

Family Leave Programs: Employer Responses and the Gender Wage Gap*

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Abstract

Job-protected and paid family leave may have unintended consequences when we consider firms' equilibrium responses. In the presence of labor market frictions, employers might incur adjustment costs upon workers' absence and turnover. We quantify the costs faced by firms that employ workers of varying durations of family-leave. Exploiting exogenous variation from a 3-month parental leave expansion in Sweden, we find that women worked 2.5 months less after childbirth whereas men worked only 1 week less. Moreover, the reform increased the probability that women separate from their pre-birth employers. Women with fewer substitutes within the workplace took up less leave and shifted take-up to their spouses, suggesting that workers internalize their employer's adjustment costs. Firms with greater exposure to the reform responded to the labor shortage by hiring temporary workers and increasing incumbents' hours. The total wage cost of these adjustments was over and above the salary for the person on leave, suggesting that such reorganization is costly. We document substantial heterogeneity in firms' strategies by the ease with which replacement workers could be found, indicating several sources of frictions associated with worker absence and exits. Finally, focusing on all active firms in the economy, we find suggestive evidence that firms with a higher predicted reform exposure reduced their relative hiring of women of childbearing ages, and those hired were less likely to be promoted than their male and older counterparts. In the aggregate, the employment composition shifted towards male and older women in industries with a higher predicted reform exposure.

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1 Introduction

Labor markets in industrialized countries have experienced substantial changes with respect to the relative positions of men and women. During the last century, there has been a gender convergence in labor force participation, paid hours of work, occupations, and women have overtaken men in education. Although a narrowing has also occurred between men's and women's wages, a pronounced residual gap remains after controlling for detailed observable characteristics, even at early stages of the career (see Figure 1).¹

An extensive literature documents that men and women behave very differently in the labor market. In particular, women have higher separation rates from their employers, especially after having children (see e.g. Angelov et al., 2016; Kleven et al., 2019; Hotz et al., 2017). To illustrate, we show in Figure 2 that women's employment-to-non-employment transitions increase considerably relative to those of men after becoming parents. In this paper, we probe the extent to which the residual gender wage gap might be a result of employers' statistical discrimination against women, due to the presence of adjustment costs associated with worker exit. For example, if replacement workers are difficult to find or temporary workers are less productive (because they lack firm-specific human capital), employers may transfer such costs to workers with higher expected turnover.

In anticipation of different labor market behaviors and turnover rates of men and women of child-bearing ages, firms could statistically discriminate against women in at least three ways. First, profit-maximizing firms could incorporate the costs of frictions, and offer lower wages to women. Second, firms may refrain from training female workers if they are likely to exit in the next period.² Third, employers might simply hire men instead of women, because women's careers are expected to have more or longer interruptions. There has been substantial effort in the search theory literature to model frictions,³ but direct empirical evidence on the extent of employers' adjustment costs for labor turnover is limited. Even less is known about the extent to which such costs translate to statistical discrimination. We quantify the costs faced by firms in replacing workers on family-related leave and examine whether employers pass through these costs onto male and female workers' employment, wages, and promotions.

The existence and magnitude of these channels have important implications for policy. If labor

¹See Goldin (2014), Blau et al. (2016) and Altonji and Blank (1999) for surveys on the gender wage gap.

²See Lazear and Rosen (1990); Thomas (2019) for evidence on different promotion rates of male and female workers.

³Bowlus (1997) finds that different labor market behaviors of men and women in the U.S. explain 20% to 30% of the wage gap in equilibrium

turnover is costly for firms, family leave policies may either alleviate or exacerbate any statistical discrimination on the part of firms. On the one hand, job-protected parental leave (PL) arguably allows women to retain job-specific human capital, and wage-replaced leave benefits may encourage women's labor force participation. Firms may thus benefit by retaining the workforce they have trained. On the other hand, generous leave durations may have unintended consequences when we consider adjustment costs faced by firms in the presence of frictions in the labor market.⁴ In particular, if family leave programs are disproportionately taken up by women, extensive leave entitlements may have implications for gender wage differentials.⁵

How large are the costs faced by firms when their employees go on extended family leave? To what extent do firms pass through these costs onto men and women's wages, promotions, and employment probabilities? A reform in the Swedish parental leave system provides an ideal setting to study these questions, and we proceed in the following two steps. First, we provide direct empirical evidence on the existence, magnitude, and sources of frictional costs associated with worker absence and exits. To this end, we exploit exogenous variation across firms in their exposure to extended leave durations of their workers. This analysis focuses on the subset of firms that had employees who gave birth around the cutoff date of the policy implementation. Second, we study the extent to which the policy intervention altered the personnel policies of all firms in the economy. In anticipation that all women will take an additional three months of leave in the post-reform regime, forward-looking firms might adjust the gender composition of their new hires and/or offer different wages. To this end, we compare the employment probabilities, wage offers and promotions of workers in the at-risk population relative to other workers who were hired at new jobs before and after the policy change.

For the first part of the analysis, we rely on the fact that the reform was unanticipated and retroactive: it was implemented in July 1989, but covered parents to children born in October 1988 and later. Eligible mothers had the possibility to extend their leave and delay coming back to work by an additional three months, and firms were by law obligated to accommodate. Thus, the policy intervention implied that firms unexpectedly and on short notice had to find replacement workers to cover for the additional leave, making it close to an ideal natural experiment to empirically test for adjustment costs. We use population-wide matched employer-employee data to analyze workplace-level demand for incumbent

⁴The 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).

⁵For a discussion on the potential link between family leave programs and statistical discrimination against women in Sweden, see Albrecht et al. (2003, 2015, 1999).

and external labor inputs. Our sample includes establishments in both the private and public sectors, and both small and large establishments.⁶

Since employer responses 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 auxiliary data on parental leave spells, we show that eligible mothers delayed their return to work by on average 2.5 months. The increase in male take-up was substantially smaller – only one week on average. Thus, the reform predominantly altered the absence duration of mothers. We document that women took 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, women with fewer potential substitutes within the workplace used up less leave and shifted take-up to their spouses, suggesting that workers internalize some of their employer's costs of finding suitable replacement. This latter finding is particularly interesting in light of recent evidence suggesting that women disproportionately switch to more occupationally specialized firms after becoming parents, as shown in Hotz et al. (2017). Finally, the reform increased the probability that women leave for a different firm by 18% in the year when parental leave ended, which we interpret as voluntary switches due to extended possibilities for job search (while on leave). The heterogeneity in take-up by the number of within-firm substitutes, and the increased job-job transition rate that we document here are novel findings and suggestive of the unintended costs of leave programs for both workers and firms.

Given that workers were unexpectedly more likely to permanently exit the firm or take longer leaves, 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 that had a child between October and December of 1988, which entitled them to 3 additional months of parental leave. Because the reform was unanticipated, women could not have manipulated the timing of birth, and firms had no possibility of altering their workforce composition to avoid having workers on extended leave. Thus, treatment intensity at the firm level is plausibly orthogonal to the unobserved determinants of the outcomes that we study. We compare the firms with the same number of women who gave birth in the baseline year, and take advantage of the exogenous variation in the *timing* of childbirth that gave rise to different leave durations. Note that any impacts on firms' re-organization costs in our setting are over and above those related to

⁶Our main sample restriction is that the firms have at least one female employee giving birth in some baseline year. We show that these establishments are representative for the full population of Swedish establishments in terms of several workplace attributes.

having a worker go on child-related leave *per se*. To take potential seasonal effects into account, we also define a corresponding measure for firms that employ women who gave birth in the preceding year, and use an 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.

Our results show that private sector firms responded to the reform by increasing their 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 the salary cost of half 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 salary of replacement staff.

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).

The ease with which firms can replace workers on leave depends on several factors: whether internal and external labor inputs are substitutable, and whether external labor market conditions are favorable for hiring. We find that 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 that are occupationally specialized – responded to the labor shortage by relying more heavily on internal workers. Taken together, our findings highlight several sources of frictions associated with finding suitable replacement for workers on leave.

What are the implications of such adjustment costs for women's relative labor market opportunities? To address this question, we analyze the aggregate effect of the reform on employment, exploiting variation in *predicted* exposure, based on age-specific fertility rates and the demographic composition of the workers-pool, across industries within local labor markets. The intuition is that predicted exposure

is correlated with actual take-up, but also captures if employers react on expectations of higher costs associated with hiring women of childbearing age after the reform. Using a difference-in-differences design, we find that in industries with higher predicted exposure, the ratio of women in childbearing age (“young” women) to all employed declined after the intervention, and this effect persisted over our eight year follow-up horizon. In the short run, the decline was driven by a reduction in employment of young women and a simultaneous increase in male employment. In the longer run, the employment composition shifted towards older women and male workers. With the research design at hand, we are not able to rule out that these effects arise because the reform reduces the supply of young women due to them being on extended PL. However, when zooming in on the hiring decisions of individual firms in the sample of all firms in the economy, we find that the fraction of new hires that are women of childbearing age (relative to all new hires) declined relatively more in firms with a higher *predicted* reform exposure immediately after the new policy was enacted. Moreover, we find that in more exposed firms, the promotion probabilities of new female young hires (during their tenure with the new firm) relative to all other new hires in the same month and firm declined among those hired immediately after the reform. We interpret these findings as suggestive evidence that the reform had unintended consequences on 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 e.g. Jäger and Heining (2019), we test empirically for the presence of frictions by using exogenous worker exits.⁷ 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 and public sector organizations in the overall economy, as opposed to case studies of certain industries or sectors.

Second, we contribute to the growing literature on parental leave programs. While there has been substantial work on the impact of parental leave programs on women’s careers and children’s outcomes, (Schönberg and Ludsteck, 2014; Lalive and Zweimüller, 2009; Lalive, Schlosser, Steinhauer, and

⁷See 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.

Zweimüller, 2013; 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 reforms on co-worker and firm 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 (e.g., delayed fertility and increased sickness absence). Friedrich and Hackmann (2017) finds a negative impact on patient outcomes in Danish hospitals and health centers that experienced a negative supply shock of nurses, which are not easily substituted due to being a licensed and female dominated profession. 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 employees' birth timing combined with matching techniques to define control events. They find no effects on firm outcomes. 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. Moreover, we study impacts of parental leave for public and private sector workplaces of various sizes.

Third, our paper relates to studies on statistical discrimination. Gruber (1994) considers labor market effects of maternity leave mandates which potentially raise the costs of employing women of childbearing ages, and the extent to which these costs are shifted to that particular groups' wages, using regional variation across U.S. states. 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 enacted was three percentage points more likely to remain employed, but eight percentage points less likely to be promoted, conditional on employment, relative to if she had been hired before the introduction of the FMLA, and that this widening of the gender gap in promotions is only observed for women under the age of 40. The firm-level analysis in our paper provides results that are in line with the aggregate effects found in Thomas (2019). All in all, our paper suggests that the group of workers whom family policies are aimed to help ultimately may bear part of the costs of the policy in the labor market.

The rest of the paper is organized as follows: section 2 describes the Swedish parental leave system and the reform that we exploit; section 3 describes the data sources used. In section 4 we present the impacts on program take-up and the labor market outcomes of women directly affected by the parental leave expansion. In section 5 we present results of extended family leave on employer outcomes, and

in section 6 we explore potential sources of frictional costs. In section 7, we analyze whether firms pass on the costs of the reform to future new hires and equilibrium effects in the labor market. section 8 concludes the paper.

2 Background & Institutional Setting

In this section we describe the historical development of the parental leave system in Sweden, with a focus on the particular features of the institutional setting that was in place during our observation period.

The Swedish parental leave system, with 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 (see Table A.1 for all changes to the parental leave system in Sweden up to 2010). 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.⁸

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.⁹ The wage-replaced benefits are conditioned on at least 240 days of employment before child birth. Individuals that do not fulfill the work requirement of 240 days pre-birth employment get a low daily amount of benefits. 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. Thus, for the employer, the direct costs of employee absence due to child rearing are associated with finding and hiring replacement workers, and potential foregone productivity.

The parental leave is job protected, and it 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 parental leave benefits on a part-time basis.¹⁰

⁸In 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.

⁹Currently, the replacement rate is 80 percent of previous earnings (see Table A.1).

¹⁰Before 1989, parental leave benefits could be used until the child's fourth birthday. In 1989, the rules changed so that benefits could be claimed on a part-time basis until the child's 8:th birthday. The new rules also retroactively covered parents to children born 1986 or later *and* still had parental leave benefits left by 1989.

However, the vast majority of parental leave benefits is taken-up during the child's first two to three years of life (see Figure A.1 in the Appendix).

Over and above the right to claim parental leave benefits (and thereby reduce working hours) on a part-time basis, the Parental Leave Act stipulates that parents are entitled to reduce their working hours with up to 25 percent until the child turns eight years old, irrespective of whether they have parental leave benefits to claim. Workers have to inform their employers about working-time reductions and parental leave at least two months in advance, but employers are prohibited to deny the reduction given that this requirement is met.¹¹

2.1 The Right to Return to Previous Job

By the Parental Leave Act, a worker has the legal right to return to the same job as he/she had before parental leave, where a *job* is characterized as the combination of tasks and salary. However, if the tasks are no longer relevant when the employee returns to the workplace after a leave spell - due to e.g., re-organizations - the employer is obligated to find a similar position within the firm, with the same pay as the job they occupied before the parental leave.

2.2 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. Between 1980 and 1989, parents were entitled to 12 months of paid leave, of which three months were compensated at the lower fixed rate of 60 SEK per day. In this paper, we study the largest expansion of parental leave entitlement during this time period; an extension of paid leave from 12 to 15 months. The three additional months of paid leave concerned the wage-replaced component of benefits. The reform was implemented on July 1st 1989, but retroactively covered parents to children born in October 1988. Transition rules following the implementation implied that parents to children born in August and September 1988 received one and two additional months of paid leave, respectively.¹²

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 parental leave 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

¹¹The work-time reduction was initially supposed to be spread out over all days of the workweek, for instance with 1 hour per day, but since 2001 it can be used to reduce work time during one day of the week.

¹²This reform was studied in Liu and Skans (2010), who examined the effect of the duration of parental leave on children's scholastic performance.

easily identify and distinguish between workers eligible for different durations of leave, and between firms by the extent to which their female employees are entitled to different durations of leave according to the month that employees gave birth. 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 multiple population-wide administrative data sets covering both workers and firms. Individual level data on childbearing (date of birth, parity, etc.) are matched with individual level panel data on annual labor income drawn from tax registers, along with 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 establishment 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; this is an annual survey of establishments that collects 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 firms, establishments, and organizations within 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.¹³ Our analyses of employer responses to extended employee absence will cover

¹³Sample weights are included in the data, which permits calculation of nationally representative statistics.

establishments in both the public and private sectors, and both small and large workplaces.

All registers that we use cover the years 1985 through 2013, but for most analyses in this paper we use data up to 1996.¹⁴

4 Program Take-up

We begin by quantifying the program take-up at the individual worker level using variation in eligibility status by date of children's birth. The effects of the policy intervention on program take-up and labor supply will facilitate interpretation of potential employer responses to the reform.

4.1 Empirical strategy

The 1989 parental leave reform increased the entitlement to wage-replaced parental leave benefits from 12 to 15 months. The reform was implemented on July 1, 1989, but retroactively covered parents to children born in October 1988 and later. Moreover, transition rules granted parents to children born in August and September of 1988 to one and two additional months of benefits, respectively.

Our research design exploits that women who gave birth in 1988 were as good as randomly assigned entitlements 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 placebo year. Thus, we implement a difference-in-differences (DD) methodology where the identifying assumption is that any birth month effects are similar across years.¹⁵ We sample all women who give birth (irrespective of birth 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 *placebo cohort*.¹⁶

Let M_{im} be an indicator for woman i giving birth in calendar month m , where $m = \{1, \dots, 12\}$. D_i indicates whether mother i gave birth in the treatment year, and thus takes the value 0 if i gave birth in the placebo year. We analyze the effect of extended paid leave entitlements on woman i 's program take-up and labor supply outcome, denoted y_i , by estimating the following regression equation:

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

¹⁴Throughout the paper all monetary values are measured in SEK as of 2015.

¹⁵This strategy also addresses potential unobserved heterogeneity by season of birth, e.g. as documented in Buckles and Hungerman (2013).

¹⁶For most of our analyses, the placebo cohort will be comprised of women giving birth in 1987.

with $m = 7$ as the omitted category. The coefficients of interest are the β^m :s, which capture the difference in the outcome variable (y_i) between individuals giving birth in calendar month m compared to giving birth in July for those gave birth in 1988, net of the corresponding difference among those who gave birth in the placebo 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 placebo-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 gives 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.

4.2 Parental leave benefit take-up

We begin by analyzing the effect of the reform on the take-up of parental leave benefits. The data 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 in the administrative registers are not assigned to specific children (it contains identifiers only for the parents, not for the child for whom the parental 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. Second, since we lack data on parental leave 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, i.e., are given an additional three months of paid leave, there should be no difference in the leave take-up between those who give birth in different months of 1989.

We estimate the regression equation (1) on the cumulative number of (gross)¹⁷ days on parental leave during the child’s first three years of life. We choose this follow-up horizon because, as we show in Figure A.1, the vast majority of PL benefits are claimed within this time period. Panel A of Figure 3 plots

¹⁷Benefits 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.

the estimated coefficients $\hat{\beta}^m$:s from estimating equation (1) for women. The results show that take-up of leave benefits is monotonically and linearly higher by month of birth starting with the August-births, in line with the transitional implementation of the policy intervention. The absence of significant differences between women giving birth in January–June (compared to July) of 1988 and 1989 provides support for our parallel trends assumption. Moreover, the reform seems to have had more or less full impact over this short (three-year) follow-up horizon, for all three treatment intensities (1–3 months).¹⁸

In Panel B of Figure 3 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 of parental leave benefits. Nevertheless, considering the low level of leave among men at the time of the reform, the effect on fathers’ take-up is substantial in relative terms (roughly a 20 percent increase relative to the baseline). It is interesting to note these effects considering that none of the additional benefit entitlement considered here are earmarked specifically to the father.

Figure 4 shows heterogeneous take-up responses by sector of employment (measured at the pre-birth employer). The reform had larger effects on the take-up of private sector workers compared to women employed in the public sector. This could happen if, for example, additional benefits crowded out unpaid (but job-protected) leave to a greater extent among public sector workers. In the next section, we thus analyze the labor supply responses to the extra paid leave entitlements.

In Table 1, we present estimates separately by gender and sector of employment, for different follow-up horizons, using a static difference-in-differences model where we exploit the full three month extension and thus exclude the sample of women who gave birth in August and September. Specifically, let T_i be 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.¹⁹ We then estimate:

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

where y_i denotes parental leave take-up pooled over the first three years of life (columns 1–3 in Table 1) or over the first eight years (columns 4–6). In the private sector, being entitled to three more months of PL benefits increased take-up during the first three years after birth by, on average, 2.5 months among

¹⁸Note that the “dip” in the magnitude of the point estimates for the birth months of November and December in Figure 3 is an artifact of parental leave take-up not being measured in exact child age in months.

¹⁹We discuss this modified specification in closer detail in section 4.4 below. 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.

women and around one week for men. In the public sector, the increase in take-up among women amounted to roughly two months, and among fathers almost two weeks. Looking at the take-up over child ages 0–8, it appears that women in the public sector spread out some of their additional leave benefits beyond the earliest child ages, but for both sectors, the bulk of the extra leave entitlements are used relatively soon after the policy announcement.

4.3 Labor supply response

We now turn to our primary data set with matched employer-employee information and estimate the regression 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 (irrespective of parity), and a corresponding placebo cohort of women giving birth in 1987, i.e. the year before eligible women gave birth. We analyze the employment and labor supply decisions using data on annual labor income which includes earnings from employment, but not governmental transfers. However, it may include top-up of parental leave benefits that are stipulated in some collective agreements. Moreover, even though labor income is a function of both hours worked and hourly wages, we argue that short-run fluctuations in labor income at the individual level are more likely driven by hours worked rather than wage-adjustments. Thus, we interpret any responses to this outcome as (intensive margin) labor supply responses.

The estimated coefficients on the interaction terms between year of birth and calendar month of birth indicators, i.e., the $\hat{\beta}^m$:s from equation (1) are plotted in Figure 5, for women’s labor income in the year after birth. The results are in line with those for parental leave benefit usage: there are no differences in the earnings of women giving birth in January–July of 1988 compared to January–July 1987. However, women giving birth from August 1988 onwards have significantly lower earnings compared to their counterparts in 1987. The decline in earnings is more or less linear in the calendar month of birth – again, in line with the transition rules. In the next section, we explore the full temporal pattern of the policy intervention on women’s post-birth earnings trajectories using a difference-in-differences design combined with an event-study.

4.4 Long-run labor supply response

The results presented in Section 4.2 show that the reform had full impact, and that the effect was linear in the number of additional months received. In the remainder of the analyses, we make use of the *full* reform of three months additional benefits, and thus ignore the transition rules (that gave 1 and 2

additional months to August–September parents). To this end, we drop all women who gave birth in either August or September. Under the assumption that month of birth is as good as randomly assigned, this sample restriction poses no threat to identification. Thus, for the samples used in the remainder of the paper, birth months correspond to $m_i = \{1, \dots \neq 8, \neq 9, \dots, 12\}$. In Table 2 we show that although pre-determined covariates differ across women with varying months of births within a year, these covariate differences are balanced across cohorts, i.e., across women giving birth in 1987 and 1988. Second, to analyze the time pattern of the effect of the reform on women’s labor supply, we estimate a dynamic difference-in-differences regression equation which, in addition to providing the temporal pattern of the labor supply responses by time since birth, has the advantage of assessing the validity of the identifying assumption via studying differences in *pre-treatment* outcomes by eligibility status.

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:

$$y_{it} = \delta_0 + \sum_{\tau=-2}^8 \beta^\tau (T_i \cdot D_i \cdot \tau_{it}) + \sum_{\tau=-2}^8 (\delta_1^\tau \tau_{it} + \delta_2^\tau T_i \cdot \tau_{it} + \delta_3^\tau D_i \cdot \tau_{it}) + \delta_4 T_i \cdot D_i + \delta_5 T_i + \delta_6 D_i + \mathbf{X}_i' \gamma + \epsilon_{it} \quad (2)$$

with event-time indicators τ_{it} for each year relative to the baseline year (year birth of individual i ’s child, i.e., 1987 or 1988).²⁰

The coefficients of interest are the β^τ ’s, which measure the difference in outcomes between women giving birth in October–December versus Jan–July of 1988 (first difference), to the corresponding difference among women giving birth in 1987 (second difference), in each year before and after birth, relative to the calendar year of birth (third difference).

We first estimate equation (2) for two outcome variables: the annual labor income earned (from employment), and a binary variable that denotes labor market participation defined as having labor earnings above a certain threshold, annually. The estimated coefficients $\hat{\beta}^\tau$ in 2 are presented in Figure 6, and show that women entitled to additional paid leave reduce their labor supply in the first two years after giving birth, but not in the longer run. Similarly, Panel B of Figure 6 shows that participation is negatively affected mainly in the short run. Thus, the longer leave entitlement does not cause women to opt

²⁰Namely, $\tau_{it} = \begin{cases} 1[t - 1988 = \tau] & \text{if } D_i = 1 \\ 1[t - 1987 = \tau] & \text{if } D_i = 0 \end{cases}$

out of the labor market entirely, or reduce their hours worked beyond the immediate years after reform announcement. Moreover, the results suggest an immediate impact of the increased leave entitlements, with eligible women delaying their return to the workplace.

4.5 Employer-employee separations

One margin that could have implications for employers is whether the female employees eligible for additional leave stay with the firm throughout the parental leave spell or after the leave has expired. Since parental leave benefits are financed through pay-roll taxes and paid to the claimant by the Social Insurance Agency (as opposed to being employer-provided), a worker can switch jobs while on parental leave without losing the PL benefits. In light of this, extended leave duration may imply a longer period of job-search for those women looking to leave their firm. In Figure 7 we plot the baseline separation hazard by time since birth, and find that 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 the possibility that separations are affected by the extension of parental leave, we estimate equation (2) on the annual likelihood of switching from the pre-birth employer to a new firm. The results are presented in Figure 8 and show that women who are entitled to extended leave are roughly 2.5 percentage points more likely to leave the pre-birth employer in year 2 after birth. Relative to the baseline hazard, this corresponds to an increase of about 18 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 intervention are involuntary.

4.6 Heterogeneous program take-up by the number of potential substitutes

The increased parental leave 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 the employers' difficulties when deciding on how much leave to take up of the three months of additional allowance, especially workers in small establishments or those who know themselves to be in unique positions at their workplace. For example, using data from Sweden, Hensvik and Rosenqvist

(2019) show that workers with few internal substitutes – measured by the number of co-workers with the same occupational title as themselves – have lower levels of absence for temporary illness, driven both by employee adjustments of absence to substitutability and by sorting. In this section, we analyze whether the results found in Hensvik and Rosenqvist (2019) extend to parental leave absence, as it will also 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 workplace.

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. (The median worker in our sample has seven internal substitutes). Specifically, we estimate the model specified in equation (2), for the cumulative take-up of PL benefits over child ages 0–2, controlling for background characteristics, occupation-fixed effects, and indicators for deciles of workplace size. We analyze heterogeneous effects on both own and spousal PL take-up. In this analysis, we thus match couples and look at within-household allocation of PL take-up. The results are presented in Figure 9. Panel A shows the results for own take-up, and panel B for the spousal take-up. While the effects do not show a monotonic pattern, the results suggest that the parental leave take-up response is increasing with the number of substitutes for women with zero, one, and two substitutes. In contrast, spousal take-up is decreasing with the number of substitutes that woman i has, among those with zero to two substitutes. Taken together, these findings suggest that women with few internal substitutes potentially internalize the employers’ adjustment costs of worker absence by shifting parental leave to their spouse.

We next provide an additional test of the hypothesis that workers vary in their absence behavior by their employers’ ability to find replacement, by estimating heterogeneous program take-up effects by workplace size. The intuition is that small firms are generally less able to insure against worker absence, and if workers take such costs into account in their absence decisions, we should see smaller effects of the policy intervention on leave durations for workers in small establishments. We thus estimate heterogeneous effects by workplace size quintile, while controlling for the number of occupational substitutes

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

and occupation-fixed effects. In other words, we compare eligible women with the same number of potential occupational substitutes, within occupations, at firms with a varying number of workers. The results are presented in Figure 10. In line with the results by the occupational substitutability, these results show that workers' own program take-up increases with the size of the firm, while spousal take-up decreases with the size of woman *i*'s workplace.

Thus, the two pieces of evidence presented here indicate that workers adjust their parental leave behavior to their firm's potential ability to insure itself from worker absence. These findings are informative in the context of parental leave extensions as they imply that replacement of temporary absences may not be frictionless, which could result in wage variation across genders conditional on workers not fully internalizing the costs by their absence behavior, and given that there is pass-through of costs from employers to workers.

4.7 Additional results: Subsequent fertility

Finally, in Table A.2 in Appendix we report results from estimating a static difference-in-difference regression equation 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 for any effect of the reform on the completed fertility of either private- or public sector workers (and by gender; see Table A.3 in Appendix).

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 (temporary and/or permanent) reductions in female labor. We sample workplaces in the private and public sectors at which at least one female employee had a child born in 1988. Our identification strategy exploits that workplaces within this set are differentially exposed to varying absence durations of their female employees in a manner that is, conditional on observables, orthogonal to any unobserved determinants of the outcome variables that we study, due to the stochastic nature or birth timing (and that the reform was unanticipated). We drop all establishments in which at least one woman gave birth in August or September in order to make full use of the full three-month extension of leave entitlement. 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 placebo set of firms. We define a treatment intensity measure that captures the extent to which firms are exposed to employees entitled to extended leave durations, relative to their baseline number of workers.

Let b_{ij} denote whether female employee i in firm j gave birth in the baseline year (1988 or 1987), and let $m_i = \{1, \dots \neq 8, \neq 9, \dots, 12\}$ be indicators for the calendar month of birth of individual i . Moreover, let N_j denote the total number of employees in firm j in the baseline year. We then define treatment intensity of firm j as:

$$\pi_j = \frac{\sum b_{ij}^{base} \mathbf{1}[m_i \geq 10]}{N_j^{base}}. \quad (3)$$

We then estimate the following triple-differences specification (similar to estimating equation (2) defined in the section above):

$$\begin{aligned} y_{jt} = & \delta_0 + \sum_{\tau=-2}^8 \beta^\tau (\pi_j \cdot D_j \cdot \tau_{jt}) + \sum_{\tau=-2}^8 (\delta_1^\tau \tau_{jt} + \delta_2^\tau \pi_j \cdot \tau_{jt} + \delta_3^\tau D_j \cdot \tau_{jt}) \\ & + \delta_4 \pi_j \cdot D_j + \delta_5 \pi_j + \delta_6 D_j + \mathbf{X}_j' \gamma + \epsilon_{jt} \end{aligned} \quad (4)$$

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

In the vector \mathbf{X} we include flexible controls for the total number of workers giving birth in the baseline year interacted with indicators for mean establishment size decile before 1989, 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 (20–40 years of age), the education (level) composition at the establishment, and a polynomial in workplace size) and fixed effects for 2-digit industry affiliation.

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 placebo and treatment samples. Having partly overlapping samples of workplaces in both placebo 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 $N = 7,086$). We cluster the standard errors at the treatment cohort-establishment level.

Finally, we note that our placebo 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 would be subject to more future employee births than the other, unless the treatment cohort firms respond to the policy by hiring more women, and these women have higher fertility, in which case 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 direct effects of the reform, whereas long-run results might also include snowballing effects as a consequence of firms’ short-run responses.

5.1 Summary statistics

Based on the mean workplace size before 1989, we exclude the smallest (fewer than ten employees) and the very largest (top 1 percentile of size distribution) establishments from our analysis data. Table 3 we reports summary statistics for pre-determined workplace attributes for our study sample of establishments as well as for the universe of all active establishments and organizations in Sweden in 1988 for comparison. In terms of education composition, earnings, wage rates, and contracted work hours, the establishments in our study sample are similar to the full population of establishments. However, our sample is comprised to a larger extent by public sector workplaces, have a higher share of female employees, more employees giving birth in a given year, and are somewhat larger compared to the average establishment in the population.

Table A.4 in the Appendix reports the industry composition of our sample firms and for all firms active in Sweden in 1988. The industry composition of our study sample is relatively representative of the full population, except for that it is over-represented in terms of Health and Social work industry. These differences between our sample firms and the full population of firms may be of relevance for issues of external validity, but does not matter for identification. What is important for the latter is that treated and control firms are balanced in terms of their characteristics. In Table A.5 we show that differences in characteristics between firms whose employees give birth in the fall vs. spring are balanced across years, for firms with 10–20 employees in which 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 the women on leave. Since the Swedish government pays for the parental leave benefits (at replacement level of 90%) and not all firms top up the remaining 10%, 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 first 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 employer from which they derive their main source of income, if they are registered with more than one workplace in a year). 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. All employees in our sample that gave birth to a child in the baseline year are, due to our sample selection criteria, primary employees. Thus, wage-bill paid to temporary workers excludes women on parental leave, by definition. Wage bill paid to primary workers will include women who gave birth to the extent that they are still on the firm's payroll. Adjustments to this outcome variable can also be driven both by the hours worked or earnings by new (primary) hires and incumbents. To measure work hours supplied by the co-workers of women on leave (new hires and incumbents), 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 percent of a full-time employment (for example, 75%).

Figure 11 presents the coefficients β^τ from specification (4) for firms total wage sum (separately for private sector and public sector establishments). The total wage bill includes all workers; primary as well as temporary employees, and the earnings of women on parental leave. The results show a negative effect on the total wage bill in year one after birth, for both private and public sector workplaces, which could potentially be attributed to the labor shortage caused by the reform. For public sector workplaces, there are no effects on labor costs beyond the first year. However, for private sector workers, 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, had they not delayed their

return to work.

In Panels A and B of Figure 12 we decompose the effect on total wage sums into a component attributed to primary employees and to temporary employees, respectively, for workplaces in the private sector. Results show that the reduction in total wage sums in year one after birth stems from primary employees, which is likely explained by the labor supply shortage caused by the reform, i.e., increased leave duration of eligible workers. The results on wage-bill paid to temporary workers show that firms adjust immediately by increasing their temporary staff, but not sufficiently to fully offset the reduction in primary employees' labor inputs in the first year. For years two and three, however, the combined effects on the salaries paid to both employee types suggest that the adjustment strategy that the average firm employs results in net increases in their wage costs. In terms of magnitudes, the estimates imply that in a workplace of average size (around 48 workers), having one female worker entitled to an extended parental leave duration would increase the total wage bill by an amount corresponding to the salary cost for roughly one half a full-time worker. Thus, the effects are sizeable.

The wage bill could be affected by both the number of primary employees (via retentions, separations, and new hires) and their work hours. In Panel C we therefore present the estimated effect on the primary workforce's contracted work hours: Results show that the work hours of the colleagues of women on child-related leave increases in year one, two and three (significant at the 10, 5, and 10% levels, respectively).

In Figure 13 we report the corresponding set of results for establishments in the public sector. 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 discernable 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 the effects on individual-level program take-up were both quantitatively and qualitatively similar (as we show in 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.

Taken together, our analyses show that firms are indeed affected by workers taking longer leave, and

that women taking additional time off for child-rearing could potentially imply that firms would have to incur costs in replacing them. In particular, the findings indicate that adjustment costs are over and above the costs of salary payments for workers on leave, and that the net impact of countering the direct and potential indirect labor supply shocks in the firm's labor cost is positive.

6 Frictions in Heterogenous Labor Markets

We have shown in the previous section that firms are indeed affected by workers taking an extended parental leave duration, and that women taking additional time off could potentially imply that firms would 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 a good substitute for the worker(s) on leave.²² In the following sections, we shed light on the sources of potential frictions facing firms that need to replace absent workers.

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 harder at retaining 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 external labor market conditions. In the following sections, we explore whether firms adopt different replacement strategies depending on whether they face an abundance of potential replacements in their local labor market, and depending on the extent to which co-workers within the firm have the same skills. If finding replacement workers is frictionless, we expect to find no heterogeneous adjustment strategies adopted by firms facing different labor market conditions, for example.

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 (Lazear, 2009). For example, if overtime hours are paid at a premium, firms may look externally for new hires rather than

²²For example, recent work by 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.

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, i.e., in labor markets where workers with the relevant skills are abundant, will have a higher probability of finding replacement workers on the external market. In contrast, in a thin market, firms will arguably find it difficult to replace workers with external hires, and thus may have to resort to internal retention and hours increases.

To capture the external labor market conditions facing the firms in our sample, we construct measures of gender-specific industry-level labor market thickness at the local level. 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:

$$\theta_{kct}^g = \frac{emp_{kct}^g}{emp_{ct}^g} / \frac{emp_{kt}^g}{emp_t^g} \quad (5)$$

for each gender $g = \{0, 1\}$, industry k , commuting zone c , in year t . We also construct an overall measure of thickness which is not gender-specific. The data used to construct this measure is population-wide data on workers (excluding self-employed) aged 19–64. To get a sense of the variation in local labor market thickness in our data, Figure A.2 shows the female labor market thickness for an example industry (financial intermediation) in the 64 commuting zones in Sweden.

We construct dummy variables of thickness as indicators for whether $\theta_{kct}^g > 1$, 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 14 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.

We find no adjustment responses for public sector organizations, in either type of market conditions (not shown but available from the authors).

6.2 Internal substitutability of workers

In Section 4.6, we show that workers potentially internalize employers' replacement costs by adjusting their response to the reform differentially depending on the extent to which they have internal substitutes. In particular, workers with few internal substitutes – measured as the number of co-workers in their own occupation category – made use of significantly fewer absence days compared to women with many (potential) substitutes, suggesting some degree of specificity of human capital. In this section, we explore whether there is pass through of such adjustment to employers.

We characterize the potential for internal substitution possibilities at the workplace by the overall occupational specialization at the establishment. In particular, we follow Cortes and Salvatori (2019) and calculate the employment share in the largest occupation category within the workplace. The intuition is that workplaces with a high degree of occupational specialization should have greater scope for internal substitution across incumbent co-workers. We estimate heterogeneous employer adjustments by the quintile of the occupational specialization index. To this end, we estimate a static difference-in-differences specification separately for firms with varying degrees of specialization, using data on three years before and five years after the focal employees give birth to a child (i.e., the baseline year).

The results are presented in Figure 15: firms with higher internal supply constraints (lower degree of occupational specialization) adopt new hires as their main adjustment strategy, while firms in the upper tail of the specialization index distribution resort to increasing the work hours of incumbents. We argue that it is not likely that these heterogeneous effects are 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 would 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 employ different strategies depending on the fraction of workers with the same (occupation-level) skills suggest 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 Implications of findings for careers of women and statistical discrimination

The predictions which we discuss in the sections above apply to firms' responses to women's absences *in general*, i.e., also in the absence of any policy change. The policy change - in the way we have exploited the reform thus far - then simply provides a natural experiment by which we can empirically test for the

existence of frictional costs. Our findings thus far clearly indicate several sources of frictions which make labor turnover costly for firms. Thus, gender differences in exit probabilities for child-rearing may be an explanation for wage differentials between (seemingly) observationally equivalent men and women. Moreover, the policy change *per se* could interact with the considerations that are indicated by our results on firm responses, as it potentially changes the frictional environment through extending leave duration of all *future* female employees giving birth, making it even more costly to hire women relative to men. For example, in anticipation that all women will now take three additional months of leave relative to the pre-reform regime, forward-looking firms might prefer male hires, especially in occupations and industries in which male and female workers are highly substitutable. In the presence of such statistical discrimination, traditionally female-dominated sectors may still continue to rely on female labor, but in industries where men and women are more substitutable, we might see a slow shift in the firms' gender composition, in particular in thick labor markets.

Because we only observe cross-sectional labor market thickness, and we do not directly observe neither firm-specific human capital, the degree of substitutability of workers, nor discrimination at the workplace level, there are a few cases where we cannot unambiguously distinguish the mechanisms of certain firm actions. For example, if we would observe that a firm in a market with an abundance of female labor supply refrains 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 simply 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 hires but not female hires in a market with a large pool of both men and women candidates would constitute suggestive evidence for statistical discrimination. Indeed, in Figure 16 we show that workplaces in thick markets for both male and female labor appear to predominantly hire male workers in response to the reform.

In the next section, we quantify the equilibrium effect of the reform on net employment and on the gender- and age composition of employment, and the extent to which such changes are driven by changes in the demand for different types of labor. Thus, we focus on all firms' hiring and wage setting behaviors to provide insights into the potential interaction between the policy intervention and gender gaps in the labor market.

7 Extended Family Leave and Gender Gaps: Statistical Discrimination?

7.1 Composition of firms' new hires in the post-reform regime

Given the evidence of the existence of adjustment costs related to labor turnover that we document in the previous sections, we are interested in whether firms shift their employment composition away from the group for which costs are potentially higher in the post-reform state, in *anticipation* of such costs. In order to account for demand-side explanations for any potential shift in the composition of employed workers, we quantify the effects of the policy on the hiring, promotion patterns and wage offers to new hires at *all* firms in the economy, based on a measure of predicted reform exposure, which we detail below. In particular, we estimate whether the reform altered the proportion of newly hired childbearing-age women to all new hires in a given year within establishments. We also study whether newly hired women of childbearing ages receive lower starting wages relative to all other new hires at the same workplaces. In this analysis, we focus on private sector establishments.

For each workplace, we calculate the number of new hires in each calendar month and year, the starting wage of the new hires, and wage growth during their tenure at the firm.²³ To measure promotion events we follow Bronson and Skogman Thoursie (2019) and use relative wage growth measures. In particular, for each new hire i , we calculate the annual wage growth relative to their co-workers, and define a promotion event for new hire i as a large upward move within their workplace's wage hierarchy. This measure of promotion transforms the relative wage growth variable into an indicator variable that takes the value one whenever a worker realizes wage gains that are 15 log points higher than the average wage growth of their co-workers that year. In the results below, we consider promotion probability in the first year of tenure in the firm to avoid selection driven by workers with longer tenures.²⁴

To estimate the effect of the reform on all firms, we create a measure of predicted exposure for each employer that relies on the age-composition of workers and age-specific fertility rates. Specifically, denote $P^a(b)$ denote the (nationwide) fertility rate for women aged a (averaged over the pre-reform period).

²³As in section 5, based on the mean workplace size before 1989, we exclude the smallest (fewer than ten employees) and the very largest (top 1 percentile of size distribution) establishments from the analysis presented here. An individual is defined as newly hired in year t if they were not employed in the workplace in $t - 1$. Since we have data from 1985, we are able to identify new hires from 1986 onwards.

²⁴The results presented in this section are similar if a more conservative threshold of 20 log points is used to define probability of within workplace promotion (not shown).

For each workplace j , we calculate the expected fertility in each year t before 1989 as:

$$\phi_{j,t} = \sum_{a=21}^{38} P_t^a(b) \times N_{j,t}^{f,a}$$

where $N_{t,j}$ denotes the total number of workers (aged 19-65) in the workplace j in year t , $t < 1989$. For each establishment j , we define predicted exposure to the reform as the average of ϕ_{jt} over the years between 1986 and 1988 that the establishment is active, i.e.,

$$\pi_j^p = \sum_{t=1986, t=1988} \left(\frac{\phi_{j,t}}{N_{j,t}} \right) / N_{j,a}$$

π_j^p is a time-invariant measure of predicted reform exposure at the workplace level. To investigate the effects of the reform on establishment-level hiring, we estimate the following regression equation:

$$y_{j,t} = \delta_0 + \sum_{t=1986, t \neq 1988}^{1996} \delta_1^t (\pi_j^p \times D_t) + \mathbf{X}_{j,t}' \theta + \delta_2 \pi_j^p + \lambda_t + \lambda_j + v_{j,t} \quad (6)$$

where D_t are indicator variables for each calendar year $t = \{1985, \dots, \neq 1988, \dots, 1996\}$. In the vector $\mathbf{X}_{j,t}$ we include industry-fixed effects to control for industry-level permanent characteristics which affect hiring patterns independently of the reform. We also include controls for the number of distinct occupational titles in at the workplace. λ_t accounts for common shocks across workplaces, and λ_j absorbs workplace time-invariant characteristics affecting hiring profiles independently of the reform. In Equation (6), the coefficients of interest, the δ_1^t 's, measure the difference in the outcome variables across establishments with different predicted exposure to the reform, before and after its implementation.²⁵

Figure 17 plots the estimates of the δ_1^t 's for all privately owned establishments. Panel A shows the estimates for the effect of the reform on the share of newly hired women workers in childbearing ages (younger than 40) of all new hires, Panel B the relative starting wage of new female young hires, and in Panel C the relative promotions of the newly hired young women workers. The estimates in Panel A of Figure 17 show that after the reform, firms with a higher predicted exposure had a temporary drop in the proportion of young women among their new hires (the reduction occurred during 1989 and 1990). A one standard deviation (0.02) higher predicted reform exposure is associated with a drop in the share of young women hired in 1989 and 1990 in the order of 1.2–1.6 percent. Reform exposure is, however, not shown to be associated with changes in relative wage offers to young women hires (Panel

²⁵Due to re-classifications over time, the industry fixed effects are not absorbed by the workplace effects.

B). However, as the estimates in Panel C shows, newly hired women workers of childbearing ages had a lower probability of receiving a promotion relative to all other new hires in the same month at the same establishment, in firms with a higher predicted exposure, after the reform. Because the promotion outcome is measured in the first year after hire, and the effects show up between 1990 and 1992, these impacts are driven by those hired immediately after the reform.

In Figure A.3 we present estimates on the relative promotion probabilities for newly hired workers of the comparison groups, i.e., men and older women. Panel A shows that in the longer run, there is a decline in the promotions also for male young workers, which is likely what drives the impacts on the young women hires back to zero after the initial drop (see Figure 17). One potential confounder for the long-run impacts is the financial crisis that hit Sweden and that lead to employment declines from 1992 onwards. The employment decline hit the tradable industries first and foremost, which are male dominated, suggesting that the impacts on male workers is not driven by the crisis. Nevertheless, it may still be that predicted exposure is correlated with other, not observed, determinants of firms' hiring patterns in the long run. Thus, we interpret the medium- and long-run effects with caution.²⁶ In the short-run, however, we interpret our findings as suggestive evidence that firms take into account that the reform potentially increases the costs of hiring women, and refrain from doing so.

In the next and final section of the paper, we explore the overall economy-wide effects of the reform on net employment and employment composition.

7.2 Aggregate employment effects

Finally, we are interested in the equilibrium effects of the policy, on net employment and on the demographic composition of those employed. To this end, we quantify the aggregate effects of the parental leave reform on employment by predicted reform exposure across local labor markets and industries, based on age-specific fertility rates and the composition of workers at baseline, similar to the research design used in the previous section. We rely on 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 local labor market (commuting zone) c , we calculate the expected fertility in 1989 as

²⁶In results not presented but available upon request from the authors, we estimate alternative version of model 6, where we allow for industry specific trends, for linear trends on pre-1989 workplace size, education (level) composition at the establishment and age composition of the workforce. The results remain unchanged.

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

where $N_{c,k}^{f,a}$ denotes the number of women aged $a = \{21, \dots, 38\}$ employed in industry k in region c in 1989. Finally, 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 1989. 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 employment, we estimate the following regression equation:

$$y_{c,k,t} = \gamma_0 + \sum_{t=1985, t \neq 1988}^{1996} \gamma_1^t (\pi_{c,k}^p \times D_t) + \gamma_2 \log(pop)'_{k,t} + \gamma_3 \pi_{c,k}^p + \lambda_t + \lambda_c + \nu_{c,k,t} \quad (7)$$

where D_t are indicator variables for each calendar year $t = \{1985, \dots, \neq 1988, \dots, 1996\}$. 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 γ_1^t s, which measure the difference in the evolution of employment across industries with different predicted exposure to the reform, before and after its implementation. (In Figure A.4 in Appendix we show that there is considerable variation in reform exposure both across industries and within industries across local labor markets.)²⁷

Figure 18 shows the results from estimating equation (7) on the ratio of employment of women of childbearing age (younger than 40 years) to all employment. The estimates show a persistent and relatively constant decline in the ratio of female young workers to overall employment. In terms of effect size, this ratio declined by roughly 0.3 percent in an industry (within a local labor market) with an average reform exposure. Next, we decompose these results in employment of different subgroups.

Figure 19 depicts the effects of predicted reform exposure on the employment of women in childbearing ages and on women beyond childbearing age (40 and older) in panels A and B, respectively. The results show an immediate reduction in the employment of women in childbearing ages which gradu-

²⁷We exclude Stockholm, which serves as its own local labor market, from the analyses.

ally dissipated by 1993. For women aged 40 and older, there was no decline in employment, with the exception of a small decline in 1989. Overall, the results indicate that the composition of female employment gradually shifted in favor of women whose childbearing years were behind them. Also male employment increased in both the short- and long-run (panels B and C). Taken together, these findings suggest that employers may have altered their workforce composition as a response to the reform in the long-run, away from the group for which the reform implied higher expected costs.²⁸

One potential confounding factor is the Swedish financial crisis which erupted in the fall of 1992 and lead to large increases in unemployment. If the industries in which the concentration of women of childbearing ages were those in which aggregate demand declined the most, this could potentially explain the differential patterns of employment across demographic groups by reform exposure that we observe in Figure 18–Figure 19. However, the financial crisis affected male and female employment in the opposite direction (see Figure A.6). While employment decreased dramatically between 1992 and 1995 for both men and women, the employment reduction was larger among male workers, and the overall female-to-male employment ratio thus *increased* during the time period. This is because the financial crisis first and foremost affected the tradable industries, which were male dominated. The increased female-to-male employment ratio is visible both for the full population and for the population of childbearing age. Nevertheless, one might still worry that a high concentration of women of childbearing age is correlated with unobserved, omitted, factors that impact hiring and employment patterns. However, the non-existing differences in the pre-reform trends of the outcome variables depicted in Figure 18–Figure 19 suggest that omitted variables are not a source of concern.

We note that we are not able to interpret the results shown here as strictly coming from the demand side since the reform likely affects the supply of women of childbearing ages in the labor market given full take-up (which is correlated with predicted exposure). Nevertheless, we conclude that the extension of paid leave entitlements potentially had unintended consequences on employment and, at least in the short run, on women’s relative promotion probabilities.

8 Conclusions

An extensive literature documents that women have higher separation rates from their employers than men, especially around the onset of parenthood. Such interruptions in employment spells may be costly

²⁸The pattern of impacts similar, but more imprecisely estimated, if Stockholm region is included in the sample (see Figure A.5).

for firms because replacement workers could be difficult to find, or because temporary workers may not be as productive if human capital is specific to the firm. Most industrialized countries have made substantial efforts to increase gender equality in labor market outcomes by directly targeting women's labor market attachment around the onset of parenthood through various family policies. On the one hand, introducing leave programs encourages women's labor force participation after childbirth. On the other hand, generous leave allowances may have unintended consequences when we consider equilibrium responses of firms in the presence of frictions in the labor market (see Olivetti and Petrongolo, 2017).

In this paper we study the effect of parental leave mandates on firms' outcomes and the 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 from 12 to 15 months. Our population-wide matched employer-employee data allows us to analyze the impacts of the reform on both workers and firms. We show that the reform had a full effect on take-up of parental leave during the first two years after the child's birth. The reform thus reduced mothers' months worked in the short-run, but had no impacts on long-run earnings or participation. We also show that effects on male take-up was substantially smaller in absolute terms, and 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, suggesting that employees partly internalize the adjustment costs of their employers when they decide on the duration of parental leave.

We find that private sector firms with greater exposure to the reform, as measured by the fraction of their workforce entitled to extended leave, adjust by hiring temporary 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. The public sector employers, in contrast, do not seem to be able to adjust to the extended absence, neither through their main workers' hours nor through temporary workers. This could potentially be due to that the public sector workplaces are institutions such as schools and hospitals, which are financed through fixed budgets. The inability to make labor adjustments may have important implications for the outcomes of these institutions, if labor shortages affect the quality of output, as other work has shown.

Moreover, we find that firms in thick external markets respond 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, resort to internal hours increases. Finally, we also find heterogeneous responses by the degree of occupational specialization within the workplace: establishments with a high concentration of its workforce in their dominant occupation group respond to the labor supply shortage by increasing incumbents' hours more compared to firms with a more diverse occupational structure. These findings thus suggest several sources of potential frictions associated with finding and hiring replacement workers. Taken together, our analyses show that firms are indeed affected by workers taking longer leave, and that women taking additional time off for child-rearing could potentially imply that firms would have to incur costs in replacing them. Our results show that adjustment costs are over and above the costs of salary payments for workers on leave, suggesting that reorganization is costly.

Finally, we analyze the aggregate effect of the reform on employment, exploiting variation in *predicted* exposure, based on age-specific fertility rates and the demographic composition of the worker pool, across industries within local labor markets. The intuition is that predicted exposure is correlated with actual take-up, but also captures if employers react on expectations of higher costs associated with hiring women of childbearing age after the reform. We find that in industries with higher predicted exposure, the ratio of women in childbearing age ("young" women) to all employed declined after the intervention. We note that we are not able to rule out that these effects arise because the reform reduced the supply of young women due to them being on extended PL. However, when zooming in on the hiring decisions of individual firms in the sample of all firms in the economy, we find that the fraction of new hires that are women of childbearing age (relative to all new hires) declined relatively more in firms with a higher *predicted* reform exposure immediately after the new policy was enacted. Moreover, in more exposed firms, the promotion probabilities of new female young hires relative to all other new hires in the same month and firm declined among those hired immediately after the reform.

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. Indeed, Brenøe et al. (2020) find no effects of parental leave *per se* on employers, which is likely predictable at least a few months in advance and could maybe be planned for. Thus, we cannot rule out that in the long-run, firms learned to deal with the longer leaves in a way that minimized disruption. However, Gallen (2019) estimates heterogeneous responses of the PL extension in Denmark by the extent to which firms were "surprised" by exploiting variation in the timing of births and finds similar effects on firms' shut-

down probabilities irrespective of how long they had to plan for the increased leave duration of their employees. 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 reduce the adjustment costs for firms when their workers go on leave. Thus, we conclude that an important avenue for future research is to look at how policies that aim to equalize the burden of care for young children, i.e., daddy-months in PL, interact with employers' considerations.

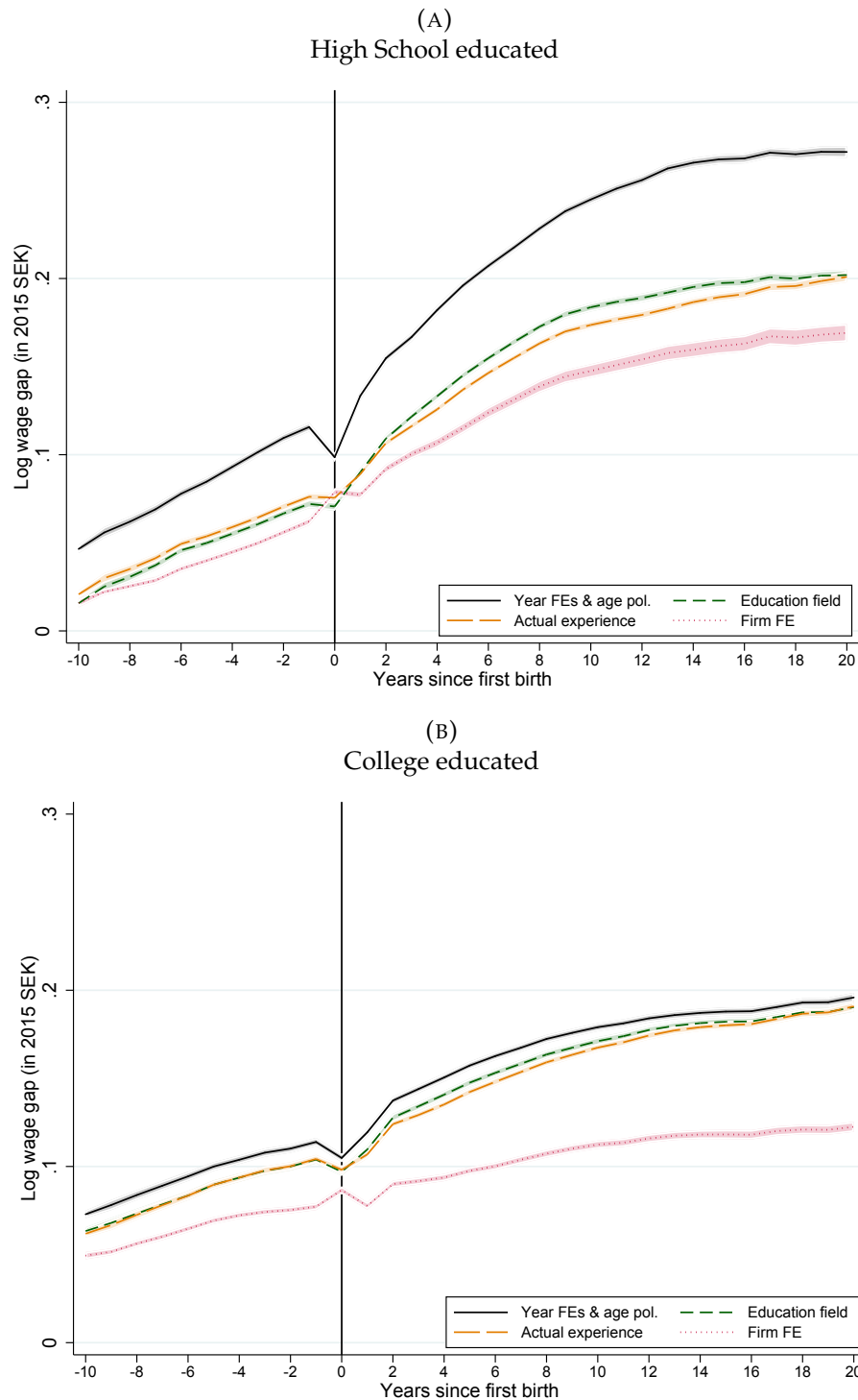
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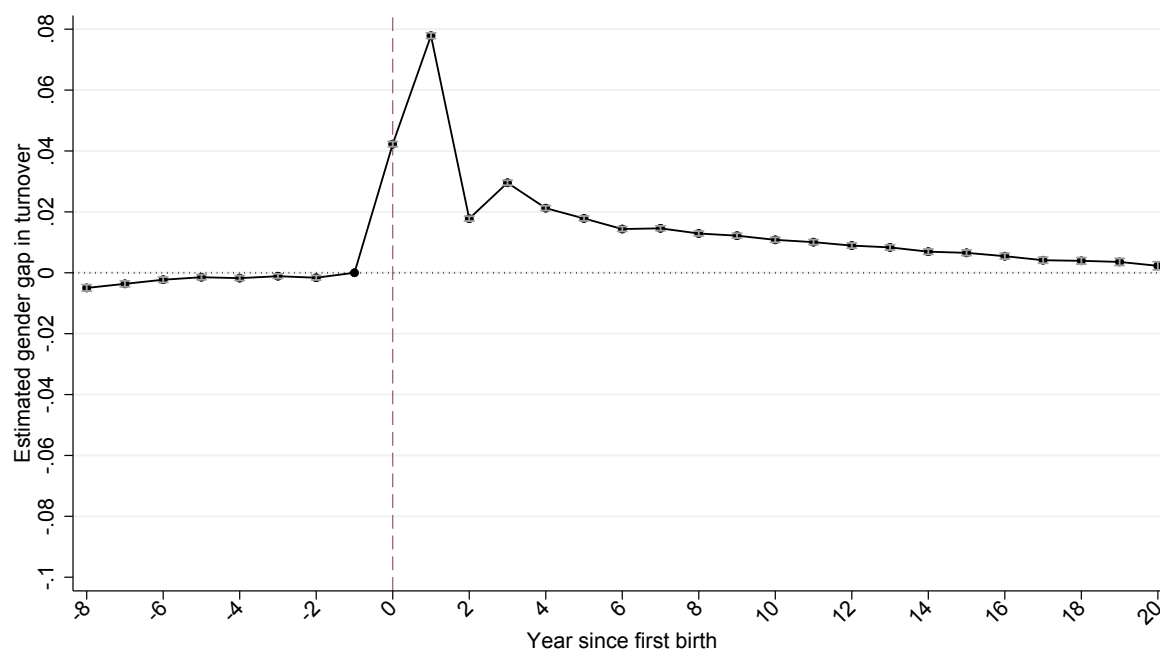
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FIGURE 1.
Residual gender wage gap by time since first birth



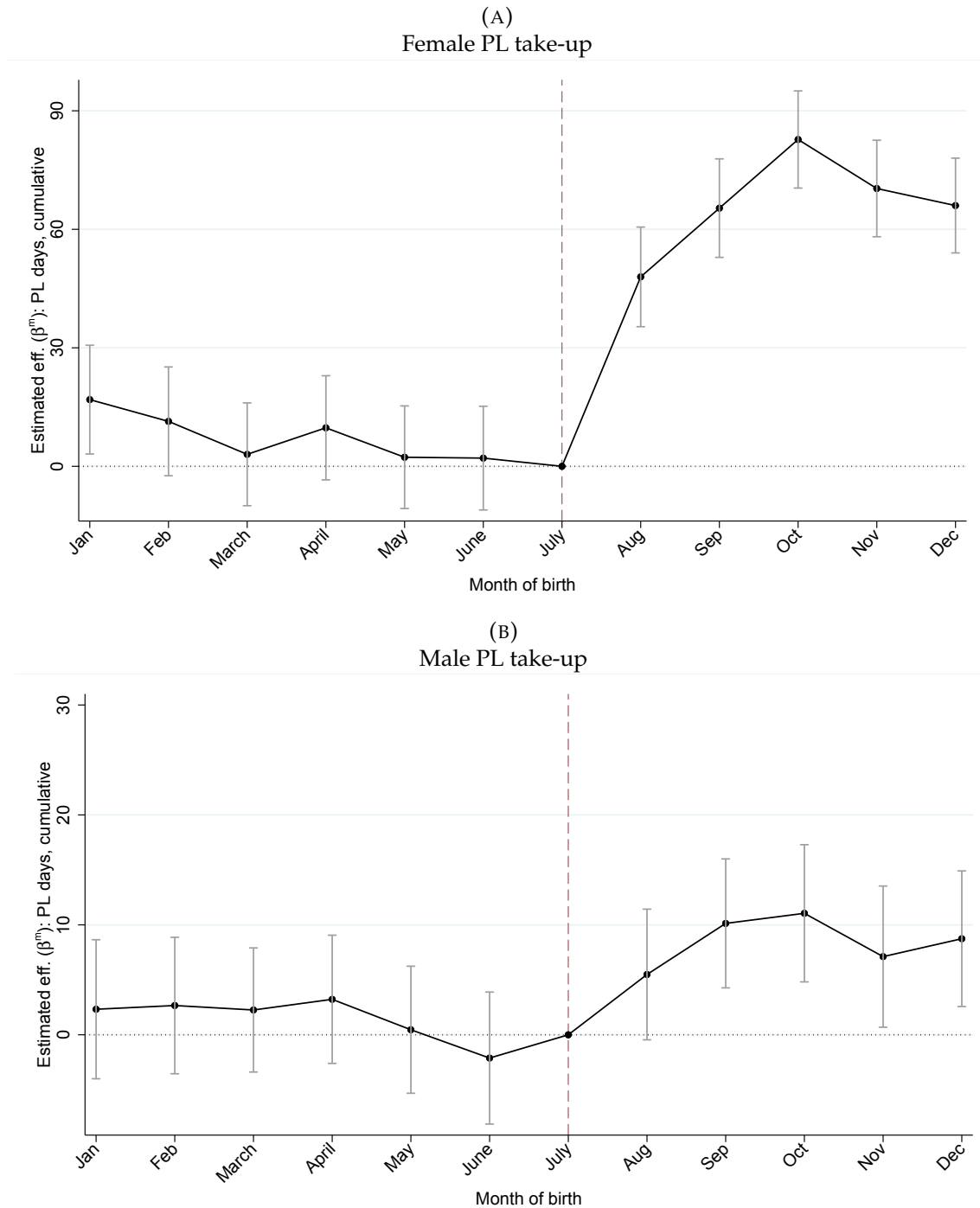
NOTE: The lines in the figure plot the residual gender gap (log) wage by time since first birth adding successively more detailed controls for the individual's characteristics.

FIGURE 2.
Gender gap in separation rates (job-to-nonemployment transitions) by years since first birth



NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between an indicator for "female" and time since first birth. Additional controls included in the regression model include year fixed effects, quadratic term on age and indicators for completed level of education (compulsory education, high school degree or at least some college).

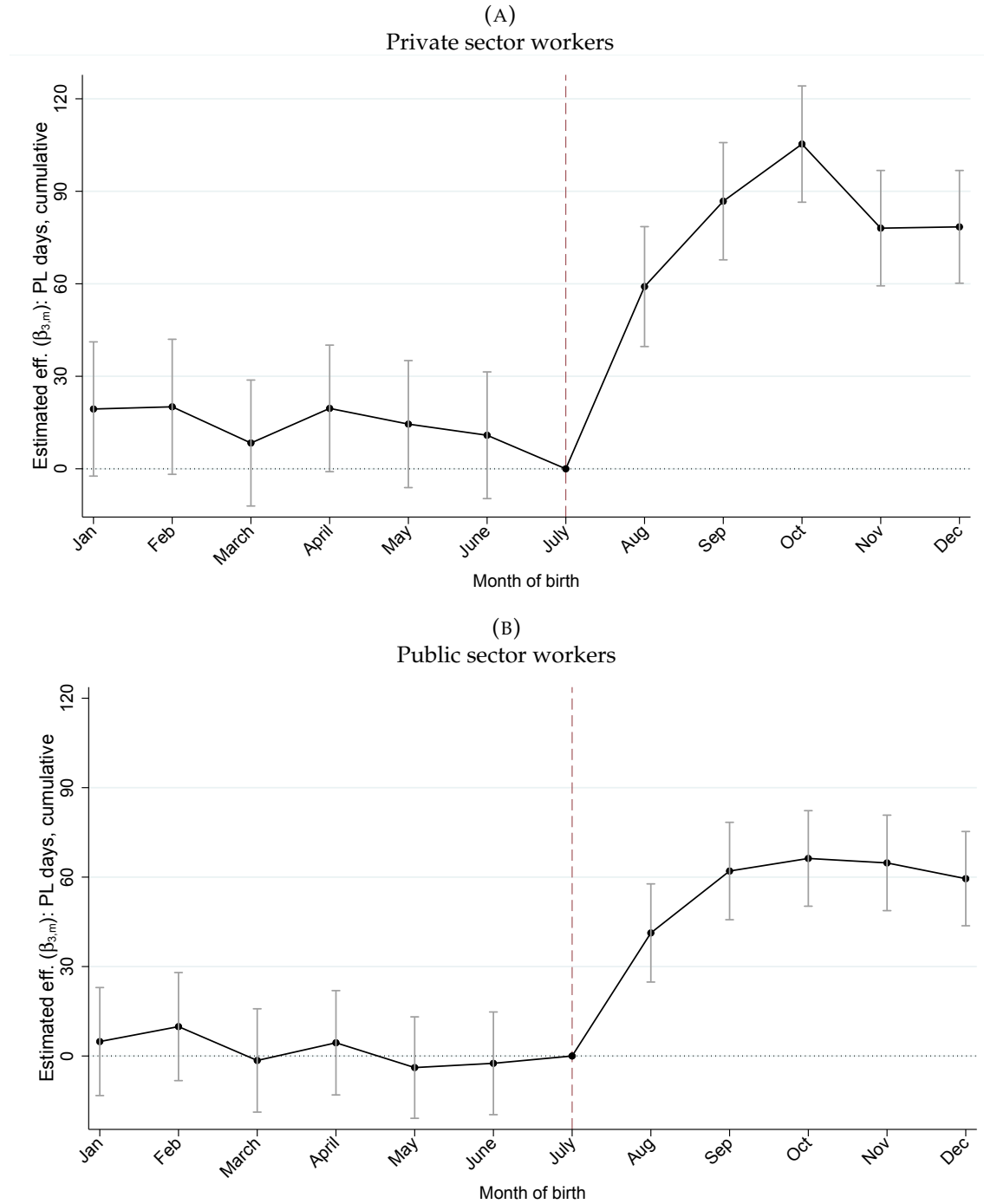
FIGURE 3.
Effects of extended entitlements to paid leave on the take-up of parental leave



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 $m = Jan, \dots, Dec$ in 1988 and corresponding months in 1989, along with the 95% confidence intervals. No control variables are included in the estimation.

FIGURE 4.

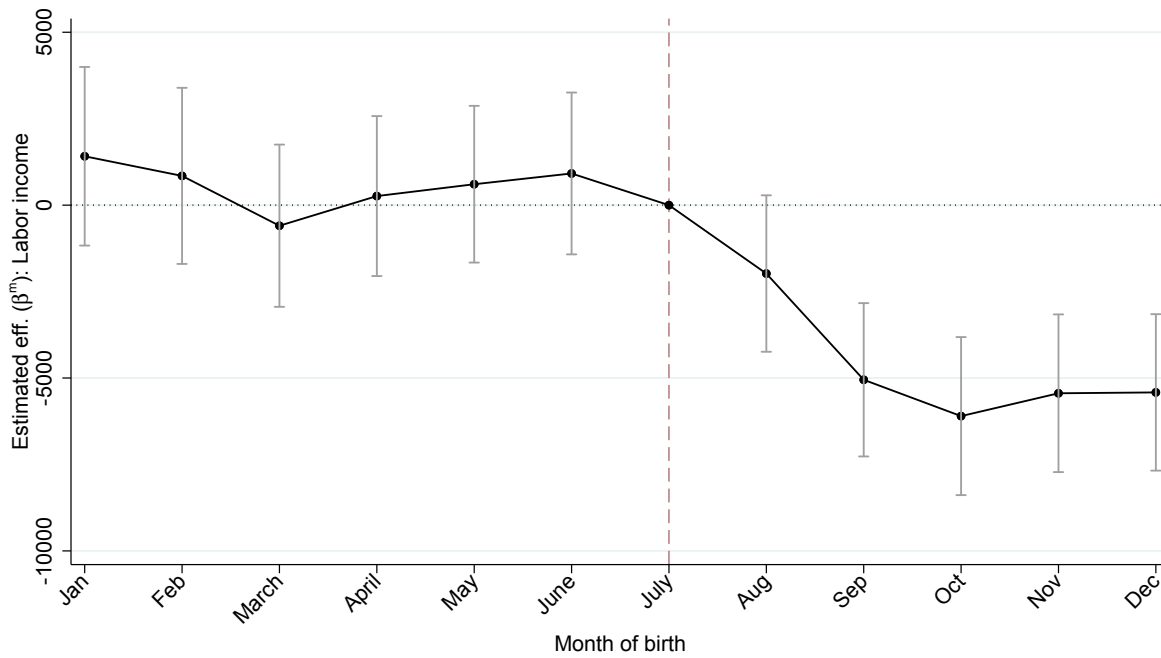
Effects of extended entitlements to paid leave on the take-up of parental leave by sector of employment



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, $\hat{\beta}_{3,m}$, from Equation 1, i.e., the difference in outcomes between women who gave birth in calendar month $m = Jan, \dots, Dec$ in 1988 and corresponding months in 1989, along with the 95% confidence intervals. No control variables are included in the estimation.

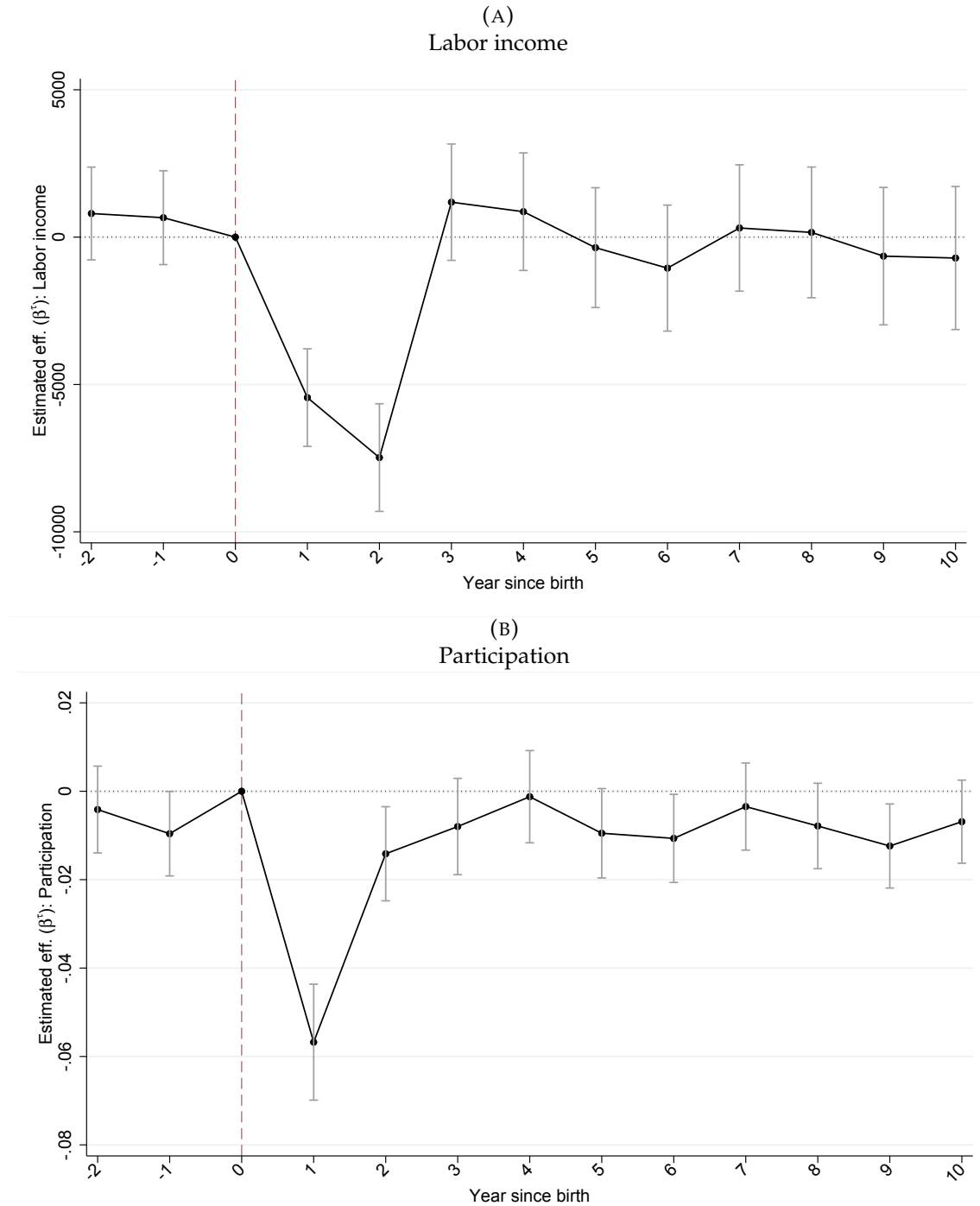
FIGURE 5.

Effects of extended entitlements to paid leave on mothers' labor earnings and months worked: one year after birth



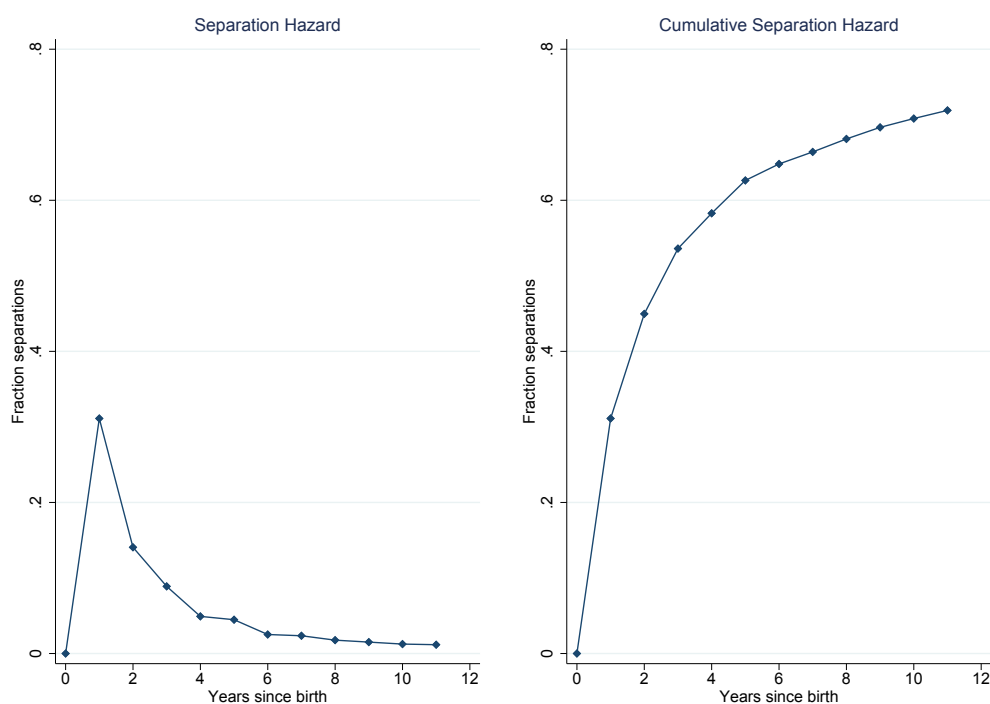
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of having a child born in 1988 and calendar month of birth, $\hat{\beta}^m$, from Equation 1, i.e., the difference in outcomes between women who gave birth in calendar month $m = Jan, \dots,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

FIGURE 6.
Effects of extended entitlements to paid leave on female labor earnings and months worked by time since birth



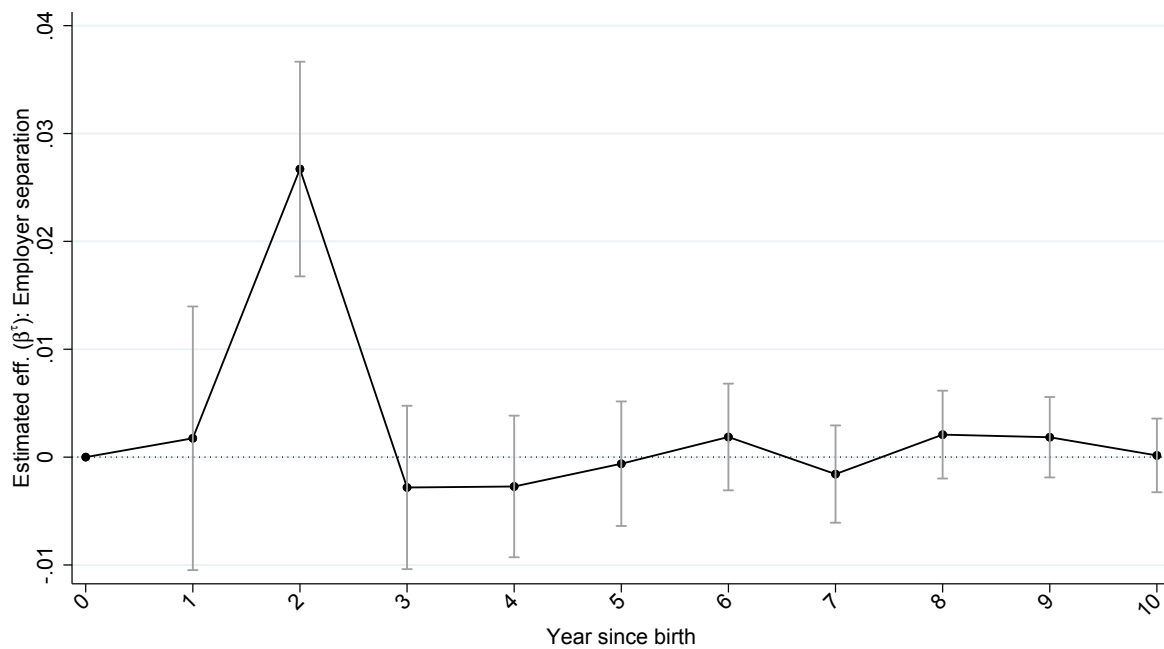
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of having a child born in 1988 D_i and the treatment indicator T_i , $\hat{\beta}^r$, from Equation 2, i.e., the difference in outcomes between women who gave birth in August–December in 1988 and corresponding months in 1987, interacted with the time-to event indicators, along with the 95% confidence intervals

FIGURE 7.
Baseline separation rate from pre-birth employer by time since birth



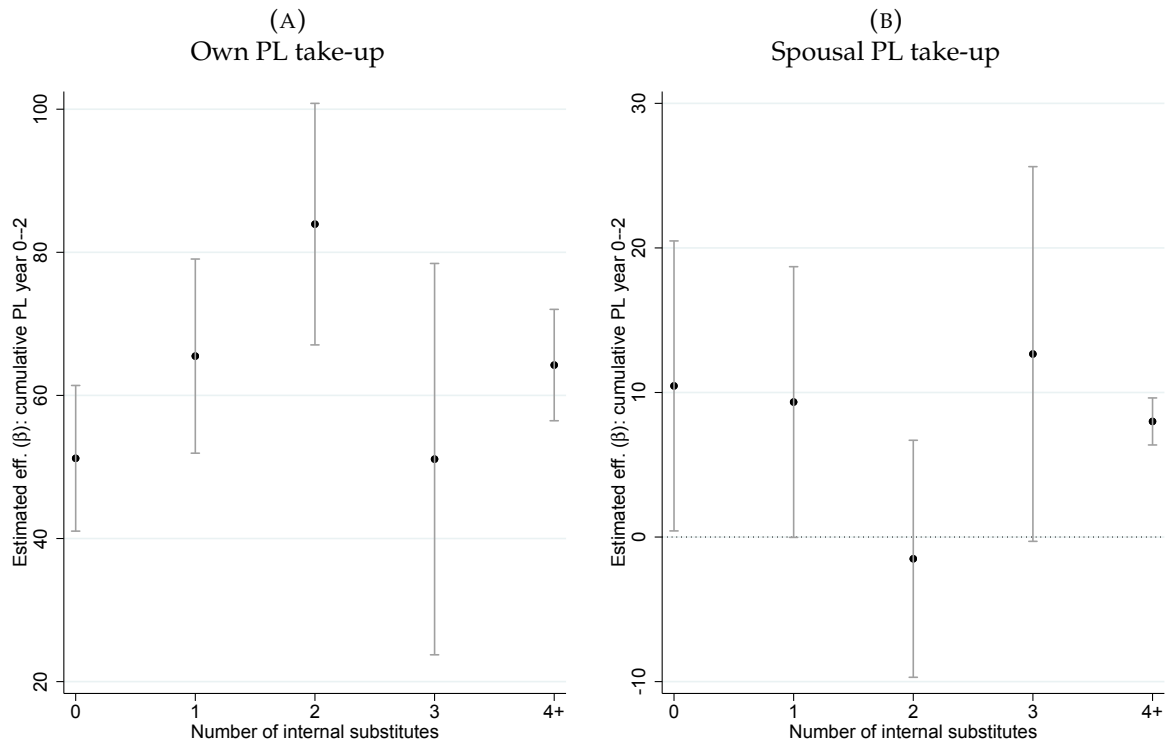
NOTE: The figure shows the separation hazards from the pre-birth employer by time since birth. The hazards are calculated on the sample of women who give birth in January–July and October–December of 1987. The left-hand panel shows the annual hazard rate, and the right-hand panel the cumulative hazard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

FIGURE 8.
Effects of extended entitlements to paid leave on separation from pre-birth employer



NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the indicator of having a child born in 1988 D_i and the treatment indicator T_i , $\hat{\beta}^\tau$, from Equation 2, i.e., the difference in outcomes between women who gave birth in October–December in 1988 and corresponding months in 1987, along with the 95% confidence intervals.

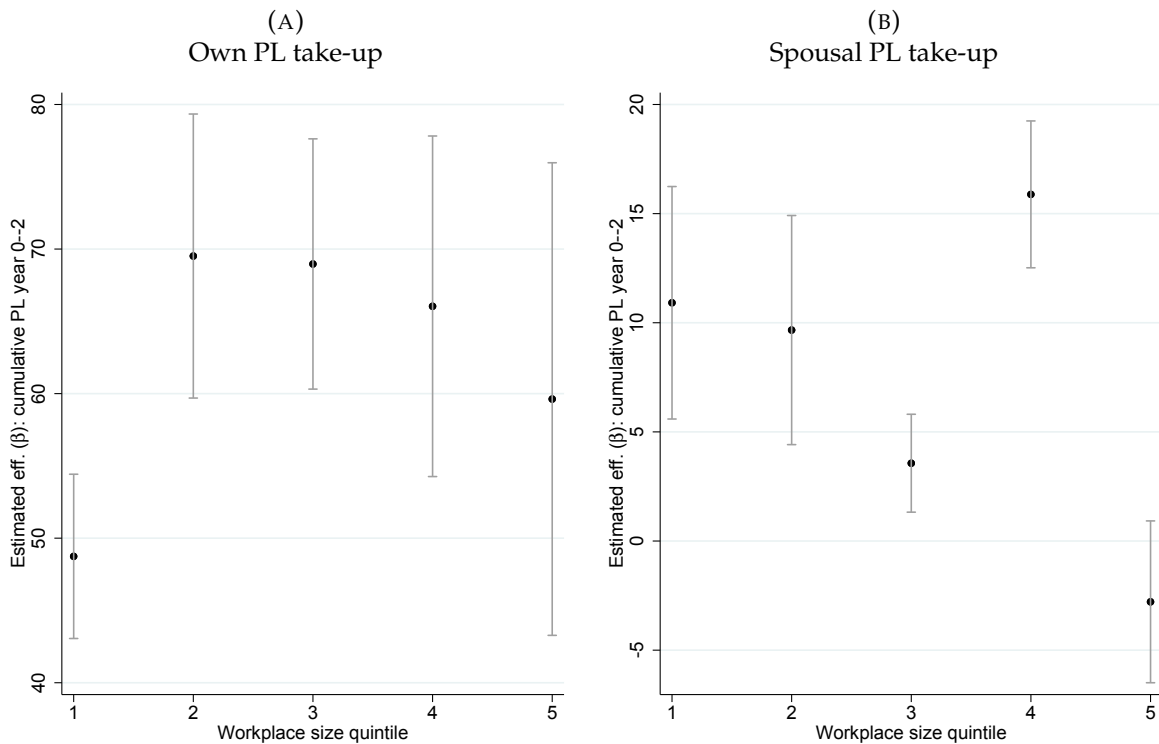
FIGURE 9.
Heterogeneous effects of extended entitlements to paid leave on program take-up by the number of internal substitutes at the workplace



NOTE: The sample includes women and men who had a first child born in 1988 and 1989. The point estimates in the figures are the effect of being eligible for three additional months of paid leave on the total number of (gross) days taken-up over child ages 0-2 on own and spousal parental leave take-up for individuals with different number of co-workers with the same occupation category as the focal worker. The graphs plot $\hat{\beta}$ from equation (2).

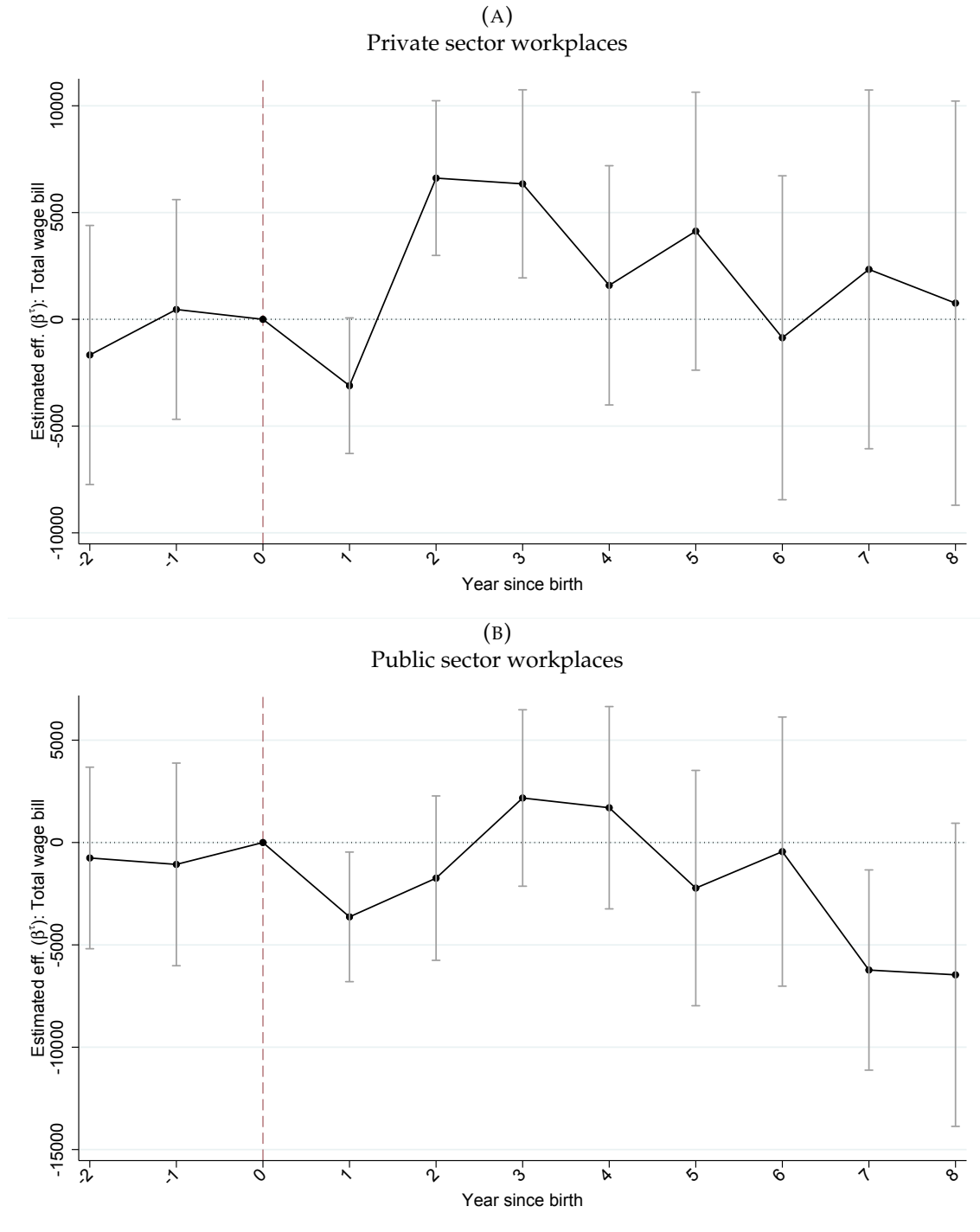
FIGURE 10.

Heterogeneous effects of extended entitlements to paid leave on program take-up by the workplace size



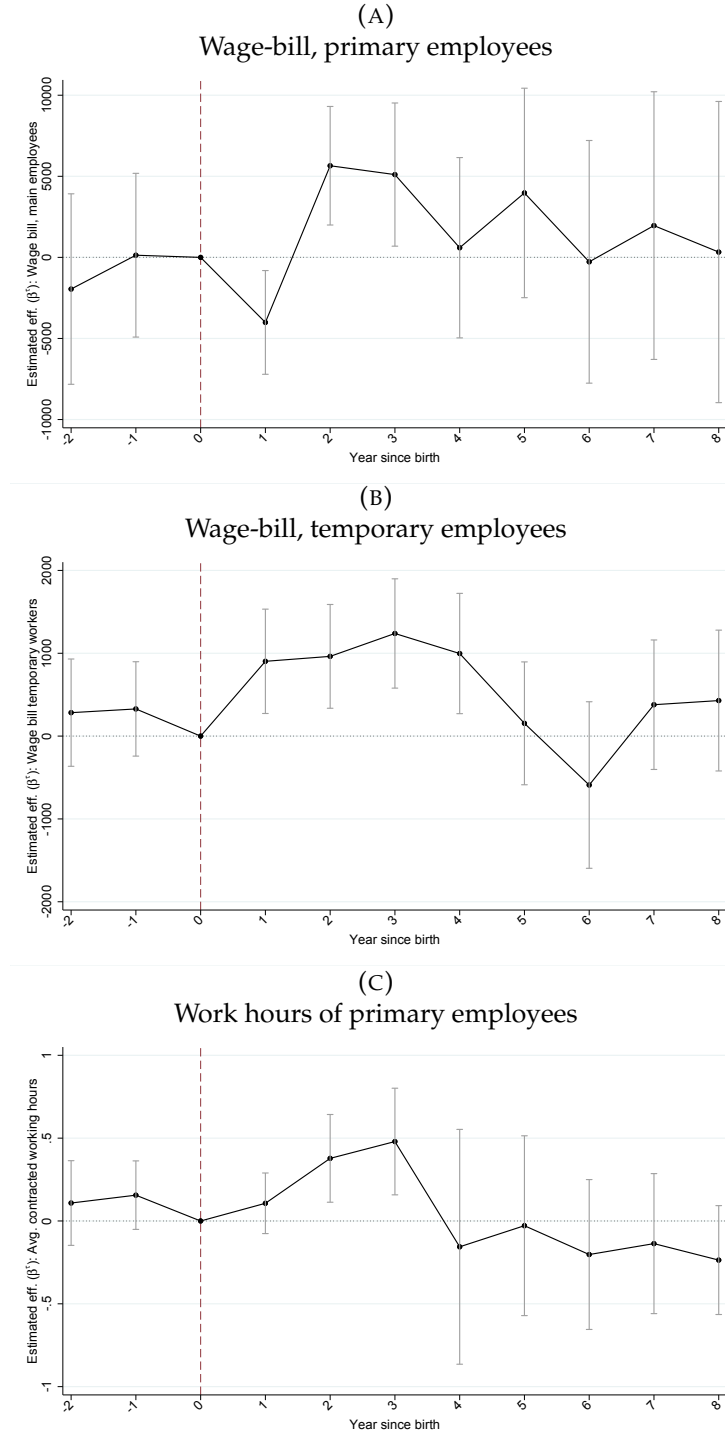
NOTE: The sample includes women and men who had a first child born in 1988 and 1989. The point estimates in the figures are the effect of being eligible for three additional months of paid leave on the total number of (gross) days taken-up over child ages 0-2 on own and spousal parental leave take-up by workplace size of the focal worker. The graphs plot $\hat{\beta}$ from equation (2).

FIGURE 11.
The effect of the extended parental leave program on Firm's Total Wage Costs



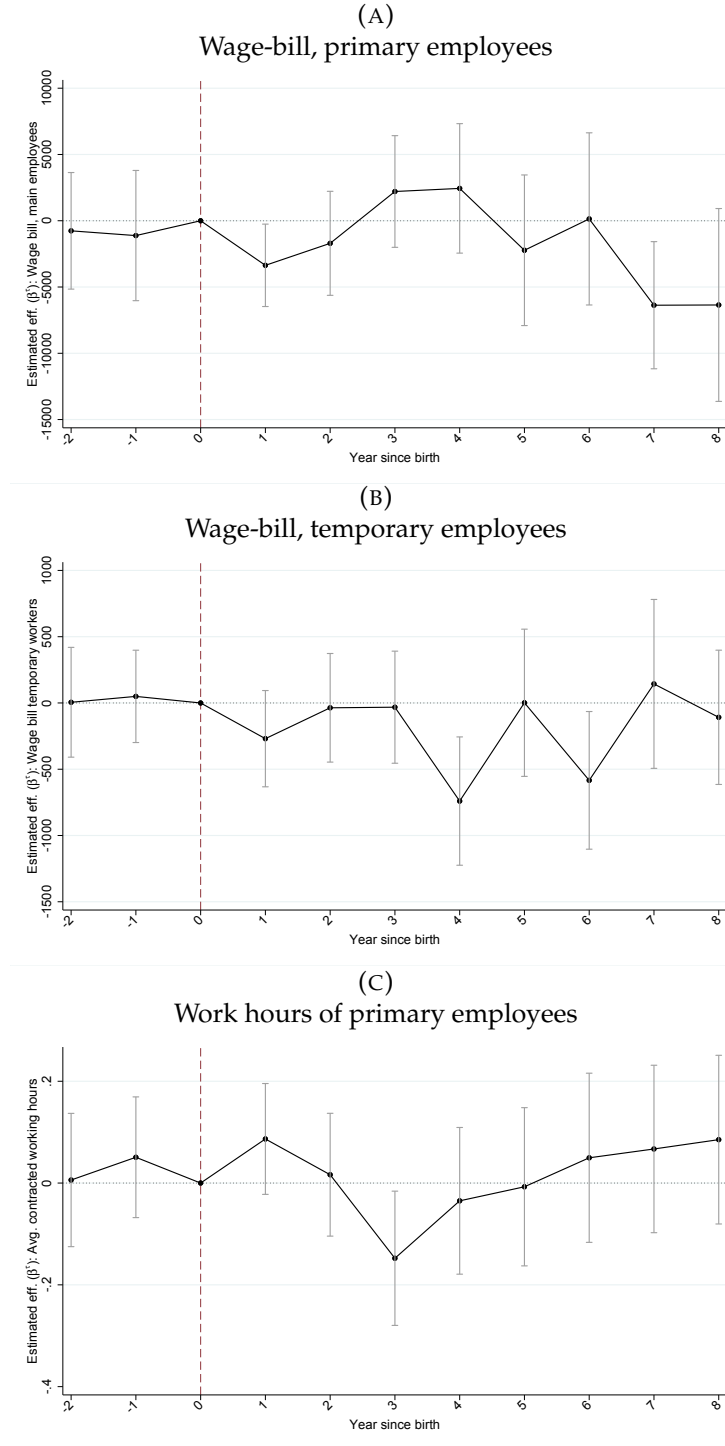
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j, \hat{\beta}^\tau$ from Equation 4, i.e., the difference in outcomes of firms with a higher fraction of its workforce giving birth in October-December 1988 and corresponding months in 1987, along with the 95% confidence intervals.

FIGURE 12.
Decomposing employer responses: primary workers' hours increases or temporary replacement workers? Private sector



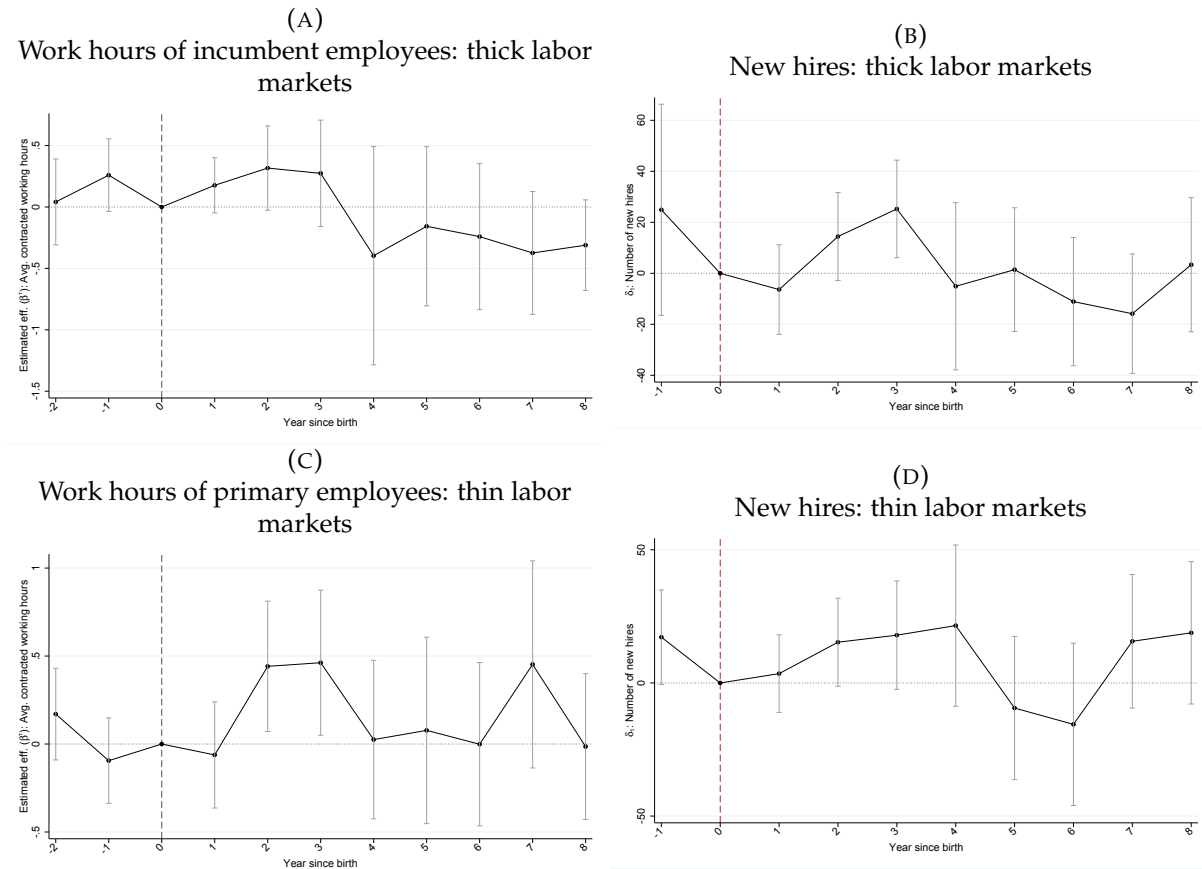
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j, \hat{\beta}^\tau$, from Equation 4, i.e., the difference in outcomes of firms with a higher fraction of its workforce giving birth in October-December 1988 and corresponding months in 1987, along with the 95% confidence intervals.

FIGURE 13.
Decomposing employer responses: primary workers' hours increases or temporary replacement workers? Public sector



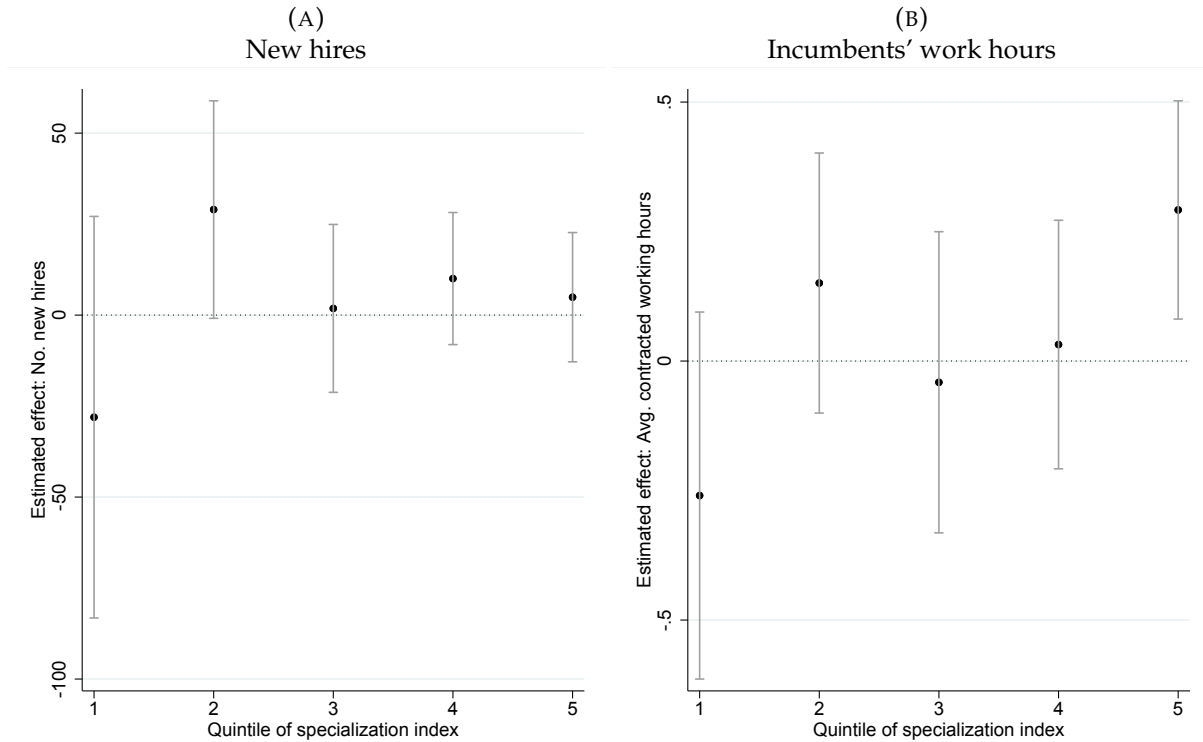
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j, \hat{\beta}^\tau$, from Equation 4, i.e., the difference in outcomes of firms with a higher fraction of its workforce giving birth in October-December 1988 and corresponding months in 1987, along with the 95% confidence intervals.

FIGURE 14.
Employer Responses to Extended Parental Leave Durations by external labor market conditions
(Private sector firms)



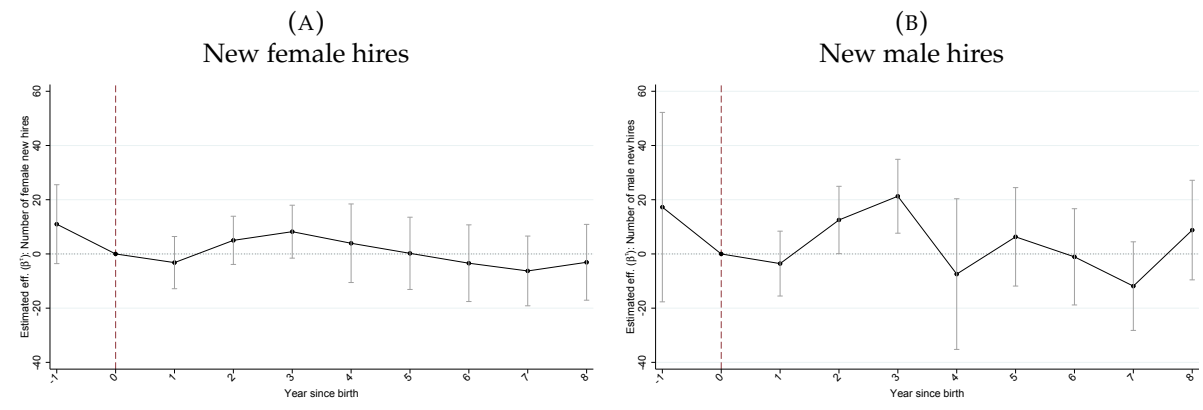
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j, \hat{\beta}^\tau$, from Equation 4, i.e., the difference in outcomes of firms with a higher fraction of its workforce giving birth in October-December 1988 and corresponding months in 1987, along with the 95% confidence intervals.

FIGURE 15.
New hires and incumbents' hours by overall workplace occupational specialization



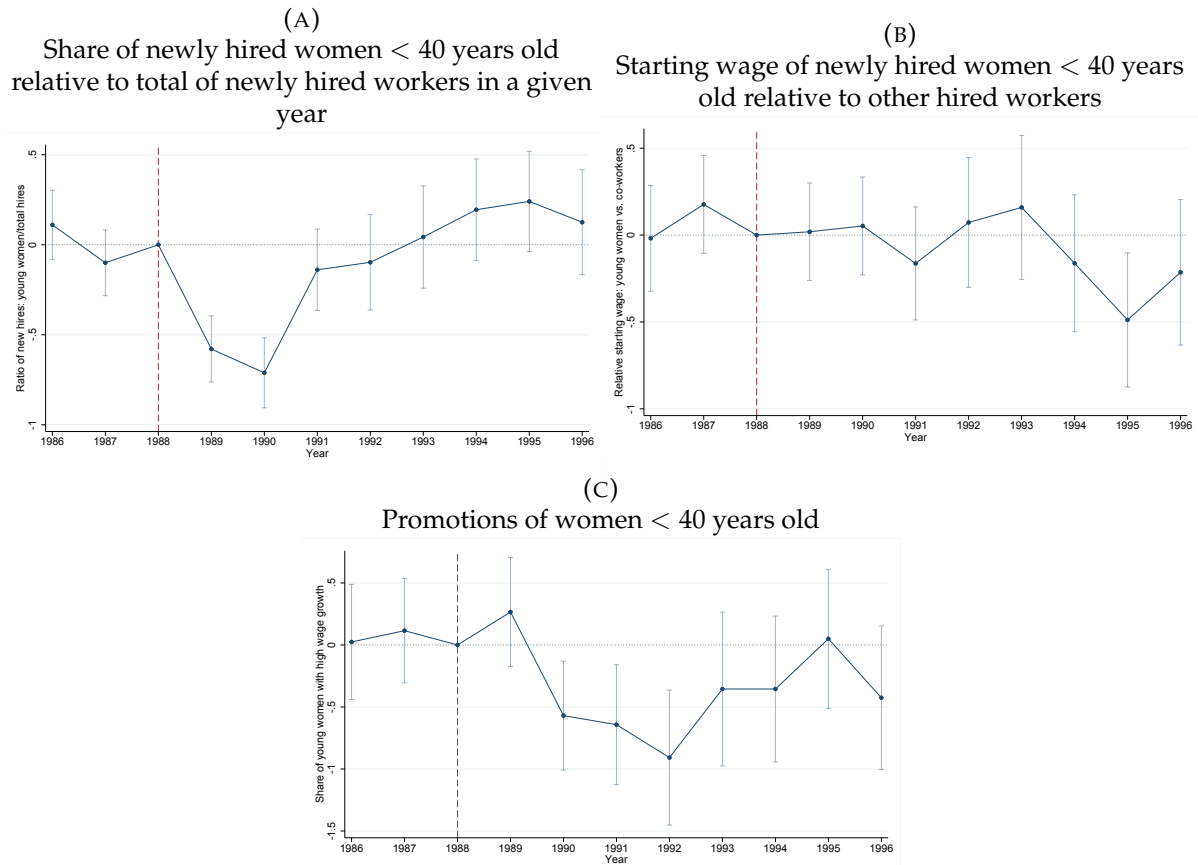
NOTE: Each point in the graphs shows the estimated coefficient on the interaction term between the indicator of employing women giving birth in 1988 and the treatment intensity $\pi_j, \hat{\beta}$, from a static version of Equation 4, i.e., the difference in outcomes of firms with a higher fraction of its workforce giving birth in October-December 1988 and corresponding months in 1987, along with the 95% confidence intervals.

FIGURE 16.
Gender composition of new hires: firms in thick local 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 $\pi_j, \hat{\beta}^\tau$, from Equation 4, i.e., the difference in outcomes of firms with a higher fraction of its workforce giving birth in October-December 1988 and corresponding months in 1987, along with the 95% confidence intervals.

FIGURE 17.
Effects on hiring, starting wage and promotion of young women within workplaces (private firms)



NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in establishment j , π_j^p , before and after its implementation, that is, the estimates for δ_1^t 's in Equation 6, along with the 95% confidence intervals.

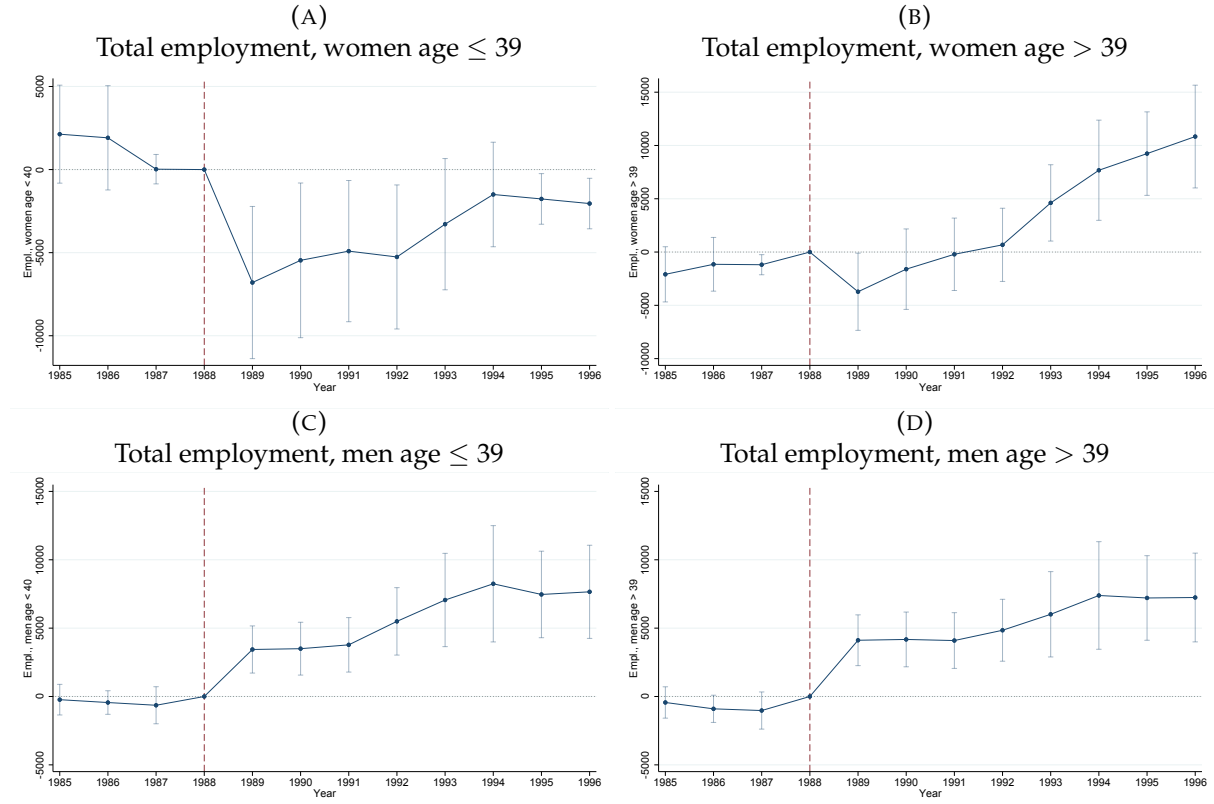
FIGURE 18.

Aggregate effects on employment ratio of young women to all workers (both private and public sectors)



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 , $\pi_{c,k}^p$, before and after its implementation, that is, the estimates for γ_1^t 's in Equation 7, along with the 95% confidence intervals.

FIGURE 19.
Aggregate effects on employment decomposed



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 , $\pi_{c,k}^p$, before and after its implementation, that is, the estimates for γ_1^t in Equation 7, along with the 95% confidence intervals.

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

	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*** (3.376)	74.391*** (5.272)	62.114*** (4.461)	82.215*** (5.786)	81.402*** (9.269)	80.545*** (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*** (1.601)	6.762*** (1.788)	13.110*** (3.427)	9.377*** (2.578)	7.745*** (2.904)	18.600*** (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 model estimated is the following

$$y_{ik} = \delta_0 + \beta T_i \times D_i + \delta_1 T_i + \delta_2 D_i + \mathbf{X}_i' \gamma + \epsilon_{ik}$$

where T_i is an indicator that takes the value 1 if person i 's child was born in October–December, and zero if the child was born in January–July, D_i takes the value 1 for those giving birth in 1988, and 0 for those whose child was born in 1989.

TABLE 2.
Summary statistics: Workers' pre-determined characteristics (by treatment status)

	Control cohort (1987)			Treatment cohort (1988)			DD	
	(1) Jan-July	(2) Oct-Dec	(3) <i>t</i> -stat for (1)-(2)	(4) Jan-July	(5) Oct-Dec	(6) <i>t</i> -stat for (4)-(5)	(7) DD est. of [(1)-(2)] - [(4)-(5)]	(8) <i>t</i> -stat for [(1)-(2)] - [(4)-(5)]
Age	28.694	28.148	-13.196	28.602	28.110	-12.318	0.054	0.935
No college	0.737	0.749	3.036	0.746	0.751	1.554	-0.006	-1.118
College	0.263	0.251	-3.036	0.254	0.249	-1.554	0.006	1.118
Labor income	117.991	114.897	-5.935	119.506	116.935	-4.944	0.524	0.710
Monthly wage rate	15.743	15.738	-0.120	16.386	16.343	-0.979	-0.038	-0.624
Contracted work hours	0.846	0.838	-2.853	0.846	0.850	1.321	0.013	2.982
Private sector	0.336	0.352	4.008	0.356	0.374	4.533	0.002	0.303
Child parity	1.821	1.804	-2.207	1.823	1.807	-2.124	0.001	0.102
Child spacing	28.595	28.075	-1.669	27.655	27.165	-1.672	0.031	0.072
Observations	56,423	19,918		60,147	21,322		157,810	

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

TABLE 3.
Summary statistics for all firms & organizations active in Sweden, and for firms in study sample

	All workplaces (mean)	All (sd)	Sample workplaces (mean)	Sample workplaces (sd)
Private sector	0.608	0.488	0.500	0.500
Tradable industry	0.170	0.376	0.129	0.335
Share female	0.487	0.319	0.621	0.280
Number of births	0.866	2.045	1.568	1.159
Share compulsory schooling	0.359	0.217	0.326	0.215
Share with high school	0.458	0.187	0.442	0.187
Share workers with college	0.091	0.131	0.116	0.152
Workplace size	44.203	64.187	49.075	54.823
Average age	37.450	5.997	37.355	5.797
Average contracted working hours	0.900	0.118	0.883	0.120
Female contracted work hours	0.851	0.144	0.847	0.134
Male contracted work hours	0.966	0.086	0.959	0.099
Average wage (SEK)	19,000.875	3,924.067	18,443.375	3,493.466
Female wage (SEK)	17,158.589	2,475.941	17,050.747	2,208.958
Male wage (SEK)	20,961.202	5,281.801	20,430.478	5,081.157
Female income (SEK)	135,979.619	52,521.727	136,731.975	46,715.279
Male income (SEK)	193,905.470	87,398.068	195,513.218	92,024.641
Observations	64,683		28,892	

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.

A Additional Tables and Figures

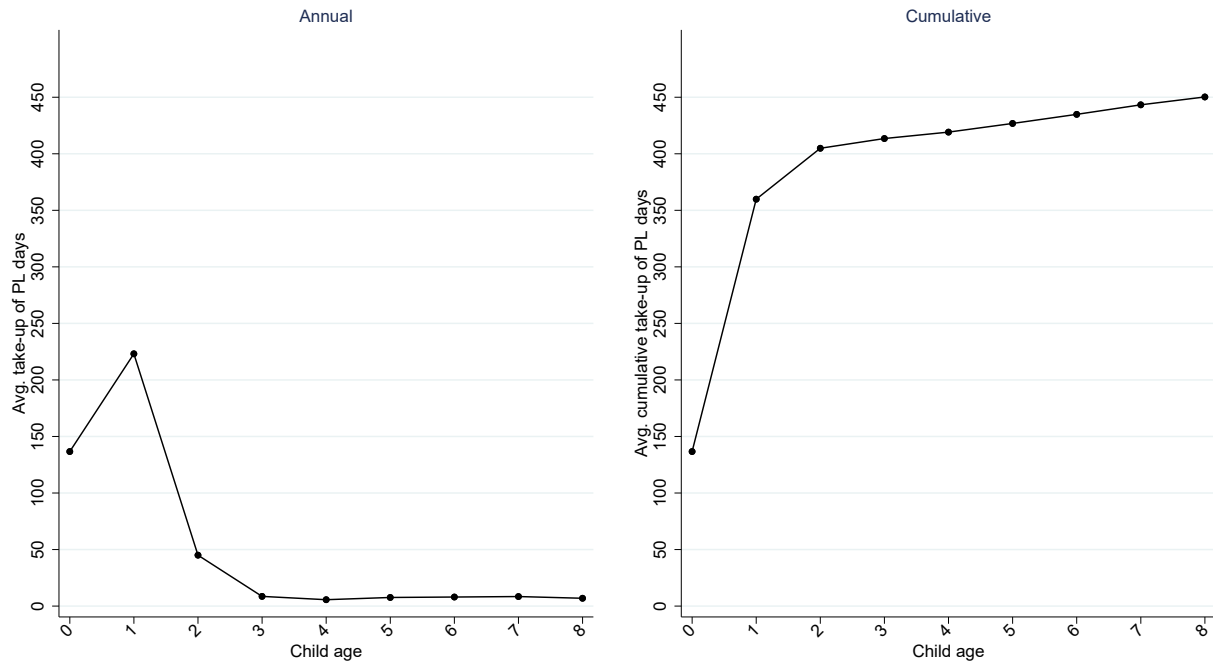
TABLE A.1.
Changes to the Swedish parental leave system over time

Year	Total paid days	Wage-replaced days	Replacement rate,%	SEK/day if SGI= 0	Flat rate days	SEK/day, flat rate
1974	180	180	90	25	0	0
1975	210	210	90	25	0	0
1976	210	210	90	25	0	0
1977	210	210	90	25	0	0
1978	270	240	90	32	30	32
1979	270	240	90	32	30	32
1980	360	270	90	37	90	37
1981	360	270	90	37	90	37
1982	360	270	90	37	90	37
1983	360	270	90	48	90	48
1984	360	270	90	48	90	48
1985	360	270	90	48	90	48
1986	360	270	90	48	90	48
1987	360	270	90	48	90	48
1988	360	270	90	60	90	60
1989	450	360	90	60	90	60
1990	450	360	90	60	90	60
1991	450	360	90	60	90	60
1992	450	360	90	60	90	60
1993	450	360	90	60	90	60
1994 ^a	450	360	90	64	90/0	60/0
1995 ^b	450	360	80	60	90	60
1996 ^c	450	360	75	60	90	60
1997	450	360	75	60	90	60
1998	450	360	80	60	90	60
1999	450	360	80	60	90	60
2000	450	360	80	60	90	60
2001	450	360	80	60	90	60
2002 ^d	480	390	80	120	90	60
2003	480	390	80	150	90	60
2004	480	390	80	180	90	60
2005	480	390	80	180	90	60
2006 ^e	480	390	80	180	90	60/180
2007	480	390	80	180	90	180
2008	480	390	80	180	90	180
2009	480	390	80	180	90	180

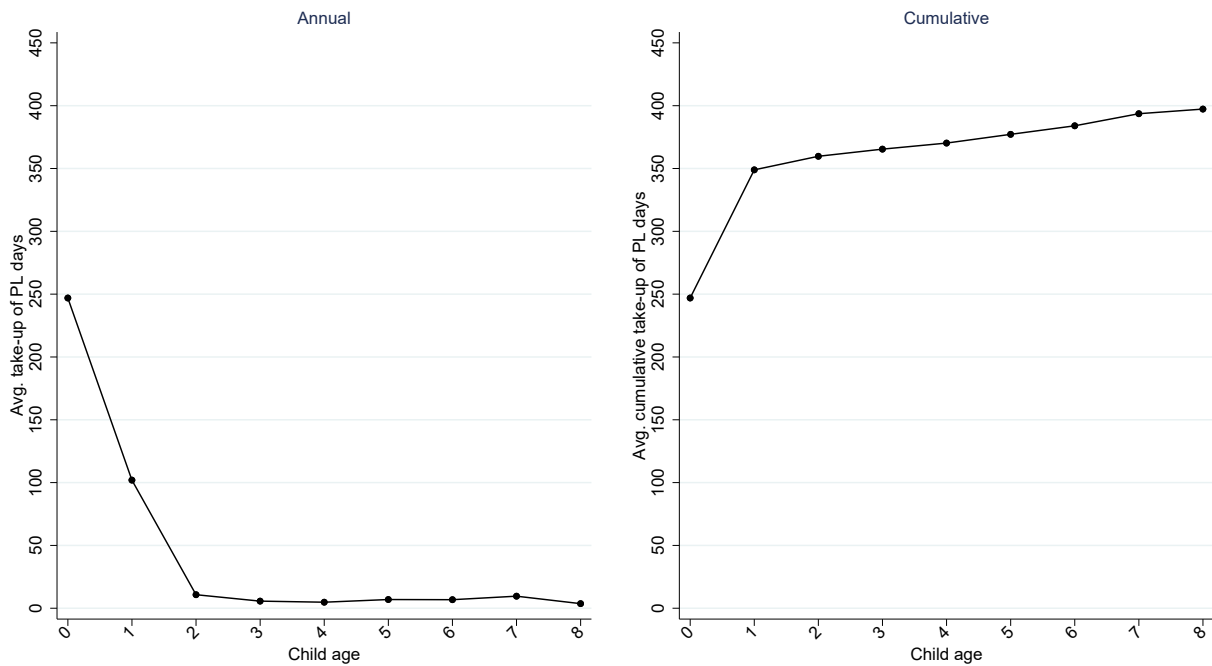
NOTE: The table shows the changes to the Swedish parental leave system since its introduction in 1974. a) During the second half of 1994, the flat-rate days were temporarily abolished for children older than one year. b) The first “daddy-month” was introduced for parents to children born on or after January 1st, 1995. For the 30 days of reserved leave, the replacement rate remained at 90 percent of previous earnings. c) For the 30 days of reserved leave, the replacement rate remained at 80 percent of previous earnings. d) The second “daddy-month” was introduced, targeting parents to children born on or after January 1, 2002. e) The flat rate was set to 180 SEK/day from July 1, 2006 onwards. (Source: National Insurance Board).

FIGURE A.1.
Average and Cumulative Parental Usage (gross days) 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. The sample restrictions are due to data limitations: Data on parental leave spells are available only from 1988, and are not possible to match to specific children.

TABLE A.2.
Effects of the reform on total fertility

	All	Private sector	Public sector
Treated	-0.006 (0.010)	-0.006 (0.017)	-0.008 (0.012)
<i>N</i>	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.

TABLE A.3.
Effects of the reform on total fertility by gender

	All	Private sector	Public sector
B. Females			
Treated	0.001 (0.015)	0.010 (0.023)	0.012 (0.020)
<i>N</i>	78,423	29,733	41,050
B. Males			
Treated	-0.016 (0.019)	0.003 (0.022)	-0.053 (0.035)
<i>N</i>	50,052	34,018	13,760

NOTES: The sample includes women and men who had a first child born in 1988 and 1989, and thus corresponds to the sample used to estimate the effect of the reform on parental leave take-up. The outcome variable measures the total number of children born to a person by year 2007.

TABLE A.4.
Industry mix for all firms & organizations active in Sweden, and for firms in study sample

	All workplaces (number)	All (pct)	Sample workplaces (number)	Sample workplaces (pct)
Armed forces	2194	3.392	946	3.274
Agriculture, hunting, forestry	998	1.543	283	0.980
Fishing	17	0.026	3	0.010
Mining and quarrying	151	0.233	38	0.132
Manufacturing	10009	15.474	3618	12.522
Electricity, gas and water	848	1.311	163	0.564
Construction	4858	7.510	569	1.969
Wholesale and retail trade etc.	11248	17.389	4624	16.004
Hotels and restaurants	2511	3.882	1236	4.278
Transport and communications	4331	6.696	1450	5.019
Financial intermediation	1502	2.322	777	2.689
Real estate, renting, other	1337	2.067	493	1.706
Data management operations	561	0.867	231	0.800
R&D	158	0.244	71	0.246
Other business activities	3229	4.992	1367	4.731
Public adm., defense, social insurance	2896	4.477	1543	5.341
Education	4808	7.433	2887	9.992
Health and social work	9958	15.395	7153	24.758
Lobbying, and religious act	1303	2.014	571	1.976
Recreation, culture, sports	1766	2.730	869	3.008
Total	64,683	100	28,892	100

NOTES: Columns (1) and (2) report the industry composition for all firms and public sector organizations 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).

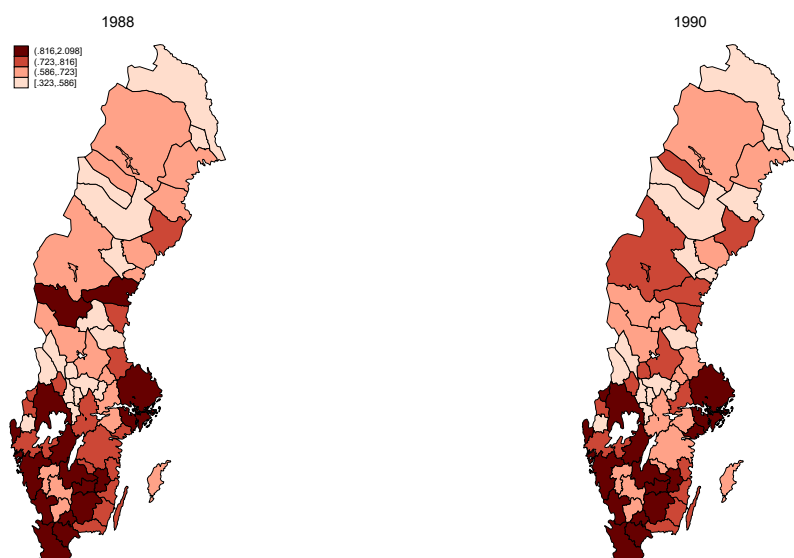
TABLE A.5.

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) <i>t</i> -stat for (1)-(2)	(4) Jan-July	(5) Oct-Dec	(6) <i>t</i> -stat for (4)-(5)	(7) DD est. of [(1)-(2)] - [(4)-(5)]	(8) <i>t</i> -stat for [(1)-(2)] - [(4)-(5)]
Number of workers	14.488	14.344	-1.386	14.337	14.446	1.075	0.254	1.743
Number of female workers	10.034	9.779	-1.786	9.796	9.910	0.816	0.370	1.847
Number of male workers	4.454	4.565	0.850	4.541	4.536	-0.040	-0.116	-0.640
Number women aged 20-40	4.814	4.726	-0.845	4.567	4.558	-0.095	0.079	0.547
Private sector	0.535	0.551	0.959	0.564	0.563	-0.042	-0.016	-0.718
Average age	36.625	35.968	-3.314	36.605	36.506	-0.493	0.558	1.975
Share female	0.670	0.661	-0.916	0.658	0.662	0.427	0.012	0.954
Private sector	0.535	0.551	0.959	0.564	0.563	-0.042	-0.016	-0.718
Average wage	18000.000	18000.000	0.365	18000.000	18000.000	-0.184	-79.886	-0.385
Average female wage	17000.000	17000.000	-0.100	17000.000	17000.000	-0.020	7.584	0.054
Average male wage	20000.000	20000.000	0.941	20000.000	20000.000	-0.786	-540.545	-1.222
Female contracted work hours	0.849	0.857	1.307	0.851	0.855	0.640	-0.004	-0.485
Male contracted work hours	0.954	0.960	0.961	0.959	0.956	-0.609	-0.010	-1.121
Wage bill main, 1000s SEK	2200.000	2200.000	0.006	2100.000	2200.000	0.978	31.637	0.688
Wage bill temp workers, 1000s SEK	147.950	135.561	-0.519	166.829	151.959	-0.856	-2.481	-0.085
Share no college	0.766	0.776	1.231	0.771	0.783	1.522	0.002	0.173
Share college	0.234	0.224	-1.231	0.229	0.217	-1.522	-0.002	-0.173
Observations	3,364	1,265		3,649	1,311		9,589	

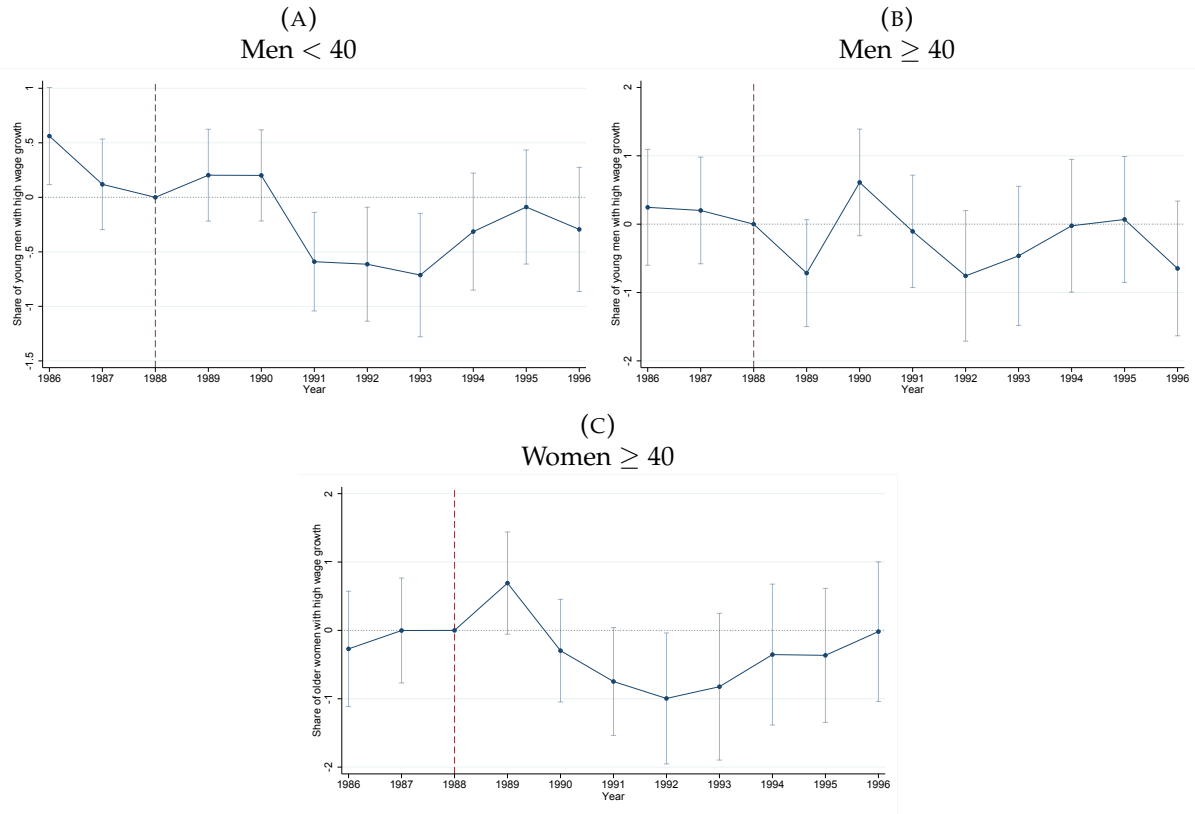
NOTES: The sample includes firms with 10-20 employees in the baseline year, of which exactly 1 woman gave birth. We exclude firms in which the woman who gave birth did so in August or September.

FIGURE A.2.
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 (5).

FIGURE A.3.
Effects on promotions of other co-workers within workplaces (private firms)



NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in establishment j , π_j^p , before and after its implementation, that is, the estimates for δ_1^t s in Equation 6, along with the 95% confidence intervals.

FIGURE A.4.
Predicted reform exposure by commuting zone in the Manufacturing, Education, and Health industries

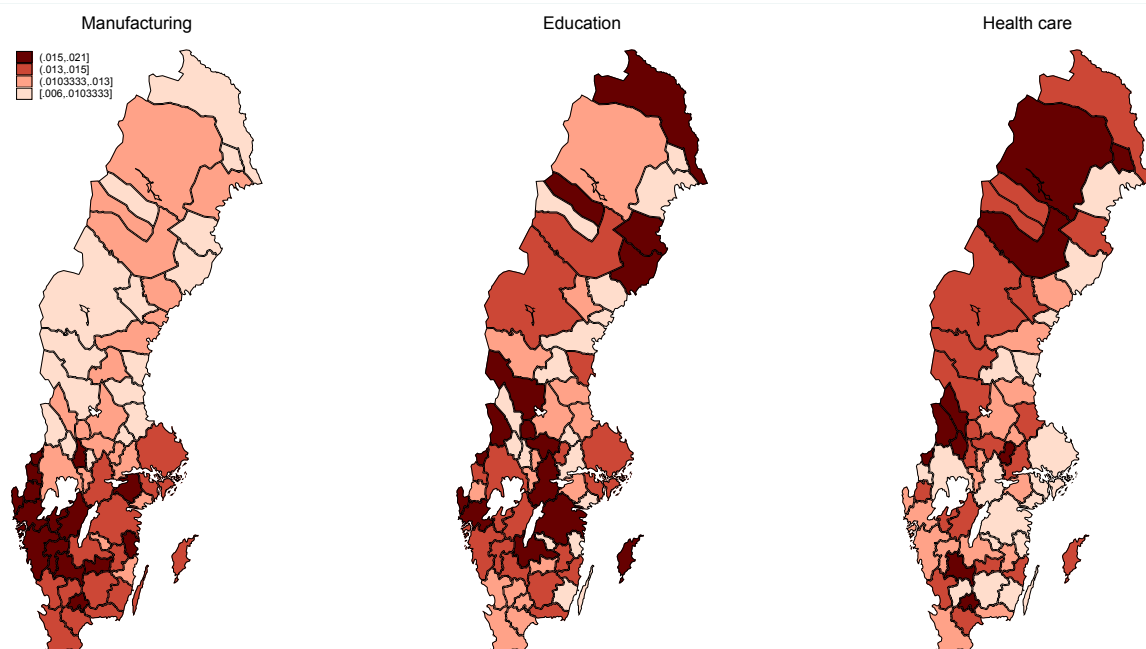
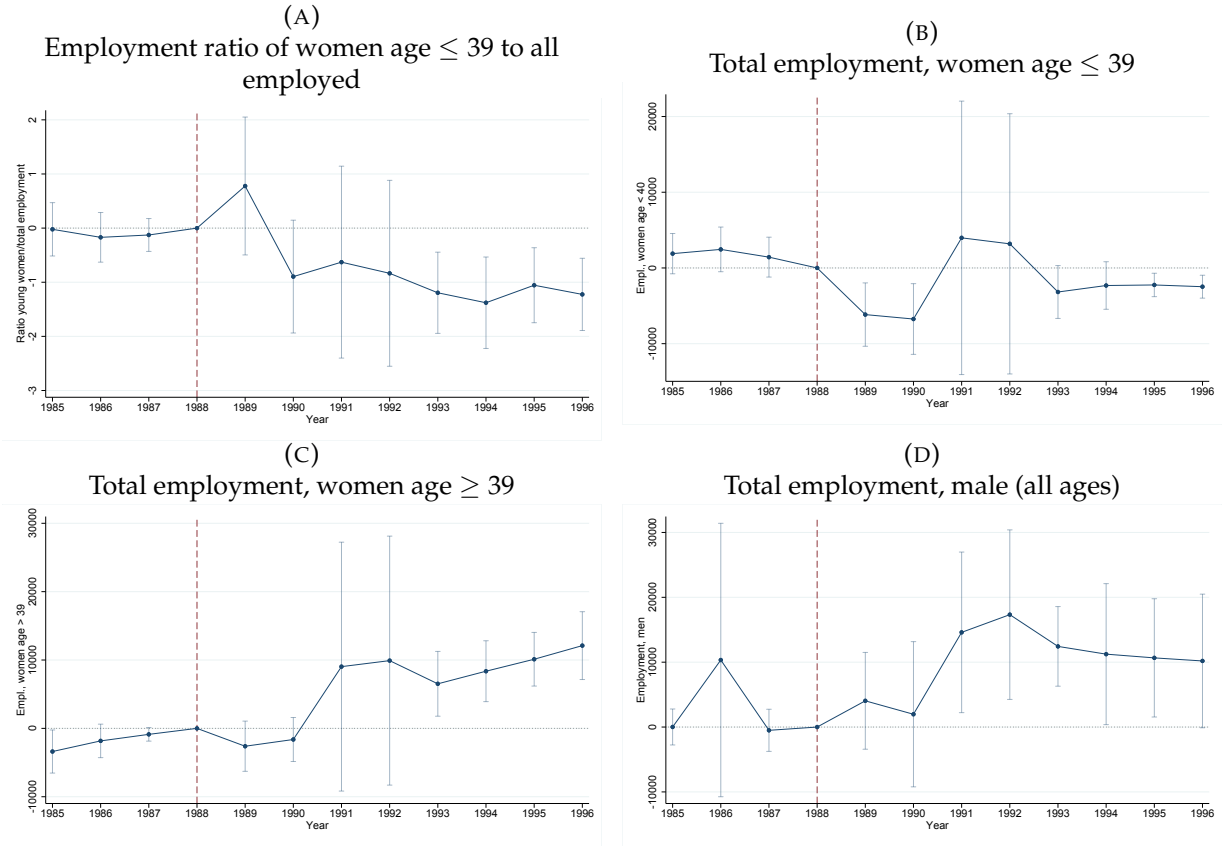
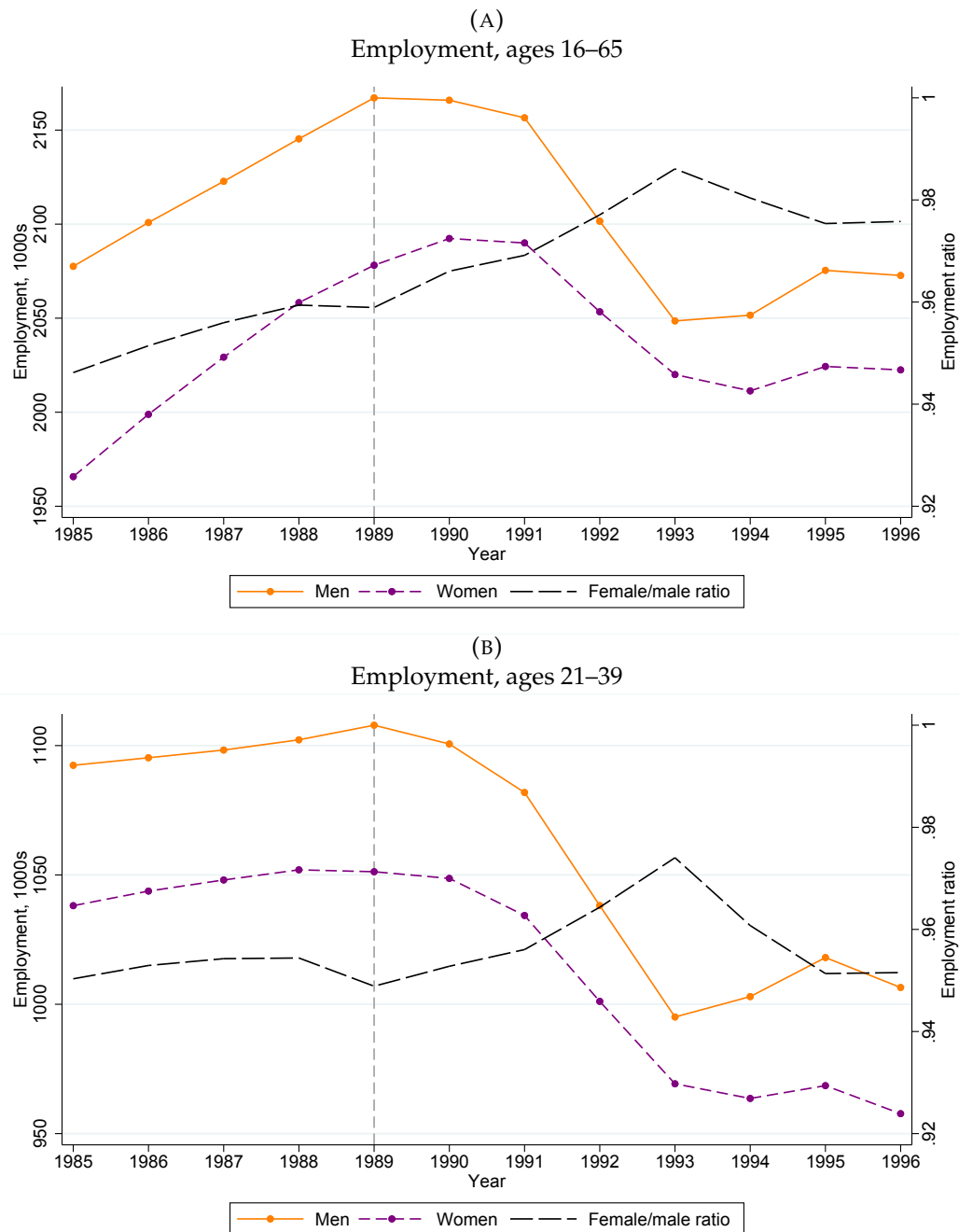


FIGURE A.5.
Aggregate effects on employment (including Stockholm region)



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 , $\pi_{c,k}^p$, before and after its implementation, that is, the estimates for γ_1^t s in Equation 7, along with the 95% confidence intervals.

FIGURE A.6.
Nationwide employment trend by gender 1985–1996



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