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# ABSTRACT

## Employer Responses to Family Leave Programs*

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 incumbent hours, incurring additional wage costs. These adjustment costs varied by firms' availability of internal and external substitutes. Economy-wide analyses show that a higher reform exposure is correlated with fewer hires and lower starting wages of young women compared to men and older women.

**JEL Classification:** J13, J16, J21, J22, J31

**Keywords:** parental leave, firm-specific human capital, statistical discrimination

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# 1 Introduction

Men and women are offered different wages even when they have similar education and experience backgrounds, and work in the same occupation and firm (see Figure A.1). One often suggested explanation is women's weaker labor force attachment and higher absence rates compared to men, especially after having children.[1] Turnover is costly for firms in the presence of search frictions and firm-specific human capital, so employers might transfer expected future costs of turnover into lower wages for women, or avoid hiring or promoting women into certain positions. Although a large theoretical literature has investigated the mechanisms behind such statistical discrimination against women (see Barron, Black, and Loewenstein (1993), Bowlus (1997)), it is in practice difficult to measure the frictions faced by firms, let alone separating the role of turnover costs from other confounding factors affecting employers' decisions towards men and women.

A unique setting in Sweden allows us to exploit random variation in workers' turnover and absence, and quantify their impacts on firms. Like many developed countries, Sweden provides generous family leave to new parents, and women spend a much longer time in parental leave than men.[2] While family leave entitlements foster stable employment of women after childbirth, they might also impose organizational challenges to firms. 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 the extent of firms' differential treatment of men and women.

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.

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

[1] See Angelov, Johansson, and Lindahl (2016),Kleven, Landais, and Søgaard (2019),Hotz, Johansson, and Karimi (2017) for evidence of the effect of children on women's labor supply.

[2] 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).

reform was unanticipated and retroactive: it was implemented in July 1989 but retroactively covered 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 subset of firms that had employees who gave birth around the cutoff date granting eligibility to the new rules.

Since employer response depend on the extent and timing of workers' take-up of the intervention, we first quantify the impact of the reform on individual labor supply and job mobility. Using an auxiliary data set on parental leave spells we show that, on average, eligible mothers took up 2.5 months out of the 3 months of additional leave, while the increase in male take-up was only one week, on average. Thus the reform predominantly altered the leave duration of new mothers. 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. Moreover, we find evidence suggesting that women with fewer potential substitutes within the workplace used up less leave and shifted take-up to their spouses, indicating that workers internalize some of their employer's costs of finding suitable replacement. 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 the sample of 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 them to three additional months of leave. Because the reform was retroactive and unanticipated, women could not have manipulated the timing of birth, and firms could not have altered their workforce composition in 1988. Thus, treatment intensity at the firm level is plausibly orthogonal to the unobserved determinants of the outcomes that we study. We compare firms 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 temporary staff, by hiring new permanent workers, and by increasing the work hours of incumbents. 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, having one additional worker going on extended leave increased the total wage bill by an amount corresponding to 60 percent of the salary cost of a full-time worker. Note that parental leave is financed through social security contributions, so the monetary cost for the employer that we document are related only to finding, hiring, and remunerating replacement staff.[3]

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. Focusing on the private sector, we find that firms in thick external local labor markets responded to the reform by predominantly relying on new hires, while keeping incumbents' work hours relatively unchanged. Firms in thin markets, on the other hand, resorted to internal hours increases. Furthermore, 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 occupational concentration relied relatively more on external labor inputs. Taken together, our findings highlight several sources of frictions associated with finding suitable replacement for workers on leave. Finally, using data on sales and productivity for firms in the manufacturing industry, we also find some evidence of declines in sales revenue and value added, although the results are statistically significant only at the 10 percent level. We note that among this subset of firms, we find similar re-organization of the personnel in response to the reform as in the main sample, suggesting that

[3]For the public sector workplaces, there is no discernible pattern that would indicate adjustment or reorganization of the workforce. Given that workers in both the public and private sectors worked 2.5 months less due to the reform, the heterogeneity in employer responses by sector is not likely to be driven by differences in the size of the labor supply shortage. The inability of public sector workplaces to adjust to new circumstances may have implications for the outcomes of these institutions, if labor shortages affect firm productivity or the quality of output (see e.g. Friedrich and Hackmann, 2017).

replacement staff may not be as productive as the women on leave.

Of course, the parental leave expansion did not just affect the firms that happened to employ women giving birth in the reform year, as discussed in our analysis above. The policy reform would affect all firms employing women of childbearing ages, as they would take additional months off work at some point in the future and potentially incur additional adjustment costs.[4]

To shed light on the equilibrium effects of the policy, we perform a descriptive analysis of the hiring and wage setting behaviors of all firms in the economy before and after the policy intervention. To this end, we compare the economy wide hiring rates, wage offers, and promotions of workers in the at-risk population relative to other workers before and after the policy change. We contrast these quantities across industries and local labor markets that are differentially exposed to the reform, measured by the (pre-reform) age-specific fertility rates and the demographic composition of the worker-pool just before the intervention. We find that after the reform was implemented, local industries with higher predicted exposure exhibited lower promotion rates, lower hiring rates, and lower starting wages of new hires for women of childbearing ages compared to male and older female workers. Moreover, the overall gender wage gap widened more after the reform in industry-localities that were more exposed. While this analysis is not causal, it provides suggestive evidence that the reform may have had unintended consequences for women's employment and promotion probabilities.

Our paper contributes to three strands of literature. We contribute to empirical work on firms' ability to find substitutes for their workers when they leave their firm, which depends on the degree of specificity of human capital and on external labor market conditions. 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. Moreover, in contrast to much of the previous work using worker exits to assess human capital specificity, productivity, or employer outcomes (see e.g. Jaravel, Petkova, and Bell, 2018; Bartel, Beaulieu, Phibbs, and Stone, 2014; Friedrich and Hackmann, 2017), we study impacts for firms in the overall econ-

[4]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, Ruhm, and Waldfogel, 2009; Kluve and Tamm, 2013; Rossin-Slater, Ruhm, and Waldfogel, 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).

[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 Friedrich and Hackmann (2017) on hospitals' and nursing homes' ability to replace nurses after a large expansion in parental leave entitlements in Denmark.

omy, 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) and Friedrich and Hackmann (2017) who both study the effect of parental leave expansions on employer outcomes. Gallen (2019) finds an increase in the probability of firm closures among small workplaces in Denmark, and some evidence that it causes strain on remaining co-workers. Friedrich and Hackmann (2017) find that the reduction in labor supply of nurses after a parental leave expansion in Denmark had a negative impact on patient outcomes in Danish hospitals and health centers. In a related paper, Brenøe, Canaan, Harmon, and Royer (2020) study the impact of child-related leave, per se, on small firms using variation in women's year of birth combined with matching techniques to define control events. Their results show that firms increase remaining workers' hours and an increase in their wage bill, which are in line with some of our findings. Our paper complements this literature by studying the substitutability of various labor inputs, both within incumbents and between incumbents and external workers, and thus providing evidence on several potential sources of frictions associated with labor turnover.

Third, our paper relates to the literature linking job protection to statistical discrimination. Gruber (1994) exploits regional variation 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 more likely to remain employed, but less likely to be promoted. The firm-level analysis in our paper provides results that are in line with the aggregate effects found in Thomas (2019). 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. Overall, our results are consistent with these studies, and suggest that the group of workers whom family policies are aimed to help ultimately may bear part of the costs of the policy.

## 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 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.[6]

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.[7] 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.[8] Thus, the overwhelming majority of women were insured at 80 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

[6] 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.

[7] 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.

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

vast majority of parental leave benefits is taken-up during the child's first two to three years of life (see Figure A.2 in the Appendix).

**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 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.[9]

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 according. 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

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

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 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 individual 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 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 a control year. Thus, we implement a difference-in-differences (DiD) methodology where the identifying assumption is that any birth month effects are similar across years.[10] We sample all women who give birth (irrespective of birth

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

parity) in 1988, which we denote the *treatment cohort*, and all women who give birth in an adjacent year, which we refer to as the *control cohort*.[11]

Let $M_{im}$ be an indicator for woman $i$ giving birth in calendar month $m$, where $m = \{1, ..., 12\}$. $D_i$ indicates whether mother $i$ gave birth in the treatment year. We analyze the effect of extended paid leave entitlement on woman $i$'s program take-up and labor supply outcome, denoted $y_i$, by estimating the following regression equation:

$$y_i = \delta_0 + \sum_{m=1, m \neq 7}^{12} \beta^m \left( M_{im} \cdot D_i \right) + \sum_{m=1, s \neq 7}^{12} \delta_1^m M_{im} + \delta_2 D_i + \mathbf{X}_i^{'} \gamma + \epsilon_i. \quad (1)$$

with $m = 7$ as the omitted category. The coefficients of interest are the $\beta^m$:s, which capture the difference in $y_i$ between individuals giving birth in calendar month $m$ compared to giving birth in July for those who gave birth in 1988, net of the corresponding difference among those who gave birth in the control year. If our identifying assumption holds there should be no significant differences in the outcomes of women giving birth in January–June relative to July across the treatment- and control-cohorts. If the reform had any effect on the $y_i$ under study, the coefficients on the interactions between indicators for August–December births and the treatment cohort indicator, $D_i$, would be significantly different from zero. 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.[12]

### 4.1 Parental leave benefit take-up

We begin by analyzing the effect of the reform on the take-up of parental leave benefits. The data set 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 exist only from 1988 onward. Second, parental leave spells recorded before 1994 are not assigned to specific children (it contains identifiers only for the parents, not

[11] For most of our analyses, the control cohort will be comprised of women giving birth in 1987.

[12] This empirical strategy was also 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. The focus of Karimi et al. (2012) was to study whether paid leave take-up crowds out unpaid leave, when job protection duration exceeds paid leave durations. They find evidence that such crowding out exists to some extent. However, as they lack data on hours worked, their labor supply measure relies on the assumption of unchanged monthly wage rates over the post-reform period, and divides annual income over wage rates to get a crude measure of months worked per year. In this paper, we instead measure labor supply by annual income earned from market work (which does not include governmental transfers, but may include top-up of benefits by employers), and labor market participation. Finally, their paper focuses on the effect of one additional month of paid leave, while we will focus on the effect of the full extension of three additional months, which we explain in sections below.

for the child for whom the leave is taken). Because of these restrictions, we sample mothers to *first-born* children in 1988 and 1989. Looking at take-up immediately after the first child is born implies that we are unlikely to confound parental leave spells for multiple children in the household. Under the (testable) assumption that the reform did not affect subsequent fertility, we can also interpret the medium-run potential differences in take-up between the treated and untreated cohorts as a direct reform effect.[13] Second, since we lack data on take-up before 1988, mothers to kids born in 1989 will serve as the control group. While all mothers of the latter group are treated, there should be no difference in the leave take-up between those who give birth in different months of 1989.

We estimate equation (1) on the cumulative number of (gross)[14] days on parental leave during the child's first three years of life (the vast majority of PL benefits are claimed within three years after birth; see Panel A of Figure A.2). Panel A of Figure 1 plots the estimated coefficients $\widehat{\beta}^m$:s from equation (1) for women. The results show that take-up is monotonically and linearly higher by month of birth starting with the August-births, in line with the implementation of the reform. The absence of significant differences between women giving birth in January–June (compared to July) of 1988 and 1989 provides support for our identifying assumption.[15]

In Panel B of Figure 1 we show that some of the additional leave was also taken-up by fathers, but considerably less so than women: fathers made use of roughly 10 days, on average, of the additional 90 days leave. Nevertheless, considering the low level of leave among men at the time, the effect on fathers' take-up is substantial in relative terms (roughly a 20 percent increase relative to the baseline). Thus, the reform had more or less full impact over the three-year follow-up horizon, for all three treatment intensities (1–3 months), driven by a large increase in the take-up among mothers.

Finally, we present estimates separately by sector of employment using a variation of model (1). Here we exploit the full three-month extension and thus exclude women who gave birth in August or September. We regress leave take-up on an indicator for giving birth in October–December interacted with an indicator for giving birth in the treatment year (along with the main effects and controls). The results are presented in Table 1. The dependent variable is parental leave take-up pooled over the first three years of life (columns 1–3) or over the first eight years (columns 4–6). In the private sector, being

[13] In Table A.1 we report results from estimating a static 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.

[14] Benefits 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.

[15] Note that the "dip" in the magnitude of the point estimates for the birth months of November and December in Figure 1 is an artifact of parental leave take-up not being measured in exact child age in months.

entitled to three months of PL benefits increased take-up during the first three years after birth by, on average, 2.5 months among women, and around 1 week among men. In the public sector, the increase in take-up among women corresponded to an increase of roughly 2 months, and almost 2 weeks among men. Looking at the take-up over child ages 0–8, it appears that women in the public sector spread out some of their extra leave beyond the earliest child ages.

### 4.2 Labor supply response

We now turn to our primary data set with matched employer-employee information and estimate equation (1) on data from the calendar year *after* woman $i$ has given birth to her child. This analysis includes the full population of women giving birth in 1988, and a corresponding control cohort of women giving birth in 1987. We analyze employment decisions using data on annual labor income. This earnings measure does 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.[16]

We plot the estimated coefficients $\widehat{\beta}^m$:s from equation (1) in Figure 2, in the year after birth. There are no significant differences in labor income between women giving birth in January–July across treatment and control cohorts, while women giving birth from September 1988 onwards have significantly lower earnings compared to their counterparts giving birth in 1987. The estimates correspond to slightly more than one month of labor income, based on the average earnings in the control group in the year after birth. As some of the leave may spill-over to the next year, we trace out the full temporal pattern of the effect of the policy intervention on women's post-birth labor supply in the next section.

### 4.3 Long-run labor supply response

In the remainder of the analyses, we make use of the *full* reform of three additional months of benefits, and thus ignore the transition rules of 1 and 2 additional months to August–September parents. Thus, for the samples used in the remainder of the paper, birth months correspond to $m_i = \{1, .. \neq 8, \neq 9, .., 12\}$. Assuming that month of birth is as good as randomly assigned, this sample restriction poses no threat to identification. In Table A.2 we show that differences in pre-determined characteristics by birth month are balanced across birth cohorts.

[16]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.

To trace out the long run reform effect on labor supply, we estimate a dynamic diff-in-diff model including both 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:

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

with event-time indicators $\tau_{it}$ for each year relative to the baseline year (year birth of individual $i$'s child, i.e., 1987 or 1988).[17]

The coefficients of interest are the $\beta^{\tau}$'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.[18]

We estimate equation (2) on annual labor income, and on a conservative indicator of labor market participation defined as having labor earnings above a certain threshold. Because all women are entitled to 12 months of paid leave in the pre-reform regime, we expect the impact of the additional leave to show up one and possibly two years after the year of birth. The estimated coefficients $\widehat{\beta}^{\tau}$ in model 2 are presented in Figure 3, and show 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, Panel B of Figure 3 shows that participation was negatively affected in the short run. 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 2.5 months increase in benefit take-up could be employer-provided top-ups of benefits (which are included in the earnings measure).

### 4.4 Employer-employee separations

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-

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

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

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.[19] In Figure A.3 we show the baseline separation hazard by time since birth; more than 30 percent of women return to a different employer than their pre-birth firm in the first year after birth. The cumulative (job-to-job) separation hazard by year two is around 45 percent.

To assess whether separations are affected by the policy, we estimate equation (2) 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 (Figure 4). Relative to the baseline hazard, this corresponds to an increase of about 15 percent.

An alternative explanation is that these 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 caused by the policy are involuntary.

### 4.5 Heterogeneous program take-up by the number of potential substitutes

The increased PL absence durations could potentially be costly to employers, in particular for those that are ill-equipped to find substitutes for the workers on leave. Workers themselves could internalize their employer's difficulties when deciding on how much to take up of the of additional allowance, especially those who know themselves to be in unique positions at their workplace. For example, Hensvik and Rosenqvist (2019) show that workers with few same-occupation co-workers have lower absence for temporary illness, driven both by employee adjustments of absence and by worker-firm sorting. We analyze if the results found in Hensvik and Rosenqvist (2019) extend to PL absence, as it may inform about potential frictional costs for employers associated with workers' leave-taking. Our research design allows shutting off the sorting explanation for any correlation between leave-taking and the degree of internal substitutability: we exploit exogenous variation in entitlement to extended PL and explore heterogeneous effects by the extent to which workers may act as substitutes for one another within the

[19] 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.

workplace.[20]

We define occupation categories by the combination of education level (four categories) and field (seven categories).[21] We estimate effects of the reform on program take-up separately for workers with different numbers of co-workers with the same occupation category as the focal worker, conditional on occupation-fixed effects. Specifically, we estimate the effect of the policy on leave take-up year by year since birth, for workers with above or below the median number of same-occupation co-workers. In these regressions, we control for occupation-fixed effects and include dummy variables for every firm size, and focus on private sector workers in workplaces with 10-100 employees. Effectively, we compare women in the same occupation category in firms with the exact same number of workers, but with different degrees of occupational concentration. In addition to own leave take-up, we also analyze heterogeneous responses on spousal take-up by the female workers' (own) substitutability.

The results are presented in Figure 5 and show that, in years 2 and 3 after birth, women with few occupational substitutes take fewer parental leave days than women with more same-occupation co-workers. Moreover, women with few substitutes seemingly shift some of the additional paid leave to their husbands in year 2 after birth. These findings imply that workers may adjust their leave behavior to their firms' ability to insure itself from worker absence.

## 5 Employer Responses

Given the documented near-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 measure to be 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,

[20] In related work, Azmat et al. (2020) document that women's likelihood of holding jobs with low substitutability decreases relative to men's after the arrival of the first child.

[21] For the time period studied, data on occupations is unavailable.

the treatment intensity measure 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 this baseline. We then 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 (2) in section 4):

$$\begin{aligned} y_{jt} &= \delta_0 + \sum_{\tau=-2}^{8} \beta^{\tau} \left( \pi_j \cdot D_j \cdot \tau_{jt} \right) + \sum_{\tau=-2}^{8} (\delta_1^{\tau} \tau_{jt} + \delta_2^{\tau}\, \pi_j \cdot \tau_{jt} + \delta_3^{\tau}\, D_j \cdot \tau_{jt}) \\ &+ \delta_4\, \pi_j \cdot D_j + \delta_5\, \pi_j + \delta_6\, D_j + \mathbf{X_j'}\gamma + \epsilon_{jt} \end{aligned} \tag{3}$$

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

**Control variables** Vector $\mathbf{X}$ 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: 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 polynomial in workplace size, and fixed effects for 2-digit industry affiliation. Our rich set of controls ensures that we are very flexibly controlling for the firm size distribution and workforce composition. 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 establishment level to take into account potential serial correlation in the outcomes within firms.

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

Our main focus in our analysis of employer responses are the private sector workplaces. In Table 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 PL 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 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* 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.

Figure 6 presents the coefficients $\beta^{\tau}$ from specification (3) for the firm's total wage bill (which includes both primary and temporary employees), measured in 1000s SEK.[22] 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, pointing to reorganization at the firm at a cost over and above the salary payments for the workers who go on extended leave. Evaluated at the average workplace, the increase in the wage bill corresponds to the salary cost of 0.6 full-time workers in both years. The adjustment costs thus appear sizeable.[23]

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. However, 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

[22] 1000 SEK amounts to circa 105 USD, or 95 EUR.

[23] The estimated coefficient suggests that going from 0 to 100 percent treatment intensity increases the wage bill by 6 million SEK. In practice, treatment intensity never increases from 0 to 1, so we interpret the result for an average workplace. For each additional worker eligible for extended parental leave at a workplace of average size (48 workers), the wage bill increases by 125,000 SEK, corresponding roughly to 60 percent of the salary of a full-time worker (the average salary of a full-time worker in our sample is 210,000 SEK).

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; about 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 7 we decompose the effect on total wage costs into a component attributed to primary employees and to temporary 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 temporary workers increases from year 1 to 4 after childbirth, showing that firms adjust immediately by increasing their temporary staff. However, during the first year after childbirth the wage bill for temporary staff does not increase sufficiently to offset the reduction in primary employees' labor inputs. Changes to the wage bill to primary workers can be driven both by the number of employees and their work hours. In panels C and D of Figure 7 we therefore decompose the wage sum to primary workers into hours and workforce size. To measure hours supplied by the coworkers of women on leave, we calculate the average contracted work hours of all primary employees, excluding the employees who gave birth in the baseline year. Contracted hours are measured as a proportion of full-time equivalent hours (for example, 75 percent). Results show that the work hours of the coworkers of the women on leave increase in the second and third years, and the number of primary employees also increase, primarily in the second and third years as well.

### 5.3 Heterogeneity by firm size

A worker's absence might constitute a substantial labor loss especially in small firms. In Figure A.4 we show heterogeneous effects by firm size. We define a small firm to those with fewer than the median 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.5. Like the private sector, there is a drop in the salary payments to primary workers in year one, but unlike the private sector firms, there are no discernible patterns of adjustments or reorganizations at the public sector establishment to offset the potential effects of the reform on their workers' absence durations. Given that individual-level program take-up were both quantitatively and qualitatively similar (as we show in section section 4), 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 is to a large extent comprised by schools and hospitals, which are financed based on a system of politically fixed budgets. The 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.

### 5.5 Effects on firm performance

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

For a subset of the firms in our sample, namely firms in the manufacturing industry, we have information on sales revenue and value added. These constitute roughly 23 percent of our sample of firms. Table A.5 shows summary statistics for 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 larger workforce.

Figure A.6 report the estimated effects of the reform on log total sales, log total value added, sales and value added per worker, and wage bills for these manufacturing firms. For the wage bill measures, the results are relatively similar to those of our main sample. The estimates for the sales and value added measures are somewhat noisy and the confidence intervals are wide (due to many fewer observations), but the overall picture suggests that firms' total sales revenue declines in the medium- to long-run, as does value added per worker (marginally significant in year 1 after employee birth).

Taken together, our analyzes 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. 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.[24]

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

## 6.1 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. However, external local labor market conditions may dictate the firm's replacement strategies. In particular, firms in thick labor markets – in labor markets

[24] 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.

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 difficult to replace workers with external hires, and thus may resort to internal retention and hours increases.

To capture the external labor market conditions facing the firms in our sample, we construct measures of overall and gender-specific industry-level labor market thickness at the local level, using population-wide data on workers (excluding self-employed) aged 19–64. We delineate 64 commuting zones (CZs), 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:

$$\theta^g_{kct} = \frac{emp^g_{kct}}{emp^g_{ct}} / \frac{emp^g_{kt}}{emp^g_t}$$

for each gender $g = \{0, 1\}$, industry $k$, commuting zone $c$, in year $t$.[25] We construct dummy variables of thickness as indicators for whether $\theta^g_{kct} > 1$, i.e., 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. We focus here on private sector employers.

Figure 8 presents the results for private sector firms in thick and thin markets, respectively. We find that firms in thick markets respond to the extended worker absence predominantly by hiring new workers. Firms that face less favorable external conditions, on the other hand, rely predominantly on incumbent workers by increasing their contracted work hours. We interpret these findings as evidence that a worker who leaves a firm cannot be costlessly replaced; external conditions could make supply constraints binding, in which case the firm's demand for the remaining workers' labor may increase.

### 6.2 Internal substitutability of workers

In Section 4.5, we show that workers potentially internalize employers' replacement costs by adjusting their response to the reform depending on the extent to which they have internal substitutes, suggesting some degree of specificity of human capital. We now turn to explore the role of internal substitutability on employers' responses to worker absence.

We characterize the potential for internal substitution possibilities at the workplace by the overall occupational specialization at the establishment.[26] We follow Cortes and Salvatori (2019) and calculate

[25] Figure A.7 shows the female labor market thickness for an example industry (financial intermediation) in the 64 commuting zones in Sweden to provide a visualization of the variation in local labor market thickness in our data.

[26] Because we sample firms who potentially have more than one woman going on leave, we are not able to easily study the

the employment share in the largest occupation category within the workplace to measure specialization. The intuition is that workplaces with a high degree of occupational concentration should have greater scope for internal substitution across incumbent coworkers. 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, given in equation (2) separately for the two groups of firms. The results are presented in Figure 9. Firms with internal supply constraints (lower degree of potential substitutability) adopted new hires and temporary workers as their main adjustment strategies, while firms with more scope for internal substitution resorted to increasing work hours of incumbents. We argue that it is not likely such heterogeneity is driven by differences in the size of the labor supply shock due to workers' adjustment behavior, based on two pieces of evidence. First, our findings for workers showed substantial labor supply reductions for workers of both types. Second, if workers fully internalize the adjustment costs, we would see no adjustment on the part of employers in firms with a heavy concentration of workers in the same occupation. In sum, the fact that firms employed different strategies depending on the availability of internal substitutes suggests that human capital specificity may imply binding supply constraints, and thus points to an additional source of frictions facing firms dealing with turnover.

### 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.

In Figure A.8, we show that there are no differences in the wage bill cost across firms facing different external labor market conditions (with the exception that the drop in the wage bill in year 1 is apparent only for firms in thick markets). With respect to internal substitutability, however, we find that firms with a low degree of internal substitutability have higher net increases in their wage bill relative to firms with more scope for internal substitutability.

heterogeneity in these effects by the number of direct occupational substitutes the firm has for the absent person.

# 7 Implications for Women's Careers and Statistical Discrimination

In the previous sections, we have shown that there exist various sources of frictions faced by firms, and that the adjustment costs could be quite substantial for firms when their workers go on extended leave. Of course, the parental leave expansion did not just affect the firms that happened to employ women giving birth in the reform year. The reform affects all firms in the economy, even if they have not yet had workers go on extended parental leave. In anticipation that all women will now take three additional months of leave in the post-reform regime, forward-looking firms might want to avoid the adjustment costs by resorting to male hires. Firms might translate these costs into statistical discrimination against women, and we might see a slow shift in the firms' gender composition away from women of childbearing ages.

Before we dive into the analysis of statistical discrimination, first we mention a few cautionary notes. Since we do not directly observe firm-specific human capital, the degree of substitutability between male and female workers, or discrimination at the workplace level, we cannot unambiguously interpret certain actions of the firms as statistical discrimination against women. For example, if we observe that a firm in a local labor market with abundant female labor supply is refraining from external female new hires, we would not be able to tell if they do so because human capital is too firm-specific, or because this firm is discriminatory. However, if we are willing to assume that female labor is at least as good as male labor to replace the women on leave, a firm making external male but not female hires in a thick market with a large pool of both men and women candidates would constitute suggestive evidence for statistical discrimination. Indeed, in Figure A.9 we show that more intensely treated workplaces in thick markets for both male and female labor appeared to predominantly hire male workers in response to the reform.

In the section below, we provide a descriptive analysis on all firms' hiring and wage setting behaviors after the reform, to shed light on the mechanisms behind statistical discrimination and gender gaps in the labor market. Since we are unable to use the exogenous eligibility cutoff for the economy-wide analysis, the results below can be interpreted as suggestive.

### 7.1 Changes in employment outcomes of men and women by predicted reform exposure in different industries and local labor markets

Forward-looking firms can respond to the reform by changing their personnel policies in several ways: whom they hire, how much they pay to newly hired workers with certain characteristics, and whom they decide to promote. In order to analyze such possible demand-side responses to the reform, we study promotion patterns, hiring rates, and starting wages of new hires at all firms in the economy, contrasting changes in these quantities for women of childbearing ages relative to other groups, before and after the policy change. Our analysis builds on the assumption that industries with a higher exposure to the reform at the time of the intervention will expect themselves to be more affected by worker absence. Thus, we use the variation in predicted reform exposure across local labor markets and industries, based on age-specific fertility rates and the composition of workers at baseline.

We use data for the full population of Swedish workers for the years 1985 through 1996. Let $P^a(b)$ denote the age-specific fertility rate (averaged over the pre-reform period 1985–1988). For each industry $k$ in commuting zone $c$, we calculate the predicted fertility in 1988 (year before reform implementation) as

$$\phi_{c,k} = \sum_{a=21}^{38} P^a(b) \times N_{c,k}^{f,a}$$

where $N_{c,k}^{f,a}$ denotes the number of women aged $a = \{21, ..., 38\}$ employed in industry $k$ in region $c$ in 1988. For each industry $k$ in each local labor market $c$, we define the predicted exposure to the reform as

$$\pi_{c,k}^{p} = \frac{\phi_{c,k}}{N_{c,k}}$$

where $N_{c,k}$ denotes the total number of workers (aged 19–65) in industry $k$, region $c$ in 1988. Thus, $\pi_{c,k}^{p}$ is a time-invariant measure of predicted reform exposure at the local labor market and industry level. To investigate the aggregate effects of the reform on outcomes, we estimate the following regression equation:

$$y_{c,k,t} = \gamma_0 + \sum_{t=1985,t\neq 1988}^{1996} \gamma_1^t \left(\pi_{c,k}^{p} \times D_t\right) + \gamma_2 log(pop)'_{k,t} + \gamma_3 \pi_{c,k}^{p} + \lambda_t + \lambda_c + \nu_{c,k,t} \quad (4)$$

where $D_t$ are indicator variables for each calendar year $t = \{1985, .., \neq 1988, .., 1996\}$. We control for the (log) population size of the commuting zone, year-fixed effects to account for secular trends in the outcome variable, and local labor market fixed effects. The coefficients of interest are the $\gamma_1^t$'s, which measure the difference in the evolution of the outcome variable across industries with different predicted exposure, before and after the intervention, within local labor markets. (In Figure A.10 we show that there is considerable variation in predicted reform exposure both across industries and within industries across local labor markets.)

We stress that a causal interpretation of the $\gamma_1^t$'s would be contentious; we do not have experimental variation in reform exposure. For a causal analysis, we would have to assume that industries differing in their concentration of young women would evolve similarly in the absence of the reform, the validity of which we could potentially gauge by looking at differences in the pre-reform trends in the outcomes. One key source of potential confounding, however, is the Swedish financial crisis which erupted in the fall of 1992 and lead to a large increase in unemployment, potentially affecting male and female-dominated industries differentially. However, we note that the employment decline was larger among male workers, such that the overall female-to-male employment ratio increased during the crisis years (see Figure A.11). Thus, differential changes in labor market outcomes driven by the crisis should arguably be in favor of women. Nevertheless, we cannot rule out potential confounding effects of the mid-1990s crisis.

We look at three outcomes: promotions, composition of new hires, and the gender gap in starting wages of new hires at the local labor market and industry level. To study promotions, we construct a measure of promotion events based on individuals' within-firm relative real wage growth as in Bronson and Skogman Thoursie (2019). For each worker in Sweden with an observed wage rate and at least a two-year tenure with a firm, we calculate the annual real wage growth relative to their co-workers. A promotion event for person $i$ is then defined to characterize a situation where $i$ realizes a real wage gain that is at least 10 log points higher than the average wage gain at their firm. We then aggregate promotion rates of men and women of different age groups at the local labor market and industry level.

To measure new hires, we calculate the number of new hires in each calendar year at each firm active in Sweden, where a worker is defined as newly hired in year $t$ if they were not employed at the workplace in year $t-1$. Thus, workers can be hired from non-employment, and from another firm. Thus, our measure of hiring rates constitutes a combination of new hires (e.g., new graduates, or non-employed) and an overall measure of the activity in a certain industry (job-to-job transitions). For each hired worker, we retain information on the starting wage. These quantities (number of hires and average starting wages) are then aggregated at the local labor market and industry level, and the number of new hires is normalized by the workforce size in the respective demographic group in the local labor market/industry. Because our data starts in 1985, we can identify promotion rates and new hires from 1986 onward.

Results are presented in Figure 10 and show a decline in promotion rates of women younger than 40, starting already in 1990, in industries with a high predicted exposure compared to industries with low predicted exposure. In comparison, the point estimates for promotion rates of older women are smaller in magnitude, and not statistically significant. For men, promotion rates do not seem to change differentially by predicted exposure, except for a statistically significant increase in 1993. In an industry with average exposure, the decline in promotion rates of young women correspond to 2-3 percent after 1989. In Figure A.12, we study promotion patterns by parental status, and find that the decline in promotion rates among women is strongly driven by those who are not yet mothers, which would be consistent with statistical discrimination.

Figure 11 shows an immediate larger post-reform decline in the number of new hires among women in childbearing ages in more exposed industries compared to low exposed ones. In 1990, the decline in female young new hires correspond to a 2 percent decrease in an industry within a local labor market with an average reform exposure. In contrast, there is no apparent change in the hiring rate of male workers or of older women workers immediately after the reform, in more exposed industries. There are such declines, however, for all three groups after 1993, i.e., during the crisis years. Thus, it is likely that the long-run responses to hirings are driven by the crisis rather than the reform.

Regarding starting wages of new hires, Figure 12 shows a gradual increase in the gender gap in wages of young workers (wage gap became more negative). We interpret these findings with caution, however, due to potential compositional changes driven by the financial crisis. In particular, unemployment increased disproportionately among men, so we might worry that those men who were hired in an environment of declining overall male employment may be positively selected compared to those women who were hired in the same time period. Nevertheless, we note that the gap in starting wages between newly hired young female workers and older female workers also declined more in more exposed industries, and selection concerns should be less important in this comparison. In the average industry, the increase in the gender (starting) wage gap corresponded to 3–4 percent, and the increase in the wage gap between younger and older women to 2–4 percent.

What would all of these changes imply for the overall gender wage gap? Declining promotion rates, hiring rates, and lower relative starting wages to female hires all suggest an increase in the gender wage gap. In Figure 13, we estimate equation (4) on the overall gender wage gap, and find a larger gender wage gap in more exposed industries immediately after the reform implementation, an "effect" that does not gradually dissipate over time.

Taken together, while these analyses remain descriptive and have several caveats, they go in the direction of the reform potentially increasing the scope for statistical discrimination on the part of employers.

## 8 Conclusions

We study the effect of parental leave mandates on firms' outcomes and potential implications for gender gaps in the labor market. We use exogenous variation across firms in their female employees' maternity leave durations induced by a reform in the Swedish parental leave system that increased the entitlement to paid leave. We show that the reform had a full effect on mothers' take-up of leave, while the effect on male take-up was substantially smaller in absolute terms. The reform thus predominantly altered the labor supply of mothers. Moreover, the additional leave entitlement increased the probability of eligible mothers to separate from their pre-birth employer. From the firm's point of view, this implies that they would have to replace workers both temporarily and permanently. Finally, we find heterogeneous responses by the extent to which workers have substitutes within the workplace, indicating that employees may partly internalize the adjustment costs of their employers when they decide on the duration of leave.

Turning to firm responses, we find that private sector firms with greater exposure to the reform adjusted by hiring temporary and permanent workers, and by increasing the work hours of incumbent workers. The net effect of these adjustments on firms' total wage bill is positive, suggesting that such adjustments come at a monetary cost. We document heterogeneity in employer adjustment based on the ease with which replacement workers can be found. In particular, firms in thick external markets responded to the reform by relying on temporary workers and new hires, while keeping incumbents' work hours unchanged. Firms in thin markets, on the other hand, resorted to internal hours increases. Similarly, we also find heterogeneous responses by the degree of internal substitutability within the workplace: establishments with a high concentration of its workforce in their dominant occupation group responded to the labor supply shortage by increasing incumbents' hours more compared to firms that had a more diverse occupational structure, which relied on external new hires instead. These findings thus suggest several sources of potential frictions associated with finding and hiring replacement. Moreover, the wage bill costs of firms' adjustments are substantially higher for firms facing low internal substitutability. Finally, using data on sales and productivity for firms in the manufacturing industry, we provide suggestive evidence of declines in sales revenue and value added measures. We note that among this subset of firms, we find similar reorganization of the personnel in response to the reform as for the main sample. This suggests that even while replacement may be easy to find, these workers may not be as productive as workers on leave due to e.g. firm-specificity of human capital.

What are the implications of these findings for the gender wage gap? To inform on the equilibrium effects of the policy, we perform a descriptive analysis of the hiring and wage setting behaviors of all firms in the economy before and after the intervention. To this end, we compare the economy wide hiring rates, wage offers, and promotions of workers in the at-risk population relative other workers before and after the policy change. We contrast these quantities across industries within local labor markets with different predicted reform exposure. We find that in industries with higher predicted exposure, the promotion rate, hiring rate, and starting wages of new hires decline relatively more for women of childbearing ages compared to male and older female workers, after the reform was implemented. However, we caution against a causal interpretation of these findings, due to the lack of experimental variation in reform exposure at the aggregated level.

As a concluding remark, we emphasize that our results are derived from a policy experiment which was sudden and unexpected to firms, similar to the reform in Denmark studied by Gallen (2019), who notes that the extent to which worker absence is a surprise to employers may affect costs. However, evidence presented in Brenøe et al. (2020), who study the effects on employers of their workers having a birth *per se*, are in line with our results: firms respond by increasing their hires and work hours of incumbents, with net positive effects on the total wage bill. Moreover, Gallen (2019) estimates heterogeneous responses of the PL extension in Denmark by the extent to which the firms were "surprised" and finds similar effects on the firms' shut-down probabilities irrespective of the time that the firm had at their disposal to plan for the increased leave duration. If human capital is very 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 their workers go on leave.

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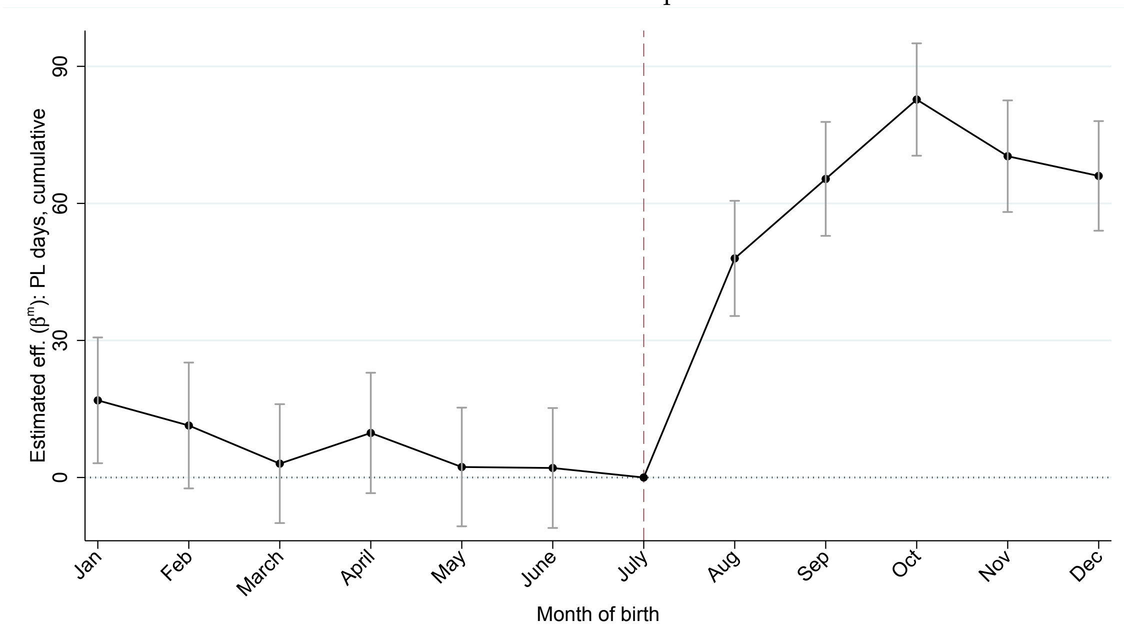
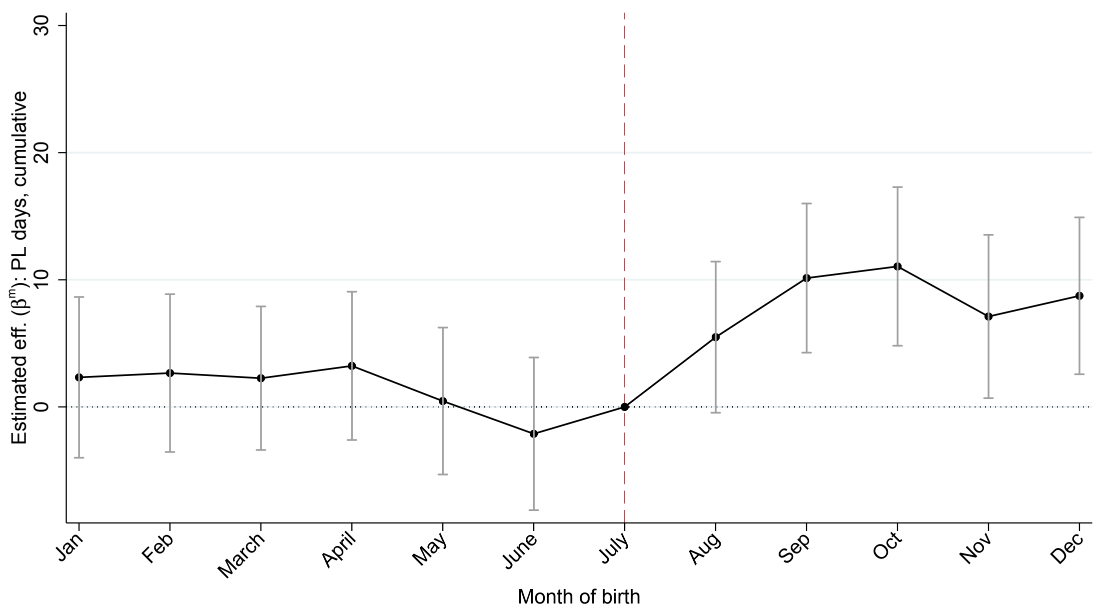
FIGURE 1.
Effects of extended entitlements to paid leave on the take-up of parental leave

(A)
Female PL take-up

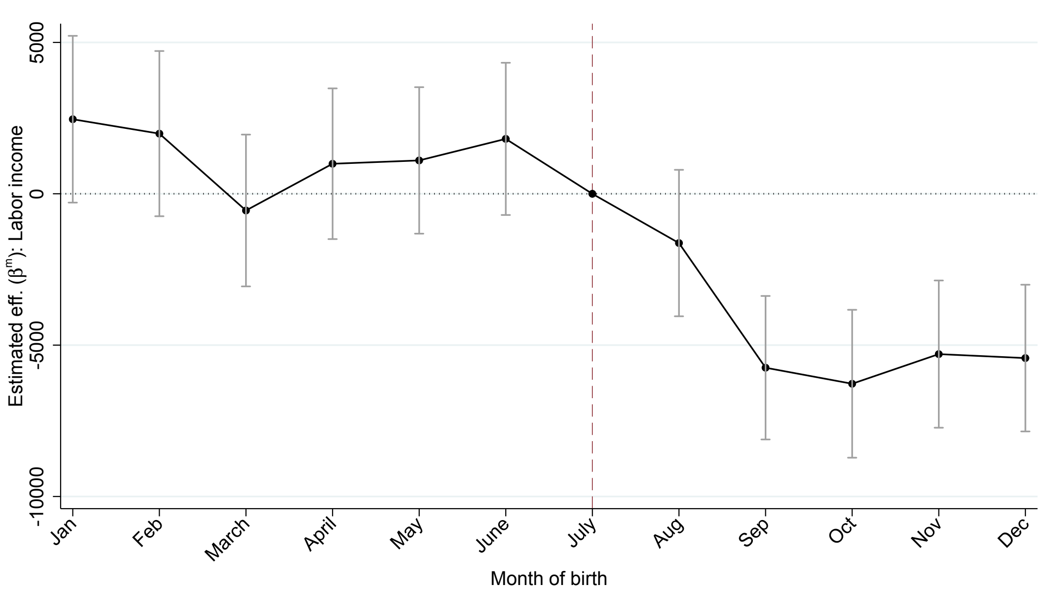



(B)
Male PL take-up




NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of having a child born in 1988 and calendar month of birth, $\hat{\beta}^m$, from equation (1), i.e., the difference in outcomes between women who gave birth in calendar month $m = Jan, ..., Dec$ in 1988 and corresponding months in 1989, along with the 95% confidence intervals. No control variables are included in the estimation.

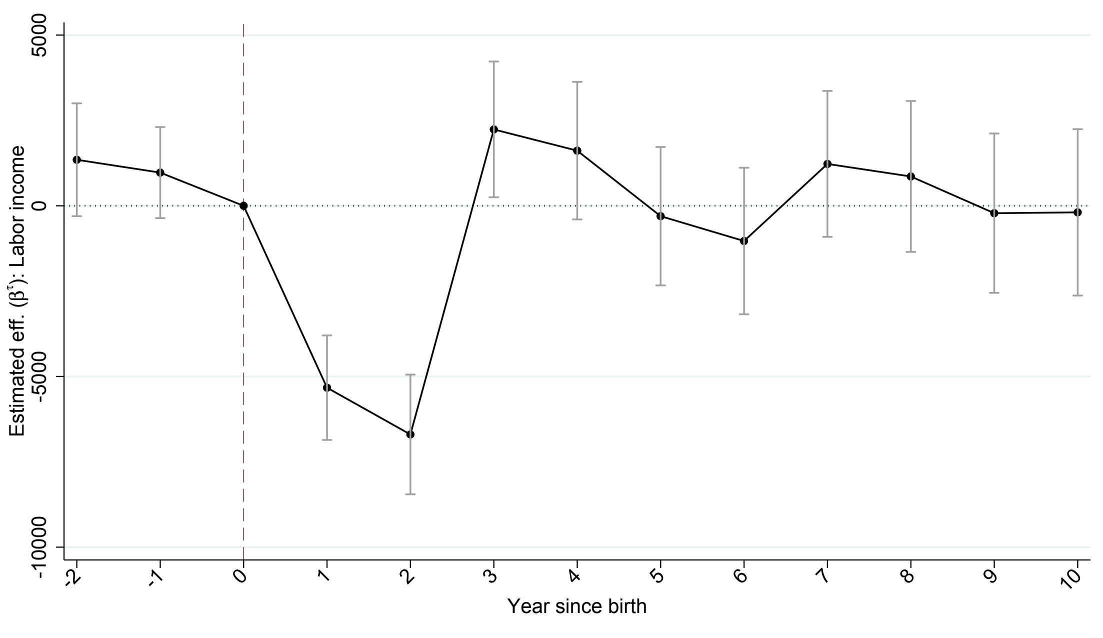
FIGURE 2.
Effects of extended entitlements to paid leave on mothers' labor income: one year after birth



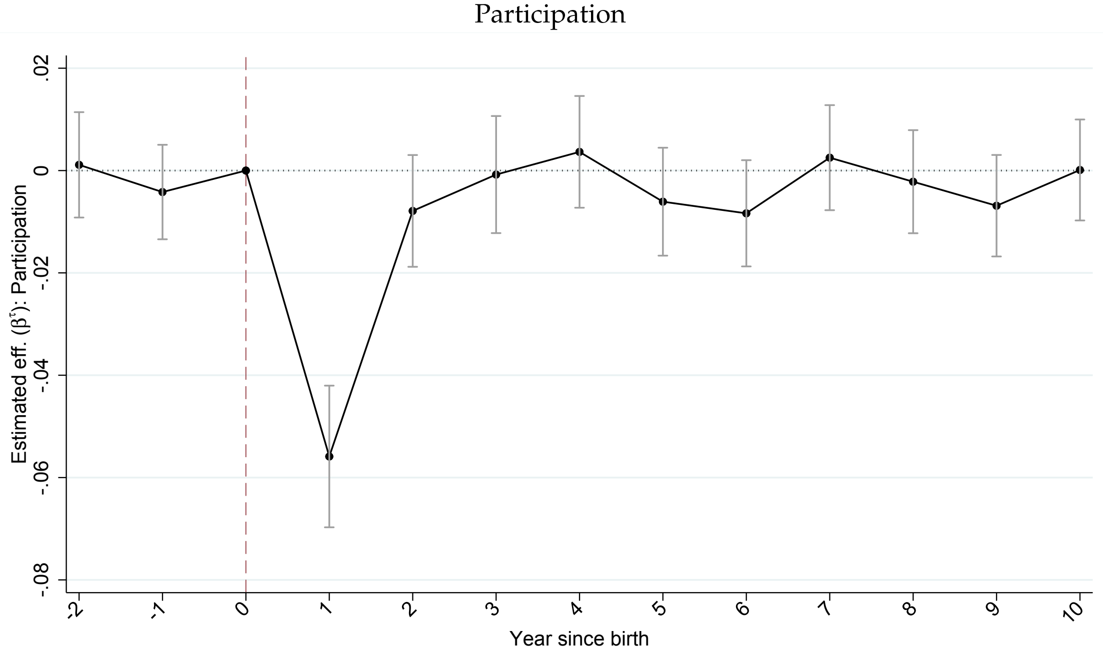

NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of having a child born in 1988 and calendar month of birth, $\hat{\beta}^m$, from Equation 1, i.e., the difference in outcomes between women who gave birth in calendar month $m = Jan, ..., Dec$ in 1988 and corresponding months in 1987, along with the 95% confidence intervals.

FIGURE 3.
Effects of extended entitlement to paid leave on female labor income and participation

(A)
Labor income

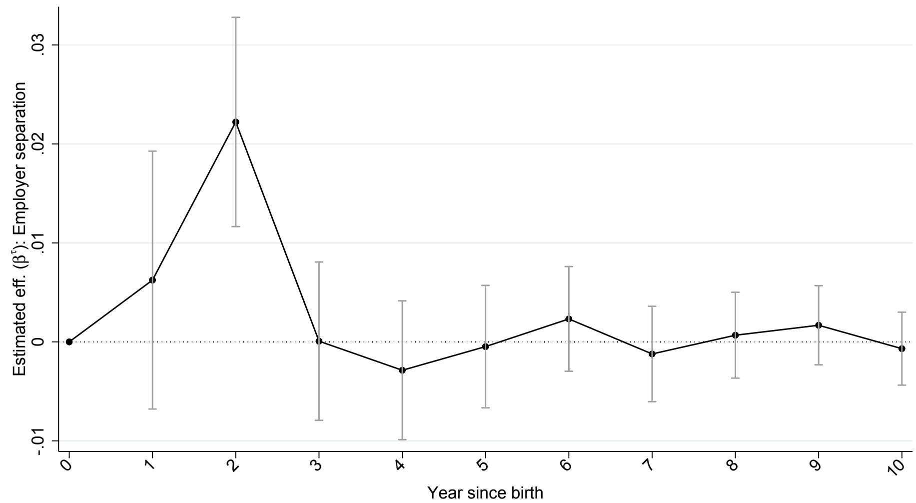



(B)
Participation




NOTE: Each point in the graphs shows the estimated $\hat{\beta}^{\tau}$ from equation (2) and the corresponding 95% confidence intervals.

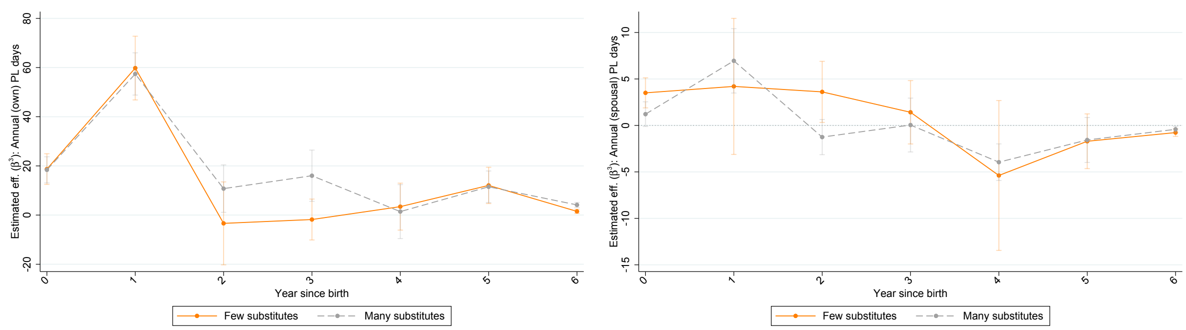
FIGURE 4.
Effects of extended entitlement to paid leave on separation from pre-birth employer




NOTE: Each point in the graph shows the estimated coefficients $\hat{\beta}^{\tau}$, from equation (2), along with the 95% confidence intervals.

FIGURE 5.
Heterogeneous effects of extended entitlement to paid leave on program take-up by the number of internal substitutes at the workplace

(A) Own PL take-up | (B) Spousal PL take-up

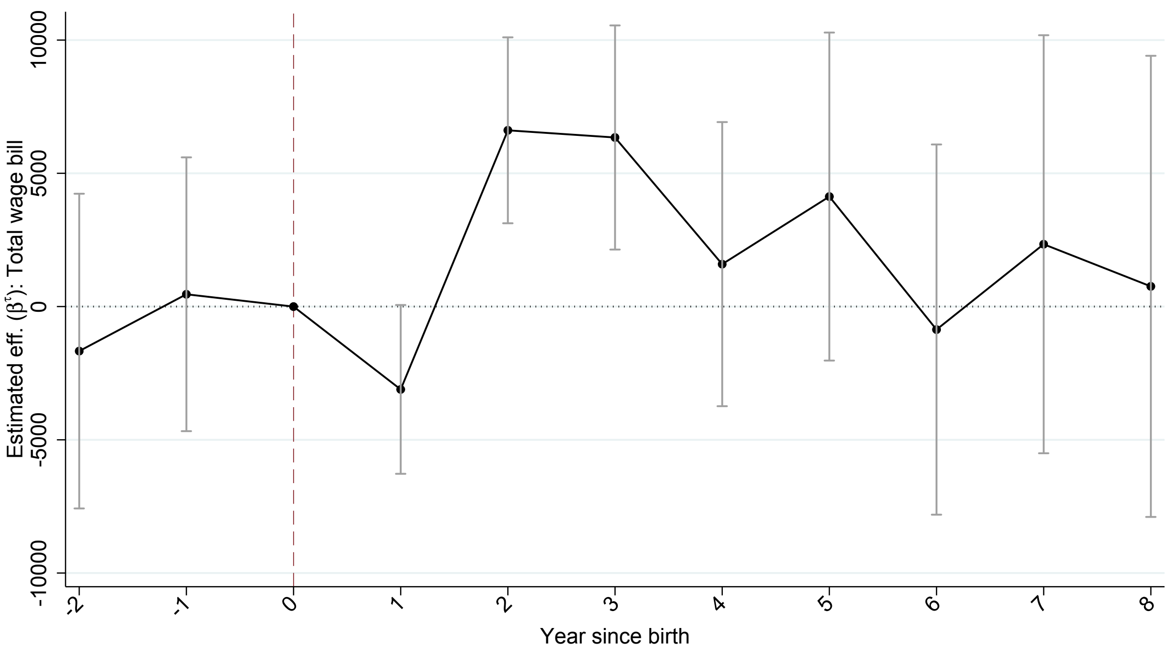



NOTE: The sample includes couples whose first child was born in 1988 or 1989, where the wife worked in the private sector (at birth) in workplaces with 10-100 employees. The outcome is number of leave days at different child ages. The estimated regression equation (separately for each follow-up year) is the following:

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

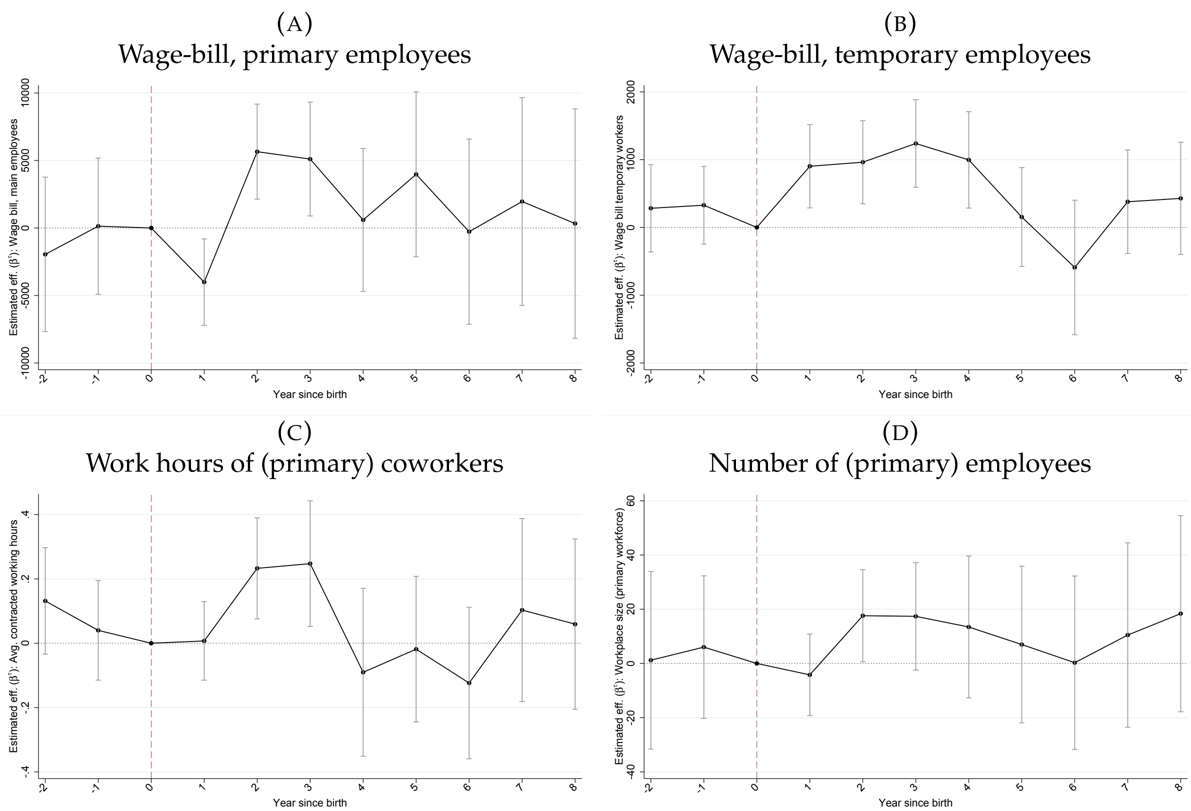
where $T_i$ is an indicator that takes the value 1 if person $i$ had a child born in October–December and 0 if person $i$'s child was born in January–July. The graph plots the estimated $\hat{\beta}$:s and the corresponding 95% confidence intervals.

FIGURE 6.
The effect of the extended parental leave program on firm's total wage costs




NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., the $\hat{\beta}^{\tau}$ from Equation 3, along with the 95% confidence intervals. The outcome variable, firm's total wage bill, is measured in 1000s SEK.

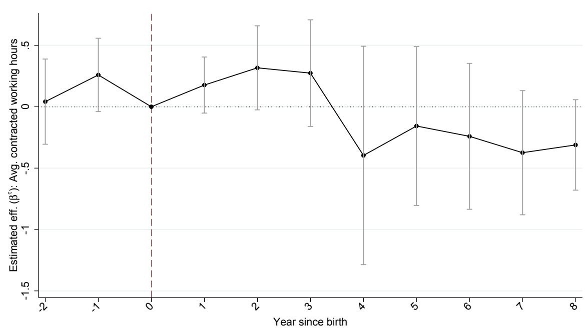
FIGURE 7.
Decomposing employer responses: primary workers' hours increases or temporary replacement workers?



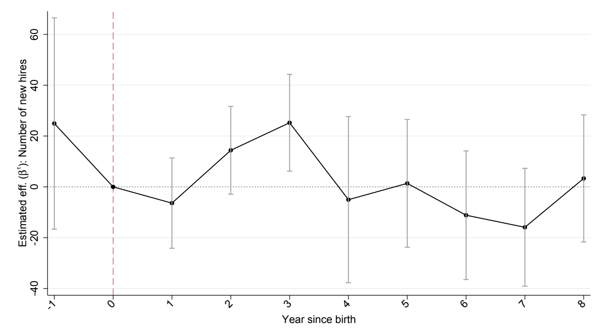

NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., the $\hat{\beta}^{\tau}$, from Equation 3, along with the 95% confidence intervals. Firms' wage-bill outcomes are measured in 1000s SEK.

FIGURE 8.
Heterogeneous employer responses by external labor market conditions

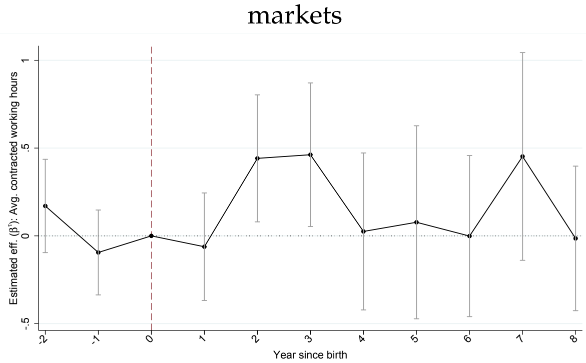
(A)
Work hours of incumbent employees: thick labor markets



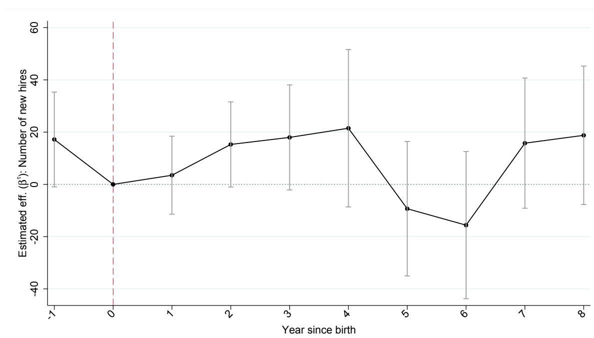

(B)
New hires: thick labor markets




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



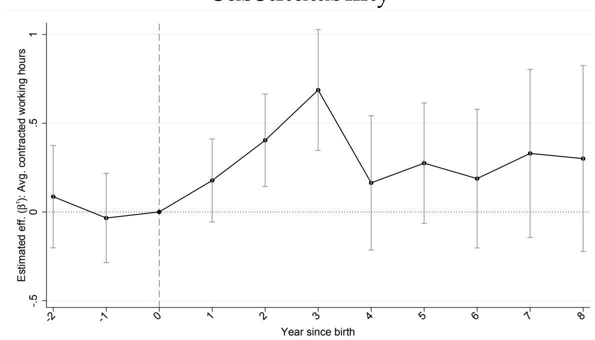

(D)
New hires: thin labor markets



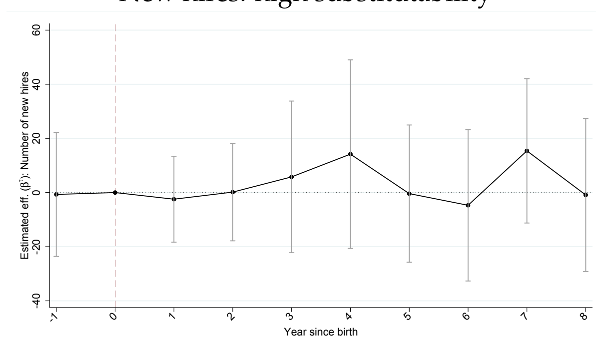

NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., $\hat{\beta}^{\tau}$, from Equation 3, along with the 95% confidence intervals.

FIGURE 9.
Heterogeneous employer responses by workplace occupational concentration (internal substitutability)

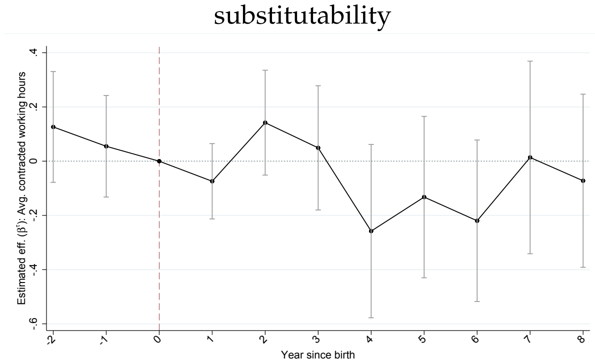
(A)
Work hours of primary employees: high substitutability



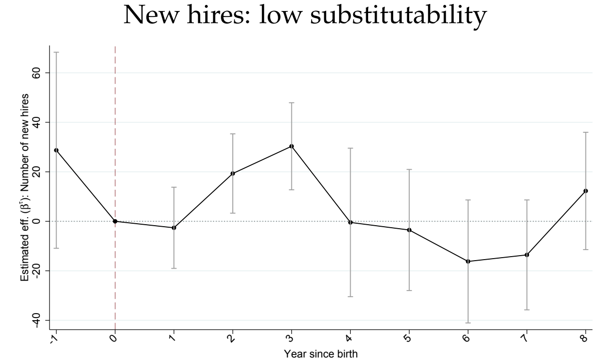

(B)
New hires: high substitutability




(C)
Work hours of primary employees: low substitutability



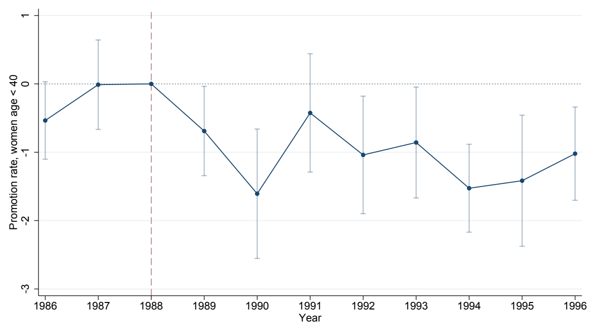

(D)
New hires: low substitutability




NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., $\hat{\beta}^{\tau}$, from Equation 3, along with the 95% confidence intervals.

FIGURE 10.
Effect of predicted reform exposure on promotion rates

(A)
Promotion rate, women age $\leq 39$




(B)
Promotion rate, women $> 39$


(C)
Promotion rate, men

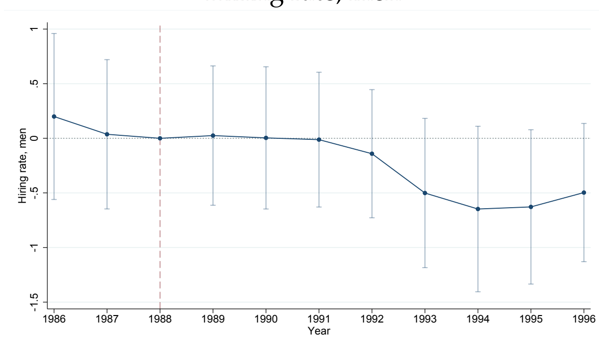

NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in industry $k$ and region $c$, $\pi^{p}_{c,k}$, before and after its implementation, that is, the estimates for $\gamma_1^t$:s in Equation 4, along with the 95% confidence intervals.

FIGURE 11.
Effect of predicted reform exposure on hiring rates

(A)
Hiring rate, women age $\leq$ 39


(B)
Hiring rate, women > 39


(C)
Hiring rate, men




NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in industry $k$ and region $c$, $\pi^{p}_{c,k}$, before and after its implementation, that is, the estimates for $\gamma^{t}_{1}$:s in Equation 4, along with the 95% confidence intervals.

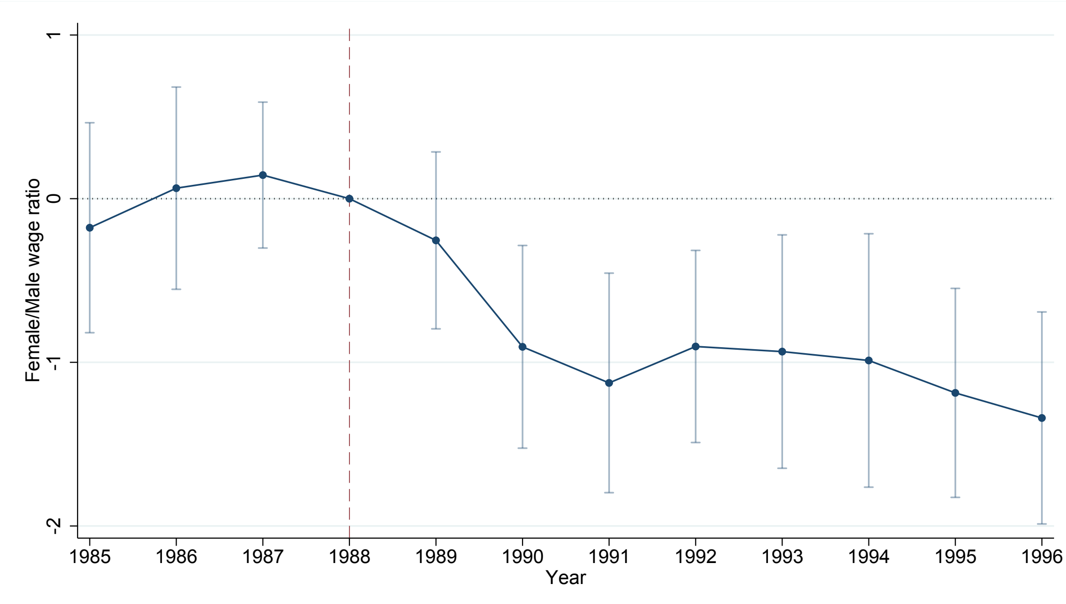
FIGURE 12.
Effect of predicted reform exposure on relative starting wages of new hires

(A)
Gap in starting wage: young women/young men


(B)
Gap in starting wage: young women/older women


NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in industry $k$ and region $c$, $\pi^{p}_{c,k}$, before and after its implementation, that is, the estimates for $\gamma^{t}_{1}$:s in Equation 4, along with the 95% confidence intervals.

FIGURE 13.
Changes in the overall gender wage gap (relative wages of women to men)




NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in industry $k$ and region $c$, $\pi^{p}_{c,k}$, before and after its implementation, that is, the estimates for $\gamma^{t}_{1}$:s in Equation 4, along with the 95% confidence intervals.

TABLE 1.
Effects of the reform on parental leave take-up by gender and child age

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | Years 0–2 | | | Years 0–8 | |
| | All | Private sector | Public sector | All | Private sector | Public sector |
| **A. Female take-up** | | | | | | |
| $D_i \times T_i$ | 66.836 | 74.391 | 62.114 | 82.215 | 81.402 | 80.545 |
| | (3.376) | (5.272) | (4.461) | (5.786) | (9.269) | (7.802) |
| Observations | 78,423 | 29,733 | 41,050 | 78,423 | 29,733 | 41,050 |
| **B. Male take-up** | | | | | | |
| $D_i \times T_i$ | 7.783 | 6.762 | 13.110 | 9.377 | 7.745 | 18.600 |
| | (1.601) | (1.788) | (3.427) | (2.578) | (2.904) | (5.409) |
| Observations | 50,052 | 34,018 | 13,760 | 50,052 | 34,018 | 13,760 |

NOTES: The sample includes women and men who had a first child born in 1988 and 1989. Columns (1)–(3) present estimates the effect of being eligible for three additional months of paid leave on the total number of (gross) days taken-up over child ages 0–3, and columns (4)–(7) shows the corresponding estimates for the total number of leave days taken over child ages 0–8. 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; $y_i$ denotes parental leave take-up pooled over the first three years of life (columns 1–3) or over the first eight years (columns 4–6). As in Equation 1, the vector $\mathbf{X_i}$ 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.

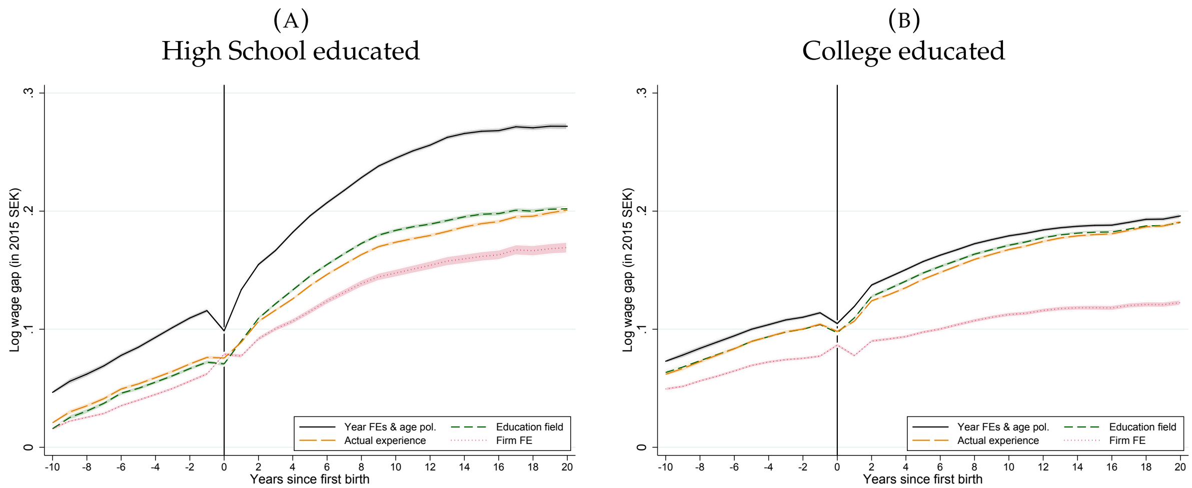
TABLE 2.
Summary statistics for all firms & organizations active in Sweden, and for firms in study sample

| | All workplaces (mean) | All (sd) | Sample workplaces (mean) | Sample workplaces (sd) |
|---|---|---|---|---|
| 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 | 0.968 |
| 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 | 38.273 | 52.492 | 49.150 | 57.191 |
| Average age | 35.771 | 5.866 | 35.424 | 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) | 20,379.255 | 4,333.878 | 19,840.332 | 4,259.369 |
| Female monthly wage (SEK) | 17,813.571 | 2,875.786 | 17,448.251 | 2,788.589 |
| Male monthly wage (SEK) | 22,519.651 | 5,567.610 | 22,214.741 | 5,580.741 |
| Female annual income (SEK) | 130,030.496 | 56,745.443 | 125,297.426 | 50,804.566 |
| Male annual income (SEK) | 190,823.995 | 89,085.239 | 192,548.684 | 96,117.094 |

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.

# A Additional Tables and Figures (For Online Publication)

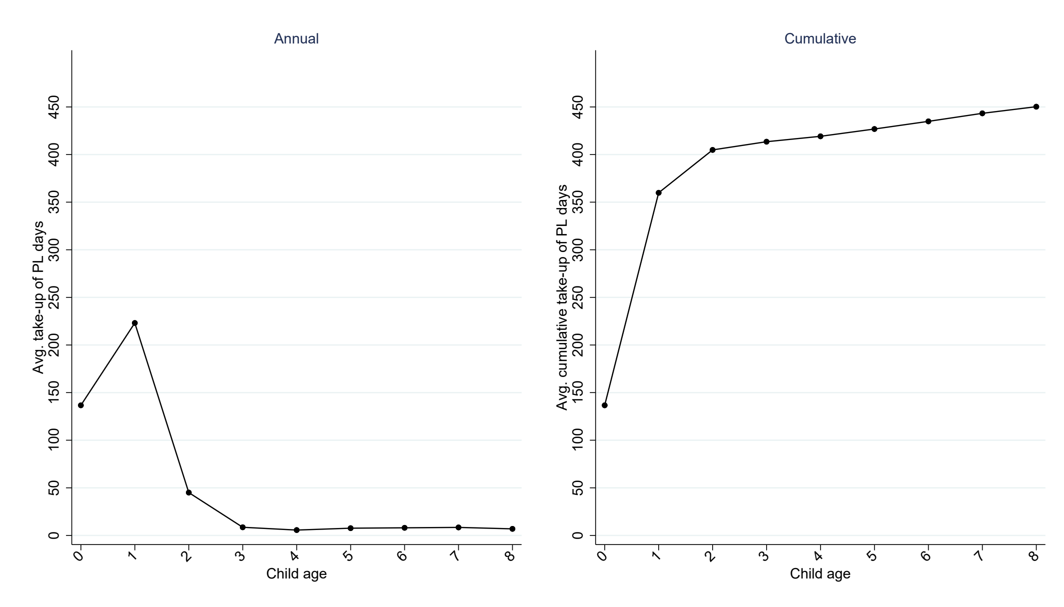
FIGURE A.1.
Residual gender wage gap by time since first birth



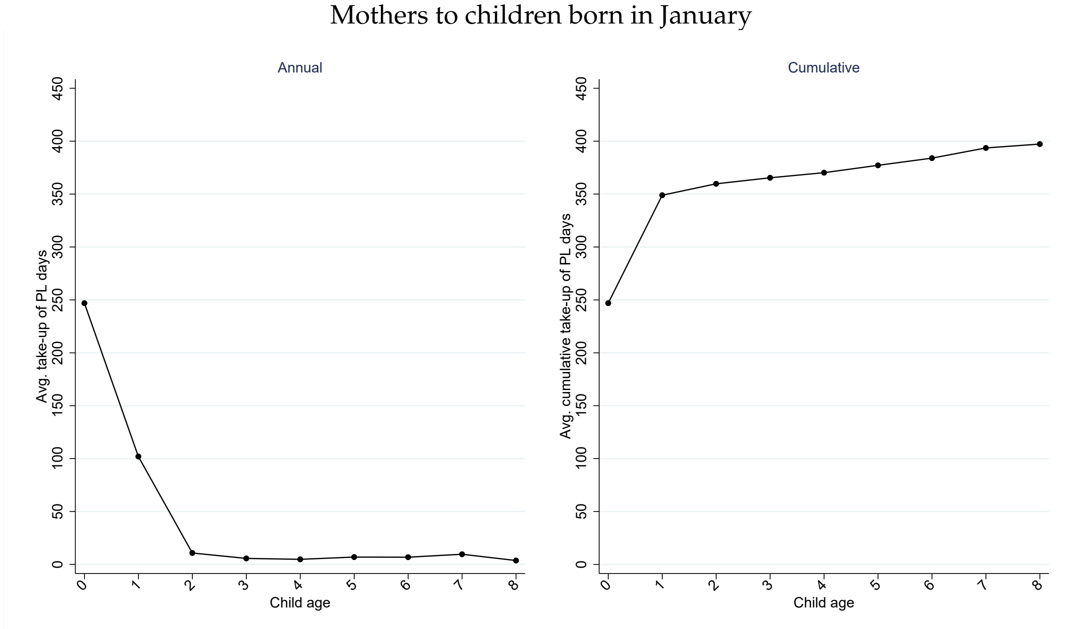

NOTE: The lines in the figure plot the residual gender gap in (log) wage by time since first birth adding successively more detailed controls for individuals' characteristics.

FIGURE A.2.
Women's parental leave take-up by child age

(A)
All Mothers

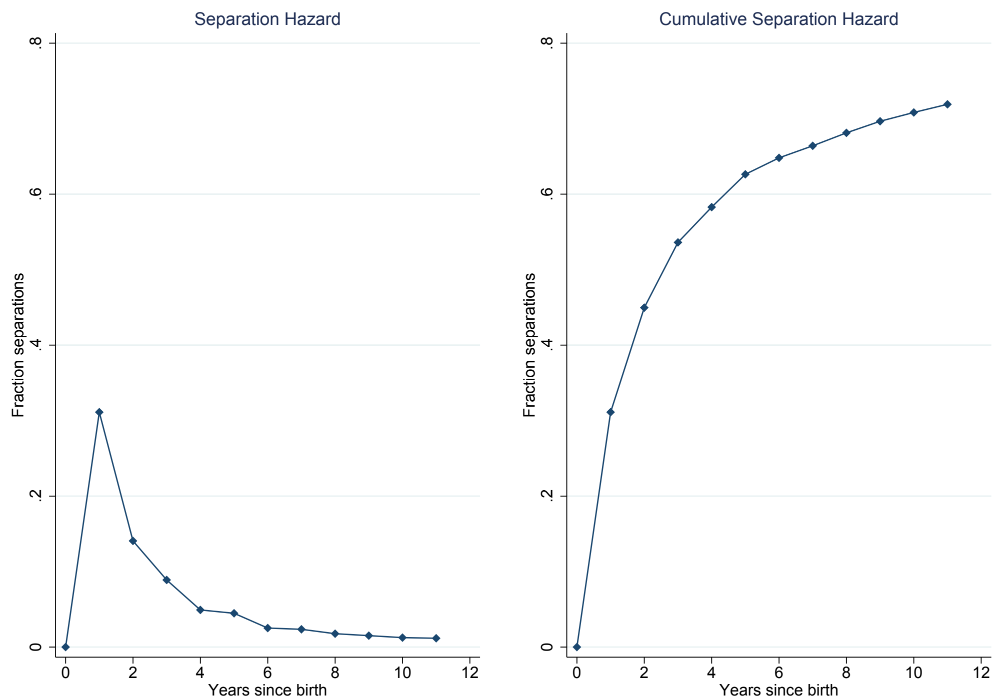



(B)
Mothers to children born in January



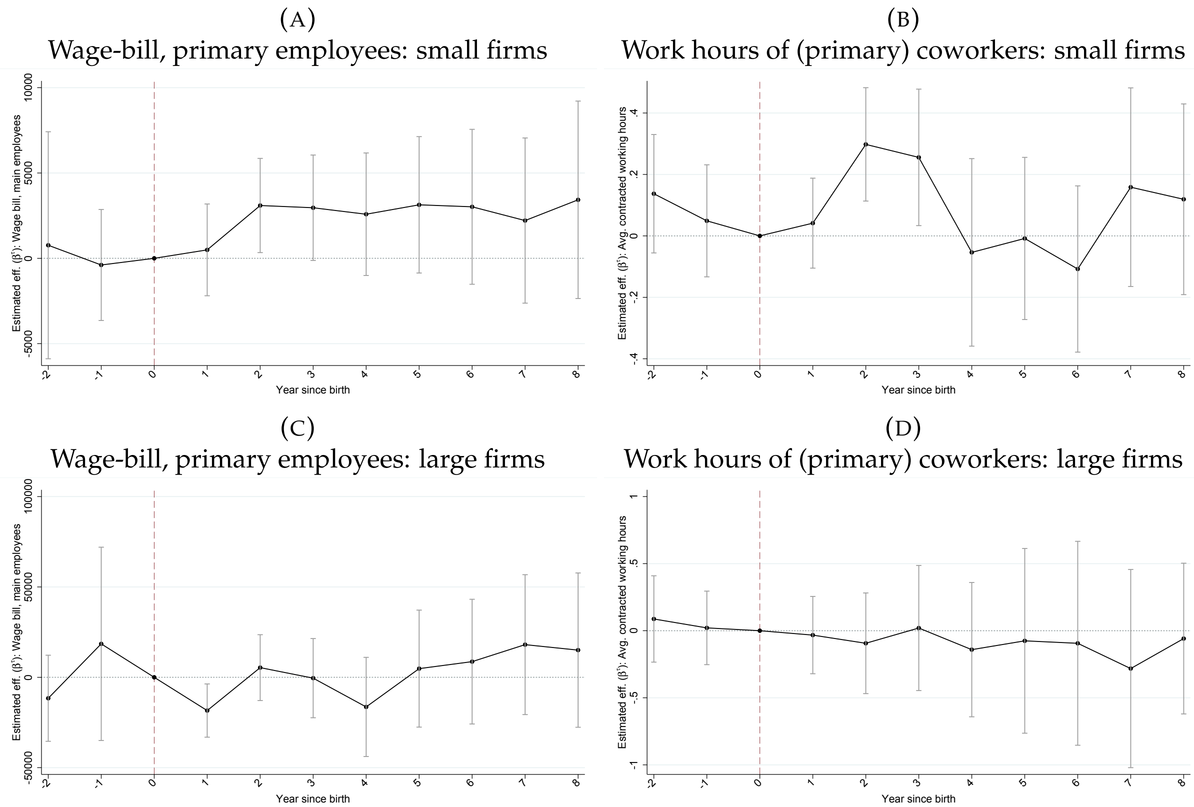

NOTE: The sample consists of all mothers with only one child, and whose child was born in 1988 or 1989. The lower panel further restricts the sample to mothers of children born in January.

FIGURE A.3.
Baseline separation rate from pre-birth employer by time since birth



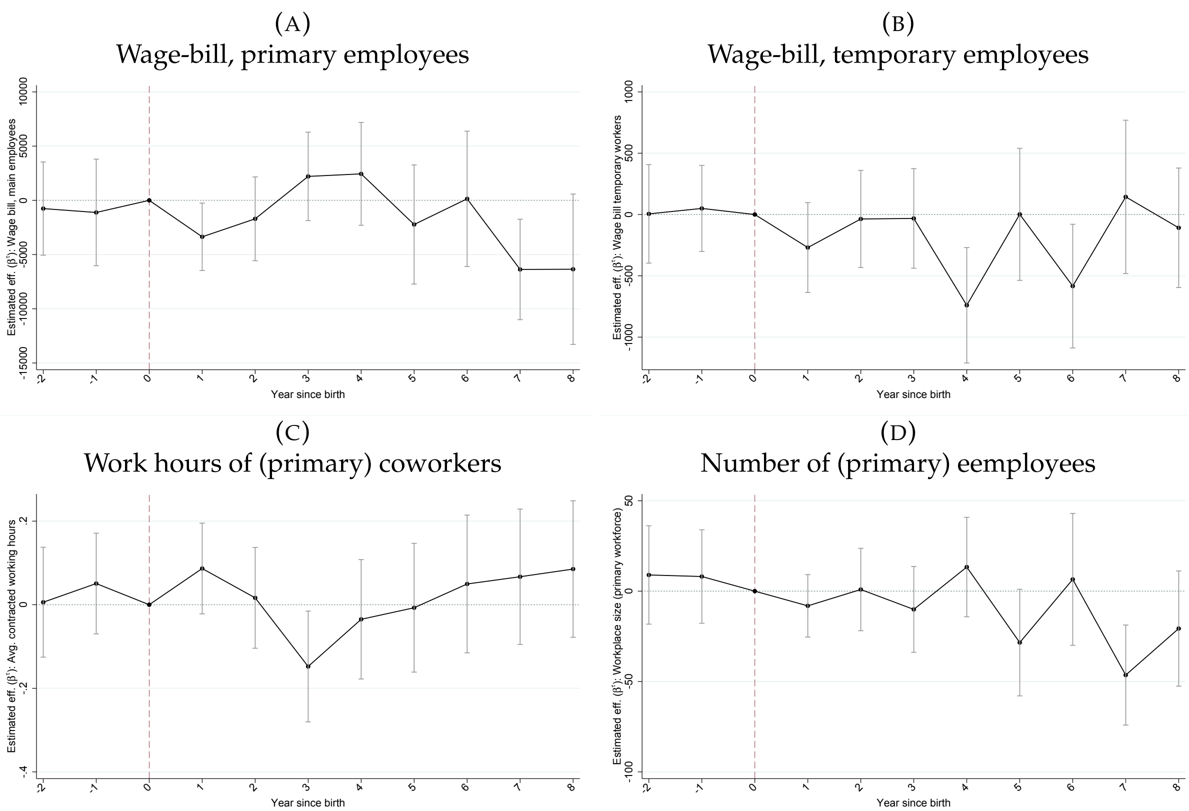

NOTE: The figure shows the separation hazards from the pre-birth employer by time since birth. The hazards are calculated on the sample of women who give birth in January–July and October–December of 1987. The left-hand panel shows the annual hazard rate, and the right-hand panel the cumulative hazard rate.

FIGURE A.4.
Heterogeneous employer responses by firm size



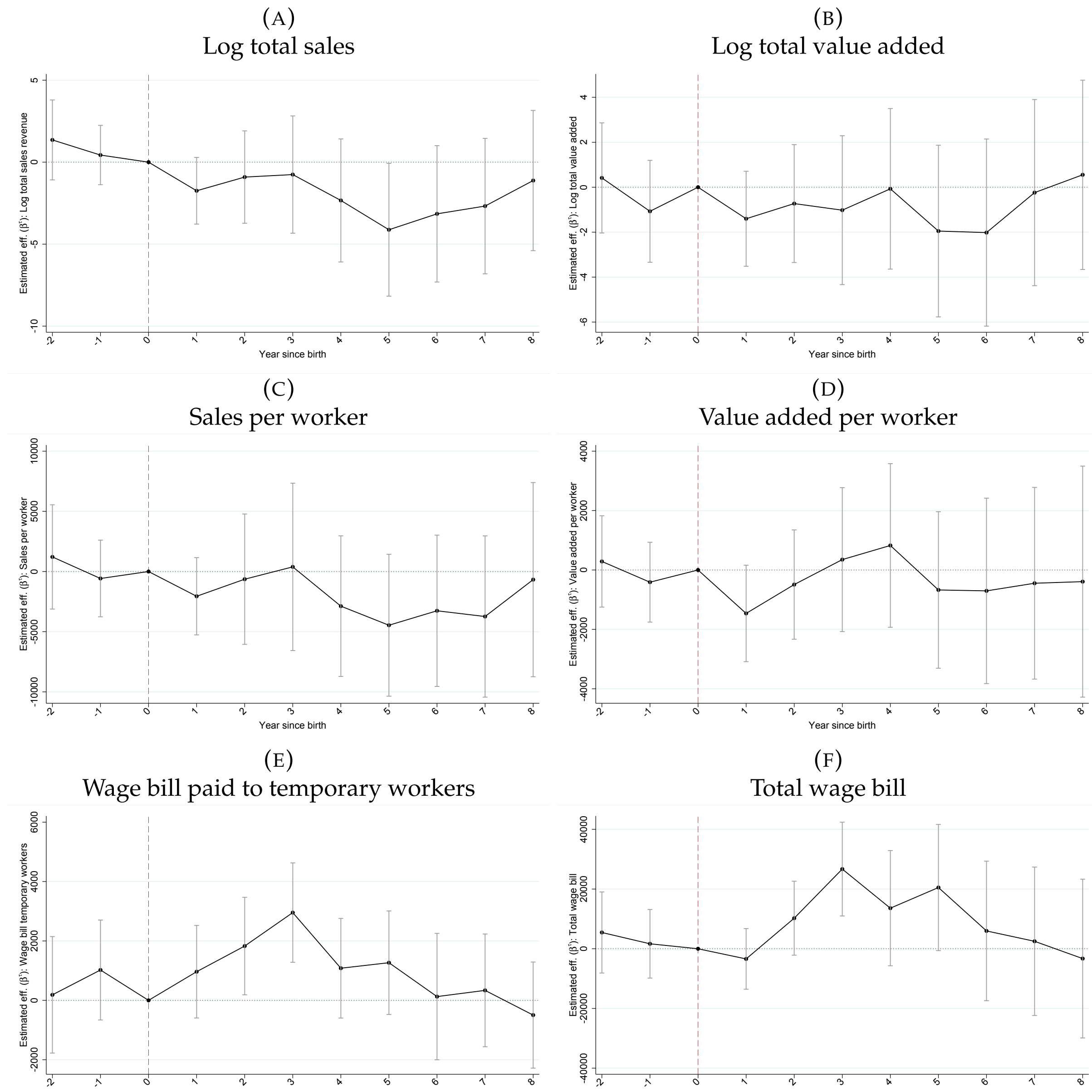

NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., $\hat{\beta}^{\tau}$, from Equation 3, with the corresponding 95% confidence intervals.

FIGURE A.5.
Decomposing employer responses: primary workers' hours increases or temporary replacement workers? Public sector




NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., $\hat{\beta}^{\tau}$, from Equation 3, with the corresponding 95% confidence intervals.

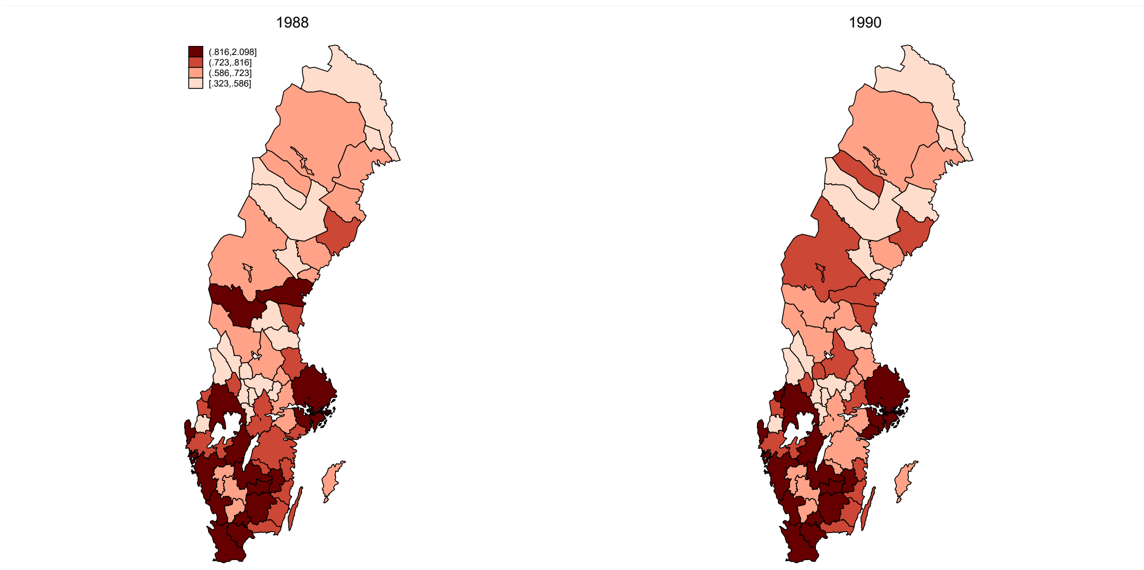
FIGURE A.6.
Effects of the reform on manufacturing firms' sales revenue, value added, and wage bill




NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., $\hat{\beta}^{\tau}$, from Equation 3, along with the 95% confidence intervals.

FIGURE A.7.

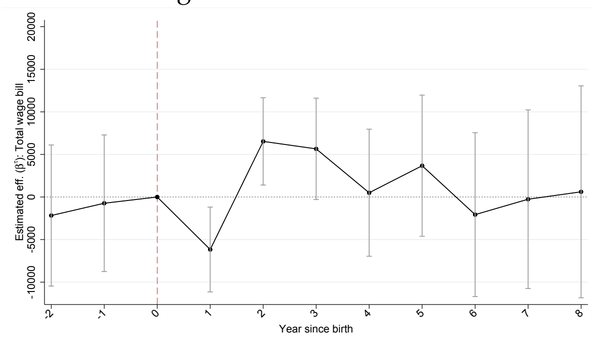
Regional thickness measure for finance industry, 1988 and 1990



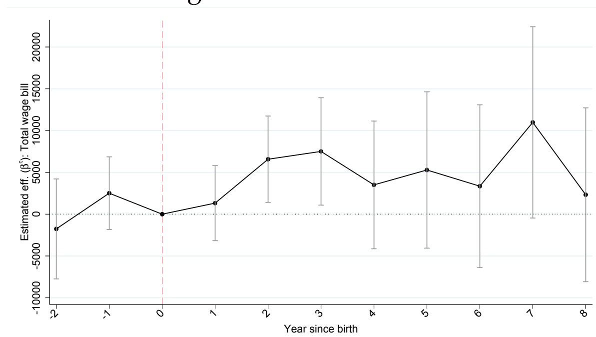

NOTE: The figure shows the female employment shares in the finance industry in each commuting zone in Sweden, according to the formula in (4).

FIGURE A.8.
Heterogeneity in total wage bill costs of adjustments by internal and external substitutability

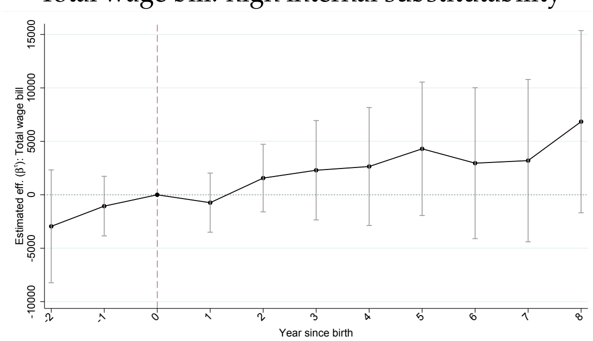
(A)
Total wage bill: thick external markets



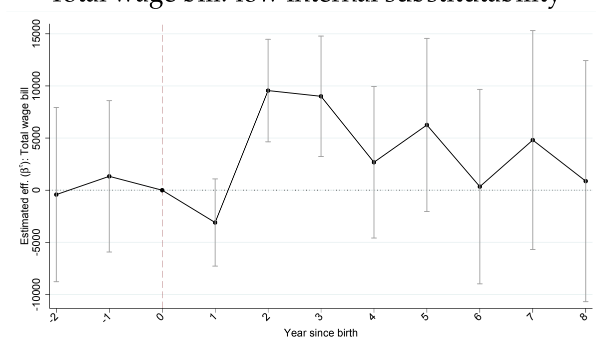

(B)
Total wage bill: thin external markets




(C)
Total wage bill: high internal substitutability



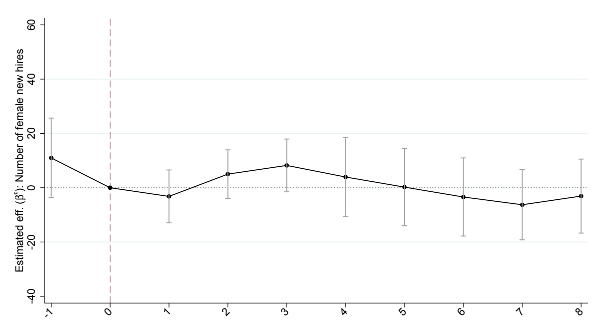

(D)
Total wage bill: low internal substitutability



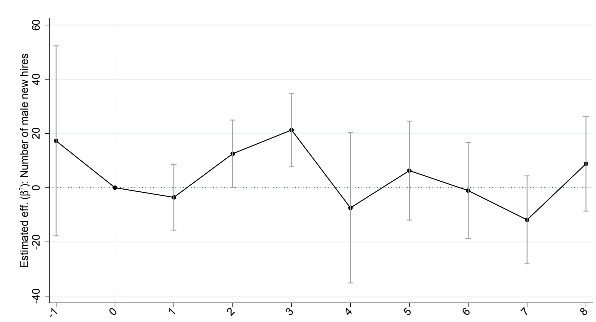

NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., $\hat{\beta}^{\tau}$, from Equation 3, with the corresponding 95% confidence intervals.

FIGURE A.9.
Gender composition of new hires: firms in thick local labor markets

(A)
New female hires




(B)
New male hires




NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j$, i.e., $\hat{\beta}^{\tau}$, from Equation 3, along with the 95% confidence intervals.

FIGURE A.10.
Predicted reform exposure by commuting zone in the Manufacturing, Education, and Health industries

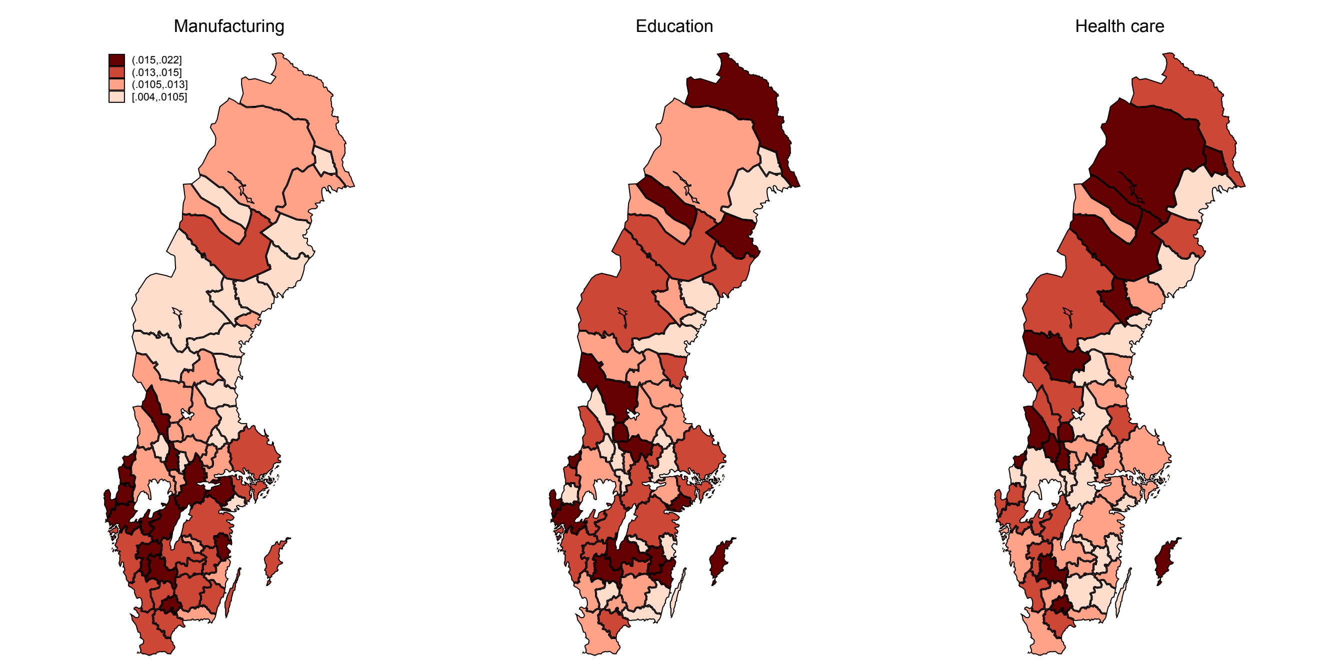
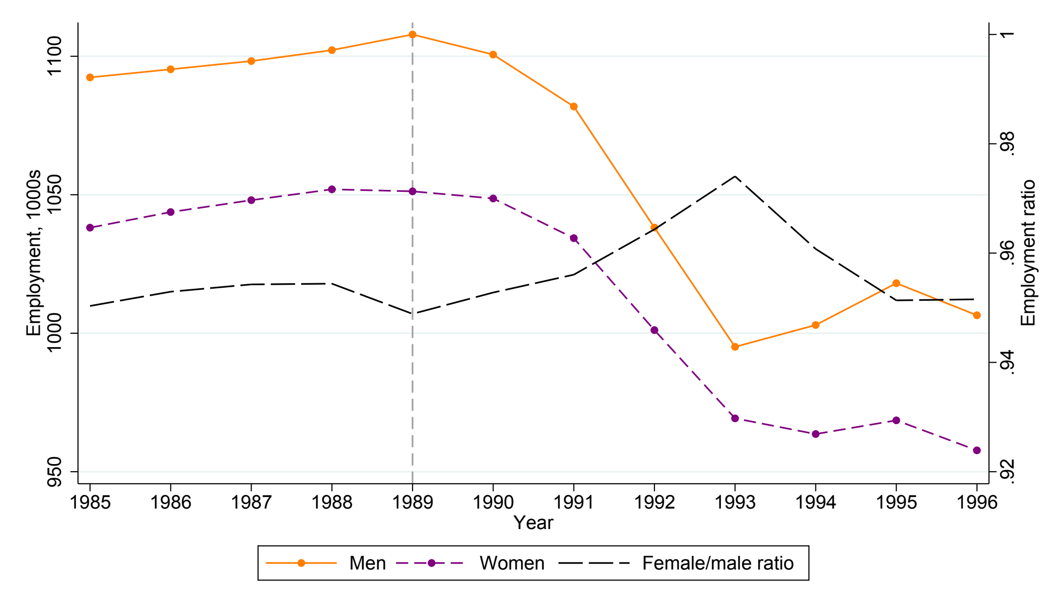


FIGURE A.11.
Nationwide employment trend by gender 1985–1996

(A)
Employment, ages 16–65


(B)
Employment, ages 21–39




NOTE: Total employment during the period 1985-1996 in Sweden.

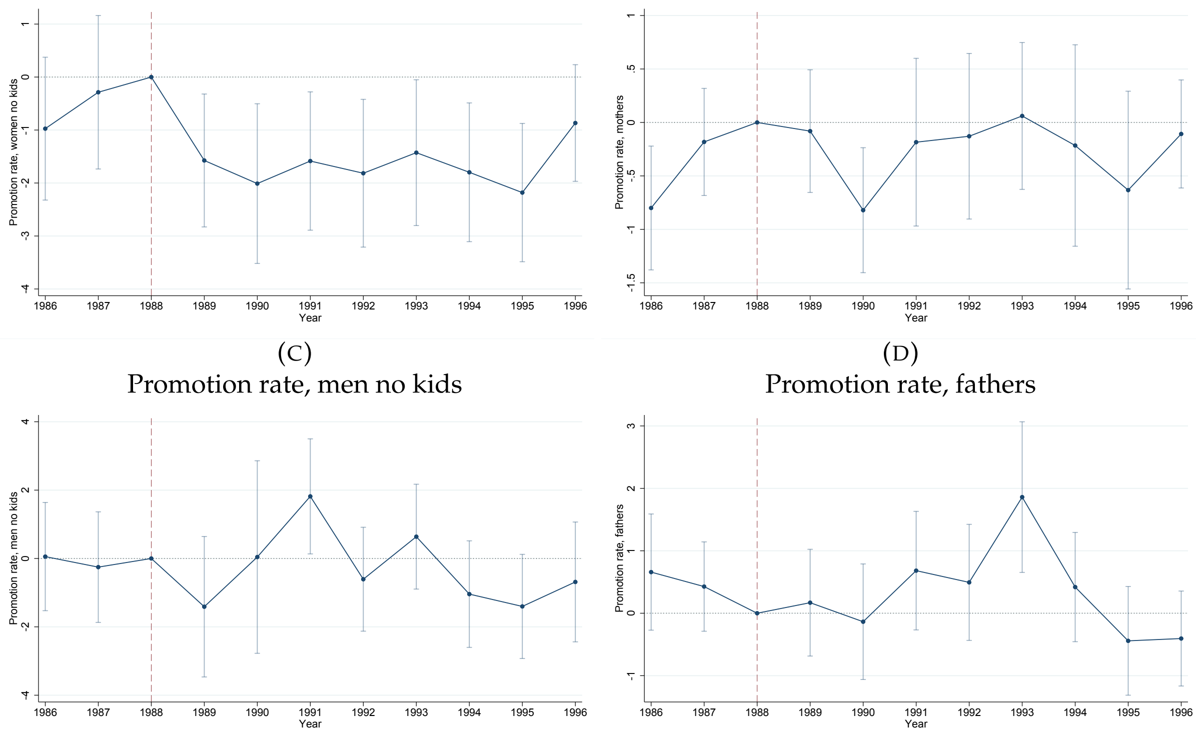
FIGURE A.12.
Effects of predicted reform exposure on promotions

(A)
Promotion rate, women no kids

(B)
Promotion rate, mothers

(C)
Promotion rate, men no kids

(D)
Promotion rate, fathers




NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in industry $k$ and region $c$, $\pi^{p}_{c,k}$, before and after its implementation, that is, the estimates for $\gamma^{t}_{1}$:s in Equation 4, along with the 95% confidence intervals.

TABLE A.1.
Effects of the reform on total fertility

| | All | Private sector | Public sector |
|---|---|---|---|
| Treated | -0.006 | -0.006 | -0.008 |
| | (0.010) | (0.017) | (0.012) |
| $N$ | 141,145 | 42,924 | 88,126 |

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 2007. The table reports estimates of $\hat{\beta}$ from the following equation:

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

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.

TABLE A.2.

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 | 28.803 | 28.204 | -13.173 | 28.707 | 28.187 | -11.762 | 0.078 | 1.235 |
| No college | 0.720 | 0.737 | 3.936 | 0.731 | 0.745 | 3.468 | -0.002 | -0.429 |
| College | 0.280 | 0.263 | -3.936 | 0.269 | 0.255 | -3.468 | 0.002 | 0.429 |
| Labor income (1000s SEK) | 124.068 | 118.783 | -9.376 | 125.338 | 120.106 | -9.216 | 0.053 | 0.066 |
| Monthly wage (1000s SEK) | 15.799 | 15.796 | -0.048 | 16.470 | 16.414 | -1.171 | -0.054 | -0.822 |
| Contracted work hours | 0.860 | 0.862 | 1.075 | 0.868 | 0.875 | 3.479 | 0.005 | 1.574 |
| Private sector | 0.296 | 0.314 | 4.223 | 0.320 | 0.338 | 4.443 | 0.001 | 0.138 |
| Child parity | 1.811 | 1.791 | -2.514 | 1.814 | 1.806 | -0.915 | 0.013 | 1.155 |
| Child spacing (months) | 28.773 | 28.169 | -1.746 | 27.644 | 27.436 | -0.639 | 0.396 | 0.834 |
| Joint $\chi^2$ | | | | | | | 8.35 | |
| $p$-Value | | | | | | | 0.400 | |
| Observations | 45,237 | 16,463 | | 47,304 | 17,375 | | 126,379 | |

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.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 | 1,597 | 4.078 | 674 | 4.678 |
| 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 | 23.765 | 3,321 | 23.050 |
| Electricity, gas and water | 265 | 0.677 | 48 | 0.333 |
| Construction | 4,017 | 10.258 | 423 | 2.936 |
| Wholesale and retail trade etc | 10,445 | 26.673 | 4,231 | 29.366 |
| Hotels and restaurants | 2,230 | 5.695 | 1,088 | 7.551 |
| Transport and communications | 2,187 | 5.585 | 572 | 3.970 |
| Financial intermediation | 1,345 | 3.435 | 684 | 4.747 |
| Real estate, renting, other | 730 | 1.864 | 243 | 1.687 |
| Data management operations | 509 | 1.300 | 209 | 1.451 |
| R&D | 91 | 0.232 | 40 | 0.278 |
| Other business activities | 2,714 | 6.931 | 1,172 | 8.134 |
| Public adm, defense, social ins | 34 | 0.087 | 20 | 0.139 |
| Education | 774 | 1.977 | 386 | 2.679 |
| Health and social work | 642 | 1.639 | 355 | 2.464 |
| Lobbying, and religious act | 592 | 1.512 | 301 | 2.089 |
| Recreation, culture, sports | 857 | 2.189 | 405 | 2.811 |
| Total | 39,159 | 100 | 14,408 | 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 | 14.329 | 14.245 | -0.598 | 14.113 | 14.232 | 0.892 | 0.203 | 1.048 |
| Number of female workers | 8.265 | 8.198 | -0.379 | 8.059 | 8.179 | 0.719 | 0.187 | 0.770 |
| Number of male workers | 6.065 | 6.048 | -0.096 | 6.054 | 6.053 | -0.007 | 0.016 | 0.065 |
| Number women aged 20-40 | 3.603 | 3.560 | -0.392 | 3.493 | 3.508 | 0.143 | 0.058 | 0.382 |
| Private sector | 1.000 | 1.000 | . | 1.000 | 1.000 | . | 0.000 | |
| Average age | 35.299 | 34.636 | -2.393 | 35.180 | 34.910 | -0.993 | 0.392 | 1.009 |
| Share female | 0.548 | 0.548 | 0.031 | 0.541 | 0.546 | 0.406 | 0.004 | 0.258 |
| Private sector | 1.000 | 1.000 | . | 1.000 | 1.000 | . | 0.000 | |
| Avg wage | 19,000 | 19,000 | 0.479 | 19,000 | 19,000 | -0.762 | -373.664 | -0.872 |
| Female work hours | 0.924 | 0.933 | 0.957 | 0.928 | 0.927 | -0.100 | -0.010 | -0.768 |
| Male work hours | 0.983 | 0.983 | 0.225 | 0.983 | 0.980 | -1.055 | -0.004 | -0.896 |
| Wage bill primary workers, 1000s SEK | 2100.000 | 2100.000 | 0.671 | 2100.000 | 2100.000 | -0.340 | -49.946 | -0.716 |
| Wage bill temporary workers, 1000s SEK | 188.094 | 182.024 | -0.395 | 225.825 | 196.467 | -1.001 | -23.288 | -0.683 |
| Share no college | 0.888 | 0.879 | -1.113 | 0.876 | 0.887 | 1.497 | 0.020 | 1.842 |
| Share college | 0.112 | 0.121 | 1.113 | 0.124 | 0.113 | -1.497 | -0.020 | -1.842 |
| 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 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 | 0.465 | 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 |
| Avg. contracted working hours | 0.945 | 0.067 |
| Female work hours | 0.884 | 0.116 |
| Male work hours | 0.983 | 0.035 |
| Average wage (SEK) | 21,252.004 | 3324.384 |
| Female wage (SEK) | 17,810.533 | 2409.006 |
| Male wage (SEK) | 23,349.020 | 3959.708 |
| Female income (SEK) | 132,810.055 | 37911.644 |
| Male income (SEK) | 196,854.005 | 49456.096 |
| Sales per worker (1000 SEK) | 1,025.371 | 1146.286 |
| Value added per worker (1000s SEK) | 484.238 | 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.