American Economic Journal: Applied Economics 2023, 15(1): 107–135
https://doi.org/10.1257/app.20200448

Employer Responses to Family Leave Programs†

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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. 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 coworkers to make it coworkers' hours, incurring wage costs corresponding to 10 full-time equivalent months in addition to replacing the workers. These adjustment costs varied by firms' availability of internal substitutes. We also analyze a daddy-month reform and find similar employer responses to male workers' leave, albeit smaller in magnitude. (*JEL* J16, J22, J32, J64, M52)

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

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† Go to https://doi.org/10.1257/app.20200448 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

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

of frictions in such statistical discrimination (see Barron, Black, and Loewenstein 1993; Bowlus 1997), it is in practice difficult to measure the frictional costs faced by firms.

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

Our research design takes advantage of the fact that treatment assignment was unrelated to any unobserved factors that might influence worker or firm outcomes. Eligibility to the extension was based on date of birth, and thus treatment was as good as randomly assigned. Furthermore, the parental leave reform was unanticipated and retroactive: it was implemented in July 1989 but retroactively covered parents of children born in October 1988 and later. Eligible mothers could postpone their return to the workplace by three months, and firms were obligated to accommodate. The retroactive implementation ensures that workers could not manipulate their birth timing to take advantage of the new rules, and neither could firms manipulate their workforce composition to avoid workers with longer leaves. Thus, the policy intervention implied that randomly assigned firms unexpectedly and on short notice had to find replacement workers to cover for the additional leave, making it close to an ideal experiment to empirically quantify adjustment costs. We use population-wide matched employer employee data to analyze workplace-level demand for incumbent and external labor inputs, using the set of firms that had employees who had children around the reform cutoff dates.

Since employer responses depend on the extent and timing of workers' take-up of the additional leave, we first quantify the impact of the reform on individual labor supply and job mobility. Using an auxiliary dataset on parental leave spells, we show that eligible mothers took up 2.6 months on average out of the 3 months of additional leave, while the increase in male take-up was only one week on average. We document that women took the bulk of their additional leave during the first two years after birth, and show that the paid-leave expansion did not simply crowd out unpaid leave. Finally, the reform increased the probability that women leave for a different firm by 15 percent in the year when parental leave ended, which we

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

interpret as voluntary switches due to extended possibilities for job search (while on leave).

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

Our results show that private sector firms responded to the reform by hiring both primary and secondary/temporary workers, and by increasing the work hours of the coworkers (both incumbents and new hires). The net effect of these adjustments on the firm's total wage bill was positive, indicating that such reorganization came at a monetary cost. Specifically, for an average-sized workplace with 48 workers, having one additional worker going on extended leave increased the total contracted hours of her coworkers by 9 hours per week in both years 2 and 3. Employers also hired more permanent and temporary workers. For each additional worker on extended leave, the average firm increased primary hires by 0.35 and 0.62 workers in the first and second year, respectively. The total effect of these adjustments implies that having one additional worker going on extended leave increased the total wage bill by an amount corresponding to the labor cost of 10 full-time equivalent months. Note that if the adjustments were perfectly frictionless, firms would be able to replace the absent worker one for one, and there would be a zero net effect on the wage bill, so our results suggest that the adjustments are indeed costly and sizable.[3] Even with added labor inputs such as extra hours and new hires, private-sector firms did not perform better. Using data on sales and productivity for firms in the manufacturing industry, we find suggestive evidence of a decline in sales revenue, although these estimates are only marginally significant. For the public sector workplaces, there is no discernible pattern that would indicate adjustment or reorganization of the workforce.[4]

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

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

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

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

We show that the 2002 reform decreased fathers' labor supply by 0.86 months, on average, spread out over the first three years after the child was born. Using a research design similar to that for the 1989 reform, we then analyze the employers' response to the labor supply reduction. We find a statistically significant (at the 10 percent level) increase in the wage bill paid to secondary/temporary staff in the two years following the birth of the child, and an increase in the contracted hours of the coworkers (significant at the 10 percent level) in the same years. However, there are no significant effects on the firms' total wage bill, suggesting that employers adjusted merely enough to replace the temporary absence of the men on leave. In addition, we also find a significant increase in the wage rates of the remaining male workers by 1 and 1.8 percent in the first two years after the reform. While we are not able to provide conclusive evidence on the mechanism, the results are consistent with an increased demand for the remaining coworkers who have firm-specific human capital. We find no effect on female workers' wages, suggesting that male and female workers within the firm might be imperfect substitutes.

Our paper contributes to three strands of literature. We contribute to empirical work on employers' ability to find substitutes for workers who leave the firm, which depends on the degree of specificity of human capital. Similar to recent work by Jäger and Heining (2019), we test empirically for the presence of frictions by using exogenous worker exits.[5] While Jäger and Heining (2019) exploit premature worker deaths, our paper contributes to this work by exploiting exogenous variation in the *duration* of worker absence generated by a parental leave reform. A related paper,

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

Friedrich and Hackmann (2017), studies the ability of hospitals and nursing homes to replace nurses after a large expansion in parental leave entitlements in Denmark. The authors find negative impacts on patient outcomes in Danish hospitals and health centers due to the labor shortage of nurses—a female dominated occupation that is hard to replace. In contrast to much of the previous work using worker exits to assess human capital specificity, productivity, or employer outcomes, we study impacts for firms in the overall economy, as opposed to case studies of certain industries or sectors.

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

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

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

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

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

I. Background and 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 father of a child are given half of the entitled days each, but they have the option of transferring paid leave days between one another.[7]

Parental leave benefits consist of two main benefit types. First, part of the leave is replaced at a fixed daily amount. Second, the largest portion of leave transfers consists of benefits that replaces 90 percent of parents' salary, subject to a requirement of at least 240 days of employment before child birth.[8] The benefits are capped,

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

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

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

however, such that the effective replacement rate is lower for workers earning above the cap. In 1989 the share of women (with positive income) earning above the cap was only around 1.5 percent, and the corresponding share among men was around 12 percent.[9] Thus, the overwhelming majority of women were insured at 90 percent of previous earnings.

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

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

The Right to Return to Previous Job.—A worker has the legal right to return to the same job after the leave spell, where a *job* is defined as the combination of tasks and salary. If the tasks are no longer relevant when the employee returns to the workplace—due to e.g., reorganizations—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. By 1989, parents were entitled to 12 months of paid leave, of which three months were compensated at the lower flat rate of 60 krona per day. The reform that we exploit is an extension of the wage-replaced component of paid leave from 12 to 15 months that took place in 1989. The reform was implemented on July 1, 1989, but retroactively covered parents to children born in October 1988. Transition rules in the implementation implied that parents to children born in August and September 1988 received one and two additional months of paid leave, respectively.[10]

Several features of this reform make it an ideal natural experiment for the study of leave durations on both workers and firms. First, entitlement to the new rules was

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

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

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

II. Data

We use several population-wide administrative datasets 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 (Statistics Sweden 2020a, 2017). 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. We use this data together with the Firm registry, also maintained by Statistics Sweden, to obtain additional firm-level information, such as industry affiliation. 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 dataset 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. The employer-employee, and firm registries are part of Statistics Sweden's administrative labor market database "RAMS" (Statistics Sweden 2021).

For each worker/establishment/year, we merge information from the Wage Structure Statistics, an annual survey of establishments collecting information on the wages and working hours for each employee that worked at least one hour during the measuring month (Statistics Sweden 2020c). Wages are reported as full-time equivalent monthly wages, and working hours are *contracted* working hours (expressed as percent of a full-time position). The Wage Structure Statistics is a population-wide register of organizations in the public sector, and includes the universe of private sector firms with at least 500 employees. For smaller private sector firms, a random sample is drawn based on a cross-classification of industry and establishment size. All in all, roughly 50 percent of all private sector employees are covered. The earliest year for which there are firm level registers in Sweden is 1985, and we use data up to 1996. We exclude the smallest (fewer than ten employees at baseline) and the very largest (top 1 percentile of size distribution; i.e., firms with more than 265 workers at baseline) establishments from our analysis data.

III. Program Take-Up

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

We sample all women who give birth in 1988 (referred to as the treatment cohort) and all women who give birth in 1987 (control cohort). Moreover, we make use of the full reform of three additional months of benefits (ignoring the transition rules of one and two additional months to August–September parents); thus, we drop all workers who give birth in August and September. Assuming that month of birth is as good as randomly assigned, this sample restriction poses no threat to identification. In Table A.1 (online Appendix A) we show that differences in predetermined characteristics by birth month are balanced across birth cohorts.

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

$$(1) \qquad y_{it} = \delta_0 + \sum_{\tau=-2}^{8} \beta^{\tau}(T_i \times D_i \times \tau_{it}) + \sum_{\tau=-2}^{8} (\delta_1^{\tau}\tau_{it} + \delta_2^{\tau} T_i \times \tau_{it} + \delta_3^{\tau} D_i \times \tau_{it}) + \delta_4 T_i \times D_i + \delta_5 T_i + \delta_6 D_i + \mathbf{X}_i'\gamma + \epsilon_{it},$$

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

The coefficients of interest are the β^{τ}'s, which measure the difference in outcomes between women giving birth in October–December versus January–July of 1988, to the corresponding difference among women giving birth in 1987, in each year before and after birth, relative to the calendar year of birth.[13] The vector $\mathbf{X}_i'$ includes flexible controls for age, educational level measured in the year that i gave birth (compulsory schooling, high school, some college, and college degree), birth

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

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

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

parity, the age difference in months to the previous child (set to 0 if parity equals 1), and the average earnings in the two years before giving birth.[14]

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

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

The estimated coefficients $\hat{\beta}^{\tau}$ in equation (1) are presented in Figure 1. Panel A shows 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. We impute the number of months worked based on pre-birth income in panel B, and find that intensive-margin labor supply decreased by 0.6 and 0.8 months in years 1 and 2, respectively. One reason for why these magnitudes do not match up with the three-months increase in benefit entitlement could be employer-provided top-ups of benefits (which are included in the earnings measure). Indeed, using an auxiliary dataset on benefit claims (Table B.1 in the online Appendix), we show that the reform increased parental leave take-up by 2.6 months for women in the private sector. On the extensive margin, panel C shows that the probability of working at all in the calendar year was negatively affected only in year 1.

One margin that could have implications for employers is whether employees stay with the firm throughout the parental leave spell and return to their previous jobs after the leave has expired. Since leave benefits are financed through payroll taxes and paid to the claimant by the Social Insurance Agency (Försäkringskassan 2020), a worker can switch jobs while on parental leave without foregoing benefits. Extended leave duration may thus imply a longer period of job search for those women looking to leave their firm.[16]

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

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

[16] Gottlieb, Townsend, and Xu (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.

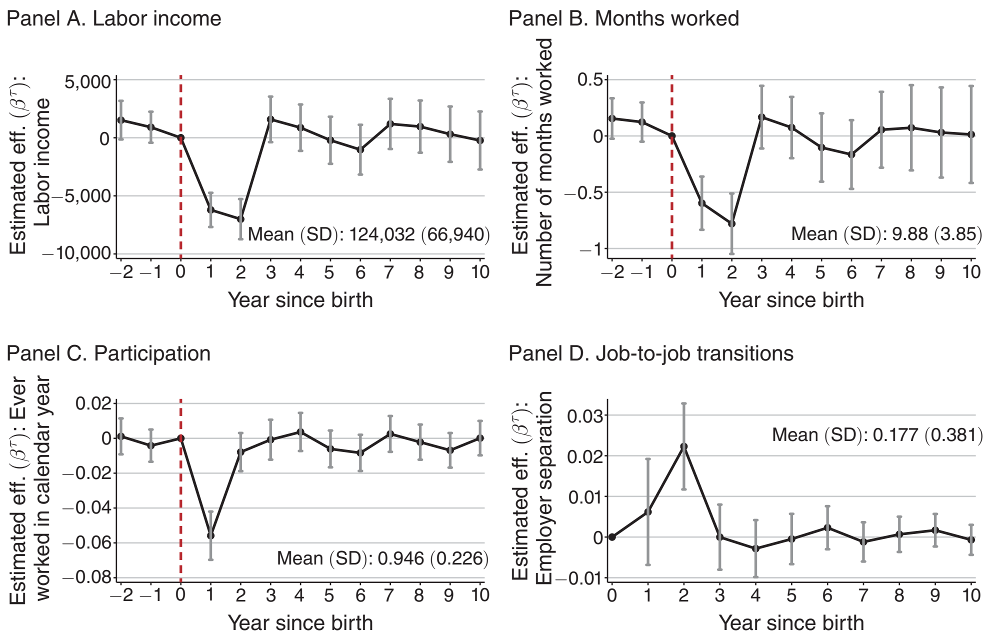

Figure 1. Effects of Extended Entitlement to Paid Leave on Female Labor Income, Participation, and Job Separations

Notes: The graph reports difference-in-differences estimates of the 1989 reform on female worker's labor supply. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for having a child in October–December (relative to January–July), an indicator for having a child born in the treatment year of 1988 (relative to 1987), and the respective event-time indicator for year since birth indicated in the *x*-axis. Thus, the points correspond to the $\hat{\beta}^\tau$ from equation (3). Ninety-five percent confidence intervals are shown by the vertical lines on each point estimate.

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

An alternative explanation is that the separations might be involuntary. However, Swedish employment protection legislation is relatively strong, so involuntary separations are arguably less likely but could result if, for example, the employee is reallocated 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.

IV. Employer Responses

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

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

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

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

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

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

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

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

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

Finally, we note that our control cohort firms could also get treated in the future—they would eventually also have employees giving birth in later years who then go on leave durations that are longer than would be in the absence of the policy changes. However, the treatment cohort firms would also have more employees giving birth in later years. There is no reason to believe that one cohort is inherently subject to higher employee child births in the future than the other cohort of firms. If the treatment cohort firms respond to the policy by hiring more women, then the long-run impact of the policy change could 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).

A. *Summary Statistics*

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

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

B. *Employer Adjustment Strategies*

To gauge overall changes in the firms' labor force, we first look at the impact of the reform on the total labor cost at the workplace—the sum of annual earnings of all workers on the firms' payroll, including women on parental leave. Since the Swedish government pays for the parental leave benefits at the replacement level

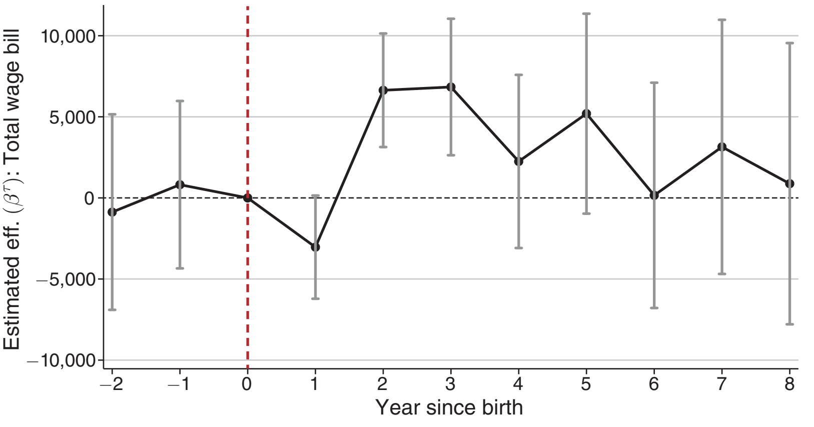

Figure 2. The Effect of the Extended Parental Leave Program on Firm's Total Wage Costs

Notes: The graph reports difference-in-differences estimates of the 1989 reform on firms' total wage bill. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the *x*-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from equation (2), along with the 95 percent confidence intervals. The outcome variable, firm's total wage bill, is expressed in 1,000s SEK. The the average firm size at baseline is 48 workers.

of 90 percent, and not all firms top up the remaining 10 percent, having workers on extended leave implies that the firm has fewer people to pay wages to in those months, if the firm does nothing to replace the women on leave.

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

Figure 2 presents the coefficients β^{τ} from specification (2) for the firm's total wage bill (which includes both primary and temporary employees), measured in 1,000s krona.[17] The results show a negative effect on the total wage bill in year one after birth. This is mainly driven by the fact that treated firms did not pay wages

[17] 1,000 kr amounts to circa US$105 or 95 euros.

for workers on leave during the additional leave months. We find an increase in the total wage bill in years two and three, where the point estimates suggest that going from 0 to 100 percent treatment intensity, the total wage bill increased by 6.6 and 6.8 million krona in years 2 and 3, respectively. Therefore, reorganization at the firm incurred a cost over and above the salary payments for the workers who go on extended leave. To get a sense of the magnitude, we evaluate the effect at an average sized firm (48 workers at baseline for the control cohort firms, see Table A.5, panel A, of the online Appendix). For each additional worker going on extended leave, the increase in the wage bill for an average firm corresponds to 1.63 and 1.69 percent of the total baseline wage bill in years 2 and 3, which amounts to the salary of 10.0 and 10.3 full-time equivalent months, respectively.[18] The adjustment costs thus appear sizable.

We note that part of this "excess wage bill" effect may be driven by the employers' top-ups of government PL benefits stipulated in collective agreements. If the firm hires exactly one full-time worker to replace the worker on leave and all else remains the same, the total wage bill of the firm would then increase by 10 percent of the income of a full-time equivalent worker for at most three months with the 10 percent top ups. This is only equivalent to 2.5 percent of the annual income of a full-time equivalent worker. However, our results show that the total wage bill increased by substantially more (84 percent of a full-time equivalent worker). There is no data on the prevalence of wage top-ups; however, even if all firms topped up the 10 percent, it can only account for a small proportion of the effect on the firm's total wage bill documented here.

In Figure 3 we decompose the effect on total wage costs into a component attributed to primary employees and to temporary/secondary employees, respectively. The total wage cost of primary employees decreased in year one after childbirth, which is likely a result of increased leave duration of eligible workers. However, there is an increase in the labor cost of primary workers in years 2 and 3 over and beyond replacing the workers on leave. The wage-bill paid to secondary workers increased immediately after the eligible workers went on extended leave, and employers kept relying on secondary/temporary workers during years 1 through 4. The increase in labor cost for secondary employees ranges from 1.5 full-time months in year 1 to 1.9 months in year 3.[19]

Changes in the total wage bill can be driven both by the number of new hires and the work hours of the workforce (both incumbents and new hires)—panels C and D of Figure 3 thus decompose the wage bill of primary workers into these two components. To measure hours supplied by the coworkers of women on leave, we calculate the average contracted work hours of all primary employees at the workplace, excluding the employees who gave birth in the baseline year. Contracted work hours

[18] Calculation: From online Table A.5, the average baseline wage bill is 8.4 million krona, and the average yearly earnings for a full-time worker is $(7{,}885/48\ =)$ 164,000 krona. For each additional worker eligible for extended leave, the treatment intensity of the firm increases by $\frac{1}{48}$, which leads to an increase in the total wage bill by $\left(6600 \times \frac{1}{48}/8{,}400\ =\right)$ 1.63 percent in year two, equivalent to $\left(6{,}600 \times \frac{1}{48}/164\ =\right)$ 0.84 full-time workers, or $(0.84 \times 12\ =)$ 10.0 full-time equivalent months.

[19] Calculation: $1{,}000 \times \frac{1}{48}/164\ =\ 0.13$ full-time workers, or 1.5 full-time months.

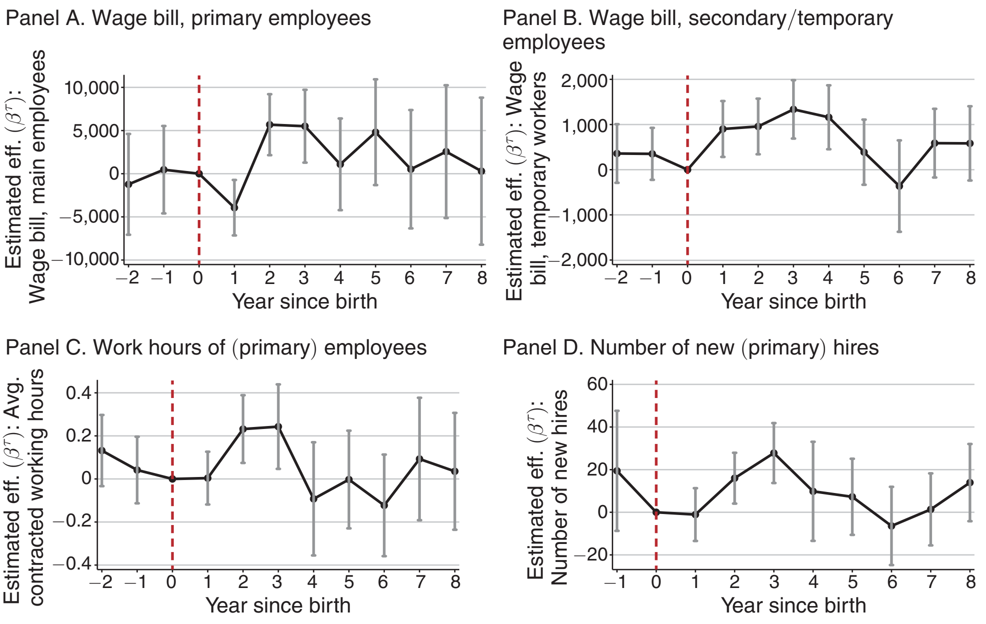

Figure 3. Decomposing Employer Responses: Primary versus Secondary Replacement Workers; Hours versus New Hires

Notes: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing women who gave birth to a child in 1988 (relative to 1987), the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the *x*-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from equation (2), along with the 95 percent confidence intervals. The firm's wage bill outcomes are expressed in 1,000s krona. Contracted work hours are expressed in percentages of full-time hours (40 hours per week). The average firm size at baseline is 48 workers.

are measured as a proportion of full-time equivalent hours.[20] Results show that for an average-sized workplace, having one additional worker going on extended leave increased the total contracted hours of her coworkers by 9.2 hours per week (or 2.8 full-time months) during both years two and three.[21] Moreover, the average firm increased the number of new primary hires by 0.35 and 0.62 workers (4.2 and 7.4 full-time equivalent months) in years 2 and 3, respectively.

Why do the employer responses last until the third year after the reform? One potential explanation could be that the observed separations in year 2 induce firms to hire new workers or increase their incumbents' hours to replace these permanent exits, which would arguably show up in years 2 and 3. Another explanation is that there is a wide distribution of parental leave lengths and women spread out their leave over two or three years (even before the reform), and we cannot

[20] For example, 0.75 means that the person works 75 percent of full-time hours, i.e., $(0.75 \times 40 =)$ 30 hours per week.

[21] Calculation: The point estimate is 0.23 in year 2 and 3. So when treatment intensity increases by $\frac{1}{48}$, the total increase in contracted hours at the workplace level is $0.23 \times \frac{1}{48} \times 48 = 23\%$, which is $(23 \text{ percent} \times 40 =)$ 9.2 hours per week.

identify the compliers in this setting without more detailed data on paid and unpaid leave. Finally, employment protection may also have played a role, since a temporary worker hired for 12 months or more would have to be made a permanent worker.

In Figure A.1 of the Appendix, we display the estimates for treatment- and control-cohort firms, respectively, to illustrate the trends in the outcome variables for the different samples. We note that all outcomes are driven by the adjustments of treatment-cohort firms in response to the reform. It is also important to net out the mechanical seasonality effects in calendar year outcomes by using the control cohort.[22]

C. *Heterogeneity by Firm Size*

A worker's absence might constitute a substantial labor loss especially in small firms. In Figure A.2 of the Appendix, we show heterogeneous effects by firm size. We define a small firm to those with fewer than the median number of employees in our sample of private sector firms. In the regressions we include the same set of control variables as in our main analysis, but define new indicators of (within-group) firm size decile interacted with the number of employees giving birth in the baseline year. We find that the effects seem to be driven by the set of smaller firms.

Limited Responses in the Public Sector.—While our main focus is on private sector employers, we report the corresponding set of results for establishments in the public sector in Figure A.3 of the Appendix. Like the private sector there is a drop in the salary payments to primary workers in year one, but unlike the private sector there are no effects on the wage bill beyond that first year. Thus, if public sector workplaces were reorganizing, they did so only to offset the labor supply reduction. However, there are no discernible patterns of adjustments in terms of secondary workers' wage bill or coworker hours. Given that individual-level program take-up were both quantitatively and qualitatively similar, the heterogeneity in employer adjustment by sector of employment is not likely driven by heterogeneity in the size of the labor supply shock caused by the reform. An alternative explanation is that the public sector—to a large extent comprised by schools and hospitals—is financed based on politically fixed budgets, leaving smaller room for replacing staff.[23]

E. *Effects on Firm Performance*

Even though we show that private sector firms reorganized their workforce and added labor inputs (extra hours and new hires), it does not immediately imply that

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

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

these adjustments were enough to maintain previous firm productivity. For example, if the new hires and overtime hours are less productive than the workers on extended leave, then the labor adjustments might only serve to ameliorate the negative impacts of worker absence but not completely offset them.

For a subset of the firms in our sample, namely firms in the manufacturing industry, we have information on firm productivity measures such as sales revenue and value added (Statistics Sweden 2020b). These manufacturing firms constitute roughly 23 percent of our sample of firms (see Table A.6 of the Appendix for summary statistics of this subset of firms). Compared to our full sample, the manufacturing firms have lower shares of female workers, fewer employees giving birth in a given year, higher average wage, and a larger workforce. Similar to firms in our main sample, firms in the manufacturing sector also responded to the reform by increasing labor inputs, as their wage bills paid to both primary and secondary workers increased in years two and three (see Figure A.4 in the Appendix). However, the smaller sample size of the performance measures implies that the effects of the reform on log total sales and log total value added are imprecisely estimated for these manufacturing firm (see Figure A.5).

Taken together, our analyses show that firms are indeed affected by workers taking longer leave. When women took additional time off for child-rearing, firms incurred costs in replacing them. In particular, our findings indicate that adjustment costs went beyond replacing the absent workers one for one. Even though Swedish firms did not need to pay the workers on leave, employers were not able to find perfect replacements for the absent workers and had to pay extra to fill in the work left behind.

V. 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 hours of incumbent workers. We show that the net effect of such adjustments to the 1989-reform in Sweden come at a cost over and above the salary cost of the workers to be replaced. The magnitude of such costs are likely to depend on how easily the firm is able to find good substitutes for the worker(s) on leave.[24]

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

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

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

A. *Internal Substitutability of Workers*

We begin by analyzing whether firms' adjustment strategies depend on the number of available substitutes within the firm. Do firms with fewer internal substitutes resort to external hires? We characterize the potential for internal substitution possibilities at the workplace by the overall occupational specialization at the establishment.[25] Similar to Cortes and Salvatori (2019), we calculate the employment share in the largest occupation category within workplaces as a measure of internal substitution.[26] The intuition is that workplaces with a high degree of occupational concentration would have many workers doing similar tasks, and thus have greater scope for internal substitution across incumbent workers. We divide workplaces into groups depending on whether they are above or below the seventy-fifth percentile of the internal substitutability index and estimate our main specification (3) with an additional interaction term indicating firms with high degrees of substitutability. We then report the coefficients for firms with high and low substitutability separately from this pooled regression in Figure 4.

We focus on two outcomes in this analysis: work hours of the incumbent staff (workers who were employed at the firm at baseline, excluding the women on leave), and the number of new primary hires. We find that firms with a high degree of internal substitutability increased incumbents' hours by 1.2 and 2 percent in years 2 and 3 in response to the reform, whereas firms with internal supply constraints (lower substitutability amongst coworkers) did not adjust work hours of incumbents. The heterogeneous responses in incumbent hours are significantly different in year 3 (see panel A of Figure 4). Moreover, for an average-sized firm with few internal substitutes, exposure to the reform led to a significant increase in new hires by 0.47 and 0.76 workers in years 2 and 3, respectively (panel B). Firms with many internal substitutes, by contrast, did not respond to the reform on the hiring margin. The differences in new hire responses across the two groups of firms are not statistically significant, but the point estimates are in line with our prediction.

The fact that firms employed different strategies depending on the availability of internal substitutes implies that human capital specificity may induce binding supply constraints, and thus points to an additional source of frictions facing firms when workers leave.[27]

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

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

[27] Brenøe et al. (2020) finds similar results on firms without close substitutes for workers on leave.

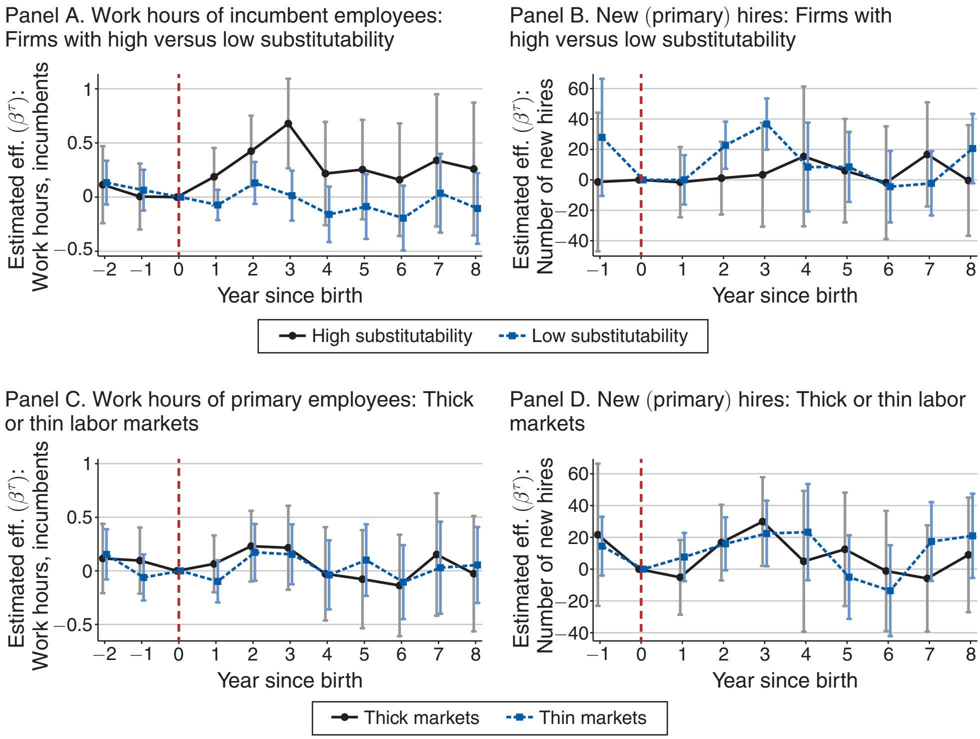

Figure 4. Heterogeneous Employer Responses by Internal and External Labor Market Conditions

Notes: The graph reports difference-in-differences estimates of the 1989 reform on firms' outcomes. Each point in the graph represents the coefficient on an interaction term consisting of an indicator for employing women who gave birth to a child in 1988 or 1987, the proportion of the workforce whose child was born in October–December (relative to January–July), and the respective event-time indicator for year since birth indicated in the *x*-axis. Ninety-five percent confidence intervals indicated by vertical lines. Contracted work hours are expressed in percentages of full-time hours (40 hours per week).

B. *External Labor Market Conditions*

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

To capture the external labor market conditions facing the firms in our sample, we construct measures of industry-level labor market thickness at each locality, using population-wide data on employed individuals aged 19–64. We delineate

64 commuting zones, and define labor market thickness as the share of employment in a 2-digit industry within a commuting zone relative to the nationwide employment share in that industry.[28] We define a market to be "thick" if the local employment share in a given industry is higher than the national employment share in the same industry, and estimate heterogeneous employer responses to extended employee absence by whether they are facing a thin or thick local labor market in each year.

Panels C and D of Figure 4 presents heterogenous effects of the reform by local labor market thickness. We find no statistically significant differences in the adjustment strategies undertaken by firms that faced thin and thick markets.

C. *Heterogeneity in Wage Costs by Internal and External Substitutability*

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

In Figure A.6 of the Appendix, we show that firms with a low degree of internal substitutability incurred significant increases in the total wage bill in years 2 and 3, and the point estimates are over two times as big as that of firms with high substitutability. This suggests that firms with little scope of internal substitution might face higher costs of adjustment, although the differences are not significant. Firms facing thin labor markets also incurred significant increases in labor costs (while those in thick labor markets did not), but there are no significant differences in the total wage bill across firms facing different external labor market conditions.

VI. Employer Responses to Male Leave-Taking: Daddy-Month Parental Leave Reforms

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

To study the effect of men's leave-taking, we make use of the second "daddy-month" reform in 2002, which gave additional monetary incentives for fathers to take up parental leave. Prior to the implementation of the reform, one month of the paid leave was nontransferable between the parents. To further encourage fathers' leave-taking, the government introduced an additional nontransferable month of paid leave—a second "daddy-month" in 2002.[29] All parents of children born on January 1, 2002 and later were eligible for the additional paid leave.

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

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

A. *Worker's Labor Supply Response*

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

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

The vector of controls, $\mathbf{X}_i'$, include a polynomial in age, indicators for education level, dummies for the pre-birth income decile, dummies for the parity of the child, dummies for the calendar month of birth, and dummies for industry affiliation (at baseline). We estimate this model on male labor income, and display the results in Figure 5. The results show a decline in men's labor income in years 0, 1, and 2 after birth, and also some decrease in years 6–8 (right before the parental leave allowance period ends).[30] The total income drop in years 0–2 combined amounts to 32,600 krona, which corresponds to 1.13 months worked for a full-time employed male worker in the private sector.

B. *Employer Responses*

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

The empirical specification is thus equivalent to that expressed in equation (2), with the same set of controls as used previously. One small difference is that firms

[30] The reduction in labor supply by fathers in years 6–8 in Figure 5 for the 2002 reform is similar to the effect documented for the 1995 "daddy month" reform. This uptake just before expiration of the benefit is consistent with the government informing parents about their outstanding entitlements for parental leave (Ekberg, Eriksson, and Friebel 2013).

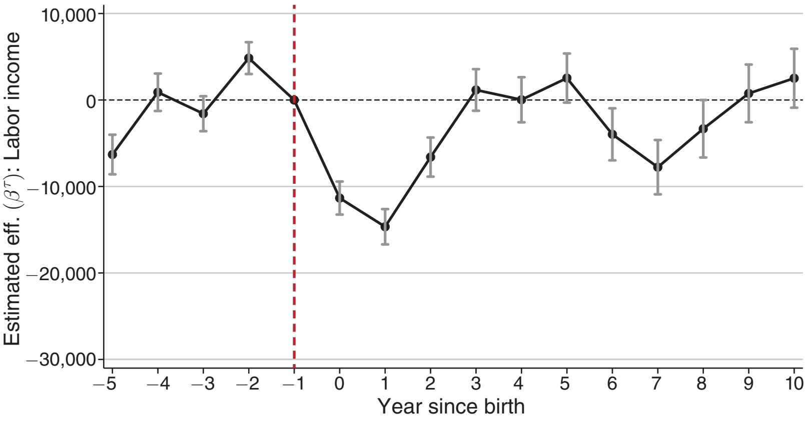

Figure 5. The Effect of the 2002 Parental Leave Reform on Male Labor Supply

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

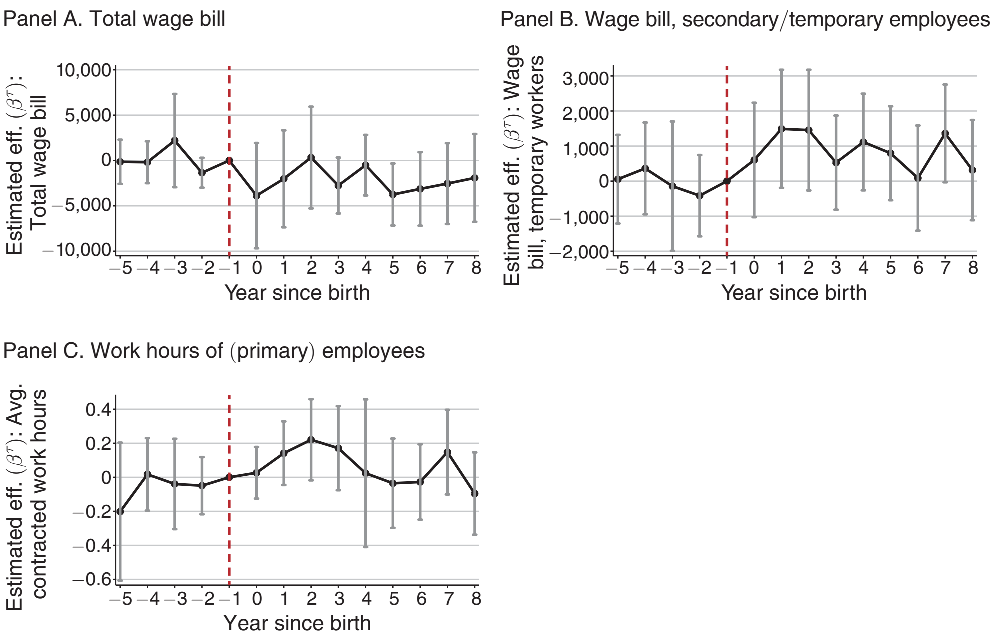

Figure 6. Employer Responses to the Male Workers' Leave: Effects of the 2002 Daddy-Month Introduction

Notes: The graph reports difference-in-differences estimates of the 2002 reform on firms' outcomes. Each point in the graph represents the coefficient on a triple interaction term consisting of an indicator for employing men whose child was born in 2002/2001 (relative to 2001/2000), the proportion of the workforce whose child was born in 2002 (2001), and the respective event-time indicator for year since birth indicated in the *x*-axis. Thus, the points correspond to the $\hat{\beta}^{\tau}$ from equation (2), along with the 95 percent confidence intervals. The firms' wage bill outcomes are expressed in 1,000s krona. Contracted work hours are expressed in percentages of full-time hours (40 hours per week). The average firm size in baseline is 41 workers.

were aware of the reform on January 1, 2002, so firm outcomes in event year 0 could already be a response to the reform. Therefore, we consider event year -1 as the baseline year for all specifications regarding the daddy-month reform.

Figure 6 presents the results for firms' total wage bill, wage bill to secondary workers, and work hours of the coworkers on leave. Since fathers took leave in years 0, 1 and 2 in response to the reform, one might expect the firms' wage bill to decrease in these years if employers do not adjust at all. Figure 6, panel A shows that there was a slight decrease in the total wage bill in the year of childbirth, but the estimates are not significantly different from zero in all years 0 to 2, suggesting that employers responded just enough to make up for the temporary absence of the male workers. Specifically, there is an increase in wage bill paid to secondary/temporary workers by 1.5 million krona in years 1 and 2 (significant at the 10 percent level). For an average-sized firm, this is equivalent to 1.8 full-time equivalent months for each additional man on leave.[31] The reform also increased the total contracted work hours of the coworkers (significant at 10 percent) by 6.0 and 8.4 hours per week in years 1 and 2.[32]

Comparing employer responses across the two reforms might be informative about key policy design features. In response to the 3-month extension of the 1989 reform, women took up 2.6 months of additional leave, and firms hired additional primary and secondary workers and increased contracted hours of existing workers. For each additional woman going on extended leave, the average firm incurred adjustment costs of 10 full-time equivalent months over and above the replacement level. In response to the one-month daddy-leave reform, men took up 0.87 months in total (see Table B.3 of the Appendix), and firms also adjusted by hiring secondary worker and increasing contracted hours of the coworkers. However, the employer adjustments in 2002 were barely at replacement level to make up for the temporary labor shortage, as there was no change in firms' total wage bill.

There might be several reasons for the smaller effects of the 2002 reform on firm outcomes compared to the 1989 reform. First, the 2002 reform is smaller than that in 1989, as there was only a one-month extension in 2002 compared to three months in 1989. Second, firms' planning horizon for the additional leave may be longer in 2002 as fathers typically take leave a year after the birth of the child (after women exhaust their leave). Third, employers might respond to men's absence differently than women's. Men's parental leaves are short even with the leave extension, so firms might be reluctant to hire permanent workers to replace men (while they might find it necessary to do so for women).[33]

[31] Calculation: From Table A.5 of the online Appendix, the average yearly earnings for a full-time worker in control cohort firms is (10,100/42=) 240,000 krona. For each additional worker eligible for extended leave, the treatment intensity of the firm increases by $\frac{1}{42}$, which leads to an increase in secondary workers' wage bill of $\left(1{,}500 \times \frac{1}{42}/240 \;=\right)$ 0.15 full-time workers, or $(0.15 \times 12 \;=)$ 1.8 full-time equivalent months.

[32] Calculation: Contracted hours range from 0 to 100 percent, where 100 percent is 40 hours per week. For each additional worker eligible for extended leave, the treatment intensity of the firm increases by $\frac{1}{42}$, so the effect size on total hours at the workplace level is $\left(40 \times 0.21 \times \frac{1}{42} \times 42 \;=\right)$ 8.4 hours per week in year 2.

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

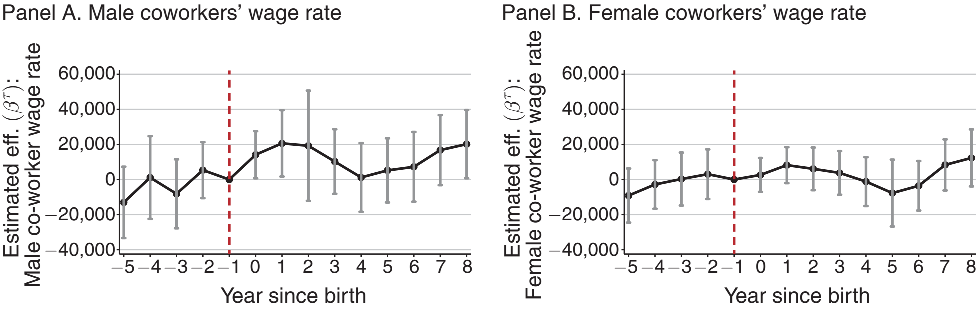

Figure 7. Effects of the 2002 Daddy-Month Reform on Remaining Coworkers' Wage Rates

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

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

Indeed, we find a statistically significant increase in the wage rates of the remaining male coworkers, in the first three years after the reform (see Figure 7). In an average firm (42 workers at baseline), the point estimate in years 0, 1, and 2 correspond to effect sizes of 1.2, 1.8, and 1.7 percent increases in male coworkers' monthly full-time equivalent wages. We find no effects on female coworkers' wages (panel B). The fact that the firms increase their demand only for the male incumbents and not female suggests that men and women might not be perfect substitutes within the workplace, which is plausible given the substantial occupational segregation by gender, even within firms.[34]

Overall, the results from the 2002 reform are in line with the results from the 1989 reform. Even though firms had a longer planning horizon in response to the 2002 reform, we find suggestive evidence of the existence of frictions as well. If human capital is firm-specific, or for any other reasons suitable replacement is not easy to find, a longer planning horizon would not necessarily eliminate the adjustment costs for firms when workers go on leave.

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

VII. Conclusions

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

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

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

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

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

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