IS PARENTAL LEAVE COSTLY FOR FIRMS AND COWORKERS?

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<u>ABSTRACT</u>

Most of the existing evidence on the effectiveness of family leave policies comes from studies focusing on their impacts on affected families - that is, mothers, fathers, and their children - without a clear understanding of the costs and effects on firms and coworkers. We use data from Denmark to evaluate the effect on firms and coworkers when a worker gives birth and goes on leave. 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. When an employee gives birth she goes on leave from her firm for 9.5 months on average. Firms respond by increasing their labor inputs along several margins such that the net effect on total work hours is close to zero. Firms' total wage bill increases in response to leave take up, but this is driven entirely by wages paid to workers on leave for which firms receive reimbursement. There are no measurable effects on firm output, profitability or survival. Finally, 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. Overall, our results suggest that employees going on parental leave impose negligible costs on their firm and coworkers.

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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.*, 2018). 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 wage replacement benefits during parental leave but also include 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 they impact 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. In 2017, then California governor Jerry Brown signed into law a bill that required small and medium-sized businesses to provide new parents with 12 weeks of leave. However, a year prior, he rejected a similar bill 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 a recent review of the literature on leave programs, Rossin-Slater (2019, p.337) concludes that "we know very little about how maternity and family leave policies may impact businesses, who often worry about being burdened with extra costs resulting from dealing with

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[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.

employee leave-taking." 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 reimbursemed 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. These data allow us to link data on individual worker fertility and leave-taking with full administrative data on their employing firm and their coworkers. To identify causal effects of leave-taking on firms and coworkers, we build on the empirical strategy used by Azoulay *et al.* (2010) and Jäger & Heining (2019) to study the effects of losing a coworker. Applying this empirical strategy to our setting, we compare a sample of treatment firms in which a female employee is about to give birth, to an observationally similar set of control firms with female employees that do not give birth over the next few years. We focus on small firms (those with three to 30 employees) which, due to their size, may bear the largest costs of parental leave policies. Relying on a parallel trends assumption for identification, we compare the evolution of outcomes between treatment and control firms in a dynamic difference-in-differences design. 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. An additional advantage of this empirical strategy is that it lends itself to several natural validity checks, which we use to substantiate our identifying assumption.

Our empirical analysis yields several key findings. First, firms in which a woman gives birth are exposed to roughly 282 extra days (about nine and a half months) of parental leave. 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. As a result of these adjustments, firms' total hours of work are roughly unchanged: the 95 percent confidence interval from our preferred specification allows us to reject 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 on firms as best as we can measure. Consistent with the increase in work hours, we document marginal increases in the earnings of existing employees, which are again driven by employees who are in the same occupation as the women on leave. Together with the temporary increase in hires and retention, these changes do lead to an increase in the treatment firms' total wage bill. However, this total wage bill includes wages paid to workers on leave for which Danish firms receive reimbursement. 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. At the same time, 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 allows us to reject that having one percent of the workforce go on leave reduces sales by more than 0.18 percent and that it reduces the likelihood of survival by more than 0.05 percentage points. Taken together, our estimates suggest that the costs of parental leave for employers are negligible.

Finally, we also find no evidence that costs of parental leave are being shifted to coworkers. As noted, coworkers see increases in their hours, earnings, and likelihood of being employed 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.

Our paper contributes to a body of literature that focuses on the impacts of parental leave on a wide range of outcomes. Within that literature, most previous studies examine the consequences of these programs for women's labor market opportunities and child welfare (see Olivetti & Petrongolo (2017) and Rossin-Slater (2019) for a review of the literature).[2]

The evidence regarding the impacts of leave on firms is very limited. Besides a handful of policy reports, we are aware of only one paper that examines the causal effect of parental leave on firms and coworkers.[3] Also using Danish data, Gallen (2017) examines the causal impact of parental leave on

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

[3]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

firms by exploiting a 2002 parental leave reform. This reform made longer leave periods more attractive, which increased the average leave duration from eight to nine months and the modal leave duration from around six to 12 months. Gallen (2017) analyzes this reform using a regression discontinuity design (RDD) based on date of birth, as only employees giving birth after December 31, 2001 were affected by the reform. Gallen (2017) thus focuses on a very different treatment effect than our study. The comparison in her RDD involves firms with the same number of employees on leave but leverages the fact that the leave duration was one month longer on average for employees affected by the reform. In contrast, our study estimates the effect of our treatment firms having an additional employee go on leave, corresponding to an increase in the total leave at these firm of about nine and a half months months on average relative to control firms. This difference may also explain why our results differ from those in Gallen (2017). In particular, we find systematic effects on both firm hiring, coworker hours and coworker earnings. Gallen (2017) finds no overall effects of longer leave durations, although results are sensitive to the choice of sample and time horizon.[4]

Our results and methodology also relate directly to a strand of literature examining the impacts of employee absences on firms and coworkers. Papers within that literature focus on settings that are very different from ours—worker absences, for example, due to deaths (Azoulay *et al.* , 2010; Isen, 2013; Bennedsen *et al.* , 2019; Jaravel *et al.* , 2018; Jäger & Heining, 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 departure of experienced nurses (Bartel *et al.* , 2014). These studies tend to find that unexpected absences for indeterminate periods of time have

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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. As other work has shown that leave take-up rates are endogenous across firms in California however (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.

[4]Among coworkers in the same occupation as the worker going on leave, Gallen (2017) finds a small but statistically significant negative effect on earnings in the year of the reform and again exactly three years later. These coworker results are sensitive to dropping elective c-section births, however, which may be important giving recent evidence on the systematic scheduling of c-sections around the December holidays (Jacobson *et al.* , 2019). When looking exactly five years after the reform, a statistically significant negative effect on firm survival emerges, driven primarily by firms with 15 to 30 employees. Before that time, the estimated effects are initially positive but statistically insignificant and then become negative and again statistically insignificant two to four years after the reform. In contrast, for firms smaller than 15 employees, there is actually a positive and significant effect on firm survival in the short run.

significant negative effects on coworkers' productivity and wages as well as firms' revenues, product quality, and overall performance.[5] Our study is distinct from this work in several dimensions because i) absences due to parental leave are generally anticipated, especially as workers must, by law, inform their employer beforehand, ii) the leave length is known with great certainty, and iii) parental leave is a temporary, not permanent, absence. Our findings suggest that firms are able to adjust and minimize costs when they anticipate the incidence and duration of worker absences—such as absences due to parental leave.

Finally, this paper has broader ties to a number of other strands of the literature. By directly estimating the cost of mandated parental leave policies on firms, the paper ties in with 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). By focusing on Danish parental leave policies, our paper is also related to two recent studies that exploit policy variation in leave programs in Denmark. Friedrich & Hackmann (2017) document that the introduction of a one-year parental leave program in 1994 resulted in a significant shortage of nurses and use this policy shock to estimate the value of nursing care. Tô (2018) uses variation in Danish parental leave policies over time to examine how parental leave take-up acts as a signal to employers about workers' attachment to the labor market. 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 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 re-

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[5]An exception is the study by Jäger & Heining (2019) who find that unexpected worker deaths increase wages and retention rates of same-occupation coworkers. This suggests the presence of labor market frictions. We return to this later in the paper in Section 3.

[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 month is sufficient to qualify for leave.

placement for 4 weeks before birth,[7] 14 weeks immediately after birth, and then have 32 weeks that they can split between themselves and the father.[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, mothers experiencing pregnancy-related health issues, such as symphysis pubis dysfunction, 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 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 the government and is not paid by 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 such 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 pre natal 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 simply increases 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

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[7]Women working in particular jobs (typically physically demanding), 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 in this paper we focus on leave periods with wage replacement that occur immediately after the child is born.

[10]A woman unable to work due to her pregnancy has a right to her full salary, for which the employer will be fully compensated by the UI system.

[11]The wage replacement amount cannot rise above 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.

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, in the majority of cases firms are able to recoup almost all the wages paid to workers on leave.[13] To account for this, our empirical analysis uses data on firms' wage bills 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 can be shared by both parents. 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. In terms of how wage replacements for people on leave are funded, the Danish system of encouraging firms to offer paid 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. In terms of their practical economic implications of course, these differences matter relatively little. In all cases, employers do not 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 in general. 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.

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[12]Prior to 2006, employers could voluntarily join such funds to replace workers wages. Since 2006, membership in a parental leave fund has been mandatory for all employers. 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 for the average woman going on leave, firms in our data get reimbursed more than 90 percent of the paid leave. In addtion, for 49 percent of the women, firms are eligible for full reimbursement for all the paid leave.

3 Understanding the Potential Effects of Parental Leave

This section describes how one may think about 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 the hiring of 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.

In the presence of costly search or other frictions, however, the predicted effects of losing a worker become much different. In this case, the firm may not be able to perfectly replace the worker 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 productivity of coworkers at the firm will change depending on whether the coworkers are complements or substitutes in production relative to the lost worker. For coworkers that are substitutes, productivity will increase, while the opposite is true 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 regardless 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.

On the empirical side, a large body of literature has tested the effect of worker absences due to deaths (Azoulay *et al.* , 2010; Isen, 2013; Bennedsen *et al.* , 2019; Jaravel *et al.* , 2018; Jäger & Heining, 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) or general departures (Bartel *et al.* , 2014). Broadly speaking, results in this literature have tended to confirm the predictions from the frictional model outlined above. Most recently, Jäger & Heining (2019) show that when a German worker dies unexpectedly, their coworkers' wages and retention rates change in ways consistent with a frictional labor market. Extrapolating these conclusions to our setting, the existence of labor market frictions implies that workers going on parental leave should impose significant costs on firms as well as on any coworkers who serve as complements to the worker on leave.

In a number of ways, however, having a worker go on parental leave in our setting is distinctly different from having a worker die, fall ill, or be absent for some other reason. First, firms can anticipate and plan around absences due to parental leave.[14] In addition, parental leave typically only implies a temporary absence that ends at a specific known time.[15] Because the leave is temporary, upward wage adjustments for similar coworkers (as found in the Jäger & Heining (2019) paper) may not be tractable as firms would hesitate to later lower wages when the absent worker returns and higher wages may no longer be justified. Finally, in the context we study, parental leave is not an uncommon event so we can expect firms to be accustomed to having employees on leave. These differences may very well allow firms to mitigate the negative effects of having a worker go on leave even if the labor market is characterized by important frictions. This motivates the empirical analysis in the present paper, which directly estimates the effects of a worker going on leave on firms and coworkers.

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.[16] By basing our analysis on these firm identifiers, 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).

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[14]The Danish parental leave policy requires mothers to announce their pregnancy to employers at least three months before giving birth. It is common for women to generally announce their pregnancy at the end of the first trimester.

[15]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, female employees who take leave are actually slightly *more* likely to stay with their employer over the next few years than are comparable women who do not give birth.

[16]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).

4.1 Worker Data

Our linked administrative data yield a range of characteristics and outcomes for workers. From the CPR, we obtain basic demographic information such as age and gender. Using parent-child linkages and information on birthdays, we further construct data on when workers give birth, as well as the number of children each worker has. We use data on the payout of parental leave benefits to individuals and the payout of leave reimbursements to firms to calculate the total number of days of paid and unpaid leave for each worker.[17] As described in Section 2, unpaid leave means that the worker receives a direct wage replacement from the government, while paid leave occurs when firms continue to pay the worker on leave (but is reimbursed for these payments). Throughout, we consider both prenatal and postnatal leave. In our measure of prenatal leave, we include instances in which the leave period is extended because of health issues related to pregnancy (e.g. symphysis pubis dysfunction, see Section 2). 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, hours of work, and wages.

To measure the stock of employees at a firm, we use the standard IDA definition of "main November employment relationship."[18] Under this definition, a worker is considered employed at a firm in a given year if his main job was at that firm in the last week of November.[19] We refer to the total number of such workers as the number of *employees* at the firm.[20] Importantly, we note that this measure of employee stock includes workers on leave.

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[17]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 leave immediately before and after delivery. Prenatal leave includes pregnancy-related sick leave. In 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.

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

[19]The main job is defined as the job with the most hours, and in the case of any ties, the one with the highest earnings.

[20]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.

In addition to examining the stock of employees at a given time, we are also interested in examining changes in hours worked. A useful feature of our data is that we can construct an approximate measure of how many hours each worker has supplied to a firm by using data on mandatory pension contributions from firms (ATP). The contribution amount increases approximately linearly with the number of weekly work hours and also scales linearly with the number of weeks worked during the year (Lund & Vejlin, 2016). Appropriately scaling the contribution amount therefore gives us an approximate measure of total hours supplied during the year. However, due to the details of the ATP contribution system, the measure does have the important drawback of not capturing overtime work for full-time employees.[21] 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 leave, we subtract the share of the year that each employee is on paid parental leave.[22] Unlike employee stock, the measure of hours worked does not include instances when the worker is on leave. Again, to the extent full-time employees work overtime in response to the leave, our FTE measure will understate the true FTE.

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.[23] By examining the effects of parental leave on both the total

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[21]The ATP contribution schedule 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. An alternative data source that better captures overtime hours is available starting in 2008. However, using this source would leave us with too short a sample window for the analysis.

[22]This introduces some measurement error for employees who hold more than one job since their total number of days on leave will reflect leave-taking from all jobs. Among worker-years in which the person has at least one job and takes some leave, 17.3 percent hold more than one job during the year. Most of the duplicate jobs are very small and make little difference in the calculation: only 3.2 percent hold a second job that has more than half the hours of the main job.

[23]For each worker, we divide their 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 has covered. This gap is then subtracted from the total wage bill to arrive at a measure of total wage bill excluding leave payments. Inherent in this measure is some error for people who hold two jobs. Cases with zero or missing pension contributions are treated as working full time all year for the purpose of these calculations. Among worker-firm years for which the worker has positive parental leave and positive earnings, 7.9 percent have missing or zero pension contributions. These are all concentrated among very low earners. If we further condition on having earnings of more than 30,000 DKK (4,000 EUR or 4,500 USD)

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.[24]

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 given the revenue exceeds some value.[25] We use total *sales* as our measure of firm output and use firm *purchases* for an identification check.[26] To get a measure of firm profitability, we create a proxy for *gross profits* by subtracting purchases and the total wage bill 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.[27]

One important feature of most firm data is that many firms enter and exit the market each year. Since 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 for 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.[28]

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. An obvious obstacle to this aim is the potential endogeneity of fertility and leave taking. Policy reforms, as have been leveraged in much of the work on parental leave, could provide a useful tool to overcome this challenge. However, in the modern era in which

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for the year, this calculation falls to 1.4 percent.

[24]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 2).

[25]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.

[26]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.

[27]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.

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

rich administrative data are available for such endeavors, reforms to parental leave policies have been relatively modest, implying that the relevant variation in leave taking is limited. For example, by 2001, when the detailed Danish firm data we use were first available, virtually all female employees were already eligible for at least six months of parental leave with the same generosity in terms of wage replacement as today (Beuchert *et al.* , 2016).

Instead of relying on policy variation, we therefore adopt an empirical strategy that has been applied in previous high-impact work on the causal effects of worker deaths (Jäger & Heining, 2019, see also Azoulay *et al.* , 2010). Adapted to our setting, the empirical strategy is based on a difference-in-differences comparison between a sample of treatment firms in which a female employee is about to give birth and a comparable sample of control firms with female employees that do not give birth over the next few years. In this section, we begin by discussing the ideal experiment that our empirical strategy tries to mimic. We then describe the construction of our samples of treatment and control firms, our firm-level and coworker difference-in-differences specifications, and the matching and reweighting procedure we use to ensure that our treatment and control firms are ex-ante similar. Finally, we discuss the validity of our empirical strategy.

5.1 Mimicking the Ideal Randomized Experiment

We begin by describing the experimental ideal that our empirical strategy tries to mimic. Under the ideal experiment, an omnipotent researcher selects a sample of women of childbearing age with stable employment relationships (in a country with a parental leave policy in place). Half of the women are randomly assigned to have a child exactly two years later (treatment) and the other half of them are assigned to not have a child at any point over the next few years (control). In addition, all of the women are forbidden from leaving their current firm. Two years later, the firms of women in the treatment group will experience an additional employee having a child and going on leave relative to the firms in the control group. Comparing firm outcomes across the two groups in the birth year of the treatment group will therefore identify the immediate causal effect of having an additional employee give birth and go on leave. In addition, comparing the groups in subsequent years would identify any causal effects that appear over time, while comparing them in the year prior to the treatment groups' childbirth would identify any effects that materialize when the employee becomes pregnant and the employer learns about the upcoming leave.

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Our empirical strategy tries to mimic this experiment using observational data. We face two main challenges. The first challenge is that future fertility is not randomly assigned. Instead, women who give birth in the near future are likely to be in systematically different firms than women who do not. Looking at firm outcomes for the group of women who do not give birth in the near future is therefore not likely to provide a valid counterfactual for the firms of women who do give birth. We attempt to remedy this in two steps. First, we condition on a rich set of observables so that the treatment and control samples are similar on predetermined individual and firm characteristics two years prior to the potential birth. Second, we employ a difference-in-differences design that compares changes over time between treatment and control firms. This approach allows for pre-existing differences between the treatment and control firms but assumes that the evolution of the firms would be the same in absence of the birth. Assuming that—after conditioning on observables—the two groups of firms experience parallel trends, this difference-in-differences will capture the causal effect of parental leave. The intuitive idea behind the identifying assumption is that once we condition on initial firm characteristics and relevant women's labor market conditions, the difference in women's fertility outcomes between the groups are driven by time-varying factors that are as good as random from the perspective of the firm (marriage market shocks, variation in conception success, etc.) As we describe in further detail later, the identifying assumptions can be examined through checking the balance of pre treatment characteristics and examining pre trends.

The second challenge we face is that, in our sample of young women, workers do not all stay with the same firm over time. In particular, some of the women in the treatment group leave their firm before giving birth, implying that not all firms in our treatment group actually end up experiencing an extra birth. As described in further detail later, we handle this issue in the same way imperfect treatment compliance is handled in randomized controlled trials: with imperfect compliance, basic difference-in-differences estimates are still valid as intent to treat (ITT) estimates. Moreover, we can use treatment assignment in an instrumental variables setup to recover a local average treatment effect (LATE).

5.2   Constructing the Treatment and Control Groups

We create a sample of what we call potential future birth events from which we compose our treatment and control samples. A potential event here 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, we refer to the year in this combination as the *baseline year*. For reasons that will become clear below, we refer to the year two years after the baseline year as the *event year*.

Within our sample of potential future birth 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.[29] For both sets of events, the associated firm is the firm at which the woman is employed in the baseline year. As we describe further below, our empirical analysis involves comparing firms in the treatment sample to firms in the control sample in a difference-in-differences framework around the event year.

Before proceeding, two things we note about our sample construction. First, we do not require that a woman stays with the firm beyond the baseline year because job mobility around childbirth may be endogenous. Thus, some treatment firms will not actually experience an additional birth in the event year because the woman giving birth has moved to a different firm. Second, the same firm may show up several times in our sample as part of different events. Each firm is observed over multiple years and has several employees so each firm could in principle enter the sample once for every female employee and every (baseline) year. In practice, the various sample restrictions discussed below imply that most firms do in fact only show up once in our analysis sample.[30] However, we still need to correct for the potential duplicity when conducting inference.

We place several restrictions on this set of potential leave events. First, 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. By the baseline year, the woman must have been with the firm for more than one year.

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

[30] Of the firms in our sample, 58.2 percent show up once in our sample, 19.8 percent show up twice, while 22.0 percent show up three or more times. Instead of including duplicate firms, we could restrict the sample such that each firm is in the sample only once. However, this sample restriction could lead to endogenous sample selection (i.e., dropping a control firm in some years because of employees giving birth in other years).

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

Restriction 1 ensures a focus on prime-childbearing-age women.[31] Restrictions 2 and 3 ensure a focus on women with reasonably strong labor market attachment. Second, we impose the following restrictions on firms:

5. Based on sales, hours, and the total wage bill, the firm must be active in the baseline year.[32]

6. The firm must not be an extreme outlier in terms of growth, sales levels, or wage bill.[33]

7. The firm must be in the private sector.

8. Following Jäger & Heining (2019), we restrict our sample to small firms in which our measure of the stock of employees is between 3 and 30 employees 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.[34]

Restrictions 4 to 6 ensure that our results are not driven by a small number of outlier firms or by firms who were de facto inactive in the baseline year. Restriction 7 is necessary because our measures of firm performance (sales, firm closure, and profits) are not well defined for the public sector.[35] Restriction 8 warrants additional discussion. As in previous work, the focus on small firms is necessary because our research design leverages year-to-year changes in whether one additional female employee gives birth. At larger firms, the effect of a single individual on leave should be smaller than that at small firms given that the implied share of workers on leave is much less at large firms. At the same time, we note that much of the interest in leave policy centers on small firms for this reason.

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[31]As described above, we examine the effect of women's fertility two years after the baseline year. Ages 21 through 35 are the prime childbearing years for Danish women over our sample period.

[32]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.

[33]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).

[34]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.

[35]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.

5.3 Firm-Level Difference-in-Differences

Using data on the treatment and control events (along with data preceding, during, and following the events), we estimate a dynamic difference-in-differences specification of the following form:

$$Y_{eft} = \gamma_e + \sum_{k \in \mathcal{T}} \alpha_k \mathbb{1}_{t=k} + \sum_{k \in \mathcal{T}} \beta_k \mathbb{1}_{t=k} \cdot Treatment_e + \varepsilon_{eft} \tag{1}$$

$$\mathcal{T} = \{-4, -3, -1, 0, 1, 2\}.$$

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. For each event e, we use data ranging from four years prior to the event to two years following the event in the estimation.[36] Turning to the regression parameters, 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. $t = -2$.

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 (again, the baseline year $t = -2$ is the reference period). Under a parallel trends assumption, these coefficients identify causal effects of workers' going on parental leave:[37] β_0 identifies the contemporaneous effect in the year of birth, whereas β_1 and β_2 demonstrate the later post birth dynamics. β_{-1} identifies any effects of a birth that materialize in the year prior to the birth's occurring. For example, women may be on pre natal leave in the year before the birth.[38] At the firm level, management may make adjustments

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[36] 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. Moreover, women in our data are very likely to have another child around three years after their most recent birth, making the comparisons across treatment and control groups problematic when we look beyond the first two years after the event.

[37] Given our definition of treatment, we note that these estimates identify the impact of a birth to a treatment firm. As we show later, for most women, giving birth is synonymous with going on parental leave for an extended time. Therefore, we refer to the effects of having a female worker give birth and a woman taking leave interchangeably.

[38] Recall that time is measured in calendar years, so a birth at the beginning of 2005 may have associated prenatal leave in 2004, the year before.

in this year before the leave in anticipation of the leave. Our estimate of β_{-1} will capture any such anticipation effects.

Finally, the coefficients β_{-4} and β_{-3} provide useful validity checks of the identifying assumption. Ideally, these coefficients should hover around zero—signifying that outcomes among treatment and control firms evolve along parallel trends in the periods before the birth occurs (i.e., a test of the parallel trends assumption).

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 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 use a balanced sample that includes only firms for which we have data for all seven years.[39] 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.[40]

5.4 Dealing with turnover and imperfect compliance

As discussed previously, the construction of our sample of treatment firms implies that there will be imperfect compliance: treatment firms are firms that in the baseline year employ a woman who gives birth in the event year, but in some cases, this woman will leave the firm before 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 ITT estimates that show qualitatively how parental leave impacts 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 that use treatment status as an instrument and recover 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):

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

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[39] 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 are missing data on event time $t = -4$ or $t = 2$. All our main conclusions hold if we instead consider an unbalanced panel.

[40] We cannot do this for all outcomes because some of our outcome variables can be zero or negative in the baseline year. None of our qualitative conclusions are sensitive to the scaling.

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.[41] 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:

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

$$BirthsInEventYear_{ef} = \delta_0 + \delta_1 Treatment_e + \epsilon_{ef} \tag{3, First Stage}$$

Under the same parallel trends assumption as before, 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 also obtain LATE estimates of any non contemporaneous effects in the year after the event year, we simply modify the outcome equation (3), so that it involves changes in the outcome variable between the baseline and the year of interest.

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. However, because many of our outcome variables are measured relative to the baseline we are sometimes more interested in quantifying the relative effect of having some specific fraction of the baseline employees go on leave. Accordingly, we also 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:

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

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

For ease of interpretation, we measure baseline employment in 100 baseline employees when estimating

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[41]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 $t = -4$.

(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.[42] 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).[43] As before, 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. For all our firm outcomes that are binary or are measured relative to baseline, we take 2SLS estimates from (4) as our preferred estimate. 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)).

5.5 Coworker Analysis

To understand the effects of leave on coworkers, we adopt a parallel analysis to the one described earlier. 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.[44] Analogous to the firm analysis, we then follow these baseline coworkers from four years prior to the event year until two years after the event.[45] Specifically, we estimate OLS and 2SLS specifications that are completely analogous to (1), (3), and (4) 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). Because effects on coworkers are likely to differ depending on the total size of the employment stock, we take (4) as our preferred 2SLS specification for all coworker outcomes. For inference, we continue to cluster

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

[43]The matching and reweighing procedure that we discuss later 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.

[44]We make this restriction to confine the sample to those with a relatively strong attachment to the firm at the baseline. We do not have relevant coworkers for only five of the events, in which case we drop these firms from the coworker analysis.

[45]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.

standard errors at the firm level. Appendix B provides additional details for the coworker specification.

5.6 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 trends. In the raw data, however, the firms and women underlying treatment and control events are already quite distinct in the baseline year. This difference is not surprising. For example, women who have decided not to have children—and who are thus contributing to our control sample—may have selected into systematically less family-friendly firms (e.g., those that require lots of travel and long work hours). These differences, however, raise obvious concerns about the validity of the parallel trends assumption.

Following Jäger & Heining (2019), we address these concerns by conditioning our analysis on a rich set of baseline observables. The point of this is to ensure that in the baseline year, firms in our treatment sample look similar to our control sample. To be consistent with previous papers, we implement the conditioning by matching and reweighting on observables. In other words, before estimating the regression specifications discussed above, we first apply a matching and reweighting procedure to our sample. As we show in Appendix C, however, we can obtain similar results using a purely regression-based approach.[46]

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 is related to labor market returns (Kleven *et al.*, 2018) 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.

The specific matching and reweighting procedure we use can be viewed either as a form of exact matching (one-to-many) or as propensity score reweighting using a nonparametric propensity score estimator. Here, we describe it in the language of propensity score reweighting.[47] For each treatment

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

[47]Viewed as exact matching, our procedure matches each treatment observations to all control events that have exactly the same values of the observables. The control events are then reweighted to account for the fact that the number of matches will differ for the different treatment units: If a treatment event is matched to K control units, then each of these

and control event, we estimate the propensity score p_e: the probability that the event is a treatment event given its observables. We then do propensity score reweighting so as to estimate the average treatment effect on the treated (ATT). This involves assigning a constant weight of one to all treatment events and weighting each control event by $\frac{p_e}{1-p_e}$.

Because our selected set of observables are all discrete, we estimate the propensity score non parametrically by simply computing what share of events are treated at each possible level of the baseline observables.[48] Using this non parametric propensity score estimate implies that the distribution of observables will be exactly identical after reweighting, even in finite samples.

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 observations with an estimated propensity score above 0.9 or 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 performance of the estimator. 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.

5.7 Descriptive statistics

Now, having completely described our analysis sample, we present descriptive statistics. Table 3 first shows how our various sample restrictions change our working sample size. Not surprisingly, the most limiting sample restriction is the restriction to small firms. This large reduction in sample size reflects that by definition, most workers work at firms with many employees.[49] The bottom of Table 3 shows that trimming based on the propensity scores also reduces the sample significantly. Of the initial 23,762 treatment events in our sample, 9,941 (41.8 percent) are left after trimming events with an estimated propensity score above 0.9 or below 0.1. The fact that we lose so many observations due to trimming

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controls receives a weight of $\frac{1}{K}$.

[48]Formally, let $N_T(x)$ be the number of treatment events whose vector of observables is equal to x and let $N_C(x)$ be the corresponding number of control events. Our estimate of the propensity score for an event e with observables equal to x is then $\hat{p}_e = \frac{N_T(x)}{N_T(x)+N_C(x)}$.

[49]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 7.5 percent. The reason for this is that public sector firms tend to be very large in our data. Once we restrict attention to small firms, most of the public sector has already been dropped.

reflects the fine-grained nature of our matching and reweighing procedure, which uses a very detailed set of observables and a non parametric propensity score estimate. In Appendix D, we examine the characteristics of our analysis sample to understand how representative our final analysis sample is. Compared to the universe of firms in Denmark, firms in our treatment sample experience more births and leave days per employee. However, 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. In Appendix E, as a robustness check, we further present results from a coarser matching and reweighting procedure that trims significantly fewer observations. The estimated effects of leave are similar; however, we see indications that the identifying assumption may fail when this less careful set of conditioning variables is used.

Table 4 shows (weighted) summary statistics for the baseline year of the firm and coworker samples that we use to estimate equation (1).[50] In the baseline year, on average, firms experience 0.79 female employees giving birth, 137 days in total of leave taken by female employees, have 12.9 employees of which 65 percent are women, and have a wage bill of 3.4 million DKK (455,000 EUR or 500,000 USD). Finally, Figure 2 shows the distribution of the length of prenatal and postnatal leave respectively among the women in our sample of treatment events. From this, it is clear 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.

5.8 Threats and Limits to Identification

The identifying assumption in our research design is that, after conditioning on our set of baseline observables, outcomes at treatment and control firms would exhibit parallel trends in the absence of a birth occurring. This assumption asserts that once we condition on women's relevant labor market characteristics and initial firm characteristics, time-varying factors driving women's future fertility outcomes are unlikely to be related to firm-level shocks.

Two threats to this assumption are salient. The first is that fertility decisions may in fact be driven directly by events occurring at treatment or control firms. A positive demand shock at the firm, for

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

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. A strength of our dynamic difference-in-differences design is that we can compare the time trends in our outcome variables between treatment and control events before a birth takes place. Recall that our difference-in-differences specification uses data up to four years before the birth. If the standard pretrends assumption is met, treatment and control firms should look similar prior to the event of the birth. As we show later, we see little evidence of differential trends in fertility (and other outcomes) in years prior to the treatment event. Adding further credence to this identifying assumption, we note that the precise planning of fertility is a rarity as many births are unintended (i.e., the couple was using fertility-control methods at the time of conception) or occur after several months of trying to become pregnant.[51]

The second threat concerns sorting. In particular, women who know their future pregnancy intentions may sort into particular types of firms and these firms may experience different time trends.[52] In our matching and reweighting procedure, we condition on a fairly detailed set of observable firm characteristics. We may still worry that treatment and control firms are different on other dimensions, however. We assess this concern in two ways. First, as discussed in the previous section, our dynamic difference-in-differences specifications show no indications that treatment and control firms experience different time trends prior to the event year. Second, we can inspect whether treatment and control firms look similar in terms of their predetermined characteristics at the baseline using characteristics that were not targeted in the 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. Appendix Figure A1 further compares the industry composition of our treatment and control samples.. The samples are well balanced on industry as well.[53]

Finally, in addition to assessing threats to identification, it is important to be precise about what

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[51]For example, about 37 percent of all births are unplanned at conception in the United States (Mosher *et al.*, 2012). Among healthy women of prime fertile ages trying to conceive in a German study, the probability of conception during one cycle is less than 40 percent (Gnoth *et al.*, 2003). Te Velde *et al.* (2000) confirm this finding and document that roughly 50 percent conceive within three months and 80 percent conceive within six months. However, these statistics provide an upper bound on the birth rates since not all conceptions will result in births. One recent estimate of the birth success rate, based on donor eggs used in vitro fertilization, suggests that the probability of a birth given a conception is roughly 50 percent (Rice, 2018).

[52]Baseline differences will not introduce bias because these will be controlled for via event fixed effects.

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

exact treatment effects our analysis aims to identify. Here, the role of anticipation effects merits some additional discussion, especially in relation to the previous literature. As noted, our empirical strategy closely follows the one used by Jäger & Heining (2019) to study the effect of worker deaths. In their implementation, Jäger & Heining (2019) focus on unanticipated worker deaths because these provide the cleanest tests of the theoretical hypotheses considered in that paper. This is different from our analysis, which focuses on evaluating a specific policy. With the parental leave policy in place, firms in both our treatment and control sample will already be aware that young female employees may later give birth and go on leave. Moreover, during the pregnancy and before workers go on leave, treatment firms will learn about the upcoming leave with certainty and will be able to respond in advance. Our empirical strategy thus estimates the effects of having a worker go on parental leave in a setting in which these types of anticipation effects are present. This is the policy-relevant effect for understanding how leave-taking affects firms in equilibrium.

At the same time, because our research design examines differences between firms under a given parental leave policy, we note that our design will not adequately capture general equilibrium adjustments that are made by all firms (or workers) at the time the leave policy is adopted and announced. This is a common limitation in most studies on the effects of parental leave, regardless of research design.[54] For our study, one potential concern is that both our sample of treatment and control firms may have undertaken costly changes to their production technology or labor-capital mix at the time the current parental leave policy was originally adopted. Such anticipatory costs of parental leave would obviously not be picked up in our comparison of treatment and control firms.

6   Results

We begin our empirical analysis by verifying that our construction of the estimation sample generates a difference in the 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

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[54]For example, many studies in the literature leverage the fact that new parental leave policies often only affect children born after a certain cutoff date and that these policies are often announced with such short notice that fertility cannot respond. This lends itself naturally to a regression discontinuity design that very credibly identifies causal effects (e.g. Lalive *et al.* , 2013; Carneiro *et al.* , 2015; Beuchert *et al.* , 2016; Dahl *et al.* , 2016; Gallen, 2017; Avdic & Karimi, 2018). Because these causal effects are estimated on samples of parents and firms that have had no time to adjust to the policy, they will miss potential effects that appear once worker plans and firm practices have adapted.

specification (1), using total births at the firm as the outcome variable.[55] As the figure shows, there are no differences in the evolution of births between the treatment and control firms in any of the years prior to the event year or in the two years following the event year. However, exactly in the event year there is a statistically significant increase in the number of births at treatment firms relative to control firms. This provides a first validation of our empirical strategy: in terms of employee fertility, treatment firms appear to evolve along the same trend as control firms except in the event year, when they experience significantly more births.[56] The apparent lack of pretrend 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 Section 5.2, this imperfect compliance mainly reflects that some baseline employees at treatment firms leave their firms before giving birth.[57] A factor working against this imperfect compliance is the possibility of causal peer effects in fertility, as some existing studies have found that one employee giving birth increases coworkers' fertility (e.g., Asphjell *et al.*, 2014; Ciliberto *et al.*, 2016). As we show in Appendix F, however, such peer effects appear very modest in our setting.[58]

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 year after the birth —giving rise to the increase in the year after birth. In terms of magnitudes, the OLS estimates for the event year and the following year are on the order of 136 and 59 extra days of leave, respectively. 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 Section

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

[56]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.

[57]In addition, some imperfect compliance may also result from our basic definition of treatment and control firms. As our definition of a control firm only requires that they have one female employee who does not give birth around the event year, some of our control firms will have another female employee that does give birth in the event year.

[58]Consistent with the previous literature, we do see a statically significant increase in coworker fertility at treatment firms exactly in the event year; however, the magnitude of this effect is small (less than 0.01 births) (see Appendix F).

5.4, we use a set of 2SLS specifications, (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 percentage point 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 increases by 196 in the event year (column (1)) and 86 in the following year (column (2)), corresponding to a total increase of 282 days or about nine and a half months. These estimates are consistent with aggregate statistics indicating that the average woman in Denmark takes a little less than ten months of leave in connection with childbirth.[59]

6.1 Labor Adjustment: Extensive Margin

The results in the previous section show that when an employee gives birth and goes on leave, the employing firm loses their 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. 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 that 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

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[59]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.

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.[60]

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 reflects that the increase in the employment stock is temporary and that firms are shedding the additional workers as 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 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 employed when an employee goes on leave thus are not replacing 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 1 percent of the workforce goes on leave, coworkers' likelihood of staying with the baseline firm increases by 0.13 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.

The coworker effects in Figure 4 and Table 5 reflect the overall effect for all types of coworkers. As discussed in Section 3, however, we might expect the effects of parental leave on coworkers to be very different for coworkers who are substitutes as opposed to complements relative to the worker on leave. In Appendix G, we therefore provide estimates separately for coworkers in the same occupation and

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[60]Column (3) of Table 5 shows that when 1 percent of the workforce goes on leave, new hires increase by 0.022 individuals, whereas turnover only drops by 0.012 individuals.

for coworkers in different occupations. Consistent with expectations, the positive effects on retention and unemployment in the overall sample are driven mostly by coworkers in the same occupation, which we expect to be closer substitutes to the worker on leave. We see no indications of adverse retention effects for coworkers in other occupations however.

6.2 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 among coworkers of women who take parental leave. Panel (a) of Figure 5 presents OLS estimates for the impact of parental leave on hours of work in the coworker sample.[61] We detect a small but statistically significant increase in the event year suggesting that when a worker takes leave, firms increase the hours of coworkers. The 2SLS estimates in Panel B of Table 5 quantify this effect. When 1 percent of the workforce goes on leave, hours of existing coworkers increase by 0.10 percent in the event year (column (3)). In Appendix G, we examine coworkers' hours after separating coworkers by occupation. The observed change in hours is driven entirely by coworkers in the same occupation as the woman on leave. For coworkers in a different occupation, we see no changes in hours.

6.3 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 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 1 percent of the workforce goes on leave, total hours decrease by only 0.045 percent in the event year (column (3)) and increase by 0.052 percent in the following year. Based on the corresponding 95 percent confidence intervals, we can reject that total hours drop by more than 0.18 percent in the first year. Overall, firms appear to very effectively counteract the loss of labor that occurs when an employee goes on leave.

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[61]Recall the measure of hours does not cover overtime hours for full-time employees.

To assess whether parental leave has impacts on the quality of labor inputs, we examine the effect of parental leave on the characteristics of the workforce in Appendix H. 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.

6.4 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 costs. 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 2 and 4, this includes wages paid to workers on leave and thus reflects the costs faced by firms before being reimbursed 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 1 percent of the workforce goes on leave, firms' total wage bills increase by 0.28 percent in the event year (column (3)).

Next, we examine the wage bill after excluding paid leave. As discussed in Section 2, 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 faced by firms 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 bill 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 95 percent confidence interval allows us to reject that the wage bill excluding paid leave increases by more than 0.01 percent in the event year when 1 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

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corresponding 2SLS estimate in Table 6 shows that coworker earnings increase by 0.14 percent in the event year when 1 percent of the baseline workforce goes on leave. This increase mirrors the increase in coworker hours documented earlier. In Appendix G, we again see that the estimated effects on coworkers is concentrated entirely among coworkers in the same occupation as the woman on leave, while coworkers in other occupations do not appear to be affected.

6.5   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 effects on total labor inputs and costs, it can still have important negative effects on firms if it lowers firm output and causes a drop in profits. A particular advantage of our data is that it allows us to directly examine such firm-level outcomes. 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 0.17 percent in the event year when 1 percent of the workforce goes on leave. In Panel (b) of Figure 7, we instead examine the effects on gross profits. Unsurprisingly given our previous results, we also see no indication that profits are affected by leave take-up, 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 finally 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 1 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 that some coworkers are 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.[62] We see no evidence that parental leave take-up affects coworkers' sick days. In Appendix G, we see that this conclusion holds also when examining only the same-occupation coworkers that drive the documented increase in coworkers' hours of work.

6.6 Robustness and Additional Results

In this section, we briefly discuss some additional robustness checks and supplementary results. The analysis so far estimates effects across all the small firms in our sample. However, the effects of parental leave could well be heterogeneous across firms. In unreported results, we have thus examined the effects of parental leave separately for various relevant subsamples. Given our sample size, however, such analyses turn out to have very limited power.[63]

As discussed in Section 5.7, the detailed set of baseline covariates that we condition on imply that we trim away a substantial part of the baseline data when constructing our main analysis sample. To assess how this affects the results, Appendix E presents results using a coarser set of baseline covariates that implies less trimming. Although the coarser conditioning may weaken the validity of our identifying assumption, the estimated effects from this approach turn out to be similar to our main results.

Finally in Appendix C, we confirm that our results are virtually unchanged if, instead of propensity score reweighting, we use a purely regression-based estimation approach that uses baseline covariates as control variables.

7 Discussion and 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' response to these programs. This paper aims to fill this gap in the existing literature by exploiting 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

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

[63]Specifically, we have examined whether the effects of leave take-up are different for the smallest firms—that is, those with less than ten employees in the baseline year—and for firms in which the woman going on leave is from a high-skilled occupation. For both subgroups, we are unable to reject that the effects are the same as in the overall sample; however, confidence intervals allow for very large differences in the estimated effects.

in which 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 firms' outcomes in the years around the birth year.

Our findings indicate that in response to leave take-up, firms hire temporary workers and slightly increase retention of existing employees. 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 costs do increase temporarily in response to the leave; however, this is completely driven by wages paid to workers on leave for which employers eventually get reimbursed. Finally, 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.

Overall, we find little support for the claim that parental leave is detrimental for coworkers or firms. To the extent that employers have correct beliefs about the costs of parental leave, this also alleviates concerns that young women's employment options are negatively affected by firms' expectations that they will eventually go on parental leave.

In relation to the broader literature on the effects of worker loss and absence, it is worth emphasizing that our results do not establish that worker absence due to parental leave has *no* negative effects—our estimated confidence intervals do allow for some negative effects to exist and a simple revealed preference argument suggests that some negative effects should be present.[64] However, in contrast to previous studies that find large significant effects of worker absences our results do suggest that the nature of parental leave absences makes it easier for firms to mitigate any negative effects. In particular, we speculate that parental leave differs from other types of absence by being both temporary and highly anticipated. Developing a more precise understanding of why different types of worker absences have different effects on firms is an important topic for future work.

Finally, our study also leaves open some unanswered questions regarding the effect of parental leave. First, our estimates abstract from the general equilibrium effects of leave as the control firms exist in an environment of parental leave. Second, while we focus on small firms, large firms may have differential impacts of leave, especially if the timing of fertility across employees is highly correlated in that setting.

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[64]If firms suffer *no* negative output effects from going without a worker for a year but face potential savings in the total wage bill, we would generally expect a rational employer to have (temporarily) dismissed the worker even before the worker goes on leave.

Finally, firm adjustment costs may vary with the length of leave. Shorter leave lengths may be easy for firms to adapt to, but also may provide additional challenges as it may be too costly to hire and train a temporary worker.

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Figure 1: Definition of treatment and control samples

	Baseline *year*		*Event* *year*	
	$t = -2$	$t = -1$	$t = 0$	$t = 1$
Treatment events (firm f, woman w)	Woman w is with firm f	Woman w has *no* birth	Woman w gives birth	Woman w has *no* birth
Control events (firm f', woman w')	Woman w' is with firm f'	Woman w' has *no* birth	Woman w' has *no* birth	Woman w' has *no* birth
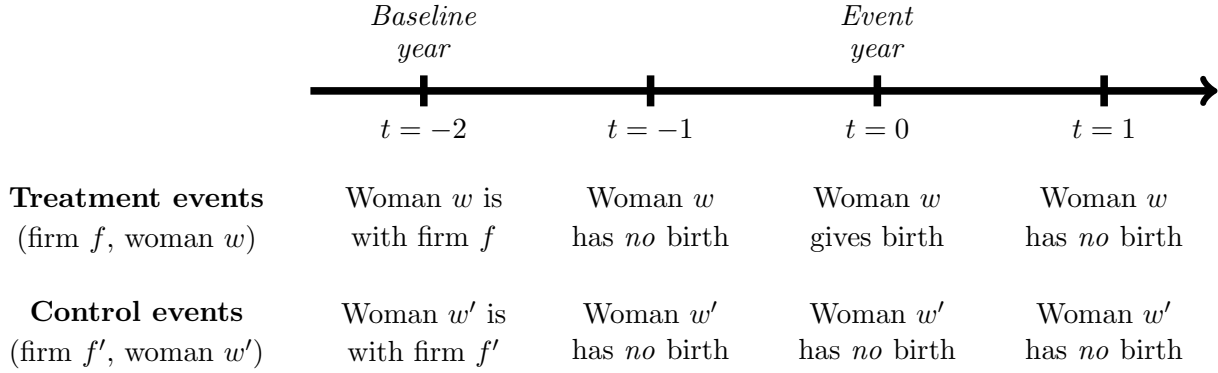
Notes: This figure summarizes the construction of the treatment and control samples as explained in Subsection 5.2.

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

(a) Prenatal leave
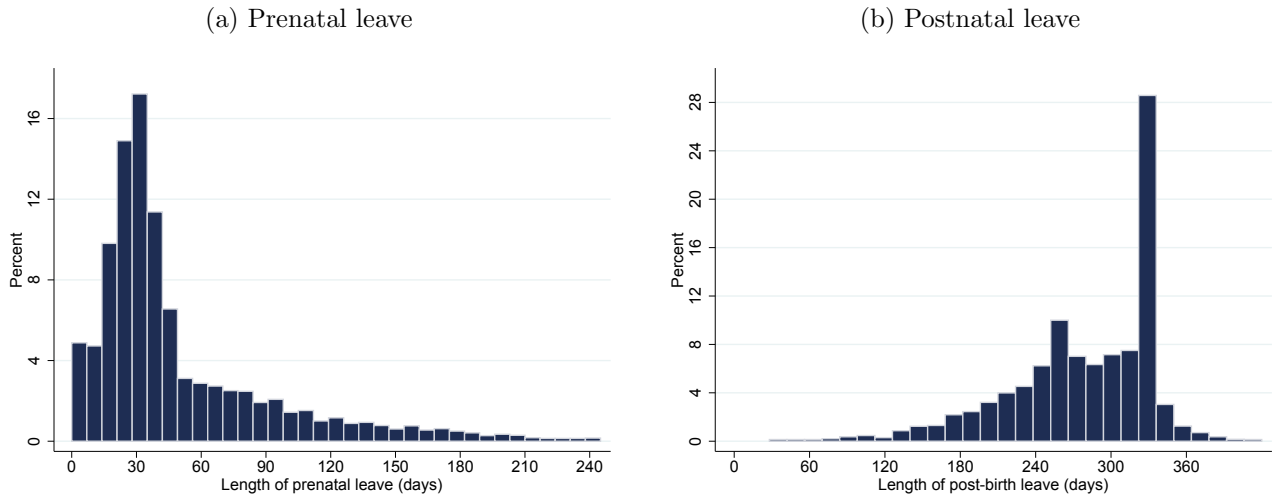
(b) Postnatal leave

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
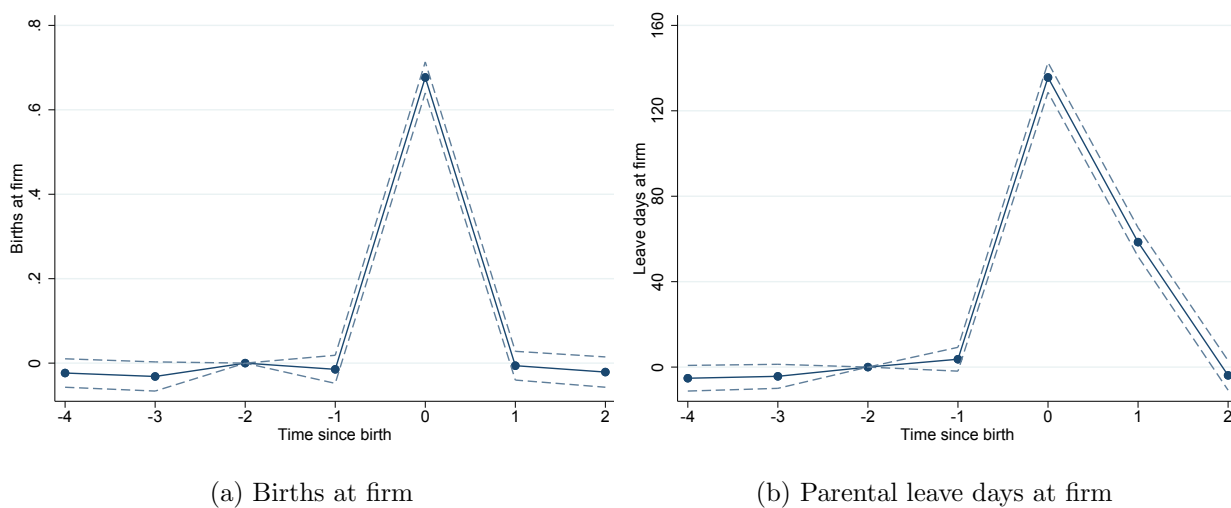
(a) Births at firm

(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
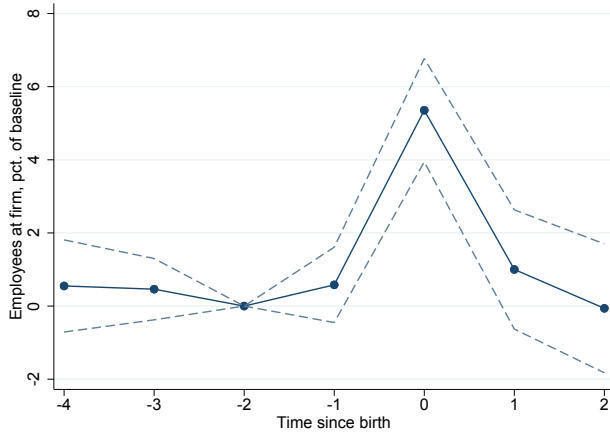
(a) Number of employees at firm
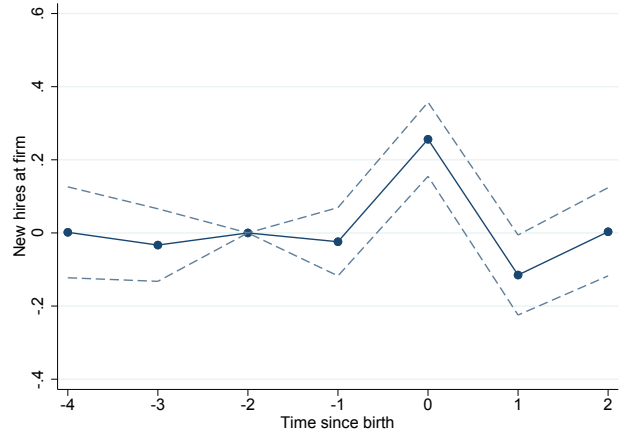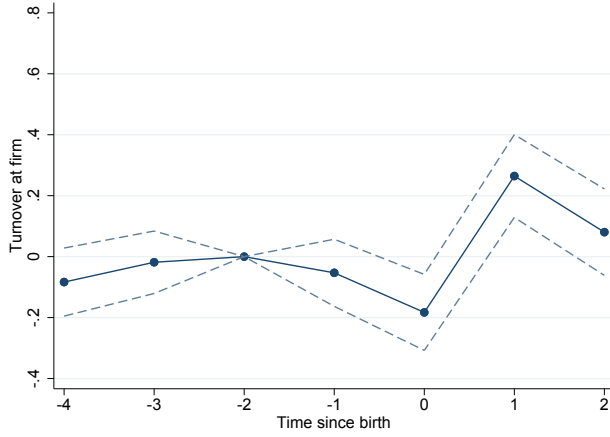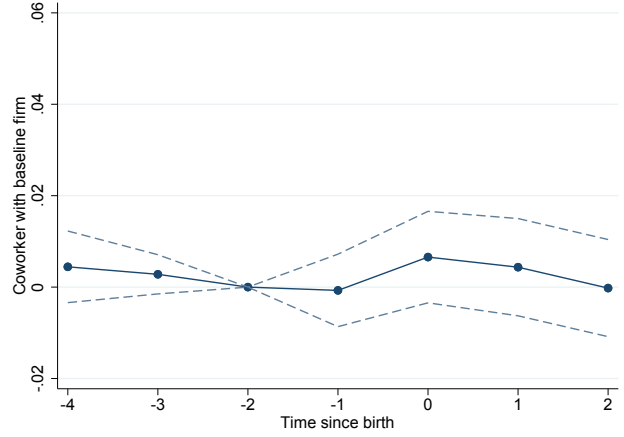
(b) New hires at firm

(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.

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Figure 5: Effects on hours of work, OLS
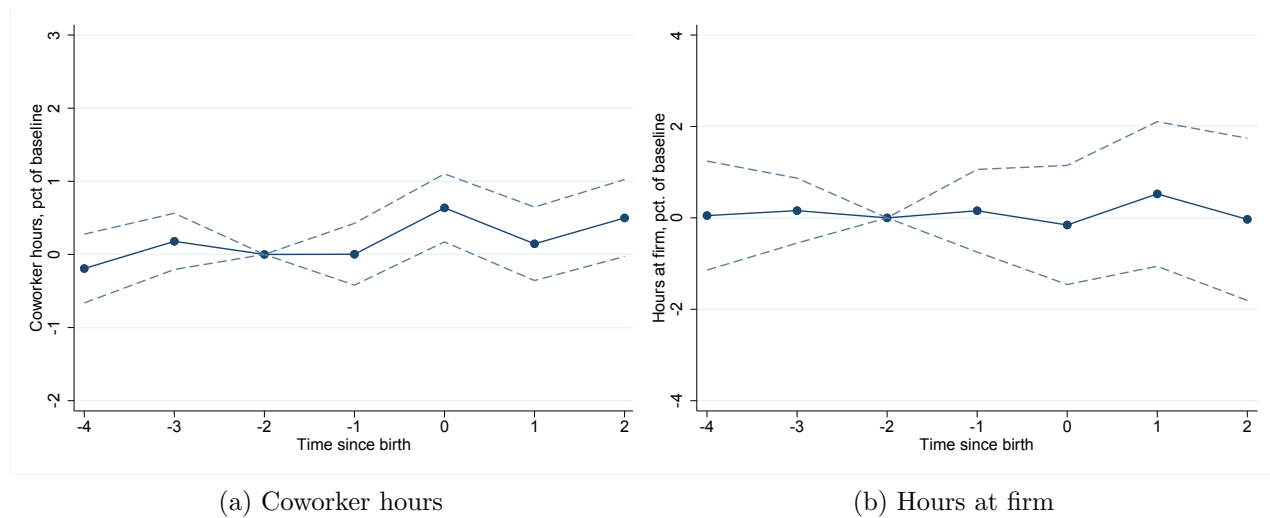
(a) Coworker hours

(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
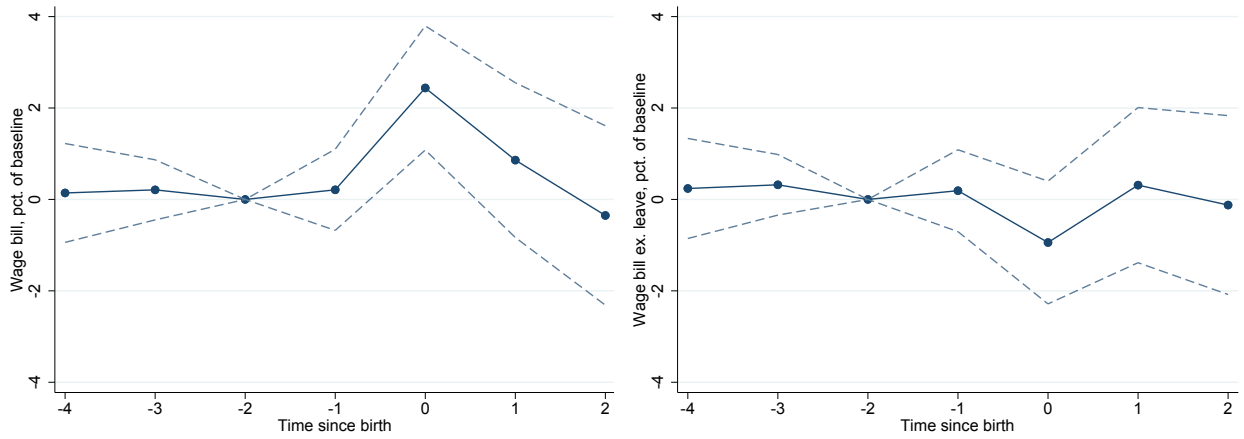
(a) Firms' wage bill

(b) Firms' wage bill (excluding paid leave)
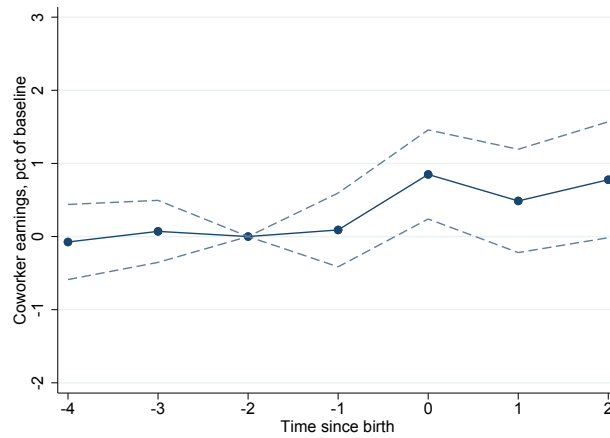
(c) Coworkers' earnings

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
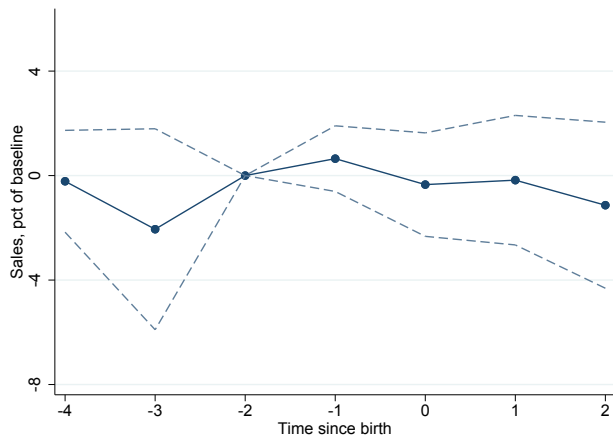
(a) Firms' total sales
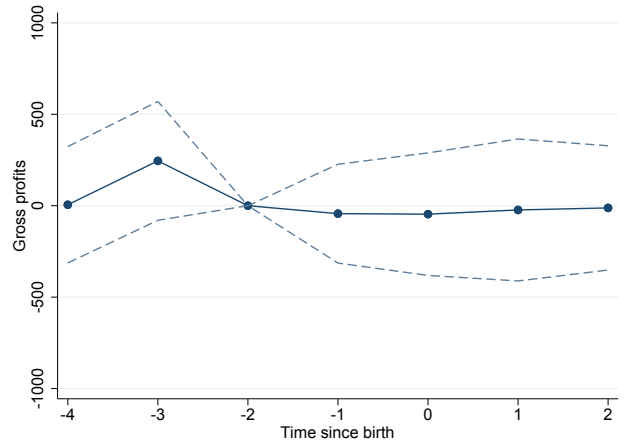
(b) Firms' gross profits (1000 DKK)
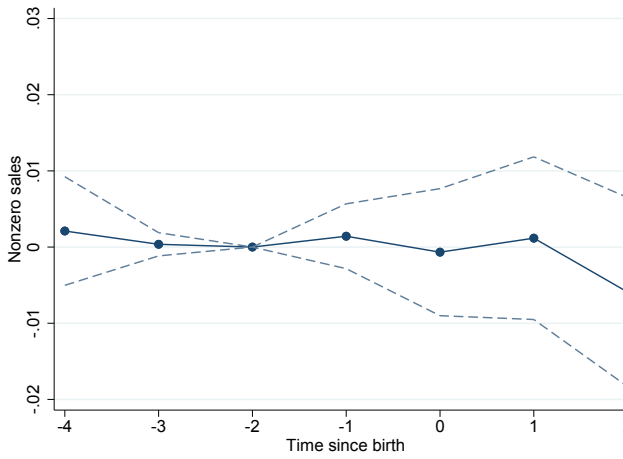
(c) Likelihood of firm survival
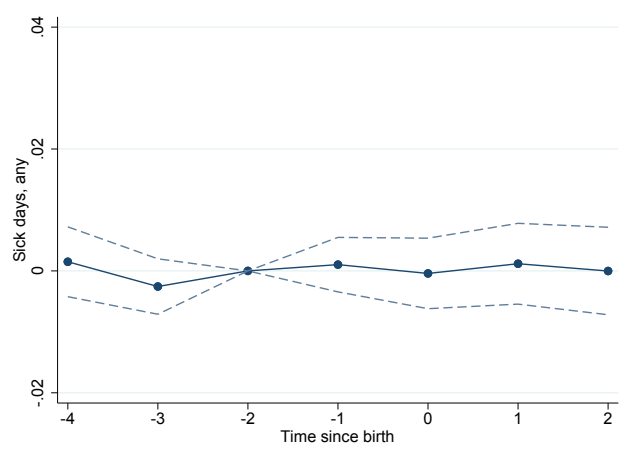
(d) Likelihood of coworkers taking sick leave

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
Law-given 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 a 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:	218,385	1,270,085	79,864
Restricted to active firms:	216,993	1,260,230	75,024
Restricted to small firms:	35,499	226,356	58,401
Restricted to private firms:	31,630	203,318	54,500
Excluding sale and wage bill outliers:	24,571	160,516	45,573
Excluding extreme growth/decline firms:	23,762	155,659	44,180
Applying trimming:	9,941	21,982	16,086
After matching/reweighing:	9,941	9,941	16,086

Notes: The table illustrates the selection of the final sample of matched treatment and control events. 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 employees 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	31,923	0.788	1.050
Pregnancies at firm	31,923	1.394	1.557
Leave days at firm	31,923	137.3	195.6
Number of employees	31,923	12.94	7.932
New hires	31,923	3.711	3.269
Turnover at firm	31,923	3.671	4.053
Wage bill (1000 DKK)	31,923	3,369	2,998
Sales (1000 DKK)	31,923	18,476	40,077
Purchases (1000 DKK)	31,923	12,587	32,892
Gross profits (1000 DKK)	31,923	2,520	17,433
Workforce share women	31,923	0.647	0.278
Workforce avg. age	31,923	33.87	6.435
Workforce avg. years schooling	31,923	11.61	1.281
Workforce avg. years education	31,923	12.29	5.327
Panel B - Coworker sample			
Coworker still with baseline firm	268,500	1.000	0.0000
Coworker unemployment (yearly share)	268,500	0.0146	0.0614
Coworker hours (FTE)	268,500	0.930	0.135
Coworker earnings (1000 DKK)	268,500	304.0	187.1

Notes: The table shows summary statistics only 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**	86.34**	11.08**	4.889**
	(4.773)	(4.545)	(0.256)	(0.246)
Number of employees at firm (pct rel. to baseline)	7.324**	1.170	0.627**	0.131
	(0.926)	(1.123)	(0.0785)	(0.0937)
New hires at firm	0.350**	−0.146	0.0224**	−0.003 71
	(0.0685)	(0.0766)	(0.00283)	(0.00305)
Turnover at firm	−0.260**	0.362**	−0.0116**	0.0234**
	(0.0849)	(0.0906)	(0.00331)	(0.00361)
Hours at firm (pct rel. to baseline)	−0.283	0.652	−0.0453	0.0524
	(0.891)	(1.082)	(0.0711)	(0.0856)
F-stat	2,211	2,211	2,312	2,312
Observations	31,923	31,923	31,923	31,923
Observations (weighted)	19,882	19,882	19,882	19,882
Clusters (firms)	16,086	16,086	16,086	16,086
B) Coworker outcomes				
Coworker with baseline firm	0.00943	0.00772	0.00127*	0.000709
	(0.00632)	(0.00678)	(0.000560)	(0.000589)
Coworker share of year unemployed	−0.002 49*	−0.002 47**	−0.000 201*	−0.000 254*
	(0.000861)	(0.000894)	(9.62e-05)	(0.000103)
Coworker hours (pct rel. to baseline)	0.859**	0.269	0.103**	0.0356
	(0.301)	(0.331)	(0.0337)	(0.0360)
F-stat	964.2	968.4	3,028	3,011
Observations	268,500	267,307	268,500	267,307
Observations (weighted)	167,653	168,416	166,580	167,653
Clusters (firms)	15,412	15,408	15,412	15,408

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the interaction terms between births at the event time and the time dummies are instrumented by interactions between treatment status and time dummies. Columns (3) and (4) show estimates from similar regressions but in which we use births per 100 baseline employees instead of births and include dummy variables for each possible number of baseline employees. Results are reported for the event year (Time 0) and the following year (Time 1), and the analysis is conducted on the matched and reweighted sample. The reported F-statistics correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. The F-statistics are numerically similar for all instruments because: (i) the endogenous variables are time dummies interacted with the same time-invariant variable, (ii) the instruments are the same set of time dummies interacted with another time-invariant variable, and (iii) there are the same number of observations in each year. 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)	3.519**	1.225	0.276**	0.0903
	(0.901)	(1.151)	(0.0719)	(0.0894)
Firm's wage bill excl. paid leave (pct rel. to baseline)	−1.342	0.377	−0.135	0.219
	(0.925)	(1.159)	(0.0728)	(0.0902)
F-stat	2,211	2,202	2,312	2,312
Observations	31,923	31,923	31,923	31,923
Observations (weighted)	19,882	19,882	19,882	19,882
Clusters (firms)	16,086	16,086	16,086	16,086
B) Coworker outcomes				
Coworkers' earnings (pct rel. to baseline)	1.124**	0.641	0.136**	0.0880
	(0.388)	(0.449)	(0.0440)	(0.0504)
F-stat	964.2	968.4	3,028	3,011
Observations	268,500	267,307	268,500	267,307
Observations (weighted)	168,416	167,653	168,416	167,653
Clusters (firms)	15,412	15,408	15,412	15,408

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the interaction terms between births at event time and the time dummies are instrumented by interactions between treatment status and time dummies. Columns (3) and (4) show estimates from similar regressions but in which we use births per 100 baseline employees instead of births and include dummy variables for each possible number of baseline employees. Results are reported for the event year (Time 0) and the following year (Time 1), and the analysis is conducted on the matched and reweighted sample. The reported F-statistics correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. The F-statistics are numerically similar for all instruments because: (i) the endogenous variables are time dummies interacted with the same time-invariant variable, (ii) the instruments are the same set of time dummies interacted with another time-invariant variable, and (iii) there are the same number of observations in each year. 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)	−0.659	−0.870	0.0270	0.0146
	(1.274)	(1.535)	(0.103)	(0.109)
Gross profits (1000 DKKs)	−98.09	−230.0	0.757	−1.500
	(217.7)	(246.7)	(6.076)	(6.781)
Nonzero sales	0.00354	0.00668	0.000245	0.000528
	(0.00486)	(0.00620)	(0.000374)	(0.000466)
F-stat	2,211	2,211	2,312	2,312
Observations	31,923	31,923	31,923	31,923
Observations (weighted)	19,882	19,882	19,882	19,882
Clusters (firms)	16,086	16,086	16,086	16,086
B) Coworker outcomes				
Coworkers, any sick days	0.0572	0.0700	0.0165	0.0265
	(0.204)	(0.215)	(0.0252)	(0.0283)
F-stat	964.2	968.4	3,028	3,011
Observations	268,500	267,307	268,500	267,307
Observations (weighted)	168,416	167,653	168,416	167,653
Clusters (firms)	15,412	15,408	15,412	15,408

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the interaction terms between births at the event time and the time dummies are instrumented by interactions between treatment status and time dummies. Columns (3) and (4) show estimates from similar regressions but in which we use births per 100 baseline employees instead of births and include dummy variables for each possible number of baseline employees. Results are reported for the event year (Time 0) and the following year (Time 1), and the analysis is conducted on the matched and reweighted sample. The reported F-statistics correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. The F-statistics are numerically similar for all instruments because: (i) the endogenous variables are time dummies interacted with the same time-invariant variable, (ii) the instruments are the same set of time dummies interacted with another time-invariant variable, and (iii) there are the same number of observations in each year. 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
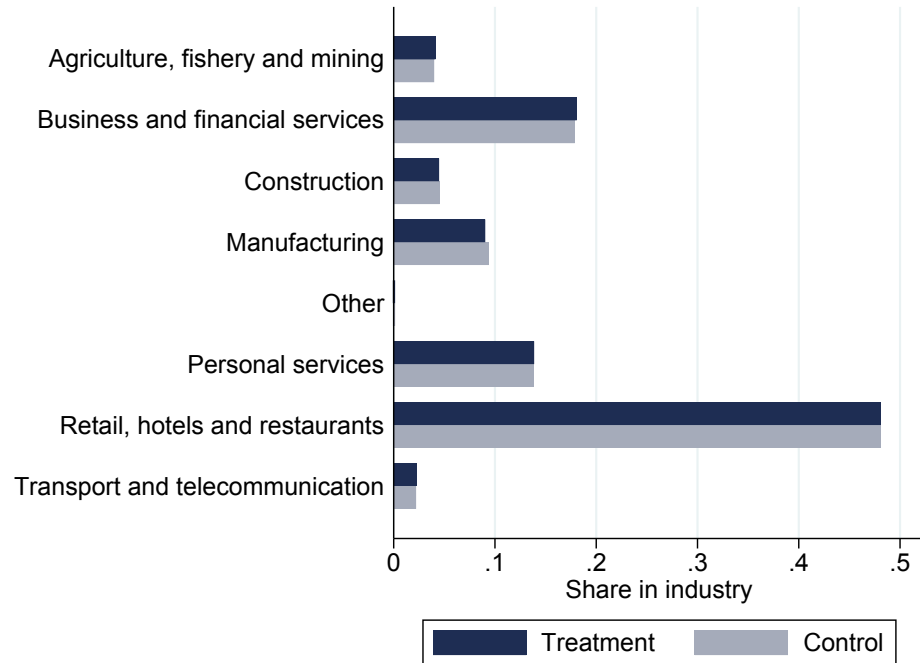
The figure shows the industrial composition of the matched and reweighted treatment and control samples across one-digit industries. Because it contains a very small number of firms, the category "Electricity and water supply" has been lumped into the "Other" category for reasons of data confidentiality. 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*	16	100	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)

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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	31,923	0.788	1.050
Pregnancies at firm	31,923	1.394	1.557
Leave days at firm	31,923	137.3	195.6
Employees	31,923	12.94	7.932
New hires	31,923	3.711	3.269
Turnover at firm	31,923	3.671	4.053
Wage bill (1000 DKK)	31,923	3,369	2,998
Sales (1000 DKK)	31,923	18,476	40,077
Purchases (1000 DKK)	31,923	12,587	32,892
Gross profits (1000 DKK)	31,923	2,520	17,433
Workforce share women	31,923	0.647	0.278
Workforce avg. age	31,923	33.87	6.435
Workforce avg. years schooling	31,923	11.61	1.281
Workforce avg. years experience	31,923	12.29	5.327
Panel B - Coworker sample			
Coworker still with baseline firm	1,858,924	0.691	0.462
Coworker unemployment (yearly share)	1,858,924	0.0264	0.104
Coworker hours (FTE)	1,858,924	0.801	0.320
Coworker earnings (1000 DKK)	1,858,924	280.8	217.8

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	Observations
Births at firm	0.80	0.78	0.02	31,923
	(1.07)	(1.03)	(0.01)	
Leave days at firm	139.35	135.33	4.03	31,923
	(197.79)	(193.36)	(2.74)	
New hires	3.70	3.73	−0.03	31,923
	(3.29)	(3.24)	(0.04)	
Hours (FTEs)	10.61	10.59	0.02	31,923
	(7.31)	(7.28)	(0.10)	
Workforce avg. years schooling	11.62	11.62	0.00	31,923
	(1.28)	(1.28)	(0.02)	
Workforce avg. age	33.78	33.84	−0.06	31,923
	(6.34)	(6.39)	(0.09)	
Workforce avg. experience	12.24	12.26	−0.02	31,923
	(5.25)	(5.30)	(0.07)	
Wage bill (1000 DKKs)	3359.66	3379.13	−19.47	31,923
	(2991.40)	(3004.30)	(39.50)	
Purchases (1000 DKKs)	12640.45	12532.66	107.79	31,923
	(34511.73)	(31189.09)	(467.58)	
Profits (1000 DKKs)	9175.89	8839.45	335.99	31,923
	(28535.46)	(27927.64)	(395.10)	
Event year	2007.09	2007.08	−0.00	31,923
	(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. ** $p < 0.01$ * $p < 0.05$.

B   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 5.5). In the baseline year, each coworker c is employed at some firm f that is part of a potential future 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):

$$y_{ecft} = \psi_e + \sum_{k \in \mathcal{T}} \omega_k \mathbb{1}_{t=k} + \sum_{k \in \mathcal{T}} \kappa_k \mathbb{1}_{t=k} \cdot Treatment_e + \nu_{eft} \tag{5}$$

$$\mathcal{T} = \{-4, -3, -1, 0, 1, 2\}$$

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

$$\Delta y_{ecf} = \varrho_0 + \mu_0 BirthsInEventYear_{ef} + \Delta \nu_{ecf} \tag{6}$$

$$BirthsInEventYear_{ef} = \iota_0 + \iota_1 Treatment_e + v_{ecf} \tag{6, First Stage}$$

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

$$\Delta y_{ecf} = \varpi_0 + \chi_0 \frac{BirthsInEventYear_{ef}}{BaselineEmployees_{ef}} + \Delta \sigma_{ecf} \tag{7}$$

$$\frac{BirthsInEventYear_{ef}}{BaselineEmployees_{ef}} = \zeta_0 + \zeta_1 \frac{Treatment_e}{BaselineEmployees_{ef}} + \sigma_{ecf} \tag{7, First Stage}$$

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

C 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. In this appendix we verify that this purely regression-based approach yields similar results.

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

$$Y_{eft} = \gamma_e + \sum_{k \in \mathcal{T}} \alpha_k \mathbb{1}_{t=k} + \sum_{k \in \mathcal{T}} \gamma_k \mathbb{1}_{t=k} \cdot Treatment_e + \sum_{k \in \mathcal{T}} \beta_k \mathbb{1}_{t=k} \cdot X_e + \varepsilon_{eft} \tag{8}$$
$$\mathcal{T} = \{-4, -3, -1, 0, 1, 2\}.$$

This specification is identical to the one 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 non parametric propensity score reweigthing used in our main analysis, we need to choose the vector of characteristics X_e in a very particular way (see Angrist (1998) for details). Specifically, 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[65] and let X_e consist of an exhaustive set of dummies indicating which of the cells event e belongs to.

Figures A2 to A6 show OLS estimates from this alternative regression-based approach. We see that they are virtually indistinguishable from the results presented in the main text.

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[65]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 A2: Effects on births and leave days, regression with controls, OLS
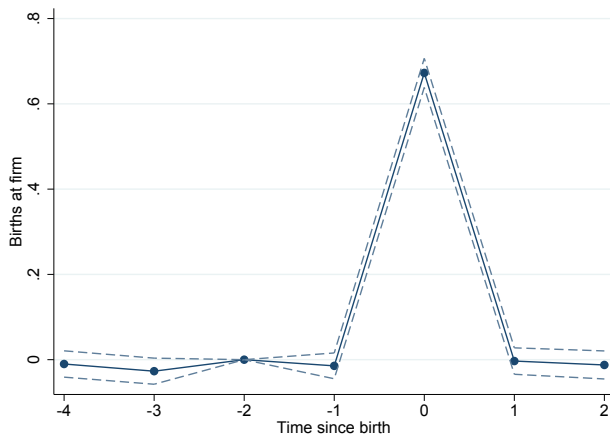
(a) Births at firm
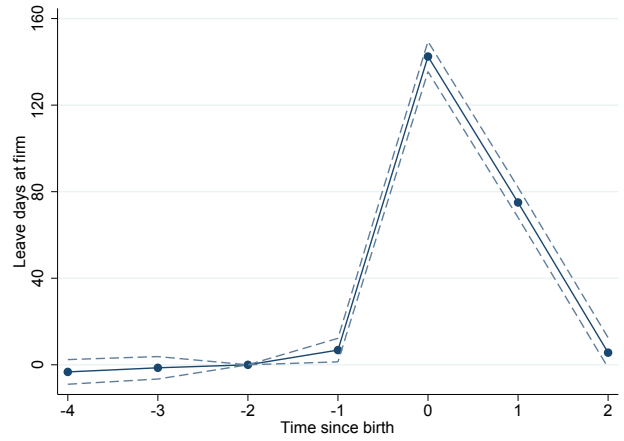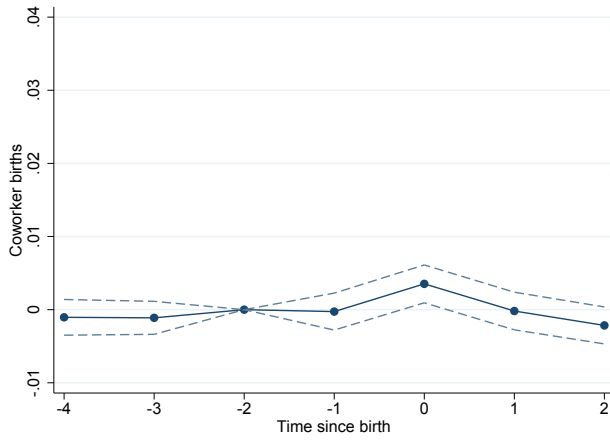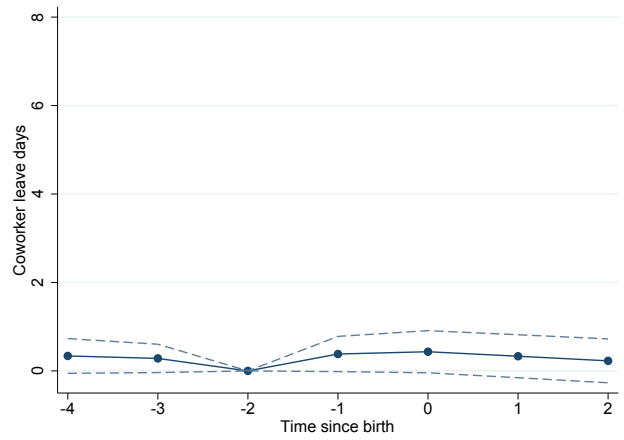
(b) Parental leave days at firm

(c) Coworkers' births
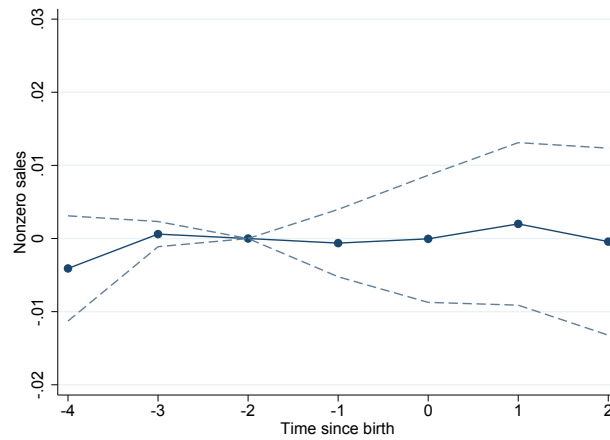
(d) Coworkers' parental leave days

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 A3: Effects on employment outcomes, regression with controls, OLS
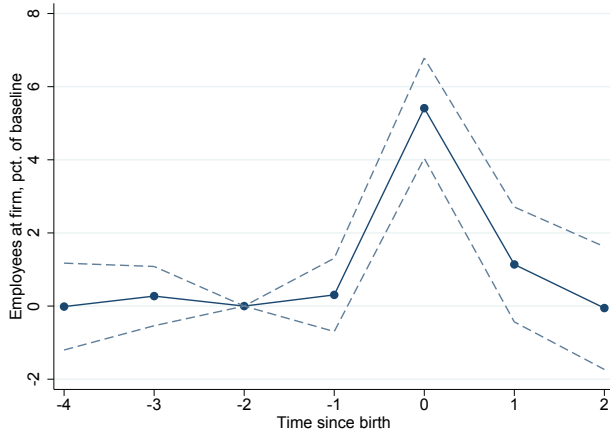
(a) Number of employees at firm
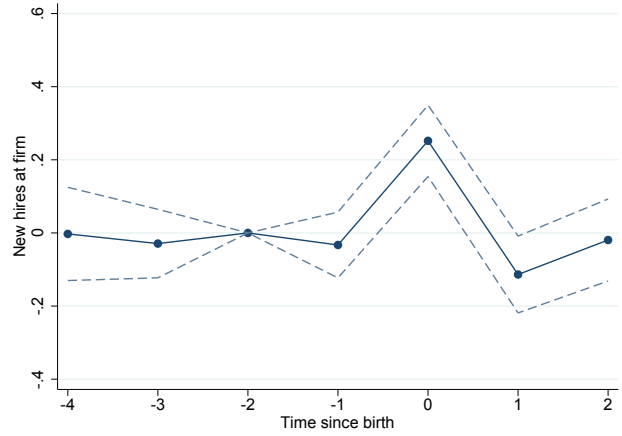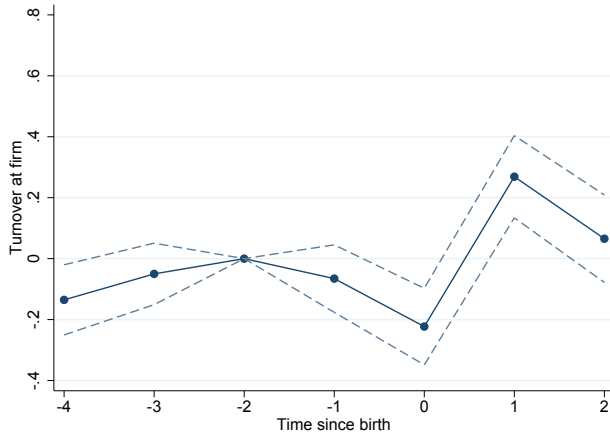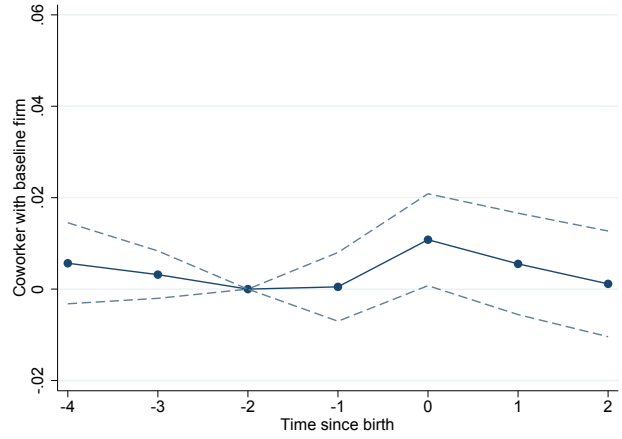
(b) New hires at firm

(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 A4: Effects on hours of work, regression with controls, OLS
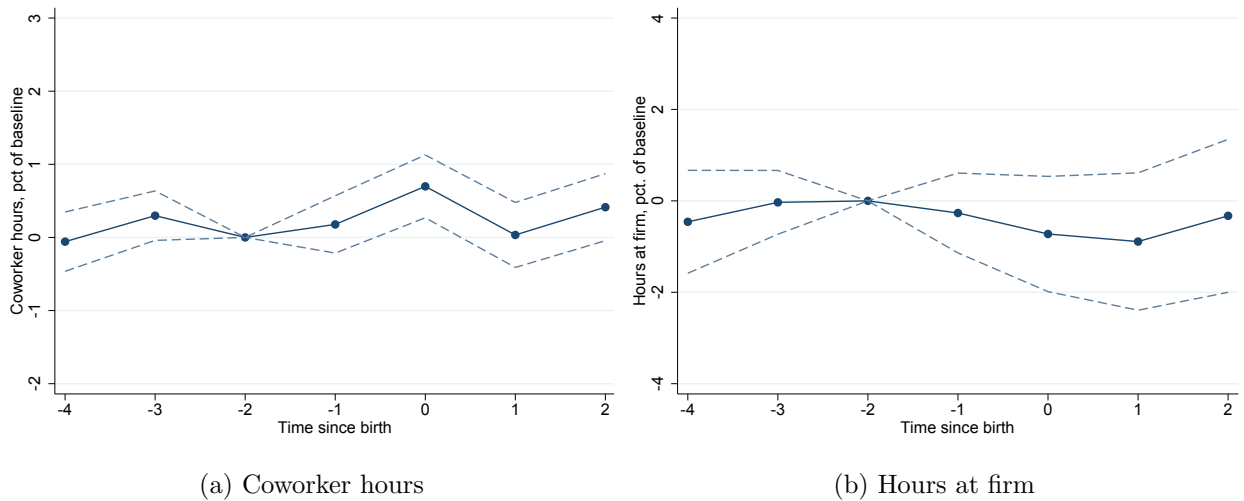
(a) Coworker hours

(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 A5: Effects on costs of labor supply adjustments, regression with controls, OLS
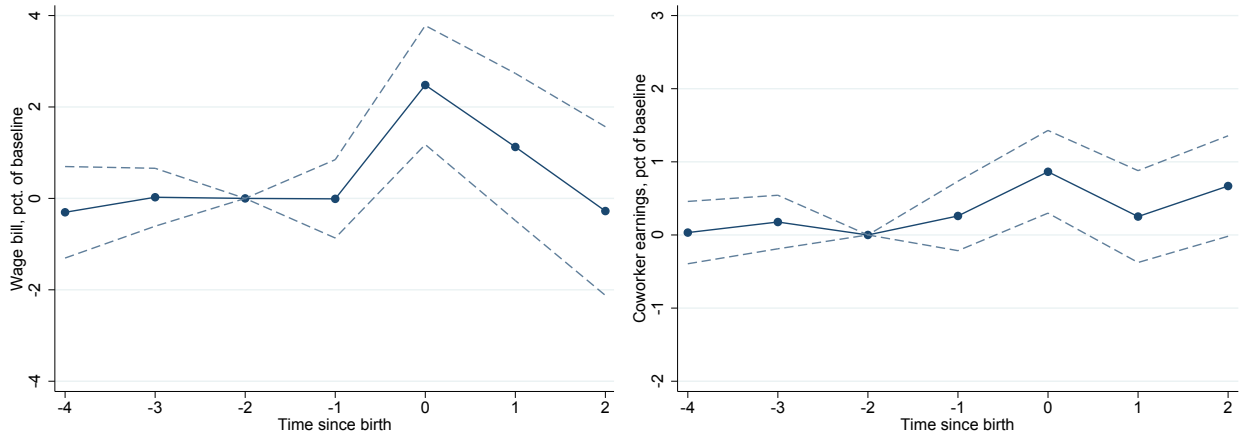
(a) Firms' wage bill
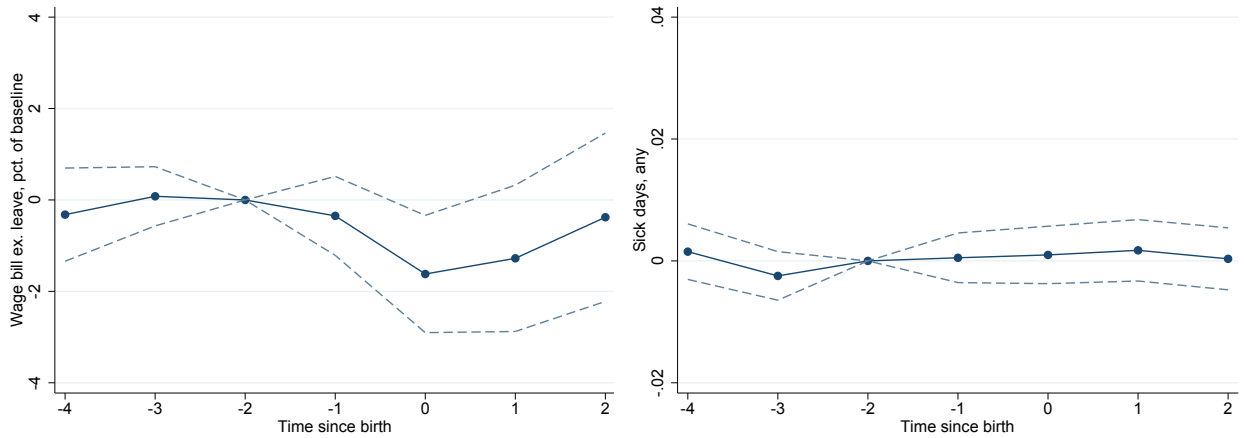
(b) Coworkers' earnings

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

(d) 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.

Figure A6: Effect on firms' overall performance, regression with controls, OLS
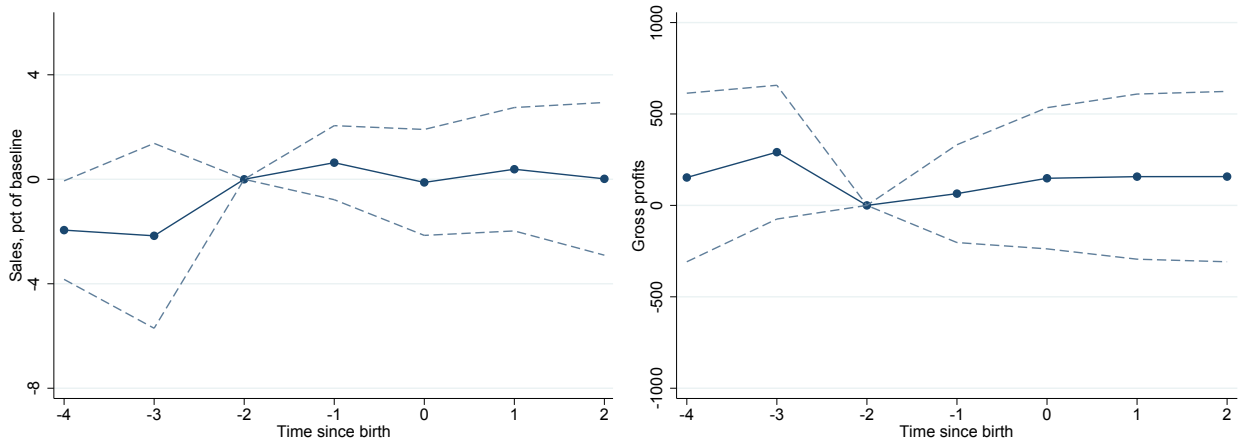
(a) Firms' total sales

(b) Firms' gross profits

(c) Likelihood of firm survival

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.

D Representativeness of firms in analysis sample

In constructing our main analysis sample, we apply a number of sample restrictions. Perhaps most notably, we restrict attention to small firms, require that both treatment and control firms have at least one young female employee at the baseline, and trim observations with extreme values of the propensity score when applying our matching and reweighting 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. Table A4 compares baseline characteristics across the three groups of firms. The table indicates that our treatment firms experience more births per employee (0.064 as opposed to 0.053 for the universe of small firms) and more leave days (14.34 as opposed to 5.4). Furthermore, the share of women at our treatment firms is higher than in other samples (at 0.645 versus 0.34), while the number of children per employee is lower (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.

In Figure A7, we further compare the one-digit industry composition of the three groups of firms. 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.[66]

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[66]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 A4: Baseline characteristics compared to universe of private and small firms

	All Firms	Size Restricted	Treatment Sample
Hours per employee (FTEs)	0.819	0.815	0.805
	(0.589)	(0.273)	(0.219)
Wage bill per employee	263.081	259.880	242.408
	(248.856)	(137.299)	(116.204)
Sales per employee	1476.184	1326.930	1223.234
	(2912.755)	(2752.943)	(1942.579)
Purchases per employee	1026.216	913.212	825.708
	(2759.786)	(2348.753)	(1687.450)
Births per employee	0.058	0.053	0.064
	(0.189)	(0.105)	(0.092)
Leave days per employee	5.671	5.398	14.343
	(28.347)	(15.627)	(24.223)
Children per employee	2.052	1.742	1.306
	(3.410)	(1.176)	(0.819)
Share women	0.335	0.344	0.645
	(0.361)	(0.322)	(0.278)
Employee avg. age	38.468	37.670	33.887
	(10.278)	(8.224)	(6.471)
Employee avg. experience (years)	15.149	15.186	12.296
	(8.366)	(7.023)	(5.351)
Employee avg. schooling (years)	11.424	11.404	11.604
	(1.784)	(1.446)	(1.290)
Observations	1,320,921	668,182	9,956

Notes: The table shows means and standard deviations for the firm and event-specific
variables in the baseline year across the restricted samples named in the column headers.

Figure A7: Industry composition by sample restrictions
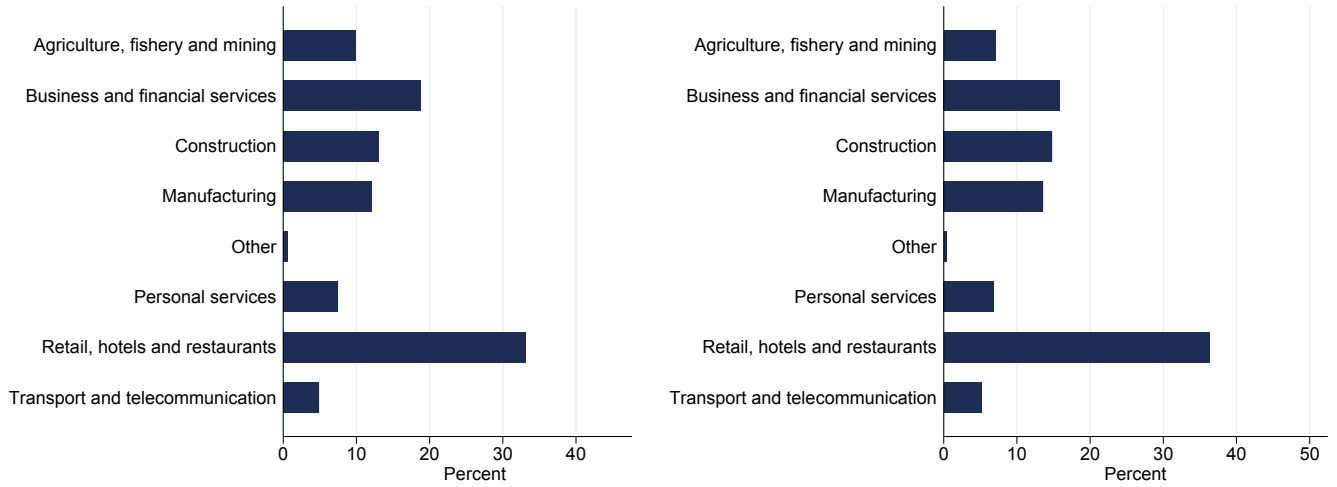
(a) All firms

(b) Size Restricted
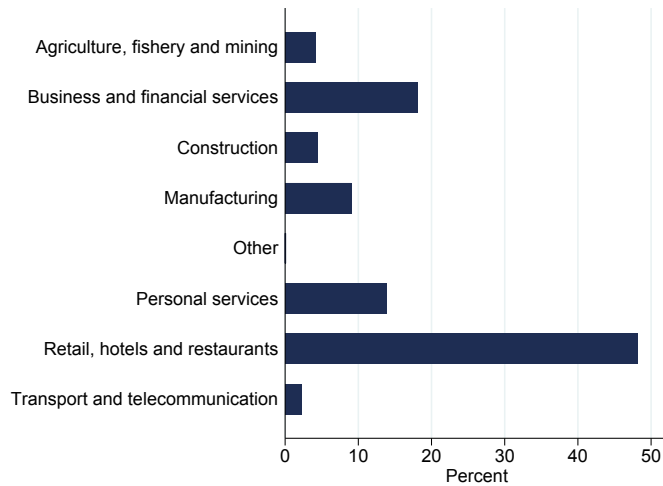
(c) Treatment Sample

Notes: The figure shows the industrial composition 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.

E   Results using coarser set of baseline covariates

As discussed in Section 5.6, our main results use a very detailed matching and reweighting procedure to condition on baseline observables. This detailed procedure gives us confidence that the treatment and control firms are ex-ante similar to ensure internal validity. As we have seen however, it also leads us to trim away a substantial part 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 23,762 treatment events, 14,289 (60.1 percent) are now left after the trimming.[67] However, the coarser set of baseline observables imply that the treatment and control groups are less comparable.

For all our main outcomes, Figures A8 to A12 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 the ones from our main analysis. We note however that some of our validity checks fail when using this alternative coarser approach. Specifically, in Table A5, we see 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.

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[67]Of the initial 155,659 control events, 38,574 remain after trimming when using the coarser set of baseline covariates.

Table A5: Covariate balance table conditioning on coarser set of observables

	Treatment	Control	Difference
Births at firm	0.84	0.82	0.02
	(1.07)	(1.06)	(0.01)
Leave days at firm	149.77	143.36	6.41**
	(201.66)	(199.69)	(2.40)
New hires	3.74	3.79	−0.06
	(3.32)	(3.31)	(0.04)
Hours (FTEs)	10.65	10.62	0.03
	(7.16)	(7.17)	(0.08)
Workforce avg. years schooling	11.70	11.68	0.02
	(1.34)	(1.33)	(0.02)
Workforce avg. age	34.25	34.37	−0.12
	(6.29)	(6.43)	(0.07)
Workforce avg. experience	12.45	12.53	−0.08
	(5.22)	(5.32)	(0.06)
Wage bill (1000 DKKs)	3,408.95	3,417.60	−8.65
	(2946.04)	(2981.50)	(33.57)
Purchases (1000 DKKs)	12,323.86	12,441.00	−117.75
	(32426.32)	(31567.84)	(352.27)
Profits (1000 DKKs)	11,697.04	10,834.22	862.81**
	(32020.01)	(29999.78)	(349.44)
Event year	2007.12	2007.13	−0.00
	(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 A8: Effects on births and leave days, conditioning on coarser set of observables, OLS
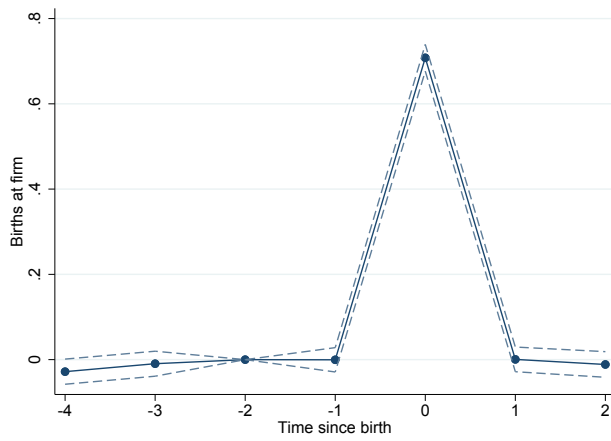
(a) Births at firm
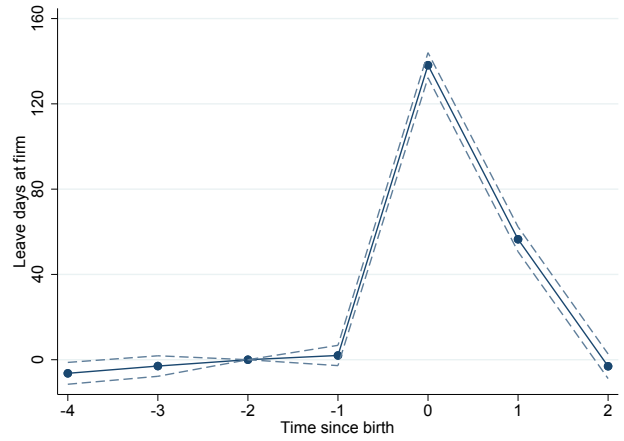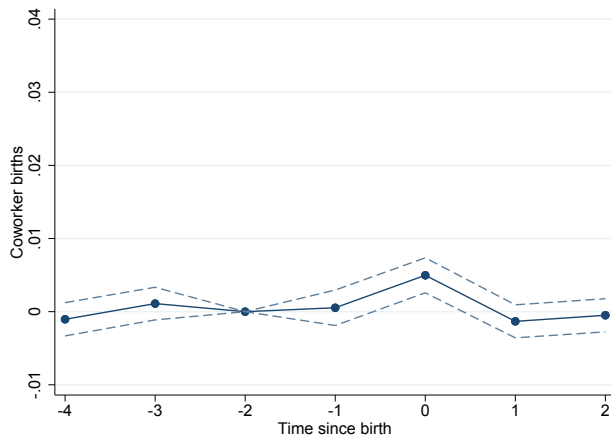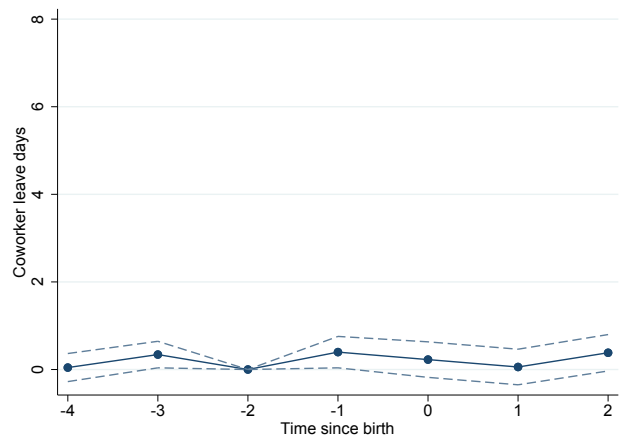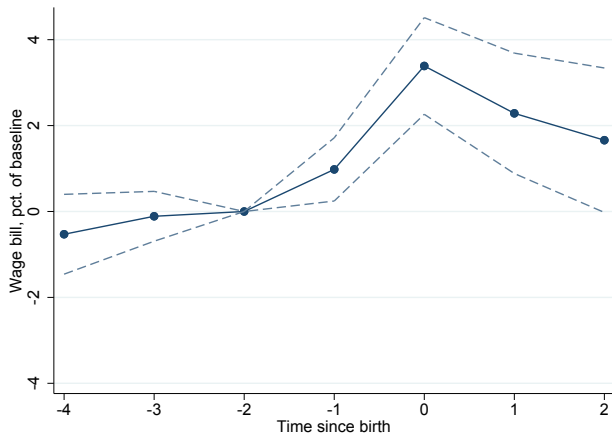
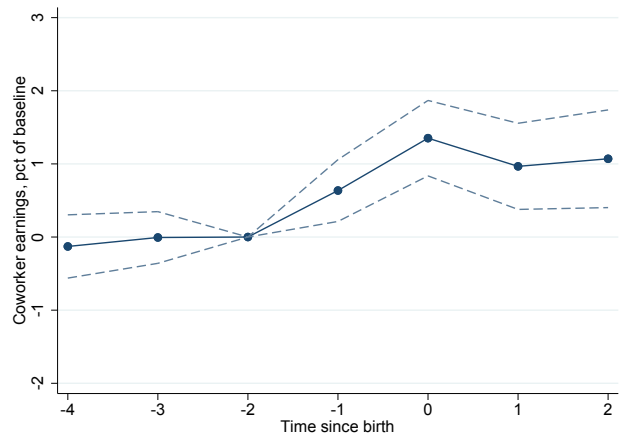(b) Parental leave days at firm

(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 A9: Effects on employment outcomes, conditioning on coarser set of observables, OLS
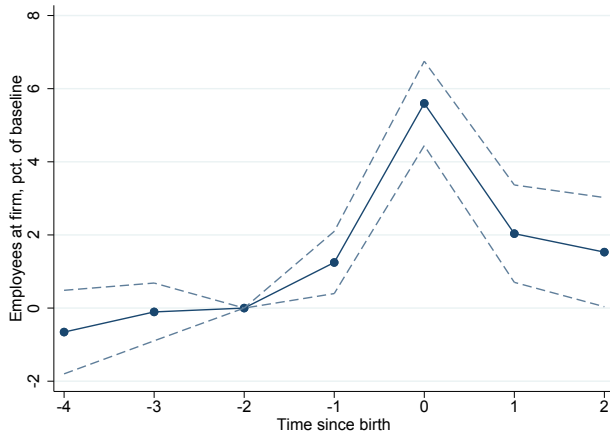
(a) Number of employees at firm
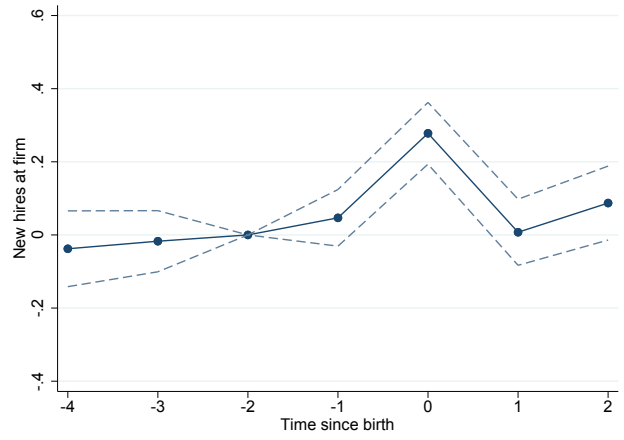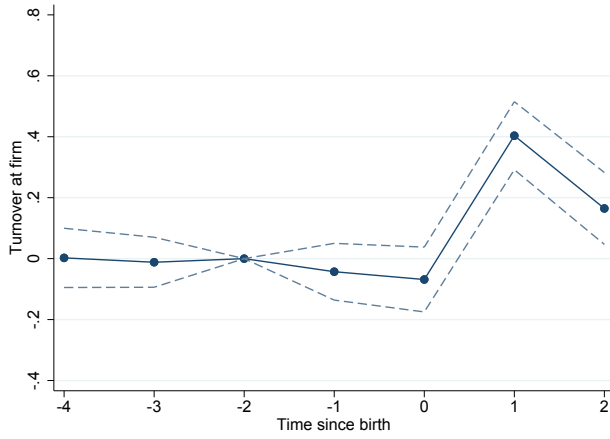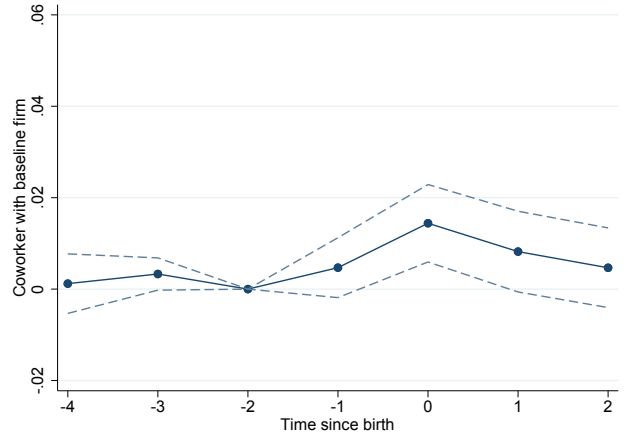
(b) New hires at firm

(c) Turnover at firm

(d) Likelihood coworkers with baseline firm
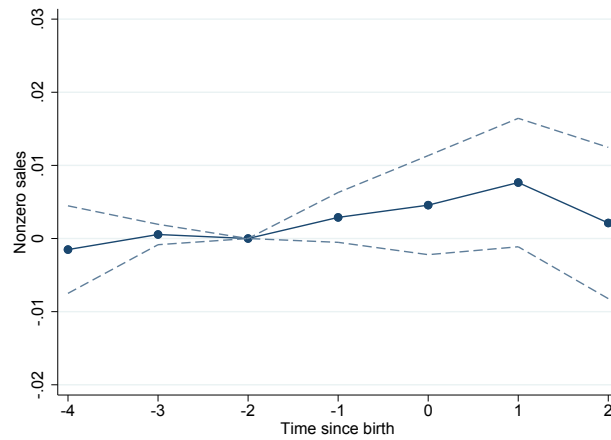
(e) Coworkers' share of year unemployed

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.

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Figure A10: Effects on hours of work, conditioning on coarser set of observables, OLS
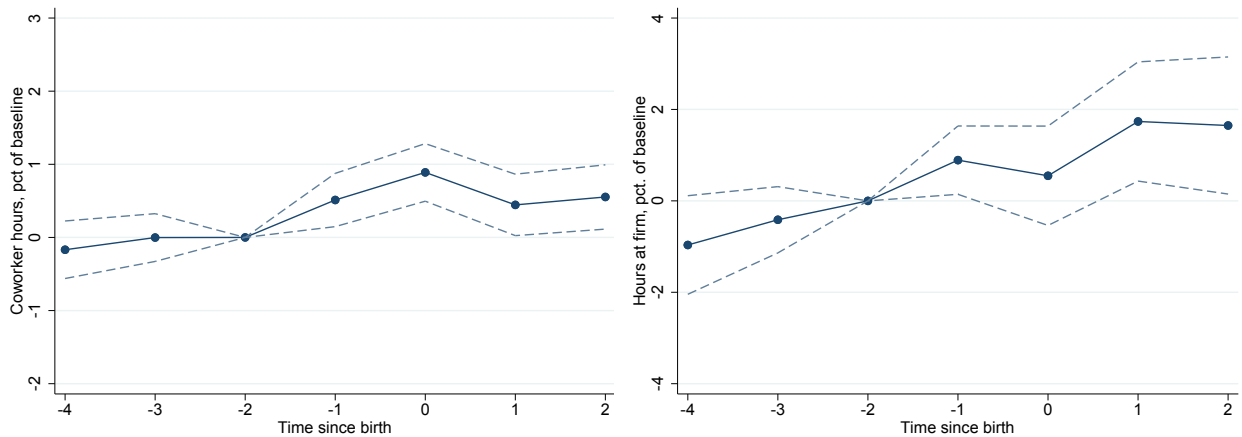
(a) Coworker hours

(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 A11: Effects on costs of labor supply adjustments, conditioning on coarser set of observables, OLS

(a) Firms' wage bill
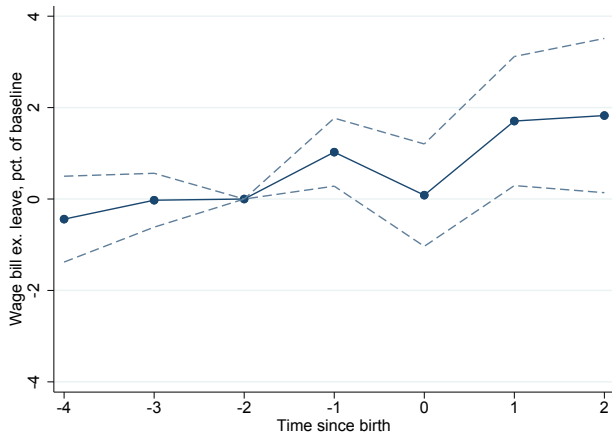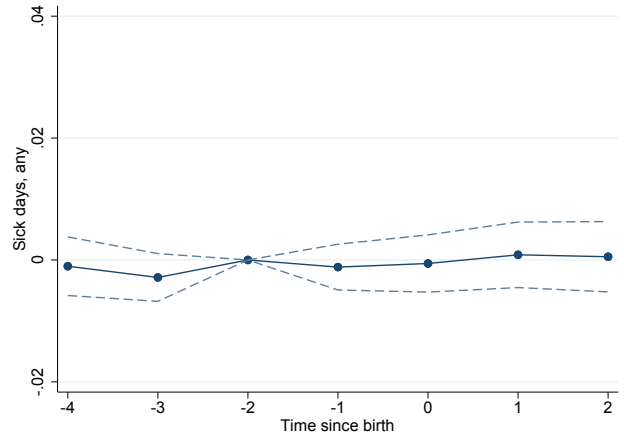
(b) Coworkers' earnings

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

(d) 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.

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Figure A12: Effect on firms' overall performance, conditioning on coarser set of observables, OLS
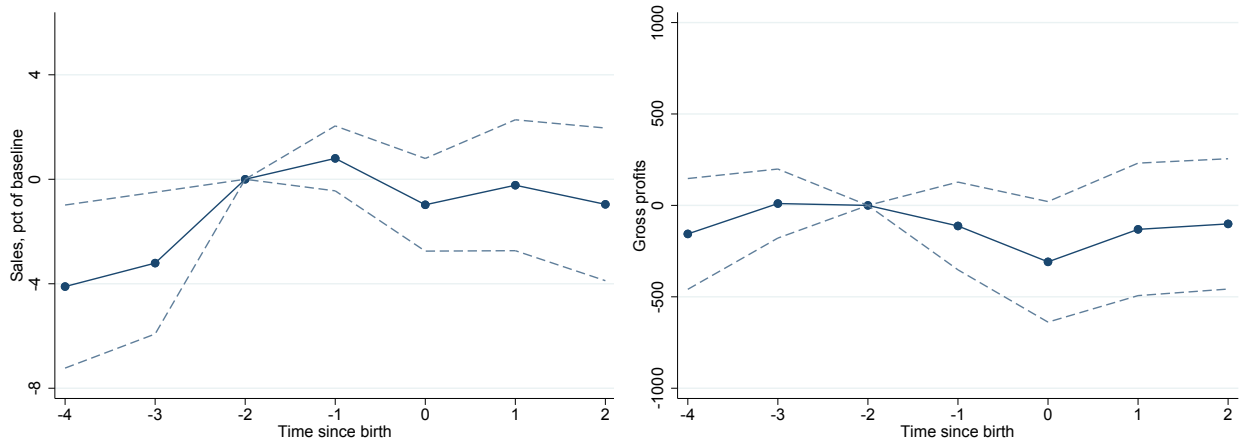
(a) Firms' total sales

(b) Firms' gross profits

(c) Likelihood of firm survival

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 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 in Swedish firms, the likelihood that an individual 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 takes leave in the following years. Specifically, with such within-firm peer effects, our estimates could be capturing the effect of multiple workers going on leave also outside the event year.

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. Appendix Panels (a) and (b) of Figure A13 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 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,[68] and we can rule out increases that are larger than 1.2 days in both the event year and the following year (column (1) and (2), Panel A of Table A6). We further show OLS (Panels (c) through (f) of Figure A13) and 2SLS estimates (Panels B and C of 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. Finally, we note that pre trends in nearly all of our outcomes are not present, suggesting that workers do not time their pregnancy in accordance with the evolution of firm outcomes.
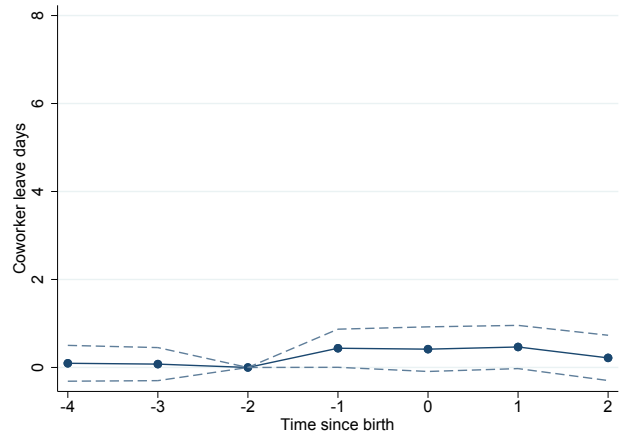
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[68]The magnitude is consistent with the effect on births.
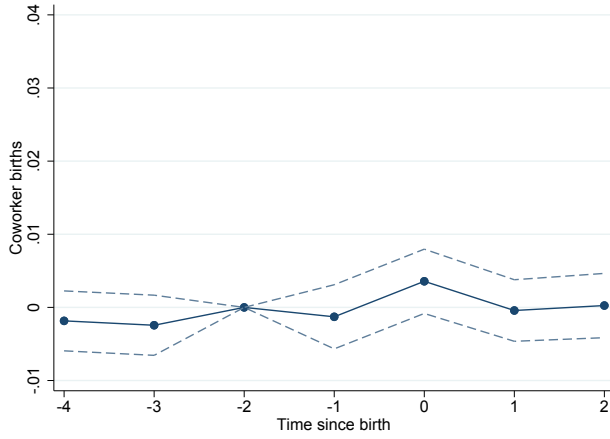
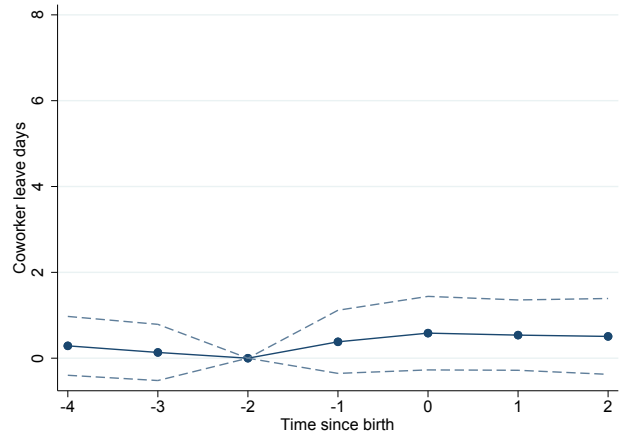Figure A13: Effects on coworkers' fertility and parental leave days, OLS
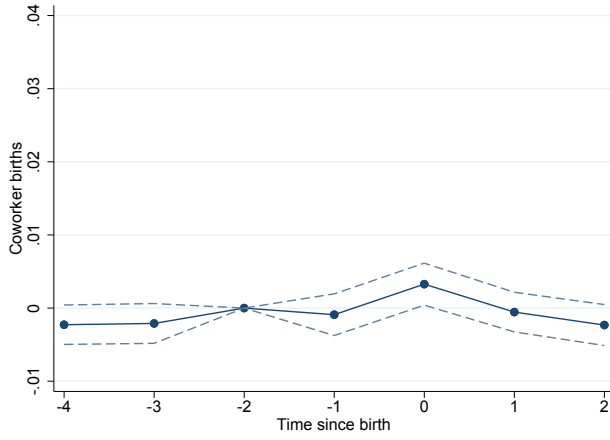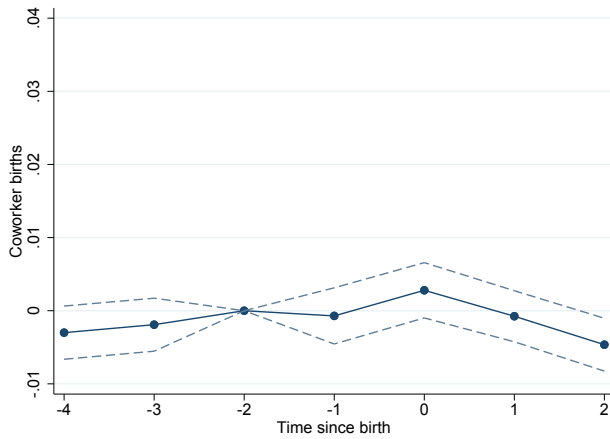

(a) Births, all coworkers
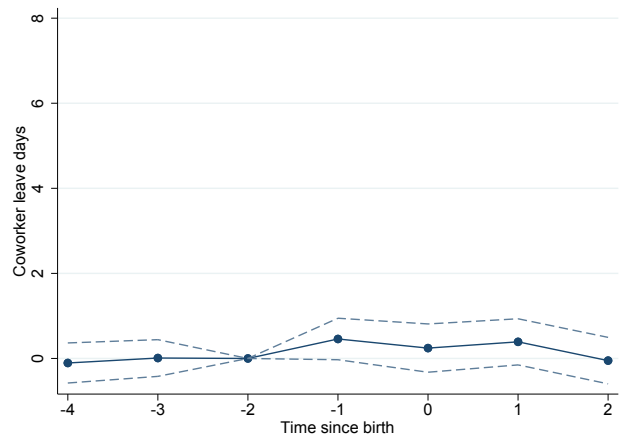
(b) Parental leave days, all coworkers

(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	0.00497**	0.000019	0.000735**	0.000178
	(0.00179)	(0.00172)	(0.000217)	(0.000206)
Leave days	0.470	0.593	0.0319	0.0779
	(0.323)	(0.311)	(0.0434)	(0.0425)
F-stat	964.2	968.4	3,028	3,015
Observations	268,500	267,307	268,500	267,307
Observations (weighted)	168,416	167,653	168,416	167,653
Clusters (firms)	15,412	15,408	15,412	15,408
B) Same-occupation coworkers				
Number of births	0.00629*	0.000229	0.000731*	0.000224
	(0.0028)	(0.00277)	(0.000326)	(0.000308)
Leave days	0.774	0.698	0.0156	0.100
	(0.561)	(0.540)	(0.0696)	(0.0702)
F-stat	645.1	648.8	1,824	1,810
Observations	121,525	121,007	121,525	121,007
Observations (weighted)	76,153	75,821	76,153	75,821
Clusters (firms)	12,535	12,518	12,535	12,518
C) Different-occupation coworkers				
Number of births	0.00376	−0.000352	0.000688*	0.000105
	(0.00230)	(0.00214)	(0.000291)	(0.000271)
Leave days	0.190	0.459	−0.0179	0.0438
	(0.350)	(0.337)	(0.0511)	(0.0468)
F-stat	772.6	775.1	2,147	2,137
Observations	145,586	144,920	145,586	144,920
Observations (weights)	91,391	90,966	91,391	90,966
Clusters (firms)	13,059	13,053	13,0599	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 interaction terms between births at event time and the time dummies are instrumented by interactions between treatment status and time dummies. Columns (3) and (4) show estimates from similar regressions but in which we use births per 100 baseline employees instead of births and include dummy variables for each possible number of baseline employees. Results are reported for the event year (Time 0) and the following year (Time 1), and the analysis is conducted on the matched and reweighted sample. The reported F-statistics correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. The F-statistics are numerically similar for all instruments because: (i) the endogenous variables are time dummies interacted with the same time-invariant variable, (ii) the instruments are the same set of time dummies interacted with another time-invariant variable, and (iii) there are the same number of observations in each year. Standard errors (in parentheses) are clustered at the firm level. ** $p < 0.01$ * $p < 0.05$.

G   Effects on coworkers in same versus different occupation

To examine whether the effects of parental leave are different for coworkers that are more likely complements or substitutes for the worker on leave, we follow Jäger & Heining (2019) and split our coworker sample by occupation. For each treatment and control event, we determine the one-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.

Figures A14 and A15 and Table A7 show OLS and 2SLS estimates for the resulting two coworker samples. Throughout, we 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.
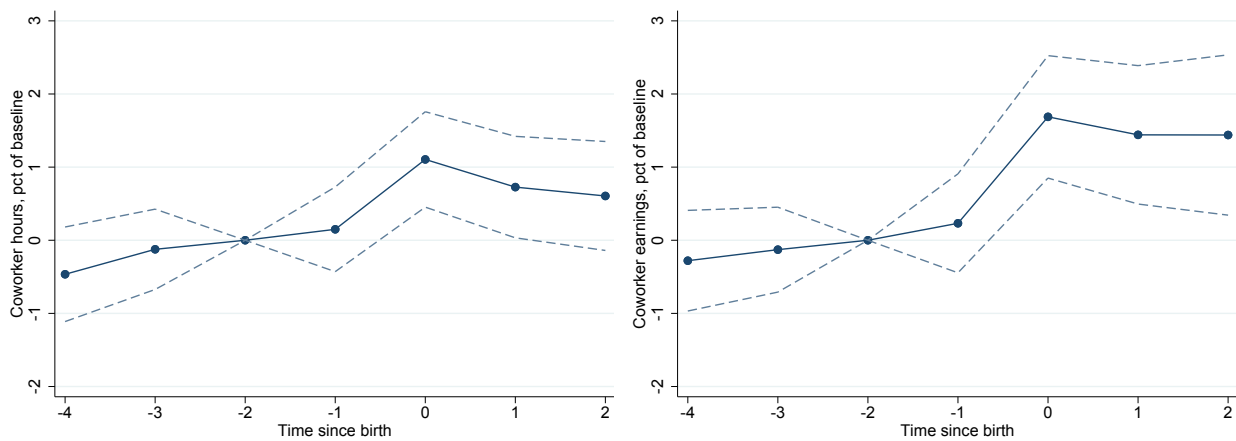
Figure A14: Effects on outcomes of coworkers in same occupations as women on leave, OLS
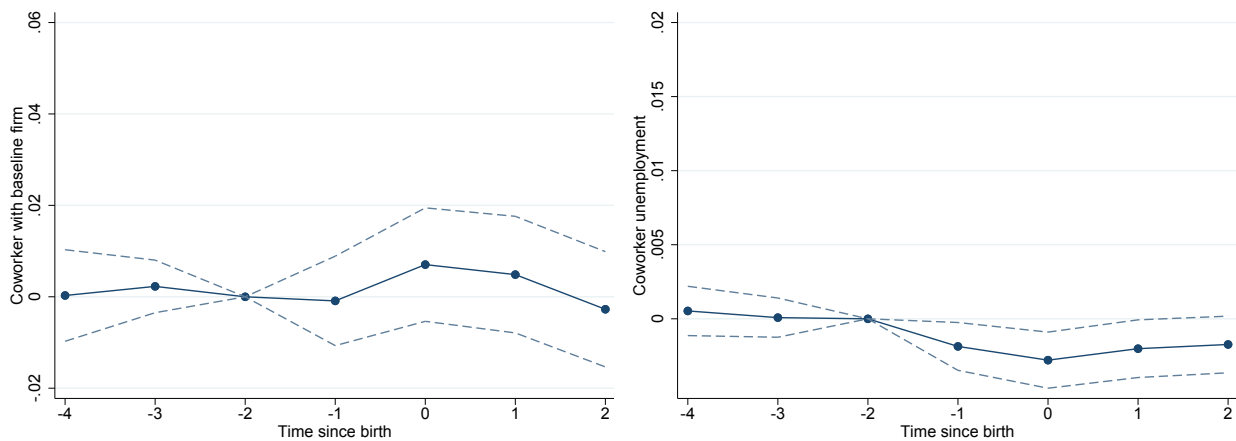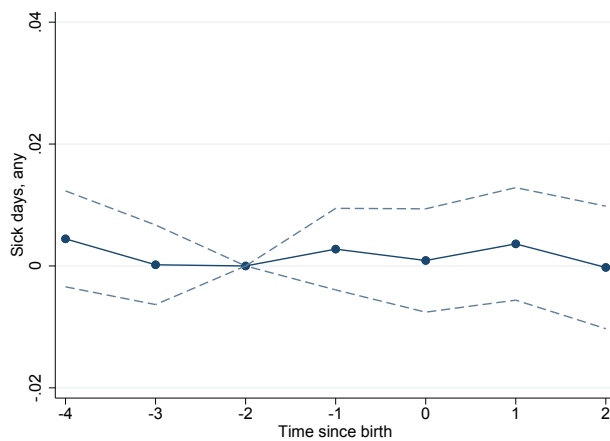

(a) Likelihood of coworker at baseline firm

(b) Coworker's share of year unemployed
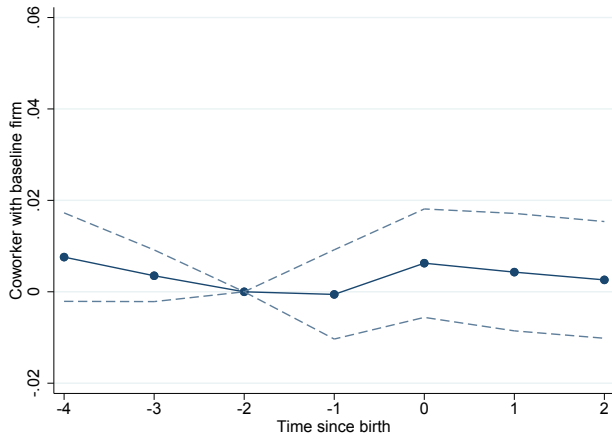
(c) Coworkers' hours at baseline firm
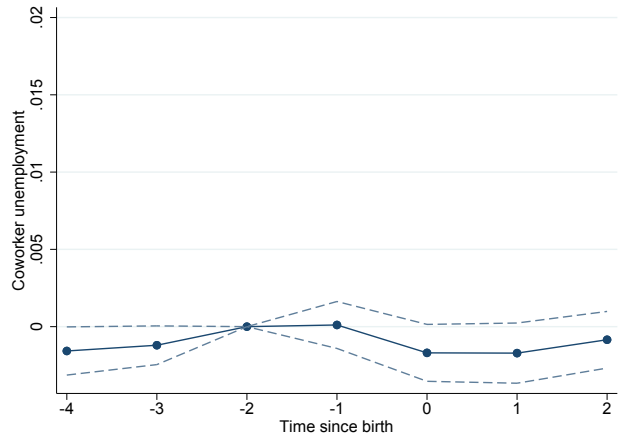
(d) Coworkers' earnings

(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.
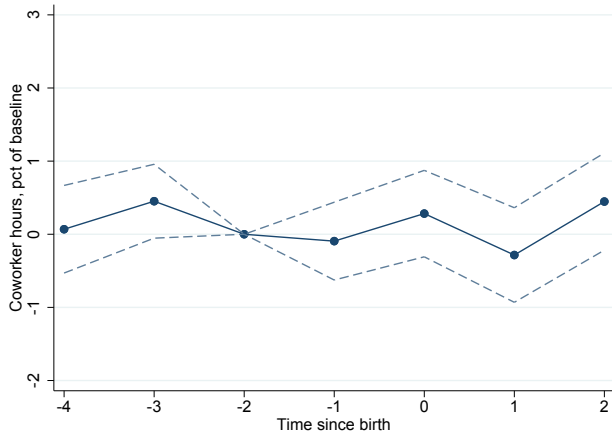
Figure A15: Effects on outcomes of coworkers in different occupations than women on leave, OLS
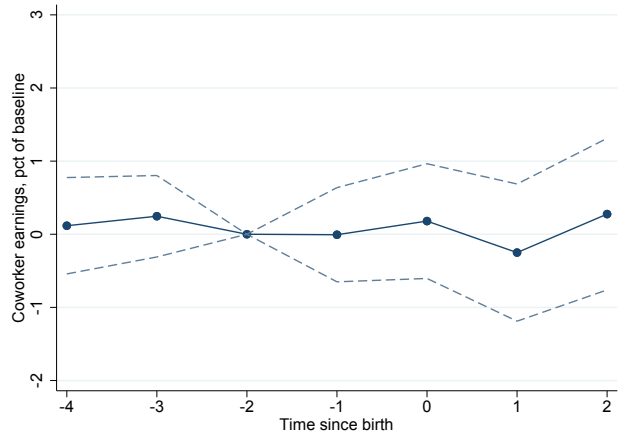

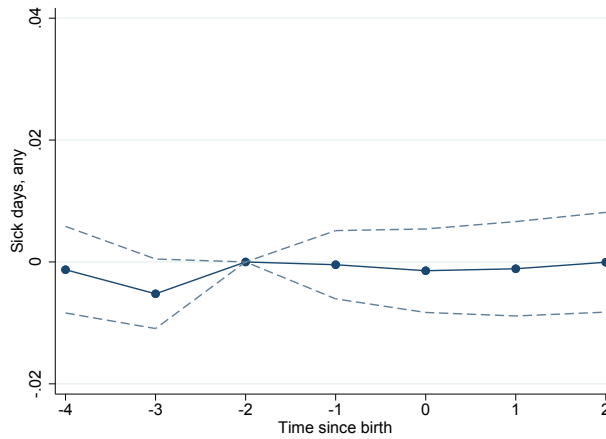(a) Likelihood of coworker at baseline firm

(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.

Table A7: 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	0.00976	0.00861	0.00141*	0.000777
	(0.00818)	(0.00854)	(0.000696)	(0.000721)
Share of year unemployed	−0.00334**	−0.00324*	-0.000247	-0.000269
	(0.00127)	(0.00132)	(0.000134)	(0.000140)
Hours at baseline firm (pct rel. to baseline)	1.611**	1.086*	0.172**	0.116*
	(0.445)	(0.482)	(0.0469)	(0.0504)
Earnings (pct rel. to baseline)	2.484**	1.900**	0.275**	0.210**
	(0.563)	(0.639)	(0.0604)	(0.0697)
Any sick days	0.0932	0.138	0.0334	0.0167
	(0.308)	(0.343)	(0.0356)	(0.0431)
F-stat	645.1	648.8	1,824	1,810
Observations	121,525	121,007	121,525	119,311
Observations (weighted)	76,153	75,821	76,153	75,821
Clusters (firms)	12,535	12,518	12,535	12,518
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 (weights)	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 interaction terms between births at the event time and the time dummies are instrumented by interactions between treatment status and time dummies. Columns (3) and (4) show estimates from similar regressions but in which we use births per 100 baseline employees instead of births and include dummy variables for each possible number of baseline employees. Results are reported for the event year (Time 0) and the following year (Time 1), and the analysis is conducted on the matched and reweighted sample. The reported F-statistics correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. The F-statistics are numerically similar for all instruments because: (i) the endogenous variables are time dummies interacted with the same time-invariant variable, (ii) the instruments are the same set of time dummies interacted with another time-invariant variable, and (iii) there are the same number of observations in each year. Standard errors (in parentheses) are clustered at the firm level. ** $p < 0.01$ * $p < 0.05$.

# H 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 change. Unfortunately, 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 (Figure A16 and Table A8).

We first find that in the event year, one additional female employee giving birth at the firm lowers the share of women by one percentage point, indicating that a leave-taking woman is being replaced by either a male or female temporary worker.[69] We also detect small changes in other characteristics. The average age of the workforce rises by 0.731 percent in the event year when an additional employee gives birth. This is concurrent with a 0.237 percent drop in the workforce's average years of education and an increase of 0.08 years in average experience. These results indicate that, on average, temporary workers are older than 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.

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[69]As previously mentioned, when computing workforce shares and averages, we weight each employee by their 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).
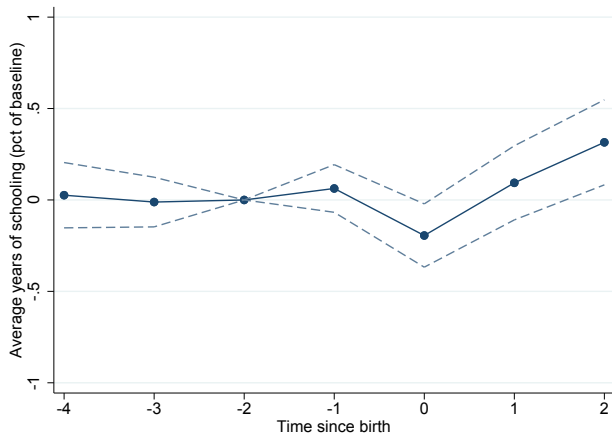
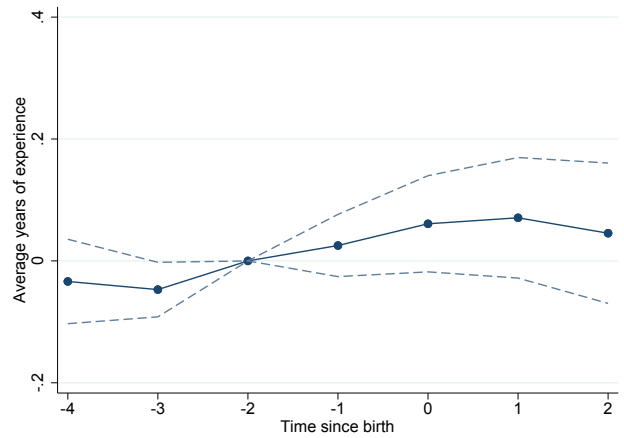Figure A16: Effect on workforce characteristics, OLS
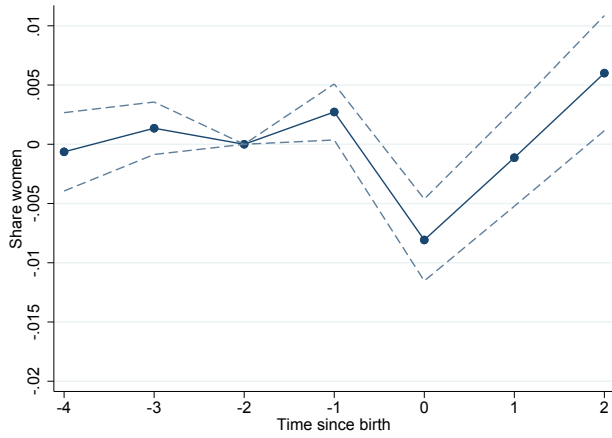
(a) Women's share of the workforce
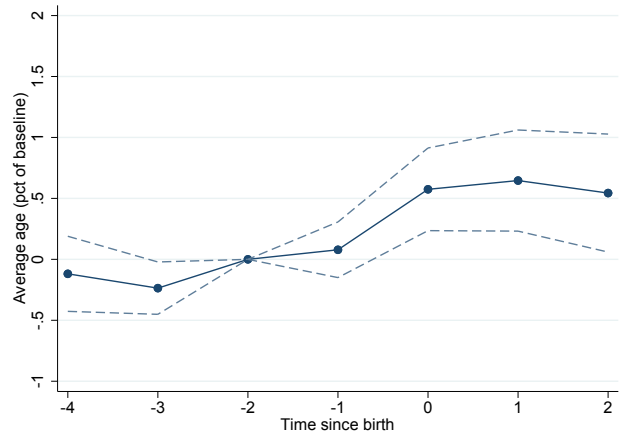
(b) Average age of the workforce

(c) Workforce average years of schooling

(d) Workforce average years of experience

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

Table A8: Effects on workforce characteristics, 2SLS

	Absolute effect Effect of one additional birth		Relative effect Effect of one additional birth per 100 employees	
	at $t = 0$	at $t = 1$	at $t = 0$	at $t = 1$
	(1)	(2)	(3)	(4)
Share women at baseline firm	-0.0118^{**}	-0.00263	-0.00103^{**}	-0.000410
	(0.00214)	(0.0796)	(0.000183)	(0.000211)
Average age (pct rel. to baseline)	0.731^{**}	0.707^{**}	0.0547^{**}	0.0525^*
	(0.219)	(0.254)	(0.0204)	(0.0236)
Average years of education (pct rel. to baseline)	-0.237^*	0.115	-0.0198	0.00714
	(0.111)	(0.131)	(0.0104)	(0.0122)
Average years of experience	0.0875	0.157^*	0.00362	0.00786
	(0.0493)	(0.0615)	(0.00427)	(0.00521)
F-stat	2,423	2,308	2,693	2,556
Observations	28,274	26,241	28,274	26,241
Observations (weighted)	17,666	16,938	17,666	16,398
Clusters (firms)	14,141	13,060	14,141	13,060

Notes: Each column-row represents the coefficient from a separate regression. Columns (1) and (2) show 2SLS estimates from regressions in which the interaction terms between births at the event time and the time dummies are instrumented by interactions between treatment status and time dummies. Columns (3) and (4) show estimates from similar regressions but in which we use births per 100 baseline employees instead of births and include dummy variables for each possible number of baseline employees. Results are reported for the event year (Time 0) and the following year (Time 1), and the analysis is conducted on the matched and reweighted sample. The reported F-statistics correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. The F-statistics are numerically similar for all instruments because: (i) the endogenous variables are time dummies interacted with the same time-invariant variable, (ii) the instruments are the same set of time dummies interacted with another time-invariant variable, and (iii) there are the same number of observations in each year. Standard errors (in parentheses) are clustered at the firm level. ** p < 0.01 * p < 0.05.