

Employer Responses to Family Leave Programs*

Rita Ginja[†]

Arizo Karimi[‡]

Pengpeng Xiao[§]

April 23, 2020

Abstract

Search frictions make worker turnover costly to firms. A three-month parental leave expansion in Sweden provides exogenous variation that we use to quantify firms' adjustment costs upon worker absence and exit. The reform increased women's leave duration and likelihood of separating from pre-birth employers. Firms with greater exposure to the reform hired additional workers and increased incumbent hours, incurring additional wage costs and experiencing sales declines. These adjustment costs varied by the availability of internal and external substitutes. Suggestive evidence shows that the reform reduced hiring of young women and lowered their starting wages compared to men and older women.

Keywords: Parental Leave, Firm-Specific Human Capital, Statistical Discrimination.

JEL-codes: J13, J16, J21, J22, J31.

*We thank Joe Altonji, Sandra Black, Peter Fredriksson, and Georg Graetz for helpful comments and suggestions. We also thank seminar participants at the 3rd Dale T. Mortensen Centre Conference, 2019 Midwest Macro Economic Meetings; 2019 Society of Labor Economists Meetings (SOLE); the 12th Nordic Conference on Register Data and Economic Modelling; Yale University; the Institute for Evaluation of Labor Market and Education Policy (IFAU); the 2018 Nordic Summer Institute in Labor Economics; 2018 York Workshop of Labour and Family Economics; Statistics Norway; the University of Bergen; Tinbergen Institute and at the University of Southampton. Arizo Karimi acknowledges financial support from the Jan Wallander and Tom Hedelius research foundation.

[†]Department of Economics, University of Bergen, Uppsala Center for Labor Studies (UCLS); IZA. rita.ginja@uib.no.

[‡]Corresponding author. Department of Economics, Uppsala University; Uppsala Center for Labor Studies (UCLS), and Institute for Evaluation of Labor Market and Education Policy (IFAU). arizo.karimi@nek.uu.se

[§]Department of Economics, Yale University. pengpeng.xiao@yale.edu.

1 Introduction

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

A unique setting in Sweden allows us to exploit random variation in workers' turnover and absence, and quantify their impacts on firms. Like many developed countries, Sweden provides generous family leave to new parents, and women spend a much longer time in parental leave than men.² While family leave entitlements foster stable employment of women after childbirth, they might also impose organizational challenges to firms. For example, it might be costly and time-consuming to find someone to replace the worker on leave, replacement workers might not be as productive, and overtime hours might cost extra. These challenges might serve as a basis for employers to statistically discriminate against women, so quantifying such adjustment costs would be a crucial step towards understanding the extent of firms' differential treatment of men and women.

Using a three-month parental leave extension in 1989, we estimate the causal effect of workers' extended absence on firm outcomes, including total labor costs, hiring and re-organization, and firm performance. This paper thus provides new causal evidence on the existence, magnitude, and sources of frictional costs faced by firms associated with worker absence and turnover.

Our approach takes advantage of the fact that treatment is randomly assigned and unrelated to any unobserved factors that might influence firm outcomes. The parental leave reform was unanticipated and retroactive: it was implemented in July 1989 but retroactively covered parents to children born in October 1988 and later. Retroactively eligible mothers could extend their leave and delay coming

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

²In 2011, women accounted for 76 percent of the total take-up of parental leave in Sweden, even though men and women had the same legal rights to paid leave (See https://www.scb.se/contentassets/813b12534a254bb28503983812d4649b/le0201_2012a01_br_x10br1201eng.pdf).

back to work by 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 quantify adjustment costs. We use population-wide matched employer-employee data to analyze workplace-level demand for incumbent and external labor inputs, using the subset of firms that had employees who give birth around the cutoff date of the policy intervention.

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 an auxiliary dataset on parental leave spells we show that, on average, eligible mothers took up 2.5 out of the 3 months of additional leave, while the increase in male take-up was only one week, on average. Thus, the reform predominantly altered the leave duration of new mothers. We document that women took their additional leave during the first two years after birth, and show that the paid-leave expansion did not simply crowd out unpaid leave. Moreover, we find suggestive evidence that women with fewer potential substitutes within the workplace used up less leave and shifted take-up to their spouses, indicating that workers internalize some of their employer's costs of finding suitable replacement. Finally, the reform increased the probability that women leave for a different firm by 18 percent in the year when parental leave ended, which we interpret as voluntary switches due to extended possibilities for job search (while on leave).

Given that workers were unexpectedly more likely to 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 with a child born between October and December of 1988, which entitled them to three additional months of leave. Because the reform was unanticipated, women could not have manipulated the timing of birth, and firms could not have altered their workforce composition in 1988. Thus, treatment intensity at the firm level is plausibly orthogonal to the unobserved determinants of the outcomes that we study. We compare firms with the same number of women who gave birth in the baseline year, and use exogenous variation in the *timing* of childbirth that gave rise to different treatment intensities. 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 a difference-in-differences empirical design. We trace out the full temporal pattern of the reform effect, including pre-reform trends in the outcomes, by combining the difference-in-difference model with

an event-time study. Note that in our setting, any impacts on firms' re-organization costs are the effects of *additional* leave, which are over and above the costs of workers going on child-related leave *per se*.

Our results show that private sector firms responded to the reform by increasing their permanent and temporary staff, by hiring new permanent workers, and by increasing the work hours of incumbents. The net impact of these adjustments on the firm's total wage bill was positive, indicating that such reorganization came at a monetary cost. Specifically, having one additional worker going on extended leave increased the total wage bill by an amount corresponding to 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 remunerating replacement staff.³ Using data on sales and productivity for firms in the manufacturing industry, we also find substantial short-run declines in sales revenue per worker, and value added per worker. We note that among this subset of firms, we find no re-organization of the personnel in response to the reform, indicating that difficulties in finding replacement workers may have negative implications for firm performance.

The ease with which firms can replace workers on leave depends on several factors: whether internal and external labor inputs are substitutable, and whether external labor market conditions are favorable for hiring. Focusing on the private sector, we find that firms in thick external local labor markets responded to the reform by predominantly relying on new hires, while keeping incumbents' work hours relatively unchanged. Firms in thin markets, on the other hand, resorted to internal hours increases. Furthermore, we find that workplaces where a large proportion of the workforce is concentrated in the same occupational category – i.e. firms where many workers can do the job of the worker on leave – tentatively responded to the labor shortage by relying more heavily on internal substitutes. Taken together, our findings highlight several sources of frictions associated with finding suitable replacement for workers on leave.

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

³For 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).

⁴For a discussion on the potential link between family leave programs and statistical discrimination against women in Sweden, see Albrecht et al. (2003, 2015, 1999). Moreover, the introduction of short leave programs have been shown to benefit

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

Our paper contributes to three strands of literature. We contribute to empirical work on firms’ ability to find substitutes for their workers when they leave their firm, which depends on the degree of specificity of human capital and on external labor market conditions. Similar to recent work by Jäger and Heining (2019), we test empirically for the presence of frictions by using exogenous worker exits.⁵ While Jäger and Heining (2019) exploit premature worker deaths, our paper contributes to this work by exploiting exogenous variation in the *duration* of worker absence generated by a parental leave reform. Moreover, in contrast to much of the previous work using worker exits to assess human capital specificity, productivity, or employer outcomes (see e.g. Jaravel, Petkova, and Bell, 2018; Bartel, Beaulieu, Phibbs, and Stone, 2014; Friedrich and Hackmann, 2017), we study impacts for firms in the overall economy, as opposed to case studies of certain industries or sectors.

Second, we contribute to the growing literature on parental leave programs. While there has been substantial work on the impact of leave programs on women’s careers and children’s outcomes, (Schönberg and Ludsteck, 2014; Lalive and Zweimüller, 2009; Lalive, Schlosser, Steinhauer, and Zweimüller, 2014; Dahl, Løken, Mogstad, and Salvanes, 2016; Liu and Skans, 2010; Bana, Bedard, and Rossin-Slater, 2018; Bailey, Byker, Patel, and Ramnath, 2019; Ginja, Jans, and Karimi, 2020), less is known about the ef-

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).

⁵See 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.

fects 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 employer outcomes. Gallen (2019) finds an increase in the probability of firm closures among small workplaces in Denmark, and some evidence that it causes strain on remaining co-workers. Friedrich and Hackmann (2017) finds that the reduction in labor supply of nurses after a parental leave expansion in Denmark had a negative impact on patient outcomes in Danish hospitals and health centers. In a related paper, Brenøe, Canaan, Harmon, and Royer (2020) studies 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.

Third, our paper relates to the literature linking job protection to statistical discrimination. Gruber (1994) exploits the regional variation in maternity leave mandates across U.S. states, and finds that employers shift the costs of the mandates onto the wages of women of childbearing ages. Thomas (2019) analyzes the effect of the Family and Medical Leave Act (FMLA) in the U.S. and finds that a woman hired after the FMLA was more likely to remain employed, but less likely to be promoted. The firm-level analysis in our paper provides results that are in line with the aggregate effects found in Thomas (2019). Moreover, Xiao (2020) estimates an equilibrium search model where firms pay adjustment costs during parental leave, and finds employers' statistical discrimination against women to be a major factor of the gender wage gap in early career. Overall, our results are consistent with these studies, and suggest that the group of workers whom family policies are aimed to help ultimately may bear part of the costs of the policy.

2 Background & Institutional Setting

In Sweden, gender neutral eligibility to government-paid parental leave was introduced in 1974. Parents were initially entitled to six months of paid parental leave, which was subsequently extended in several steps to today's 16 months of paid leave per child. From 1974 onward, the mother and the father of a child are given half of the entitled days each, but have the option of transferring paid leave days between one another.⁶

⁶In 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

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.⁷ 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 can be used flexibly. During the first 18 months after birth both parents are legally entitled to full-time job protected leave, irrespective of whether they claim parental leave benefits. Thereafter, parents have the option of reducing their working hours with up to 25 percent until the child turns 8 years old and claim leave benefits on a part-time basis.⁸ 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.2 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.

The Right to Return to Previous Job A worker has the legal right to return to the same job after the leave spell, where a *job* is defined as the combination of tasks and salary. If the tasks are no longer relevant when the employee returns to the workplace - due to e.g., re-organizations - the employer is obligated to find a similar position within the firm, with the same pay as before.

Extension of Paid Parental Leave: The 1989-reform Since the introduction in 1974 the parental leave system in Sweden has been subject to several extensions, and by 1989 parents were entitled to 12 months of paid leave, of which three months were compensated at the lower flat rate of 60 SEK per day. The

their leave-taking. In 2002 and 2016, a second and third month of paid leave were earmarked to each parent.

⁷Today, the replacement rate is 80 percent of previous earnings.

⁸Before 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.

reform that we exploit is an extension of the wage-replaced component of paid leave from 12 to 15 months that took place in 1989. The reform was implemented on July 1st 1989, but retroactively covered parents to children born in October 1988. Transition rules in the implementation implied that parents to children born in August and September 1988 received one and two additional months of paid leave, respectively.⁹

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

3 Data

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

For each worker/establishment/year, we merge information from the Wage Structure Statistics; an annual survey of establishments collecting information on the wages and working hours for each em-

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

ployee that worked at least one hour during the measuring month. Wages are reported as full-time equivalent monthly wages, and working hours are *contracted* working hours (expressed as percent of a full-time position). The Wage Structure Statistics is a population-wide register of organizations in the public sector, and includes the universe of private sector firms with at least 500 employees. For smaller private sector firms, a random sample is drawn based on a cross-classification of industry and establishment size. All in all, roughly 50 percent of all private sector employees are covered. All registers that we use start in 1985 and we use data up to 1996.

4 Program Take-up

We begin by quantifying the program take-up at the individual worker level using variation in eligibility status by child birth date. 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 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. In addition, 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 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.¹⁰ 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*.¹¹

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

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

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

in the placebo year. We analyze the effect of extended paid leave entitlement on woman i 's program take-up and labor supply outcome, denoted y_i , by estimating the following regression equation:

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

with $m = 7$ as the omitted category. The coefficients of interest are the β^m 's, which capture the difference in y_i between individuals giving birth in calendar month m compared to giving birth in July for those who gave birth in 1988, net of the corresponding difference among those who gave birth in the 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.¹²

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 are not assigned to specific children (it contains identifiers only for the parents, not for the child for whom the leave is taken). Because of these restrictions, we sample mothers to *first-born* children in 1988 and 1989. Looking at take-up immediately after the first child is born implies that we are unlikely to confound parental leave spells for multiple children in the household. Under the (testable) assumption that the reform did not affect subsequent fertility, we can also interpret the medium-run potential

¹²This empirical strategy was also used in Karimi et al. (2012), who studied the labor supply responses to 1989-reform and two additional reforms in the Swedish parental leave system. The focus of Karimi et al. (2012) was to study whether paid leave take-up crowds out unpaid leave, when job protection duration exceeds paid leave durations. They find evidence that such crowding out exists to some extent. However, as they lack data on hours worked, their labor supply measure relies on the assumption of unchanged monthly wage rates over the post-reform period, and divides annual income over wage rates to get a crude measure of months worked per year. In this paper, we instead measure labor supply by annual income earned from market work (which does not include governmental transfers, but may include top-up of benefits by employers), and labor market participation. Finally, their paper focuses on the effect of one additional month of paid leave, while we will focus on the effect of the full extension of three additional months, which we explain in sections below.

differences in take-up between the treated and untreated cohorts as a direct reform effect. Second, since we lack data on take-up before 1988, mothers to kids born in 1989 will serve as the control group. While all mothers of the latter group are treated, 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 equation (1) on the cumulative number of (gross)¹³ days on parental leave during the child's first three years of life (the vast majority of PL benefits are claimed within three years after birth; see Panel A of Figure A.2). Panel A of Figure 1 plots the estimated coefficients $\hat{\beta}^m$:s from equation (1) for women. The results show that take-up is monotonically and linearly higher by month of birth starting with the August-births, in line with the 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 identifying assumption.¹⁴

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

Figure A.3 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.

To get a sense of the magnitudes, we next 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

¹³Benefits 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.

¹⁴Note that the “dip” in the magnitude of the point estimates for the birth months of November and December in Figure 1 is an artifact of parental leave take-up not being measured in exact child age in months.

October–December and 0 if person i 's child was born in January–July.¹⁵ We then estimate:

$$y_i = \delta_0 + \beta(T_i \times D_i) + \delta_1 T_i + \delta_2 D_i + \mathbf{X}_i' \gamma + \epsilon_i \quad (2)$$

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 of Table 1). 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 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 beyond the earliest child ages, but for both sectors, the bulk of the extra leave is used 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 equation (1) on data from the calendar year *after* woman i has given birth to her child. This analysis includes the full population of women giving birth in 1988, and a corresponding placebo cohort of women giving birth in 1987. 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 2, 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

¹⁵We 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.

counterparts giving birth 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

In the remainder of the analyses, we make use of the *full* reform of three additional months of benefits, and thus ignore the transition rules of 1 and 2 additional months to August–September parents. To this end, we drop women who gave birth in August or September from the sample. Thus, for the samples used in the remainder of the paper, birth months correspond to $m_i = \{1, \dots \neq 8, \neq 9, \dots, 12\}$. Assuming that month of birth is as good as randomly assigned, this sample restriction poses no threat to identification. 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 birth cohorts.

To trace out the long run reform effect on labor supply, we estimate a dynamic difference-in-differences model which also 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:

$$y_{it} = \delta_0 + \sum_{\tau=-2}^8 \beta^\tau (T_i \cdot D_i \cdot \tau_{it}) + \sum_{\tau=-2}^8 (\delta_1^\tau \tau_{it} + \delta_2^\tau T_i \cdot \tau_{it} + \delta_3^\tau D_i \cdot \tau_{it}) \quad (3)$$

$$+ \delta_4 T_i \cdot D_i + \delta_5 T_i + \delta_6 D_i + \mathbf{X}_i' \gamma + \epsilon_{it}$$

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).¹⁶

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

¹⁶Namely, $\tau_{it} = \begin{cases} 1[t - 1988 = \tau] & \text{if } D_i = 1 \\ 1[t - 1987 = \tau] & \text{if } D_i = 0 \end{cases}$.

to the calendar year of birth (third difference).¹⁷

We estimate equation (3) 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. Because all women are entitled to 12 months of paid leave in the pre-reform regime, we expect the impact of the additional leave to show up one and possibly two years after the year of birth. The estimated coefficients $\hat{\beta}^{\tau}$ in 3 are presented in Figure 3, and show that women entitled to additional paid leave reduced their labor supply in the first two years after giving birth, but not in the longer run. Similarly, Panel B of Figure 3 shows that participation was negatively affected in the short run. Moreover, the results suggest an immediate impact of the increased leave entitlements, with eligible women delaying their return to the workplace compared to the counterfactual scenario of no extension.

4.5 Employer-employee separations

One margin that could have implications for employers is whether 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, a worker can switch jobs while on parental leave without losing the benefits. In light of this, extended leave duration may imply a longer period of job-search for those women looking to leave their firm.¹⁸ In Figure A.4 we plot the baseline separation hazard by time since birth, which shows 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 policy, we estimate equation (3) on the annual likelihood of switching from the pre-birth employer to a new firm. The results show that women who are entitled to extended leave are roughly 2.5 percentage points more likely to leave the pre-birth employer in year 2 after birth (Figure 4). Relative to the baseline hazard, this corresponds to an increase of about 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 re-

¹⁷In these event study analyses, the standard errors of estimates are clustered at individual level.

¹⁸Gottlieb et al. (2016) find that a Canadian reform that extended job-protected leave to one year for women giving birth after a cutoff date increases entrepreneurship by 1.9 percentage points. Moreover, Lalive et al. (2014) also find that access to job-protected parental leave changes women's job search behavior.

sult if, for example, the employee is re-allocated to an inferior position, with new tasks etc., prompting the worker to leave. With the data at hand, we are not able to explicitly rule out that the excess separations caused by the policy are involuntary.

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

The increased PL absence durations could potentially be costly to employers, in particular for those that are ill-equipped to find substitutes for the workers on leave. Workers themselves could internalize their employer’s difficulties when deciding on how much to take up of the of additional allowance, especially those who know themselves to be in unique positions at their workplace. For example, Hensvik and Rosenqvist (2019) show that workers with few 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 and by worker-firm sorting. In this section, we analyze whether the results found in Hensvik and Rosenqvist (2019) extend to PL 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).¹⁹ 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.²⁰ 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 (we thus match couples and look at within-household allocation of PL take-up). The results are presented in Figure 5. Panel A shows the results for own take-up, and panel B for spousal take-up (matched couples). While the effects do not show a monotonic pattern, the results suggest that the take-up response is increasing with the number of substitutes for women with zero, one, and two substitutes, conditional on workplace size and occupation-fixed effects. By comparison, spousal

¹⁹For the time period studied, data on occupations is unavailable.

²⁰The median worker in our sample has seven internal substitutes.

take-up is decreasing with the number of substitutes that woman i has, among those with zero to two substitutes. Taken together, these findings provide suggestive evidence that women with few internal substitutes potentially internalize the employers' adjustment costs of worker absence by shifting some of the additional parental leave allowance to their spouse.

We 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 and occupation-fixed effects. The results are presented in Figure A.5 and suggest 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 leave behavior to their firm's potential ability to insure itself from worker absence. These findings are informative in the context of PL 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 Subsequent fertility

In Table A.1 we report results from estimating a static difference-in-difference model (2) 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.

5 Employer Responses

Given the documented near-full take-up of the extended family leave program at the individual level, we now turn to firms' reactions to the reductions in female labor. We sample workplaces in the private sector at which at least one female employee had a child born in 1988.²¹ Our identification strategy

²¹Similar to section 4, we make use of the *full* reform of three additional months, and drop workplaces that had women giving birth in August or September in 1988.

exploits the fact that workplaces are differentially exposed to varying leave durations of their female employees, depending on whether these employees happened to give birth before or after the eligibility cutoff date. We define the establishment's treatment intensity measure to be the proportion of the workforce that gave birth from October to December in 1988. Since the reform was unanticipated, neither the workers nor the firms could have manipulated the timing of births to be before or after the eligibility date. Therefore, the treatment intensity measure is orthogonal to any unobserved determinants of the firm level outcomes that we study. Moreover, we extract data for the corresponding set of workplaces in which at least one female employee gave birth in 1987, which will serve as a placebo set of firms. Hence, we will refer to these firms as the "placebo cohort", and the 1988 firms as the "treatment cohort".

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

$$\pi_j = \frac{N_j^{OctDec}}{N_j}. \quad (4)$$

We estimate the following triple-differences specification (similar to equation (3) in section 4):

$$\begin{aligned} y_{jt} = & \delta_0 + \sum_{\tau=-2}^8 \beta^\tau \left(\pi_j \cdot D_j \cdot \tau_{jt} \right) + \sum_{\tau=-2}^8 (\delta_1^\tau \tau_{jt} + \delta_2^\tau \pi_j \cdot \tau_{jt} + \delta_3^\tau D_j \cdot \tau_{jt}) \\ & + \delta_4 \pi_j \cdot D_j + \delta_5 \pi_j + \delta_6 D_j + \mathbf{X}_j' \gamma + \epsilon_{jt} \end{aligned} \quad (5)$$

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. Vector \mathbf{X} includes flexible controls for the total number of workers giving birth in the baseline year interacted with indicators for baseline establishment size decile, pre-reform workplace characteristics (polynomial in the share of the workforce that is female, the age composition of the workforce, the share of the workforce that consists of women in childbearing ages, the educational composition at the establishment, and a polynomial in workplace size) and fixed effects for 2-digit industry affiliation.

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 un-

conditional 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 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 is inherently subject to higher employee child births in the future than the other cohort of firms. If the treatment cohort firms respond to the policy by hiring more women, then the long-run impact of the policy change would be compounded by the firm’s hiring decisions immediately after the reform. Thus, our results within a relatively short window (around three years) could be interpreted as the direct effects of the reform, whereas long-run results might also include snowballing effects from firms’ short-run responses (as workforce compositions change).

5.1 Summary statistics

Table 3 reports summary statistics for pre-determined workplace attributes for our study sample of establishments as well as for the universe of all active private sector establishments in Sweden in 1988 for comparison.²² The establishments in our study sample are similar to the full population of establishments in terms of education composition, earnings, wage rates, and contracted work hours. However, our sample have a higher share of female employees, more employees giving birth in a given year, and are larger compared to the average establishment in the population.

Table A.2 shows that the industry composition of our study sample is representative of the full population of private sector firms. Finally, in Table A.3 we show that there are no differences in the characteristics of firms whose employees give birth in the fall vs. spring, for firms with 10–20 employees where only one woman gave birth.

5.2 Employer adjustment strategies

To gauge overall changes in the firms’ labor force, we first look at the impact of the reform on the total labor cost at the workplace – the sum of annual earnings of all workers on the firms’ payroll, including

²²We exclude the smallest (fewer than ten employees) and the very largest (top 1 percentile of size distribution) establishments from our analysis data.

women on parental leave. Since the Swedish government pays for the PL benefits at the replacement level of 90 percent and not all firms top up the remaining 10 percent, having workers on extended leave implies that the firm has fewer people to pay wages to in those months, if the firm does nothing to replace the women on leave.

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

Figure 6 presents the coefficients β^T from specification (5) for the firm's total wage bill (which includes both primary and temporary employees). The results show a negative effect on the total wage bill in year one after birth. This is mainly driven by the fact that "treated" firms did not pay wages for workers on leave during the additional leave months. We find an increase in the total wage bill in years two and three, pointing to reorganization at the firm at a cost over and above the salary payments for the workers who go on extended leave. In terms of magnitude, the estimates imply that in a workplace of average size (around 48 workers), having one additional female worker entitled to an extended parental leave would increase the total wage bill by an amount corresponding to the salary cost of 0.5 full-time workers. Therefore the adjustment costs are quite sizeable.

In Figure 7 we decompose the effect on total wage costs into a component attributed to primary employees and to temporary employees, respectively. The total wage cost of primary employees decreases in year one after childbirth, which is likely a result of increased leave duration of eligible workers. However, in years 2 and 3, there is an increase in the payments made to primary workers. The wage-bill paid to temporary workers increases from year 1 to 4 after childbirth, showing that firms adjust immediately by increasing their temporary staff. However, during the first year after childbirth the wage bill for temporary staff does not increase sufficiently to offset the reduction in primary employees' labor inputs. Changes to the wage bill to primary workers can be driven both by the number of employees and their work hours. In panels C and D of Figure 7 we therefore decompose the wage sum to primary workers

into hours and workforce size. To measure hours supplied by the coworkers of women on leave, we calculate the average contracted work hours of all primary employees, excluding the employees who gave birth in the baseline year. Contracted hours are measured as a proportion of full-time equivalent hours (for example, 75 percent). Results show that the work hours of the coworkers of the women on leave increase in year 1, 2, and 3 (significant at the 10, 5, and 10 percent levels, respectively), and the number of primary employees also increase, primarily in the second and third years.

While our main focus is on private sector employers, we report the corresponding set of results for establishments in the public sector in Figure A.6. Like the private sector, there is a drop in the salary payments to primary workers in year one, but unlike the private sector firms, there are no discernible patterns of adjustments or reorganizations at the public sector establishment to offset the potential effects of the reform on their workers' absence durations. Given that individual-level program take-up were both quantitatively and qualitatively similar (as we show in section section 4), the heterogeneity in employer adjustment by sector of employment is not likely driven by heterogeneity in the size of the labor supply shock caused by the reform. An alternative explanation is that the public sector is to a large extent comprised by schools and hospitals, which are financed based on a system of politically fixed budgets. The inability to make labor adjustments may have important implications for the outcomes of these institutions. A recent example is emphasized by Friedrich and Hackmann (2017), who show that labor shortages of nurses in Denmark - due to a parental leave reform - had detrimental impacts on patient outcomes.

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

5.3 Effects on measures of firm performance

For a subset of the firms in our sample, namely firms in the manufacturing industry, we have information on sales revenue and value added. These constitute roughly 23 percent of our sample of firms. Table A.4 shows summary statistics for this subset of firms; compared to our full sample, the manufacturing firms have lower shares of female workers, fewer employees giving birth in a given year, higher average wage,

and larger workforce.

Panels A and B of Figure 8 report the estimated effects of the reform on the sales revenue per worker and the value added per worker, respectively, for this subset of firms. For both measures, there is a significant reduction in firm performance in year two (when program take-up was the largest). In a firm of average size, having one worker go on extended leave amounts to a drop in sales revenue of 224,000 SEK, which corresponds to an 18 percent reduction relative to the sample mean. For value added, the corresponding effect amounts to a 13 percent decline relative to the sample mean. Thus, these effects are sizeable. In panels C and D of Figure 8, we show, however, that firms in this sub-sample are not reorganizing their personnel in the same way as firms in the overall sample: there is no change in the number of employees, and there are no statistically significant effects on the wage bill paid to temporary workers (even if the point estimates suggest an increase in year 3).

Thus, there may be heterogeneity in the ease with which firms can quickly find replacement for workers on leave, which would have implications for the costs related to worker turnover. In the next section, we explore such heterogeneity in frictions in closer detail.

6 Heterogeneity in Frictions across Labor Markets

We have shown in the previous section that firms are indeed affected by workers taking extended parental leaves. When women take additional time off, firms have to incur costs in finding, hiring, and training temporary workers, or paying for more overtime hours of incumbent workers. The magnitude of such costs are likely to depend on how easily the firm is able to find good substitutes for the worker(s) on leave.²³

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

²³For example, Jäger and Heining (2019) suggest that incumbent workers are closer substitutes to one another compared to outsiders, and that thin external markets lead to higher firm-specificity of human capital and lower replaceability of incumbents.

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

6.1 External labor market conditions

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

To capture the external labor market conditions facing the firms in our sample, we construct measures of overall and gender-specific industry-level labor market thickness at the local level, using population-wide data on workers (excluding self-employed) aged 19–64. We delineate 64 commuting zones (CZs), and define labor market thickness as the share of employment in a 2-digit industry within a commuting zone relative to the nationwide employment share in that industry:

$$\theta_{kct}^g = \frac{emp_{kct}^g}{emp_{ct}^g} / \frac{emp_{kt}^g}{emp_t^g} \quad (6)$$

for each gender $g = \{0, 1\}$, industry k , commuting zone c , in year t .²⁴ 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 9 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

²⁴Figure A.7 shows the female labor market thickness for an example industry (financial intermediation) in the 64 commuting zones in Sweden to provide a visualization of the variation in local labor market thickness in our data.

that a worker who leaves a firm cannot be costlessly replaced; external conditions could make supply constraints binding, in which case the firm's demand for the remaining workers' labor may increase.

6.2 Internal substitutability of workers

In Section 4.6, we show that workers potentially internalize employers' replacement costs by adjusting their response to the reform depending on the extent to which they have internal substitutes, suggesting some degree of specificity of human capital. We now turn to explore the role of internal substitutability on employers' responses to worker absence.

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

The results are presented in Figure 10. Firms with higher internal supply constraints (lower degree of specialization) tentatively adopted new hires as their main adjustment strategy, while firms in the upper tail of the substitutability index distribution resorted to increasing work hours of incumbents. We argue that it is not likely such heterogeneity is driven by differences in the size of the labor supply shock due to workers' adjustment behavior, based on two pieces of evidence. First, our findings for workers showed substantial labor supply reductions for workers of both types. Second, if workers fully internalize the adjustment costs, we would see no adjustment on the part of employers in firms with a heavy concentration of workers in the same occupation. In sum, the fact that firms employed different strategies depending on the availability of internal substitutes suggests that human capital specificity may imply binding supply constraints, and thus points to an additional source of frictions facing firms dealing with turnover.

7 Implications for Women's Careers and Statistical Discrimination

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

Before we dive into the analysis of statistical discrimination, first we mention a few cautionary notes. Since we do not directly observe firm-specific human capital, the degree of substitutability between male and female workers, or discrimination at the workplace level, we cannot unambiguously interpret certain actions of the firms as statistical discrimination against women. For example, if we observe that a firm in a local labor market with abundant female labor supply is refraining from external female new hires, we would not be able to tell if they do so because human capital is too firm-specific, or because this firm is discriminatory. However, if we are willing to assume that female labor is at least as good as male labor to replace the women on leave, a firm making external male but not female hires in a thick market with a large pool of both men and women candidates would constitute suggestive evidence for statistical discrimination. Indeed, in Figure 11 we show that more intensely treated workplaces in thick markets for both male and female labor appeared to predominantly hire male workers in response to the reform.

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

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

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

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

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

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

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

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

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

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 the outcome variable across industries with different predicted exposure, before and after the intervention. (In Figure A.8 we show that there is considerable variation in predicted reform exposure both across industries and within industries across local labor markets.)

We stress that a causal interpretation of the γ_1^t s would be contentious; we do not have experimental variation in reform exposure. For a causal analysis, we would have to assume that industries differing in their concentration of young women would evolve similarly in the absence of the reform, the validity of which we could potentially gauge by looking at differences in the pre-reform trends in the outcomes. One key source of potential confounding, however, is the Swedish financial crisis which erupted in the fall of 1992 and led to a large increase in unemployment, potentially affecting male and female-dominated industries differentially. However, we note that the employment decline was larger among male workers, such that the overall female-to-male employment ratio increased during the crisis years (see Figure A.9). Thus, differential changes in labor market outcomes driven by the crisis should arguably be in favor of women. Nevertheless, we cannot rule out potential confounding effects of the mid-1990s crisis.

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

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

market/industry. Because our data starts in 1985, we can identify promotion rates and new hires from 1986 onward.

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

Figure 13 shows an immediate larger post-reform decline in the number of new hires among women in childbearing ages in more exposed industries compared to low exposed ones. In 1990, the decline in female young new hires correspond to a 2 percent decrease in an industry within a local labor market with an average reform exposure. In contrast, there is no apparent change in the hiring rate of male workers or of older women workers immediately after the reform, in more exposed industries. There are such declines, however, for all three groups after 1993, i.e., during the crisis years. Thus, it is likely that the long-run responses to hirings are driven by the crisis rather than the reform.

Regarding starting wages of new hires, Figure 14 shows a gradual increase in the gender gap in wages of young workers (wage gap became more negative). We interpret these findings with caution, however, due to potential compositional changes driven by the financial crisis. In particular, unemployment increased disproportionately among men, so we might worry that those men who were hired in an environment of declining overall male employment may be positively selected compared to those women who were hired in the same time period. Nevertheless, we note that the gap in starting wages between newly hired young female workers and older female workers also declined more in more exposed industries, and selection concerns should be less important in this comparison. In the average industry, the increase in the gender (starting) wage gap corresponded to 3–4 percent, and the increase in the wage gap between younger and older women to 2–4 percent.

What would all of these changes imply for the overall gender wage gap? Declining promotion rates, hiring rates, and lower relative starting wages to female hires all suggest an increase in the gender wage gap. In Figure 15, we estimate equation (7) on the overall gender wage gap, and find a larger gender

wage gap in more exposed industries immediately after the reform implementation, an "effect" that does not gradually dissipate over time.

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

8 Conclusions

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

Turning to firm responses, we find that private sector firms with greater exposure to the reform adjusted by hiring temporary and permanent workers, and by increasing the work hours of incumbent workers. The net effect of these adjustments on firms' total wage bill is positive, suggesting that such adjustments come at a monetary cost. Using data on sales and productivity for firms in the manufacturing industry, we also find substantial short-run declines in sales revenue per worker, and value added per worker. We note that among this subset of firms, we find no reorganization of the personnel in response to the reform, indicating that difficulties in finding replacement workers may have implications for firm performance. Indeed, we document heterogeneity in employer adjustment based on the ease with which replacement workers can be found. In particular, firms in thick external markets responded to the reform by relying on temporary workers and new hires, while keeping incumbents' work hours unchanged. Firms in thin markets, on the other hand, resorted to internal hours increases. Finally, we also tentatively conclude that there may have been heterogeneous responses by the degree of internal

substitutability within the workplace: establishments with a high concentration of its workforce in their dominant occupation group seems to have responded to the labor supply shortage by increasing incumbents' hours more compared to firms that had a more diverse occupational structure. These findings thus suggest several sources of potential frictions associated with finding and hiring replacement workers.

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

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" and finds similar effects on firms' shut-down probabilities irrespective of the time that the firm had at their disposal to plan for the increased leave duration. Moreover, if human capital is very firm-specific, or for any other reasons suitable replacement is not easy to find, a longer planning horizon would not necessarily eliminate the adjustment costs for firms when their workers go on leave.

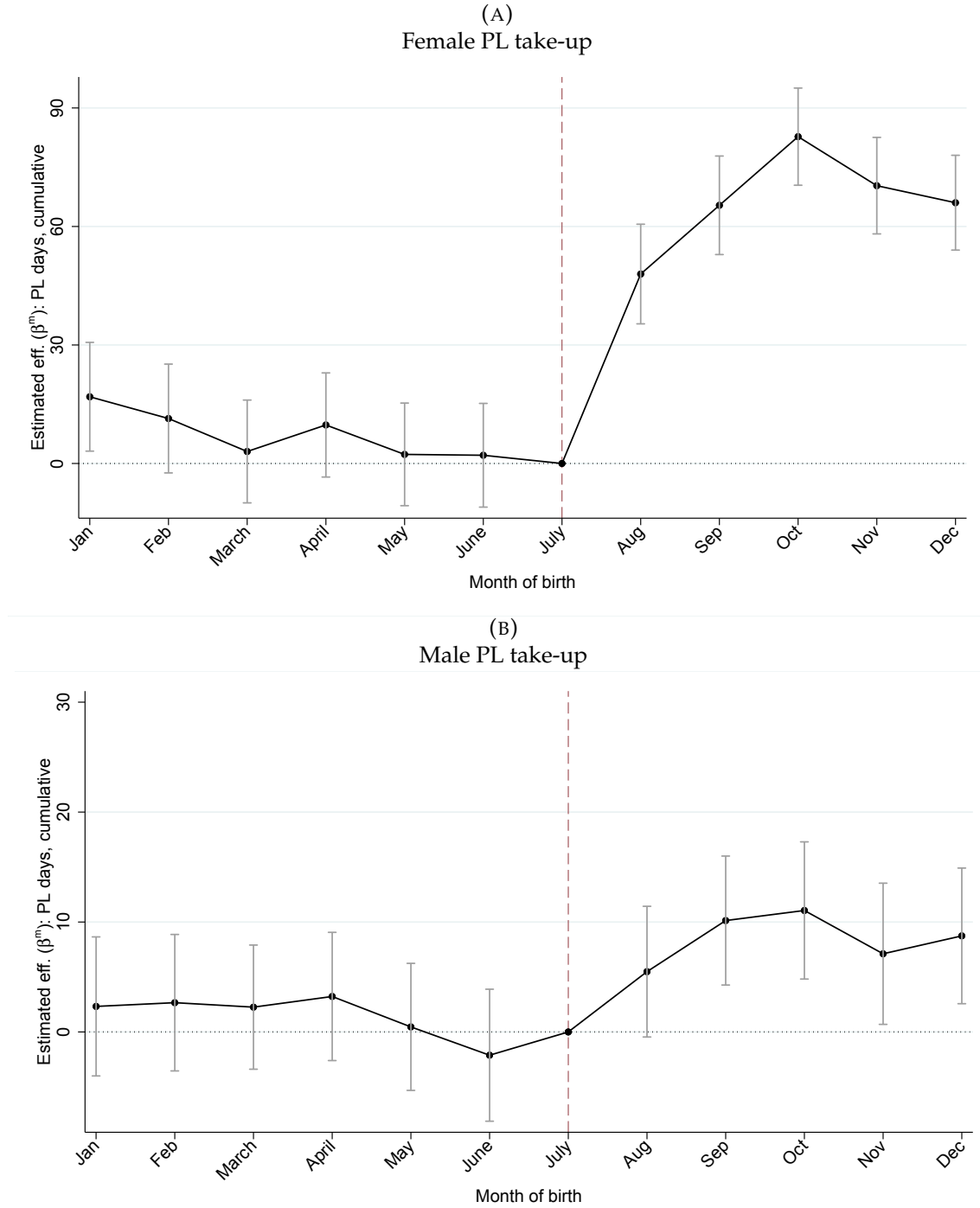
References

- Albrecht, J., A. Björklund, and S. Vroman (2003). Is there a glass ceiling in sweden? *Journal of Labor economics* 21(1), 145–177.
- Albrecht, J., P. S. Thoursie, and S. Vroman (2015). Parental leave and the glass ceiling in sweden. In *Gender Convergence in the Labor Market*, pp. 89–114. Emerald Group Publishing Limited.
- Albrecht, J. W., P.-A. Edin, M. Sundström, and S. B. Vroman (1999). Career interruptions and subsequent earnings: A reexamination using swedish data. *Journal of human Resources*, 294–311.
- Angelov, N., P. Johansson, and E. Lindahl (2016). Parenthood and the gender gap in pay. *Journal of Labor Economics* 34(3), 545–579.
- Bailey, M. J., T. S. Byker, E. Patel, and S. Ramnath (2019). The long-term effects of california’s 2004 paid family leave act on women’s careers: Evidence from us tax data. Technical report, National Bureau of Economic Research.
- Baker, M. and K. Milligan (2008). How does job-protected maternity leave affect mothers’ employment? *Journal of Labor Economics* 26(4), 655–691.
- Bana, S., K. Bedard, and M. Rossin-Slater (2018). The impacts of paid family leave benefits: regression kink evidence from california administrative data. Technical report, National Bureau of Economic Research.
- Barron, J. M., D. A. Black, and M. A. Loewenstein (1993). Gender Differences in Training, Capital, and Wages. *The Journal of Human Resources* 28(2), 343–364.
- Bartel, A. P., N. D. Beaulieu, C. S. Phibbs, and P. W. Stone (2014). Human capital and productivity in a team environment: evidence from the healthcare sector. *American Economic Journal: Applied Economics* 6(2), 231–59.
- Baum, C. L. (2003). Does early maternal employment harm child development? an analysis of the potential benefits of leave taking. *Journal of Labor Economics* 21(2), 409–448.
- Bergemann, A. and R. T. Riphahn (2015). Maternal employment effects of paid parental leave. Working Paper 9073, IZA.
- Bowlus, A. J. (1997). A search interpretation of male-female wage differentials. *Journal of Labor Economics* 15(4), 625–657.
- Brenøe, A. A., S. P. Canaan, N. A. Harmon, and H. N. Royer (2020, January). Is parental leave costly for firms and coworkers? Working Paper 26622, National Bureau of Economic Research.
- Bronson, M. A. and P. Skogman Thoursie (2019). The wage growth and within-firm mobility of men and women: New evidence and theory. Technical report.
- Buckles, K. S. and D. M. Hungerman (2013). Season of birth and later outcomes: Old questions, new answers. *The Review of Economics and Statistics* 95(3), 711–724.
- Cortes, G. M. and A. Salvatori (2019). Delving into the demand side: changes in workplace specialization and job polarization. *Labour Economics* 57, 164–176.
- Dahl, G. B., K. V. Løken, M. Mogstad, and K. V. Salvanes (2016). What is the case for paid maternity leave? *Review of Economics and Statistics* 98(4), 655–670.

- Friedrich, B. U. and M. B. Hackmann (2017). The returns to nursing: Evidence from a parental leave program. Technical report, National Bureau of Economic Research.
- Gallen, Y. (2019). The effect of maternity leave extensions on firms and coworkers. Technical report.
- Ginja, R., J. Jans, and A. Karimi (2020). Parental leave benefits, household labor supply, and children's long-run outcomes. *Journal of Labor Economics* 38(1), 261–320.
- Gottlieb, J. D., R. R. Townsend, and T. Xu (2016, July). Does career risk deter potential entrepreneurs? Working Paper 22446, National Bureau of Economic Research.
- Gruber, J. (1994). The incidence of mandated maternity benefits. *The American economic review*, 622–641.
- Han, W.-J., C. Ruhm, and J. Waldfogel (2009). Parental leave policies and parents' employment and leave-taking. *Journal of Policy Analysis and Management* 28(1), 29–54.
- Hensvik, L. and O. Rosenqvist (2019). Keeping the production line running internal substitution and employee absence. *Journal of Human Resources* 54(1), 200–224.
- Hotz, V. J., P. Johansson, and A. Karimi (2017). Parenthood, family friendly firms, and the gender gaps in early work careers. Technical report, National Bureau of Economic Research.
- Jäger, S. and J. Heining (2019). How substitutable are workers? evidence from worker deaths.
- Jaravel, X., N. Petkova, and A. Bell (2018). Team-specific capital and innovation. *American Economic Review* 108(4-5), 1034–73.
- Karimi, A., E. Lindahl, and P. Skogman Thoursie (2012). Labour supply responses to paid parental leave. Technical report, Working Paper, IFAU-Institute for Evaluation of Labour Market and Education Policy.
- Kleven, H., C. Landais, and J. E. Søgaaard (2019). Children and gender inequality: Evidence from denmark. *American Economic Journal: Applied Economics* 11(4), 181–209.
- Kluve, J. and M. Tamm (2013). Parental leave regulations, mothers' labor force attachment and fathers' childcare involvement: evidence from a natural experiment. *Journal of Population Economics* 26(3), 983–1005.
- Lalive, R., A. Schlosser, A. Steinhauer, and J. Zweimüller (2014). Parental leave and mothers' careers: The relative importance of job protection and cash benefits. *Review of Economic Studies* 81(1), 219–265.
- Lalive, R. and J. Zweimüller (2009). How does parental leave affect fertility and return to work? evidence from two natural experiments. *The Quarterly Journal of Economics* 124(3), 1363–1402.
- Lequien, L. (2012). The impact of parental leave duration on later wages. *Annals of Economics and Statistics* (107/108), 267–285.
- Liu, Q. and O. N. Skans (2010). The duration of paid parental leave and children's scholastic performance. *The BE Journal of Economic Analysis & Policy* 10(1).
- Rossin-Slater, M., C. J. Ruhm, and J. Waldfogel (2013). The effects of california's paid family leave program on mothers' leave-taking and subsequent labor market outcomes. *Journal of Policy Analysis and Management* 32(2), 224–245.
- Ruhm, C. J. (1998). The economic consequences of parental leave mandates: Lessons from europe*. *The Quarterly Journal of Economics* 113(1), 285.

- Schönberg, U. and J. Ludsteck (2014). Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth. *Journal of Labor Economics* 32(3), 469–505.
- Stearns, J. (2018). The long-run effects of wage replacement and job protection: Evidence from two maternity leave reforms in great britain. Technical report, Mimeo.
- Thomas, M. (2019). The impact of mandated maternity benefits on the gender differential in promotions: Examining the role of adverse selection. Technical report, Mimeo.
- Waldfogel, J. (1999). The impact of the family and medical leave act. *Journal of Policy Analysis and Management* 18(2), 281–302.
- Xiao, P. (2020). Wage and employment discrimination by gender in labor market equilibrium. Technical report, Unpublished Manuscript.

FIGURE 1.
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.

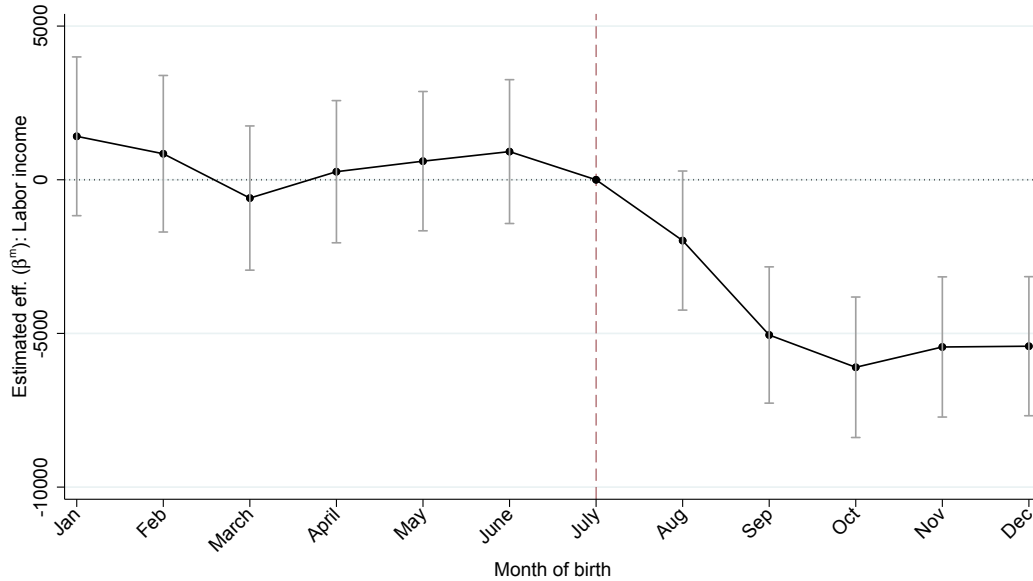
TABLE 1.
Effects of the reform on parental leave take-up by gender and child age

	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 table reports estimates of $\hat{\beta}$ from equation (2).

FIGURE 2.

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



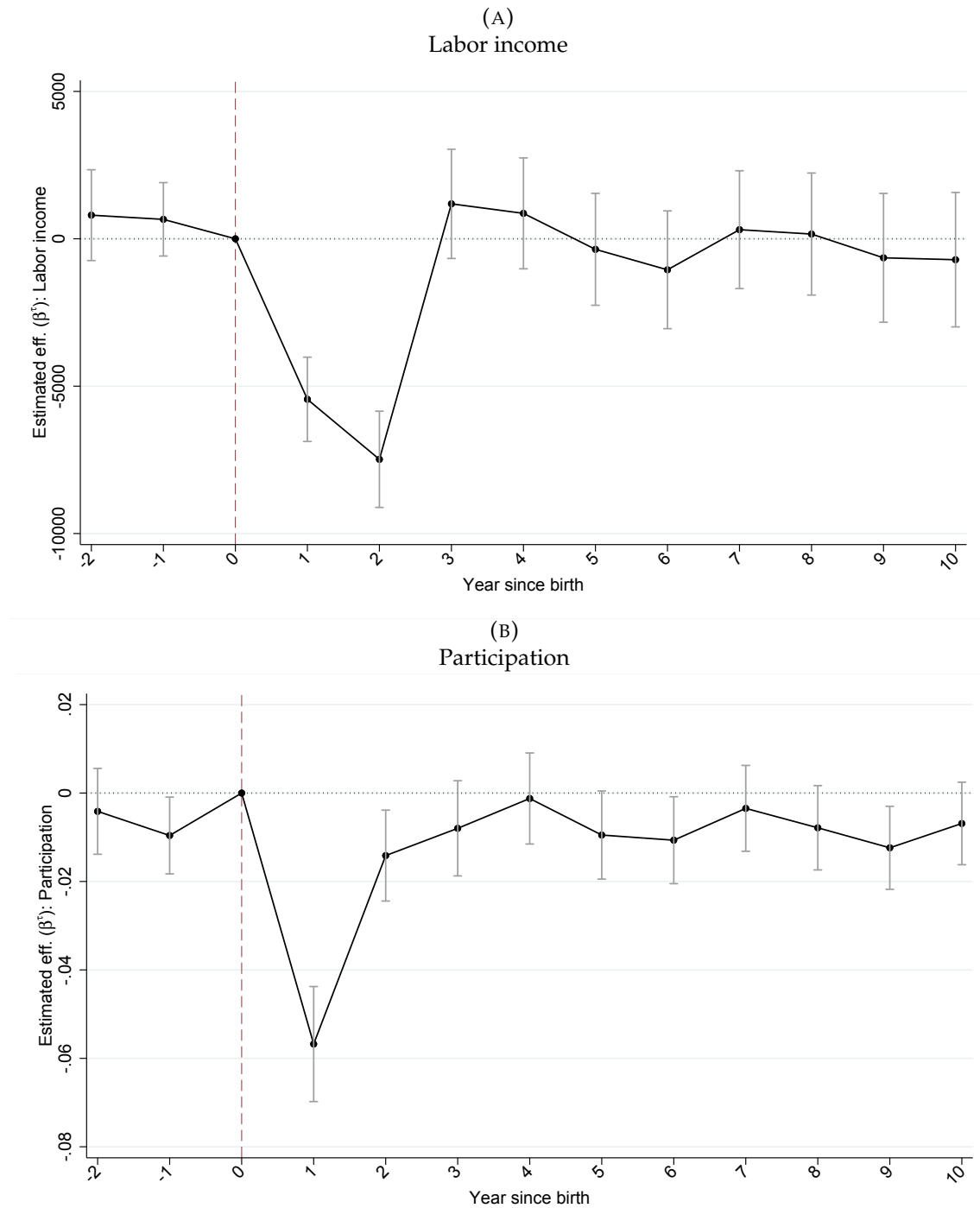
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of having a child born in 1988 and calendar month of birth, $\hat{\beta}^m$, from Equation 1, i.e., the difference in outcomes between women who gave birth in calendar month $m = Jan, \dots,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

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	0.857	0.861	1.590	0.867	0.875	4.258	0.005	1.687		
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		
Joint χ^2									9.020	
<i>p</i> -Value									0.341	
Observations	56,423	19,918		60,147	21,322			157,810		

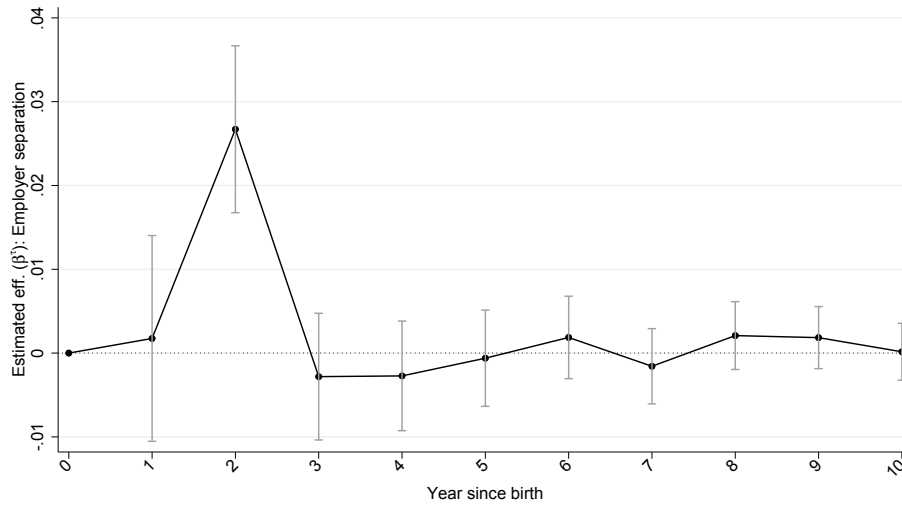
NOTES: The sample includes women who gave birth in 1987 and 1988, who earned at least SEK 10,000 in the calendar year prior to birth, and who did not give birth in the months of August or September.

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



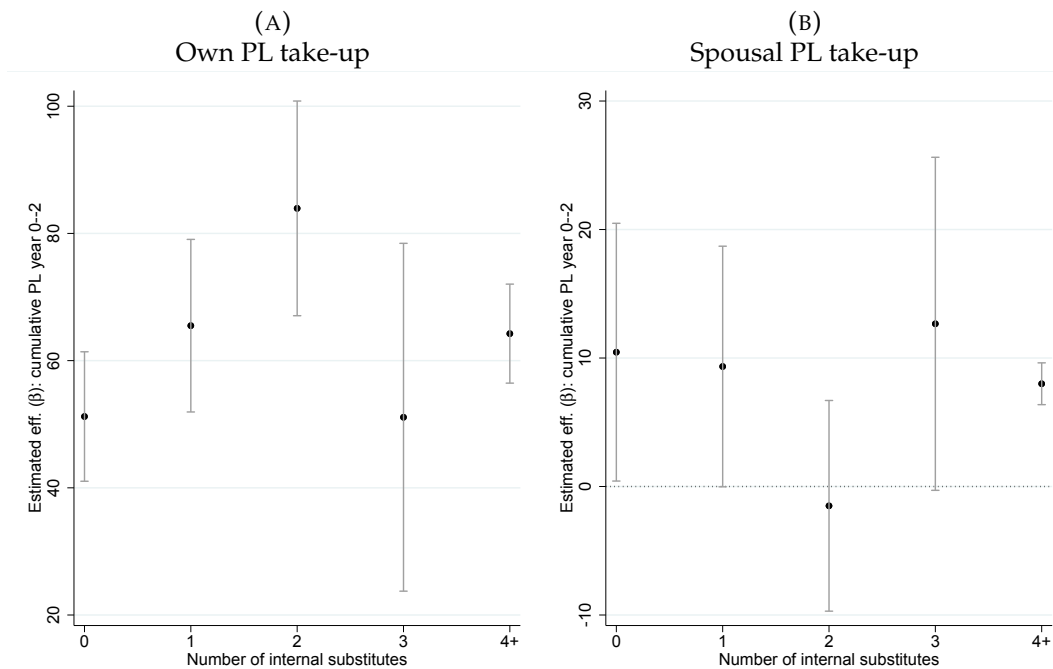
NOTE: Each point in the graphs shows the estimated $\hat{\beta}^\tau$ from equation (3) and the corresponding 95% confidence intervals.

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



NOTE: Each point in the graph shows the estimated coefficients $\hat{\beta}^\tau$, from equation (3), along with the 95% confidence intervals.

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



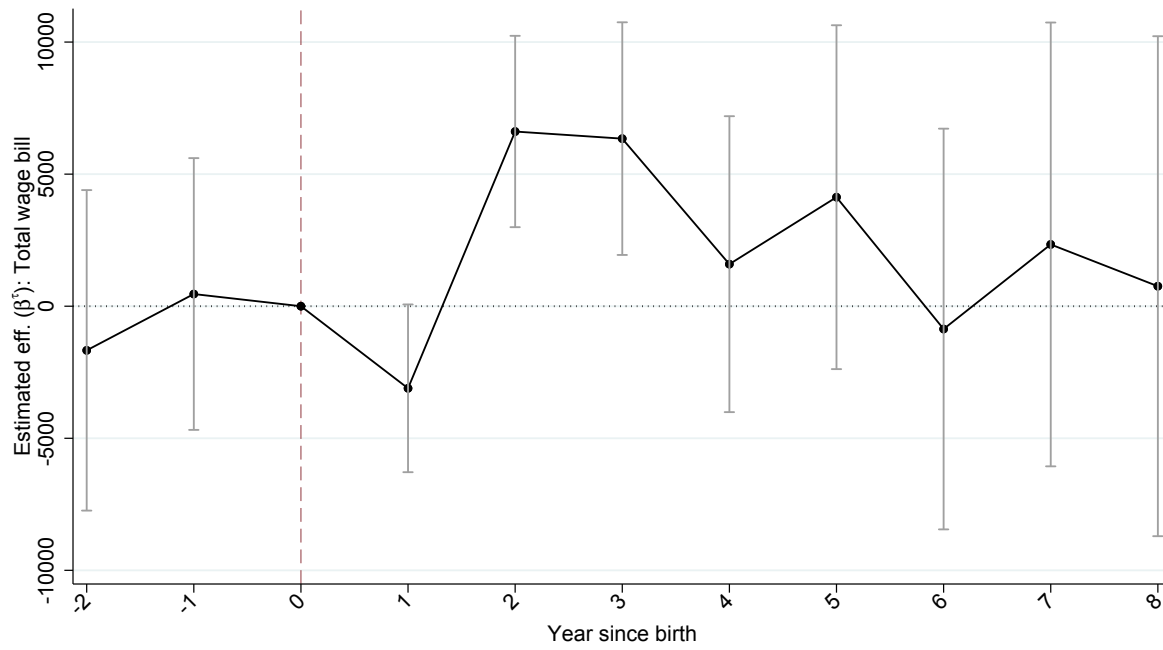
NOTE: The sample includes couples whose first child was born in 1988 or 1989; the outcome measures the total number of leave days over child ages 0-2. Each point in the graph depicts the effect of the reform on own and spousal take-up for by the numbers of occupational substitutes of the woman (the graph plots the $\hat{\beta}$ from equation (2)).

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)
Tradable industry	0.265	0.441	0.251	0.434
Share female	0.376	0.279	0.500	0.261
Number of births	0.550	1.287	1.423	0.968
Share compulsory schooling	0.419	0.208	0.416	0.211
Share with high school	0.479	0.167	0.469	0.161
Share workers with some college	0.057	0.087	0.059	0.088
Share workers with college	0.045	0.102	0.056	0.112
Workplace size	38.273	52.492	49.150	57.191
Average age	35.771	5.866	35.424	5.808
Average contracted working hours	0.952	0.079	0.957	0.067
Female contracted work hours	0.905	0.127	0.919	0.109
Male contracted work hours	0.984	0.042	0.983	0.037
Average monthly wage (SEK)	20,379.255	4,333.878	19,840.332	4,259.369
Female monthly wage (SEK)	17,813.571	2,875.786	17,448.251	2,788.589
Male monthly wage (SEK)	22,519.651	5,567.610	22,214.741	5,580.741
Female annual income (SEK)	130,030.496	56,745.443	125,297.426	50,804.566
Male annual income (SEK)	190,823.995	89,085.239	192,548.684	96,117.094

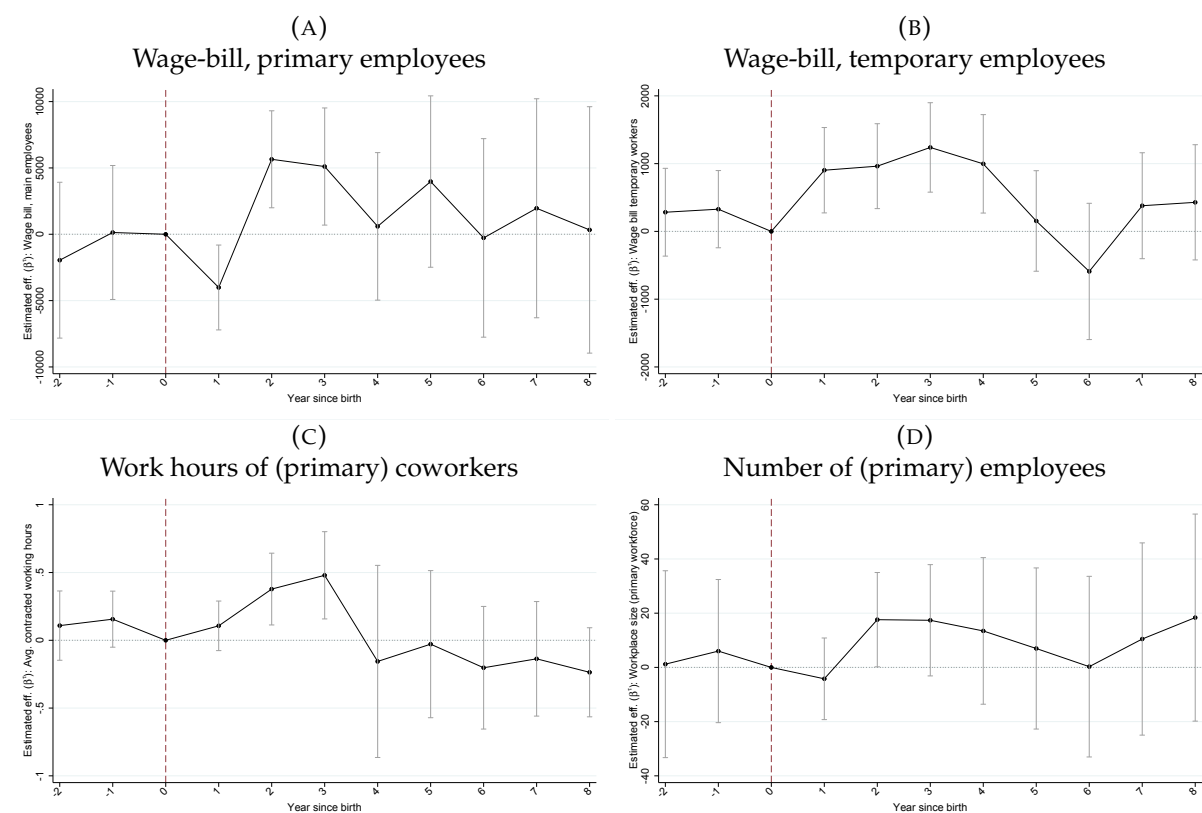
NOTES: Columns (1) and (2) report the means and standard deviations, respectively, for all private sector firms active in Sweden in 1988, and the characteristics are measured in 1988. Columns (3) and (4) report the means and standard deviations of characteristics for the workplaces in our sample, which consists of establishments that employ at least one woman in 1988 (treatment year) or 1987 (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.

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



NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity π_j , i.e., the $\hat{\beta}^\tau$ from Equation 5, along with the 95% confidence intervals.

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



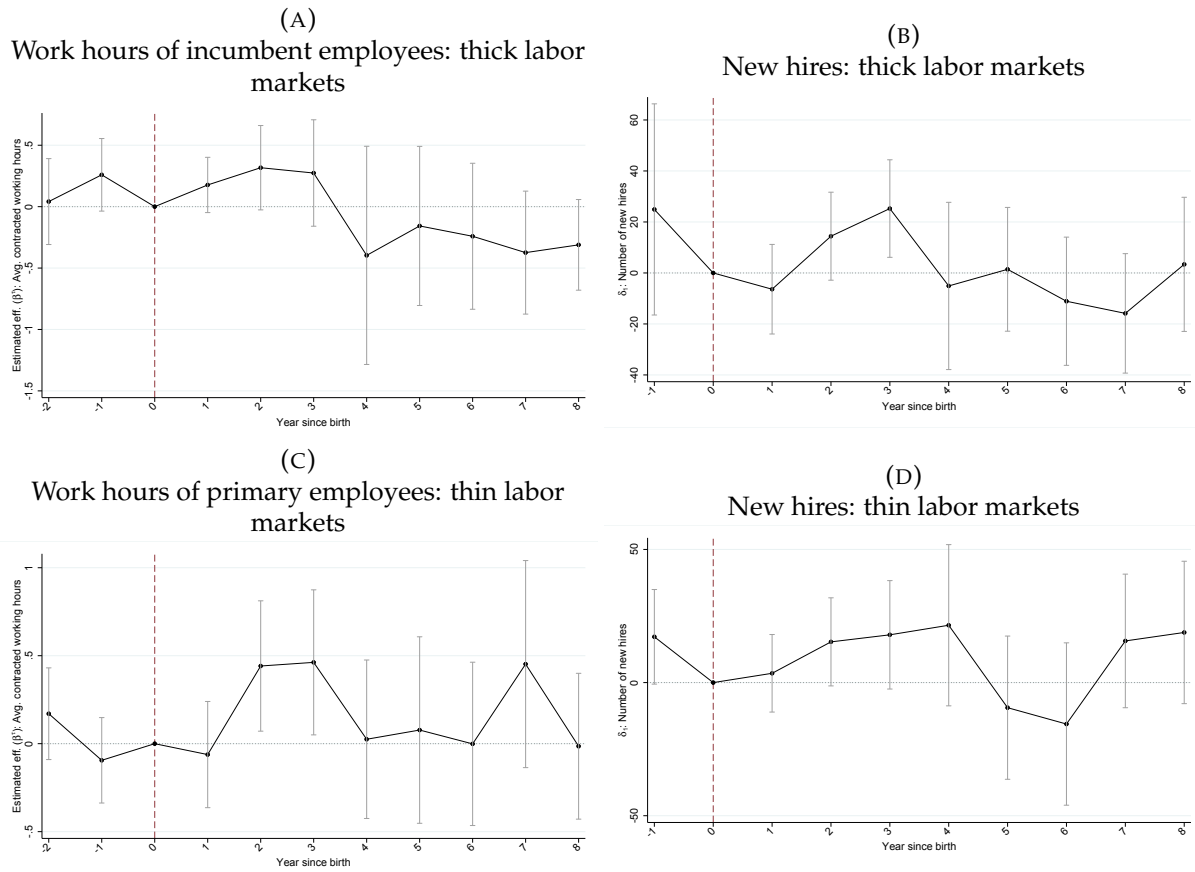
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity π_j , i.e., the $\hat{\beta}^\tau$, from Equation 5, along with the 95% confidence intervals.

FIGURE 8.
Effects of the reform on manufacturing firms' sales revenue and value added



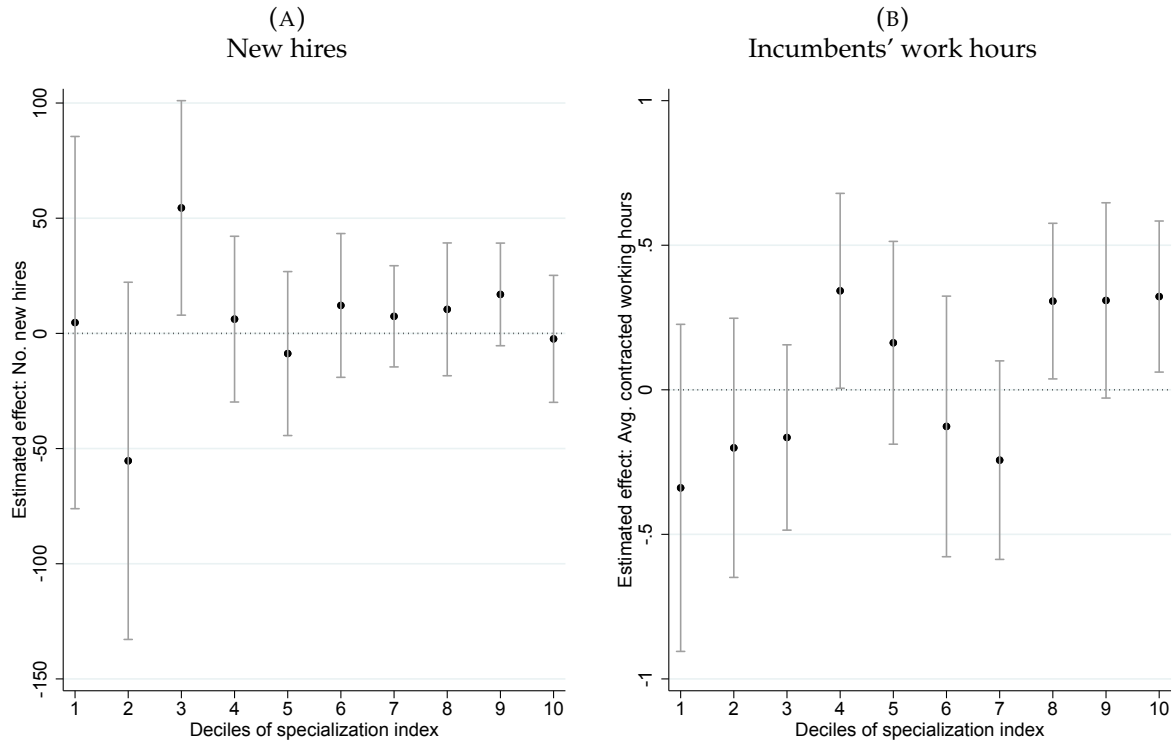
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity π_j , i.e., $\hat{\beta}^\tau$, from Equation 5, along with the 95% confidence intervals.

FIGURE 9.
Heterogeneous employer responses by external labor market conditions



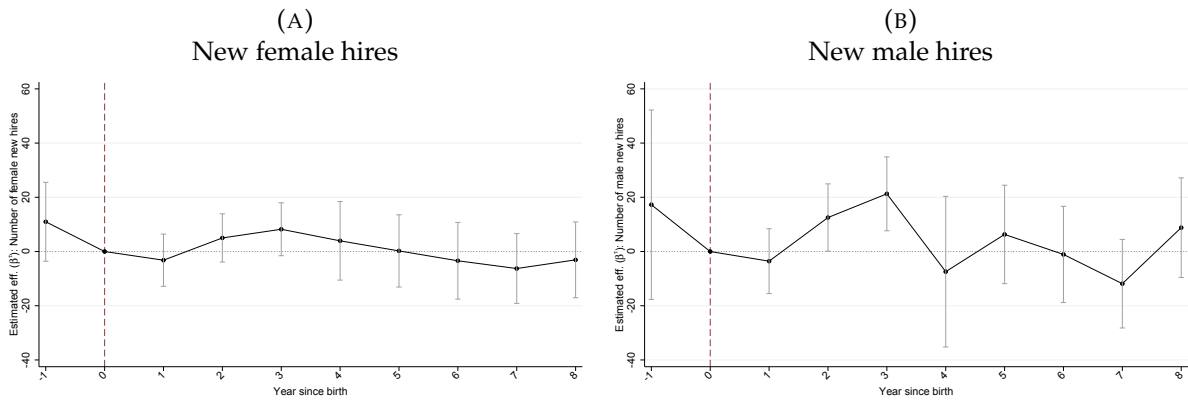
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity π_j , i.e., $\hat{\beta}^\tau$, from Equation 5, along with the 95% confidence intervals.

FIGURE 10.
New hires and incumbents' hours by overall workplace occupational specialization



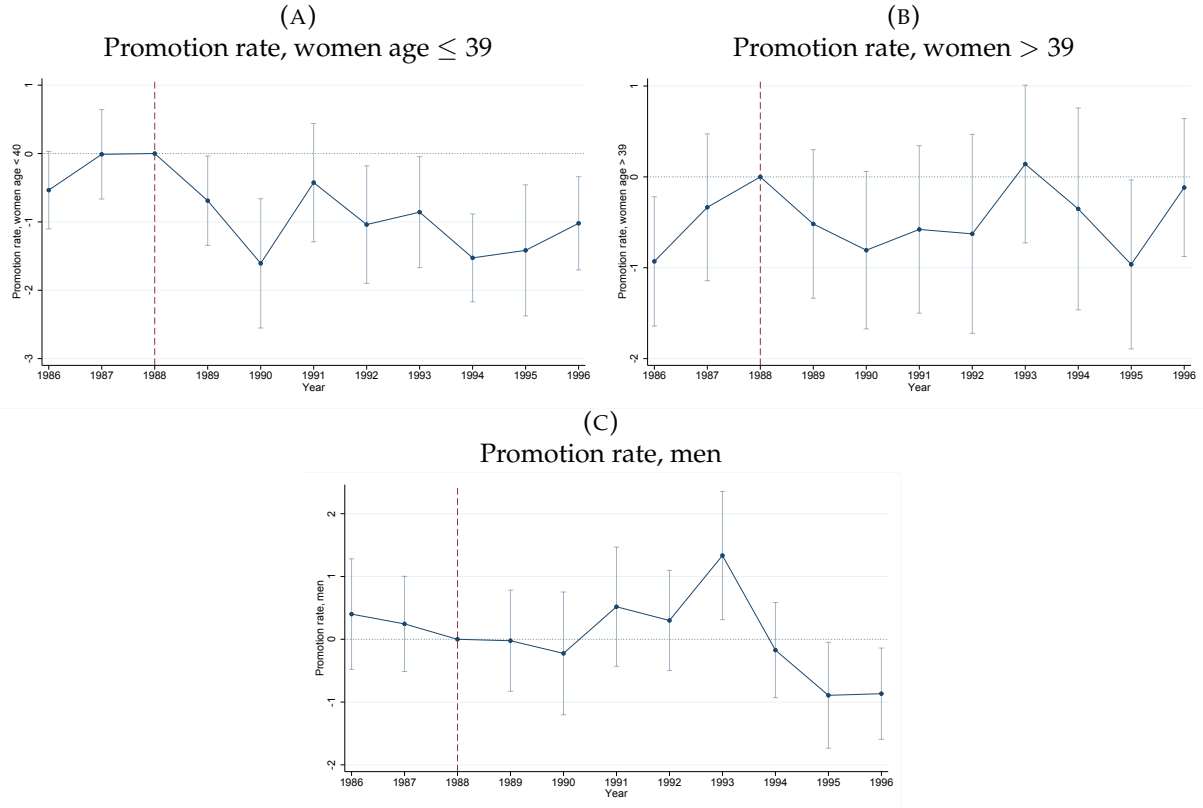
NOTE: Each point in the graphs shows the estimated coefficient on the interaction term between the indicator of employing women giving birth in 1988 and the treatment intensity π_j , $\hat{\beta}$, from a static version of Equation 5, i.e., the difference in outcomes of firms with a higher fraction of its workforce giving birth in October-December 1988 and corresponding months in 1987, along with the 95% confidence intervals.

FIGURE 11.
Gender composition of new hires: firms in thick local labor markets



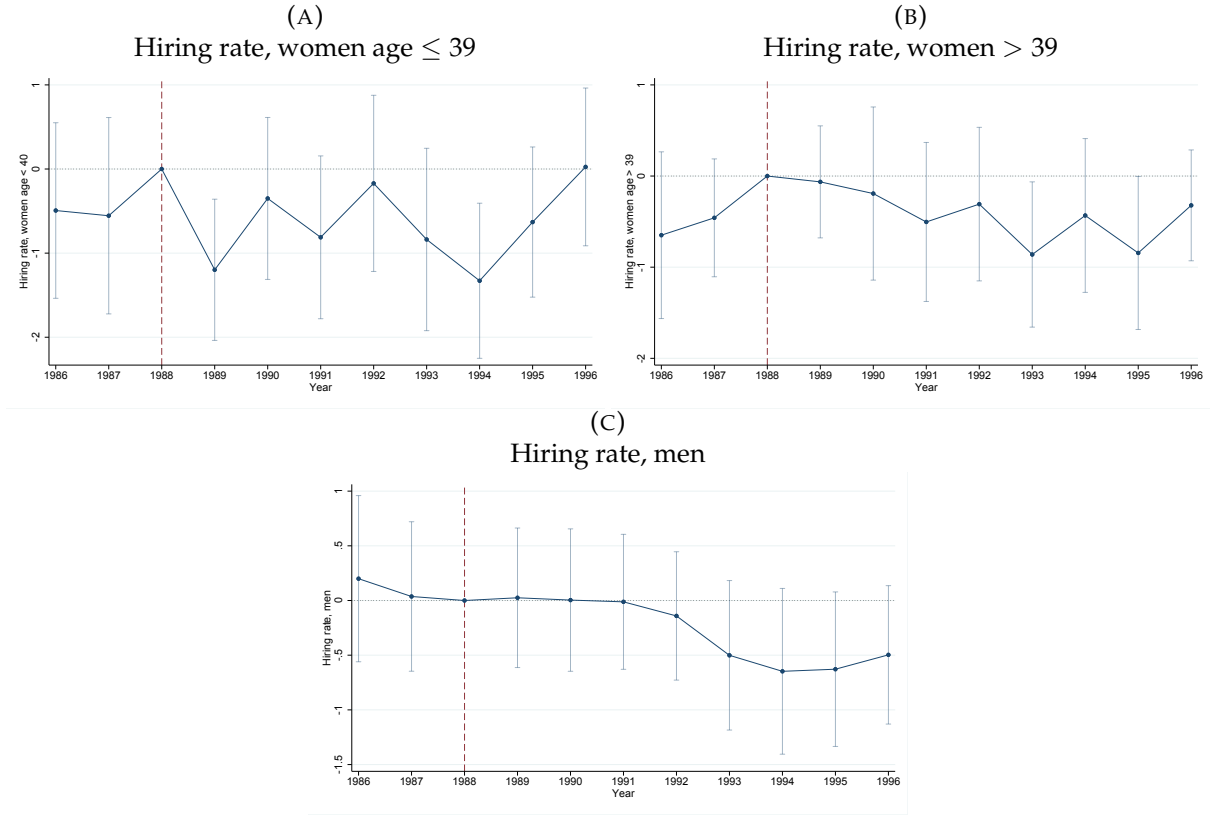
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity π_j , i.e., $\hat{\beta}^\tau$, from Equation 5, along with the 95% confidence intervals.

FIGURE 12.
Effect of predicted reform exposure on promotion rates



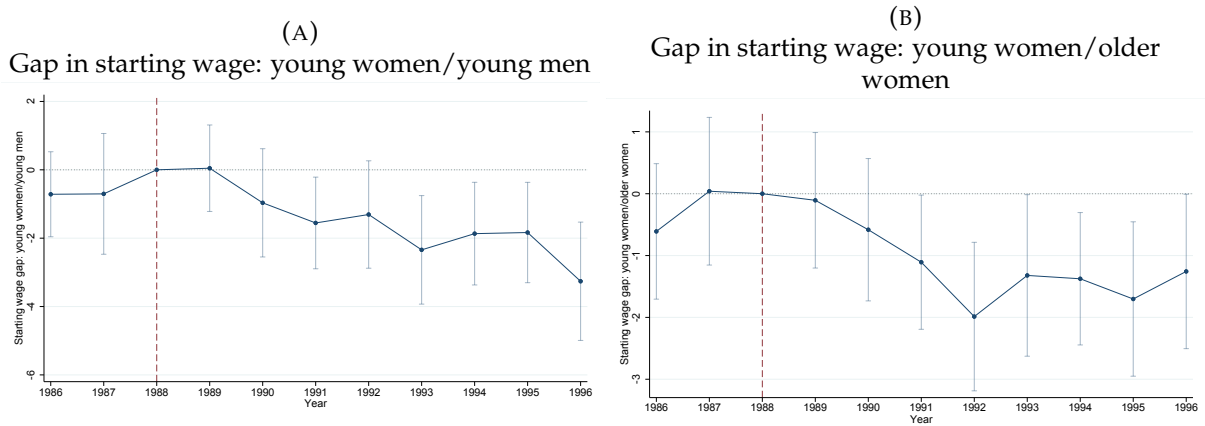
NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in industry k and region c , $\pi_{c,k'}^p$ before and after its implementation, that is, the estimates for γ_1^t 's in Equation 7, along with the 95% confidence intervals.

FIGURE 13.
Effect of predicted reform exposure on hiring rates



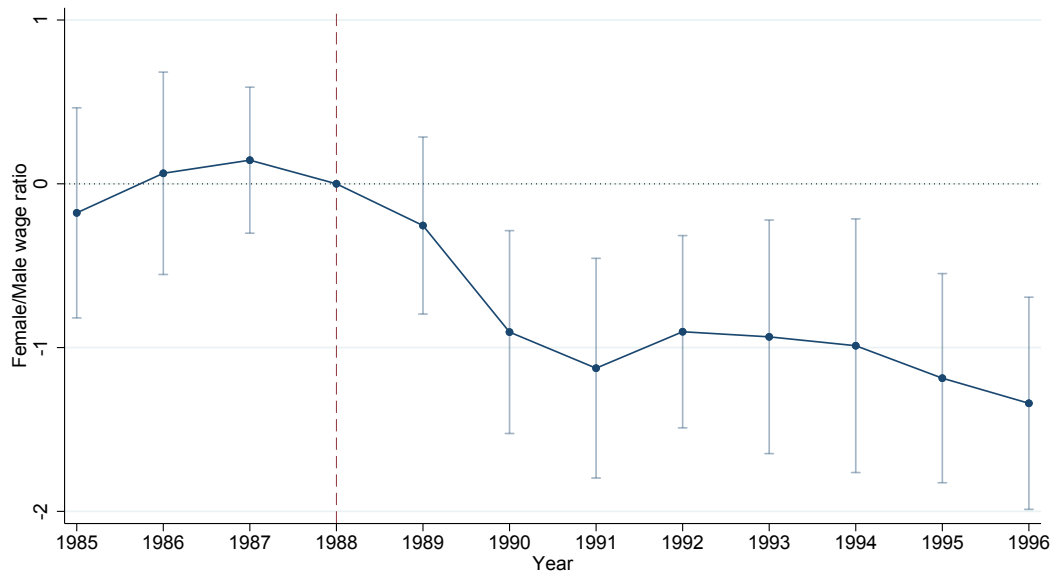
NOTE: Each point in the graph shows the estimated coefficients on the interaction terms between the predicted exposure to the reform in industry k and region c , $\pi_{c,k}^p$, before and after its implementation, that is, the estimates for γ_1^t 's in Equation 7, along with the 95% confidence intervals.

FIGURE 14.
Effect of predicted reform exposure on starting wages of new hires



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.

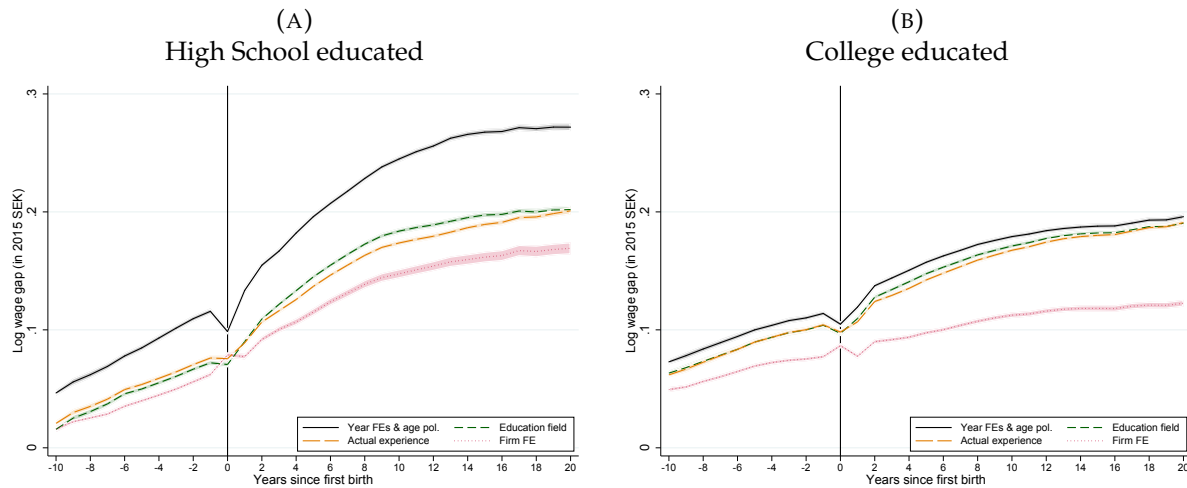
FIGURE 15.
Changes in the overall gender wage gap



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.

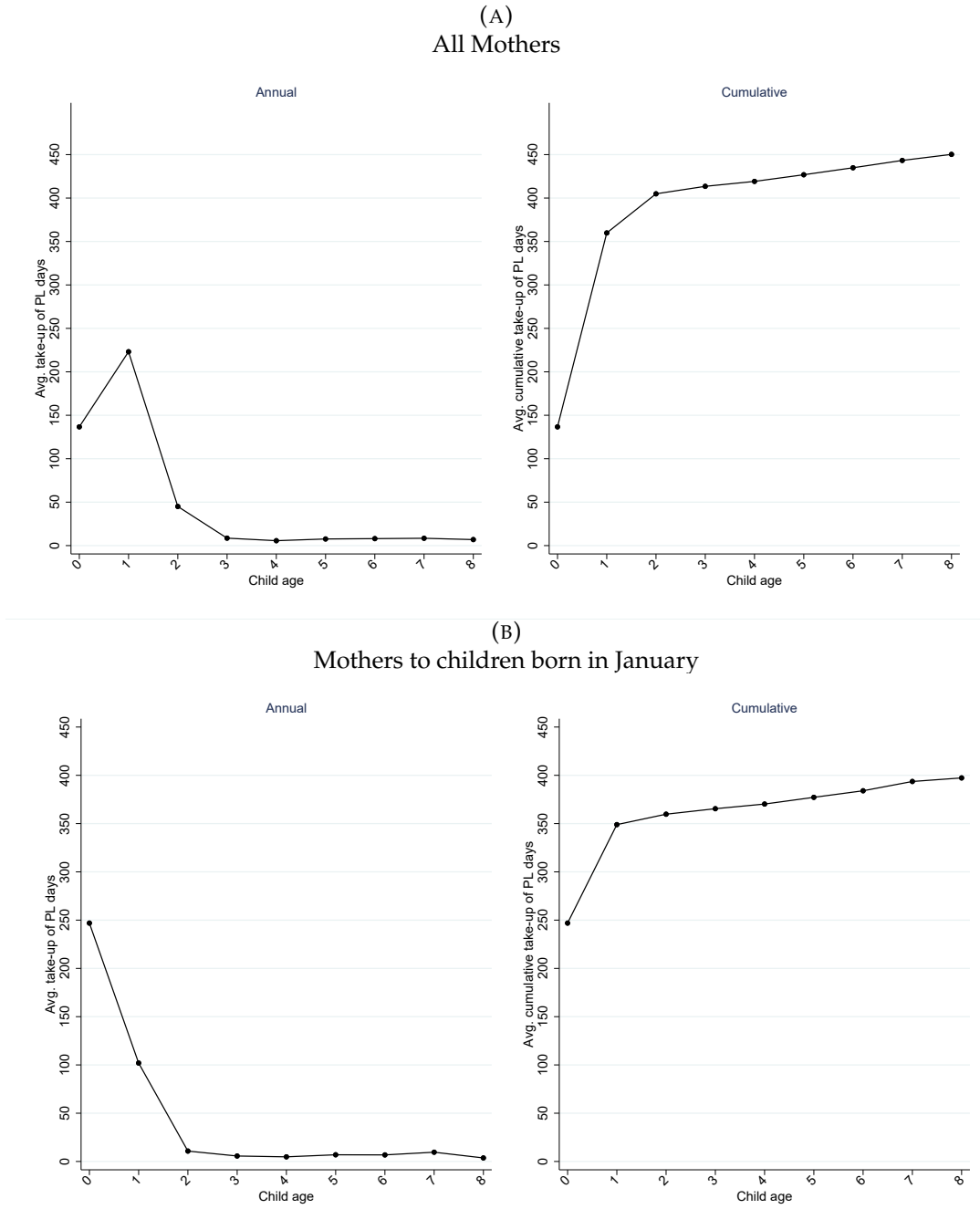
A Additional Tables and Figures (For Online Publication)

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



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

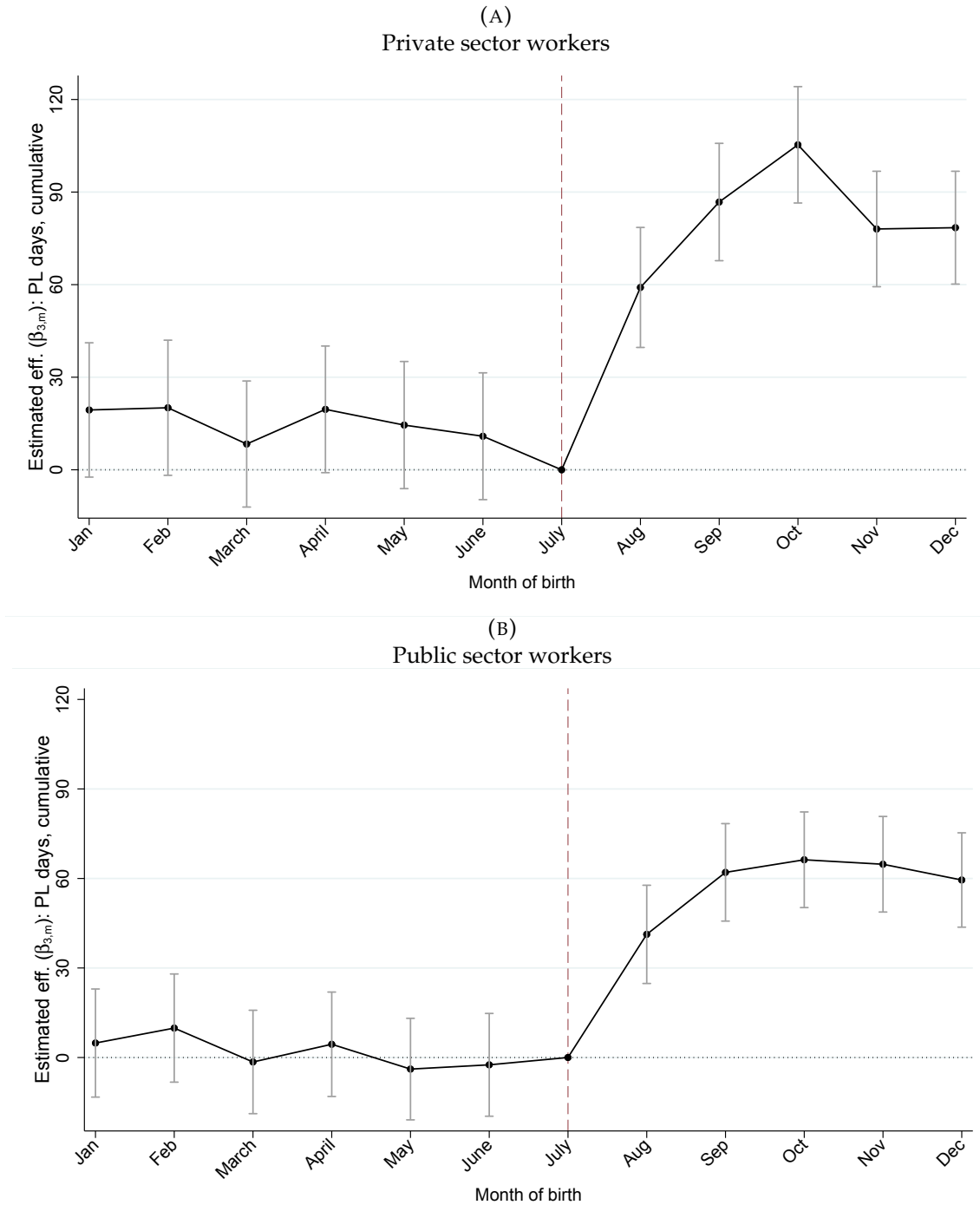
FIGURE A.2.
Women's parental leave take-up by child age



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

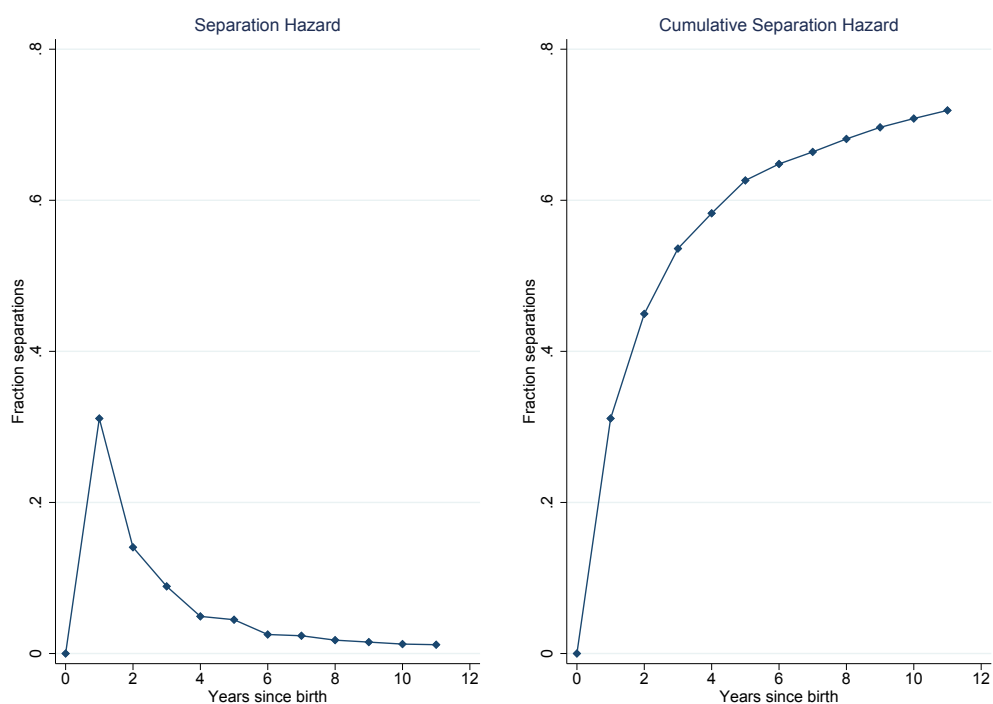
FIGURE A.3.

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, $\beta_{3,m}$, from equation (1), with the corresponding 95% confidence intervals. No control variables are included in the estimation.

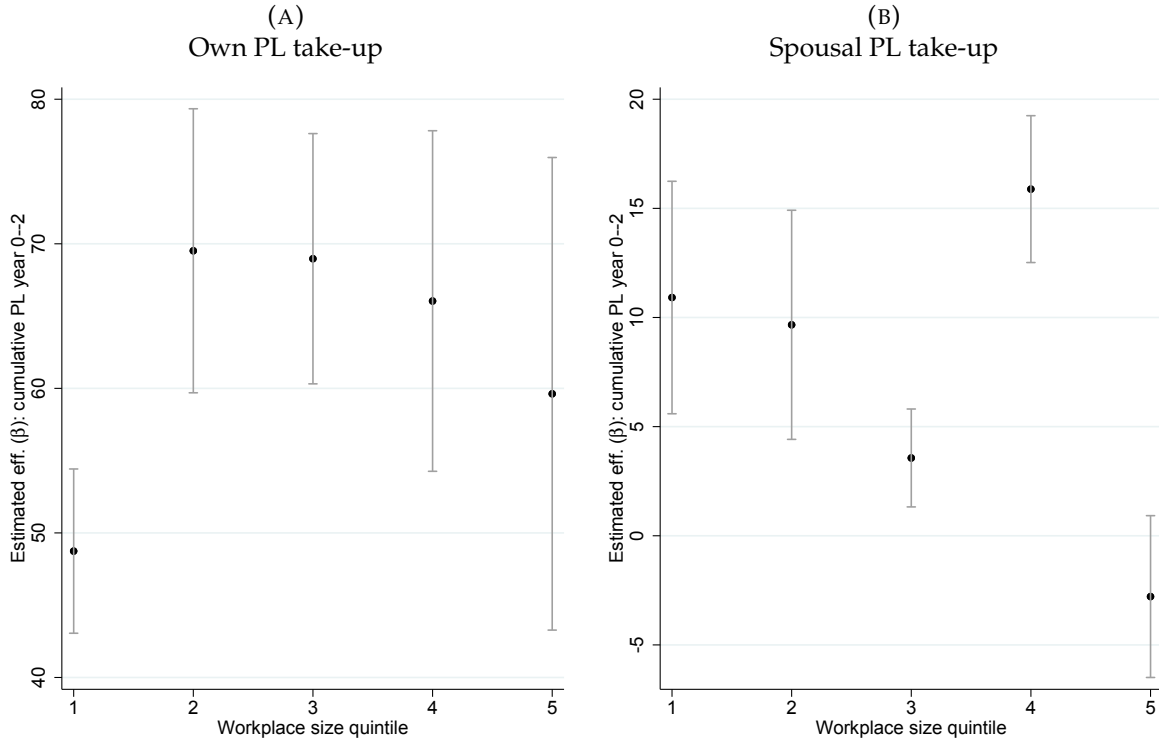
FIGURE A.4.
Baseline separation rate from pre-birth employer by time since birth



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

FIGURE A.5.

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).

TABLE A.1.
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)
N	141,145	42,924	88,126

NOTES: The sample includes women who gave birth in 1987 and 1988, who earned at least SEK 10,000 in the calendar year prior to birth, and who did not give birth in the months of August or September. The outcome variable measures the total number of children born to a person by year 2007.

TABLE A.2.
Industry mix for all private sector firms & organizations active in Sweden, and for firms in study sample

	All workplaces		Sample workplaces	
	# of workplaces	% workplaces	# of workplaces	% workplaces
Armed forces	1,597	4.078	674	4.678
Agriculture, hunting, forestry	671	1.714	203	1.409
Fishing	14	0.036	1	0.007
Mining and quarrying	139	0.355	32	0.222
Manufacturing	9,306	23.765	3,321	23.050
Electricity, gas and water	265	0.677	48	0.333
Construction	4,017	10.258	423	2.936
Wholesale and retail trade etc	10,445	26.673	4,231	29.366
Hotels and restaurants	2,230	5.695	1,088	7.551
Transport and communications	2,187	5.585	572	3.970
Financial intermediation	1,345	3.435	684	4.747
Real estate, renting, other	730	1.864	243	1.687
Data management operations	509	1.300	209	1.451
R&D	91	0.232	40	0.278
Other business activities	2,714	6.931	1,172	8.134
Public adm, defense, social ins	34	0.087	20	0.139
Education	774	1.977	386	2.679
Health and social work	642	1.639	355	2.464
Lobbying, and religious act	592	1.512	301	2.089
Recreation, culture, sports	857	2.189	405	2.811
Total	39,159	100	14,408	100

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

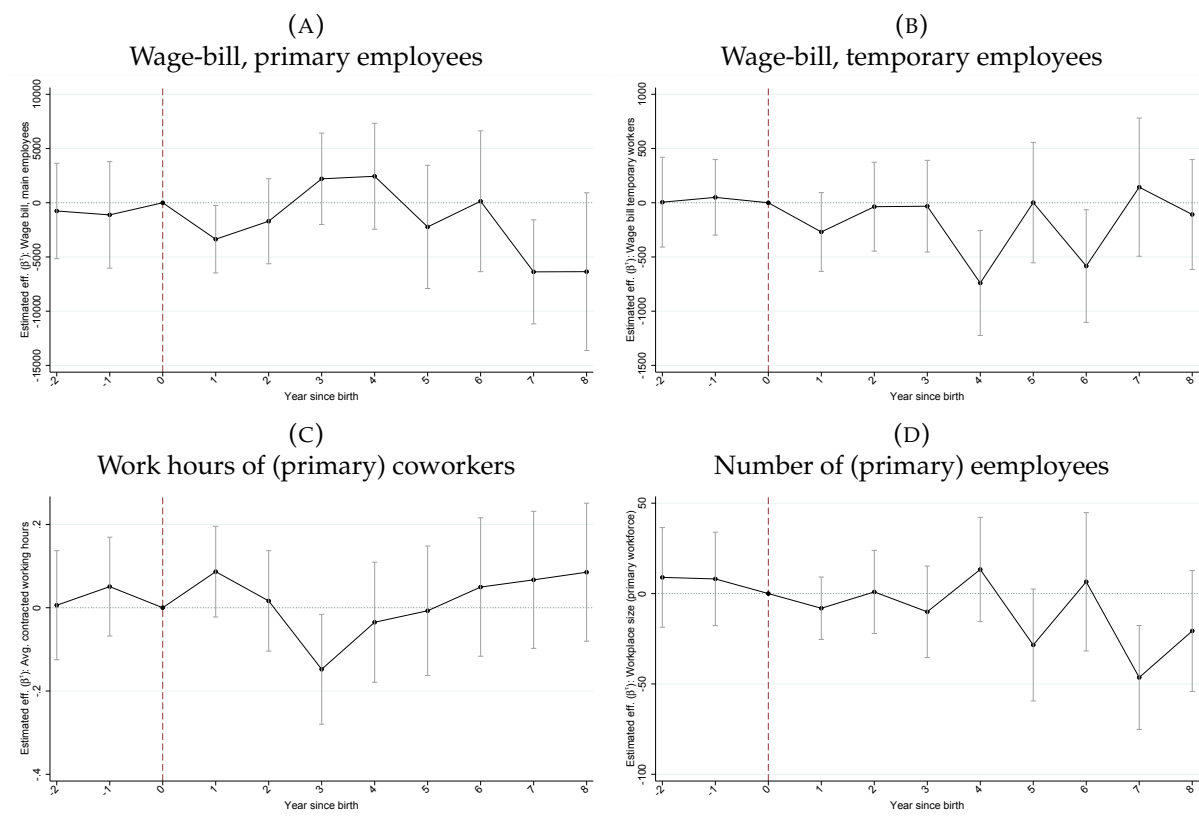
TABLE A.3.

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	14,329	14,245	-0.598	14,113	14,232	0.892	0.203	1.048
Number of female workers	8,265	8,198	-0.379	8,059	8,179	0.719	0.187	0.770
Number of male workers	6,065	6,048	-0.096	6,054	6,053	-0.007	0.016	0.065
Number women aged 20-40	3,603	3,560	-0.392	3,493	3,508	0.143	0.058	0.382
Private sector	1,000	1,000	.	1,000	1,000	.	0.000	
Average age	35.299	34.636	-2.393	35.180	34.910	-0.993	0.392	1.009
Share female	0.548	0.548	0.031	0.541	0.546	0.406	0.004	0.258
Private sector	1,000	1,000	.	1,000	1,000	.	0.000	
Avg wage	19,000	19,000	0.479	19,000	19,000	-0.762	-373.664	-0.872
Female work hours	0.924	0.933	0.957	0.928	0.927	-0.100	-0.010	-0.768
Male work hours	0.983	0.983	0.225	0.983	0.980	-1.055	-0.004	-0.896
Wage bill primary workers, 1000s SEK	2100.000	2100.000	0.671	2100.000	2100.000	-0.340	-49.946	-0.716
Wage bill temporary workers, 1000s SEK	188.094	182.024	-0.395	225.825	196.467	-1.001	-23.288	-0.683
Share no college	0.888	0.879	-1.113	0.876	0.887	1.497	0.020	1.842
Share college	0.112	0.121	1.113	0.124	0.113	-1.497	-0.020	-1.842
Observations	1,795	693		2,056	738		5,282	

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

FIGURE A.6.
Decomposing employer responses: primary workers' hours increases or temporary replacement workers? Public sector



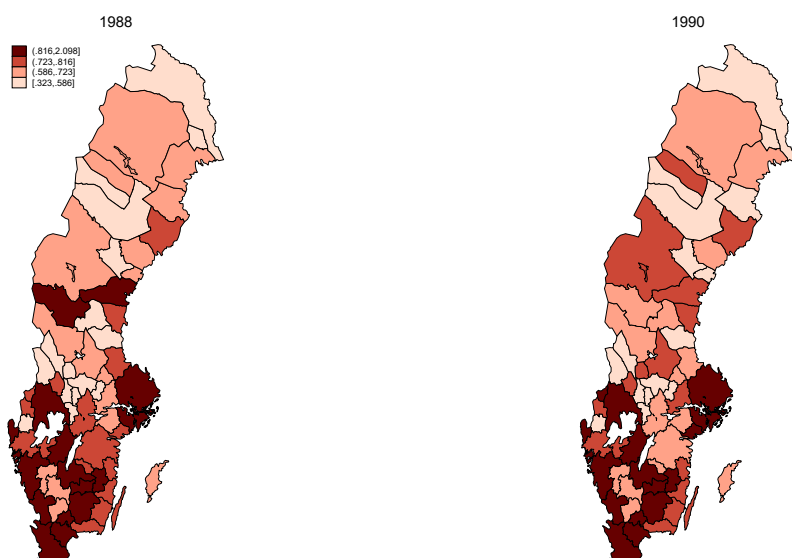
NOTE: Each point in the graphs shows the estimated coefficients on the interaction terms between the indicator of employing women giving birth in 1988 and the treatment intensity π_j , i.e., $\hat{\beta}^\tau$, from Equation 5, with the corresponding 95% confidence intervals.

TABLE A.4.
Summary statistics for the subset of firms with observations on sales revenue and value added measures

	Mean	Standard deviation
Tradable industry	0.968	0.177
Share female	0.353	0.218
Number of births	1.190	1.629
Share compulsory schooling	0.465	0.171
Share with high school	0.453	0.134
Share workers with some college	0.059	0.067
Share workers with college	0.023	0.047
Workplace size	66.088	64.336
Average age	37.155	4.795
Average contracted working hours	0.945	0.067
Female contracted work hours	0.884	0.115
Male contracted work hours	0.983	0.035
Average monthly wage (SEK)	21,269.007	3,326.018
Female monthly wage (SEK)	17,823.000	2,424.546
Male monthly wage (SEK)	23,365.147	3,954.726
Female annual income (SEK)	132,984.254	37,906.103
Male annual income (SEK)	197,141.220	49,521.140
Sales revenue per worker (1000s SEK)	1,223.554	1,231.264
Value added per worker (1000s SEK)	562.346	411.093

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.

FIGURE A.7.
Regional thickness measure for finance industry, 1988 and 1990



NOTE: The figure shows the female employment shares in the finance industry in each commuting zone in Sweden, according to the formula in (6).

FIGURE A.8.
Predicted reform exposure by commuting zone in the Manufacturing, Education, and Health industries

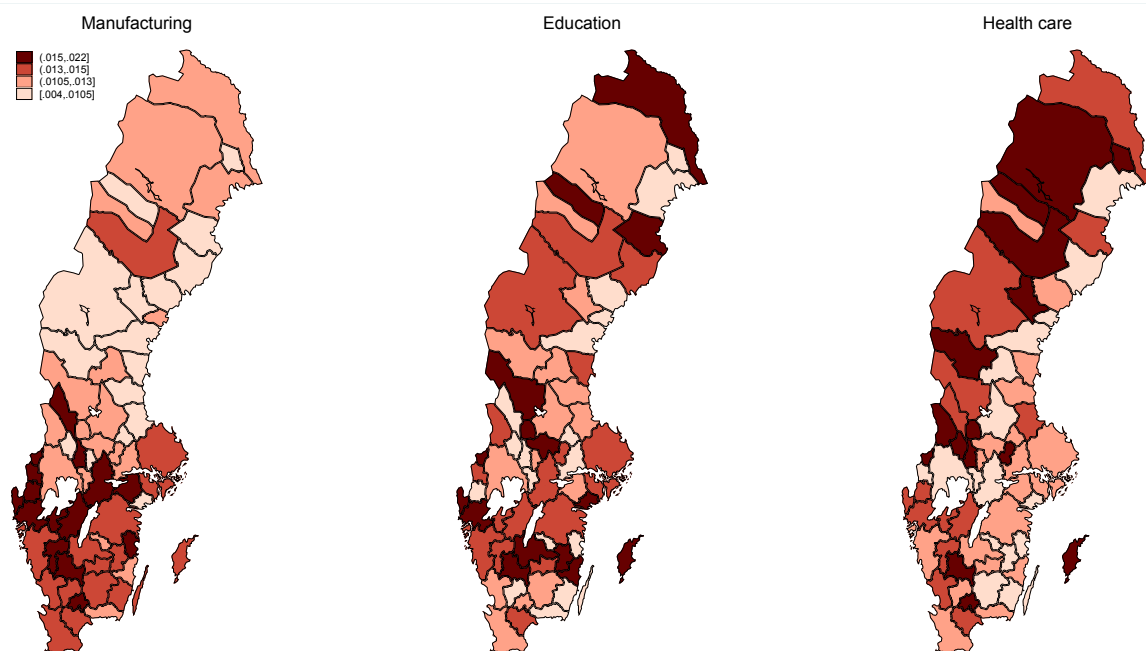
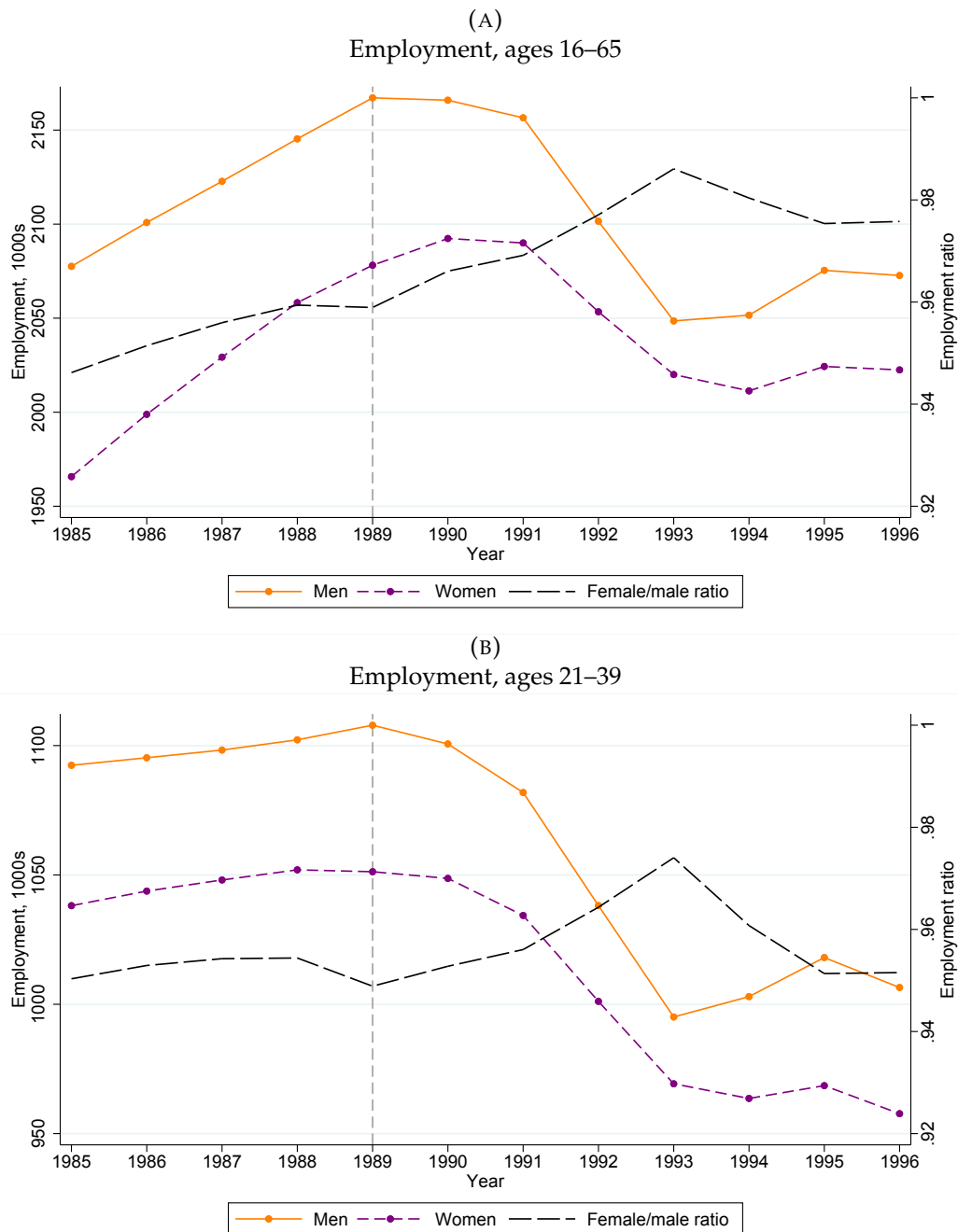
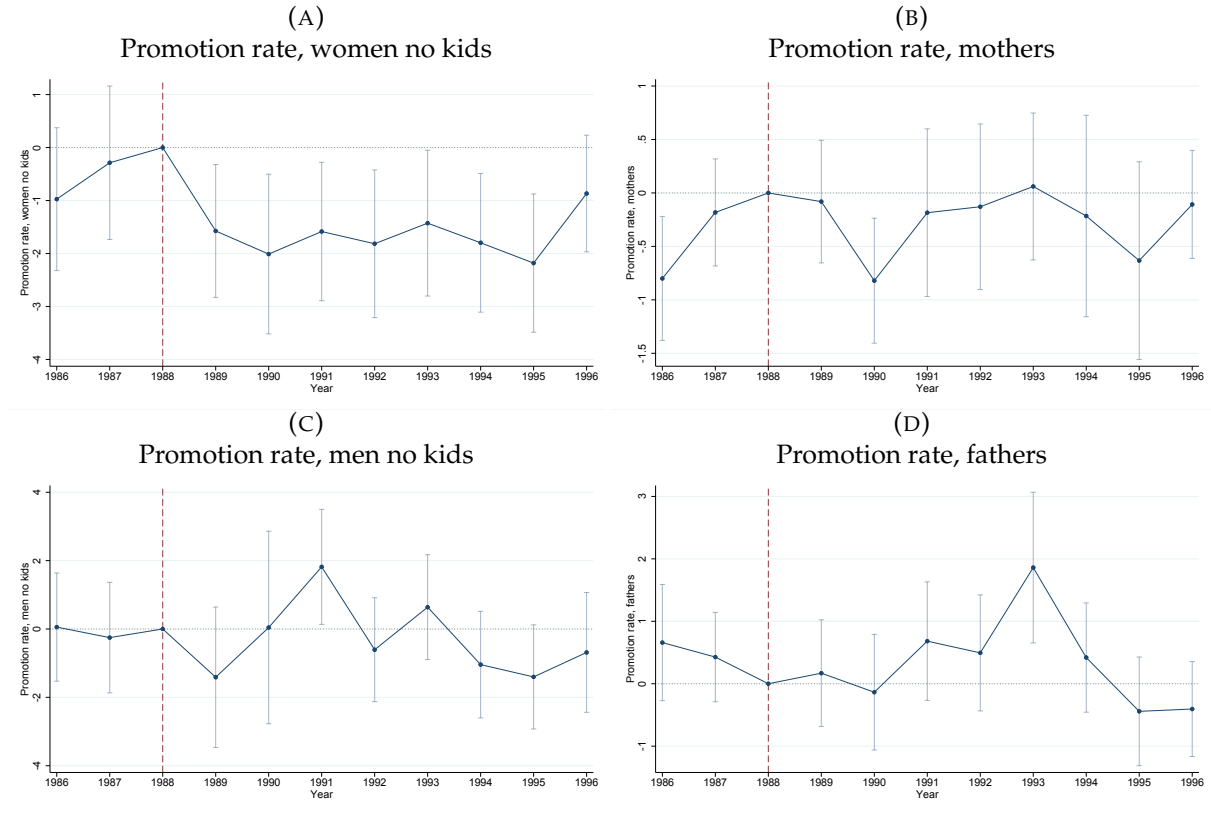


FIGURE A.9.
Nationwide employment trend by gender 1985–1996



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

FIGURE A.10.
Effects of predicted reform exposure on promotions



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.