Employer Responses to Family Leave Programs*

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Abstract

Search frictions make worker turnover costly to firms. A three-month parental leave expansion in Sweden provides exogenous variation that we use to quantify firms' adjustment costs upon worker absence and exit. The reform increased women's leave duration and likelihood of separating from pre-birth employers. Firms with greater exposure to the reform hired additional workers and increased coworker hours, incurring wage costs corresponding to 60% of the salary cost of a full-time worker. These adjustment costs varied by firms' availability of internal substitutes. We also analyze a daddy-month reform and find largely similar employer responses to male workers' extended leave.

Keywords: Parental Leave, Firm-Specific Human Capital, Statistical Discrimination.
JEL-codes: J13, J16, J21, J22, J31.

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1 Introduction

Most high-income countries today have enacted generous family leave programs to help individuals transition into parenthood. New parents are entitled to wage-replaced benefits while taking a leave of absence from work, and firms are mandated to provide job protection for their employees on parental leave. While these family leave policies improve child and maternal health and foster stable employment of women after childbirth, they might also impose organizational challenges to firms.[1] For example, it might be costly and time-consuming to find someone to replace the worker on leave; replacement workers might not be as productive; and overtime hours might be remunerated at higher wages. These challenges might serve as a basis for employers to statistically discriminate against women, so quantifying such adjustment costs would be a crucial step towards understanding gender gaps in the labor market. Although a large theoretical literature has investigated the role of frictions in such statistical discrimination (see Barron et al., 1993; Bowlus, 1997), it is in practice difficult to measure the frictional costs faced by firms.

What are the costs faced by employers when their workers go on extended family leave, and how do firms respond to leave programs? These questions are difficult to answer empirically because workers' decisions about when, where (in which firm and job) and how long to go on leave are typically not random. The parental leave reforms in Sweden offer a unique setting for us to quantify their causal impact on firms' outcomes, since the reforms induced random variations in workers' turnover and duration of absence. Using a three-month parental leave extension in 1989 that increased paid leave from 12 to 15 months, we estimate the causal effect of workers' extended absence on firm outcomes, including total labor costs, hiring and re-organization, and firm performance. This paper thus provides new causal evidence on the existence, magnitude, and sources of frictional costs faced by firms associated with worker absence and turnover.[2]

Our research design takes advantage of the fact that treatment assignment was unrelated to any unobserved factors that might influence worker or firm outcomes. Eligibility to the extension was based on date of birth, and thus treatment was as good as randomly assigned. Furthermore, the parental leave reform was unanticipated and retroactive: it was implemented in July 1989 but retroactively covered

[1]Family policies are considered key policy instruments to address gender gaps in the labor market due to well-documented relationship between fertility and female labor supply. See, for instance, Angelov et al. (2016); Kleven et al. (2019); Hotz et al. (2017) for evidence of the effect of children on women's labor supply.

[2]Like many developed countries, Sweden provides generous leave to new parents, and women spend much longer time in parental leave than men: In 2011, women accounted for 76 percent of the total take-up of parental leave in Sweden, even though men and women had the same legal rights to paid leave (See https://www.scb.se/contentassets/813b12534a254bb28503983812d4649b/le0201_2012a01_br_x10br1201eng.pdf).

parents to children born in October 1988 and later. Eligible mothers could postpone their return to the workplace by three months, and firms were obligated to accommodate. The retroactive implementation ensures that workers could not manipulate their birth timing to take advantage of the new rules, and neither could firms manipulate their workforce composition to avoid workers with longer leaves. Thus, the policy intervention implied that randomly assigned firms unexpectedly and on short notice had to find replacement workers to cover for the additional leave, making it close to an ideal experiment to empirically quantify adjustment costs. We use population-wide matched employer employee data to analyze workplace-level demand for incumbent and external labor inputs, using the set of firms that had employees who had children around the reform cutoff dates.

Since employer responses depend on the extent and timing of workers' take-up of the additional leave, we first quantify the impact of the reform on individual labor supply and job mobility. Using an auxiliary dataset on parental leave spells, we show that eligible mothers took up 2.5 months on average out of the 3 months of additional leave, while the increase in male take-up was only one week on average. We document that women took the bulk of their additional leave during the first two years after birth, and show that the paid-leave expansion did not simply crowd out unpaid leave. Finally, the reform increased the probability that women leave for a different firm by 15 percent in the year when parental leave ended, which we interpret as voluntary switches due to extended possibilities for job search (while on leave).

Given that workers were unexpectedly more likely to take longer leaves or permanently exit the firm, we examine the adjustment behavior of employers. We focus on workplaces that employed at least one woman giving birth in the reform year, and construct a workplace-specific treatment intensity measure defined as the proportion of the workforce with a child born between October and December of 1988, which entitled workers to three additional months of leave. We compare workplaces with the same number of women who gave birth in the baseline year, and use exogenous variation in the months of childbirth that gave rise to different treatment intensities. To take potential seasonal effects into account, we define a corresponding measure for firms that employ women who gave birth in the preceding year, and use a difference-in-differences empirical design. We trace out the full temporal pattern of the reform effect, including pre-reform trends in the outcomes, by combining the difference-in-difference model with an event-time study. Note that in our setting, any impacts on firms' re-organization costs are the effects of *additional* leave, which are over and above the costs of workers going on child-related leave *per se*.

Our results show that private sector firms responded to the reform by increasing their permanent and secondary/temporary staff, by hiring new permanent workers, and by increasing the work hours of remaining workers (both incumbents and new hires). The net impact of these adjustments on the firm's total wage bill was positive, indicating that such reorganization came at a monetary cost. Specifically, for an average-sized workplace with 45 workers, having one additional worker going on extended leave increased the mean hours of her coworkers by 0.5 percent in each of the two years that eligible women use their additional leave. In other words, it is equivalent to having two coworkers increasing their contracted hours by 4.5 hours each per week over the two years. In terms of new hires, for each additional worker eligible for the leave extension, the number of new hires increased by 0.35 and 0.62 workers in the first and second year, respectively. The total effect of these adjustments implies that having one additional worker going on extended leave increased the total wage bill by an amount corresponding to 61–63 percent of the salary cost of a full-time worker. Note that if the adjustments were perfectly frictionless, firms would be able to replace the absent worker one for one and there would be a zero net effect on the wage bill, so our results suggest that the adjustments are indeed costly and sizeable.[3] Even with added labor inputs such as extra hours and new hires, private-sector firms did not perform better. Using data on sales and productivity for firms in the manufacturing industry, we find suggestive evidence of a decline in sales revenue, although these estimates are only marginally significant. For the public sector workplaces, there is no discernible pattern that would indicate adjustment or reorganization of the workforce.[4]

The ease with which firms can replace workers on leave depends on several factors: whether internal and external labor inputs are substitutable, and whether external labor market conditions are favorable for hiring. We find that workplaces where a large proportion of the workforce is concentrated in the same occupational category – i.e., firms where potentially many workers can do the job of the worker on leave – responded to the labor shortage by relying more heavily on internal substitutes, while firms with a lower degree of coworker substitutability relied relatively more on external labor inputs. We find no heterogeneous responses by local labor market thickness, however. Taken together, our findings thus highlight several sources of frictions associated with finding suitable replacement for workers on leave.

[3]The monetary costs for the employer that we document here are only related to hiring and remunerating replacement staff, since Swedish firms do not pay benefits to workers on leave (parental leave benefits are financed through social security contributions).

[4]Given that workers in both the public and private sectors worked 2.5 months less due to the reform, the lack of response in the public sector is not due to a smaller take-up of leave by the workers. The inability of public sector workplaces to adjust to new circumstances may have implications for the outcomes of these institutions (see e.g. Friedrich and Hackmann, 2017), although this is outside of the scope of our paper.

Finally, we add an additional piece of evidence on the existence and source of costs related to worker turnover by studying a reform that increased *male* workers' parental leave. In 2002, the Swedish government introduced a second "daddy-month" in the parental leave system. We might expect employer responses to be different for the 1989 and 2002 reforms for several reasons: first, the 2002 reform is smaller than that in 1989 (one month extension in 2002 versus three months in 1989); second, firms' planning horizon for the additional leave may be longer in 2002 as fathers take leave mostly after women exhaust their leave; and third, employers might respond to men's absence differently than women's. Thus, comparing employer responses across the two settings might be informative about key policy design features.

We show that the 2002 reform decreased fathers' labor supply by roughly one month, on average, spread out over the first three years after the child was born. Using a research design similar to that for the 1989 reform, we then analyze the employers' response to the labor supply reduction. We find a statistically significant (at the 10% level) increase in the wage bill paid to secondary/temporary staff in the two years following the birth of the child, and an increase in the work hours of the remaining workers (significant at the 10% level) in the same years. There are no significant effects, however, on the firms' total wage bill. These adjustments thus seem to merely compensate for the reduced labor supply of male workers. In addition, we also find a significant increase in the wage rates of remaining male workers by around 1 and 1.8 percent in the first two years after the reform. While we are not able to provide conclusive evidence on the mechanism, the results are consistent with an increased demand for the remaining workers who have firm-specific human capital. We find no effect on female workers' wages, suggesting that male and female workers within the firm might be imperfect substitutes.

Our paper contributes to three strands of literature. We contribute to empirical work on employers' ability to find substitutes for workers who leave the firm, which depends on the degree of specificity of human capital. Similar to recent work by Jäger and Heining (2019), we test empirically for the presence of frictions by using exogenous worker exits.[5] While Jäger and Heining (2019) exploit premature worker deaths, our paper contributes to this work by exploiting exogenous variation in the *duration* of worker absence generated by a parental leave reform. A related paper, Friedrich and Hackmann (2017), studies the ability of hospitals and nursing homes to replace nurses after a large expansion in parental leave entitlements in Denmark. The authors find negative impacts on patient outcomes in Danish hospitals

[5]See also Jaravel, Petkova, and Bell (2018) for evidence of team-specific human capital among inventors using premature deaths, and Bartel, Beaulieu, Phibbs, and Stone (2014) for similar evidence of decreased productivity in the health care industry attributed to the departure of experienced nurses.

and health centers due to the labor shortage of nurses – a female dominated occupation that is hard to replace. In contrast to much of the previous work using worker exits to assess human capital specificity, productivity, or employer outcomes, we study impacts for firms in the overall economy, as opposed to case studies of certain industries or sectors.

Second, we contribute to the growing literature on parental leave programs. While there has been substantial work on the impact of leave programs on women's careers and children's outcomes (Schönberg and Ludsteck, 2014; Carneiro, Løken, and Salvanes, 2015; Lalive and Zweimüller, 2009; Lalive, Schlosser, Steinhauer, and Zweimüller, 2014; Dahl, Løken, Mogstad, and Salvanes, 2016; Liu and Skans, 2010; Bana, Bedard, and Rossin-Slater, 2018; Bailey, Byker, Patel, and Ramnath, 2019; Ginja, Jans, and Karimi, 2020), less is known about the effects of such policies on firm outcomes and on their hiring strategies. Our paper is closest to Gallen (2019), which studies the effects of prolonged parental leave entitlement in Denmark on employer and coworker outcomes. Exploiting the retroactive implementation of the Danish reform, Gallen (2019) documents that small firms exposed to prolonged worker absence were 3 percentage points more likely to shut down in the five years after the reform. It finds no effects on firms' hiring practices, wage bill or coworker hours conditional on survival. Even though these results differ from what we find, Gallen (2019) provides other evidence indicating that leave-taking is costly for firms: the reform delayed the timing of coworkers' leave-taking, and sick leave among remaining coworkers increased in the years following the reform. We complement Gallen's paper by studying the substitutability of various labor inputs and providing evidence on various potential sources of frictions associated with labor turnover. We focus on a broader set of firms in terms of sector and firm size, and make a small methodological contribution by including firms with any number of births in the treatment year instead of restricting to firms with only one birth in a small window around the reform cutoff date.

A related paper, Brenøe, Canaan, Harmon, and Royer (2020), studies the effect of a female employee giving birth and taking parental leave on the outcomes of small firms, and finds an increase in coworkers' hours and employment probability. The net effect of these adjustments on the firms' total wage bill is positive, but the authors stress that these costs are reimbursed by the social security system so net costs are negligible for firms. While we undertake similar research questions, Brenøe et al. (2020) uses variations in women's year of birth combined with matching techniques to define control events, whereas our paper exploits exogenous variations in workers' labor supply stemming from parental leave reforms. Our different research designs also imply that the effects we identify are potentially different: while firms might anticipate a birth in advance and make necessary plans in their setting, employers experienced an unexpected and sudden increase in workers' leave-taking in our paper.

It is, however, difficult to generalize the relationship between employers' adjustment costs and the degree to which worker exits are unanticipated. Gallen (2019) estimates heterogeneous responses of the parental leave extension in Denmark by the extent to which the firms were "surprised", and finds similar effects on the firms' shut-down probabilities irrespective of their lengths of planning horizons. We also study the 2002 daddy-month reform that potentially gave firms a longer planning horizon, and find suggestive evidence of the existence of frictions there as well. If human capital is firm-specific, or for any other reasons suitable replacement is not easy to find, a longer planning horizon would not necessarily eliminate the adjustment costs for firms when workers go on leave. In general, the fact that workers and employers might find ways to smooth the shocks does not mean that adjustment costs are nil, nor does it imply that it is easy to avoid the costs. These are all important policy design features that deserve closer attention in future research.

Finally, our paper informs the literature on the implications of parental leave policies for the overall gender wage gap.[6] A few studies suggest that such costs may pass through to women's wages. For example, Gruber (1994) exploits regional variations in maternity leave mandates across U.S. states, and finds that employers shift the costs of the mandates onto the wages of women of childbearing ages. Thomas (2019) analyzes the effect of the Family and Medical Leave Act (FMLA) in the U.S. and finds that a woman hired after the FMLA was less likely to be promoted. Moreover, Xiao (2020) estimates an equilibrium search model where firms pay adjustment costs during parental leave, and finds employers' statistical discrimination against women to be a major factor of the gender wage gap in early career.

While it is out of the scope of this paper to provide evidence on the equilibrium effects of the policies studied, quantifying the trade-offs between equity and efficiency will be important for the design considerations of family policies.

2 Background & Institutional Setting

In Sweden, gender neutral eligibility to government-paid parental leave was introduced in 1974. Parents were initially entitled to six months of paid parental leave, which was subsequently extended in several

[6]For a discussion on the potential link between family leave programs and statistical discrimination against women in Sweden, see Albrecht et al. (2003, 2015, 1999). Moreover, the introduction of short leave programs have been shown to benefit subsequent maternal labor supply (Baum, 2003; Waldfogel, 1999; Baker and Milligan, 2008; Han et al., 2009; Kluve and Tamm, 2013; Rossin-Slater et al., 2013; Bergemann and Riphahn, 2015), but more generous leave policies may have adverse consequences on women's careers (Ruhm, 1998; Lequien, 2012; Schönberg and Ludsteck, 2014; Stearns, 2018).

steps to today's 16 months of paid leave per child. From 1974 onward, the mother and the father of a child are given half of the entitled days each, but have the option of transferring paid leave days between one another.[7]

Parental leave benefits consist of two main benefit types. First, part of the leave is replaced at a fixed daily amount. Second, the largest portion of leave transfers consists of benefits that replaces 90 percent of parents' salary, subject to a requirement of at least 240 days of employment before child birth.[8] The benefits are capped, however, such that the effective replacement rate is lower for workers earning above the cap. In 1989, the share of women (with positive income) earning above the cap was only around 1.5 percent, and the corresponding share among men around 12 percent.[9] Thus, the overwhelming majority of women were insured at 90 percent of previous earnings.

Parental leave benefits in Sweden are raised by employer social security contributions and are paid out by the governmental social insurance agency, as a part of the universal social insurance system. However, many collective agreements stipulate top-up insurances of parental leave benefits. These top-ups usually cover an additional 10 percent on top of the benefits the worker receives from the social insurance agency, up to the cap and – in some agreements – an additional 90 percent of the salary above the cap. However, because most workers at the time of the reform earned an income lower than the social insurance cap, the employer-provided replacements would simply top up the government-provided benefits with the additional 10 percent of foregone earnings. Thus, for the employer, the direct costs of employee absence due to child rearing are mainly associated with finding and hiring replacement workers, and potential foregone productivity.

The parental leave is job protected, and can be used flexibly. During the first 18 months after birth both parents are legally entitled to full-time job protected leave, irrespective of whether they claim parental leave benefits. Thereafter, parents have the option of reducing their working hours with up to 25 percent until the child turns 8 years old and claim leave benefits on a part-time basis. However, the vast majority of parental leave benefits is taken-up during the child's first two to three years of life.

The Right to Return to Previous Job A worker has the legal right to return to the same job after the leave spell, where a *job* is defined as the combination of tasks and salary. If the tasks are no longer

[7] In 1995, one month of paid leave became earmarked to each parent, implying that fathers could not transfer all of their paid leave to the mother of their child. This "daddy-month" was introduced to increase the incentives for fathers to increase their leave-taking. In 2002 and 2016, a second and third month of paid leave were earmarked to each parent.

[8] Today, the replacement rate is 80 percent of previous earnings. Individuals that do not fulfill the work requirement of 240 days pre-birth employment get a low daily amount of benefits.

[9] Own calculations based on population-wide data from 1989.

relevant when the employee returns to the workplace - due to e.g., re-organizations - the employer is obligated to find a similar position within the firm, with the same pay as before.

Extension of Paid Parental Leave: The 1989-reform Since the introduction in 1974 the parental leave system in Sweden has been subject to several extensions, and by 1989 parents were entitled to 12 months of paid leave, of which three months were compensated at the lower flat rate of 60 SEK per day. The reform that we exploit is an extension of the wage-replaced component of paid leave from 12 to 15 months that took place in 1989. The reform was implemented on July 1^{st} 1989, but retroactively covered parents to children born in October 1988. Transition rules in the implementation implied that parents to children born in August and September 1988 received one and two additional months of paid leave, respectively.[10]

Several features of this reform make it an ideal natural experiment for the study of leave durations on both workers and firms. First, entitlement to the new rules was based on the birth month of children, covering only a subgroup of the cohort giving birth in 1988. This means that we can easily identify workers eligible for different durations of leave, and distinguish firms by the extent to which their female employees are entitled to different durations of leave accordingly. Moreover, the reform was launched after the targeted women had already given birth, and after the conception of children born at the date of reform launch. Thus, the reform was unanticipated by both workers and firms, so the composition of women giving birth should be unaffected by the reform, and firms should have no possibility of manipulating the fraction of workers giving birth in anticipation of the intervention.

3 Data

We use several population-wide administrative data sets covering both workers and firms. Individual level data on childbearing (date of birth, parity, etc.) are matched with individual level panel data on annual labor income and background characteristics (e.g. year of birth, sex, education). We merge these data to a linked employer-employee register that covers all employed individuals in Sweden. We can identify both firms and establishments (workplaces), and the latter is our unit of analysis. For workers with multiple employment spells within a calendar year, we keep the workplace where they earn their main income. Thus, for each establishment in our sample we retain the primary workforce. The linked

[10]This reform was studied in Liu and Skans (2010), who examined the effect of the duration of parental leave on children's scholastic performance.

employer-employee data set includes industry classification (NACE), establishment size, and location (municipality). We exploit the population-wide nature of the matched worker-firm data to further characterize establishment by the composition of their workforce in terms of e.g., gender, age, education, earnings, occupation, etc.

For each worker/establishment/year, we merge information from the Wage Structure Statistics; an annual survey of establishments collecting information on the wages and working hours for each employee that worked at least one hour during the measuring month. Wages are reported as full-time equivalent monthly wages, and working hours are *contracted* working hours (expressed as percent of a full-time position). The Wage Structure Statistics is a population-wide register of organizations in the public sector, and includes the universe of private sector firms with at least 500 employees. For smaller private sector firms, a random sample is drawn based on a cross-classification of industry and establishment size. All in all, roughly 50 percent of all private sector employees are covered. The earliest year for which there are firm level registers in Sweden is 1985, and we use data up to 1996. We exclude the smallest (fewer than ten employees at baseline) and the very largest (top 1 percentile of size distribution; i.e., firms with more than 265 workers at baseline) establishments from our analysis data.

4 Program Take-up

We begin by quantifying the program take-up at the worker level using variation in eligibility status by child birth date. Our research design exploits that women who gave birth in 1988 were as good as randomly assigned to paid leave of varying durations, due to the stochastic nature of exact birth timing. To take account of seasonality in the outcome variables by calendar month of birth, we net out differences in the outcomes between women giving birth in different calendar months in an adjacent year. Thus, we implement a difference-in-differences (DD) methodology where the identifying assumption is that any birth month effects are similar across years.[11]

We sample all women who give birth in 1988 (referred to as the treatment cohort), and all women who give birth in 1987 (control cohort). Moreover, we make use of the full reform of three additional months of benefits (ignoring the transition rules of 1 and 2 additional months to August–September parents); thus, we drop all workers who give birth in August and September. Assuming that month of birth is as good as randomly assigned, this sample restriction poses no threat to identification. In

[11] This strategy also addresses potential unobserved heterogeneity by season of birth, e.g., as documented in Buckles and Hungerman (2013).

Table A.1 we show that differences in pre-determined characteristics by birth month are balanced across birth cohorts.

To trace the temporal pattern of the reform effect on labor supply, we estimate a dynamic DD model including pre- and post-reform outcomes. Let T_i be an indicator that takes the value 1 if mother i's child was born in October–December, and zero if her child was born in January–July. Let t denote calendar year, and let D_i take the value 1 for mothers who gave birth in 1988, and 0 for those who gave birth in 1987. We exploit the reform variation in combination with an event-time model in a triple-differences (DDD) empirical strategy:

$$\begin{aligned} y_{it} &= \delta_0 + \sum_{\tau=-2}^{8} \beta^{\tau} \Big(T_i \cdot D_i \cdot \tau_{it} \Big) + \sum_{\tau=-2}^{8} (\delta_1^{\tau}\, \tau_{it} + \delta_2^{\tau}\, T_i \cdot \tau_{it} + \delta_3^{\tau}\, D_i \cdot \tau_{it}) \\ &+ \delta_4\, T_i \cdot D_i + \delta_5\, T_i + \delta_6\, D_i + \mathbf{X_i'}\,\gamma + \epsilon_{it} \end{aligned} \tag{1}$$

with event-time indicators τ_{it} for each year relative to the baseline year (year birth of individual i's child, i.e., 1987 or 1988).[12]

The coefficients of interest are the β^{τ}'s, which measure the difference in outcomes between women giving birth in October–December versus Jan–July of 1988, to the corresponding difference among women giving birth in 1987, in each year before and after birth, relative to the calendar year of birth.[13] The vector $\mathbf{X_i'}$ includes flexible controls for age, educational level measured in the year that i gave birth (compulsory schooling, high school, some college, and college degree), birth parity, the age difference in months to the previous child (set to 0 if parity equals 1), and the average earnings in the two years before giving birth.[14]

To estimate women's labor supply response to the leave extension, we estimate the effect on labor earnings, and on a conservative indicator of labor market participation defined as having labor earnings above a certain threshold. Earnings do not include governmental transfers, but may include employer-provided top-ups of parental leave benefits that are stipulated in some collective agreements. Thus, effects on earnings provide a conservative estimate of labor supply responses to the policy.[15]

Note that our labor market outcome variables are recorded on a calendar year basis, so child age –

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[12]Namely, $\tau_{it} = \begin{cases} \mathbb{1}[t-1988=\tau] & \text{if } D_i = 1 \\ \mathbb{1}[t-1987=\tau] & \text{if } D_i = 0 \end{cases}$.

[13]In these event study analyses, the standard errors of estimates are clustered at individual level.

[14]This empirical strategy is similar to that used in Karimi et al. (2012), who studied the labor supply responses to 1989-reform and two additional reforms in the Swedish parental leave system.

[15]While labor income is a function of both hours worked and hourly wages, short-run fluctuations in labor income at the individual level are more likely to be driven by hours worked rather than wage-adjustments.

expressed by τ in Equation (1) – is measured in years. To assess the plausible timing of the reform effect on women's labor supply, we use an auxiliary data set on parental leave benefit claims (not matched with our primary data) and analyze the effect of the reform on leave take-up by child age in months, in Appendix B. The effects show that the majority of additional leave was used when the child was between 12 and 24 months old, and some leave was also used when the child was 24–36 months old (see Figure B.1). Thus, we expect the effects estimated with Equation (1) on labor market outcomes recorded annually to show up in years 1 and 2 after birth.

The estimated coefficients $\widehat{\beta}^{\tau}$ in Equation (1) are presented in Figure 1, and show (in panel A) that women entitled to additional paid leave reduced their labor supply in the first two years after giving birth, but not in the longer run. Similarly, participation was negatively affected in the short run (Panel B). The earnings estimates correspond to a decline of roughly 1.5 months worked, based on the average earnings in the control group in corresponding event times. One reason for why these magnitudes do not match up with the 3 months increase in benefit entitlement could be employer-provided top-ups of benefits (which are included in the earnings measure). Indeed, in Appendix B, we show that the reform had full effect on parental leave take-up measured by benefit claims.

One margin that could have implications for employers is whether employees stay with the firm throughout the parental leave spell or after the leave has expired. Since leave benefits are financed through pay-roll taxes and paid to the claimant by the Social Insurance Agency, a worker can switch jobs while on parental leave without foregoing benefits. Extended leave duration may thus imply a longer period of job-search for those women looking to leave their firm.[16]

To assess whether separations are affected by the policy, we estimate Equation (1) on the annual likelihood of switching from the pre-birth employer to a new firm. The results show that women who are entitled to extended leave are roughly 2 percentage points more likely to leave the pre-birth employer in year 2 after birth (panel C, Figure 1). Relative to the baseline hazard, this corresponds to an increase of about 15 percent.

An alternative explanation is that the separations are involuntary. Because Swedish employment protection legislation is relatively strong, involuntary separations are arguably less likely but could result if, for example, the employee is re-allocated to an inferior position, with new tasks etc., prompting the worker to leave. With the data at hand, we are not able to explicitly rule out that the excess separations

[16]Gottlieb et al. (2016) find that a Canadian reform that extended job-protected leave to one year for women giving birth after a cutoff date increases entrepreneurship by 1.9 percentage points. Moreover, Lalive et al. (2014) also find that access to job-protected parental leave changes women's job search behavior.

caused by the policy are involuntary.

5 Employer Responses

Given the documented full take-up of the extended family leave program at the individual level, we now turn to firms' reactions to the reductions in female labor. We sample workplaces in the private sector at which at least one female employee had a child born in 1988. As in section 4, we make use of the *full* reform of three additional months, and exclude workplaces that had women giving birth in August or September in 1988. Our identification strategy exploits the fact that workplaces are differentially exposed to varying leave durations of their female employees, depending on whether these employees happened to give birth before or after the eligibility cutoff date. We define the workplace's treatment intensity as the proportion of the workforce that gave birth from October to December in 1988. Since the reform was unanticipated, retroactive, and based on month of birth, neither the workers nor firms could have manipulated the timing of births to be before or after the eligibility date. Therefore, treatment intensity is orthogonal to any unobserved determinants of the firm level outcomes that we study. Moreover, we extract data for the corresponding set of workplaces in which at least one female employee gave birth in 1987, which will serve as a set of control firms.

Let N_j^{OctDec} denote the number of women who gave birth between October and December in the baseline year (1988 or 1987), and let N_j denote the total number of employees in firm j at baseline. We define treatment intensity of firm j as:

$$\pi_j = \frac{N_j^{OctDec}}{N_j}.$$

We estimate the following triple-differences specification (similar to equation (1) in section 4):

$$\begin{aligned} y_{jt} &= \delta_0 + \sum_{\tau=-2}^{8} \beta^\tau \left(\pi_j \cdot D_j \cdot \tau_{jt}\right) + \sum_{\tau=-2}^{8} (\delta_1^\tau \tau_{jt} + \delta_2^\tau \, \pi_j \cdot \tau_{jt} + \delta_3^\tau \, D_j \cdot \tau_{jt}) \\ &+ \delta_4 \, \pi_j \cdot D_j + \delta_5 \, \pi_j + \delta_6 \, D_j + \mathbf{X_j'}\gamma + \epsilon_{jt} \end{aligned} \tag{2}$$

where D_j indicates firms in the 1988 cohort, and τ_{jt} are event time indicators ranging from -2 to 8 years relative to the baseline year.

Control variables Vector **X** includes flexible controls for the total number of workers giving birth in the baseline year interacted with indicators for baseline establishment size decile. Moreover, we include controls for pre-reform workplace characteristics: a second order polynomial in the share of the workforce that is female, the age composition of the workforce, the share of the workforce that consists of women in childbearing ages, the educational composition at the establishment, and a second order polynomial in workplace size, and fixed effects for 2-digit industry affiliation. Our rich set of controls ensures that we are flexibly controlling for the firm size distribution and workforce composition. We also include firm-size group specific linear trends in the outcome variables. Essentially, we are comparing firms within a narrow size category that experienced the same number of births in the baseline year, so the variation in treatment intensities of these firms stems only from the proportion of baseline-year births that happened to be in October–December.

We note that the same firm could have some female employees giving birth in 1987, and again some other employees giving birth in 1988, which would imply that this firm is in both our control and treatment samples. Having partly overlapping samples of workplaces in both control and treatment cohorts does not pose a threat to our identification strategy as long as the distribution of births across months is random from one year to another. In other words, the fact that a firm has many births concentrated in the fall of 1987 should not imply that the same firm is intensely treated also in 1988. Indeed, the unconditional correlation between the fraction of employees having children born in October–December of 1987 and the corresponding proportion in 1988 for the same firm is -0.00033 (p-value: 0.783, and $N =$ 7,086).

In all regressions, we cluster the standard errors at the workplace level to take into account potential serial correlation in the outcomes within establishments.

Finally, we note that our control cohort firms could also get treated in the future – they would eventually also have employees giving birth in later years who then go on leave durations that are longer than would be in the absence of the policy changes. However, the treatment cohort firms would also have more employees giving birth in later years. There is no reason to believe that one cohort is inherently subject to higher employee child births in the future than the other cohort of firms. If the treatment cohort firms respond to the policy by hiring more women, then the long-run impact of the policy change would be compounded by the firm's hiring decisions immediately after the reform. Thus, our results within a relatively short window (around three years) could be interpreted as the direct effects of the reform, whereas long-run results might also include snowballing effects from firms' short-run responses (as workforce compositions change).

5.1 Summary statistics

The main focus in our analysis of employer responses are the private sector workplaces. In Table A.2 we report summary statistics for pre-determined workplace attributes for our study sample of establishments as well as for the universe of all active private sector establishments in Sweden in 1988 for comparison. The establishments in our study sample are similar to the full population of establishments in terms of education composition, earnings, wage rates, and contracted work hours. However, our sample firms have a higher share of female employees, more employees giving birth in a given year, and are larger compared to the average establishment in the population.

In Table A.3 we show that the industry composition of our study sample is representative of the full population of private sector firms. Finally, in Table A.4 we show that there are no differences in the characteristics of firms whose employees give birth in the fall vs. spring, for firms with 10–20 employees where only one woman gave birth.

5.2 Employer adjustment strategies

To gauge overall changes in the firms' labor force, we first look at the impact of the reform on the total labor cost at the workplace – the sum of annual earnings of all workers on the firms' payroll, including women on parental leave. Since the Swedish government pays for the parental leave benefits at the replacement level of 90 percent and not all firms top up the remaining 10 percent, having workers on extended leave implies that the firm has fewer people to pay wages to in those months, if the firm does nothing to replace the women on leave.

If there are signs of reorganization at the firm, our interest lies in investigating the different margins of adjustment. We decompose the total wage bill into portions associated with primary employees versus secondary/temporary workers. *Primary* employees are defined as those for whom the establishment is their primary employer, i.e. the establishment from where they derive most of their annual income (if they have more than one employer in the same calendar year). All employees in our sample that gave birth to a child in the baseline year are, due to our sample selection criteria, primary employees. We measure wage bill paid to *temporary/secondary* workers as the portion of the total wage bill net of that paid to primary employees. This measure will include both temporary employments and part-time workers for whom the employment is not their primary source of income, and does not include the women on parental leave by definition. Moreover, the variable will also capture the wage bill paid to new hires if they spent more months working with their old employer than with their new firm in the year that they joined the new employer.

Figure 2 presents the coefficients β^{τ} from specification (2) for the firm's total wage bill (which includes both primary and temporary employees), measured in 1000s SEK.[17] The results show a negative effect on the total wage bill in year one after birth. This is mainly driven by the fact that "treated" firms did not pay wages for workers on leave during the additional leave months. We find an increase in the total wage bill in years two and three, where the point estimates suggest that going from 0 to 100 percent treatment intensity, the total wage bill increased by 6,6 and 6,8 million SEK in years two and three, respectively. Therefore, reorganization at the firm incurred a cost over and above the salary payments for the workers who go on extended leave. To get a sense of the magnitude, we evaluate the effect at an average sized firm (45 workers at baseline for the treatment cohort firms, see Table A.5), where the increase in the wage bill in year corresponds to 1.8 and 1.9 percent of the total baseline wage bill in years 1 and 2, which is equivalent to the salary of 0.61 and 0.63 full-time workers, respectively.[18] The adjustment costs thus appear sizeable.

We note that part of this "excess wage bill" effect may be driven by the employers' top-ups of government PL benefits stipulated in collective agreements. If the firm hires exactly one full-time worker to replace the worker on leave and all else remains the same, the total wage bill of the firm would then increase by 10 percent of the income of a full-time equivalent worker. However, our results show that the total wage bill increases by substantially more; slightly more than 60 percent of a full-time equivalent worker. There is no data on the prevalence of wage top-ups; however, even if all firms top up the 10 percent, it can only account for a small proportion of the effect on the firm's total wage bill documented here.

In Figure 3 we decompose the effect on total wage costs into a component attributed to primary employees and to temporary/secondary employees, respectively. The total wage cost of primary employees decreases in year one after childbirth, which is likely a result of increased leave duration of eligible workers. However, in years 2 and 3, there is an increase in the payments made to primary workers. The wage-bill paid to secondary workers increased from year 1 to 4 after childbirth, showing that firms adjusted immediately by increasing their temporary/secondary staff, or by hiring new primary workers. However, during the first year after childbirth the wage bill for the secondary/new staff does not increase sufficiently to offset the reduction in primary employees' labor inputs. Changes in the wage bills

[17] 1000 SEK amounts to circa 105 USD, or 95 EUR.

[18] Calculation: The average baseline wage bill 8,100 (1000s SEK), and the average yearly earnings for a full-time worker 240. Thus, ($6600 \times \frac{1}{45}/8100 = 0.018$), and ($6600 \times \frac{1}{45}/240 = 0.61$).

can be driven both by the number of new hires and the work hours of the workforce (both incumbents and new hires) – panels C and D of Figure 3 thus decompose the wage bill of primary workers into these two components. To measure hours supplied by the coworkers of women on leave, we calculate the average contracted work hours of all primary employees at the workplace, excluding the employees who gave birth in the baseline year. Contracted work hours are measured as a proportion of full-time equivalent hours (for example, 75 percent). Results show that for an average-sized workplace with 45 workers, having one additional worker going on extended leave increased the mean hours of her coworkers by 0.5 percent in each of the two years that eligible women use their additional leave. In other words, it is equivalent to having two coworkers increasing their contracted hours by 4.5 hours each per week over the two years. Moreover, the number of new hires increased by 0.35 and 0.62 workers in years two and three, respectively.

Why do the employer responses last until the third year after the reform? One potential explanation could be that the observed separations in year 2 induce firms to hire new workers or increase their incumbents' hours to replace these exits, which would arguably show up in years 2 and 3. Another explanation is that there is a wide distribution of parental leave lengths and women spread out their leave over two or three years (even before the reform), and we cannot identify the compliers in this setting without more detailed data on paid and unpaid leave. Finally, employment protection may also have played a role, since a temporary worker hired for 12 months or more would have to be made a permanent worker.

In Figure A.1 in Appendix A, we display the estimates for the firms in the treatment- and control samples, respectively, to illustrate the trends in the outcome variables for the different samples. We note that all outcomes are driven by the adjustments of treatment-cohort firms in response to the reform. It is also important to net out the mechanical seasonality effects in calendar year outcomes by using the control cohort.[19]

5.3 Heterogeneity by firm size

A worker's absence might constitute a substantial labor loss especially in small firms. In Figure A.2 we show heterogeneous effects by firm size. We define a small firm to those with fewer than the median

[19] Since women who gave birth in 1987 took on average 20 months of paid parental leave (including days taken on part-time basis), then for example January mothers might have come back to work in year 2 while December mothers were still on leave, so it is unsurprising that high-intensity firms in the control cohort paid out a lower wage bill in those calendar years than low-intensity firms. Our identification strategy relies on the fact that these mechanical calendar year effects by birth month would have stayed the same in the absence of reform.

number of employees in our sample of private sector firms. In the regressions, we include the same set of control variables as in our main analysis, but define new indicators of (within-group) firm size decile interacted with the number of employees giving birth in the baseline year. We find that the effects seem to be driven by the set of smaller firms.

5.4 Limited responses in the public sector

While our main focus is on private sector employers, we report the corresponding set of results for establishments in the public sector in Figure A.3. Like the private sector, there is a drop in the salary payments to primary workers in year one, but unlike the private sector, there are no effects on the wage bill beyond that first year. Thus, if public sector workplaces were re-organizing, they did so only to offset the labor supply reduction. However, there are no discernable patterns of adjustments in terms of secondary workers' wage bill, or coworker hours. Given that individual-level program take-up were both quantitatively and qualitatively similar, the heterogeneity in employer adjustment by sector of employment is not likely driven by heterogeneity in the size of the labor supply shock caused by the reform. An alternative explanation is that the public sector – to a large extent comprised by schools and hospitals – is financed based on politically fixed budgets, leaving a smaller room for replacing staff.[20]

5.5 Effects on firm performance

Even though we show that private sector firms re-organized their workforce and added labor inputs (extra hours and new hires), it does not immediately imply that these adjustments were enough to maintain previous firm productivity. For example, if the new hires and overtime hours are less productive than the workers on extended leave, then the labor adjustments might only serve to ameliorate the negative impacts of worker absence but not completely offset them.

For a subset of the firms in our sample, namely firms in the manufacturing industry, we have information on firm productivity measures such as sales revenue and value added. These constitute roughly 23 percent of our sample of firms (see Table A.6 for summary statistics of this subset of firms). Compared to our full sample, the manufacturing firms have lower shares of female workers, fewer employees giving birth in a given year, higher average wage, and a larger workforce.

Similar to firms in our main sample, firms in the manufacturing sector also responded to the reform

[20] An inability to make labor adjustments may have important implications for the outcomes of these institutions. A recent example is emphasized by Friedrich and Hackmann (2017), who show that labor shortages of nurses in Denmark - due to a parental leave reform - had detrimental impacts on patient outcomes.

by increasing labor inputs, as their wage bills paid to both primary and secondary workers increased in years two and three (see Figure A.4). However, these additional labor inputs did not lead to higher output in production; if anything, firms' performance seemed to have declined. Figure 4 report the estimated effects of the reform on log total sales and log total value added for these manufacturing firms. Although the estimates for performance measures are somewhat noisy and the confidence intervals are wide (due to fewer observations), the overall picture suggests that the firms' total sales revenues declined slightly as a result of the reform exposure.

Taken together, our analyses show that firms are indeed affected by workers taking longer leave. Women taking additional time off for child-rearing implies that firms would have to incur costs in replacing them. In particular, our findings indicate that adjustment costs are over and above the costs of salary payments for workers on leave. Even though the firms do not need to pay the workers on leave, employers are not able to find perfect replacements for the absent workers and have to pay extra to fill in the work left behind.

6 Heterogeneity in Frictions across Labor Markets

We have shown in the previous section that firms are indeed affected by workers taking extended parental leaves. When women take additional time off, firms have to incur costs in finding, hiring, and training temporary workers, or paying for more overtime hours of incumbent workers. We show that the net effect of such adjustments to the 1989-reform in Sweden come at a cost over and above the salary cost of the workers to be replaced. The magnitude of such costs are likely to depend on how easily the firm is able to find good substitutes for the worker(s) on leave.[21]

In general, the firm could employ any of the following three strategies to pick up the work left behind by workers on leave: it could try to retain existing workers, hire new workers, or increase hours of incumbent workers. Which strategies the firm ends up choosing will depend on how substitutable human capital is between workers from within the firm and external hires (i.e., whether human capital is firm-specific or general). Given the production technology and substitutability of its inputs, the number of hires may also depend on the availability of workers in external labor markets. In this section, we explore whether firms adopt different replacement strategies depending on the extent of substitutability

[21]For example, Jäger and Heining (2019) suggest that incumbent workers are closer substitutes to one another compared to outsiders, and that thin external markets lead to higher firm-specificity of human capital and lower replaceability of incumbents.

between coworkers within the firm, and on the abundance of potential replacements in their local labor market. If finding replacement workers is frictionless, we expect to find no heterogeneous adjustment strategies adopted by firms facing different labor market conditions.

6.1 Internal substitutability of workers

We begin by analyzing whether firms' adjustment strategies depend on the number of available substitutes within the firm. Do firms with fewer internal substitutes resort to external hires? We characterize the potential for internal substitution possibilities at the workplace by the overall occupational specialization at the establishment.[22] Similar to Cortes and Salvatori (2019), we calculate the employment share in the largest occupation category within workplaces as a measure of internal substitution.[23] The intuition is that workplaces with a high degree of occupational concentration would have many workers doing similar tasks, and thus have greater scope for internal substitution across incumbent workers. We divide workplaces into groups depending on whether they are above or below the 75th percentile of the internal substitutability index and estimate our main specification (1) with an additional interaction term indicating firms with high degrees of substitutability. We then report the coefficients for firms with high and low substitutability separately from this pooled regression, in Figure 5.

We focus on two outcomes in this analysis: work hours of the incumbent staff (workers who were employed at the firm at baseline, excluding the women on leave), and the number of new primary hires. We find that firms with a high degree of internal substitutability increased incumbents' hours by 1.2 and 2 percent in years 2 and 3 in response to the reform, whereas firms with internal supply constraints (lower substitutability amongst coworkers) did not adjust work hours of incumbents. The heterogeneous responses in incumbent hours are significantly different in year 3 (see panel A of Figure 5). Moreover, for an average-sized firm with few internal substitutes, exposure to the reform led to a significant increase in new hires by 0.47 and 0.76 workers in years 2 and 3, respectively (panel B). Firms with many internal substitutes, by contrast, did not respond to the reform on the hiring margin. The differences in new hire responses across the two groups of firms are not statistically significant, but the point estimates are in line with our prediction.

The fact that firms employed different strategies depending on the availability of internal substitutes

[22]Because we sample firms who potentially have more than one woman going on leave, we are not able to easily study the heterogeneity in these effects by the number of direct occupational substitutes the firm has for the absent person.

[23]We define occupation categories by the combination of education level (four categories) and field (seven categories), as occupational codes are unavailable during the time period studied.

implies that human capital specificity may induce binding supply constraints, and thus points to an additional source of frictions facing firms when workers leave.

6.2 External labor market conditions

If human capital is not entirely firm-specific, internal and external workers should be somewhat substitutable, and the firm will simply choose the less costly of the replacement options. For example, if overtime hours are paid at a premium, firms may look externally for new hires rather than having remaining workers increase their work hours. The ability to hire externally might depend on local labor market conditions, which also affect the firms' replacement strategies. In particular, firms in thick labor markets – in labor markets where workers with the relevant skills are abundant – will have a higher probability of finding replacement workers on the external market. In contrast, in a thin market, firms will arguably find it more difficult to replace workers with external hires, and thus may resort to internal retention and hour increases.

To capture the external labor market conditions facing the firms in our sample, we construct measures of industry-level labor market thickness at each locality, using population-wide data on employed individuals aged 19–64. We delineate 64 commuting zones, and define labor market thickness as the share of employment in a 2-digit industry within a commuting zone relative to the nationwide employment share in that industry.[24] We define a market to be "thick" if the local employment share in a given industry is higher than the national employment share in the same industry, and estimate heterogeneous employer responses to extended employee absence by whether they are facing a thin or thick local labor market in each year.

Panels C and D of Figure 5 presents heterogenous effects of the reform by local labor market thickness. We find no statistically significant differences in the adjustment strategies undertaken by firms that faced thin and thick markets.

6.3 Heterogeneity in wage costs by internal and external substitutability

We have shown in previous sections that both internal and external supply constraints may dictate which adjustment strategies are available to firms. It is interesting to ask whether relying on internal or external replacement is the most costly option.

[24] $\theta_{kct} = \frac{emp_{kct}}{emp_{ct}} / \frac{emp_{kt}}{emp_{t}}$, for each industry k, commuting zone c, in year t.

In Figure A.5, we show that firms with a low degree of internal substitutability incurred significant increases in the total wage bill in years 2 and 3, and the point estimates are over two times as big as that of firms with high substitutability. This suggests that firms with little scope of internal substitution might face higher costs of adjustment, although the differences are not significant. Firms facing thin labor markets also incurred significant increases in labor costs (while those in thick labor markets did not), but there are no differences in the total wage bill across firms facing different external labor market conditions.

7 Employer Responses to Male Leave-taking: Daddy-Month Parental Leave Reforms

In this section, we complement our main results with an analysis of employer responses to male workers' parental leave, in order to investigate whether firms' adjustment strategies are symmetric towards men and women's additional leave.

To study the effect of men's leave-taking, we make use of the second "daddy-month" reform in 2002, which gave additional monetary incentives for fathers to take up parental leave. Prior to the implementation of the reform, one month of the paid leave was non-transferable between the parents. To further encourage fathers' leave-taking, the government introduced an additional non-transferable month of paid leave – a second "daddy-month" in 2002.[25] All parents to children born on January 1st, 2002 and later were eligible to the additional paid leave.

7.1 Worker's Labor Supply Response

To quantify men's labor supply response to the reform, we estimate a dynamic difference-in-differences specification, contrasting the labor income of men who had a child born in 2002 to labor income of men whose child was born in the same calendar month in 2001. Specifically, in the sample of private-sector employed men, let D_i be an indicator taking the value 1 if individual i's child was born in 2002, and 0 if his child was born in 2001. Moreover, let τ be an indicator for event-year, where event-time $\tau = 0$ indicates the year that i's child was born. We estimate the following regression equation using OLS:

[25] At the same time, the total number of leave months was increased from 15 to 16 months, where this additional month could be used by either the mother or the father. Previous work has shown that this additional, non-reserved, month was mainly used by mothers; see e.g. Avdic and Karimi (2018).

$$y_{it} = \delta_0 + \sum_{\tau=-5}^{10} \beta^{\tau} \Big(D_i \cdot \tau_{it} \Big) + \sum_{\tau=-5}^{10} \delta_1^{\tau} \tau_{it} + \delta_2 D_i + \mathbf{X_i'} + \epsilon_{it}. \tag{3}$$

The vector of controls, $\mathbf{X_i'}$, include a polynomial in age, indicators for education level, dummies for the pre-birth income decile, dummies for the parity of the child, dummies for the calendar month of birth, and dummies for industry affiliation (at baseline). We estimate this model on male labor income, and display the results in Figure 6. The results show a decline in men's labor income in years 0, 1, and 2 after birth, and also some decrease in years 6–8 (right before the parental leave allowance period ends). The total income drop in years 0–2 combined amounts to SEK 32,600, which corresponds to 1.13 months worked for a full-time employed male worker in the private sector in the year before the birth of their child.

7.2 Employer Responses

To study the employers' responses to the 2002 daddy-month reform, we use a research design that is similar to the strategy used for the 1989 reform. We sample workplaces in the private sector in which at least one male employee had a child born in 2002 or 2001, and define treatment intensity π_j as the proportion of the baseline workforce that were eligible to the new parental leave rules (i.e., the number of male workers with a child born in 2002 as a proportion of the workforce). Moreover, we extract a set of control group firms, which had at least one male worker with a child born in 2001 or 2000, and calculate a treatment intensity for the set of control firms in a manner similar to the "treatment cohort" firms. Our identification strategy thus relies on contrasting the outcomes of firms that had more male workers with children born to the right of the cutoff relative to those on the left, after netting out any seasonality in the outcomes using the corresponding difference across firms in the "control cohort".

The empirical specification is thus equivalent to that expressed in Equation (2), with the same set of controls as used previously. One small difference is that firms were aware of the reform on January 1, 2002, so firm outcomes in event year 0 could already be a response to the reform. Therefore, we consider event year -1 as the baseline year for all specifications regarding the daddy-month reform.

Figure 7 presents the results for firms' total wage bill, wage bill to secondary/temporary workers, and work hours of the coworkers on leave. The confidence intervals around the estimates are wide, and the effects are only marginally significant at the 10% level, but the overall pattern of the point estimates provide a similar picture of employer responses to employee absence. Specifically, there is an increase in wage bill paid to secondary/temporary workers in years 1 and 2 (significant at the 10% level), and an increase in the work hours of the coworkers (significant at 10%) in the same years. There are no significant effects on the firms' total wage bill.

Although only marginally significant, the estimates suggest that firms adjusted to men's leave-taking in response to the 2002 daddy-month reform, with similar strategies compared to women's leave-taking after the 1989-reform. However, the adjustments merely compensated for the reduced labor supply of male workers, as there was no change in firms' total wage bill.

There might be several reasons for the smaller effects of the 2002 reform on firm outcomes compared to the 1989 reform. First, the 2002 reform is smaller than that in 1989, as there was only a one-month extension in 2002 compared to three months in 1989. Second, firms' planning horizon for the additional leave may be longer in 2002 as fathers typically take leave after women exhaust their leave. Third, employers might respond to men's absence differently than women's. Men's parental leaves are short even with the leave extension, so firms might be reluctant to hire permanent workers to replace men (while they might find it necessary to do so for women).[26] Thus, comparing employer responses across the two settings might be informative about key policy design features.

7.2.1 Effects on coworkers' wages

In order to study other potential margins in which the firms might have adjusted, we turn to the effects of the reform on coworker wages to determine any presence of frictions. For example, if human capital has firm-specific components, employers might be unwilling to use external hires to replace men on leave, and instead resort to increasing the wage rates of remaining coworkers in order to retain them.

Indeed, we find a statistically significant increase in the wage rates of the remaining male coworkers, in the first three years after the reform (see Figure 8). In an average firm (41.5 workers at baseline), the point estimate in years 0, 1, and 2 correspond to effect sizes of 1.2, 1.8, and 1.7 percent increases in male coworkers' monthly full-time equivalent wages. We find no effects on female coworkers' wages (panel B).[27] The fact that the firms increase their demand only for the male incumbents and not female suggests that men and women might not be perfect substitutes within the workplace, which is plausible given the substantial occupational segregation by gender, even within firms.

[26] Employment protection laws in Sweden stipulate that temporary workers hired for 12 months or more have to be made formal (permanent) employees.

[27] An alternative explanation for the increase in male coworkers' wages is proposed by Johnsen et al. (2020), who argue that remaining coworkers gain by having fewer competitors present at the workplace.

Overall, the results from the 2002 reform are in line with the results from the 1989 reform.

8 Conclusions

We study the effect of parental leave mandates on firms' outcomes and potential implications for gender gaps in the labor market. We exploit the exogenous variation in firms' exposure to extended employee absence induced by the 1989 reform in Sweden that increased paid parental leave by 3 months. We show that the additional leave was almost fully taken up by mothers, while fathers' take-up was minimal. Moreover, the additional leave entitlement increased the probability that new mothers separate from their pre-childbirth employer (and switch to a different employer). From the firm's point of view, this implies that they would have to replace workers both temporarily and permanently.

Turning to firms' responses, we find that private sector firms with greater exposure to the reform adjusted primarily by hiring new permanent workers and temporary workers, and to a lesser extent by increasing the contracted hours of remaining coworkers. Employers were not able to replace the workers one-for-one, and the re-organization came at a cost over and beyond the salaries of the women on leave (which the firms did not have to pay). Using data on sales and value-add for firms in the manufacturing industry, we provide suggestive evidence of declines in firm performance even with the additional labor inputs. This suggests that even when firms are able to find replacement labor, these workers may not be as productive as workers on leave due to e.g. firm-specificity of human capital. We further document heterogeneity in employer adjustment based on the ease with which replacement workers can be found. In particular, we show that firms with high internal substitutability within the workplace relied more heavily on incumbents' hours than firms with lower substitutability, and the former hired new workers relatively less than the latter.

We also extend our analysis to the 2002 daddy-month reform to see if firms' responses to men's leave in 2002 were symmetric to those towards women's leave in 1989. We first show that the 2002 reform decreased fathers' labor supply by roughly one month on average, spread out over the first three years after the child was born. We then study employers' responses to men's extended absence, and find that firms adjusted by marginally increasing their temporary staff and work hours of the remaining workforce. There was no significant change in the total wage bill, suggesting that employers adjusted barely enough to offset the reduced labor supply of men. We also find a significant increase in the wage rates of male coworkers by around 1 and 1.8 percent in the first two years after the reform. While we are not able to provide conclusive evidence on the mechanism behind the wage increase, the results are consistent with an increased demand for the remaining workers' labor, suggesting firm-specificity of human capital.

Overall, the evidence provided in this paper points to the existence of sizeable adjustment costs for firms when workers go on extended parental leave. These findings may have important implications for the overall gender wage gap, to the extent that employers pass through such costs on the wages of women – who take the bulk of leave to care for young children. Because family leave entitlements are widely considered as key policy instruments to promote gender equality in the labor market, it is important to quantify any unintended consequences that may potentially undermine the policy goals. An important avenue for future research thus lies in analyzing the equilibrium effects of family policies in firms' wage offers (and other employment decisions) towards men and women.

Finally, we note that the public sector firms in our sample also experienced a substantial labor supply reduction due to the 1989 reform, but re-adjustments were limited and only minimally offset the labor shortage. The limited responses in the public sector may be driven both by budget constraints and by its reliance on licensed occupations that are hard to replace, such as nurses and teachers. Irrespective of the mechanism, labor shortages in the public sector may have important implications for the quality of service delivery, and thus deserve closer attention in future research.

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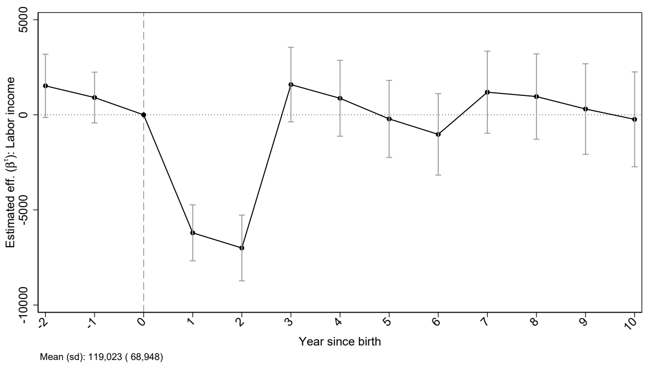
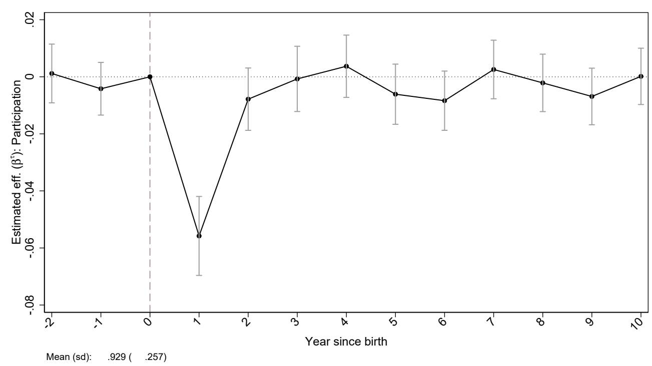
FIGURE 1.
Effects of extended entitlement to paid leave on female labor income, participation, and job separations

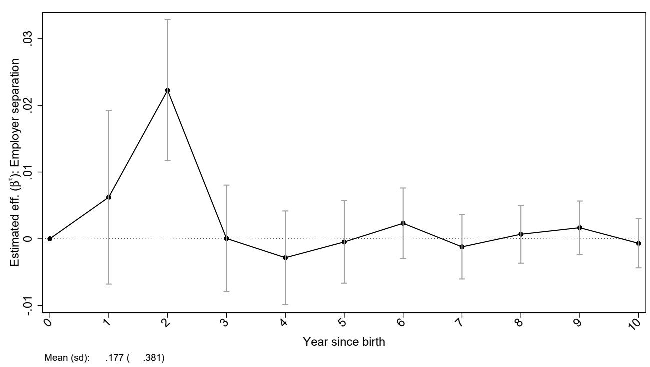
(A)
Labor income



(B)
Participation

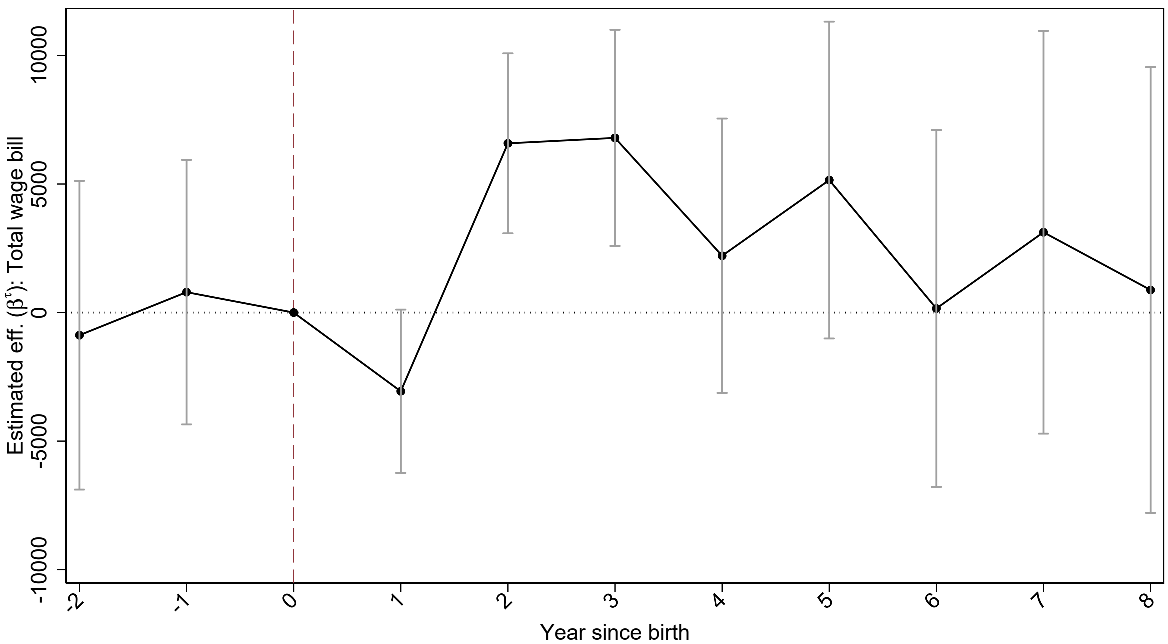


(C)
Job-to-job transitions



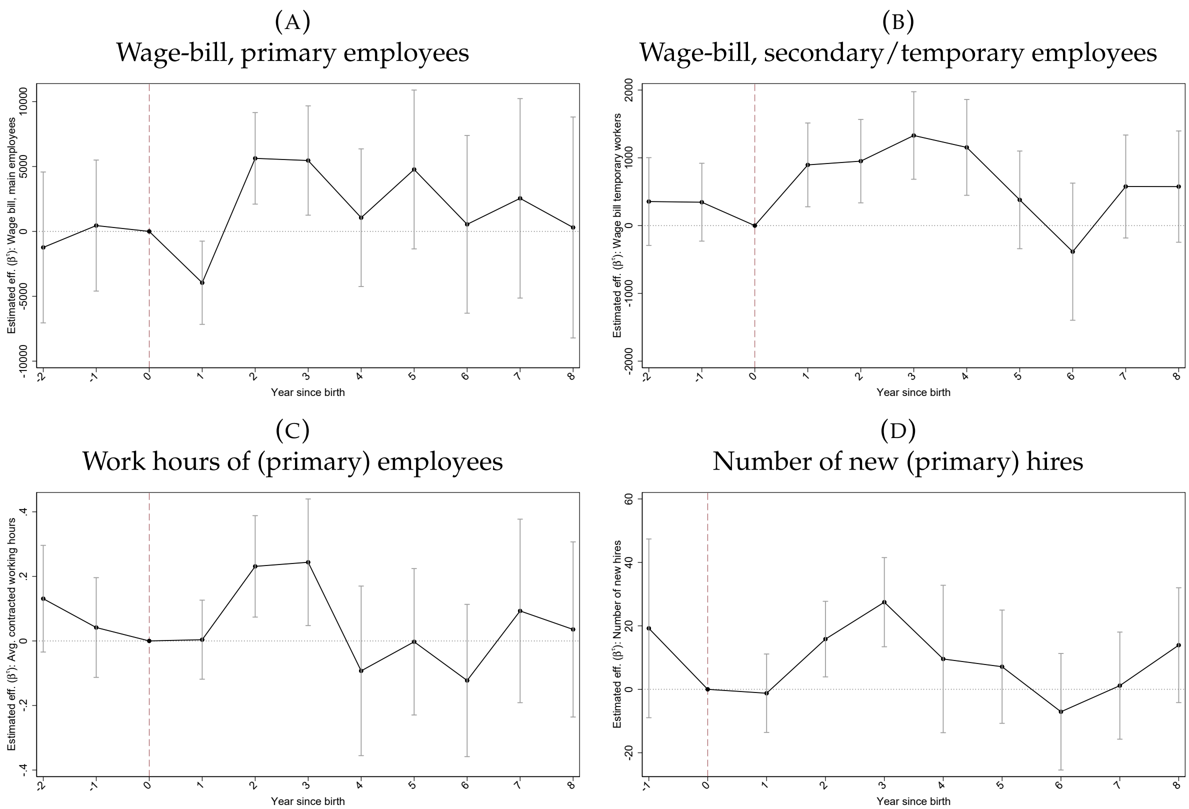
NOTE: The graph reports difference-in-differences estimates of the 1989 reform on female worker's labor supply. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for having a child in October–December (relative to January–July), an indicator for having a child born in the treatment year of 1988 (relative to 1987), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from equation (1). 95% confidence intervals are shown by the vertical lines on each point estimate.

FIGURE 2.
The effect of the extended parental leave program on firm's total wage costs



NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' total wage bill. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from Equation 2, along with the 95% confidence intervals. The outcome variable, firm's total wage bill, is expressed in 1000s SEK. The the average firm size at baseline is 45 workers.

FIGURE 3.
Decomposing employer responses: primary vs. secondary replacement workers; hours vs. new hires

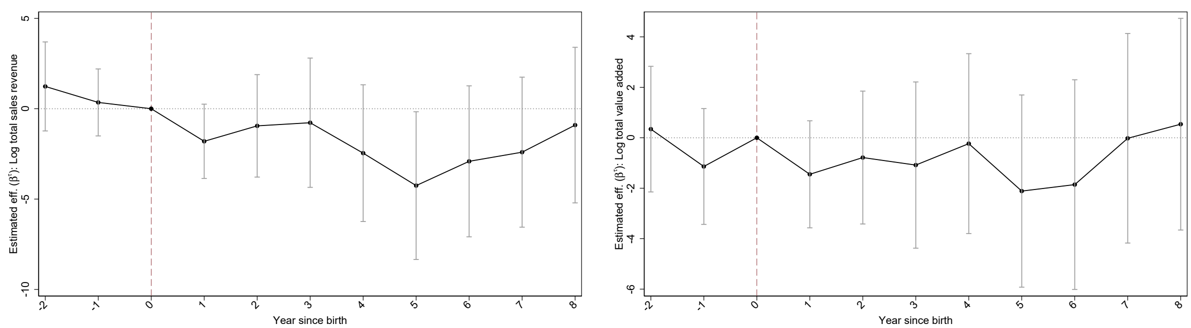


NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from Equation 2, along with the 95% confidence intervals. The firm's wage bill outcomes are expressed in 1000s SEK. The average firm size at baseline is 45 workers.

FIGURE 4.
Effects of the reform on firm performances in the manufacturing sector

(A)
Log total sales

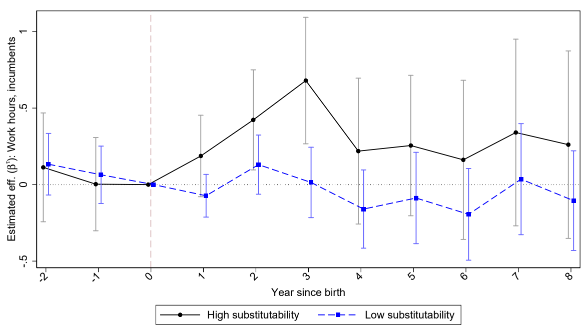
(B)
Log total value added



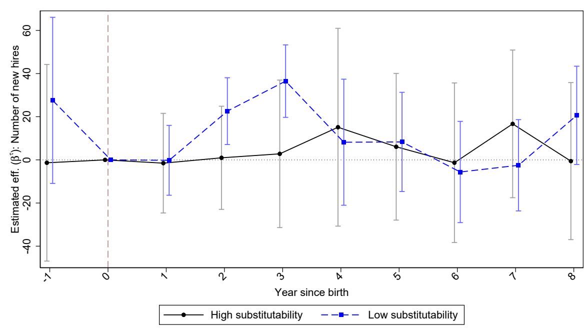
NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^\tau$ from Equation 2, along with the 95% confidence intervals. The firm's revenues and value added measures are expressed in 1000s SEK.

FIGURE 5.
Heterogeneous employer responses by internal and external labor market conditions

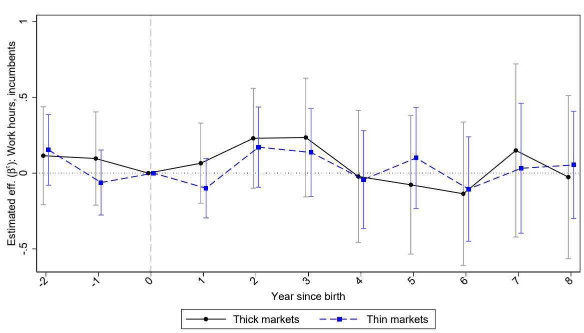
(A)
Work hours of incumbent employees: firms with high vs. low substitutability



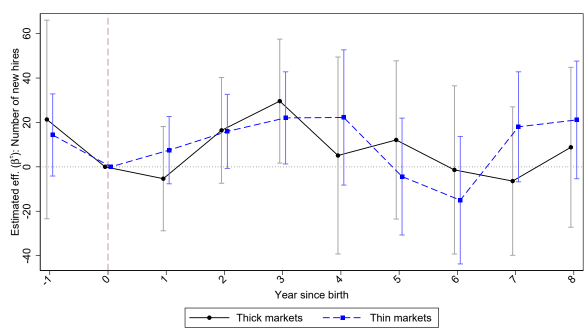
(B)
New (primary) hires: firms with high vs. low substitutability



(C)
Work hours of primary employees: thick or thin labor markets

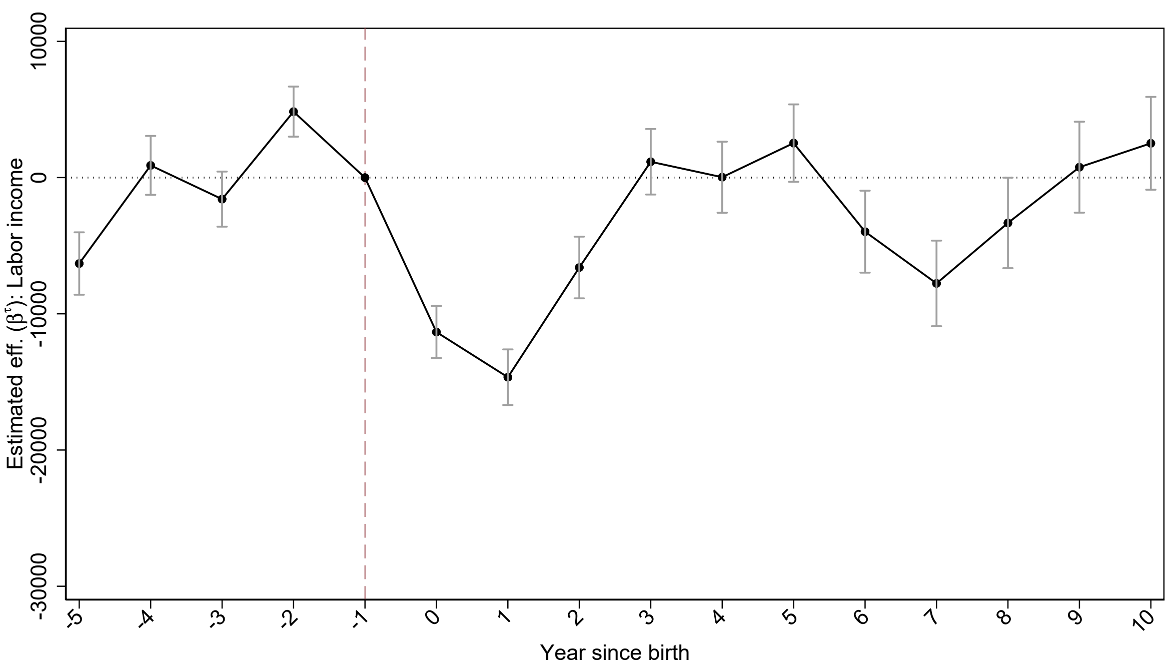


(D)
New (primary) hires: thick or thin labor markets



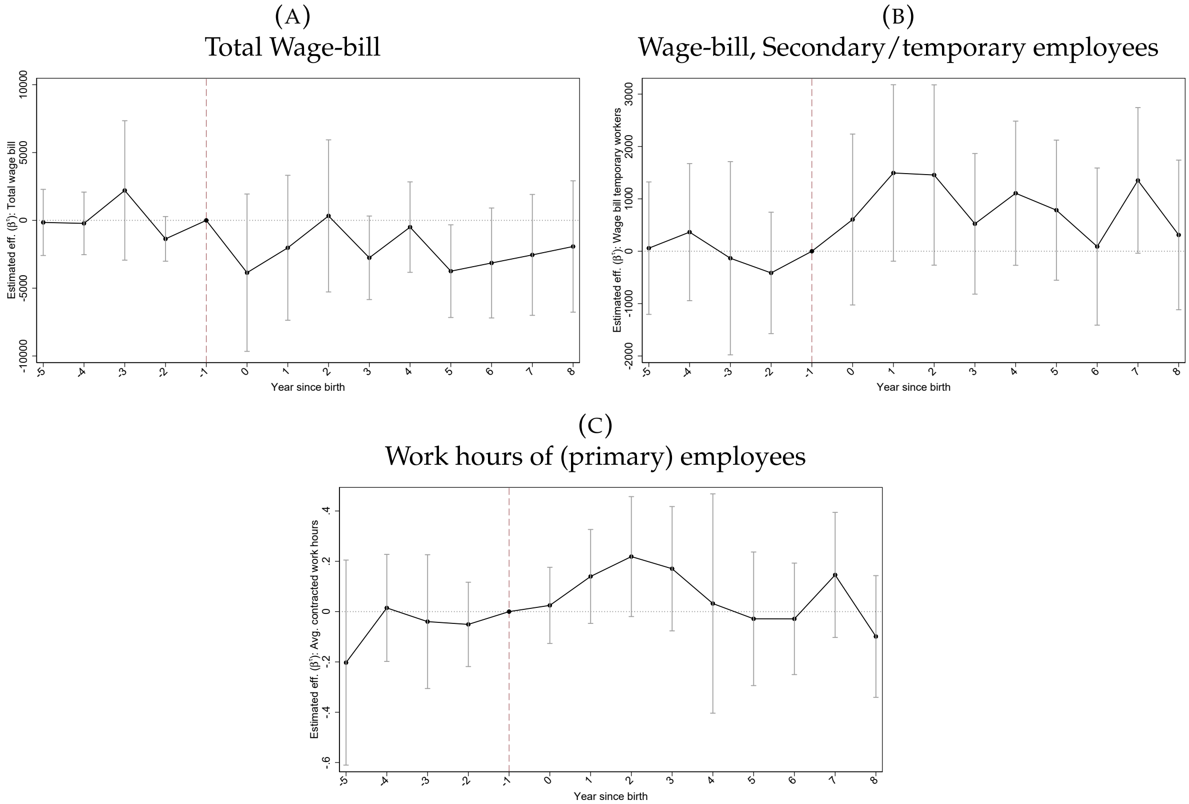
NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on an interaction term consisting of an indicator for employing women who gave birth to a child in 1988 or 1987, the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. 95% confidence intervals indicated by vertical lines. The firm's wage bill outcomes are expressed in 1000s SEK.

FIGURE 6.
The effect of the 2002 parental leave reform on male labor supply



NOTE: The graph reports difference-in-differences estimates of the 2002 reform on fathers' labor supply. Each point in the graph represents the coefficient on an interaction term consisting of an indicator for having a child born in 2002 (relative to the same calendar month in 2001) and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from Equation 3, along with the 95% confidence intervals.

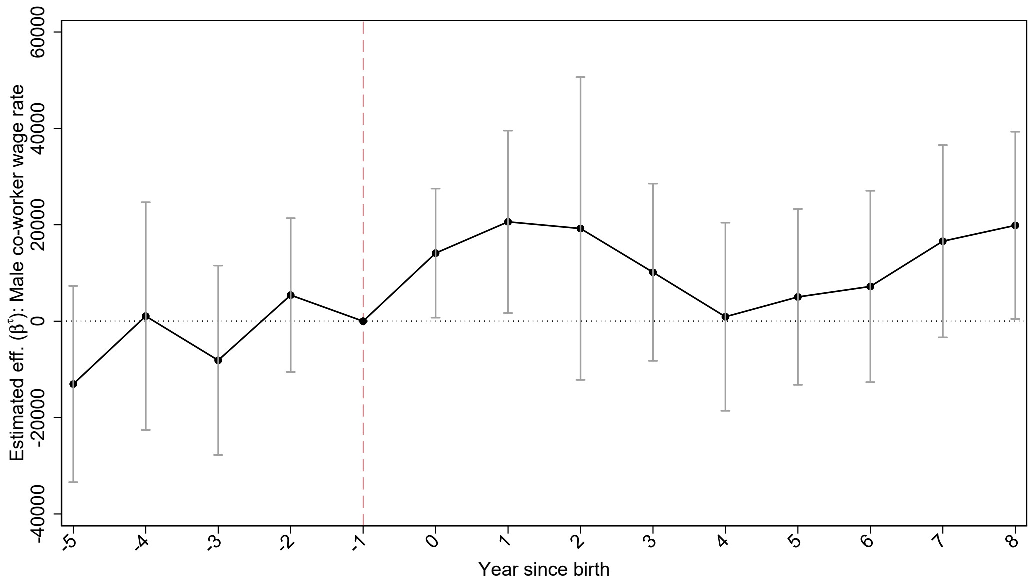
FIGURE 7.
Employer responses to the male workers' leave: Effects of the 2002 daddy-month introduction



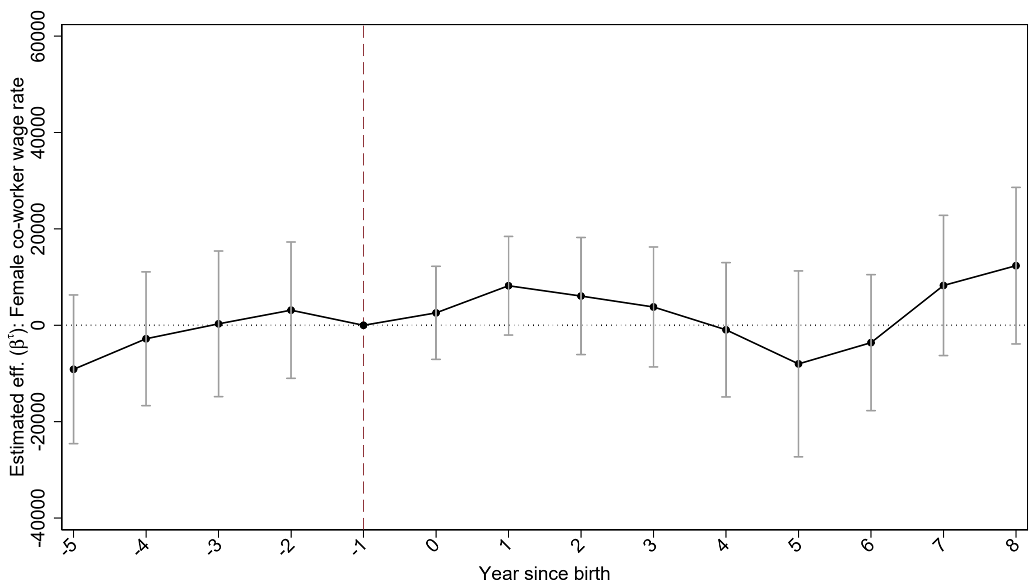
NOTE: The graph reports difference-in-differences estimates of the 2002 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing men whose child was born child in 2002/2001 (relative to 2001/2000), the proportion of the workforce whose child was born in 2002 (2001), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from Equation 2, along with the 95% confidence intervals. The firms' wage bill outcomes are expressed in 1000s SEK.

FIGURE 8.
Effects of the 2002 daddy-month reform on remaining coworkers' wage rates

(A)
Male coworkers' wage rate



(B)
Female coworker's wage rate

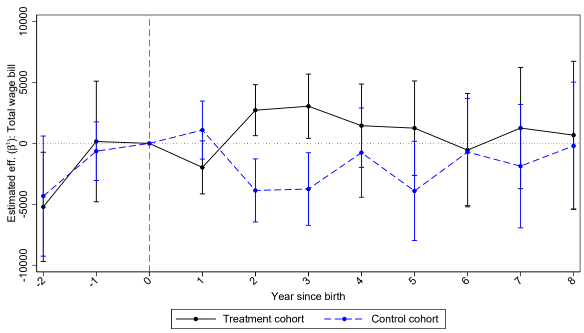


NOTE: The graph reports difference-in-differences estimates of the 2002 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing men whose child was born child in 2002/2001 (relative to 2001/2000), the proportion of the workforce whose child was born in 2002 (2001), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from Equation 2, along with the 95% confidence intervals. Wage rates are expressed as monthly full-time equivalent wages in SEK (averaged over all workers at the firm excluding the male workers who had a child born in 2002 (2001)).

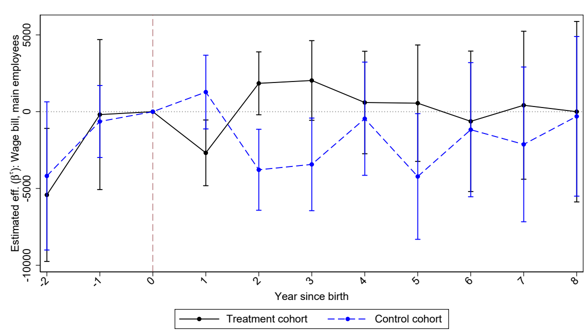
A Additional Tables and Figures

FIGURE A.1.
Employer responses by treatment- and control-cohort firms separately

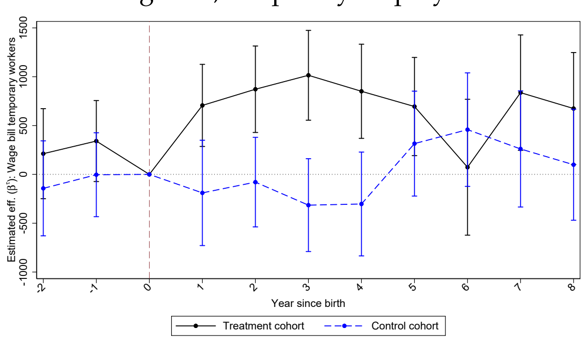
(A)
Total Wage-bill



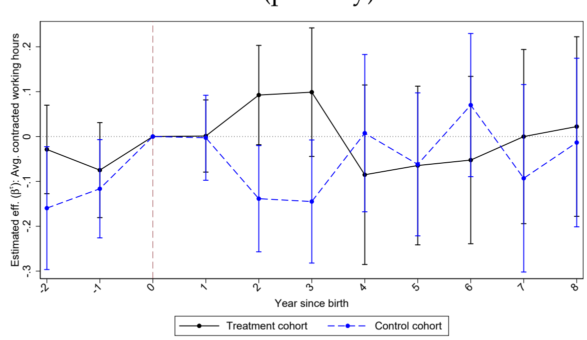
(B)
Wage-bill, primary employees



(C)
Wage-bill, temporary employees



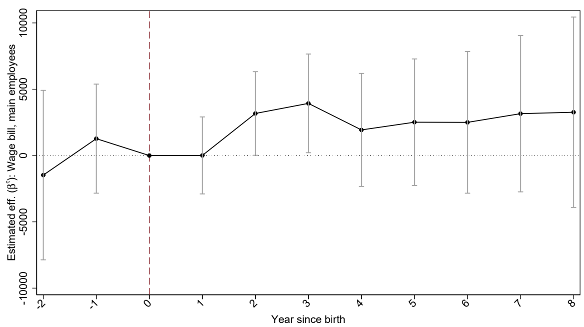
(D)
Work hours of (primary) coworkers



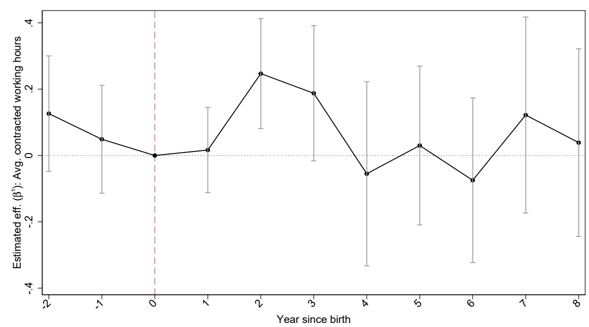
NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 or 1987, the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. 95% confidence intervals indicated by the vertical lines. The firm's wage bill outcomes are measured in 1000s SEK.

FIGURE A.2.
Heterogeneous employer responses by firm size

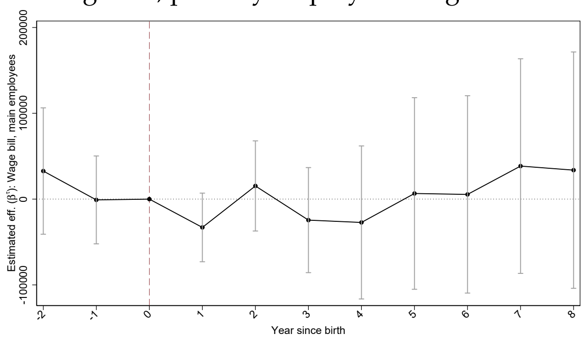
(A)
Wage-bill, primary employees: small firms



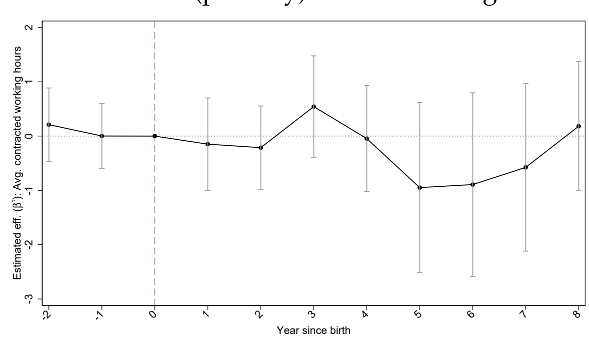
(B)
Work hours of (primary) coworkers: small firms



(C)
Wage-bill, primary employees: large firms



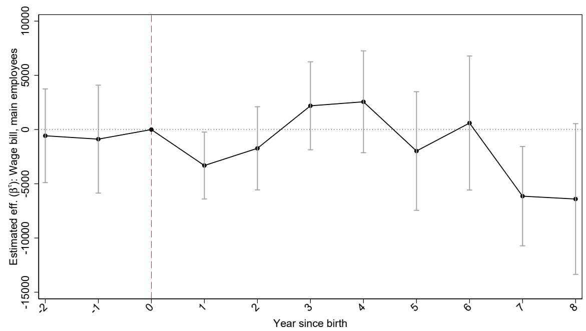
(D)
Work hours of (primary) coworkers: large firms



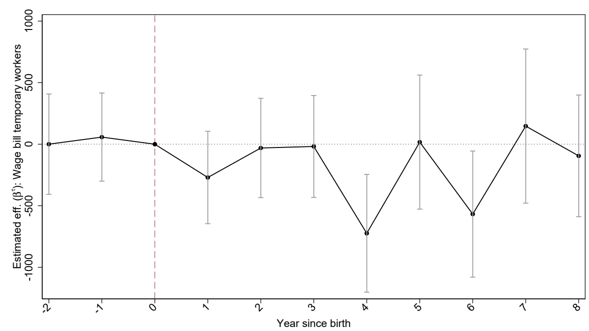
NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from Equation 2, along with the 95% confidence intervals. The firm's wage bill outcomes are measured in 1000s SEK.

FIGURE A.3.
Decomposing employer responses: primary workers' hours increases or temporary replacement workers? Public sector

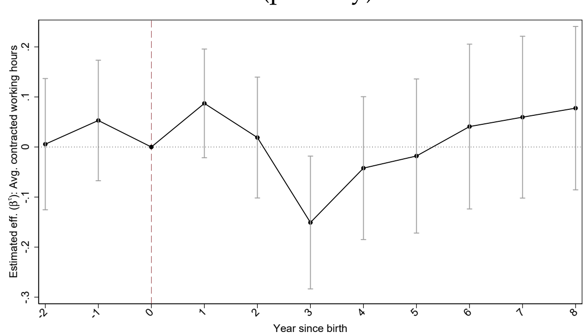
(A)
Wage-bill, primary employees



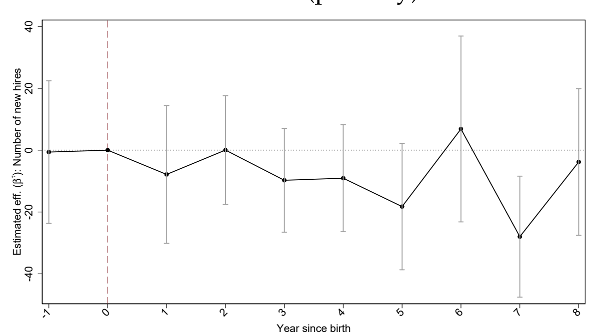
(B)
Wage-bill, secondary/temporary employees



(C)
Work hours of (primary) coworkers



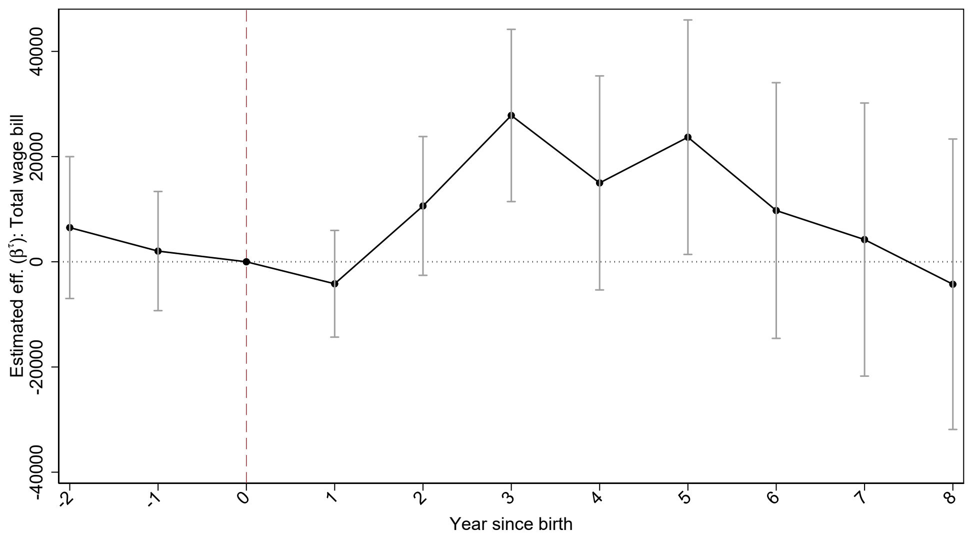
(D)
Number of new (primary) hires



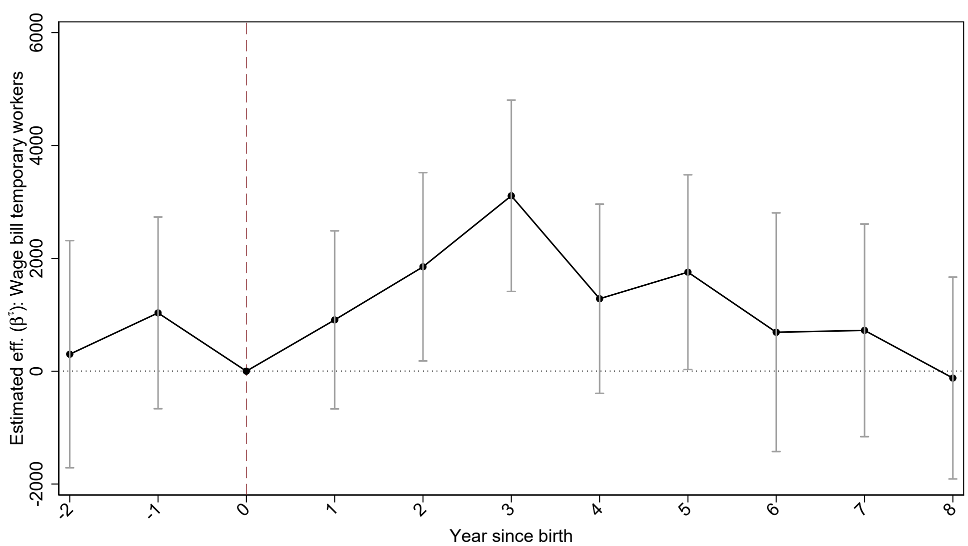
NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^\tau$ from Equation 2, along with the 95% confidence intervals. The firm's wage bill outcomes are measured in 1000s SEK.

FIGURE A.4.
Effects of the reform on manufacturing firms

(A)
Total wage bill



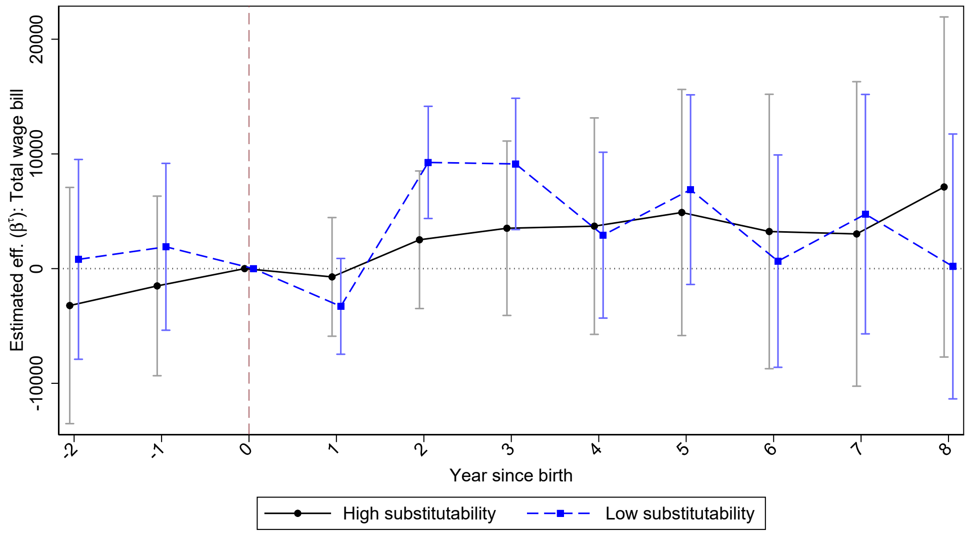
(B)
Wage bill paid to secondary/temporary workers



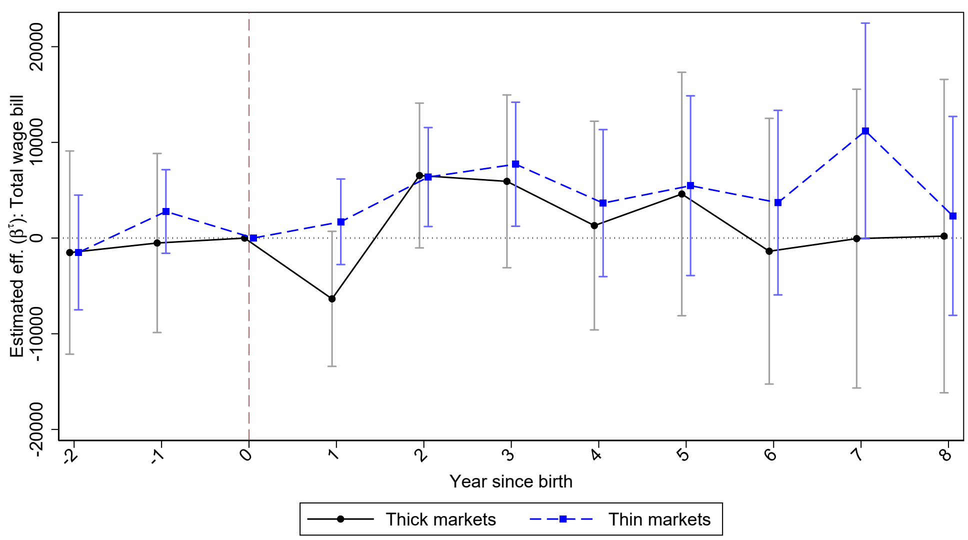
NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^\tau$ from Equation 2, along with the 95% confidence intervals. The firms' wage bill measures are expressed in 1000s SEK.

FIGURE A.5.
Heterogeneity in total wage bill costs by external and internal labor market conditions

(A)
Total wage bill: firms with high vs. low substitutability



(B)
Total wage bill: thick or thin markets



NOTE: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the x-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from Equation 2, along with the 95% confidence intervals. The firm's wage bill is measured in 1000s SEK.

TABLE A.1.
Summary statistics: Workers' pre-determined characteristics (by treatment status)

	Control cohort (1987)			Treatment cohort (1988)			DD	
	(1) Jan-July	(2) Oct-Dec	(3) *t*-stat for (1)-(2)	(4) Jan-July	(5) Oct-Dec	(6) *t*-stat for (4)-(5)	(7) DD est. of [(1)-(2)] - [(4)-(5)]	(8) *t*-stat for [(1)-(2)] - [(4)-(5)]
Age	28.713	28.149	-13.659	28.602	28.110	-12.318	0.073	1.263
No college	0.737	0.749	3.036	0.746	0.751	1.554	-0.006	-1.118
College	0.263	0.251	-3.036	0.254	0.249	-1.554	0.006	1.118
Labor earnings (1000s SEK)	117.991	114.897	-5.935	119.506	116.935	-4.944	0.524	0.710
Monthly wage (1000s SEK)	15.743	15.738	-0.120	16.386	16.343	-0.979	-0.038	-0.624
Contracted work hours	0.846	0.838	-2.705	0.846	0.850	1.416	0.013	2.941
Private sector	0.336	0.352	4.008	0.356	0.374	4.533	0.002	0.303
Child parity	1.821	1.804	-2.207	1.823	1.807	-2.124	0.001	0.102
Child spacing	28.595	28.075	-1.669	27.658	27.165	-1.683	0.028	0.065
Observations	56,423	19,918		60,147	21,322		157,810	

NOTES: The sample includes women who gave birth in 1987 and 1988, who earned at least SEK 10,000 in the calendar year prior to birth, who did not give birth in the months of August or September, and employed at workplaces with at least 10 employees.

TABLE A.2.
Summary statistics for all firms & organizations active in Sweden, and for firms in study sample

	All workplaces (mean)	All (sd)	Sample workplaces (mean)	Sample workplaces (sd)
Private sector	1.000	0.000	1.000	0.000
Tradable industry	0.265	0.441	0.251	0.434
Share female	0.376	0.279	0.500	0.261
Number of births	0.550	1.287	1.423	0.967
Share compulsory schooling	0.419	0.208	0.416	0.211
Share with high school	0.479	0.167	0.469	0.161
Share workers with some college	0.057	0.087	0.059	0.088
Share workers with college	0.045	0.102	0.056	0.112
Workplace size	38.274	52.492	49.144	57.189
Average age	35.771	5.866	35.423	5.808
Average contracted working hours	0.952	0.079	0.957	0.067
Female contracted work hours	0.905	0.127	0.919	0.109
Male contracted work hours	0.984	0.042	0.983	0.037
Average monthly wage (SEK)	20,379.28	4,333.96	19,840.44	4,259.47
Female monthly wage (SEK)	17,813.72	2,875.81	17,448.22	2,788.60
Male monthly wage (SEK)	22,519.52	5,567.73	22,214.74	5,580.74
Female annual earnings (SEK)	130,031.53	56,744.79	125,298.39	50,806.04
Male annual earnings (SEK)	190,820.21	89,082.05	192,533.02	96,125.15

NOTES: Columns (1) and (2) report the means and standard deviations, respectively, for all private sector firms active in Sweden in 1988, and the characteristics are measured in 1988. Columns (3) and (4) report the means and standard deviations of characteristics for the workplaces in our sample, which consists of establishments that employ at least one woman in 1988 (treatment year) or 1987 (control year), and who employ at least 10 people in the baseline year. The characteristics for the study sample of firms are measured in the baseline year of the respective cohorts, i.e., in year 1988 for the treatment firms and in 1987 for the control group firms.

TABLE A.3.
Industry mix for all private sector firms & organizations active in Sweden, and for firms in study sample

	All workplaces		Sample workplaces	
	# of workplaces	% workplaces	# of workplaces	% workplaces
Armed forces	1,596	4.076	674	4.677
Agriculture, hunting, forestry	671	1.714	203	1.409
Fishing	14	0.036	1	0.007
Mining and quarrying	139	0.355	32	0.222
Manufacturing	9,306	23.766	3,321	23.046
Electricity, gas and water	265	0.677	48	0.333
Construction	4,018	10.261	423	2.935
Wholesale and retail trade etc	10,445	26.675	4,231	29.362
Hotels and restaurants	2,230	5.695	1,089	7.557
Transport and communications	2,187	5.585	572	3.969
Financial intermediation	1,345	3.435	684	4.747
Real estate, renting, other	730	1.864	243	1.686
Data management operations	509	1.300	209	1.450
R&D	91	0.232	40	0.278
Other business activities	2,713	6.929	1,172	8.133
Public adm., defense, social insurance	34	0.087	20	0.139
Education	773	1.974	386	2.679
Health and social work	642	1.640	355	2.464
Lobbying, and religious activities	592	1.512	302	2.096
Recreation, culture, sports	857	2.189	405	2.811
Total	39,157	100	14,410	100

NOTES: Columns (1) and (2) report the industry composition for all firms active in Sweden in 1988. Columns (3) and (4) report industry composition for the workplaces in our sample, which consists of establishments that employ at least one woman in 1988 (treatment year) or 1987 (placebo year).

TABLE A.4.

Summary statistics: Firms' pre-determined characteristics (by treatment status). Sample: firms with 10-20 employees, and only 1 woman giving birth in the baseline year

	Control cohort (1987)			Treatment cohort (1988)			DD	
	(1) Jan-July	(2) Oct-Dec	(3) t-stat for (1)-(2)	(4) Jan-July	(5) Oct-Dec	(6) t-stat for (4)-(5)	(7) DD est. of [(1)-(2)] - [(4)-(5)]	(8) t-stat for [(1)-(2)] - [(4)-(5)]
Number of workers	14.335	14.232	-0.735	14.111	14.232	0.903	0.224	1.155
Number of female workers	8.268	8.192	-0.435	8.057	8.179	0.728	0.198	0.817
Number of male workers	6.067	6.040	-0.149	6.054	6.053	-0.007	0.025	0.104
Number women aged 20-40	3.604	3.561	-0.394	3.493	3.508	0.150	0.059	0.388
Average age	35.302	34.616	-2.479	35.179	34.908	-0.998	0.414	1.065
Share female	0.548	0.548	0.008	0.541	0.546	0.411	0.005	0.278
Average monthly wage (SEK)	19,000	19,000	0.489	19,000	19,000	-0.762	-376.754	-0.880
Average female monthly wage (SEK)	17,000	17,000	-0.119	17,000	17,000	-0.766	-137.561	-0.447
Average male monthly wage (SEK)	21,000	22,000	0.889	22,000	21,000	-1.526	-1216.215	-1.690
Female contracted work hours	0.924	0.933	0.957	0.928	0.927	-0.100	-0.010	-0.768
Male contracted work hours	0.983	0.983	0.225	0.983	0.980	-1.055	-0.004	-0.896
Wage bill, primary employees (1000s SEK)	2,100	2,100	0.605	2,100	2,100	-0.323	-45.821	-0.657
Wage bill, temporary workers (1000s SEK)	188.781	181.232	-0.491	225.762	196.467	-0.999	-21.745	-0.638
Share no college	0.888	0.879	-1.122	0.876	0.887	1.486	0.020	1.840
Share college	0.112	0.121	1.122	0.124	0.113	-1.486	-0.020	-1.840
Observations	1,795	693		2,056	738		5,282	

NOTES: The sample includes firms with 10–20 employees in the baseline year, out of which exactly 1 woman gave birth.

TABLE A.5.
Summary statistics: Outcomes measured at baseline)

	Control cohort firms	Treatment cohort firms
Workplace size (baseline)	48.205	45.776
Treatment intensity	0.014	0.015
Total wage bill (1000s SEK)	8,400	8,100
Primary wage bill (1000s SEK)	7,900	7,600
Temp wage bill (1000s SEK)	518.440	534.407
Incumbent work hours	0.956	0.958
Incumbent wage rate	19,596	20,014
Observations	7,982	8,653

NOTES: The sample includes private sector firms, and the characteristics/outcomes displayed in the table are measured at baseline ($\tau = -1$).

TABLE A.6.
Summary statistics for the subset of firms with observations on sales revenue and value added measures

	Mean	Standard deviation
Tradable industry	0.967	0.177
Share female	0.354	0.218
Number of births	1.172	1.568
Share compulsory schooling	0.466	0.171
Share with high school	0.453	0.134
Share workers with some college	0.059	0.067
Share workers with college	0.023	0.047
Workplace size	63.991	58.469
Average age	37.153	4.809
Average contracted working hours	0.945	0.067
Female contracted work hours	0.884	0.116
Male contracted work hours	0.983	0.035
Average monthly wage (SEK)	21,251.972	3,324.396
Female monthly wage (SEK)	17,810.421	2,408.935
Male monthly wage (SEK)	23,349.065	3,959.676
Female annual earnings (SEK)	132,806.812	37,902.761
Male annual earnings (SEK)	196,857.314	49,450.665
Sales per worker (1000s SEK)	1,025.382	1,146.283
Value added per worker (1000s SEK)	484.244	495.747

NOTES: Columns (1) and (2) report the means and standard deviations, respectively, for all firms and public sector organizations active in Sweden in 1988, and the characteristics are measured in 1988. Columns (3) and (4) report the means and standard deviations of characteristics for the workplaces in our sample, which consists of establishments that employ at least one woman in 1988 (treatment year) or 1987 (placebo year), and who employ at least 10 people in the baseline year. The characteristics for the study sample of firms are measured in the baseline year of the respective cohorts, i.e., in year 1988 for the treatment firms and in 1987 for the control group firms.

B Parental Leave Benefit Take-up

Our auxiliary data on parental leave benefits covers the universe of parental leave spells (start- and end-dates) at the individual level, but are subject to a few caveats. First, data on leave spells start in 1988. Second, leave spells recorded before 1994 only contains identifiers only for the parents, not for the child for whom the leave is taken. Because of these limitations, we sample workers to *first-born* children in 1988 and 1989. Looking at take-up after the first child is born implies that we are unlikely to confound parental leave spells for multiple children in the household. Under the assumption that the reform did not affect subsequent fertility, we can interpret the medium-run potential differences in take-up between the treated and untreated cohorts as a direct reform effect.[28] Second, since we lack data on take-up before 1988, parents to kids born in 1989 will serve as the control group. While all parents of the latter group are treated, there should be no difference in the leave take-up between those who give birth in different calendar months within 1989.

Let T_i be an indicator that takes the value 1 if individual i's child was born in October–December, and zero if her child was born in January–July. let D_i take the value 1 for workers whose child was born in 1988, and 0 for those whose child was born in 1989, and let s denote the age of individual i:s first child in months. We estimate the following regression specification by OLS:

$$y_{ia} = \delta_0 + \beta_a \big(T_i \cdot D_i\big) + \delta_1 T_i + \delta_2 D_i + \nu_{ia} \tag{4}$$

We estimate (4) on the number of (gross)[29] days on parental leave separately for each month after the birth of i:s first child. Panel A of Figure B.1 plots the estimated coefficients $\widehat{\beta}_a$:s from equation (4) for women. The results show that women used most of the additional leave during the child's second year of life, but leave days also increased during year three. In Panel B of Figure B.1 we show that some of the additional leave was also taken-up by fathers, but considerably less than among women.

In Table B.1 we present estimates of the effect of the reform on take-up pooled over the first four years of the child's life, separately by gender and sector of employment. We find that eligible women increased their leave take-up by 78 days on average, while eligible male workers increased leave-taking by 8 days, on average. Thus, the reform had full impact on take-up. Moreover, we conclude that there is virtually

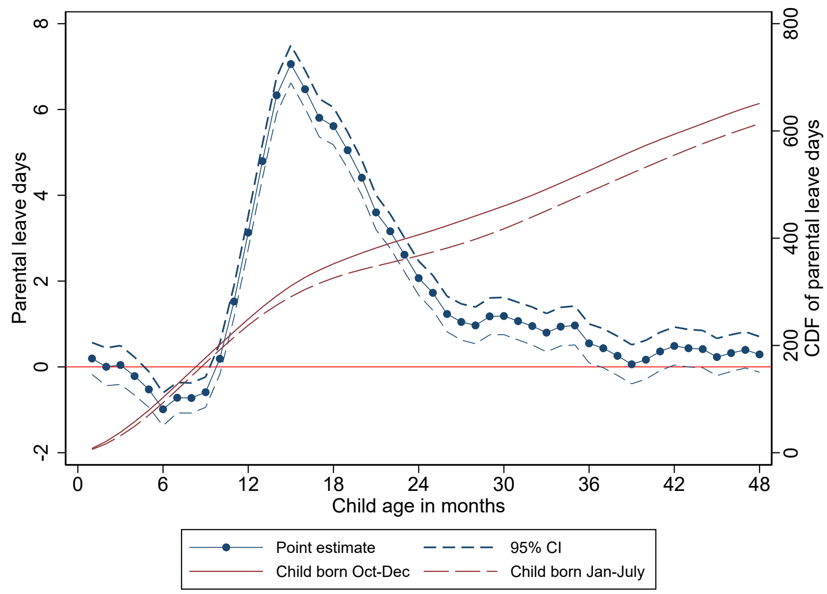
[28] In Table B.2 we report results from estimating a difference-in-difference model comparing the completed fertility of women that are eligible to the additional three months of leave to that of non-eligible mothers, netting out seasonality in the outcome variable by birth month using the sample of individuals with a child born in 1987. We find no evidence suggesting that the reform affected subsequent fertility.

[29] Benefit can be collected on a part-time basis, e.g., 50 percent of a day. We do not have information on the intensity of benefit usage, so we are unable to calculate net benefit days.

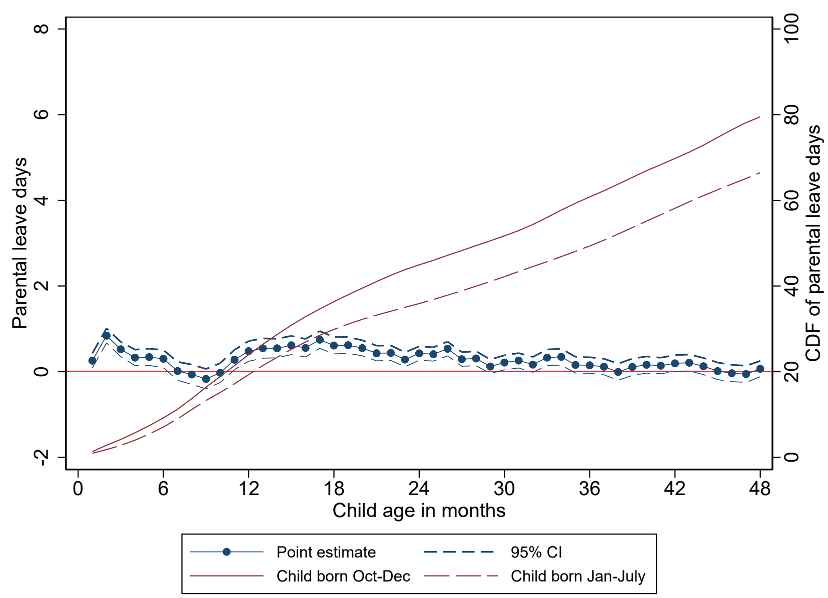
no difference in the effect of the reform on female take-up by sector of employment, but that male public sector workers made more use of the additional leave relative to male private sector workers.

FIGURE B.1.
Effects of extended entitlements to paid leave on the take-up of parental leave

(A)
Female PL take-up



(B)
Male PL take-up



NOTE: The graph reports difference-in-differences estimates of the 1989 parental leave reform on take-up. Each point in the graph represents the coefficient on an interaction term between having a child born in October–December (relative to January–July), and an indicator between having a child in 1988 (relative to 1989), estimated separately for each month since the child was born. Thus, the points correspond to the estimated coefficients β_a from equation (4), which capture the difference in parental leave take-up between workers giving birth in October–December and January–July 1988, net of the corresponding difference among workers whose child was born in 1989. 95% confidence intervals are indicated with dashed lines. The right-hand-side y-axis shows the cumulative distribution function of leave take-up among parents to children born in the first- and second half of 1988.

TABLE B.1.
Effects of the reform on parental leave take-up days by gender and sector of employment

	(1) All	(2) Private	(3) Public
A. Female take-up			
$D_i \times T_i$	77.497***	77.704***	73.323***
	(5.707)	(9.156)	(7.680)
Observations	78,423	29,734	41,049
B. Male take-up			
$D_i \times T_i$	7.982***	5.856**	16.718***
	(2.432)	(2.737)	(5.109)
Observations	50,052	34,017	13,760

NOTES: The table reports the estimated coefficient β_a from equation (4) where the outcome measures the total days of parental leave benefit take-up until the child's eighth birthday (full period during which leave can be used). The estimation includes flexible controls for age, educational level measured in the year that i gives birth (compulsory schooling, high school, some college, and college degree), and the average earnings in the two years before giving birth. Robust standard errors in parentheses.

TABLE B.2.
Effects of the reform on completed fertility

	All	Private sector	Public sector
$D_i \times T_i$	0.001	0.010	0.013
	(0.015)	(0.023)	(0.020)
Observations	78,423	29,734	41,049

NOTES: The sample includes women who gave birth in 1987 and 1988, who earned at least SEK 10,000 in the calendar year prior to birth, and who did not give birth in the months of August or September. The outcome variable measures the total number of children born to a person by year 2017. The table reports estimates of $\hat{\beta}$ from the following equation:

$$y_i \quad = \quad \delta_0 + \beta(T_i \times D_i) + \delta_1 T_i + \delta_2 D_i + \mathbf{X_i'}\gamma + \epsilon_i$$

where T_i is an indicator that takes the value 1 if person i had a child born in October–December and 0 if person i's child was born in January–July. Robust standard errors in parentheses.