

Is Parental Leave Costly for Firms and Coworkers?*

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Abstract

The recent rise in female labor force participation presents new challenges for public policy. New mothers may want or need to leave the labor force temporarily to take care of their newborns. To ease this transition and encourage women to return to work, most countries have adopted parental leave policies that typically replace lost income due to temporary labor force exit and provide job protection. As of yet, much of the existing evidence on the effectiveness of such policies comes from studies that focus on their impacts on affected families —i.e., mothers, fathers, and their children —without a clear understanding of the effects on firms and co-workers.

We use Danish administrative data linking the universe of firms, workers and births to fill this gap in the literature. We evaluate the impact on firms and co-workers of an additional woman giving birth in an environment with a generous leave policy. We utilize a dynamic difference-in-difference design that leverages the timing of a woman’s leave and matches women who give birth in a particular year with women who do not give birth in that year. We argue that while the timing of a woman’s childbearing may be endogenous to her own outcomes, the timing is likely less endogenous from the firm’s perspective because a firm is composed of many employees. We rule out this concern that the timing of birth is endogenous from the firm’s point of view via the examination of firm-level trends prior to the birth. Firms respond to leave-taking by increasing employment via hiring additional temporary employees and slightly increasing work hours of existing employees. Total hours are unchanged with only slight adjustments to workforce composition. The firm’s total wage bill increases but once we account for the reimbursements that firms receive for leave payments, the total wage bill is unaffected. We observe no effect on the unemployment risk of existing employees, firm output, gross profits or firm closures. Overall, our estimates suggest that the costs of parental leave on firms and coworkers are small at best.

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; Kleven et al., 2018). In light of these facts, policy discussions surrounding family leaves have become more prominent. Nearly all high-income countries currently have generous leave entitlements with the goals of decreasing gender inequality and improving child development (Olivetti and Petrongolo, 2017). While many of these programs benefit mothers and their children (Rossin-Slater, *ming*), critics argue that leave take-up could impose substantial costs on employers. These costs include wage replacement benefits during parental leave but also more indirect expenses such as the cost of training and recruiting replacement labor. Although one of the goals of the parental leave policies is to improve the well-being of women, these incurred costs could harm women by making employers more likely to discriminate against women in hiring and promotion decisions.

To fully understand the benefits and costs of family leaves, it is not only essential to examine how they impact households but also firms. 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, California governor Jerry Brown signed into law a bill that requires small and medium-sized businesses to provide new parents with 12 weeks of leave. However, a year prior, he rejected a similar bill stating 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 maternal leave on firms and co-workers. Despite considerable policy relevance, direct estimates of the effects of leave on employers and co-workers are relatively scarce. In a recent review of the literature on leave programs, Rossin-Slater (*ming*) concludes that “we know very little about how maternity and family leave policies may impact businesses, who often worry about being burdened with extra costs resulting from dealing with employee leave-taking.” This is largely because answering this question requires comprehensive data linking firm and worker outcomes to information on fertility and leave-taking, a usually challenging undertaking. Identifying causal effects poses an additional challenge as leave-taking is likely correlated with unobservable factors, such as

worker productivity, that may simultaneously affect firm outcomes.

We study the effects of a woman giving birth and taking leave on firms' labor supply, costs, overall performance and co-workers' labor outcomes. We exploit rich administrative data that enables us to link the universe of firms and workers in Denmark from the years 2001 to 2013. To identify causal effects of leave-taking on firms, we build on the empirical strategy used by Azoulay et al. (2010) and Jäger (2016) matching treated (at which a woman goes on leave in a particular year) firms to untreated (at which a woman does not go on leave in a particular year) firms, focusing on small firms who may be most affected by leave taking. We utilize such comparisons in a dynamic difference-in-difference design where we compare firms over time to understand the evolution of the treatment effects (rather than comparing the post-treatment to the pre-treatment period). An advantage of this design is that it lends itself to several natural checks of the identifying assumption —i.e., treatment firms prior to childbearing should be similar to control firms.

From the firm's perspective, the costs of parental leave are multifold. First, in a paid parental leave system, absent workers are entitled to receive wage replacement and firms may have to bear these costs. However, in most high-income countries, employers are not responsible for paying for wages of workers on leave.¹ In the case of Denmark, firms are reimbursed for these direct costs. Second, in order to avoid losses in productivity, firms have to either hire new workers or require existing employees to take on additional work. In a frictionless labor market without firm-specific human capital, employers are expected to fully replace workers on leave by hiring other employees at the market wage. In this case, the total costs would be equivalent to the disbursed maternity leave benefits, which then, in the context of Denmark, would be fully reimbursed. On the other hand, in a labor market with frictions and/or human capital specificity, employers might incur additional costs associated with searching for and recruiting temporary workers, training existing or new workers, paying for overtime work and losses in labor productivity (Appelbaum and Milkman, 2011).

Our empirical analysis yields several key findings. First, firms in which a woman gives birth are exposed to roughly 325 extra days (nearly 11 months) of parental leave compared to counterfactual firms. This estimate confirms national statistics indicating that on average, women in Denmark take roughly 10 months of parental leave. Our finding is also consistent with previous studies documenting that new mothers increase their time away from work when maternity leave programs are available (Waldfogel, 1999; Rossin-Slater et al., 2013; Carneiro

¹The International Labour Organization (ILO) estimates that 88% of developed economies rely on social security systems to finance maternity leave benefits. Similarly, paid family leave programs in California, New Jersey, New York and Rhode Island are entirely funded through employee payroll tax contributions.

et al., 2015; Dahl et al., 2016), and that they can even be incentivized to take long periods of leave (Piketty, 2005; Lalive and Zweimüller, 2009; Schönberg and Ludsteck, 2014).

Second, we show that treated firms are able to compensate for labor supply losses due to leave take-up by making adjustments both at the intensive and extensive margins. Compared to the control group, they hire more temporary workers and slightly raise work hours of existing employees. As a result, total hours of work are unchanged while the overall number of employees increases by 4.2 percent.

Third, 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 wages of existing employees. Nonetheless, treated firms' overall wage bill drops by 2.8 percent when we exclude parental leave payments. This likely reflects that temporary workers are paid less than women on leave. Finally, a birth does not seem to affect overall firm performance, as we do not find significant effects on output, gross profits or the likelihood of firm survival. Taken together, our estimates suggest that the costs of parental leave for employers are negligible.

Our paper contributes to a large 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 and Petrongolo (2017) and Rossin-Slater (ming) for a review of the literature).² The evidence regarding the impacts of leave on firms is scant. Most of the existing evidence on firm effects comes from policy reports that aim to shed light on how employers fare with family leaves in several U.S. states. Bedard and Rossin-Slater (2016) use panel data from California and employer fixed effects to compare firms that are exposed to different leave take-up rates, before and after an employee takes leave. They find that a worker going on leave does not raise firms' wage costs and turnover rates but cannot rule out that their estimates are confounded by other factors that simultaneously change across employers and over time. Bartel et al. (2016) survey 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-difference approach and compare employers in the state to those in neighboring Massachusetts and Connecticut before and after the policy. They also find no significant impact on turnover

²The 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 and Milligan, 2008; Blau and Kahn, 2013; Lalive and Zweimüller, 2009; Lequien, 2012; Schönberg and Ludsteck, 2014). Furthermore, the introduction of maternity leave improves children's health, education and earnings (Rossin, 2011; Carneiro et al., 2015) but further expansions in the duration of leave have no significant effects on a range of child outcomes (Baker and Milligan, 2010; Rasmussen, 2010; Dustmann and Schönberg, 2012; Dahl et al., 2016; Danzer and Lavy, 2018).

rates, employee productivity and morale but warn that their small sample size precludes them from drawing definitive conclusions.³

To the best of our knowledge, only one existing study looks directly at the causal impact of maternity leave on firms’ performance and co-workers’ career trajectories. Gallen (2017) studies a reform in Denmark which increased the average maternity leave duration from 8 to 9 months. Using a regression discontinuity design based on the date of the reform, the author finds that providing an additional month of leave has no impact co-workers’ earnings or promotions but that it marginally increases the likelihood of firms shutting down. Our paper differs as we focus on the impact of an employee taking leave, rather than expanding leave duration for individuals who have already been on leave for a lengthy period of 8 months. Our treatment is thus more extensive and allows us to document the initial adjustments made by employers following leave take-up.

Our study is related to other literatures examining the impacts of employee absences on firms and co-workers. Papers within that literature focus on settings that are very different from ours—that is worker absences due to deaths (Azoulay et al., 2010; Bennedsen et al., 2013; Isen, 2013; Jäger, 2016; Jaravel et al., 2018), labor disputes (Gruber and Kleiner, 2012; Krueger and Mas, 2004; Mas, 2008), illness (Herrmann and Rockoff, 2012) and departure of experienced nurses (Bartel et al., 2014). They find that these types of absences have significant negative effects on co-workers’ productivity and wages as well as firms’ revenues, product quality and overall performance.⁴ In contrast, we show that the costs of absences due to parental leave are negligible.

The rest of this paper is organized as follows. Sections 2 and 3 respectively detail the institutional setting and the data we use. Section 4 outlines our research design and Section 5 presents our results. Finally, we conclude in section 6.

2 Institutional setting

2.1 Family leave policies

As early as 1901, Danish women have been entitled to some leave following childbirth. However, this leave, consisting of 2 weeks for female factory workers, was not generous as it is today.

³Additionally, Appelbaum and Milkman (2011) and Lerner and Appelbaum (2014) provide descriptive analysis of in-depth interviews and survey data collected after the introduction of California and New Jersey’s paid family leave programs. They report that on average, surveyed firms saw no changes in terms of profits, costs and employees’ productivity, but the authors do not attempt to identify *causal* effects of parental leave on employer outcomes.

⁴An exception to the literature is the study by Jäger (2016) who finds that unexpected worker deaths increase co-workers’ wages and retention rates suggesting the presence of labor market frictions.

In 1960, Denmark adopted universally-paid leave comparable to that seen in many countries today of 14 weeks but without job protection (DICE Database, 2015). The introduction of parental leave with 6 weeks of parental leave (on top of the 14 weeks of maternity leave) came in 1984. Throughout the 1980s and 1990s, the Danish family leave policies were continually changing - extending the length of maternity and paternity leave, changing the rate of wage replacement, and including job protection provisions.

Today Danish family leave consists of two parts: 1) wage replacement for a specified number of weeks at a specified rate, which we discuss below and 2) job protection - the assurance that a similar job will be available at the firm at the end of the family leave period. Since 2002 (the time period relevant for our study given our data range of our sample), mothers are eligible to 4 weeks before birth and 14 weeks after birth (all 18 weeks at full unemployment benefit level; many employers provide extra compensation, though). In addition, there is additional family leave of 32 weeks of leave, which is to be shared amongst partners. For children born in the time span of our sample, mothers take on average 8.7 weeks before giving birth and 44.6 weeks after birth. The distribution of leave is highly peaked with an identifiable mode at 322 days of postnatal leave (the maximum amount of leave). All women take some leave; by European Union law, women are required to take at least 2 weeks of leave. For fathers, length of leave is considerably shorter - 5.3 weeks.

While on family leave, wage replacement is based on the wages and hours of one's job. These benefits are paid through the Unemployment Insurance (UI) system. For the roughly three quarters of firms that have collective bargaining agreements, mothers receive full pay for pre-birth leave and the first 14 weeks of leave post birth. Additional leave for those salaried employees covered under a collective bargaining agreement is reimbursed as follows: 5 weeks of additional full pay to the mother, 5 weeks of additional full pay to the father, and 3 weeks of full pay to be split amongst the two parents as they choose. For those mothers employed under *Funktionaerloven* (i.e., not an hourly employee) and who do not have coverage from a collective bargaining agreement, they will be paid half her wage during the 4 weeks prior to birth and 14 weeks after birth (or maximum UI if that is higher than half the wage).

These monetary benefits parents receive are funded through contributions taken from worker's wages to the Unemployment Insurance system. Prior to 2006, employers could join "parental leave funds" to replace fully worker's wages. Since 2006, membership in a parental leave fund has been mandatory for all employers, implying that firms in our data are almost fully reimbursed for wages paid to women on leave.⁵

⁵The parental leave funds are required to reimburse for at least 4 weeks of leave before birth, 2 weeks to the mother

In addition to the benefits offered to women on parental leave, Danish parental leave policies offer employment protection for the length of leave although there are certain exemptions. In particular, employers are not allowed to terminate the employee because of the leave, although they can terminate her for other reasons such as downsizing or plant closing.

Low levels of employment protection and high turnover and mobility is an important feature of the Danish labor market in general, as turnover and job mobility rates in Denmark are more similar to the US labor market than other European labor markets (Andersen and Svarer, 2007). Employers thus have much scope for firing other employees and/or temporarily increasing their workforce when they experience an employee going on leave. Firms also frequently hire temporary workers. In the case that a worker goes on family leave, firms often employ such workers .

3 Data

Our administrative data are collected from several sources and covers the universe of Danish firms and workers from 2001 to 2013. Data on workers' childbirths, parental leave payments and person identifiers —which uniquely identify all individuals who have ever resided in Denmark —are taken from the central population registry (CPR). We then obtain data on employment relationships allowing us to link workers to firms, and generate measures of firm-level employment, hours of work and wages. Additionally, we collect 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.⁶ We note that we can distinguish between different firms but not different establishments of the same firm. In other words, each firm may include several establishments but the information that we have is at the firm-level. Nonetheless, our sample includes mostly single-establishment firms since as further discussed in the Research design section, our focus is on small firms, who are likely to be most affected by leave taking.

3.1 Data on workers

Individual-level data for all workers are taken from the central population registry (CPR), which contains links between parents and children, dates of births and information on leave

following birth, 2 weeks to the father following birth, and 25 weeks to the parents collectively which the parents can split as they wish ("Law on maternity compensation on the private labor market"/"Lov om barseludligning p det private arbejdsmarked").

⁶Participation in the CVR registry is required for all firms with a yearly revenue above 50,000 DKK (about 7,000 euro).

payments and demographic characteristics. We use these data to construct several key variables. First, under assumption that women had full-term pregnancies, we extrapolate their pregnancy periods from the dates of births of their children. Second, using data on the amount of parental leave benefits paid out to each worker and the amount of leave reimbursements firms receive, we calculate total days of paid and unpaid leave for each worker. Throughout, we include both prenatal and postnatal leave. We supplement the CPR with data from the central education register and labor market data that allow us to obtain a range of worker characteristics measured prior to our sample period such as average age, average schooling and average size of the firms' workforce in each year as well as the share of women in the firms' workforce. When computing these yearly shares and averages, we weight workers with weights equal to the number of hours they have worked at the firm in that year.

3.2 Matched employer-employee data

Information on employment relationships comes from yearly administrative data on wage payments from firms to workers (CON and RAS), as well as the Integrated Database for Labor Research (IDA). We use these data to construct measures of firm-level employment, hours of work and wages.

For the stock of employees at a firm, we use the main November employment relationships from the IDA. 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.⁷ We refer to the total number of such workers as the number of *employees* at the firm.⁸ We note that employees who go on parental leave will continue to be included in this definition regardless of whether their leave spans the last week of November.

In addition to examining the stock of employees at a point in time, we are also interested in examining changes in hours worked. We construct a measure of how many hours each worker has supplied to a firm based on 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 weeks worked during the year (Lund and Vejlin, nd). Appropriately scaling the contribution amount therefore gives us a measure of total hours supplied during the year. We scale contributions so that hours are measured in full-time equivalent workers (FTEs). To correct for the fact that ATP contributions continue

⁷We define the main job as the job with most work hours and in the case of any ties, the one with the highest earnings.

⁸The results we present later are virtually unchanged if we instead include all workers who were ever at the firm in any capacity during the year.

while employees are on paid leave, we subtract the share of the year that each employee is on paid parental leave.⁹ Unlike the employee stock, the measure of hours worked thus does not include instances when the worker is on leave.

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 his/her main job. We then calculate the firm-level total wage bill as the sum of all payments to workers during the year. This total *wage bill* could include payments made to workers on (paid) parental leave. As an alternative measure, we therefore construct the *wage bill ex. leave* where we remove payments made to workers on leave.¹⁰ By examining the effects of parental leave on both the total *wage bill* and the *wage bill ex. leave*, we can shed light on how firms are affected both in the absence of reimbursements and when paid leave is fully reimbursed.

3.3 Additional firm data

Information on firms' output and profitability is taken from the VAT data. As part of the administering the Danish VAT, all firms are required to report their total sales and purchases given that revenue exceeds some value.¹¹ We use total *sales* as our measure of firm output and firm *purchases* when checking covariate balance further below.¹²

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 includes purchases of capital equipment, which would not normally be included when calculating gross profits.¹³ If firms in our sample respond to employee leave-taking by systematically increasing investments, this will understate gross profits and vice versa. Accounting data that separate investments

⁹This introduces some measurement error for employees who hold more than one job since their total number of days on paid 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 however: only 3.2 percent hold a second job which has more than half the hours of the main job.

¹⁰For 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 without paid leave (based on ATP contributions and total days on paid leave). Again, this introduces some measurement 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 kr for the year, this calculation falls to 1.4 percent.

¹¹As of 2018, this value is 50,000 kr but was even smaller during our sample period. Most items are subject to VAT except exports. However, exports are included in the sales data we use.

¹²Due 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.

¹³Normally, capital purchases only affects net profits because these include capital depreciation.

from material costs and other inputs are unfortunately not available for most of the small firms of our analysis. We further compute *gross profits ex. leave* by removing wages paid to workers on paid leave from profits. Examining these two different measures allows us to examine how firm profitability is affected both in the absence of reimbursements and when firms receive full reimbursements for paid leave.

One thing to note 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 have shutdown 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.

4 Research design

The goal of our study is to isolate the effect of parental leave on firms and coworkers. A natural starting point is the examination of leave policy changes in Denmark. For example, in 1960, a universal paid leave program for mothers for 14 weeks was introduced. Prior to that change, leave availability was very limited. Thus, leveraging such variation would give the effect of moving from no leave policy to a 14-week leave policy. However, Danish employment and firm data, as typical with many data, are only available as of recently. Alternatively, one could exploit more recent policy changes. A reform in 2002 extended parental level by 20 weeks but the effect on leave taking was one month on average (Gallen, 2017). As this treatment is relatively small, the effect on firms may be negligible. From a policy perspective, especially from the context of countries contemplating extending their parental leave significantly or introducing it for the first time, these shorter leave changes in Denmark may be less useful for policy decisions.

We take a different approach. Our analysis relies on an event study framework in which we compare the evolution of outcomes of the firms (and coworkers) of a childbearing woman proximate in time to her childbearing with the evolution of outcomes of the firms (and coworkers) of a similar woman who does not bear a child at that time. Our control firms include firms with both women who do not give birth ever and those who give birth in a different year than the matched treatment. In this sense, many of our control firms will eventually be treated. This design primarily leverages the timing of a birth at a firm rather than whether a

firm has a female employee who gives birth during her time at the firm.¹⁴ We note that since our treatment is defined as a woman giving birth, we cannot separate the effects of parental leave from any direct effects of employees giving birth that would occur even in the absence of parental leave. Given the very long leaves taken by women in our sample, however, we expect the direct effects of birth to compose a very small part of the effects we estimate.

The validity of this approach in identifying the effects of a woman’s leave due to childbearing rests on the assumption that the timing of her childbirth is random from the perspective of the firm. Note that this assumption is not that the timing of the birth is exogenous from the woman’s perspective —a requirement that would be necessary if we were using this identification strategy to isolate the impact of a woman’s own childbirth on her own outcomes (Kleven et al., 2018). The event study framework gives a natural method of testing whether the timing is correlated with firm outcomes by testing whether there are trends in the outcomes prior to the woman giving birth.

We should note, however, it is possible that the effects of leave precede the actual leave taking. In the Danish setting, employees are required by law to notify their employer at least 3 months before they go on parental leave and it is a common norm to broadly announce pregnancies at the end of the first trimester. All of these things may allow firms to mitigate the negative impacts of parental leave by postponing or speeding up certain tasks, by having the mother-to-be pass on instructions to temporary replacement workers, etc. When estimating the effects of parental leave, we want our estimates also to capture these anticipation effects.

Our research design, since it exploits differences between firms, will miss economy-wide effects of leave. In particular, firms could tailor their production technology or make other costly changes in response because they anticipate their workers going on leave in the future. Additionally, firms may avoid hiring women to circumvent having employees take lengthy parental leaves. These type of effects will be missed in our analysis since they likely affect treated and control firms similarly.

4.1 Definition of treatment and control events

A standard event study setup is insufficient for this research question. Family leave is too common and thus, thinking about a control group that is unaffected by family leave is not possible. A firm for which no employee is on parental leave in one year (thus, making a natural control) is likely to have at least one employee on leave in the future.

¹⁴Limiting the control firms to firms with no female employees who ever give birth would greatly reduce our control sample size and make the treatment and control firms very different, particularly on the female employee age dimension.

Our departure from the usual event study approach involves defining treatment events and control events. Once these events are defined as we discuss in detail below, we match each treatment event to a set of control events.

Treatment events occur when a woman gives birth in a year whereas control events happen when a woman does not give birth in a year. Specifically, a *treatment event* is defined as a woman x firm x year combination where the year denotes a year in which the woman gave birth.¹⁵ These births can occur any year between 2005 and 2011, allowing us to have the same amount of pre-birth and post-birth data for each birth. For the means of defining a treatment event, the mother must be employed at a firm two years prior to the birth and the treated firm is the firm at which she was employed two years before the birth. We refer to the year of the birth as the *event year* and refer to the (calendar) year two years prior as the *baseline year*. As women may be pregnant in the calendar year prior to birth, the baseline year outcomes will not be directly impacted by the birth unless there are anticipatory effects. For each event, we refer to the mother giving birth as the *treatment woman* and the firm where she was employed in the baseline year as the *treatment firm*.

We define *control events* completely analogously to the treatment events, only now we require that the woman in question does *not* give birth in the year prior to the event, in the event year, or the year following the event year. More precisely, a control event is a woman x firm x year who did not give birth in that year and was employed at that firm in year-2. These control events are drawn from years 2005 to 2011. For each such event, we refer to the women as the *control woman* and the firm where she was employed two years prior to the event year as the *control firm*.

4.1.1 Sample restrictions on treatment and control events

To be included in our baseline sample of events (treatment or control), we further require that the woman and firm satisfy the following restrictions: As we require two years of data prior to the birth and the ages 21 through 35 are the prime childbearing years, we choose women who are between 19 and 33 in the baseline year. We also exclude women who are students from the analysis. Births of all pluralities are included. The woman's job at the treatment or control firm must constitute her main attachment to the labor market in the baseline year and she must have been working at a treatment or control firm for at least one year prior to the

¹⁵We denote the woman's employment firm as the firm of her main employer in November two years prior to her giving birth.

baseline year.¹⁶ The point of these requirements is to make sure that we are looking at women during their prime childbearing years and women with a stable attachment to their firm in the baseline year. Finally, for both treatment and control events, we require that the woman must not have given birth in the year immediately following or preceding the event year. We do this in order to identify cleanly the effect of the parental leave that the woman will take following the birth occurring in the event year.

After imposing the above restrictions, our baseline sample contains a total of 140,063 treatment events and 792,620 control events. The combined sample of treatment and control events consists of 71,117 unique firms.

We make a few remarks about this sample. First, we note that the same woman and/or firm can be included in several different treatment or control events. For example, consider some woman, *A*, who stays employed at firm *X* and gives birth in 2005 and 2008. For event years 2010 and 2011, woman *A* and firm *X* will contribute control events. For event years 2005 and 2008, woman *A* and firm *X* will constitute treatment events. Note that years 2006, 2007, and 2009 will not be included in the analysis given the control event requirement that a woman cannot give birth in the year prior or the year following the event year. We correct for the duplicity of woman x firm combinations in our inference later. Similarly, for control events, a woman can contribute to multiple events. For example, if woman *B* stays employed at firm *Y* throughout our sample period and never gives birth, woman *B* and firm *Y* will be part of a control event corresponding to each year of the sample, i.e., {woman *B*, firm *Y*, event year 2005} will be one control event, {woman *B*, firm *Y*, event year 2006} will be another control event and so on. Second, we note that the same woman and/or firm can both be part of treatment and control events.¹⁷ Accordingly, the combined sample of treatment and control events only consists of 71,117 unique firms. Third, with the exception of births (which spark the leave-taking behavior we are interested in), our sample selection (and later matching algorithm) only uses characteristics as measured in the baseline year.

4.2 Further restrictions on the sample

We impose some further restrictions on our baseline sample of treatment and control events.

We list and discuss these below.

¹⁶To ensure that the job is the woman's main attachment to the labor market we require that the job is associated with positive earnings in our data and that it is a main November job according to the IDA database, see Section 3.

¹⁷Our results are virtually identical if we impose the additional restriction that a control event can not contain a firm that is also part of a treatment event in that year.

1. **The size of the firm is between 3 and 30 employees in the baseline year.**¹⁸
Total hours over the year must equal at least that of 1 full-time equivalent worker.¹⁹

We focus on small firms for several reasons: First, year-to-year variation in whether a single employee goes on leave is more likely to have detectable effects on firm-level outcomes at smaller firms if such effects exist. Second, at large firms, the law of large numbers tend to eliminate the year-to-year variation in leave taking that our research design aims to exploit.²⁰ Third, much of the leave policy interest centers on small firms, who could possibly incur larger costs because one worker constitutes a larger fraction of the total workforce at a small firm. Fourth, at smaller firms, the probability that remaining employees interact with the women on leave is higher. Furthermore, because we do not have establishment-level data, smaller firms are likely to have fewer establishments, and thus, the probability of two employees, chosen at random, interacting with one another is larger. In the end, this selection criteria reduces to 22,435 treatment and 139,731 control events covering 44,013 unique firms (Table 1 summarizes our sample selection process). This large reduction reflects that by definition, most workers work at firms with many employees.

2. **Firms must be private.**

Measures of sales and profitability, two of our main outcome measures, are irrelevant for public firms. This further reduces our sample to 19,909 treatment and 125,131 control events covering 40,713 unique firms.²¹

3. **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 euros) and 100 million DKK (1.3 million euros) and wages per worker must be between 10,000 DKK (1,300 euros) and 1 million DKK (130,000 euros).**²²

The resulting sample is 17,141 treatment events and 110,523 control events for 36,455 unique firms.

¹⁸This restriction is also invoked in Jäger (2016) who studies how a worker’s death impacts the firm.

¹⁹As discussed in Section 3, number of employees always refers to the main November job count. To deal with highly seasonal firms who shed most of their work force in November, however, we impose the additional requirement that the firm can not have had more than 60 workers attached throughout the baseline year.

²⁰As the number of employees grows, the firm will tend to have a constant share of employees on leave every year.

²¹The Danish public sector is large and women are overrepresented in public sector jobs, however, the reason we only lose relatively few events here is because most public sector workplaces are very large and thus have already been removed from the sample when conditioning on size.

²²We define a firm to have been active if it either had positive sales or purchases or had a positive wage bill.

4. **Firms experiencing extreme changes in sales or employment are dropped. Specifically, firms with a change in employees or hours exceeding 150 full-time equivalent employees in a single year, firms who more than triple their employment or hours in a single year if the change constitutes an increase of more than 15 employees, firms whose sales or purchases change more than 1 billion DKK (130 million euros) in one year, and firms where the total number of employees in a year changes by more than 300 or where it more than triples if it constitutes an increase of more than 30.**

Dropping these outliers brings our sample down to 16,565 treatment events and 106,892 control events covering 35,283 unique firms.

Table 1 documents the changes to the sample size with each restriction.

[Table 1 about here.]

4.3 Matching

To find appropriate counterfactuals for the treatment events, we select a set of control events for each treatment event. The primary goal of the matching is to ensure that the women precipitating the treatment events are similar in terms of their past fertility and have similar employment characteristics. The use of all non-treatment events as controls results in the treatment and control events not being very comparable in terms of pre-event trends and baseline characteristics.

For each treatment event, we select a worker-firm-year control event based on exact matching on the following characteristics in the baseline year:

1. Woman characteristics: Total number of children, number of 2-year old children, number of 1-year old children, number of newborns, education group,²³ whether the woman has at least two years of tenure, quintiles of age, and quintiles of earnings.²⁴
2. Firm characteristics: Quintiles of the number of employees, quintiles of sales, quintiles of share of female employees and quintiles of average number of children per employee.

This matching procedure results in some treatment events being unmatched to at least one similar control event. We drop these treatment events from our analysis. In some cases, there will be multiple matched control events for a treatment event. In that case, we reweight the

²³We use the standard 6 Danish education groups. We treat missing education information as a separate category.

²⁴When computing quintiles for monetary variables, we compute them separately for each calendar year to deal with inflation. When we computing quintiles of other variables, the quintiles are calculated for the overall sample.

control events with weights equivalent to the ratio of the number of treatment events to control events in the matched cell. For example, suppose that treatment event A and treatment event B have identical values for the matching variables defined above. Further suppose there exist 3 control events that also match these two events. Each of the control events will then receive a weight of $\frac{2}{3}$. Treatment events receive a weight of 1.

After applying the matching procedure, there are 6,089 treatment events (matched with 14,575 control events) covering 11,653 unique firms. For only 36% of our treatment events do we find a suitable match because of our fine-grained nature of our matching procedure. This highlights the tradeoff we face in our estimation —matching on fewer variables would increase the match rate but would reduce the comparability of the treatment and control events. Table 2 provides a comparison of the successfully matched treatment events to the unmatched treatment events. In general, the women of matched treatment events have fewer children, particularly fewer young children, have slightly higher earnings, work at firms with slightly higher sales and fewer children per employee. Given the likely heterogeneity in the response of the firm, these differential characteristics are important to keep in mind when extrapolating our results to other populations.

We recognize that the match rate of 36% may be lower than desired. This is the direct result of our fine-grained nature of our matching procedure which involves matching on roughly 50 variables, and thus, a very large number of cells. Therefore, for every cell, there are not necessarily both treatment and control observations (i.e., there are some cells without little overlap of support). We categorize the concern with a low match rate into two categories: concerns regarding identification and concerns regarding estimation.

Regarding identification, because of the treatment and control observations are so well balanced due to the wealth of matching data, it is arguable that the treatment-control contrast provides us an estimate of an effect close to the treatment effect of being on leave. However, one might argue that while then the estimates are internally valid, the estimates may not be externally valid in the presence of heterogenous treatment effects. The largest contrast between the matched treatment events and the unmatched treatment events is the number of children 0 to 2 years old. For unmatched treatment events, it appears that the women’s children are closer in age. Thus, many of the women contributing to the unmatched treatment events may be going on leave and then become pregnant on leave or shortly there after. For this group of women, estimating a treatment effect is more difficult because finding a matched control event is challenging.

Regarding estimation, things are a bit more complicated. Initially, one can view the issue

as a bias/variance trade-off. If we use (more) inexact matching and/or a more coarse exact matching we end up comparing some individuals who are in fact not 100 percent comparable. This introduces a bias, which is bad. On the other hand we end up with a bigger sample. This decreases variance, which is good. In addition to this fairly standard bias/variance trade-off, however, there is the issue of "thin" support: Even if we focus on groups of women that are theoretically in the common support, it could be that the probability of treatment is very low (or very high) in some groups. In finite samples this could imply that we (sometimes) have some control individuals that receive an unusually large amount of weight. This can cause estimates to be very poorly behaved (Bodory et al., 2018). To address this issue, we apply the recommendation of Crump et al. (2009) and remove cells where the fraction of treated events exceeds 0.9 or falls below 0.1.

Coarser matching or non-exact matching would increase our match rate but is likely to make our treatment and control groups less comparable. Future versions of the paper will include matching with matching variables chosen through machine learning. For robustness, we will also present results using fewer matching variables.

[Table 2 about here.]

4.4 Regression specifications

We operationalize this research design with a dynamic difference-in-difference specification (i.e., event study specification) using the sample of matched treatment and control events. The dynamic nature allows us to look at how the effects vary around the time of leave. In a typical event study design, event time (e.g., year - year of birth in this case) is typically defined only for the treatment group because the control group does not experience an event. However, here because of our matching procedure, we designate each control event an event year based on the year of birth associated with the treatment event. For example, assume woman A at firm F in year 2010 has a birth to which we match a control event of woman B at firm G in year 2010. As one of the matching variables is year, the control event's event time will be defined by the timing of the treatment event. Specifically, event time for this control event will be year - 2010.

Firm analysis

For our firm analysis, the difference-in-difference specification is as follows:

$$Y_{ift} = \gamma_i + \sum_{k \in \mathcal{T}} Time_{ikt} \cdot \alpha_k + \sum_{k \in \mathcal{T}} \beta_k Time_{ikt} \cdot Treatment_i + \varepsilon_{ift} \quad (1)$$

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

where Y_{ift} is some firm outcome in calendar year t for firm f as a part of event i , $Time_{ikt}$ is a series of event year dummies (i.e., dummy variables for event years -4 to 2, excluding the baseline year -2), and $Treatment_i$ is an indicator for whether event i is a treatment event. We estimate this regression specification via weighted OLS on yearly data four years prior to the event through two years following the event using the weights defined earlier. Note we limit the event window to these years because of employee mobility. We examine the outcomes of firms at which the woman is employed in event year -2, and the wider the event window the higher the probability that the woman will not be employed at that firm for years away from the birth. This would make the event study approach less informative about the effects of leave. For example, suppose a woman is employed at firm A in the baseline year but moves to firm B three years following the birth. Then, the event study estimates of the effect on firm A of the woman's leave in event year 3 are not very relevant since the woman is no longer employed at firm A. However, if the event study was based instead on firm B's outcomes, one would worry about endogeneity. However, we do not view the limited event study window as a significant drawback because the effects of leave —to the extent they exist— are short-lived. When estimating equation (1) we 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).

γ_i are woman x firm fixed effects such that we leverage changes over time occurring at the woman's firm at baseline (i.e., controls for unobserved heterogeneity across firms and individuals). The uninteracted event time fixed effects (α_k) capture the counterfactual (control) trends in the control firms.

Our main parameters of interest are β_{-4} , β_{-3} , β_{-1} , β_0 , β_1 , and β_2 . The reference period for all of these β coefficients is event year -2, the benchmark year we use for our matching procedure. Together the β s map out the evolution of the treatment effect over time. Specifically, these β coefficients demonstrate how the treatment group differs from the control group in event time space. β_0 identifies the effect of leave in the year of leave whereas β_1 and β_2 demonstrate the later post-birth dynamics and are our prime interest as they measure the post-birth treatment effects.

The estimates of β_{-4} , β_{-3} , and β_{-1} provide useful validity checks of the design. In an ideal

design, these coefficients should hover around 0 —signifying that there is no effect before the birth occurs and more formally, that the treatment firm outcomes are not different four years before the birth, three years before the birth, and one year before the birth than the control firm outcomes. However, it should be noted that there could be anticipatory effects (i.e., effects of the leave before the leave happens, especially given the time gap between conception and birth so event year of -1 could in fact be the year of conception) and result in $\widehat{\beta}_{-1}$ being different from 0.

The estimation of equation (1) provides intent-to-treat (ITT) effects. In other words, a treatment firm may not experience a childbirth. Since we define the treatment firm as the firm where the childbearing woman was employed two years prior to her giving birth, she may no longer be at that same firm when she gives birth. This is analogous to the typical non-compliance issue in the treatment effects literature. To address this, we use an instrumental variables approach to scale our reduced-form ITT estimates. Our outcome equation becomes:

$$Y_{ift} = \pi_i + \sum_{k \in \mathcal{T}} Time_{ikt} \cdot \rho_k + \sum_{k \in \mathcal{T}} \tau_k Time_{ikt} \cdot BirthsInEventYear_{if} + \eta_{ift} \quad (2)$$

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

where $BirthsInEventYear_{if}$ replaces $Treatment_i$ and measures the number of births occurring at firm f in the event year. To handle the endogeneity of $BirthsInEventYear_{if}$, we instrument it with $Treatment_i$.²⁵ The validity of this analysis hinges on the assumption that differences in the evolution of treatment and control differences only impact firm outcomes through the number of births. τ_1 and τ_2 capture the post-birth effect of one additional female giving birth among complier firms.²⁶

An interpretation subtlety related to equation (2) is that our specification estimates the effect of one additional woman having a child. Recall that the requirement for a control event is that a woman, *not a firm*, does not experience a birth in the event year, the year prior to the event, or the year following the event. Thus, a firm x year combination can appear in both

²⁵Because the endogenous variables are time dummies interacted with the same time-invariant variable (birth in the event year) and the instruments are the same set of time dummies interacted with another time-invariant variable (the treatment dummy), the first stage of this 2SLS estimation has some particular properties to note. First, in each of the first stages, the coefficient on five of the six instruments will be identically zero (both in the population and in the sample). For example, in the first stage regression for $Time_{i0t} \cdot BirthsInEventYear_{if}$, only $Time_{i0t} \cdot Treatment_i$ can have a nonzero coefficient. Second, when estimated on a sample that has the same number of observations each year, the estimated coefficients and other statistics from the first stages will be identical.

²⁶As most women who give birth go on leave, giving birth is synonymous with going on leave. But technically, our estimates identify the effect of a birth to a woman at the firm.

the treatment and control sample. There may be births occurring at control firms and more than one birth happening at the treatment firms. As a result, the gap in the number of births between treatment and control firms in the event year may deviate from one. For that reason, the 2SLS estimates may be preferred.

Table 3 shows (weighted) summary statistics of the firm sample that we use to estimate (1).

[Table 3 about here.]

Co-worker analysis

We take a parallel approach to estimating the effects on a woman’s birth on co-workers at her firm. We define treatment and control events as before. However, there is a slight difference in the sample. Each treatment event contributes X number of observations per event year where X is equal to the number of co-workers of the treated woman at the treatment firm in the baseline year. The co-worker sample stays fixed for event years -4 to 2. For example, some of the co-workers of a treated woman may change over time, but because these changes may be endogenous, the sample of co-workers are the employees whose job at the treatment firm constituted their main November job in the baseline year. The co-worker sample for the control firms is formed in a similar manner.²⁷ Aside from this sample difference, the regression specifications are analogous (i.e., use data for the same event years and reweight sample):

$$y_{i w f t} = \psi_i + \sum_{k \in \mathcal{T}} Time_{ikt} \cdot \omega_k + \sum_{k \in \mathcal{T}} \theta_k Time_{ikt} \cdot Treatment_i + \psi_{i f t} \quad (3)$$

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

We are primarily interested in θ_1 and θ_2 , the effects on co-workers when a female co-worker gives birth. Tests of whether θ_{-4} and θ_{-3} differ from zero are useful checks of the primary identifying assumption: outcomes of co-workers at control firms provide a valid counterfactual for the co-workers at treatment firms. Non-compliance is also an issue for the estimation of the co-worker impacts. Therefore, we also construct LATE estimates for the co-worker sample using treatment status as an instrument for the number of births, analogously to equation (2) above.

²⁷For five firms, we have missing data on all of their relevant co-workers so we drop these firms from the co-worker analysis.

Table 4 shows (weighted) summary statistics of the co-worker sample that we use to estimate equation (3).

[Table 4 about here.]

4.5 Identification issues

The event study design we leverage relies on the assumption that the timing of leave following birth is random conditional on the observables we include in our regressions. Two of the more nascent threats to identification include:

1. The time trends of the firms that contribute to treatment events are systematically different from those of the firms that comprise control events.
2. The outcomes of the firm may directly influence the timing of female childbearing.

We address these concerns in two ways. First, we can inspect whether treatment and control firms look similar in terms of their predetermined characteristics at baseline. Table 5 compares predetermined characteristics of our treatment and control firms in the baseline year. Across all variables there are only small differences between the treatment and (weighted) control sample. These differences are not statistically significant. This balance is not a mechanical result of the way the samples were constructed: none of variables in the table were targeted in the matching and reweighting procedure.

[Table 5 about here.]

Figure 1 compares the industry composition of our treatment and control firms using two-digit industry codes. The samples are well balanced.

[Figure 1 about here.]

At the individual level as opposed to the firm level, we can test whether there is covariate balance amongst the treatment and control women in Table 6. These two groups of women look similar except on the dimension of family structure. Treatment women are 20 percentage points more likely to have a partner and 12 percentage points more likely to have been with a partner for more than 2 years. However, for outcomes more related to firm outcomes (e.g., hours and leave days) the means for the treatment and control women are more similar.

[Table 6 about here.]

Second, we examine differences in pre-event year trends between treatment and control firms. The vast majority of births happening in the event year will reflect fertility decisions made in previous years. If these fertility decisions are driven by shocks at the firm level, our treatment firms should exhibit systematic changes or spikes/dips in outcomes such as firm output or wages in the years leading up to the event year. Thus, when discussing our dynamic difference-in-difference results later, we present estimates of the pre-treatment effects, which are in most cases negligible.

5 Results

5.1 First Stage

We begin by establishing that firms in our treatment and control groups diverge in terms of number of pregnancies, births and days of parental leave (i.e. that a treatment occurred). Figure 2a plots OLS estimates of the difference in the number of pregnancies between treatment and control firms—that is the β_{ks} following equation 1—as a function of distance to the event year. We see a sharp increase in pregnancies both in the event year and the preceding time period. Treated firms experience around 0.68 additional pregnancies than firms associated with control events. The significant treatment effect in the year before the event suggests that births happening in the event year typically involve pregnancies spanning also the previous year. It also possibly indicates that firms might anticipate leave-taking and make adjustments prior to childbirth to mitigate their impacts.

Next, we examine whether the number of births at the firm follows a pattern that is consistent with the pregnancies result. As predicted, Figure 2b plots the corresponding OLS estimates and reveals a significant difference in births between treatment and control firms but only in the event year. Compared to the control group, treated firms experience 0.65 extra births. As previously discussed, the gap in births is less than one since some women leave their baseline firms prior to the event year. No significant differences are apparent in other time periods, suggesting that our treatment and control groups exhibit a similar trend in number of births prior to the event year, a test of validity of our design.²⁸

Lastly, we examine whether these new births induce a change in the difference in leave take-up between treated and control firms. Figure 2c shows that treated firms see a significant increase in total number of parental leave days both in the event year and in the following

²⁸There is a marginally significant yet small difference of -0.036 births ($p = 0.09$) three years prior to the event year. Nonetheless, we cannot jointly reject that the number of births is the same across the treatment and control groups in the two years prior to baseline ($p = 0.16$) or in all years other than the event year ($p = 0.46$).

year. The magnitudes of these OLS estimates are on the order of 142 and 77 extra days of leave for the event year and the year after respectively. These estimates should be interpreted as ITT effects and understate the actual number of leave days that a firm experiences after an employee gives birth. To get a LATE estimate, we use treatment status as an instrument for actual births. We report the corresponding 2SLS estimates in Table 7. Accordingly, we estimate that relative to the control group, treated firms experience 211 and 114 additional parental leave days in the event year and the year after respectively. These estimates are consistent with aggregate statistics indicating that the average woman in Denmark takes a little over 10 months of leave following childbirth.

[Figure 2 about here.]

[Table 7 about here.]

5.2 Labor Supply Adjustments

Having established that treated firms experience more leave-taking, we next analyze the impact of parental leave on firms' labor supply. As the leave take-up results in a shortage of labor supply, firms might make adjustments both at the extensive and intensive margins. First, firms might respond by hiring additional workers to replace women on leave. Figure 3a shows the OLS estimates of the difference in the number of new hires between treatment and control firms around the event year. The difference rises in the event year and drops back again afterwards. The 2SLS estimate of this increase, reported in column 1 of Table 8, is on the order of 0.34 additional employees. This result is consistent with firms hiring temporary workers to mitigate labor supply losses from parental leave.

Second, a worker's prolonged absence can make other existing employees more valuable prompting firms to increase their retention rates (Jäger, 2016). To test this hypothesis, we start by looking at differences in employee turnover between treatment and control firms in Figure 3b. Existing employees' turnover drops by 0.36 in the event year (column 2 of Table 8) but sharply increases by 0.35 in the following year. The latter finding captures the fact that temporary workers are leaving the firm. Meanwhile, the initial drop in turnover could potentially indicate that coworkers are more likely to be retained. We further examine this by focusing on coworkers' likelihood of staying at the baseline firm and the share of their year spent unemployed. Figures 3c and 3d do not show any discernible effects in any of the time periods and the 2SLS estimates in columns 3 and 4 of Table 8 are small and not statistically or economically significant. In the event year, we can reject changes that are larger than 1.9

percentage points for the likelihood of coworkers staying at the firm and 0.05 percentage points for the share of the year spent unemployed. Hence, coworkers' retention rates do not increase and with no drop in turnover for coworkers, we can interpret the overall drop in turnover to the woman on leave being less likely to change jobs while on leave.

We then look at whether these observed extensive margin adjustments raise the growth of the number of employees at the firm. For all outcomes involving growth rates, we avoid using log transformations since parental leave might affect the probability of firms shutting down—and having zero workers. For firms with a strictly positive outcome in the baseline year, growth is measured relative to its baseline. Figure 3e reveals a significant increase in the growth of the number of employees in the event year that dissipates in the following time periods. The 2SLS estimates in columns 5 and 6 of Table 8 further show that when one employee goes on leave, the total number of workers rises by 8.4 percentage points or around 0.5 individuals. We note that the employment variable includes employees who are on leave. These results are reflective of the fact that firms are increasing the retention of leave-takers and hiring temporary workers.

As a third method of adjustment, firms can compensate for labor supply losses through intensive margin adjustments. Specifically, firms might increase work hours among coworkers of women who take parental leave. Figure 4a presents OLS estimates of the impact of leave on coworkers' hours of work. We detect a small and statistically significant increase in the event year. The corresponding 2SLS estimate in column 1 of Table 9 shows that each coworker raises his/her hours by 0.007 FTEs in the year an employee goes on leave. Since the average number of coworkers is 11.8, this constitutes a small increase of approximately 0.085 FTEs at the average firm.

Fourth and finally, we examine how a woman's parental leave-taking affects a firm's total hours of work. As firms hire temporary workers and raise work hours among existing employees, we should not expect to see a sizable decrease in overall work hours. Consistent with this, Figure 4b reveals no significant treatment effects on firm-level hours of work. Based on the 2SLS estimates in column 2 of Table 9, we can reject that hours drop by more than 0.41 FTEs or 4 percent in column 3.

[Figure 3 about here.]

[Table 8 about here.]

[Figure 4 about here.]

[Table 9 about here.]

5.3 Costs of Labor Adjustments

We next examine whether these labor supply adjustments in terms of hiring temporary workers and increasing work hours among existing employees lead to additional costs among treated firms. We first consider whether treated firms experience increases in their wage bill. As previously discussed, this can occur if firms finance parental leave payments and/or make costly labor supply adjustments to replace a woman on leave such as paying for overtime work or additional wages for temporary workers. We show estimates for the total wage bill both including and excluding wages paid to women on leave.

Wages including leave payments significantly increase in the event year and slowly return to their initial levels (Figure 5a). The 2SLS estimates in columns 1 and 2 of Table 10 indicate that the firm’s total wage bill rises by 65,920 DKK (8,847 euros) or 3.7 percent in the year an employee goes on leave. This likely reflects that firms have to pay for parental leave as well as additional wages of temporary hires and employees who increase their work hours. While we do not have information on wages of temporary workers, we present OLS estimates of the treatment effect on coworkers’ earnings in Figure 5b. Consistent with the rise in work hours, we see a small increase in earnings in the event year in the order of around 3,069 DKK or 412 euros (2SLS estimate in column 3 of Table 10).

In Figure 5c, the wage bill excluding leave payments exhibits a completely different pattern. In the event year, we see a significant drop of approximately 60,000 DKK (or 8,052 euros) or 3.0 percent (columns 4 and 5 of Table 10). Given the increase in earnings among coworkers, this suggests that treated firms are paying temporary hires lower wages than women who go on leave. This results in an overall lower wage bill despite the small increase in earnings among existing employees.

[Figure 5 about here.]

[Table 10 about here.]

Although treated firms do not pay higher wages when replacing women on leave, they might still incur more indirect costs through productivity losses. Unfortunately, as is typical, we do not have good measures of productivity. As the next best alternative to characterizing the replacement worker, we look for changes in workforce characteristics. Figure 6a examines the share of women in the firm’s workforce.²⁹ The figure along with the 2SLS estimate reported

²⁹As 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 where 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—that is having zero employees or zero work hours.

in column 1 of Table 11 reveal statistically significant decreases in the share of women at the firm in the event year and the following year. This is because the employee going on leave is female and she is being replaced by either a male or female temporary worker. This indicates that the share of women in the workforce decreases. However, by event year 2, women are more represented in firms than they were at baseline.

We examine the impact on other characteristics of the workforce such as average age, work experience, and years of education. Figure 6b shows that average age at the firm increases in the year prior to and through the year after an employee goes on leave. The 2SLS estimate in column 2 of Table 11 indicates that average age peaks one year after the event, with a 1 percent increase relative to the baseline year (column 3). This result suggests that women who go on leave are younger than the average temporary worker who replaces them. The results for average years of experience are similar to those for average age at the firm (Figure 6c and column 4 of Table 11). In all time periods surrounding the leave, we see a small positive effect of approximately 0.09 years relative to the baseline. On the other hand, firm-level average years of schooling drop in the event year (Figure 6d) by 0.37 percent or 0.04 years relative to the baseline (columns 5 and 6 of Table 11). These results are consistent with older workers typically having more years of experience but less years of schooling. Our findings suggest that the characteristics of the firm's workforce are not substantially changed when having a woman going on leave and any changes are quite temporary.

[Figure 6 about here.]

[Table 11 about here.]

5.4 Firm Performance

An advantage of our data is that it allows to observe a range of firm-level outcomes. We can thus examine whether leave taking affects overall firm performance. Figure 7a shows the OLS estimates of the impact on output measured by firm sales. Despite a slight drop in the event year, the estimates are not statistically significant. The corresponding 2SLS estimate in column 2 of Table 12 suggests that sales decrease by 2.3 percent relative to baseline and we rule out decreases that are larger than 5.3 percent. In Figures 7b and 7c, we plot the estimated effects on gross profits with and without paid leave, respectively. Similar to output, we do not observe any clear impact in various time periods. For both measures, the 2SLS estimates in columns 3 and 4 of Table 12 allow us to rule out drops that are larger than 497,000 or 622,000 DKK (66,704 or 83,480 euros) in the year the employee goes on leave.

Finally, we look at the impact of leave on the likelihood of firm survival. In Figure 7d, we proxy survival by the probability that a firm has positive sales. No noticeable effects are apparent and the 2SLS estimates in column 5 of Table 12 allow us to rule out that parental leave increases the likelihood of firm shutdown by more than 0.9 percentage points in the event year.³⁰ In sum, we find no compelling evidence that parental leave affects overall firm performance.

[Figure 7 about here.]

[Table 12 about here.]

6 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, workers and firms from Denmark— a country with generous maternity leave benefits. Our main identification strategy relies on matching small firms where a female employee is about to give birth to observationally equivalent firms with a female employee who is not. 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. Additionally, existing workers see marginal increases in their hours of work and earnings. Firms’ total wage costs increase in response to the leave, however this is completely driven by maternity leave payments which employers can eventually get reimbursements. Finally, we cannot find any evidence of leave take-up affecting firms’ output, gross profit, closure as well as existing employees’ unemployment rates. The effects that do exist are short-lived as best we can measure.

Overall, we find no support for the claim that maternity leaves can be detrimental for co-workers or firms. This suggests that generous parental leave policies can have limited harm. It also alleviates concerns that young women’s employment options are negatively affected by the expectation that they will eventually go on maternity leave.

While we believe that our research design is compelling, there are some limitations to our work. First, our estimates abstract from the general equilibrium effects of leave since the

³⁰We obtain similar results when we use other measures of firm survival such as positive hours, employment, or a positive wage bill.

control firms exist in an environment of parental leave. Such general equilibrium effects are potentially important as overall, firms may react to implementation or extension of parental leave benefits by altering their labor and capital mix or hiring fewer women —two effects our design would not pick up to the extent both treated and control firms are equally affected. Second, while our matching procedure delivers balanced treatment and control groups it does so at the expense of reducing the match rate. As such, while our results may not be generalizable to all firms, we do believe that they are directly applicable to firms whose female employees are at risk of going leave, which is likely the most relevant counterfactual. Finally, we focus on small firms and are unable to say what the effects on large firms may be. However, the possible inability of small firms to absorb the costs of parental leave is the focus of much of the popular rhetoric. Thus, small firms are a natural focus for such analyses.

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Appendices

A Effects by occupation and firm size

This section presents some additional results, checking whether we see heterogeneous effects of parental leave in terms of firm size or the occupation of the woman going on leave. Regarding firm size, for each firm in each treatment and control event, we construct an indicator for whether the firm is a small firm, based on whether the firm has 10 or fewer employees in the baseline year. This splits both the treatment and control sample roughly in half.

The coverage for occupation data in the Danish administrative data is unfortunately somewhat spotty, especially among the small firms we consider. Based on the data that is available and imputation based on other variables, however, we can assign all workers in our data a 2 digit occupation code. Based on this, for each event we define an indicator for whether the treatment or control women is in a high-skill occupation, which we define as a 2 digit occupation

that has a mean wage above the median. This allows us to check whether effects are different when the women going on leave comes from a highly-paid and highly-skilled occupation.

To check for heterogeneous effects in the two dimensions we consider, we estimate versions of our main OLS and 2SLS specifications where all the included variables are interacted with either the small firm dummy or the high skill occupation dummy. We can then check for heterogenous effects by looking at the coefficient on the interactions term between treatment, event time and either the small firm or high skill occupation dummy.

Tables 13 to 16 show the results for all the main firm variables. We see little evidence that the effect of leave-taking varies systematically across firms. There are some indications that small firms experience more leave days after a birth and also see bigger employment effects but these are primarily significant in the year just prior to the event year and the years after. At the same time we note that standard errors are also large enough that confidence intervals allow for substantial differences in effects across groups. In sum, our sample sizes hamper our investigation of the potential heterogeneity. As data beyond 2013 continues to amass, we could expand our sample to study the heterogeneity more extensively

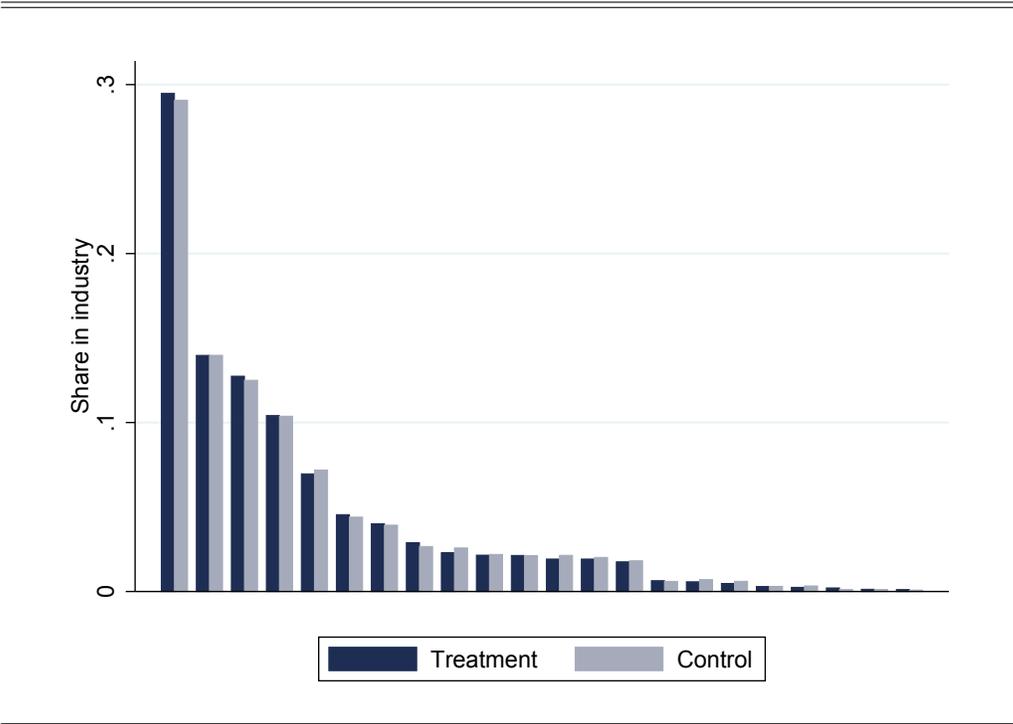
[Table 13 about here.]

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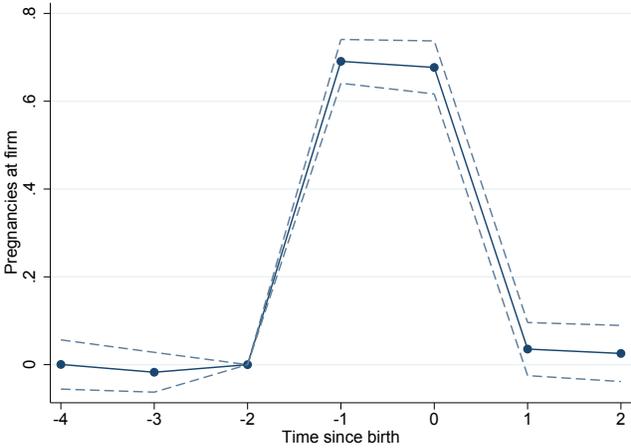
[Table 16 about here.]

Figure 1: Industry composition of treatment and control samples, two digit industries

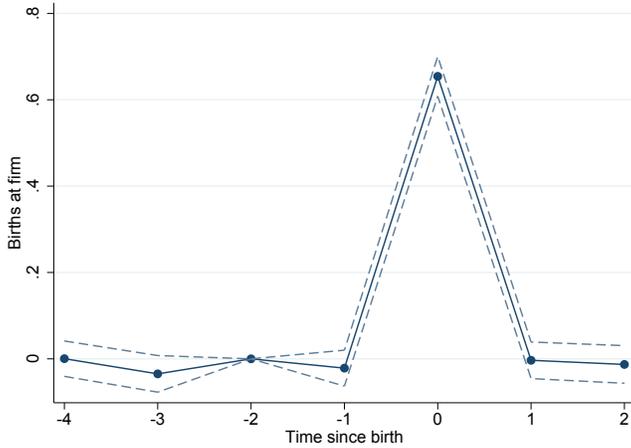


The figure shows the industrial composition of the matched and reweighted treatment and control sample across two digit industries. Industries with less than five firms in either sample are omitted due to confidentiality restrictions. Industries in the figure are ordered according to the number of firms in the treatment group.

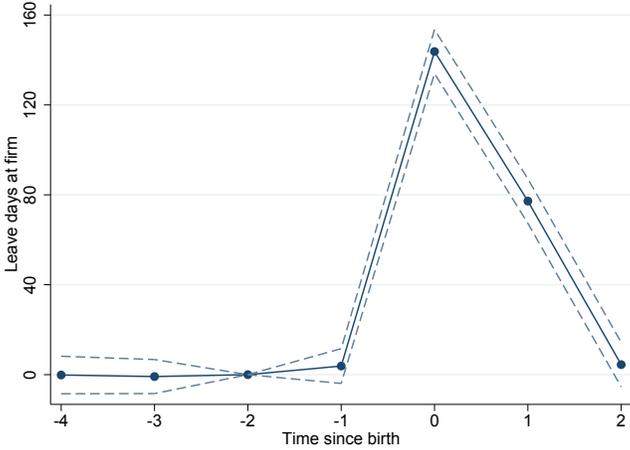
Figure 2: Effect on pregnancies, births and parental leave days



(a) Pregnancies at firm



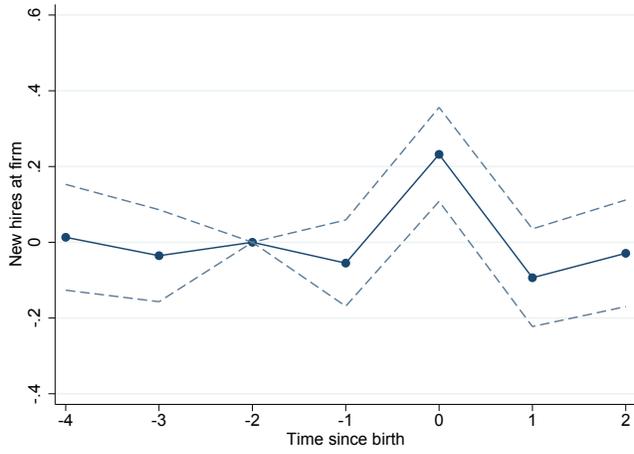
(b) Births at firm



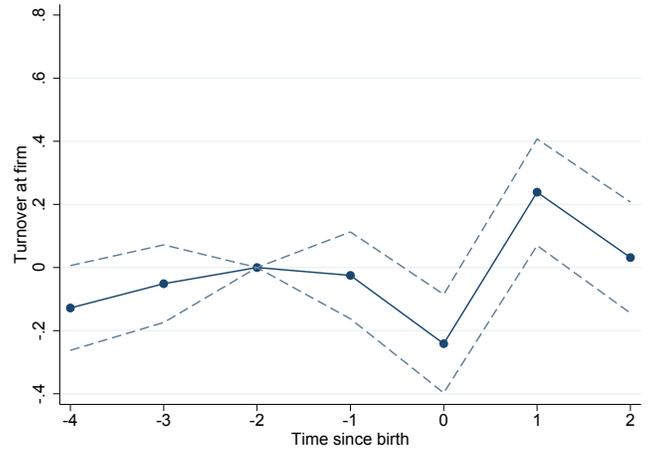
(c) Leave days at firm

Notes: The dots and solid line shows 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 line shows the 95% confidence interval based on standard errors clustered at the firm level.

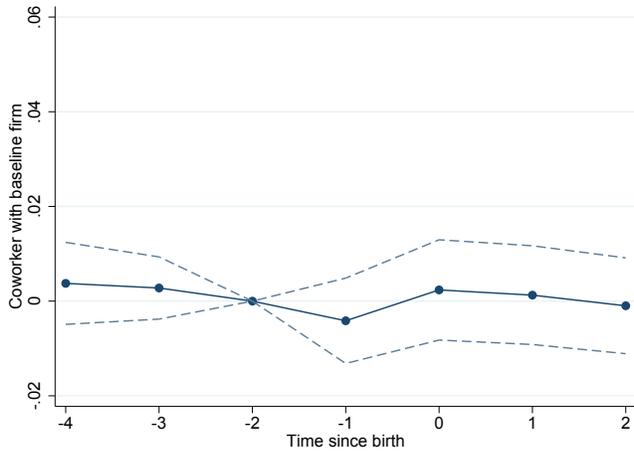
Figure 3: Effect on employment outcomes



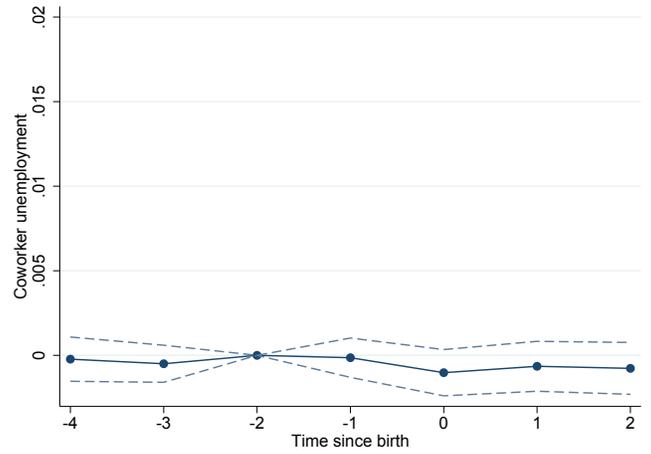
(a) New hires at firm



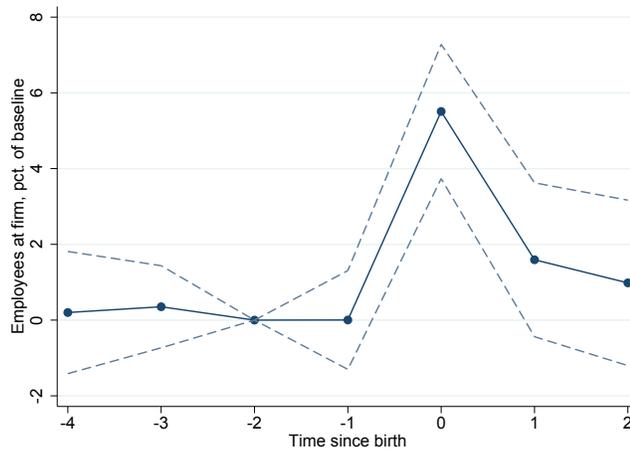
(b) Turnover at firm



(c) Likelihood of coworker staying at firm



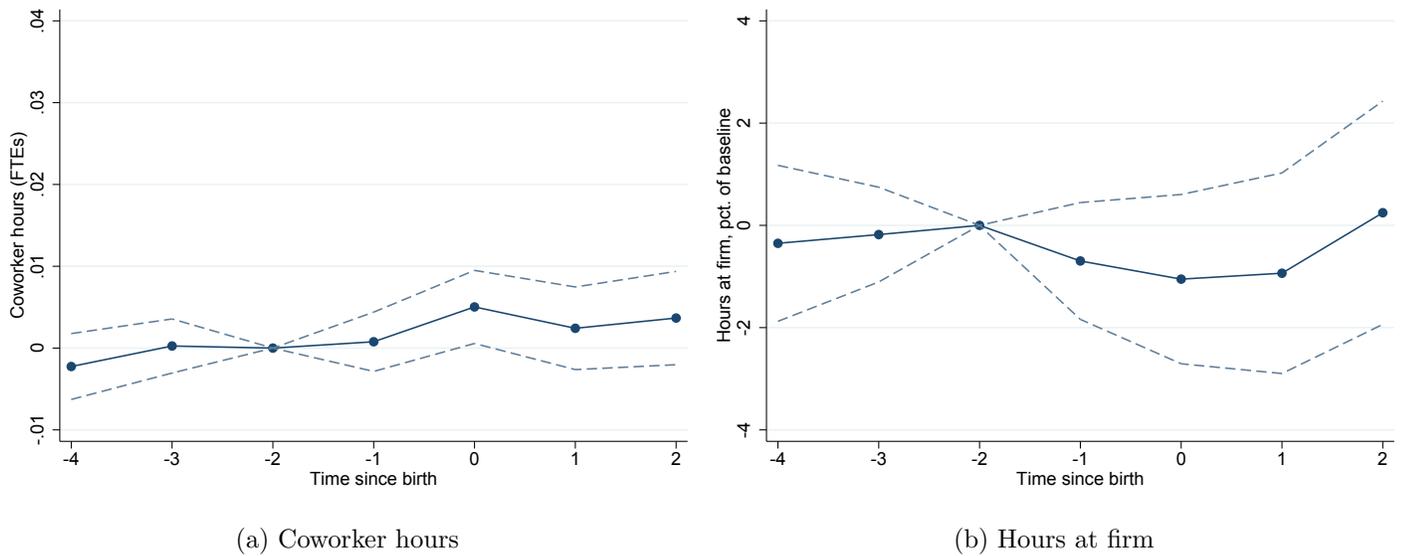
(d) Coworker unemployment



(e) Firm employment

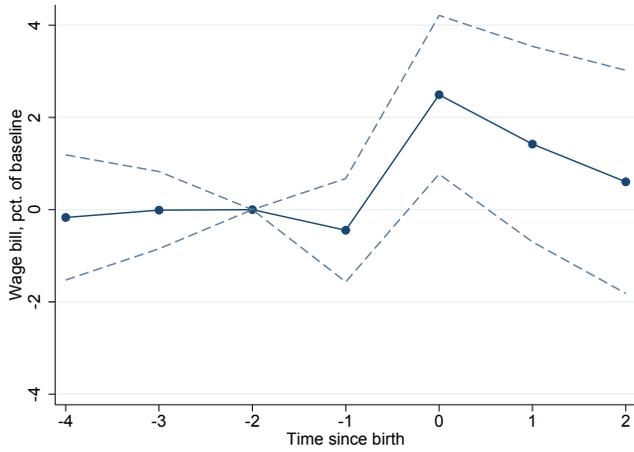
Notes: The dots and solid line shows the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year implying that the difference is identically zero here. The dashed line shows the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

Figure 4: Effect on hours of work

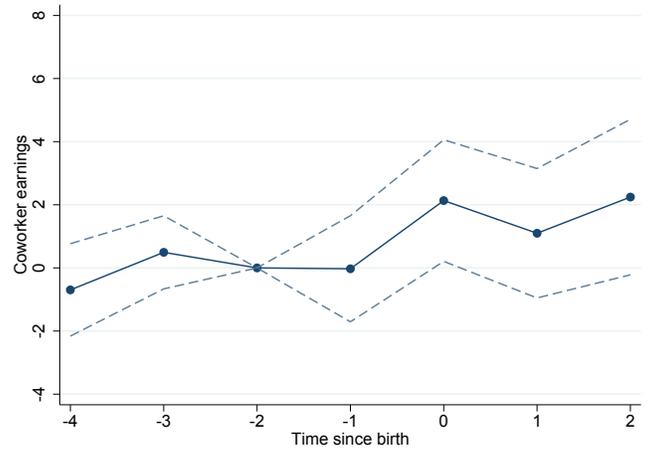


Notes: The dots and solid line shows the estimated difference between the treatment and control firms from four years prior to the event year until two years after. The baseline year is two years prior to the event year implying that the difference is identically zero here. The dashed line shows the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

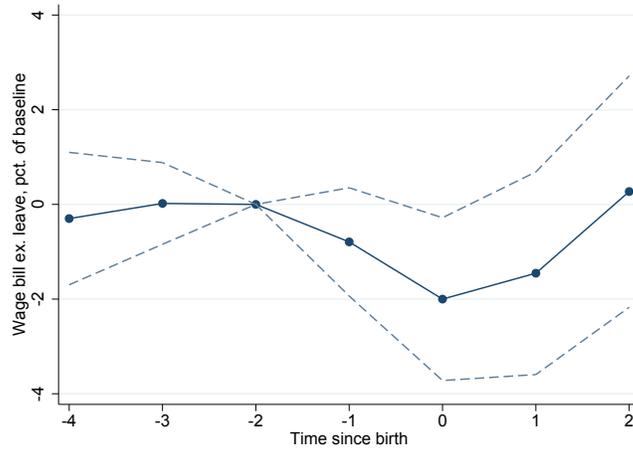
Figure 5: Effect on wages



(a) Firm wage bill



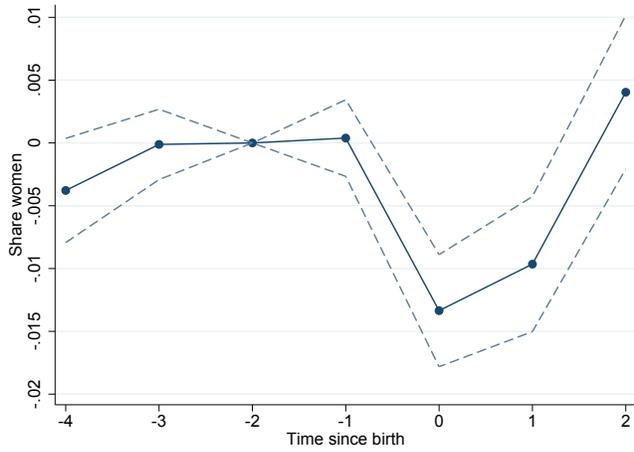
(b) Coworker earnings



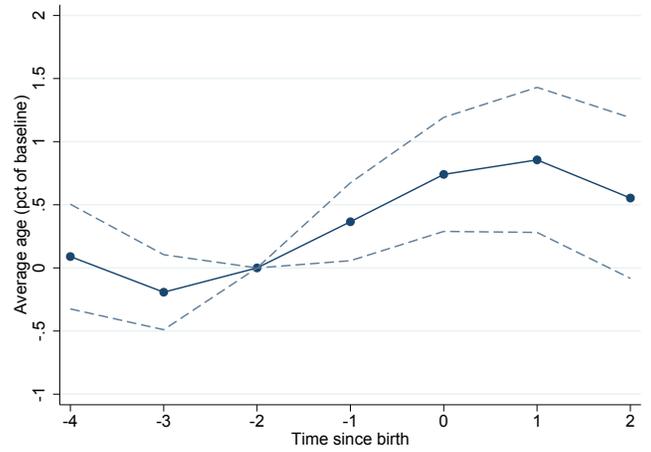
(c) Firm wage bill ex. paid leave

Notes: The dots and solid line shows 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 line shows the 95% confidence interval based on standard errors clustered at the firm level.

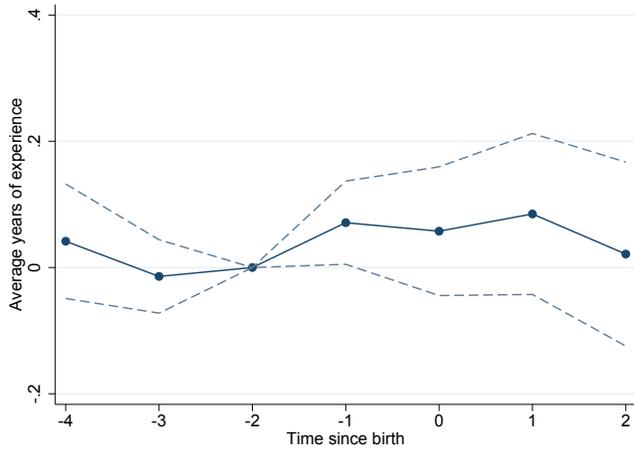
Figure 6: Effect on workforce characteristics



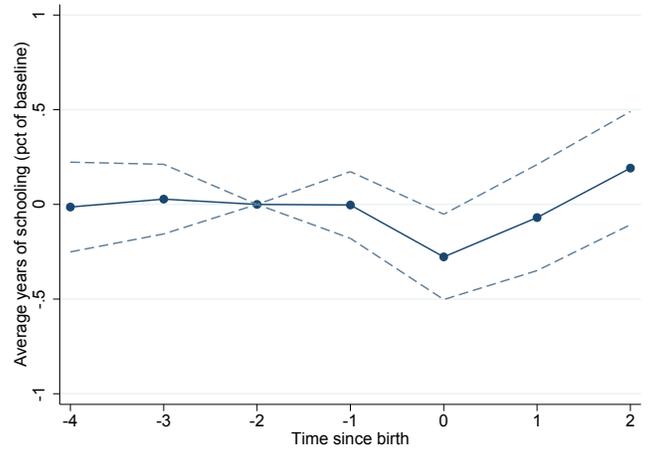
(a) Women share of workforce



(b) Workforce average age



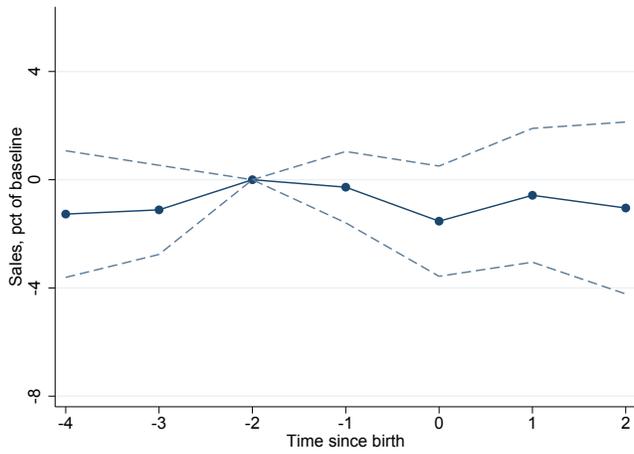
(c) Workforce average years of experience



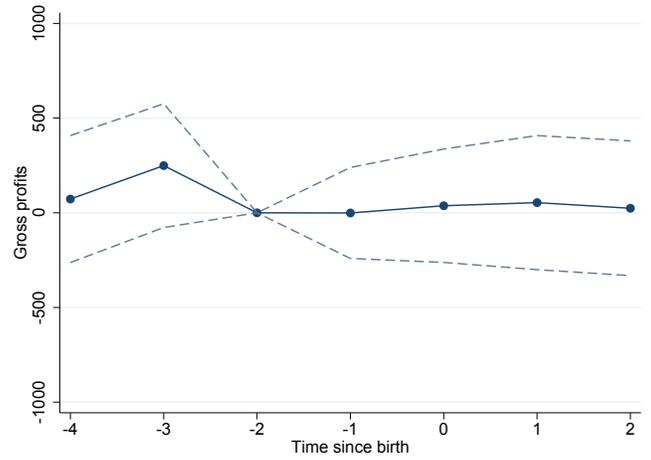
(d) Workforce average years of schooling

Notes: The dots and solid line shows 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 line shows the 95% confidence interval based on standard errors clustered at the firm level.

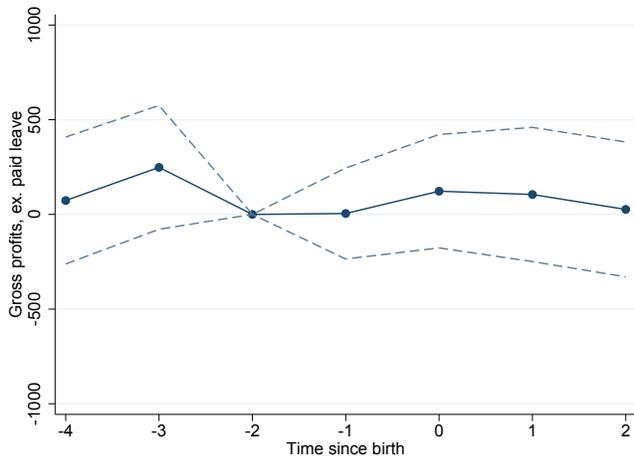
Figure 7: Effect on overall firm performance



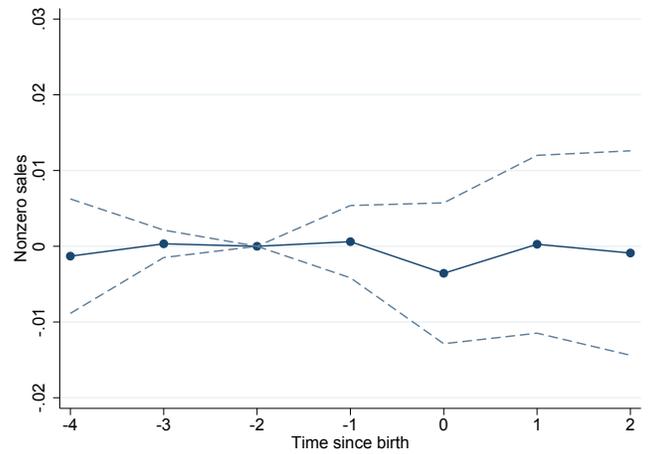
(a) Firm output



(b) Firm gross profits



(c) Firm gross profits ex. paid leave



(d) Firm survival

Notes: The dots and solid line shows 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 line shows the 95% confidence interval based on standard errors clustered at the firm level.

Table 1: Sample selection

	Treatment events	Control events	Total unique firms
Baseline sample:	140,074	792,561	61,117
Restricted to small firms:	22,438	139,735	44,014
Restricted to private firms:	19,911	125,136	40,716
Excluding sale and wage bill outliers:	17,137	110,464	36,430
Excluding extreme growth/decline firms:	16,566	106,896	35,287
Applying trimming:	5,995	10,571	10,560
After matching/reweighing:	5,995	5,995	10,560

The table illustrates the selection of the final sample of matched treatment and control events

Table 2: Comparing matched and unmatched treatment events

	Matched treatment events	Unmatched treatment events
Woman age	26.63 (3.46)	28.26 (3.15)
Woman total children	0.33 (0.63)	0.90 (0.73)
Woman children 0 to 2 years	0.16 (0.38)	0.60 (0.54)
Woman years of schooling	12.11 (1.74)	12.49 (2.26)
Woman tenure ge 2	0.63 (0.48)	0.60 (0.49)
Woman earnings	240.52 (99.39)	227.49 (107.60)
Firm sales	18252.22 (42673.94)	17270.66 (36379.03)
Firm employees	12.83 (8.05)	12.75 (7.37)
Firm share women employees	0.67 (0.27)	0.61 (0.25)
Firm children per employee	0.87 (0.51)	1.06 (0.52)
Event year	2007.73 (2.01)	2007.86 (2.00)

The table shows means and standard deviations for the sample of treatment events that we successfully match to at least one control events and for the sample of treatment events that we do not match to any control events. Standard deviations are in parentheses.

Table 3: Summary stats, firm sample

	Observations (unweighted)	Mean	Standard Deviation
Panel A - Baseline year			
Births at firm	20,664	0.767	1.047
Pregnancies at firm	20,664	1.375	1.552
Leave days at firm	20,664	161.5	227.2
Employees	20,664	12.81	8.003
New hires	20,664	3.744	3.287
Turnover at firm	20,664	3.706	4.180
Wage bill (1000 DKKs)	20,664	3,326	3,024
Sales (1000 DKKs)	20,664	18,316	40,546
Purchases (1000 DKKs)	20,664	12,465	34,353
Gross profits (1000 DKKs)	20,664	2,525	16,068
Gross profits ex. leave (1000 DKKs)	20,664	2,601	16,078
Workforce share women	20,664	0.659	0.279
Workforce avg. age	20,664	33.60	6.630
Workforce avg. years schooling	20,664	11.59	1.311
Workforce avg. years experience	20,664	12.07	5.473
Panel B - All seven years			
Births at firm	144,648	0.708	1.061
Pregnancies at firm	144,648	1.322	1.646
Leave days at firm	144,648	147.9	227.4
Employees	144,648	11.55	9.638
New hires	144,648	3.614	4.084
Turnover at firm	144,648	3.785	4.457
Wage bill (1000 DKKs)	144,648	3,138	3,562
Sales (1000 DKKs)	144,648	17,458	41,746
Purchases (1000 DKKs)	144,648	11,918	34,918
Gross profits (1000 DKKs)	144,648	2,402	15,966
Gross profits ex. leave (1000 DKKs)	144,648	2,479	15,979
Workforce share women	130,558	0.641	0.291
Workforce avg. age	130,558	34.34	7.120
Workforce avg. years schooling	130,558	11.61	1.372
Workforce avg. years experience	130,558	12.65	5.841

The table shows summary statistics for the matched firm sample used in the analysis. Panel A shows summary statistics for the baseline year only, while Panel B shows summary statistics 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 observation number shown is unweighted.

Table 4: Summary stats, coworker sample

	Observations (unweighted)	Mean	Standard Deviation
Panel A - Baseline Year			
Coworker unemployment (yearly share)	269,512	0.0269	0.105
Coworker hours (FTEs)	269,512	0.623	0.405
Coworker earnings (1000 DKKs)	269,512	202.9	196.8
Panel B - All seven years			
Coworker still with baseline firm	1,886,584	0.578	0.494
Coworker unemployment (yearly share)	1,886,584	0.0272	0.107
Coworker hours (FTEs)	1,886,584	0.597	0.416
Coworker earnings (1000 DKKs)	1,886,584	205.5	213.3

The table shows summary statistics for the matched coworker sample used in the analysis. Panel A shows summary statistics for the baseline year only, while Panel B shows summary statistics 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 observation number shown is unweighted.

Table 5: Covariate balance at baseline, firm and event-specific variables

	Treatment	Control	Difference
Births at firm	0.086 (0.281)	0.086 (0.281)	0.000 (0.005)
Leave days at firm	29.806 (83.733)	27.959 (79.796)	1.847 (1.564)
New hires	3.743 (3.297)	3.745 (3.277)	-0.002 (0.056)
Hours (FTEs)	0.881 (0.237)	0.879 (0.240)	0.003 (0.004)
Workforce avg. years schooling	11.616 (1.305)	11.610 (1.303)	0.005 (0.023)
Workforce avg. age	33.469 (6.480)	33.541 (6.523)	-0.071 (0.114)
Workforce avg. experience	11.978 (5.372)	12.050 (5.369)	-0.072 (0.094)
Wage bill (1000 DKKs)	3315.347 (2998.568)	3336.899 (3049.771)	-21.552 (51.893)
Purchases (1000 DKKs)	12584.320 (37838.174)	12345.768 (30475.294)	238.552 (623.749)
Profits (1000 DKKs)	12136.710 (32970.613)	12536.329 (33892.890)	-399.619 (606.653)
Event year	2007.734 (2.005)	2007.744 (2.013)	-0.009 (0.035)

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 using clustering at the firm level.
 ** p<0.01, * p<0.05

Table 6: Covariate balance at baseline, treatment/control woman variables

	Treatment	Control	Difference
Woman hours (FTE)	0.829 (0.297)	0.826 (0.299)	0.003 (0.005)
Woman leave days	29.806 (83.733)	27.959 (79.796)	1.847 (1.564)
Of Danish origin	0.953 (0.212)	(0.952 (0.214)	0.001 (0.004)
Experience	7.286 (3.705)	7.169 (3.792)	0.117 (0.064)
Has partner	0.712 (0.453)	0.518 (0.500)	0.195** (0.008)
With partner more than 2 years	0.454 (0.498)	0.337 (0.473)	0.117** (0.009)

The table shows means and standard deviations for the variables specific to the treatment/control woman 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 using clustering at the firm level. ** p<0.01, * p<0.05

Table 7: Validating treatment, 2SLS results

	Leave days at firm	Pregnancies at firm
Births at 0 X Time -4	-0.229 (6.282)	0.000677 (0.0422)
Births at 0 X Time -3	-1.246 (5.695)	-0.0255 (0.0340)
Births at 0 X Time -1	5.648 (5.798)	1.019** (0.0381)
Births at 0 X Time 0	212.0** (7.142)	0.998** (0.0385)
Births at 0 X Time 1	113.9** (7.193)	0.0524 (0.0453)
Births at 0 X Time 2	6.584 (7.427)	0.0377 (0.0480)
Time fixed effects	Yes	Yes
Observations	126,861	126,861
Obs. (weighted)	83,930	83,930
Clusters (firms)	10,560	10,560
F-stat	1190	1190

The table shows shows 2SLS regression results estimated on the matched and reweighted sample. The interaction terms between births at event time and the time dummies are instrumented by interactions between treatment status and time dummies. The F-statistic reported at the bottom of the table correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. Because the endogenous variables are time dummies interacted with the same time-invariant variable, because the instruments are the same set of time dummies interacted with another time-invariant variable and because there is the same number of observations in each year, the F-statistic is numerically the same for all the instruments. Standard errors (in parentheses) are clustered at the firm level. ** $p < 0.01$, * $p < 0.05$

Table 8: Effect on employment outcomes, 2SLS results

	New hires at firm	Turnover at firm	Coworker with baseline firm	Coworker unemployed	Employees at firm	Employees at firm (pct. of baseline)
Births in event year X Time -4	0.0194 (0.105)	-0.189 (0.101)	0.00540 (0.00635)	-0.000325 (0.000960)	0.0701 (0.147)	0.295 (1.214)
Births in event year X Time -3	-0.0522 (0.0913)	-0.0755 (0.0926)	0.00399 (0.00482)	-0.000716 (0.000806)	0.0467 (0.0895)	0.519 (0.816)
Births in event year X Time -1	-0.0811 (0.0860)	-0.0367 (0.103)	-0.00597 (0.00670)	-0.000201 (0.000852)	-0.0911 (0.126)	0.00440 (0.981)
Births in event year X Time 0	0.342** (0.0915)	-0.356** (0.118)	0.00341 (0.00772)	-0.00147 (0.00100)	0.560** (0.169)	8.122** (1.265)
Births in event year X Time 1	-0.138 (0.0976)	0.352** (0.125)	0.00182 (0.00763)	-0.000932 (0.00108)	0.0236 (0.200)	2.349 (1.510)
Births in event year X Time 2	-0.0429 (0.106)	0.0466 (0.132)	-0.00141 (0.00744)	-0.00111 (0.00113)	-0.113 (0.227)	1.449 (1.632)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	126,861	126,861	1,694,105	1,694,105	126,861	126,861
Obs. (weighted)	83,930	83,930	1.120e+06	1.120e+06	83,930	83,930
Clusters (firms)	10,560	10,560	10,555	10,555	10,560	10,560
F-stat	1190	1190	593.8	593.8	1190	1190

The table shows 2SLS regression results estimated on the matched and reweighted sample. The interaction terms between births at event time and the time dummies are instrumented by interactions between treatment status and time dummies. The F-statistic reported at the bottom of the table correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. Because the endogenous variables are time dummies interacted with the same time-invariant variable, because the instruments are the same set of time dummies interacted with another time-invariant variable and because there is the same number of observations in each year, the F-statistic is numerically the same for all the instruments. Standard errors (in parentheses) are clustered at the firm level. ** p<0.01, * p<0.05

Table 9: Effect on hours, 2SLS results

	Coworker hours (FTEs)	Hours at firm (FTEs)	Hours at firm (pct of baseline)
Births in event year X Time -4	-0.00324 (0.00295)	-0.0572 (0.115)	-0.518 (1.147)
Births in event year X Time -3	0.000367 (0.00243)	0.0119 (0.0635)	-0.265 (0.696)
Births in event year X Time -1	0.00111 (0.00265)	-0.126 (0.0917)	-1.027 (0.868)
Births in event year X Time 0	0.00724* (0.00330)	-0.140 (0.137)	-1.550 (1.261)
Births in event year X Time 1	0.00348 (0.00372)	-0.173 (0.162)	-1.380 (1.487)
Births in event year X Time 2	0.00528 (0.00420)	-0.135 (0.183)	0.364 (1.640)
Time fixed effects	Yes	Yes	Yes
Observations	1,694,105	126,861	126,861
Obs. (weighted)	1.120e+06	83,930	83,930
Clusters (firms)	10,555	10,560	10,560
F-stat	593.8	1190	1190

The table shows 2SLS regression results estimated on the matched and reweighted sample. The interaction terms between births at event time and the time dummies are instrumented by interactions between treatment status and time dummies. The F-statistic reported at the bottom of the table correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. Because the endogenous variables are time dummies interacted with the same time-invariant variable, because the instruments are the same set of time dummies interacted with another time-invariant variable and because there is the same number of observations in each year, the F-statistic is numerically the same for all the instruments (see footnote 25). Standard errors (in parentheses) are clustered at the firm level. ** $p < 0.01$, * $p < 0.05$

Table 10: Effect on wages, 2SLS results

	Firm wage bill (1000 DKKs)	Firm wage bill (pct of baseline)	Coworker earnings (1000 DKKs)	Firm wage bill ex. leave (1000 DKKs)	Firm wage bill ex. leave (pct of baseline)
Births in event year X Time -4	-12.84 (40.81)	-0.249 (1.020)	-1.002 (1.069)	-14.28 (40.40)	-0.442 (1.052)
Births in event year X Time -3	6.128 (22.18)	-0.0150 (0.629)	0.711 (0.852)	7.230 (22.00)	0.0301 (0.649)
Births in event year X Time -1	-26.60 (33.67)	-0.657 (0.848)	-0.0397 (1.234)	-35.12 (33.17)	-1.168 (0.870)
Births in event year X Time 0	65.92 (51.10)	3.674** (1.258)	3.069* (1.398)	-59.74 (51.03)	-2.950* (1.325)
Births in event year X Time 1	31.42 (62.94)	2.096 (1.577)	1.580 (1.499)	-44.57 (62.03)	-2.144 (1.628)
Births in event year X Time 2	-16.70 (73.80)	0.890 (1.813)	3.230 (1.795)	-20.50 (72.00)	0.400 (1.836)
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	126,861	126,861	1,694,105	126,861	126,861
Obs. (weighted)	83,930	83,930	1.120e+06	83,930	83,930
Clusters (firms)	10,560	10,560	10,555	10,560	10,560
F-stat	1190	1190	593.8	1190	1190

The table shows 2SLS regression results estimated on the matched and reweighted sample. The interaction terms between births at event time and the time dummies are instrumented by interactions between treatment status and time dummies. The F-statistic reported at the bottom of the table correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. Because the endogenous variables are time dummies interacted with the same time-invariant variable, because the instruments are the same set of time dummies interacted with another time-invariant variable and because there is the same number of observations in each year, the F-statistic is numerically the same for all the instruments (see footnote 25). Standard errors (in parentheses) are clustered at the firm level. ** p<0.01, * p<0.05

Table 11: Effect on workforce composition, 2SLS results

	Workforce share women	Workforce avg. age (years)	Workforce avg. age (pct. of baseline)	Workforce avg. years experience	Workforce avg. years school. (pct of baseline)
Births in event year X Time -4	-0.00444 (0.00292)	0.0101 (0.0871)	0.0809 (0.289)	0.0405 (0.0632)	-0.00431 (0.0182)
Births in event year X Time -3	0.000135 (0.00192)	-0.0613 (0.0605)	-0.201 (0.208)	-0.0151 (0.0403)	0.00126 (0.0134)
Births in event year X Time -1	0.00107 (0.00199)	0.117* (0.0593)	0.380 (0.199)	0.0793 (0.0415)	-0.00260 (0.0127)
Births in event year X Time 0	-0.0171** (0.00301)	0.347** (0.0928)	0.943** (0.302)	0.0669 (0.0677)	-0.0405* (0.0166)
Births in event year X Time 1	-0.0119** (0.00353)	0.349** (0.116)	1.025** (0.376)	0.0907 (0.0830)	-0.0101 (0.0202)
Births in event year X Time 2	0.00468 (0.00390)	0.189 (0.126)	0.728 (0.405)	0.0212 (0.0940)	0.0224 (0.0218)
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	107,281	107,281	107,281	107,281	107,281
Obs. (weighted)	71,008	71,008	71,008	71,008	71,008
Clusters (firms)	9,283	9,283	9,283	9,283	9,283
F-stat	1159	1159	1159	1159	1159

The table shows 2SLS regression results estimated on the matched and reweighted sample. The interaction terms between births at event time and the time dummies are instrumented by interactions between treatment status and time dummies. The F-statistic reported at the bottom of the table correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. For brevity we do not report the F-stat for each of the six first stage but only report the smallest of the six F-stats. Standard errors (in parentheses) are clustered at the firm level. ** p<0.01, * p<0.05

Table 12: Effect on output and profits, 2SLS results

	Firm sales (1000 DKKs)	Firm sales (pct of baseline)	Gross profits (1000 DKKs)	Gross profits ex. leave (1000 DKKs)	Nonzero sales
Births in event year X Time -4	591.3 (476.1)	-1.872 (1.759)	106.7 (252.3)	108.2 (252.2)	-0.00192 (0.00568)
Births in event year X Time -3	645.9 (346.4)	-1.641 (1.238)	367.2 (245.9)	366.1 (245.9)	0.000481 (0.00136)
Births in event year X Time -1	-424.6 (450.7)	-0.406 (0.994)	-1.441 (180.7)	7.078 (180.7)	0.000897 (0.00359)
Births in event year X Time 0	-431.8 (740.5)	-2.259 (1.550)	54.90 (225.3)	180.6 (225.3)	-0.00526 (0.00703)
Births in event year X Time 1	-688.5 (808.1)	-0.849 (1.866)	79.12 (266.2)	155.1 (266.4)	0.000393 (0.00883)
Births in event year X Time 2	-674.9 (872.6)	-1.541 (2.399)	35.12 (267.6)	38.91 (267.9)	-0.00131 (0.0102)
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	126,861	126,861	126,861	126,861	126,861
Obs. (weighted)	83,930	83,930	83,930	83,930	83,930
Clusters (firms)	10,560	10,560	10,560	10,560	10,560
F-stat	1190	1190	1190	1190	1190

The table shows 2SLS regression results estimated on the matched and reweighted sample. The interaction terms between births at event time and the time dummies are instrumented by interactions between treatment status and time dummies. The F-statistic reported at the bottom of the table correspond to the Sanderson and Windmeijer (2016) statistic for assessing instrument strength in the face of multiple instruments and endogenous regressors. Because the endogenous variables are time dummies interacted with the same time-invariant variable, because the instruments are the same set of time dummies interacted with another time-invariant variable and because there is the same number of observations in each year, the F-statistic is numerically the same for all the instruments (see footnote 25). Standard errors (in parentheses) are clustered at the firm level. ** $p < 0.01$, * $p < 0.05$

Table 13: Heterogeneous effects by firm size, OLS results

	Leave days	Employees	Hours (FTEs) DKKs)	Wage bill (1000 (1000 DKKs)	Wage ex. leave DKKs)	Gross profits (1000 (1000 DKKs)	Gross profits ex. leave
Small firm X Treatment X Time -4	8.161 (8.253)	0.00963 (0.192)	0.0945 (0.149)	48.42 (51.70)	42.95 (51.21)	-44.27 (325.7)	-38.80 (325.6)
Small firm X Treatment X Time -3	9.897 (7.470)	0.0843 (0.117)	-0.0356 (0.0823)	12.69 (28.53)	7.741 (28.32)	-406.0 (316.1)	-401.1 (316.0)
Small firm X Treatment X Time -1	14.95 (7.657)	0.362* (0.161)	0.219 (0.117)	64.29 (43.36)	60.87 (42.63)	95.02 (235.3)	98.44 (235.3)
Small firm X Treatment X Time 0	12.00 (9.617)	0.407 (0.224)	0.202 (0.175)	49.01 (66.81)	65.66 (65.03)	-177.4 (295.4)	-194.1 (295.4)
Small firm X Treatment X Time 1	19.37* (9.789)	0.493 (0.258)	0.241 (0.207)	38.10 (81.76)	43.12 (79.80)	-125.3 (345.1)	-130.3 (345.4)
Small firm X Treatment X Time 2	26.87** (9.796)	0.664* (0.288)	0.365 (0.232)	94.86 (94.40)	83.07 (92.11)	-7.057 (344.7)	4.740 (345.0)
Treatment X Time -4	-4.888 (6.601)	0.0267 (0.176)	-0.0873 (0.139)	-35.71 (49.32)	-33.12 (48.84)	72.25 (316.2)	69.67 (316.1)
Treatment X Time -3	-6.227 (6.011)	-0.0220 (0.105)	0.0189 (0.0759)	-5.245 (26.91)	-1.641 (26.71)	430.0 (309.7)	426.3 (309.7)
Treatment X Time -1	-3.680 (6.228)	-0.256 (0.147)	-0.209 (0.108)	-52.64 (41.27)	-56.59 (40.58)	-40.27 (229.2)	-36.32 (229.2)
Treatment X Time 0	136.6** (7.719)	0.157 (0.207)	-0.214 (0.163)	13.92 (63.57)	-78.12 (61.91)	125.4 (283.6)	217.4 (283.6)
Treatment X Time 1	67.25** (7.966)	-0.260 (0.238)	-0.258 (0.193)	-4.008 (77.65)	-56.90 (75.84)	112.8 (332.7)	165.7 (332.9)
Treatment X Time 2	-8.450 (8.136)	-0.437 (0.268)	-0.295 (0.218)	-66.16 (90.17)	-62.76 (87.99)	24.30 (332.8)	20.90 (333.1)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects interacted	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,648	144,648	144,648	144,648	144,648	144,648	144,648
Obs. (weighted)	85246	85246	85246	85246	85246	85246	85246
Clusters (firms)	11653	11653	11653	11653	11653	11653	11653

The table shows OLS regression results estimated on the matched and reweighted sample. Standard errors (in parentheses) are clustered at the firm level. ** p<0.01, * p<0.05

Table 14: Heterogeneous effects by firm size, 2SLS results

	Leave days	Employees	Hours (FTEs) DKKs	Wage bill (1000 DKKs)	Wage ex. leave DKKs)	Gross profits (1000 DKKs)	Gross profits ex. leave
small firm X Births in event year X Time -4	11.98 (12.08)	0.0169 (0.277)	0.136 (0.215)	70.49 (74.34)	62.40 (73.64)	-61.02 (468.6)	-52.93 (468.4)
Small firm X Births in event year X Time -3	14.50 (10.93)	0.126 (0.170)	-0.0525 (0.119)	18.83 (41.13)	11.62 (40.85)	-579.7 (454.4)	-572.5 (454.4)
Small firm X Births in event year X Time -1	22.39* (11.21)	0.528* (0.238)	0.314 (0.173)	93.13 (63.64)	87.59 (62.68)	140.9 (338.3)	146.4 (338.3)
Small firm X Births in event year X Time 0	30.02* (12.98)	0.632* (0.318)	0.288 (0.258)	75.66 (95.65)	93.02 (95.76)	-258.7 (425.3)	-276.1 (425.3)
Small firm X Births in event year X Time 1	35.24** (13.64)	0.727 (0.377)	0.343 (0.304)	57.53 (117.8)	60.60 (116.3)	-180.6 (496.7)	-183.6 (497.0)
Small firm X Births in event year X Time 2	40.09** (14.35)	0.972* (0.423)	0.529 (0.341)	138.4 (137.2)	120.8 (133.8)	-8.624 (496.0)	9.005 (496.4)
Births in event year X Time -4	-7.004 (9.456)	0.0383 (0.252)	-0.125 (0.199)	-51.17 (70.47)	-47.46 (69.80)	103.5 (453.2)	99.82 (453.1)
Births in event year X Time -3	-8.923 (8.614)	-0.0315 (0.150)	0.0271 (0.109)	-7.516 (38.52)	-2.351 (38.26)	616.1 (444.2)	610.9 (444.1)
Births in event year X Time -1	-5.273 (8.961)	-0.367 (0.218)	-0.299 (0.160)	-75.43 (60.38)	-81.08 (59.48)	-57.70 (328.4)	-52.04 (328.3)
Births in event year X Time 0	195.8** (10.47)	0.224 (0.293)	-0.306 (0.239)	19.95 (90.71)	-111.9 (90.80)	179.6 (406.5)	311.5 (406.4)
Births in event year X Time 1	96.36** (10.87)	-0.373 (0.347)	-0.370 (0.282)	-5.743 (111.3)	-81.54 (109.9)	161.6 (476.6)	237.4 (476.9)
Births in event year X Time 2	-12.11 (11.76)	-0.626 (0.393)	-0.423 (0.318)	-94.80 (130.5)	-89.93 (127.3)	34.82 (476.9)	29.95 (477.2)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects interacted	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,648	144,648	144,648	144,648	144,648	144,648	144,648
Obs. (weighted)	85246	85246	85246	85246	85246	85246	85246
Clusters (firms)	11653	11653	11653	11653	11653	11653	11653

The table shows results from 2SLS regression on the matched and reweighted sample. Standard errors (in parentheses) are clustered at the firm level. ** p<0.01, * p<0.05

Table 15: Heterogeneous effects by woman occupation, OLS results

	Leave days	Employees	Hours (FTEs) DKKs)	Wage bill (1000 DKKs)	Wage ex. leave DKKs)	Gross profits (1000 DKKs)	Gross profits ex. leave
High skill occupation X Treatment X Time -4	2.895 (8.317)	0.101 (0.195)	0.124 (0.148)	44.55 (50.65)	42.07 (50.11)	136.8 (301.8)	139.3 (301.8)
High skill occupation X Treatment X Time -3	9.871 (7.574)	0.0207 (0.123)	0.0380 (0.0825)	26.72 (27.80)	23.21 (27.59)	526.6 (295.4)	530.1 (295.4)
High skill occupation X Treatment X Time -1	-6.082 (7.764)	0.0591 (0.164)	-0.0150 (0.117)	-9.951 (41.75)	-5.662 (41.01)	-43.56 (220.6)	-47.85 (220.6)
High skill occupation X Treatment X Time 0	9.402 (9.799)	0.147 (0.230)	0.0397 (0.175)	43.39 (64.22)	4.122 (62.50)	344.8 (287.1)	384.1 (287.1)
High skill occupation X Treatment X Time 1	9.449 (9.898)	-0.0509 (0.261)	-0.140 (0.206)	-13.30 (78.84)	-36.98 (77.03)	377.5 (344.2)	401.2 (344.3)
High skill occupation X Treatment X Time 2	2.234 (9.698)	-0.281 (0.291)	-0.222 (0.232)	-52.03 (90.99)	-55.41 (88.84)	211.9 (359.2)	215.3 (359.5)
Treatment X Time -4	-2.503 (5.922)	-0.0213 (0.132)	-0.107 (0.0897)	-32.55 (23.75)	-31.42 (23.44)	-14.68 (80.97)	-15.80 (81.00)
Treatment X Time -3	-7.031 (5.480)	0.00686 (0.0906)	-0.0196 (0.0528)	-12.75 (14.03)	-9.646 (13.91)	-56.64 (63.34)	-59.75 (63.49)
Treatment X Time -1	6.933 (5.630)	-0.113 (0.113)	-0.0919 (0.0699)	-16.64 (19.73)	-24.67 (19.27)	32.12 (68.43)	40.15 (68.62)
Treatment X Time 0	136.9**	0.274	-0.133	11.85	-49.64	-153.2	-91.70
Treatment X Time 1	(7.290)	(0.152)	(0.103)	(30.11)	(29.30)	(103.4)	(103.5)
Treatment X Time 2	71.06**	0.0165	-0.0538	20.75	-16.03	-157.8	-121.0
	(7.218)	(0.170)	(0.120)	(36.34)	(35.44)	(133.6)	(133.8)
	3.199	0.0590	0.0208	9.244	9.005	-97.38	-97.14
	(7.061)	(0.188)	(0.134)	(40.72)	(39.70)	(128.9)	(129.2)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects interacted	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,648	144,648	144,648	144,648	144,648	144,648	144,648
Obs. (weighted)	85246	85246	85246	85246	85246	85246	85246
Clusters (firms)	11653	11653	11653	11653	11653	11653	11653

The table shows OLS regression results estimated on the matched and reweighted sample. Standard errors (in parentheses) are clustered at the firm level. ** p<0.01, * p<0.05

Table 16: Heterogeneous effects by woman occupation, 2SLS results

	Leave days	Employees	Hours (FTEs) DKKs	Wage bill (1000 DKKs)	Wage ex. leave DKKs	Gross profits (1000 DKKs)	Gross profits ex. leave
High skill occupation X Births in event year X Time -4	4.587 (12.62)	0.146 (0.294)	0.196 (0.220)	69.40 (73.80)	65.67 (72.99)	196.2 (431.2)	199.9 (431.2)
High skill occupation X Births in event year X Time -3	15.34 (11.54)	0.0279 (0.188)	0.0575 (0.123)	40.28 (40.78)	34.71 (40.48)	755.3 (421.3)	760.9 (421.4)
High skill occupation X Births in event year X Time -1	-9.969 (11.78)	0.106 (0.251)	-0.00301 (0.176)	-10.76 (61.57)	-3.109 (60.61)	-67.92 (316.5)	-75.57 (316.5)
High skill occupation X Births in event year X Time 0	-13.86 (14.01)	0.153 (0.336)	0.0825 (0.264)	58.97 (92.56)	15.67 (92.59)	517.6 (415.0)	560.9 (415.1)
High skill occupation X Births in event year X Time 1	-0.737 (14.56)	-0.0752 (0.392)	-0.187 (0.308)	-22.91 (114.7)	-49.08 (113.3)	564.7 (499.3)	590.9 (499.6)
High skill occupation X Births in event year X Time 2	2.522 (14.69)	-0.409 (0.438)	-0.317 (0.346)	-75.36 (133.0)	-80.08 (129.9)	318.7 (520.6)	323.4 (521.0)
Births in event year X Time -4	-4.033 (9.543)	-0.0343 (0.212)	-0.173 (0.144)	-52.44 (37.93)	-50.63 (37.45)	-23.65 (130.4)	-25.46 (130.5)
Births in event year X Time -3	-11.33 (8.860)	0.0110 (0.146)	-0.0315 (0.0849)	-20.54 (22.49)	-15.54 (22.32)	-91.27 (102.2)	-96.27 (102.4)
Births in event year X Time -1	11.17 (9.048)	-0.182 (0.184)	-0.148 (0.114)	-26.82 (32.09)	-39.75 (31.50)	51.76 (110.3)	64.69 (110.6)
Births in event year X Time 0	220.7**	0.442	-0.215	19.09	-79.99	-246.8	-147.8
Births in event year X Time 1	(10.64)	(0.240)	(0.169)	(48.29)	(48.15)	(166.9)	(167.1)
Births in event year X Time 2	114.5**	0.0265	-0.0867	33.43	-25.83	-254.2	-194.9
	(11.23)	(0.273)	(0.194)	(58.26)	(57.32)	(215.9)	(216.0)
Births in event year X Time 2	5.154 (11.36)	0.0950 (0.302)	0.0335 (0.216)	14.89 (65.50)	14.51 (63.85)	-156.9 (207.9)	-156.5 (208.3)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects interacted	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,648	144,648	144,648	144,648	144,648	144,648	144,648
Obs. (weighted)	85246	85246	85246	85246	85246	85246	85246
Clusters (firms)	11653	11653	11653	11653	11653	11653	11653

The table shows results from 2SLS regression estimated on the matched and reweighted sample. Standard errors (in parentheses) are clustered at the firm level. ** p<0.01, * p<0.05