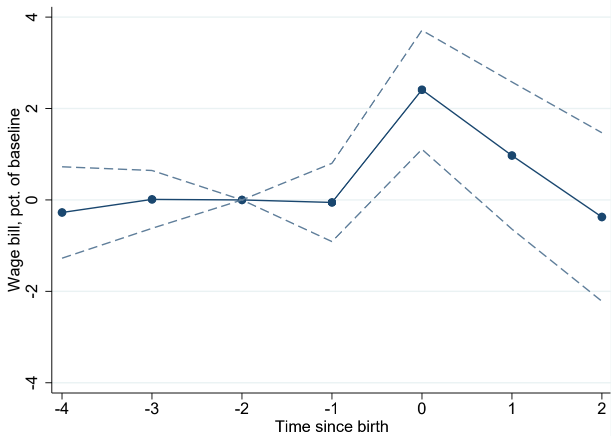


(a) Coworker hours

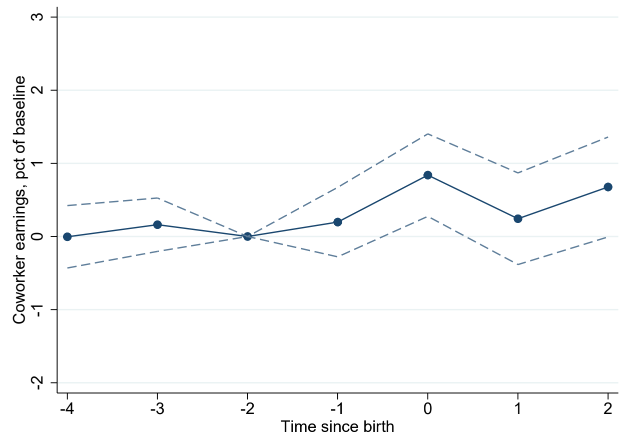
(b) Hours at firm

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.

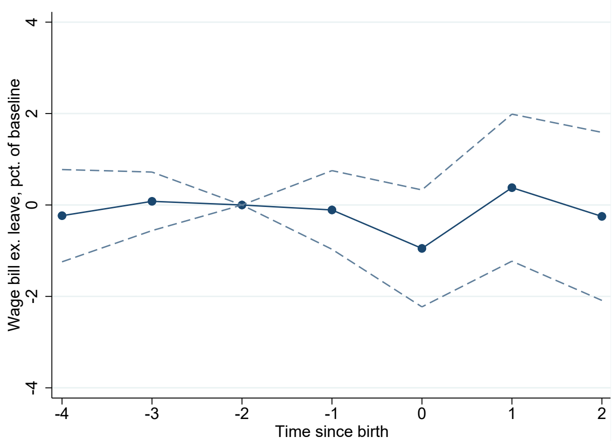
Figure A7: Effects on costs of labor supply adjustments, regression with controls, OLS



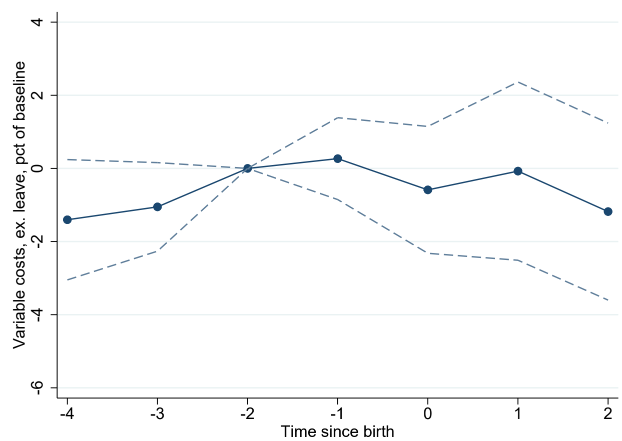
(a) Firms' wage bill



(b) Coworkers' earnings



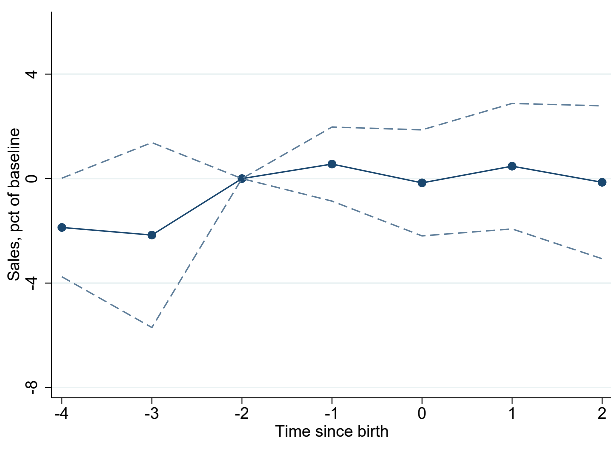
(c) Firms' wage bill (excluding paid leave)



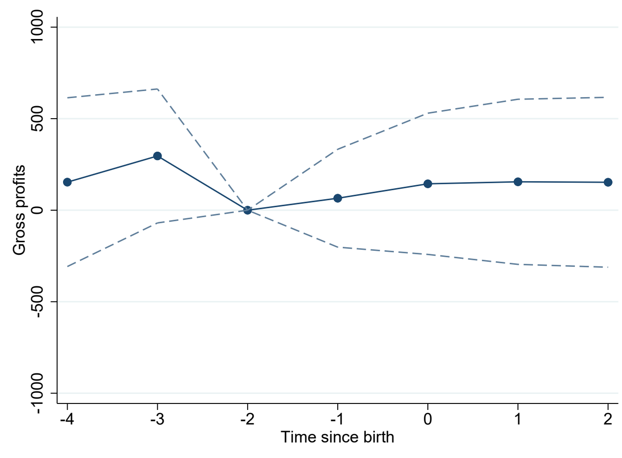
(d) Firms' total variable costs (excluding paid leave)

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.

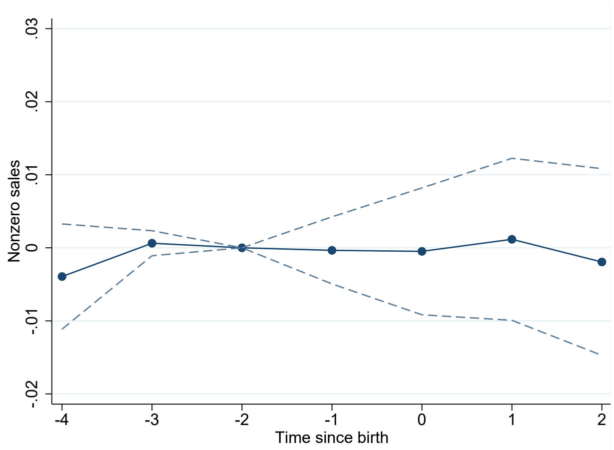
Figure A8: Effect on firms' overall performance and coworkers' sick leave, regression with controls, OLS



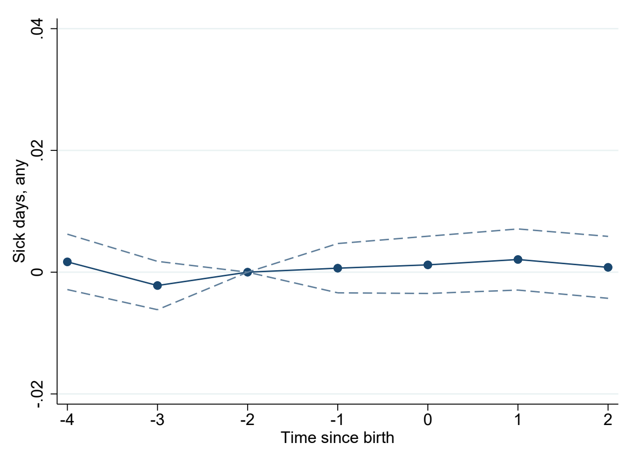
(a) Firms' total sales



(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.

F Results Using Coarser Set of Baseline Covariates

As discussed in Section 5.6.2, 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.[74] 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 A9 to A13 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 A7 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.

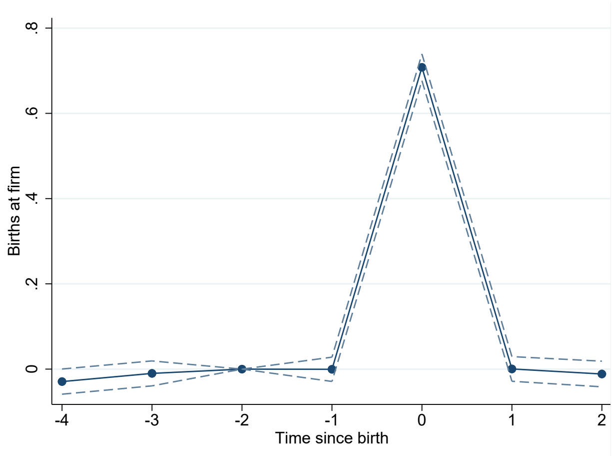
[74] Of the initial 155,625 control events, 38,533 remain after trimming when using the coarser set of baseline covariates.

Table A7: Covariate balance table conditioning on coarser set of observables

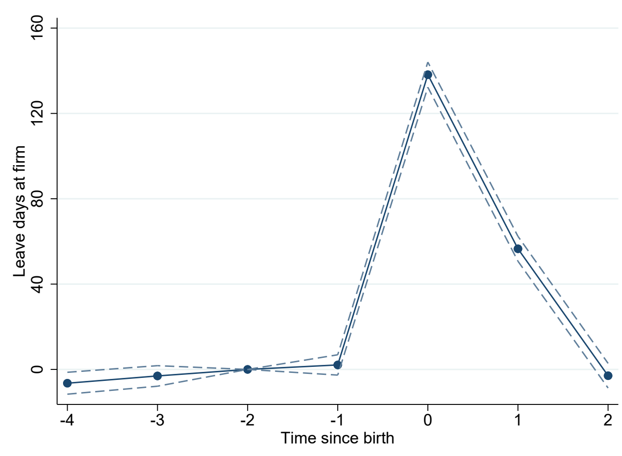
	Treatment	Control	Difference	p-Value
Births at firm	0.84	0.82	0.02	0.11
	(1.07)	(1.06)	(0.01)	
Leave days at firm	149.80	143.40	6.40	0.01
	(201.57)	(199.79)	(2.40)	
New hires	3.74	3.79	−0.05	0.14
	(3.32)	(3.31)	(0.04)	
Hours (FTEs)	10.66	10.63	0.03	0.69
	(7.16)	(7.17)	(0.08)	
Workforce avg. years schooling	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. ** $p < 0.01$ * $p < 0.05$.

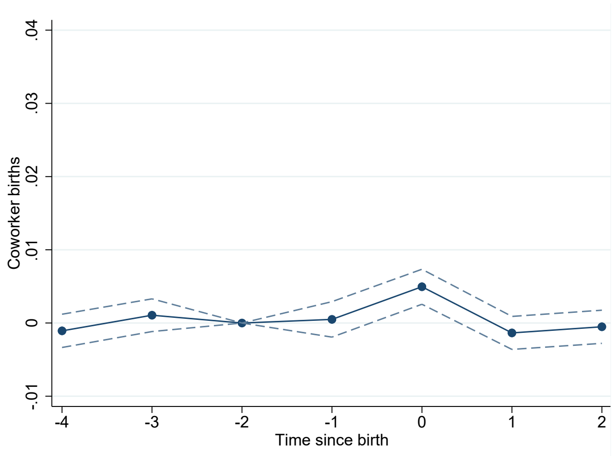
Figure A9: Effects on births and leave days, conditioning on coarser set of observables, OLS



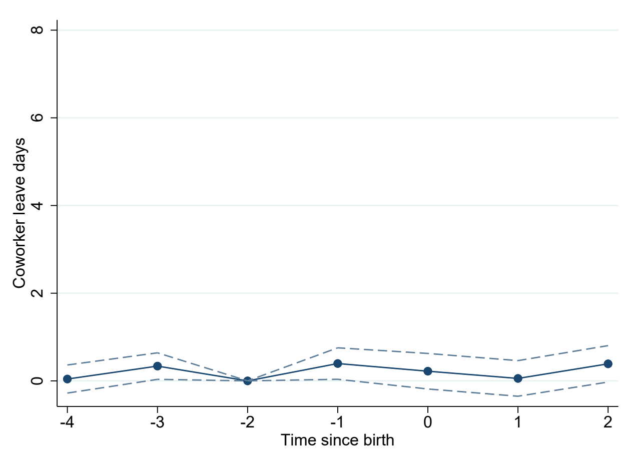
(a) Births at firm



(b) Parental leave days at firm



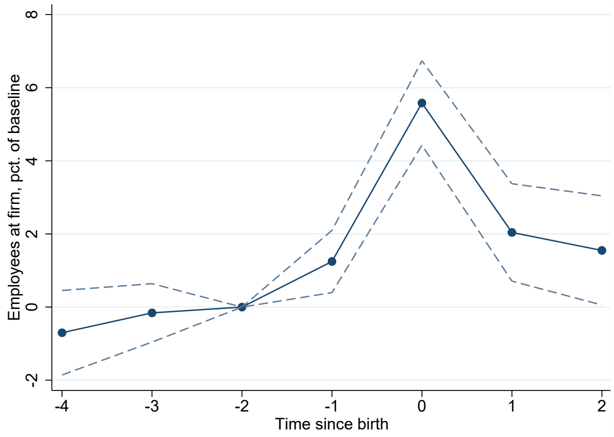
(c) Coworkers' births



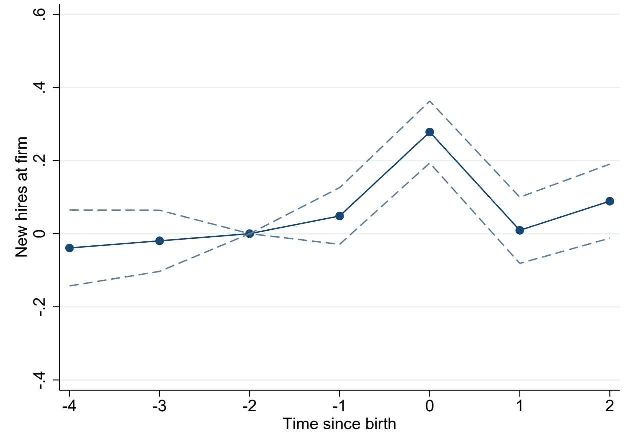
(d) Coworkers' parental leave days

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.

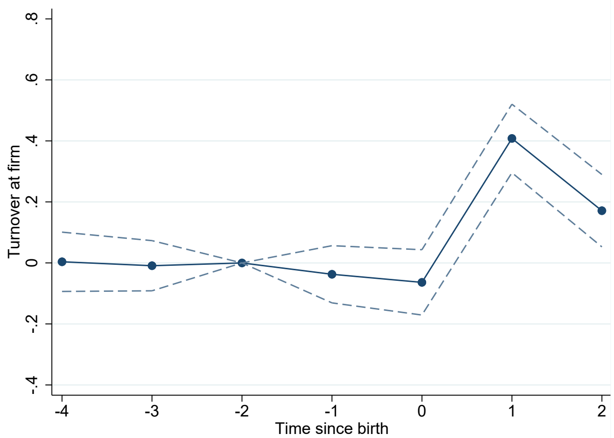
Figure A10: Effects on employment outcomes, conditioning on coarser set of observables, OLS



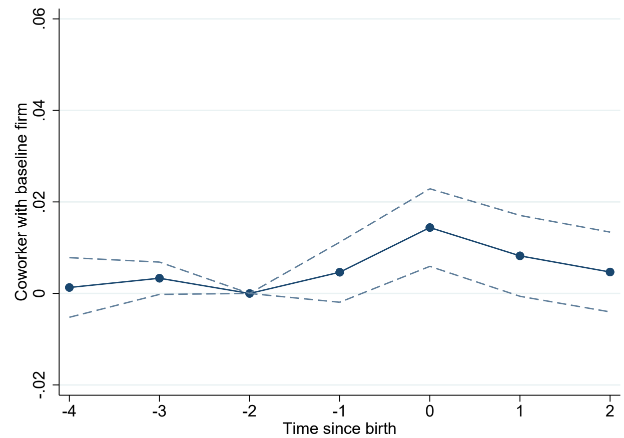
(a) Number of employees at firm



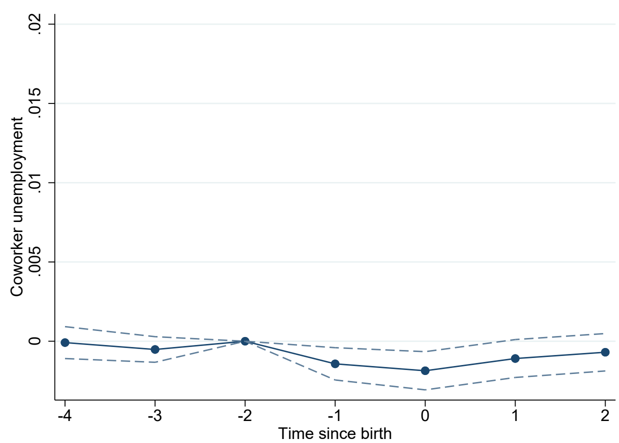
(b) New hires at firm



(c) Turnover at firm



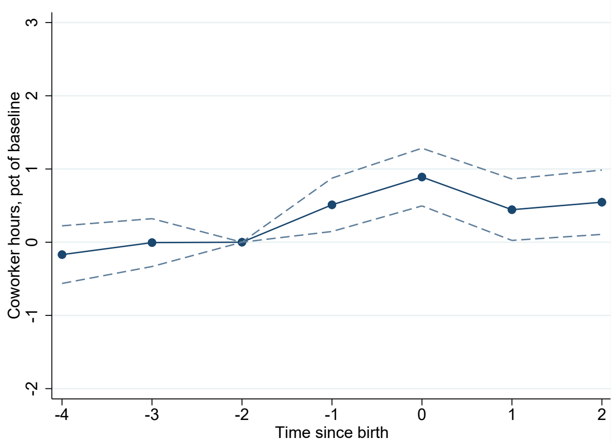
(d) Likelihood coworkers with baseline firm



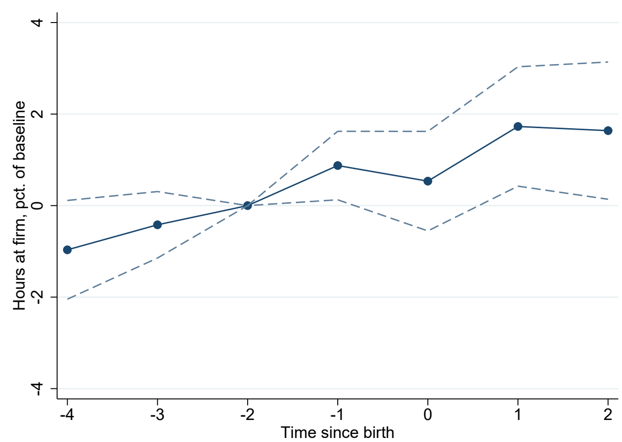
(e) Coworkers' share of year unemployed

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 A11: Effects on hours of work, conditioning on coarser set of observables, OLS



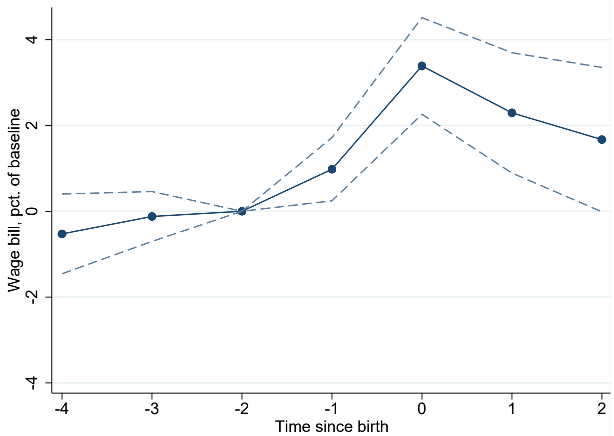
(a) Coworker hours



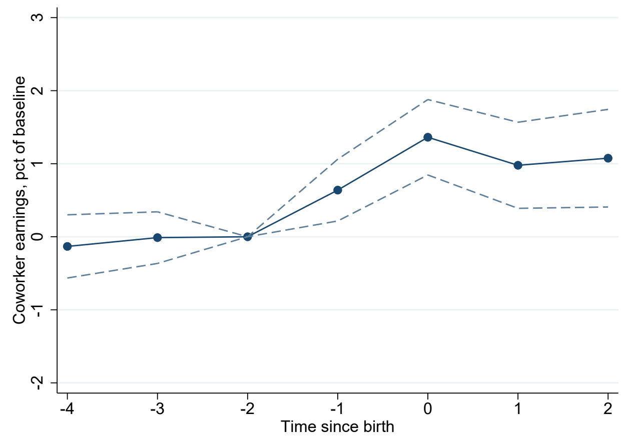
(b) Hours at firm

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.

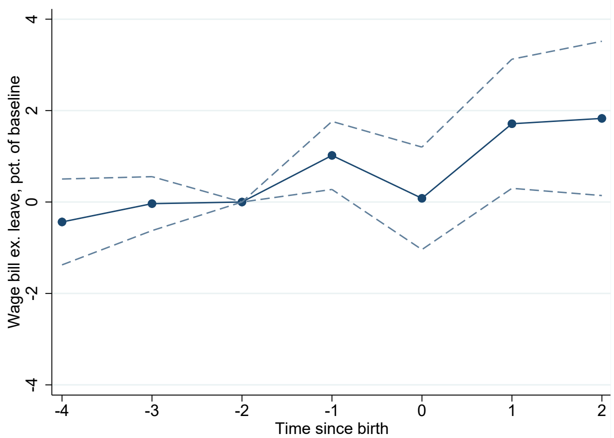
Figure A12: Effects on costs of labor supply adjustments, conditioning on coarser set of observables, OLS



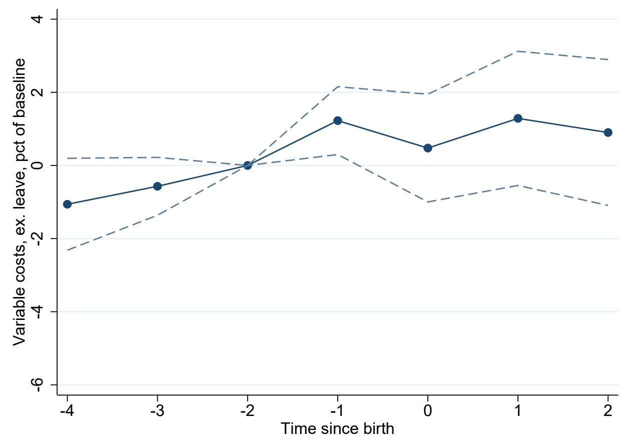
(a) Firms' wage bill



(b) Coworkers' earnings



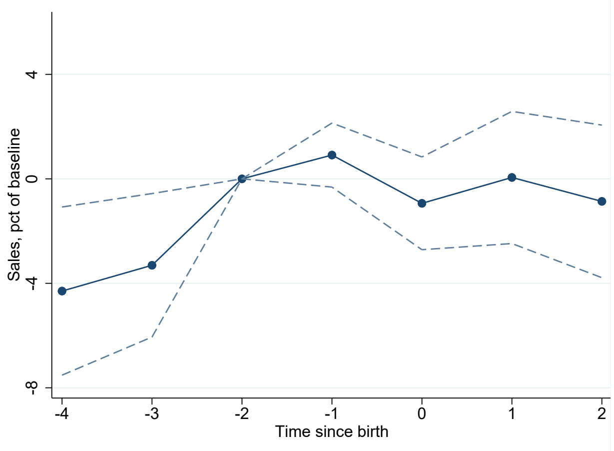
(c) Firms' wage bill (excluding paid leave)



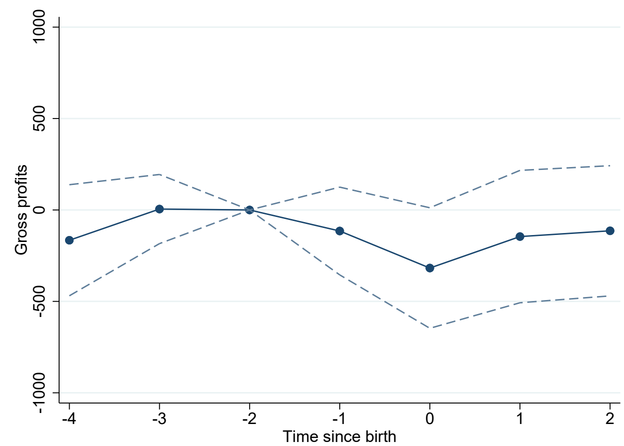
(d) Firms' total variable costs (excluding paid leave)

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.

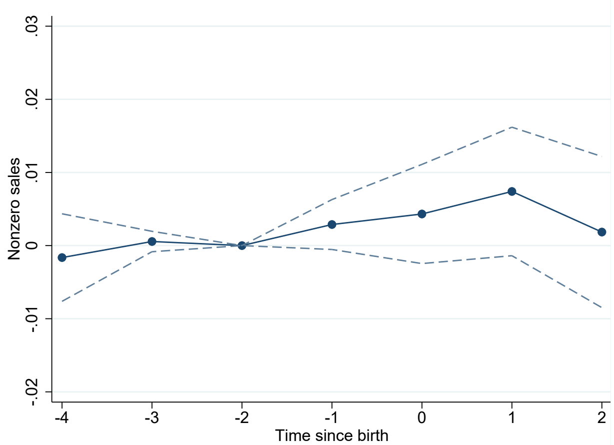
Figure A13: Effect on firms' overall performance and coworkers' sick leave, conditioning on coarser set of observables, OLS



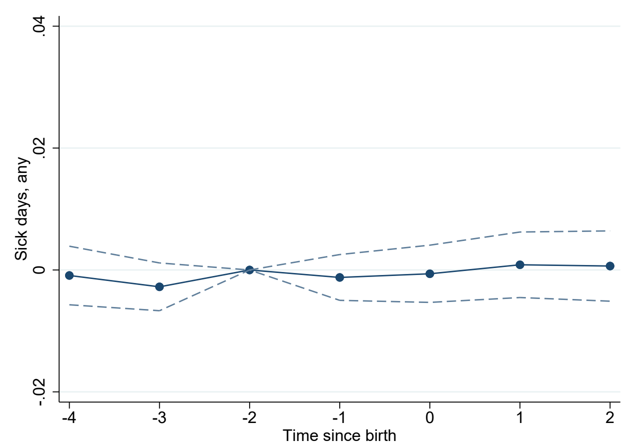
(a) Firms' total sales



(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.

G Results Excluding Duplicate Firms

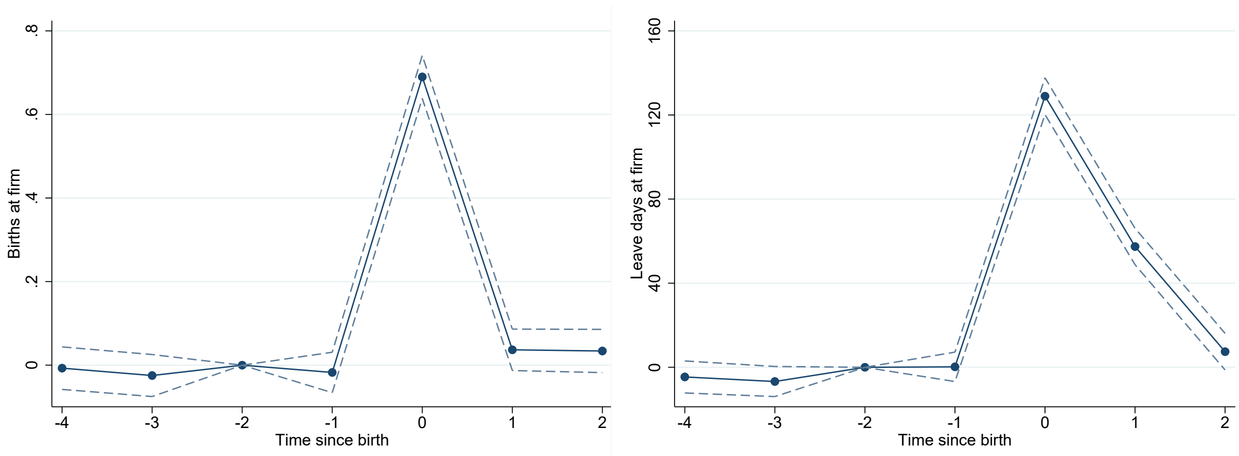
The main analysis sample is defined in terms of potential birth events. As described in the main text, this implies that a single firm may be in the sample several times as part of different treatment and/or control events. Throughout the main analysis, we correct our inference for this duplicity by 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.[75] 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 A14a to A18 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.

[75] 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 A14: Estimates for firms total births and parental leave days, excluding duplicates, OLS

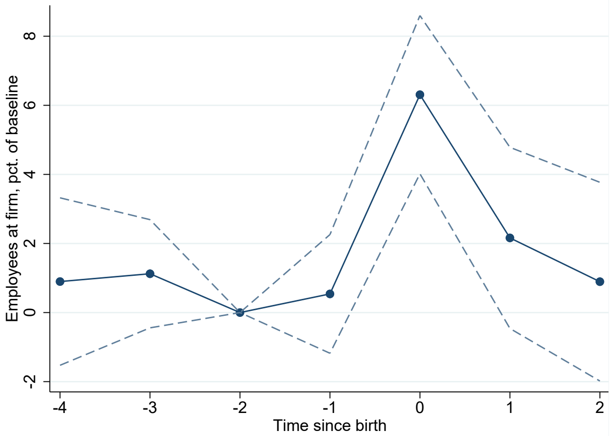


(a) Births at firm

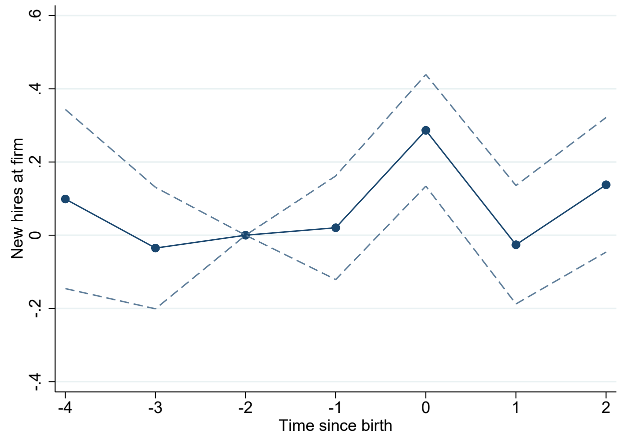
(b) Parental leave days at firm

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.

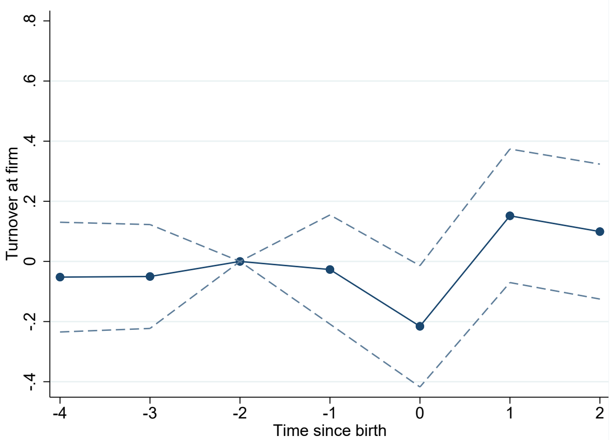
Figure A15: Effects on employment outcomes, excluding duplicates, OLS



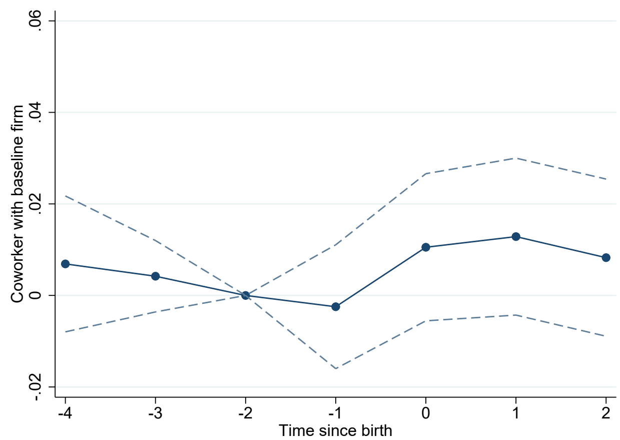
(a) Number of employees at firm



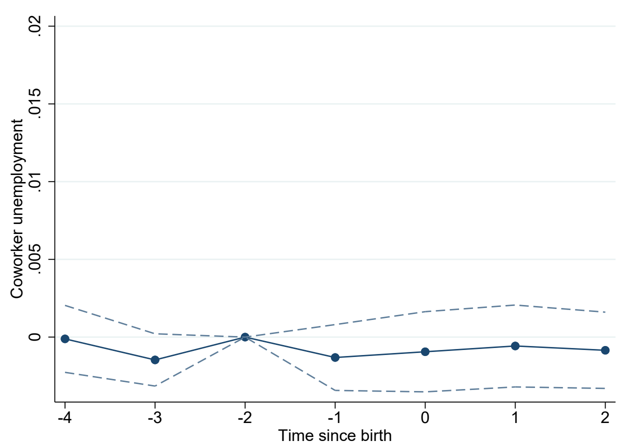
(b) New hires at firm



(c) Turnover at firm



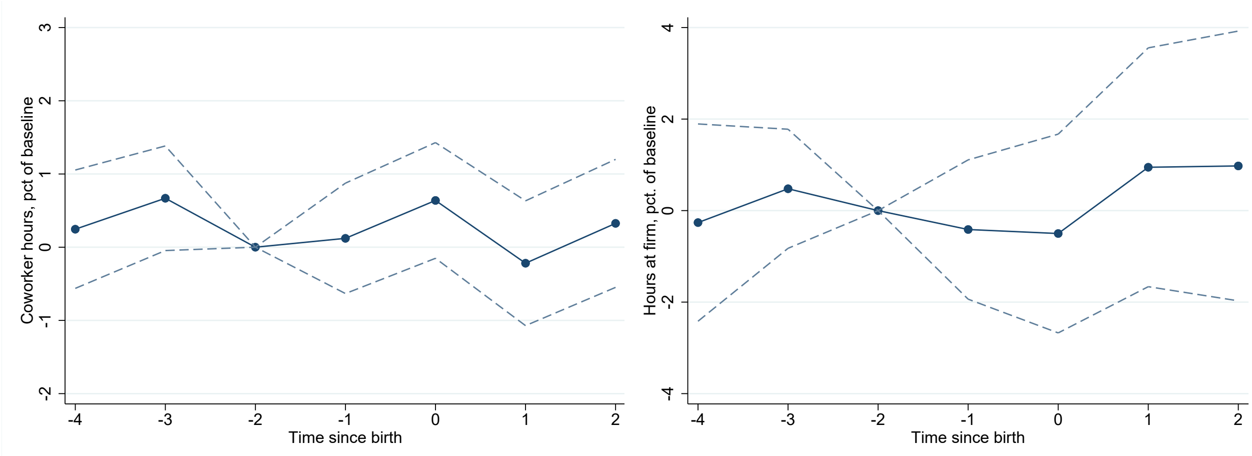
(d) Likelihood coworkers with baseline firm



(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 A16: Effects on hours of work, excluding duplicates, OLS

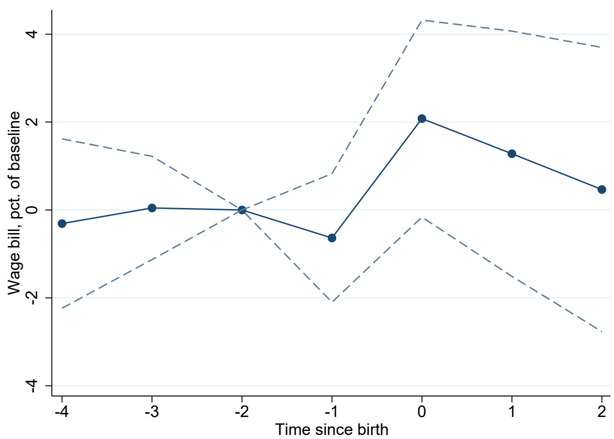


(a) Coworker hours

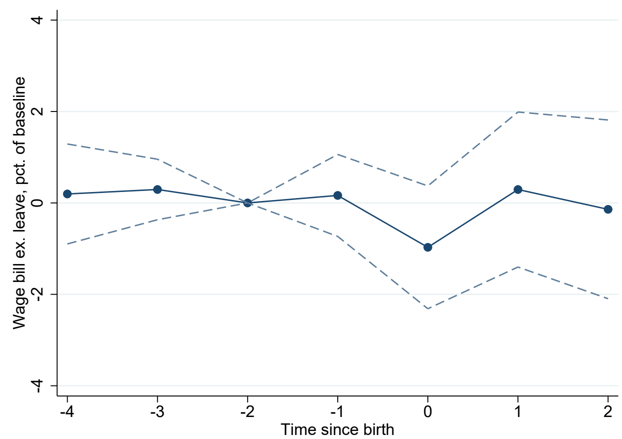
(b) Hours at firm

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.

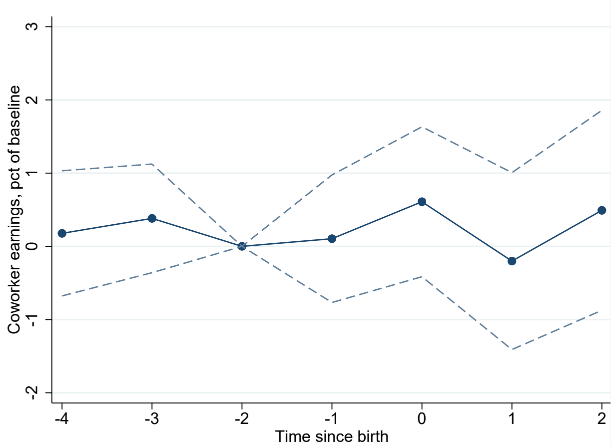
Figure A17: Effects on wage costs and earnings, excluding duplicates, OLS



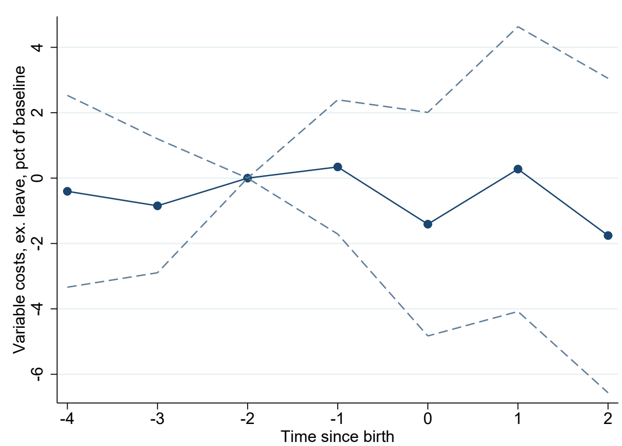
(a) Firms' wage bill



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



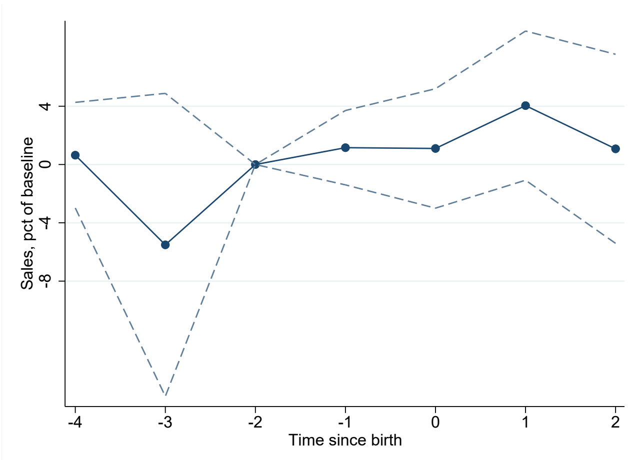
(c) Coworkers' earnings



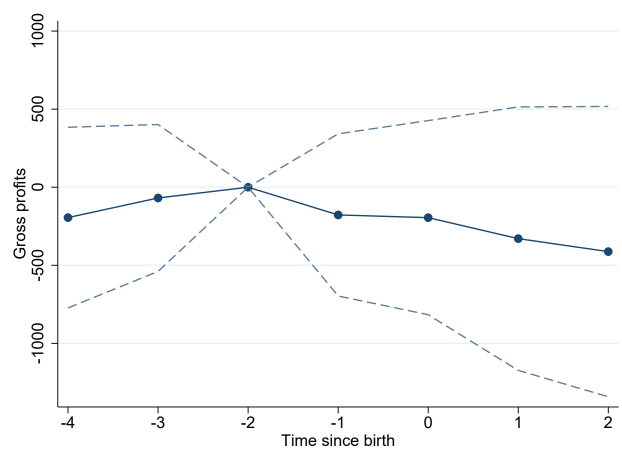
(d) Firms' total variable costs (excluding paid leave)

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.

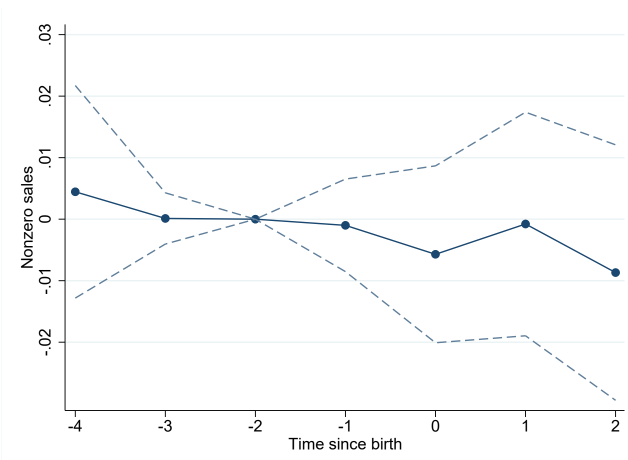
Figure A18: Effect on firms' overall performance and coworkers' sick leave, excluding duplicates, OLS



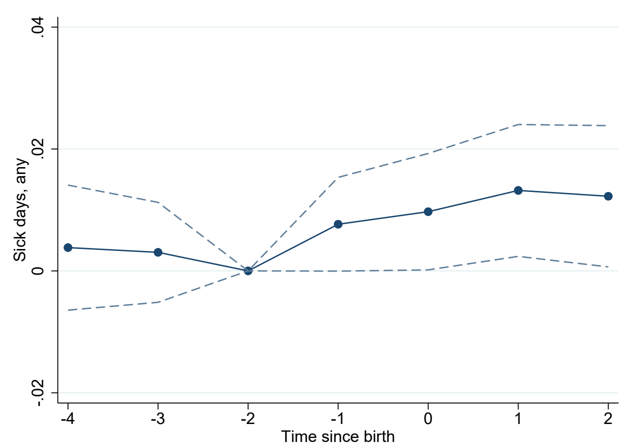
(a) Firms' total sales



(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.

H 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 A8 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 A19. 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.[76]

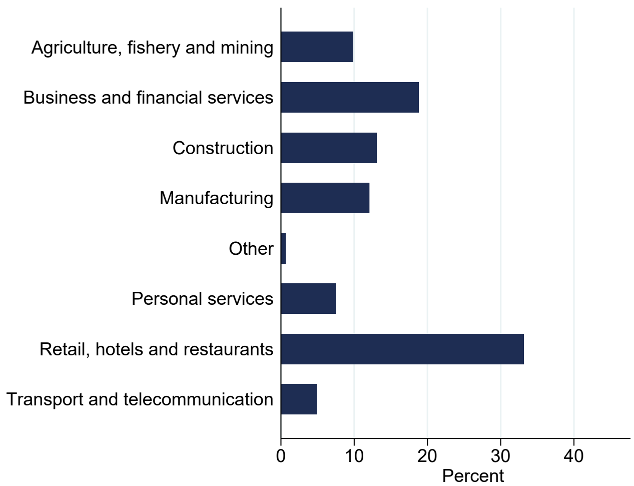
[76] The only exception is the "electricity and water supply" industry. However, even among the universe of private firms and small firms, the share of firms belonging to this industry is very small.

Table A8: Baseline characteristics compared to universe of private and small firms

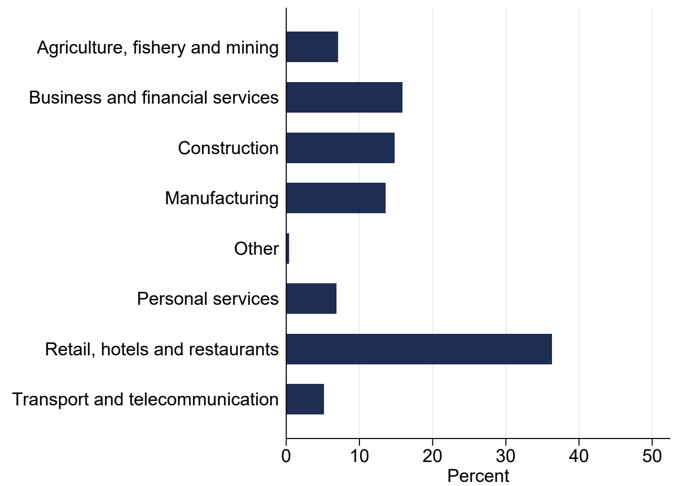
	All Firms	Size Restricted	Treatment Sample
Hours per employee (FTEs)	0.821	0.817	0.813
	(0.594)	(0.273)	(0.219)
Wage bill per employee	259.882	256.816	242.472
	(248.120)	(135.502)	(116.117)
Sales per employee	1461.935	1312.535	1220.743
	(2894.591)	(2735.783)	(1937.685)
Purchases per employee	1018.727	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.

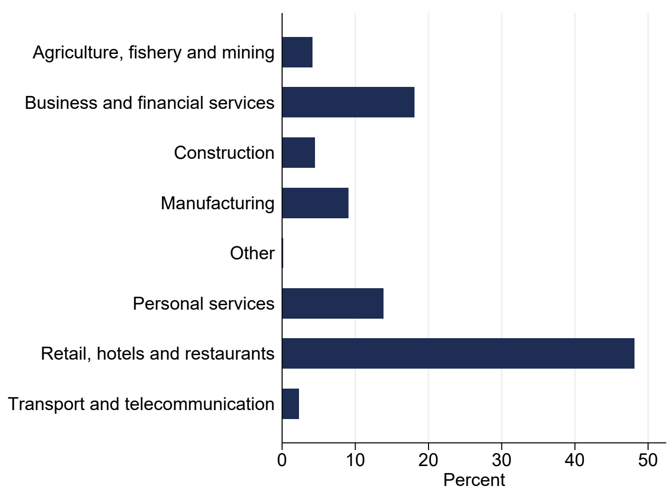
Figure A19: Industry composition by sample restrictions



(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.

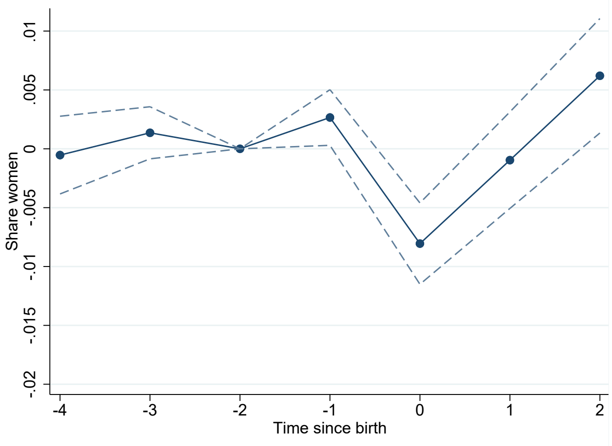
I Effects on Workforce Characteristics

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 A20 and Appendix Table A9).

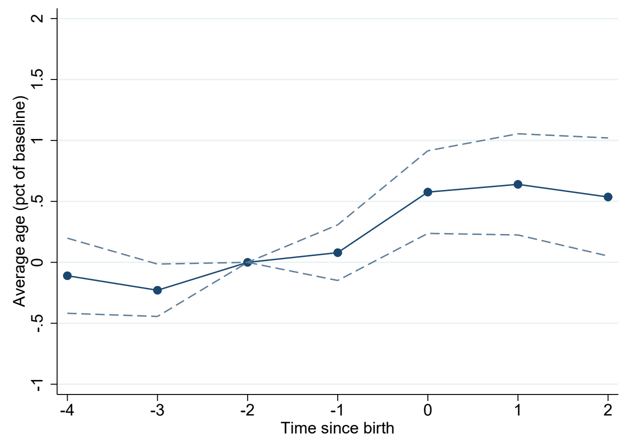
We first find that in the event year, one additional female employee giving birth at the firm lowers the share of women by one percentage point, indicating that a leave-taking woman is replaced by temporary worker of either gender.[77] We also detect small changes in other characteristics. The average 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.

[77] 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).

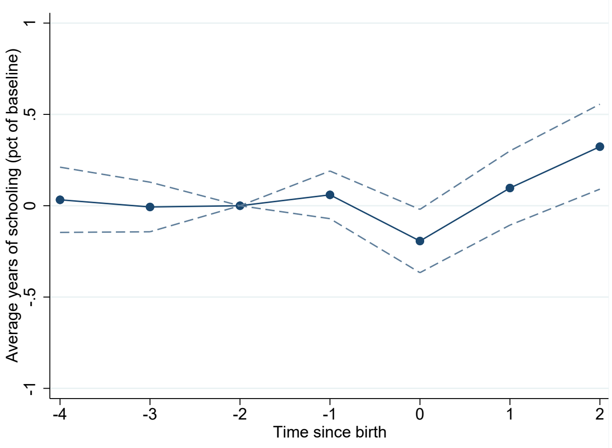
Figure A20: Effect on workforce characteristics, OLS



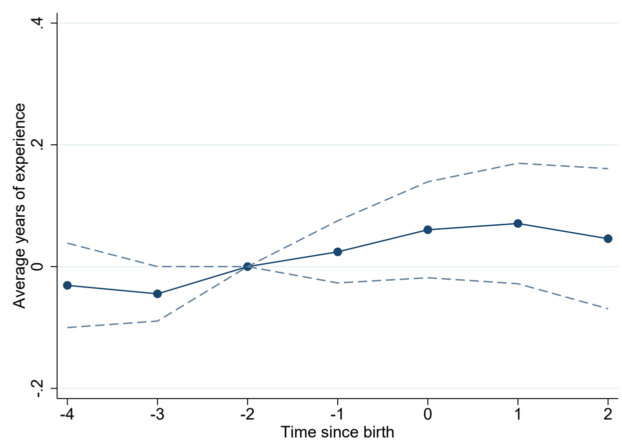
(a) Women's share of the workforce



(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 A9: 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

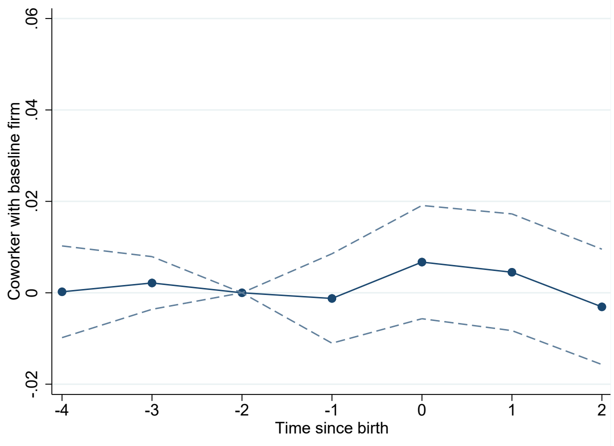
Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the number of births at the firm in the event year is instrumented by the treatment dummy. Columns (3) and (4) show estimates from similar regressions but in 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.

J Effects on Coworkers in Same vs. Different Occupation

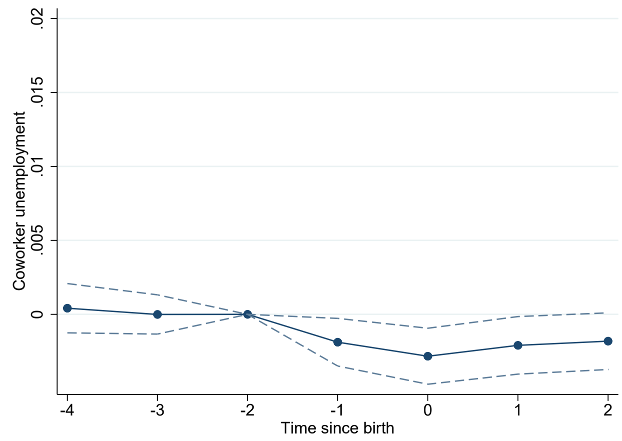
To examine whether the effects of parental leave are different for coworkers who are more likely complements or substitutes for the worker on leave, we split our coworker sample by occupation. For each treatment and control event, we determine the 1-digit occupation of the woman defining the event and 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.

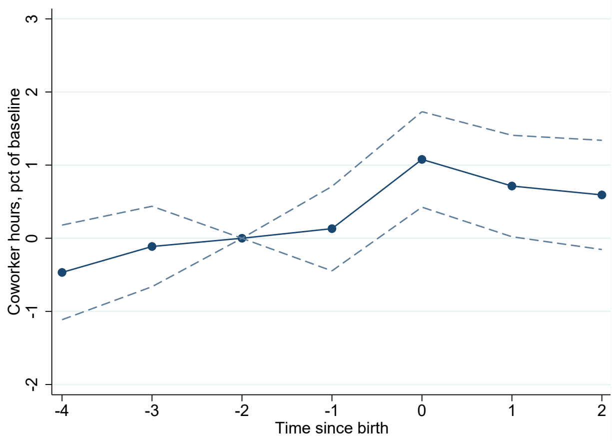
Figure A21: Effects on outcomes of coworkers in same occupations as women on leave, OLS



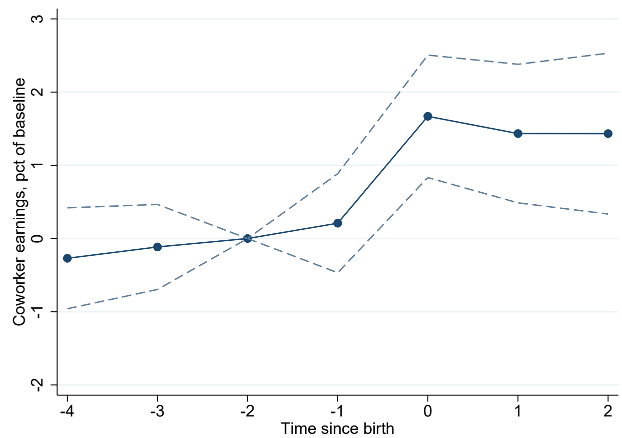
(a) Likelihood of coworker at baseline firm



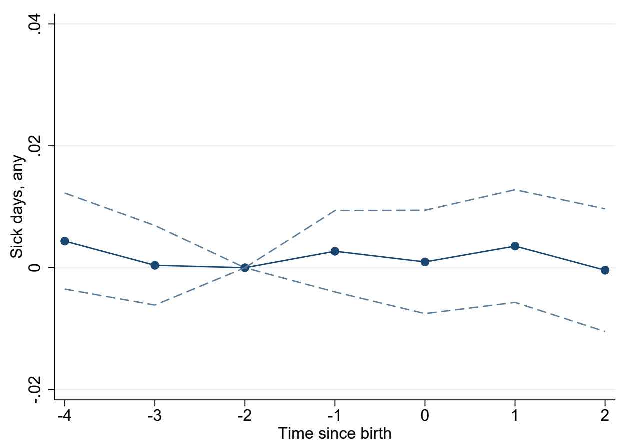
(b) Coworker's share of year unemployed



(c) Coworkers' hours at baseline firm



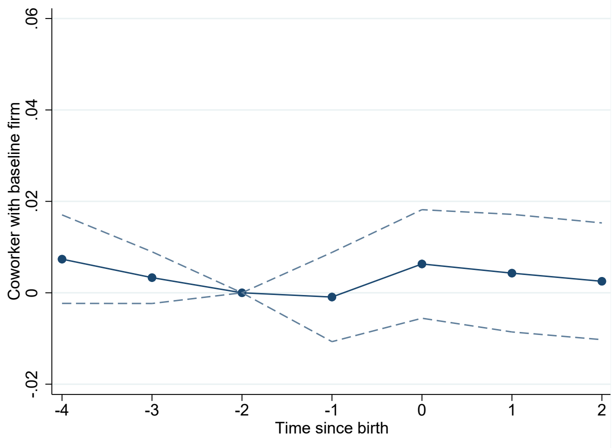
(d) Coworkers' earnings



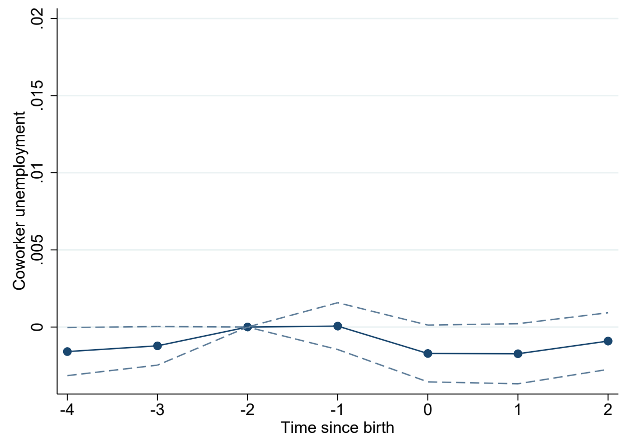
(e) Likelihood of coworker taking a sick day

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.

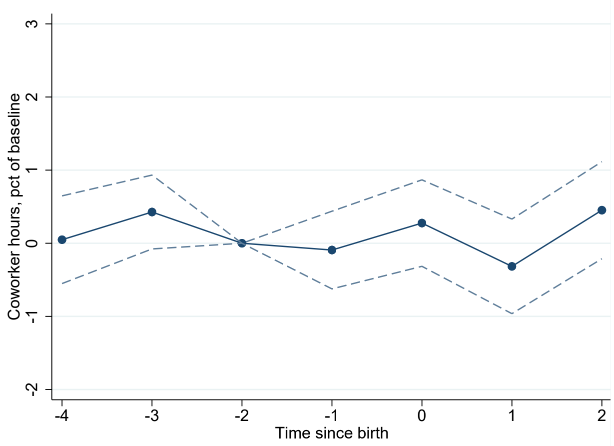
Figure A22: Effects on outcomes of coworkers in different occupations than women on leave, OLS



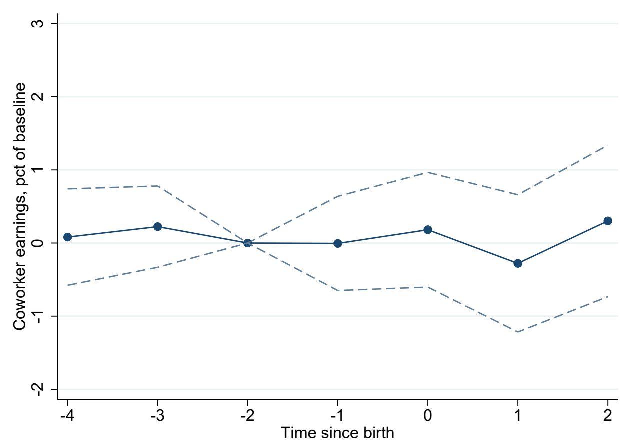
(a) Likelihood of coworker at baseline firm



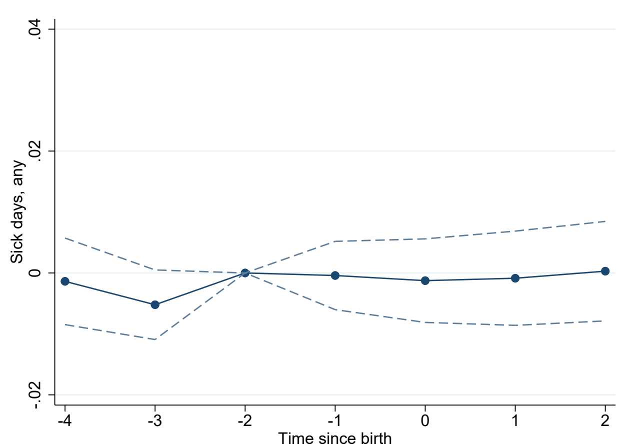
(b) Coworker's share of year unemployed



(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.