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ABSTRACT

Parental Leave from the Firm's Perspective*

This study investigates the firm's response to parental leave induced worker absence. Combining a 20-week maternal leave expansion in Norway and detailed matched employer-employee data between 1983 and 2013, we identify the causal impact of absence on outcomes using a shift-share design. Employers with greater exposure to absence hire more women aged 40 or less and face more employment turnover. These adjustments do not affect profits, but lead to greater investments and sales and to a lower value added and a lower wage bill. One important channel behind such changes is a significant growth of young female part-time employment.

JEL Classification: L23, L25, J16, J21, J23, J81

Keywords: workforce composition, firm-level gender employment dynamics, corporate outcomes, part-time employment, employer-employee matched data, shift-share research design

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1 Introduction

During the past fifty years, most high-income countries have introduced a host of generous parental leave policies, with a variety of objectives, ranging from mothers' stronger labor market participation to gender equality, and from promoting higher fertility to facilitating early child development. A broad and expansive literature has documented the impact of such policies on mother's behavior especially, but also on father's and child outcomes (e.g., Ruhm, 1998; Baum, 2003; Tanaka and Waldfogel, 2007; Baker and Milligan, 2008; Han et al., 2009; Lalive and Zweimüller, 2009; Rossin, 2011; Dustmann and Schönberg, 2012; Ejrnæs and Kunze, 2013; Ekberg et al., 2013; Schönberg and Ludsteck, 2014; Lalive et al., 2014; Dahl et al., 2016; Stearns, 2018; Bana et al., 2020; Gruber et al., 2023; Kleven et al., 2024; Bailey et al., 2025).[1] Much less, instead, is known about the effect on firms.

One of the main contributions of this paper is the focus on the firm's long-term responses to substantial expansions to parental leave. We believe this is important not only because businesses play a crucial part in this policy initiative and may hold the key to its successful implementation in any economy around the globe, but also because many commentators have raised concerns about the potential burden that paid parental leave and its associated worker absence impose on all companies (Kamal et al., 2025; Bartel et al., 2025).[2]

Even when paid parental leave is fully government funded through general taxation and businesses do not face any direct financial cost of leave taking, job interruptions and worker absence may indeed be costly to employers. This is the case, for example, if workers on leave cannot be readily replaced by other incumbent workers (because of skill shortage or mismatch; i.e., not everyone can perform the same tasks) or by external recruitment (because of labor market frictions; i.e., it takes time to find suitable replacements, possibly just on a temporary basis).[3] In both cases, whether relying on internal workforce reorganization or external hiring, companies must face a constellation of adjustment costs, including advertising for job openings, training new recruits or redeploying incumbent staff to different posts, and operating below potential because of the likely discrepancy between the portfolio of skills required in the jobs left vacant and the portfolio of abilities possessed by the individuals who eventually fill those vacancies. Internal redeployment may not be used in small firms and it can stretch teams even in large organizations and induce stress, while new substitutes could be expected to speed up learning the required skills more

[1] Olivetti and Petrongolo (2017), Rossin-Slater (2018), and Albanesi et al. (2023) provide insightful reviews of this research.

[2] For convenience, the terms employer, firm, plant, business, establishment, company, and organization are used interchangeably when there is no risk of confusion. When needed, however, we make the appropriate distinctions.

[3] Recent empirical evidence on these types of frictions and related difficulties faced by firms can be found in Kerr et al. (2016), Guvenen et al. (2020), Le Barbanchon et al. (2023), and Bertheau et al. (2023), among others.

quickly than is considered ideal. Both responses might result in greater constraints, which can negatively affect staff morale, and this, in turn, may feed back into lower productivity. They may also have a longer-term strategic impact on firms' human capital composition.

For any national economy, the human capital embedded in its workforce is key, with the labor share still accounting for 60–80% of global value added, despite its secular decline in most industrialized countries (e.g., Autor et al., 2020). Human capital and its quality are also key to any organization that faces sustained competition to recruit and retain the most productive employees, especially in times of declining fertility and shortages of skilled workers with specific competencies, while possibly aiming to achieve gender equality and inclusivity in the workplace (e.g., Tarique and Schuler, 2010; Autor et al., 2024; Hoffman and Stanton, 2024; Benson et al., 2024). Worker absence, therefore, is a fundamental issue for firms to address and for us to understand, in particular if absence is systematically more pronounced among specific groups of workers with protected characteristics, such as young women and mothers, and employers respond strategically to it by changing recruitment and composition of their workforce.

Our focus is on Norway. Norway has rich administrative data, covering the universe of corporations and the entire population of workers over a long time period. Norway has also been one of the first countries that passed generous parental leave policies, extending job-protected government-funded maternity leave around childbirth from 18 to 38 weeks with full wage replacement through seven successive reforms each year between 1987 and 1993.[4] We leverage these seven reforms to rely on the exogenous large variation in worker absence faced by all employers over a long period of time post-reform up to 2013.

Existing research has provided a great deal of empirical evidence on the effect of those reforms on workers and their children. By and large most of the results point to zero effects. For instance, Dahl et al. (2016) find that the maternity leave expansions driven by the first six reforms were accompanied by 100% take-up in terms of participation and utilization of all weeks of leave, but had little effect on a wide set of outcomes, including children's schooling, parental earnings, and mothers' participation in the labor market both in the short and the long run (up to 14 years after childbirth), completed fertility, marriage, and divorce. Corekcioglu et al. (2024) examine the impact of the same six extensions (and two more) on the likelihood that mothers reach top-paying jobs and executive positions. They find that the extensions neither helped nor hurt mothers' chances to be at the top of their companies' pay ranking or in leadership roles up to a quarter of a century after childbirth. Rege and Solli (2013) show that the introduction of the four-week paternity leave quota in 1993 led to a small reduction in fathers' future earnings up to the child's fifth birthday. Using the same reform, Cools et al. (2015) instead report a null effect on fathers. From

[4]The 1993 reform allowed for an additional four-week quota of leave reserved to fathers. Other leave policy regulations have been enacted more recently, essentially extending the father's quota.

such studies, however, we do not know whether and how firms adjust to worker absence due to parental leave.

In addition to being costly for employers, worker absence is also likely to be correlated with firm-specific unobservables, such as organizational practices, personnel management, and business culture (e.g., Bloom and van Reenen, 2010; Syverson, 2011), which cannot be observed with matched employer-employee administrative data. Some of these characteristics may be relatively stable over time and could be dealt with by models with firm fixed effects. Others, instead, are time-varying and require different identification strategies (Bloom et al., 2019; Maestas et al., 2023; Hoffman and Stanton, 2024; Ashraf et al., 2024). To provide causal evidence on how parental leave extensions affect firms' outcomes, we combine several Norwegian registers covering all businesses and all workers in Norway from 1983 to 2013 with the seven leave reforms, which are expected to affect firms differently depending on the age-gender composition of their workforce.

More specifically, identification of the effects of interest comes from plausibly exogenous variation in worker absence due to the reform-induced extensions to parental leave at the plant level by using a new shift-share design. This combines labor-input-specific changes in absence within a local labor market (shifts) with variation in firm exposure to interruptions given by their historical, pre-sample age-gender employment mix (shares). We define two labor inputs, one of which is likely to be characterized by work interruptions more than the other (female employees aged 40 or less versus older women and male workers of all ages), and assume that predicted leave duration increases the effective marginal cost of labor, because either external replacement or internal redeployment strategies, or both, impose financial strain on companies. With this design, therefore, we take advantage of the fact that different firms face different degrees of labor market gender segregation and are exposed to the leave reforms at different intensities (e.g., Flory et al., 2015; Hartung et al., 2025; Kleven et al., 2025).

Taking into account some of the insights from the recent econometric literature on shift-share instruments (e.g., Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022, 2025), we emphasize two features of our research design. First, to ensure that the shifts are exogenous to the focal firm, we apply a leave-one-out approach at the industry level and instrument worker absence in a given plant in a given industry by using the average duration of parental leave taken by workers in the same local labor market but in establishments in different industries. Second, to minimize the risk that contemporaneous shocks to firm technologies would affect both firm-specific employment structure and firm outcomes, we use pre-reform information on plant-level age-gender employment composition and estimate the impact on outcomes more than 10 years later. As organizations differ in their baseline employment mix within an industry and local labor market, this design allows us to exploit changes in parental-leave-induced worker's stoppages that are exogenous from the employer's perspective when we also control for fine-grained fixed effects (i.e., firm, year, and industry×local labor market×year) to absorb potential confounding shocks that might arise in the firm's own product market.

There is considerable variation both in pre-reform exposure to firms' labor input composition across industries and in year-by-year changes in average parental leave duration across industries and local labor markets, i.e., the underlying sources of identification in our empirical application. In the first-stage analysis, we show that our firm-level shift-share measure of predicted worker absence due to extended parental leave is strongly positively related to the actual total number of parental leave days taken by all employees in a firm in a given year. A firm facing a one standard deviation increase in predicted absence (about 42 days) would experience an increase of 79 days in actual parental leave related absence in total, compared to a counterfactual firm operating in the same local labor market and industry, but with no exposure to absence.

Our main result is that a longer predicted leave is accompanied by greater employment of young women. A one standard deviation increase in predicted leave duration leads to an increase of young women's employment share by almost 91% over the 20 years post-reform, from an average of 9% in the firm's workforce at baseline. The headcount effect on other workers (older women and all men) is economically small and statistically indistinguishable from zero, while there is evidence of increased labor turnover, with more elevated hiring and separation rates, which may in part reflect the mobility of leave-takers' replacements, as leave-takers return to their old jobs. These changes tend to leave the likelihood of plant closure and the growth of corporate profits unaffected. They are accompanied, instead, by higher firms' investments and sales and by a lower value added and a reduced total wage bill. These effects, in turn, reveal greater reliance on new workplace technologies and capital deepening, as well as significant internal labor restructuring.

How do companies manage to increase employment while reducing their wage bill? We find that employers achieve their labor expansion strategy by increasing the share of young female part-timers, a work arrangement that is typically cheaper and more flexible than their full-time counterparts. Companies do not move incumbent young female employees to lower wage-hour packages on average, but they do reduce pay for workers in the bottom quartile of the wage distribution. Moreover, although they do not rely on young female workers' greater time flexibility by changing their overtime hours, employers rely instead on marginally more overtime work performed by other full-time incumbents (i.e., older women and men of all ages), which is arguably a more efficient cost-saving plan than recruiting more full-timers, regardless of age and gender. We also find little evidence of a major long-term change in the human capital composition of the average plant workforce. Finally, in terms of cross-firm heterogeneity, small private enterprises and female-led businesses turn out to be the main drivers of the observed adjustments.

We interpret our results as providing strong evidence that firms adjust their workforce quite flexibly to face worker absence, particularly young women's, who are disproportionately more likely to use parental leave than fathers or older women. As we take advantage of the cumulative parental leave expansion induced by the seven Norwegian reforms under analysis, our findings bear on the broader question of how firms are affected by family policies, providing new answers from a long-term perspective. This issue has generally received little attention in the literature, with the possible exception of the study by Bennedsen et al. (2022), which examines the effect of wage transparency (but not parental leave) on firm financial outcomes in Denmark.[5]

Our evidence speaks to the growing empirical research on worker absence, which may be driven by unexpected deaths (e.g., Azoulay et al., 2010; Jaravel et al., 2018; Bertheau et al., 2022; Jäger et al., 2025), or hiring difficulties (e.g., Le Barbanchon et al., 2023; Bertheau et al., 2023), or sickness (e.g., Hoey et al., 2023; Adhvaryu et al., 2024; Schmutte and Skira, 2025). The strongest links of our research are with the parental leave literature, which we review in detail in the next section, where we also qualify some of the novelties of our contribution. Section 3 describes the institutional context. Section 4 presents the key features of our research design and provides evidence on the credibility of the identification strategy, involving both shifts and shares and using sickness leave as a falsification test. Section 5 describes the data and discusses our sample selection, while Section 6 shows and interprets the first-stage estimates and the main results on employment and firm performance outcomes. Section 7 focuses on the mechanisms of firms' adjustment and summarizes the results from several heterogeneity checks. Section 8 concludes. Supplementary results and additional information on the data are available in the Online Appendix.

2 Related Literature

As mentioned in the Introduction, a large economic literature has focused on the question whether longer parental leave has effects on mothers' labor market outcomes, including employment, wages, family income, and subsequent fertility. Extensions to parental leave duration are generally found to have negligible impacts on maternal outcomes, whether or not the leave policy is government mandated, job protected, paid at the pre-birth wage, and funded through general taxation (e.g., Lalive and Zweimüller, 2009; Lalive et al., 2014; Dahl et al., 2016; Bana et al., 2020; Kleven et al., 2024; Corekcioglu et al., 2024; Machado et al., 2024), although there is evidence of small negative short-run effects in some cases

[5]For an equilibrium analysis of pay transparency, see also Cullen and Pakzad-Hurson (2023). For recent reviews of the role of government policies in shaping the landscape in which families and the labor market (albeit not firms specifically) interact, see Albanesi et al. (2023) and Dahl and Løken (2024).

(Ejrnæs and Kunze, 2013; Schönberg and Ludsteck, 2014; Stearns, 2018).[6]

Much less is known about the effect of parental leave policies on firm behavior.[7] Ginja et al. (2023) estimate firm adjustment costs to worker's interruptions using an extension of the wage-replaced component of paid leave from 12 to 15 months introduced in Sweden in 1989. Focusing on firms with ten or more workers, and following them up to eight years after the reform, Ginja and colleagues find that the sizeable three-month leave expansion raises both women's leave duration and likelihood of separating from pre-birth employers. In response to this, firms with greater exposure to the reform hire additional workers and, to a lesser extent, increase contractual hours of the remaining coworkers. Taking all such adjustments together implies that having one additional worker going on extended leave increases the total wage bill by a monetary amount corresponding to ten full-time equivalent months. This effect, however, is relatively short-lived, with the bill remaining unchanged after the fourth year post-reform.

Two other studies corroborate the previous evidence that extensions in the duration of parental leave have detrimental effects on firms' employment. First, Huebener et al. (2025) show that a paid parental leave reform enacted in 2007 in Germany has a modest short-term employment impact among companies with up to 50 employees. This is accompanied by a null effect on firms' wage bill and their likelihood of shutting down. Second, the paper by Gallen (2019) exploits a 2002 Danish reform which substantially increases the length of fully-compensated parental leave by 22 weeks. Following companies up to three years post-reform, it finds that the expansion has a short-term negative impact on both firm survival and the retention of mothers in establishments with five or more workers, while leaving the wage bill broadly unaffected.

For Denmark again, Brenøe et al. (2024) investigate the effect of mothers' work absence due to parental leave following childbirth on small firms (i.e., companies with three to 30 employees) and coworkers. They document that firms hire temporary workers and slightly increase retention of incumbent employees in response to a birth and subsequent leave take-up. Hours of work and earnings of existing employees also increase temporarily, contributing to a greater wage bill. Overall, however, the wage cost of parental leave to firms is negligible, and so are the impacts on firms' output, profits, and closure. Schmutte and Skira (2025) analyze how Brazilian companies respond to maternity leave interruptions. They find that, on average, employers add one-fifth of a worker to replace an employee on leave up to three years from the start of leave, concluding that firms rely on internal labor

[6]There is also early evidence, mainly from North America, that the introduction of short leave programs may benefit subsequent maternal labor supply (e.g., Waldfogel, 1999; Baum, 2003; Baker and Milligan, 2008; Rossin-Slater et al., 2013). This is countered by the recent null results found by Bailey et al. (2025).

[7]A special case between the worker's and the firm's perspective is the study by Bonney et al. (2025), which examines how firm performance changes after Norwegian entrepreneurs become parents. They find that female-owned (but not male-owned) businesses experience a substantial decline in profits, mainly driven by the time demands of motherhood and childcare.

markets in anticipation of predictable labor supply disruptions.[8]

A compelling feature of these studies is that they model the response by companies as if firms were individuals. While the treatment assignment is clean at the worker's level (e.g., a woman has — or does not have — a birth by a given date when a reform is introduced), it becomes challenging at the firm level. Some, especially large, organizations in female dominated sectors could be continuously treated, i.e., at least one of their employees has a birth every year. This may explain why much research often focuses on small firms (e.g., Brenøe et al., 2024; Bartel et al., 2025; Huebener et al., 2025) or short-term responses (e.g., Gallen, 2019; Bennett et al., 2020; Friedrich and Hackmann, 2021; Schmutte and Skira, 2025; Huebener et al., 2025).

In this paper, we use a different identification strategy. We highlight three of its key ingredients. First, to estimate long-term firm responses to worker absence, we leverage changes through the enactment of seven parental leave reforms in Norway between 1987 and 1993, which extended job-protected maternal leave jointly from 18 to 38 weeks with full wage replacement. Second, we consider all establishments with at least four employees pre-1987 and do not impose any upper limit on the number of workers, either pre-1987 or post-1993. Focusing on small companies, as done by many of the existing papers, could raise issues of sample representativeness and generalizability of results. The economic mechanisms underlying the response to extended leave may also differ across firms of different size.

Third, we use a new shift-share design, whereby a Bartik-style instrument is expected to predict worker absence related to the parental leave extensions induced by the seven reforms, which are described in the next section. In a nutshell, we ask whether firms with different pre-reform *shares* of young women perform differently in the long-run post-reform, when mothers can actually take longer leave. We then combine this variation with the possibility that other companies in the same local labor market but in different industries experience different *shifts* in parental leaving taking post-reform. Section 4 presents these features formally.

[8] Another related study is the paper by Friedrich and Hackmann (2021), which focuses on the effect of nurses on health care delivery and patient health outcomes across sectors. This work takes advantage of a government-funded leave program introduced in Denmark in 1994, which offered parents the opportunity to take up to one year of absence per child aged 0–8. The main finding of the paper is that the program led to a 12% reduction in nurse employment, which in turn had a particularly detrimental effect for nursing homes. For the United States, Bartel et al. (2025) use survey data collected between 2016 and 2019 on a sample of firms with 10–99 employees in New York State (which introduced paid family policy in 2018) and Pennsylvania (which did not have a comparable leave policy in place). Using matched difference-in-differences, they find no evidence that paid family leave had any adverse impact on employer ratings of employee performance or their difficulty of handling long employee interruptions. See also the work by Bennett et al. (2020), which finds that establishments that adopt state-level Paid Family Leave acts experience productivity gains relative to neighboring establishments that do not.

3 Parental Leave Reforms

Under the Norwegian government-mandated parental leave system, firms bear no direct monetary cost of the leave of absence.[9] However, publicly funded job-protected parental leave matters to all employers, as they are legally required to re-employ their absent employees in the same (or a comparable) position as they were before taking leave.

Our analysis leverages the large expansion in leave duration that occurred between 1987 and 1993. Before 1987, working parents had access to 18 weeks of job-protected government-funded paid leave after childbirth. Eligibility required parents to be employed for six of the 10 months before birth and have earnings higher than the basic income. Mothers were entitled to a minimum of six weeks of leave around childbirth and they usually took all the remaining 12 weeks of leave as well.[10] During the leave, firms did not have to pay wages. The wage replacement was funded through general taxation and firms did not bear any additional leave-related monetary cost. Government mandates provided for 100% income replacement through benefit payments up to a generous earnings threshold, equivalent to six times the basic income. Finally, employers had to keep the work contract on hold and could not dismiss mothers during pregnancy or a parent during parental leave, while parents had the right to return to the same (or comparable) job. These features have remained in place since 1978.

In each year between 1987 and 1993, Norway rolled out a new policy that expanded paid parental leave from 18 weeks to 42 weeks in total (including four weeks reserved to fathers) at 100% income replacement.[11] Table 1 summarizes the timeline of all the reforms. The table also includes the preexisting default in place since 1978 and information on the maximum number of weeks of paid parental leave, minimum maternal and paternal quotas, and wage replacement rates. All reforms until 1992 only affected mothers, since at the time no fathers took leave, apart from two customary weeks after childbirth. The 1993 reform set aside a four-week quota of paternity leave for the first time worldwide.[12] Starting in 1989, parents could also choose longer leave, e.g., 52 weeks from 1993, at a reduced 80%

[9] The right to leave in case of pregnancy, birth, adoption and care for young children has been enshrined in the 1978 Working Environment Act (Lov om Arbeidsmiljø, arbeidstid of stillingsvern mv. (Arbeidsmiljøloven), Kapittel 12), while the rules on benefits linked to this have been articulated in the 1978 Social Insurance Act (Folketrygdeloven Kapittel 14).

[10] Working fathers had the right to two weeks of unpaid leave after birth. Their income would have been typically replaced by their employers through bilateral agreements (Work Environment Act, 1 July 1977). Parents could share the remaining 12 weeks. Fathers' take-up rate, however, was negligible.

[11] Besides political feasibility, the staggered introduction of the maternity leave extensions might have been due to the 1985 oil price shock which led to an unexpected public deficit and a significant devaluation of the Norwegian krone. This was then compounded by the ensuing banking crisis, which began biting in 1988 and continued through to 1992. More details are in Corekcioglu et al. (2024).

[12] After 1993, other reforms were introduced, which expanded the father's quota, but left the duration of maternity leave unchanged. Besides modeling issues, which will become clear in the next section, this is why we do not consider leave reforms beyond 1993.

wage replacement.[13]

Table 1: Parental Leave Reforms in Norway: 1978–1993

Reform Date	Weeks of Leave	Wage Replacement	Maternal Quota	Paternal Quota
01.07.1978	18	100%	6 weeks	
01.05.1987	20	100%	6 weeks	
01.07.1988	22	100 %	6 weeks	
01.04.1989	24 (30)	100 (80)%	6 weeks	
01.05.1990	28 (35)	100 (80)%	6 weeks	
01.07.1991	32 (40)	100 (80)%	2+6 weeks	
01.04.1992	35 (44.4)	100 (80)%	2+6 weeks	
01.04.1993	42 (52)	100 (80)%	3+6 weeks	4 weeks

Notes: The figures in parentheses in the second column correspond to the total number of weeks of leave when wage replacement is at 80% of pre-leave wages.

4 Research Design

Our aim is to investigate the impact of plant level worker absence related to parental leave on long-term firm outcomes. The challenge is that worker absence is endogenous from the viewpoint of the firm, even if parental-leave-induced job interruptions can be anticipated, at least in part. To address this challenge, we combine two distinctive features. First, we leverage the large exogenous extensions of job-protected, government-funded maternity leave of 20 weeks discussed in the previous section. Second, even though all businesses are affected by these reforms at the same point in time, we exploit firm-level heterogeneity in exposure to the reforms with respect to firms' labor force mix. Intensity of treatment is expected to be positively related both to the *pre*-reform share of young women and to the amount of leave taken *post*-reform. Companies employing a higher share of young women pre-reform may be exposed to longer absences than similar firms which rely less intensely

[13] While parental leave take-up rates and duration did go up during this period (see Dahl et al., 2016; Corekcioglu et al., 2024), we have no evidence of an increase in the direct pecuniary costs to firms, which could be linked to the greater fiscal burden induced by higher parental leave transfers. For instance, the payroll tax paid by employers remained substantially constant over time, possibly it declined slightly (see Helde, 1998; Strøm, 2002, and various years at <https://www.skatteetaten.no/satser/arbeidsgiveravgift/>). Similarly, the corporate tax rate, which was high in pre-reform years up to 1991 at 50.8%, was dramatically reduced to 28% in 1992 until the end of the sample period. See <https://www.regjeringen.no/no/dokumenter/nou-2003-9/id381734/?ch=9> and <https://tradingeconomics.com/norway/corporate-tax-rate>. Finally, formal free childcare in the first year of a child's life is fairly limited, based on the premise that the year-long parental leave provision would allow parents to be the main/sole carers (Black et al., 2014).

on young female employees before the enactment of the leave reforms. Similarly, employers would face longer absences if workers take full advantage of the extensions.

We predict each firm's total parental-leave-related worker absence using a shift-share research design. In particular, let us assume that firms employ two labor inputs, that is, women below 40 years of age (whom we refer to as young women) and the rest, i.e., older women and men of all ages. We then construct a Bartik-type instrument as the product between *shifts*, i.e., exposure of all companies in the same local labor market (LLM) but different industries to labor-specific leave, and *shares*, i.e., exposure of each firm through pre-reform employment composition of its labor inputs. We therefore have a weighted average of a common set of shocks (shifts), with weights reflecting firm-specific heterogeneity in shock exposure (shares).

For each plant i in local labor market l and in industry j and year $t \in [1994, 2013]$, our instrument is formally defined by:

$$B_{i,l,j,t} = \sum_k s_{i,k,0} \times \overline{PLdays}_{k,l,-j,t}, \tag{1}$$

where $k = \{$women aged 40 years or less, older women and all men$\}$ denotes the two labor inputs, which we also refer to as input f and input o, respectively, $\overline{PLdays}_{k,l,-j,t}$ is the average parental leave (PL) duration by labor input k at time t in all industries except j (where i operates) in a given LLM l, and $s_{i,k,0}$ denotes the employment share of labor input k in plant i over the pre-reform period, 1983–1986.

A few comments on the instrument are in order. The basic intuition behind $B_{i,l,j,t}$ is that while the aggregate variation in the expansion of parental leave is the same for all enterprises due to the national roll-out and government-mandated nature of the seven reforms, their impact is likely to differ significantly across plants because each of them, even within the same industry and local labor market, has a different employment composition.

To illustrate the role played by the pre-reform employment shares, s, consider a simple example in which two plants, labeled 1 and 2, that operate in the same local labor market, face the same shifts, $\overline{PLdays}_{f,l,-j,t} = 300$, and $\overline{PLdays}_{o,l,-j,t} = 50$ (which are close to the median values found in the data; see Table 3 in Section 5), but are characterized by different shares, i.e., $s_{1,f,0} = 0.8$ and $s_{2,f,0} = 0.2$, respectively. The predicted PL-related absence for plant 1 is $B_{1,l,j,t} = \sum_k s_{1,k,0} \times \overline{PLdays}_{k,l,-j,t} = 250$, while the corresponding figure for plant 2 is $B_{2,l,j,t} = \sum_k s_{2,k,0} \times \overline{PLdays}_{k,l,-j,t} = 100$. Although parental leave reforms are nationwide and common to all firms, those with a larger pre-reform share of young women may be exposed to absences more heavily, like plant 1 in this example. As the reforms expand the number of days of leave available to new mothers, employing them (or employing young women who become mothers) is more costly to employers, even if the actual pecuniary cost of leave is borne out of general tax revenues. Longer job interruptions, in fact, would imply higher costs if employers, for instance, had to recruit and train (temporary) replacements for mothers on leave and would have to reintegrate them one year after childbirth with a possible human capital loss.

The shift component of B in expression (1) is given by the time variation in the number of days of parental leave taken by either young women or all the other workers averaged over a given local labor market. To strengthen the credibility that the shifts be exogenous to the firm and avoid a mechanical correlation in the first-stage relationship, we use a leave-one-out correction at the industry level and exclude from our measure workers on leave in the focal firm as well as all other firms in the same 3-digit industry within the same LLM. The share component, instead, which is specific to each firm, is given by the proportion of a company's total workforce that is represented by each of the two labor inputs. To avoid that contemporaneous shocks affecting both the firm's employment structure and its productivity bias our estimates, we pre-sample information on the historical employment mix and construct time-invariant shares using pre-reform information on firm-level employment composition between 1983 and 1986. Identification, therefore, rests on the assumption that historical pre-reform firm-specific exposure and LLM-level shifts driven by exogenous reforms, which pick up parental leave duration across establishments in other industries of the same local labor market and over time, are orthogonal to the idiosyncratic shocks to firms' labor demand and productivity.

Empirical Specification — For each plant i in local labor market l, industry j, and year t, we estimate the following first-stage equation:

$$Absence_{i,l,j,t} = \delta_i + \gamma B_{i,l,j,t} + \tau_t + \zeta_{l,j,t} + \eta_{i,l,j,t}, \tag{2}$$

where $Absence_{i,l,j,t}$ denotes the number of total parental leave days taken by all employees in plant i in year t, with $t \in [1994, 2013]$, $B_{i,l,j,t}$ is the Bartik instrument described above, and δ_i, τ_t, and $\zeta_{l,j,t}$ are plant, year, and 2-digit industry$\times$local labor market$\times$year fixed effects, respectively. Standard errors are clustered at the local labor market level.

Our second-stage equation, which we fit using two-stage least squares (2SLS), is then:

$$Y_{i,l,j,t} = \alpha_i + \beta \widehat{Absence}_{i,l,j,t} + \lambda_t + \mu_{l,j,t} + \varepsilon_{i,l,j,t}, \tag{3}$$

where Y represents firm i's outcomes (e.g., employment, wage bill, and profits), $\widehat{Absence}$ is the expected worker absence predicted from Equation (2), α_i, λ_t, and $\mu_{l,j,t}$ denote plant, year, and 2-digit industry$\times$local labor market$\times$year fixed effects, as defined earlier; β is the parameter of interest, which captures the causal impact of worker absence on firm outcomes.

It is worth discussing potential threats to our identification strategy and how we address them. First, there might be local labor market or industry shocks that simultaneously affect firm outcomes and average parental leave duration. We stress that both Equations (2) and (3) include plant, year, and industry×local labor market×year fixed effects, which absorb any other potentially confounding shocks that drive changes in leave take-up and duration as well as changes in firm behavior. Second, establishments may choose their location depending on the gender segregation at the LLM level, that is, local availability of the two labor inputs which could then affect their performance. We should emphasize that in our setup plant location is by and large pre-determined before the introduction of the seven reforms, and therefore this may not constitute a threat in and of itself. Furthermore, should the most vulnerable firms locate where there is a larger supply of workers for which they expect to have higher demand, the impact on β is likely to be muted.

To provide evidence on the plausibility of our identification approach, we conduct two sets of balance tests and one major falsification exercise, using data described in the next section. With the first set of balance tests, we check whether the two shift components of our Bartik-style instrument, $\overline{PLdays}_{k,l,-j,t}$, $k = \{f, o\}$, are uncorrelated with baseline characteristics at the industry level, measured in the pre-reform years (1983–1986), as suggested by Borusyak et al. (2022). We consider five potential confounders, which are expected to reflect the structure of employment and firm growth potential across 3-digit industries (e.g., baseline firm size, earnings, the share of highly educated workers, and the share of part-time workers), and we perform our tests for both labor inputs for a total of 10 different tests. If the shocks are as-good-as-randomly assigned to industries within periods, we expect them to not predict these 10 predetermined variables.

Panel A of Table 2 shows that, out of the 10 reported tests, we detect imbalance at the 5% level only in three cases, i.e., the share of highly educated workers for both shift components and the part-time employment share of women above age 40 and all men (input o). These do not lead to a bias in β if they are not correlated with the second-stage residual $\varepsilon_{i,l,j,t}$ in Equation (3). To assess this potential for omitted variable bias, not only do we include firm fixed effects, α_i (which absorb the baseline shares of highly educated workers and part-time employment in estimating the employer's response in terms of both inputs f and o), but we also investigate their role as possible mechanisms.

Following the same procedure, we perform a second set of balance tests on local labor market characteristics measured in 1986, the year before the first reform was introduced. In this exercise, we use eight potential confounders for each of the two labor inputs (e.g., male and female employment and unemployment as measures of tightness in the local labor market, various measures of population as indicators of density, and unemployment rate). Panel B of Table 2 reveals that there is no statistically significant correlation within local labor markets, consistent with the notion of a quasi-random shock assignment across local labor markets.

Table 2: Balance Tests à la Borusyak et al. (2022)

	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
	Shift, input f				Shift, input o			
Balance variable	Coefficient	SE	p-value	N	Coefficient	SE	p-value	N
Panel A: Industry-level balance								
log(Firm size)	0.0006	0.0022	0.7888	711	-0.0119	0.0151	0.4352	731
log(Earnings)	0.0010	0.0008	0.2108	711	-0.0014	0.0031	0.6401	731
log(Earnings, women below 40)	0.0010	0.0006	0.0742	711	0.0008	0.0023	0.7357	731
% Highly educated	0.0010	0.0004	0.0236	711	0.0032	0.0013	0.0158	731
% Part-time	0.0001	0.0004	0.7609	711	0.0029	0.0013	0.0273	731
Panel B: Local-labor-market-level balance								
Unemployment rate, 1986	0.000	0.000	0.513	920	-0.000	0.000	0.893	920
Male unemployment, 1986	-1.300	1.569	0.412	920	-2.490	5.579	0.658	920
Female unemployment, 1986	-0.722	0.913	0.433	920	-1.371	2.963	0.646	920
Male employment, 1986	-169.158	172.135	0.331	920	-515.092	626.632	0.415	920
Female employment, 1986	-156.007	168.462	0.359	920	-479.680	615.312	0.440	920
log(Male population), 1986	-0.007	0.011	0.557	920	0.004	0.039	0.929	920
log(Female population), 1986	-0.007	0.011	0.554	920	0.005	0.040	0.907	920
log(Total population), 1986	-0.007	0.011	0.555	920	0.004	0.040	0.918	920

Notes: Panel A reports coefficients, standard errors, corresponding p-values, and number of observations (N, industries) from regressions of the pre-reform industry-level mean characteristics (computed over the 1983–1986 period) on the shift variable for input f, which is the leave-own-industry-out average PL days by women aged 40 or less (columns (a)–(d)), and on the shift variable for input o, which is the leave-own-industry-out average PL days by older women and all men (columns (e)–(h)), both measured in year t. The analysis period is 1994–2013. Panel B reports coefficients, standard errors, corresponding p-values, and number of observations (N, local labor markets) from regressions of the pre-reform local-labor-market-level mean characteristics for 1986 on the shift variable for input f (defined above, columns (a)–(d)) and on the shift variable for input o (defined above, columns (e)–(h)), both measured in year t. The analysis period is 1994–2013. The leave-own-local labor market out correction is implemented when computing the shift variables in Panel B. All regressions in both panels control for year indicators, and are weighted by the pre-reform share of the relevant labor input. Standard errors are clustered at the 3-digit industry level in panel A and at the local labor market level in panel B.

In sum, we reject imbalance in 23 out of the 26 potential confounders reported in Table 2 at conventional levels of statistical significance. Overall, this is a sound endorsement of our shift-share measure and of the assumption of a quasi-random shock assignment that underpins it. Besides the already mentioned additional steps we take to address the violations, we emphasize that the size of the three offending coefficients is quantitatively very small and unlikely to invalidate the research design.[14]

To alleviate misspecification concerns, Goldsmith-Pinkham et al. (2020) suggest presenting results using alternative methods, such as the Limited Information Maximum Likelihood (LIML) estimator, and comparing them to the Bartik-type 2SLS model we implement. Following this suggestion, we replicate our main results presented in the next

[14] Another test suggested by Goldsmith-Pinkham et al. (2020), which decomposes the Bartik estimator into a weighted sum of the just-identified instrumental variable estimators that use each share, $s_{i,k,0}$, as a separate instrument, is not necessary in our setting, as we essentially have a continuous difference-in-difference model with the pre-reform share of young women.

sections using a LIML procedure, whose results shown in Appendix Tables D.1–D.6 are very close to ours. This similarity should improve confidence in our identification strategy.

Finally, we perform a falsification exercise using data on worker absence related to long sickness leave.[15] The share component of the Bartik instrument is the same as in expression (1), but now the shift component is defined by average sickness leave duration. We then repeat the 2SLS estimation where the endogenous absence variable is defined analogously to Equation (2). The point of this test is to check whether the new instrument, which leverages sickness leave duration rather than parental leave duration, be a good predictor of worker absence when the technology of the shift-share design is still based on the timing of the parental leave reforms and the age-gender employment mix discussed above. There is no good economic reason that a PL-based sickness-leave-duration instrument should predict parental-leave-induced worker absence. This in turn is expected to have no effect on firms' employment outcomes. If instead the new instrument works out, one could be legitimately skeptical of the credibility of our identification strategy. The estimates from this analysis are displayed in Appendix Tables B.1–B.4. We find no statistically significant effect on the firm's employment decisions, with relatively weak first-stage results. In light of the results presented in Section 6, this vindicates our approach.

We conclude by linking our research design back to some of the studies described in Section 2. Our policy environment is characterized by a large variation induced by seven successive reforms, and we are interested in long-term firm responses to absences due to parental leave duration extensions. We thus cannot apply research designs similar to those used by Gallen (2019) or Huebener et al. (2025), even if we were willing to restrict our attention to small firms at baseline. Our shift-share approach is instead partly related to Ginja et al. (2023)'s triple difference specification, to the extent that the share component of our instrument, $s_{i,k,0}$, captures similar features to their treatment intensity at baseline.[16]

Understanding the Variation in Shifts and Shares — We supplement our discussion by providing additional insights on the two components that make up $B_{i,l,j,t}$, which help to clarify our research design.

Panel A of Figure 1 displays $\overline{PLdays}_{k,l,t}$, averaged over industries, for $k = \{f, o\}$ when $t = 2005$. It shows a great deal of variation in the shift component for the two inputs, f

[15] There is growing empirical evidence that sickness leave duration does matter to firm performance (e.g., Grinza and Rycx, 2020; Hoey et al., 2023; Adhvaryu et al., 2024; Schmutte and Skira, 2025).

[16] An alternative strategy is to estimate two-way fixed effects difference-in-differences with heterogeneous treatment effects that would allow for repeated cycles of firms going from untreated to treated from $t-1$ to t (or "switchers in") as well as for other firms going at the same time from treated to untreated ("switchers out"), as reviewed by de Chaisemartin and D'Haultfœuille (2023). An obvious challenge with this approach in our context is that of parallel trends. We leave this analysis for future research. Another strategy is to rely exclusively on $s_{i,k,o}$ to identify our effects of interest. This strategy, however, would not directly account for the enactment of the leave extension reforms, which we believe play a direct key role in our setting.

and o, across local labor markets. Some of the largest LLMs are named on the vertical axis. Panel B shows $s_{k,0} \times \overline{PLdays}_{k,l,t}$, averaging s over all firms for the two inputs k separately, $k = \{f, o\}$, and again when $t = 2005$. This provides an aggregate version of the two components of B, in which we remove the granularity at the plant$\times$industry level for the sake of visual exposition purposes. Had we shown its disaggregated version, we would have observed even greater variation. The panel, nonetheless, permits us to assess the importance played by the share component in conjunction with the shifts.

Figure 1: Shifts and Shares by Local Labor Markets, 2005

Notes: Panel A shows $\overline{PLdays}$ across all firms and industries in a given local labor market. Panel B shows $s \times \overline{PLdays}$, where $\overline{PLdays}$ is calculated as in Panel A, while s is averaged over all firms. This boils down to an aggregate version of the Bartik instrument, i.e., $B_{l,2005}$.

Even after integrating out the firm$\times$industry variation, the two panels jointly reveal a great deal of cross-sectional variation in the two terms of $B_{l,2005}$ at the local labor market level, which is driven by both share and shift components of the instrument, especially through young female workers. Repeating the exercise for other years leads to similar evidence.[17]

As plants differ in their baseline employment composition even within an industry and local labor market, our approach allows us to leverage substantial variation in workers' job interruptions that are exogenous from the employers' perspective in specifications in which we include market-level fixed effects to absorb any other confounding shocks that could

[17] This is not presented for the sake of brevity, but it is available from the authors.

occur in the firm's own product market. Our identification strategy thus relies on shifts and shares which are intended to be as close to random as possible, as we document in Section 4.[18]

5 Data and Summary Statistics

Main Data Sources — We use multiple administrative registers that cover the universe of Norwegian establishments and workers to build a novel panel of firms containing micro-based plant-level measures.[19] The employment statistics from 1983 to 2013 are used to construct an annual employer-employee matched data set for each worker's main job. Besides information on earnings and hours worked, this register crucially contains unique worker and employer identifiers. As unique employer (plant) identifiers are available from 1983, this defines the first year of our sample period.

Worker's characteristics are obtained from several different sources. To measure the duration of absence from work due to parental leave, we use the welfare register, which started in 1992 and contains exact information on the total number of days of a worker's absence due to paid parental leave and payout of parental leave benefits for each parent. We observe all birth dates in the birth register, which we merge with the parental leave taken by mothers and fathers. The demographic registers have information on worker's age and gender, which are needed to construct the two labor inputs, $k = \{$women aged 40 years or less, older women and all men$\}$ in each plant i. Information on exact contractual hours of work and overtime hours is available from the wage statistics register starting from 1997.

Collapsing the data to generate a plant-year panel dataset, we can then construct our key variables, $s_{i,k,0}$, $\overline{PLdays}_{k,l,-j,t}$, and $Absence_{i,l,j,t}$, as well as firm size and all the employment variables as our outcomes at the plant-year level. For each plant, we additionally have data on sector (public or private), industry at the 3-digit level, and the local labor market as classified by Statistics Norway (2009). From the balance sheets data available at the enterprise-year level from 1995 onward, we have information on profits, sales, assets, equity, investment, and total wage bill. All monetary variables are inflation-adjusted using the Norwegian consumer price index with 2000 as the base year.

Human capital variables are constructed combining different sources, after aggregating all workers from the same plant. The data on education come from the education register. Work experience, computed as the total number of years of positive earnings above the

[18]In terms of asymptotics, our empirical setup most naturally falls into the case of location$\times$industry going to infinity, while the number of periods and labor inputs are fixed i.e., 18,189 (=47 (unique local labor markets)$\times$387 (unique industries)), as opposed to 40 (=20 (periods)$\times$2 (inputs)). For similar setups, but in different contexts, see Mohnen (2024) and Le Barbanchon et al. (2023).

[19]More details on the construction of our dataset are available in Online Appendix E.

basic income level, is obtained from the register of taxable earnings which goes back to 1967. Firm tenure, defined as the total number of years worked in the same plant, can be constructed through the earnings register and the worker/employer identifiers. Worker mobility (i.e., inflows and outflows) can be readily constructed from the employer-employee matched data at the plant level. In a similar way, we can determine CEO gender. From the welfare registers we have the number of days of long sickness absence at the worker's level (available since 1992). Long sickness absence is registered only if it exceeds 10 working days. Finally, specific characteristics of local labor markets (e.g., unemployment rate and population by gender) are extracted from the municipality database.

Sample Selection and Descriptive Statistics — As anticipated in Section 2 and discussed in Section 4, the share component of the Bartik instrument, $s_{i,k,0}$, is the employment share of two labor inputs (k refers either to young women aged 40 or less or to all other workers), for each plant i at baseline (i.e., at time 0). In the analysis, the baseline period refers to pre-reform years, and we can go as far back as 1983 to compute the shares. It is thus defined over the 1983–1986 years. We exclude firms with three or fewer employees at baseline, in part because in this way we avoid an overrepresentation of companies with employment shares at the two extremes of the distribution, and in part because the vast majority of very small employers never hire (e.g. Fairlie and Miranda, 2017; Akcigit and Ates, 2021).

We follow each firm i until the end of the sample period in 2013. Plants that close down before are followed until the year prior to their death. This means we have an unbalanced panel of plants. Because of the construction of the shifts, $\overline{PL\text{days}}_{k,l,-j,t}$, we require firms to survive at least until 1995, two years after the last parental leave reform under analysis. Companies that shut down before 1995 are therefore excluded from the sample. This should not be a problem, given our focus on firms' long-term responses. Likewise, companies born after 1986 cannot contribute to our analysis by definition. For them, in fact, we cannot construct the share components pre-reform. Since we leverage the expansionary change from short, 18 weeks, to long, almost a year, paid parental leave over seven years, it is important for us to be able to observe firms before and after such policies.

Table 3 displays the summary statistics of the key variables in our study, averaged over the 1994–2013 period, which is the time span we focus on in the analysis. On average, we have information on almost 15,000 plants, for a total of over 178,000 plant-year observations. Reflecting the double selection related to plant closure on the one hand and lack of replenishment through new firms on the other, there are approximately 15,000 establishments in the early post-reform years and about 8,500 at the end of the sample period. Appendix Table A.1 presents descriptive statistics for the pre-reform period, 1983–1986.

Panel A of Table 3 shows that about 19% of the plants in the data have at least one employee on parental leave every year. Employers face 82 PL-related days of absence a year averaging across all their workers, including those who do not take any leave. Conditioning on taking leave, however, reveals large differences between the two labor inputs. The median number of PL days taken by women aged 40 or less is over 300, while the corresponding value for all other workers is 53 days (see the example we discussed in Section 4, where use these figures). The Bartik instrument, $B_{i,l,j,t}$, has an average value of 52 days and a standard deviation of 42, which we will use in the empirical analysis below.

Table 3: Descriptive Statistics, 1994–2013

	Mean	SD	Observations
Panel A: Parental Leave (PL) Variables			
%Firms with at least one employee taking PL	0.191	0.393	224,865
Total PL days	81.520	564.930	224,865
Median PL days, women aged 40 or less	301.932	76.968	23,554
Median PL days, older women and all men	52.903	75.213	27,405
Bartik instrument ($B_{i,l,j,t}$)	51.989	42.033	224,865
Panel B: Employment Variables and Outcomes			
Firm size	49.470	172.392	224,865
Number of women aged 40 or less	2.322	15.058	224,865
Employment share, women aged 40 or less	0.091	0.166	224,865
Employment share, older women and all men	0.909	0.166	224,865
Employment share, all women	0.459	0.355	224,865
Inflow share, women aged 40 or less	0.224	0.356	30,785
Outflow share, women aged 40 or less	0.147	0.289	112,606
Employment share, part-timers	0.092	0.151	224,865
Employment share, short part-timers	0.043	0.096	224,865
Panel C: Corporate Outcomes			
Operating Profits/Total Assets	0.060	1.336	119,734
Investment/Total assets	0.018	0.077	119,734
Income from sales/Total assets	2.159	2.697	119,734
Total assets/Firm size (in NOK 1,000)	725.340	12,565.598	119,750
Operating profits/Firm size (in NOK 1,000)	11.180	174.576	119,750
Total wage bill (in NOK 1,000)	144.520	528.504	119,750
Value added (in NOK 1,000)	277.613	1,936.965	119,750

Notes: Figures refer to plants that exist in at least one of the pre-reform years (1983–86), have more than 3 employees in the pre-reform period, and exist in the first two post-reform years 1994 and 1995. The observation window is 1994–2013. PL duration figures in panel A are reported for plants where the relevant type of employees take a positive number of parental leave days. In panel B, 'short part-timers' refers to employees who work fewer than 20 hours per week. Inflow and outflow shares are computed only on firms that have new hires or face separations of women aged 40 or less, respectively. Outcomes in panel C are reported at the enterprise level over the 1995–2013 period. All monetary variables are expressed in NOK and are inflation-adjusted using the Norwegian consumer price index with 2000 as base year.

Over the whole period, the average employment share of women aged 40 or less is 9.1%, totaling 2.3 young women out of the 49 employees in each plant (see panel B of Table 3). The fraction of women aged above 40 is substantially larger, at about 37%, while men of all ages make up the remaining 54% of workers across all organizations in the sample. Young women account for more than 22% of all plants' new hires and nearly 15% of all separations. The relatively small fraction of young women employed by the firms in our post-reform period underlines that a large bulk of young female workers tend to stick to their companies and move away only infrequently. It may also reflect employers' lower propensity to recruit young women, possibly to reduce the risk of work absence. Firms employ relatively small fractions of their workforce in part-time contracts and 'short' part-time jobs (i.e., jobs that require fewer than 20 hours worked per week), about 9% and 4% across all years, respectively.

Finally, panel C of Table 3 summarizes the corporate outcomes under analysis, which are measured over the 1995–2013 period. These include operating profits over total assets as a standard measure for firm profitability, investments over total assets as a measure of business dynamism, sales over total assets and total assets per employee as proxies of firm productivity. Another key outcome is the total wage bill, which combines information on average plant-specific wages and total employment and is thus informative about changes in firm-specific overall labor demand (Acemoglu and Restrepo, 2019). Unsurprisingly, many businesses in the sample report negative values for some of these measures, as they are likely to be tied to dividend policies which are highly sensitive to changes in tax treatments (see Alstadsæter et al., 2025, for the case of Norway).

6 Results

6.1 First-stage Estimates

We start by checking whether there is a strong relationship between the shift-share prediction of worker's absence due to parental leave taken by all employees, $B_{i,l,j,t}$, and the actual parental leave related worker absence the firm faces, $Absence_{i,l,j,t}$.

Table 4 reports the γ coefficients from the estimation of Equation (2), using different measures of $Absence_{i,l,j,t}$. Our main result in column (a) refers to the number of total parental leave days taken by all employees in plant i in year t. It reveals a strongly positive first stage, with an F statistic well above 100. A firm facing a one standard deviation increase in predicted parental leave related absence $B_{i,l,j,t}$ (which corresponds to 42 days, as reported in Table 3) would experience an increase of 79 ($= 42 \times 1.874$) actual parental leave related absence days, compared to a counterfactual plant operating in the same local labor market and industry, but with no exposure to parental leave related absence.

Table 4: First-Stage Estimates, 1994–2013

	(a) Total PLdays	(b) Mean PLdays	(c) Median PLdays	(d) $PL(0,1)$	(e) B (with s_t)
$B_{i,l,j,t}$ (γ)	1.874***	0.463***	0.488***	0.001***	0.409***
	(0.182)	(0.033)	(0.037)	(0.000)	(0.008)
F-test	105.786	196.680	173.166	110.957	2,911.707
Outcome Mean	80.059	31.033	30.849	0.189	34.297
Observations	224,885	224,885	224,885	224,885	224,885

Notes: The dependent variables are: the number of total parental leave days taken by all employees in plant i in year t (column (a)); the mean parental leave days taken by employees in plant i in year t (column (b)); the median parental leave days taken by employees in plant i in year t (column (c)); whether any parental leave was taken in plant i in year t (=1, if yes, and =0, otherwise; column (d)); and a Bartik-type measure of parental leave exposure computed using contemporaneous (time t) labor shares rather than historical shares (column (e)). All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. 'Outcome Mean' refers to baseline figures, while 'Observations' refers to the number of plant×year observations.
*** $p < 0.01$.

The same clear evidence of instrument relevance emerges for the other alternative measures of absence in columns (b) and (c), that is, for the mean and median parental leave days taken by all employees, respectively. Column (d) shows that a one standard deviation increase in $B_{i,l,j,t}$ predicts a 22% (=42×0.001/0.189) increase in the probability of parental leave related absence, while column (e) documents a substantial, statistically significant, correlation of about 0.41 between $B_{i,l,j,t}$ and a Bartik-style measure constructed with contemporaneous (rather than historical) labor shares. In the rest of the analysis, we will use the measure shown in column (a) as our instrumental variable.

6.2 Results on Employment

Table 5 presents the two-stage least squares (2SLS) and the reduced form estimates on the employment stock, measured in terms of either headcounts (column (a)) or shares of women aged 40 or less (column (b), expressed in relation to each respective input factor). Column (c) shows the headcount results for older women and all men, while column (d) displays the headcount estimates for all workers, regardless of age and gender. The table reports the 2SLS β coefficient from Equation (3) in panel A, and the reduced form coefficient, $\beta\gamma$, in panel B.

Column (a) in panel A shows that a one standard deviation increase in $B_{i,l,j,t}$, which implies a rise in $\widehat{Absence}_{i,l,j,t}$ of 79 days as predicted in the first stage (see column (a) of Table 4), will lead firms to expand their employment of women aged 40 or less by 2.69 $(= 0.034 \times 79)$ headcounts on average. Not only is this effect statistically significant, it is also economically substantial, as it corresponds to doubling the number of young women employed in each plant from an average of 2.32 (as shown in Table 3) to 5 over the 20 year period from 1994 to 2013. Consistent with this estimate, column (b) implies that, in response to a one standard deviation increase in B, the share of labor input f (i.e., young women) grows by 8 percentage points (=0.001×79), almost doubling the baseline share of 9 percentage points. As we control for year and industry×local labor market×year fixed effects, these estimates capture the plant-specific impact of absence on employment in addition to secular increases in input f across the local economies and industries and over time.

Table 5: The Impact of Parental Leave Related Absence on Firm Employment, 1994–2013

		Women Aged 40 or Less (input f)		Other Workers (input o)	All Workers (both f and o)
		Headcount	Share	Headcount	Headcount
		(a)	(b)	(c)	(d)
Panel A: 2SLS Estimates					
$\widehat{Absence}_{i,l,j,t}$	(β)	0.03432***	0.00104***	-0.00543	0.02888***
		(0.00082)	(0.00014)	(0.01022)	(0.01072)
Panel B: Reduced Form Estimates					
$B_{i,l,j,t}$	($\beta\gamma$)	0.06436***	0.00195***	-0.01019	0.05417**
		(0.00590)	(0.00011)	(0.01903)	(0.02148)
Outcome Mean		2.28623	0.08997	46.65311	48.93934
Observations		224,885	224,885	224,885	224,885

Notes: The dependent variables are: the number of employed women aged 40 or less (column (a), input f); the share of employed women aged 40 or less (column (b)); the number of older women (aged more than 40) and all men employed (column (c), input o); and the total number of employees in plant i in year t (column (d)). All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. 'Outcome Mean' refers to baseline figures, while 'Observations' refers to the number of plant×year observations.
*** $p < 0.01$, ** $p < 0.05$.

The effect on the employment stock of the other labor input, o, shown in column (c) of panel A, is mildly negative but statistically indistinguishable from zero and economically negligible. There is no evidence, therefore, of a substitution effect between the two inputs. The last column of Table 5 reveals that the net effect on total headcount employment is positive, with an increase in $\widehat{Absence}$ of 79 days leading to a statistically significant growth in each plant's overall employment of 2.29 (=0.029×79) workers, which represents a 4.7% increase over the baseline total headcount of nearly 49 employees.

Finally, the $\beta\gamma$ reduced form estimates in panel B are fully consistent with the previous results. For instance, the 0.064 estimate in column (a) is just the product of the 0.032 β estimate of panel A and the 1.874 γ estimate in column (a) of Table 4. As this consistency is confirmed across all our other outcomes in Table 5 as well as in the subsequent analysis, we do not report the reduced form results in what follows.

Next, we consider the impact on employment flows. Table 6 reports the 2SLS estimates for inflows and outflows of labor input f. A one standard deviation increase in $\widehat{Absence}$ leads to a 0.24 (= 0.003 × 79) significant increase in the number of young women hired (see column (a)). This represents a 150% growth in hiring with respect to the baseline average inflow of young women recruited in each firm over the sample period (i.e., about 7.5% every year). Should a woman aged 40 or less take a maternal leave of 330 days (just slightly above the median value of 302 days reported in Table 3), her employer will then replace her by hiring another young female employee. Likewise, a one standard deviation increase in $\widehat{Absence}$ is associated with a 0.47 (= 0.006 × 79) significant increase in the number of separations among women aged 40 or less (column (c)). This corresponds to a 116% increase in worker exits (or 5.8% each year), which could in part reflect the separation of leave-takers' replacements as leave-takers' return draws near (as emphasized also by Friedrich and Hackmann, 2021; Ginja et al., 2023; Schmutte and Skira, 2025, among others). The turnover of labor input f, thus, goes up significantly in response to longer parental leave related absence.

Table 6: The 2SLS Effect of Parental Leave Related Absence on Employment Flows of Women Aged 40 or Less, 1994–2013

	Inflows		Outflows	
	Headcount	Share	Headcount	Share
	(a)	(b)	(c)	(d)
$\widehat{Absence}_{i,l,j,t}$ (β)	0.003***	0.000***	0.006***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Outcome Mean	0.157	0.221	0.407	0.145
Observations	224,885	20,716	224,885	107,931

Notes: The dependent variables are: the number of newly hired women aged 40 or less (column (a)); the share of newly hired women aged 40 or less among all new hires (column (b)); the number of separations involving women aged 40 or less (column (c)); and the share of separations involving women aged 40 or less among all separations in plant i in year t (column (d)). The estimates are obtained from 2SLS regressions. All regressions control for plant, year, and industry×local labor market×year fixed effects. For other details, see the notes to Table 5.
*** $p < 0.01$.

Columns (b) and (d) of Table 6 refer to the share estimates for new hires and separations and confirm our results on headcounts. We find, instead, no evidence of an effect of parental leave induced absence on labor market turnover for the other labor input, o. These results are therefore not reported. Thus, not only is there imperfect substitutability of labor inputs within plants, but our estimates also suggest that companies are likely to benefit from recruiting and retaining young women, despite the potentially higher adjustment costs associated with them after childbirth.

6.3 Results on Corporate Outcomes

As parental-leave-induced predicted absence affects the long-term composition of the workforce in the average plant, corporate performance may then be directly influenced. Worker absence due to longer parental leave might lead companies to operate below potential, since interruptions could be associated with lower production efficiency or inferior investment dynamism. This could be the case if absence leads employers to recruit poorer quality replacements. Conversely, should firms adjust successfully to absence, they would develop greater flexibility and resilience and eventually their profitability could be impacted positively. To assess which of the two channels dominates, we estimate the long-term effect of parental leave related absence on a battery of firm-level outcomes, namely, operating profits, investment, sales, assets, and the firm's total wage bill. The last two outcomes are measured in logs, while the first three are defined in terms of the firm's total value added. Table 7 summarizes the 2SLS results.[20]

Table 7: The 2SLS Effect of Parental Leave Related Absence on Corporate Outcomes, 1995–2013

	(a) Operating Profits/ Total Assets	(b) Investment/ Total Assets	(c) Income from Sales/ Total Assets	(d) log(Value Added)	(e) log(Total Wage Bill)
$\widehat{Absence}_{i,lj,t}$ (β)	0.0001	0.0001**	0.0011**	-0.0017***	-0.0015***
	(0.0003)	(0.0000)	(0.0005)	(0.0005)	(0.0005)
Outcome Mean	0.060	0.018	2.170	9.692	9.464
Observations	137,635	137,635	137,635	135,689	136,173

Notes: The dependent variables are: operating profits divided by total assets (column (a)); investment divided by total assets (column (b)); income from sales divided by total assets (column (c)); log of value added, computed as the difference between operating profits and total payroll (column (d)); and log of total payroll of firm i in year t (column (e)). All regressions control for plant, year, and industry×local labor market×year fixed effects. In all regressions, standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.
*** $p < 0.01$, ** $p < 0.05$.

[20] We can only use a subsample of firms for this analysis because our corporate outcomes are not available for public sector firms and very small enterprises. We have reproduced the main results on employment stocks and flows, wages, part-time employment, and hours on this same subsample. The estimates from this robustness exercise are almost identical to the results reported in the paper, and are available upon request.

We detect no statistically significant effects on operating profits (column (a)), while investment and income from sales increase (columns (b) and (c), respectively). Both value added, computed as the difference between operating profits and total payroll, and the firm's total wage bill decrease with higher exposure to parental leave related absence at the plant level (columns (d) and (e)).[21] This last finding is in contrast to the short-term estimates presented by Ginja et al. (2023) and Brenøe et al. (2024), while Huebener et al. (2025) report a null result. To get a sense of these effects, consider the usual one standard deviation increase in parental leave related absence, which corresponds to 79 days. This leads to a significant increase in investment by 0.008, representing a substantial 44% increase over the baseline mean, and another significant increase in sales by 4% (=0.0011×79/2.17). The firm's wage bill declines by 12% (=–0.0015×79), which contributes to 90% of the reduction in real value added of about 13% (=–0.0017×79). Put differently, a company that has one young female worker taking the 100 additional days of leave made available cumulatively by the seven reforms would effect a wage bill reduction corresponding to almost two full-time-equivalent months over the 20-year post-reform period.[22]

In response to prolonged absence spells, therefore, firms go through a great deal of reorganization of their workforce, which entails a long-term expansion of the labor input most at risk of parental leave (i.e., women aged 40 or less), as we document in the previous subsection, accompanied by greater investment and sales and a lower total wage bill. These adjustments, which do not seem to have any impact on profits, echo some of the effects found by a growing body of research that focuses on firm responses to increases in the minimum wage and surveyed by Dube and Lindner (2024).

We draw attention to the negative effect on the wage bill, as this could suggest an important efficiency enhancing channel through which businesses adjust their employment strategy to longer absence. In the next section, we ask whether this comes about through a direct wage retrenchment of young women rather than a more nuanced employment policy, which could involve the recruitment of cheaper or more flexible types of labor, such as workers on part-time contracts. The positive impact on investment is also informative, as it is consistent with internal labor restructuring and organizational change and suggests firms' possible growing reliance on new workplace technologies and capital deepening (e.g., Autor et al., 2003; Bartel et al., 2007; Acemoglu and Guerrieri, 2008).

Before turning to the analysis of some of these channels in greater detail, we focus on the effect that our corporate outcome results might have on plant closure. It is possible,

[21] In line with the arguments put forward by Acemoglu and Restrepo (2019), the reduction in the wage bill could be directly driven by the negative effect on value added.

[22] This figure is calculated as follows: –[(100×0.0015)×NOK144,520]/[(NOK147,212/12)]=1.77, where NOK144,520 is the average wage bill (see Table 3) and NOK147,212 is the baseline real annual earnings for women aged below 40 (see Appendix Table A.1). The denominator is divided by 12 to back out a rough estimate of the monthly salary for young women.

for example, that the decline in value added, as shown in column (d) of Table 7, drives the weakest firms out of the market, keeping in mind that all the establishments in our sample had to survive up to 1995 by construction. To explore this issue, we re-estimate Equation (3) where the new outcome variable takes value 1 if plant i, which has survived up to year t, shuts down in the following year $t+1$, and 0 otherwise. The estimates are reported in Appendix Table C.2. Regardless of whether we use the full sample (as in Table 5) or the subsample for which the corporate outcomes are available (as in Table 7), the results unambiguously indicate that the likelihood of plant closure is unaffected by longer parental leave related worker absences. This is consistent with the findings shown in Brenøe et al. (2024) and Huebener et al. (2025), and it dispels the notion that longer worker absence imperils firm survival. Overall, therefore, we find no compelling evidence that parental leave induced worker absence has detrimental effects on firm performance.

7 Mechanisms

The previous analysis establishes that, in response to worker absence due to longer parental leave, firms adjust their workforce composition by expanding the employment of young women and experience a reduction in their total wage bill. Could this reflect a movement down the labor demand implying a reduction in young women's wages?

To address this question, we need to have an appropriate individual-level wage measure that reflects the actual payment made by employers, net of state benefits or other transfers from the government, including parental leave related payments. We thus use three measures of wages averaged within each plant i in each year t and separately for each of the two labor inputs $k = \{f, o\}$. The first, labeled 'income', is the annual total pay made by plant i in year t, net of transfers and parental leave benefits, which is likely to be a good proxy of the labor cost to the firm. A second measure, labeled 'earnings', is annual total taxable earnings comprising all types of formal remuneration from work, including overtime pay, performance-related pay and bonuses, and benefit income. The last measure, labeled 'FTE wages', is the total monthly income adjusted for hours of work, using the full-time equivalent (FTE) wage measure constructed by Statistics Norway with hours data from wage statistics information. This last measure is unavailable for small private firms, which explains the smaller sample size for this outcome.

Focusing on labor input f (i.e., women aged 40 or less), Table 8 reports the 2SLS estimates of the impact of parental leave induced absence on the three wage measures. Regardless of how wages are measured, the estimates are quantitatively indistinguishable from zero and never statistically significant at conventional levels. This means that the firm's employment expansion of labor input f is not accompanied by a wage retrenchment for this labor input. Appendix Table C.3 shows that this is the case also for the other labor input, o, and the same result emerges when we consider only men, regardless of age (see Appendix Table C.4). Moreover, there is no evidence of a significant impact on the within-firm wage gap between young women and the other labor input (i.e., women aged more than 40 and men of all ages) or on the wage gap between young women and men of all ages (see Appendix Tables C.5 and C.6, respectively).[23]

Table 8: The 2SLS Effect of Parental Leave Related Absence on Three Measures of Wages, Women Aged 40 or Less

	(a) Income	(b) Earnings	(c) FTE Wages
$\widehat{Absence}_{i,l,j,t}$ (β)	0.0001*	0.00002	0.001
	(0.000)	(0.000)	(0.001)
Outcome Mean	12.073	12.234	9.742
Observations	83,198	82,201	34,131

Notes: The dependent variables are: log of mean annual total pay net of transfers and parental leave benefits (column (a)); log of mean annual total taxable earnings comprising all types of formal remuneration from work, including overtime pay, performance-related pay and bonuses, and benefit income (column (b)); and log of mean total monthly income adjusted for hours of work (column (c), where 'FTE' stands for full-time equivalent). Each outcome is the within-plant mean computed over all female employees aged 40 or less in plant i in year t and is inflation-adjusted using the Norwegian consumer price index with 2000 as the base year. The sample period for income and earnings is 1994–2013 (columns (a) and (b), and for wage is 1997–2013 (column (c)), due to data availability. All regressions control for plant, year, and industry×local labor market×year fixed effects. In all regressions, standard errors are clustered at the local labor market level.
* $p < 0.10$.

Despite this null result at the mean, the estimates in Appendix Table C.8 indicate that there is a significant wage reduction for all workers in the bottom quartile of both the earnings and the FTE wage distributions, while there is barely any change in the upper quartile. This suggests a possible shift towards cheaper labor, such as part-timers. Employers, therefore, do not appear to combine the expansion of young women's recruitment with an adjustment of their wages or the wages of other labor inputs on average. They, however, tend to drop wages of all workers at the bottom of the distribution and leave it unchanged at the top. This suggests that, along with an increase in the supply, there is also an increase in the demand for young female workers, although possibly requiring different skill levels. It also suggests that the reduction in the wage bill we documented above is likely driven by other margins of adjustment.

[23] Repeating the analysis summarized in Table 8 without firm fixed effects leads to negative and statistically significant effects of absence on two of the three wage measures. Specifically, we find that a one standard deviation increase in $\widehat{Absence}$ leads to a 1% and a 1.6% decline in earnings and FTE wages, respectively, for women aged 40 or less over their baseline means. See Appendix Table C.7. There are, therefore, differential wage impacts *across* firms that may separately identify low- and high-paying organizations (see also Appendix Table C.8 for within-firm evidence across the wage distribution). These in turn could reflect response heterogeneity, which we will examine at the end of this section.

As anticipated, one of such margins could involve the recruitment of cheaper and more flexible types of labor, such as workers on part-time contracts. We thus repeat our standard analysis in which the $Y_{i,l,j,t}$ outcomes are given by stocks and flows of labor input f in part-time jobs, in terms of headcounts or shares. The plant-level 2SLS results presented in Table 9 do spotlight part-time employment as an important channel.

Table 9: The 2SLS Effect of Parental Leave Related Absence on Part-Time Employment, Women Aged 40 or Less, 1994–2013

	Stock		Inflow	
	Headcount (a)	Share (b)	Headcount (c)	Share (d)
$\widehat{Absence}_{i,l,j,t}$ (β)	0.01146***	0.0003***	0.00116***	0.00013***
	(0.00082)	(0.00006)	(0.00012)	(0.00003)
Outcome Mean	0.696	0.018	0.058	0.095
Observations	224,885	224,885	224,885	20,716

Notes: The dependent variables are: the number of women aged 40 or less employed on a part-time contract (column (a)); the within-plant share of women aged 40 or less employed on a part-time contract (column (b)); the number of newly hired women aged 40 or less working on a part-time contract (column (c)); and the share of newly hired women aged 40 or less working on a part-time contract among all new hires in firm i in year t (column (d)). Part-time employment is defined as working 30 or fewer hours per week. The analysis sample in column (d) only includes firms that have new hires. All regressions control for plant, year, and industry×local labor market×year fixed effects. In all regressions, standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.
*** $p < 0.01$.

A one standard deviation increase in $\widehat{Absence}$ leads to an increase of 0.91 (= 0.0115×79) young female part-timers (column (a)), which entails more than doubling the average part-time headcount over the post-reform period in the sample. This accounts for approximately one-third of the estimated rise in total young women's employment reported in column (a), panel A of Table 5. Similarly, a one standard deviation increase in predicted parental leave induced absence implies an expansion of 0.095 (= 0.0012 × 79) young female new hires on part-time contracts (column (c)), corresponding to an annual increase of approximately 8% over the baseline mean (i.e., (0.095×100/0.058)/20, where 20 represents the time period from 1994 to 2013). This specific sort of recruitment makes up about 40% of the total hiring undertaken by the average plant we report in Table 6. The results on shares of women aged 40 or less for both stocks of, and inflows into, part-time jobs, in columns (b) and (d) of Table 9 respectively, confirm the estimates on headcounts.[24] Appendix Table C.11 provides evidence of a positive effect also on part-time headcounts for labor input o

[24]Similar estimates to those shown in Table 9 emerge also if we consider short part-time employment (i.e., working less than 20 hours per week) for women aged 40 or less (see Appendix Table C.10).

(older women and men), although its share declines, indicating a relatively greater growth among young women.

Companies' greater reliance on young female part-timers, which corroborates the earlier evidence on reduced wages for workers in the bottom quartile of the distribution, could be achieved through an overall reduction in hours worked by young women (input f) and possibly other workers (input o). This in turn would translate into a lower wage bill, as we document in Table 7. We explore this possibility by analyzing whether firms adjust two dimensions of the intensive margin, that is, contractual average hours and overtime hours.

Table 10 reports the 2SLS estimates of this exercise. We find no evidence that firms achieve the observed reduction in their wage bill by changing either dimension of the intensive margin for labor input f (columns (a) and (b)). We also cannot detect a change in contractual hours affecting all other workers (column (c)). A one standard deviation increase in $\widehat{Absence}$, instead, is found to lead to a small growth in input o's overtime hours by 7.1 per year (=0.09×79), which corresponds to a 50% increase over a very low baseline mean (column (d)). This effect however is statistically significant only at the 10% level and, quantitatively, it is unlikely to explain the 12% wage bill cutback highlighted in subsection 6.3.

Table 10: The 2SLS Effect of Parental Leave Related Absence on Contractual Hours and Overtime Hours

	Women Aged 40 or Less (input f)		Other Workers (input o)	
	Weekly Hours (a)	Annual Overtime Hours (b)	Weekly Hours (c)	Annual Overtime Hours (d)
$\widehat{Absence}_{i,l,j,t}$ (β)	-0.014	0.019	0.045	0.090*
	(0.016)	(0.012)	(0.116)	(0.052)
Outcome Mean	31.944	12.986	34.124	14.418
Observations	10,470	784	88,986	25,377

Notes: The dependent variables are: mean (contractual) weekly working hours for women aged 40 or less (column (a)); mean yearly overtime hours for women aged 40 or less (column (b)); mean (contractual) weekly working hours for women aged above 40 and all men (column (c)); and mean yearly overtime hours for women age above 40 and all men employed in plant i in year t (column (d)). All regressions control for plant, year, and industry×local labor market×year fixed effects. The sample period in columns (a) and (c) is 2004–2013, while in columns (b) and (d) it is 2009–2013, due to data availability. In all regressions, standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.
* $p < 0.10$.

Is it possible that businesses achieve their employment adjustment through a change in the composition of their workforce human capital? Could human capital depreciation, which is possibly due to longer absences, contribute to the observed higher labor turnover and firms' greater reliance on part-time employment? To address these questions, we analyze companies' responses in terms of the average level of education, average tenure, and average work experience of both newly hired employees ('incomers') and employees separating from the firm ('leavers'). The 2SLS results are in Table 11.[25] In response to prolonged parental leave induced worker absence, firms recruit workers with educational levels and labor market experience comparable to those of their incumbent employees (columns (a) and (d), respectively). From Table 6, we know most of these new hires are women aged 40 or less who replace young female leave-takers.

Table 11: The 2SLS Effect of Parental Leave Related Absence on Firms' Workforce Human Capital, 1994–2013

	Education		Firm Tenure	Work Experience	
	Incomers	Leavers	Leavers	Incomers	Leavers
	(a)	(b)	(c)	(d)	(e)
$\widehat{Absence}_{i,l,j,t}$ (β)	0.00001	0.00026***	-0.00002	-0.00068	-0.00145**
	(0.00007)	(0.00008)	(0.00039)	(0.00047)	(0.00071)
Outcome Mean	2.27941	2.11264	7.02356	17.65730	23.40788
Observations	20,683	107,701	107,926	20,717	107,926

Notes: The dependent variables are: average education level of newly hired and separating employees (incomers and leavers, columns (a) and (b), respectively); average years of firm tenure among leavers (column (c)); and average work experience of incomers and leavers (columns (d) and (e), respectively). Tenure and work experience are measured in years. Education is measured in three categories (=1 if elementary/compulsory, =2 if high school/some college, =3 if university or higher). All regressions control for plant, year, and industry×local labor market×year fixed effects. In all regressions, standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.
*** $p < 0.01$, ** $p < 0.05$.

Employers instead face both a relatively larger outflow of better educated workers (column (b)) and a more contained exit of more experienced employees (column (e)), perhaps because businesses end up retaining more of them. These effects however are quantitatively small. A one standard deviation increase in $\widehat{Absence}$, in fact, implies a 0.02 (=0.00026×79) increase in the education level among the group of leavers, which represents a 1% increase over the baseline mean, and a 0.11 (=-0.00145×79) year reduction in leavers' work experience, corresponding to a 0.5% decrease with respect to the baseline mean. Overall, therefore, the human capital composition of the workforce of the average plant does not seem to be much affected by prolonged parental leave induced absence, suggesting that employers neither lower nor raise their hiring standards. This is poignant, given that businesses faced an increasingly better educated workforce over the sample period. At the

[25]The estimates for education come from a linear model, but they are unaltered if we fit an ordered probit model. The results are available from the authors.

same time, companies appear to be unscathed by the likely human capital depreciation of their workforce induced by longer absences.

To sum up, firms respond to parental leave related worker absence with an expansion of their workforce, substituting leave takers with young female employees, who are more at risk of absence than other workers. Companies do not face any change in profits, while they enhance investments and lower their wage bill. These long-term adjustments do not seem to operate through a reduction in young women's wages on average (and also not in other workers' average pay, although we find evidence of lower wages in the bottom quartile of the distribution), or adjustments along the intensive margin of labor supply, or a different human capital composition of the workforce.

Businesses, however, tend to retain and recruit more young women in part-time jobs. Although we find no evidence of intra-firm wage differentials among young and older women or gender pay gaps, typically part-time employment tends to reflect lower occupational skill requirements (e.g., Hirsch, 2005; Blau et al., 2024), flatter career profiles with only modest promotion opportunities (e.g., Kunze and Miller, 2017; Ellingsæter and Jensen, 2019), and to be more concentrated in low-pay sectors. This is consistent with the finding of a negative wage response in the bottom quartile of the pay distribution. Interestingly, firms accompany this expansion with a 44% boost in their investments, which over the sample period is likely to have coincided with greater adoption of new workplace information technologies that would have changed the task content of production (see Autor, 2015; Acemoglu and Restrepo, 2019, among others).

Heterogeneity — We conclude by focusing on the possibility of heterogeneous responses. We examine heterogeneity by plant size, industry, sector (i.e., private companies versus public enterprises) and CEO gender, as these types of stratification can reveal important differences in the way companies adjust to worker absence. To be parsimonious with this exercise, we consider the effects of parental leave related absence on firms' employment share, part-time employment share, and recruitment of women aged 40 or less as well as on companies' wage bill. The 2SLS estimates are summarized in Appendix Figures C.1–C.3. The results on the other outcomes are available from the authors.

Most of the positive overall effects on both employment and part-time employment shares of women aged 40 or less, shown in Tables 5 and 9 respectively, are observed among small businesses (i.e., firms with 50 or fewer employees at baseline). These companies also carry out most of the recruitment of the same group of workers and face the largest reduction in their wage bill. While the impacts on total and part-time employment are more pronounced among private firms and companies managed by women, the effects on hiring and wage bill are shared across all enterprises, regardless of ownership type or pay-leader gender. There is little evidence of heterogeneous impacts by industry, except that the employment response for young women on part-time contracts is particularly strong among employers in the manufacturing sector.

Small and private firms and businesses where the CEO is a woman tend to be the main drivers of the response to the observed parental leave induced worker absence. Perhaps, this is not surprising, given that small enterprises tend to face more competitive environments and may have to rely on a more flexible structure to make the necessary organizational change and minimize the adjustment costs associated with a labor expansion, while counting less on internal redeployment (e.g., O'Reilly III and Tushman, 2008; Galdon-Sanchez et al., 2025).[26] Public sector organizations with high exposure to longer absence, on the other hand, might have less flexibility (or more built-in institutional provision) and harder budget constraints to respond to such interruptions (for similar findings on public firms in Sweden, see Ginja et al., 2023). Finally, female-led firms may be better at interpreting signals of productivity from female workers (e.g., Kunze and Miller, 2017; Flabbi et al., 2019) or more effective in mentoring female subordinates (e.g., Benson et al., 2024).

8 Conclusion

We study firms' long-term responses to worker absence due to parental leave. We rely on a large variation in parental leave provision in Norway, leveraging the extension of job-protected maternal leave around childbirth from 18 to 38 weeks with full, government-funded wage replacement through seven successive reforms between 1987 and 1993. Using administrative matched employer-employee data between 1983 and 2013, we identify the causal impact of parental leave related absence using a shift-share design, which combines labor-input-specific changes in absence within a local labor market across plants in different industries (shifts) with variation in firms' exposure to interruptions given by their historical, pre-sample age-gender employment mix (shares). We thus take advantage of the fact that firms face different degrees of labor market gender segregation and are exposed to leave reforms at different intensities.

We find large employment effects involving young women, the group of workers who are more likely to be leave-takers. A one standard deviation increase in expected absence implies a statistically significant twofold increase in the headcount, and a 90% growth in the share, of women aged 40 or less (referred to as 'young women') in each plant over the post-reform period, between 1994 and 2013. A parental leave of 300 days, which represents the mean absence taken by young women in the sample, would inflate the

[26]Since larger multi-plant companies (about 20% of the firms in our sample; see Appendix E) could more readily absorb worker absence and call on internal labor reallocation, we repeated the analysis on single-plant firms. The estimates from this subsample (not shown, but available from the authors) confirm our previous findings and strengthen the interpretation that internal redeployment, albeit unlikely to explain our employment expansion results, could happen in larger organizations.

previous figures by a factor of four. Firms manage this adjustment with increased labor turnover, generating an average 7.5% annual expansion in hiring and just less than 6% annual increase in separations, which could in part reflect the departure of leave-takers' replacements as leave-takers' return draws near. Companies therefore do not substitute away from young female employees, even though they may give rise to greater adjustment costs.

Although these changes tend to leave the likelihood of plant closure and the growth of corporate profits unaffected, they are accompanied by greater firms' investments and sales and by a lower value added and a lower total wage bill. Taken together, such effects suggest a major efficiency enhancing strategy that businesses bring in to face prolonged worker absence. On the one hand, the growth in investment may reveal a greater reliance on new workplace technologies and capital deepening. On the other hand, the reduction in the wage bill and value added could indicate internal labor restructuring, which could happen even without a decline in the labor share (Acemoglu and Restrepo, 2019). We find evidence that one powerful channel of this restructuring is the significant expansion in the use of young female part-time employment, which is known to offer limited career progression and to be concentrated in low-pay sectors.

Small private enterprises and female-led businesses tend to be the main drivers of the observed adjustments. This evidence indicates a great deal of flexibility on the part of nimble companies facing a high exposure to longer parental leave induced worker absence. It also suggests potential vulnerability for young female workers, as they go through greater turnover and a higher likelihood of being on part-time contracts, which can on average require lower skills and lead to lower pay over time. This is supported by a negative wage response at the bottom of the pay distribution, even though there is no change at the mean or at the top. Governments that view expansions in family leave entitlements as a key policy tool to promote gender equality in the labor market may need to take account of these results which could potentially undermine their goals.

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Online Appendix

A Additional Descriptives

Table A.1: Descriptive Statistics (1983-1986)

Panel I: Employment Variables	Mean	SD	Obs.
Number of women below 40	9.458	41.717	58503
Employment share of women below 40	0.304	0.244	58503
Inflow share, women below 40	0.382	0.340	50336
Outflow share, women below 40	0.396	0.380	45425
Employment share of older women and all men	0.696	0.244	58503
Firm size	31.397	110.668	58503
Proportion of female employees	0.472	0.317	58503
Proportion of part-time employees	0.217	0.230	58503
Proportion of short part-time employees	0.100	0.148	58503
Panel II: Income Variables			
Real annual earnings	199316.149	60523.346	58443
Real annual earnings of women below 40	147212.386	45977.182	47319
Real annual earnings of older women and all men	219116.264	70967.684	57492

Notes: This figure describes the sample of analysis: plants that exist in at least one of the pre-reform years (1983-86), have more than 3 employees in the pre-reform years, and exist in the first two post-reform years 1994 and 1995. The window of observation is 1983-1986.

B Falsification Exercise Using Sickness Leave Duration

Table B.1: First-stage Results with Sickness Leave: 1994–2013

	(a) Total SLdays	(b) Mean SLdays	(c) Median SLdays	(d) SL (0,1)	(e) B with s_t
$B^{SL}_{i,l,j,t}$	-0.951***	-0.432***	-0.431***	0.000	0.128***
	(0.255)	(0.062)	(0.063)	(0.000)	(0.012)
F-test	13.916	48.241	45.998	0.160	116.095
Outcome Mean	323.842	70.553	63.344	0.598	116.600
Observations	218,617	218,617	218,617	218,617	218,617

Notes: The dependent variables are: the number of total sickness leave days taken by all employees in firm i in year t (column (a)); the mean sickness leave days taken in firm i in year t (column (b)); the median sickness leave days taken in firm i in year t (column (c)); whether any sickness leave was taken in firm i in year t (column (d)); and a Bartik-type measure of sickness leave exposure computed using contemporaneous (time t) labor shares rather than historical shares (column (e)). All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. 'Outcome Mean' refers to baseline figures, while 'Observations' refers to the number of plant×year observations.
*** $p < 0.01$.

Table B.2: The Effect of Sickness Leave Absence on Firm Employment Stocks

	Women Below 40		All Others	All
	Headcount (a)	Share (b)	Headcount (c)	Headcount (d)
Panel A: 2SLS Estimates				
$\widehat{Absence}^{SL}{}_{i,l,j,t}$	0.00786*	-0.00001	-0.00350	-0.00013
	(0.00465)	(0.00015)	(0.01551)	(0.00019)
Panel B: Reduced Form Estimates				
$B^{SL}_{i,l,j,t}$	-0.00748	0.00001	0.00333	0.00012
	(0.00571)	(0.00014)	(0.01466)	(0.00016)
Outcome Mean	2.28623	0.08997	46.65311	2.95602
Observations	218,617	218,617	218,617	218,617

Notes: The dependent variables are: the number of women aged below 40 employed (column (a)); the share of women below 40 employed (column (b)); the number of older women (above 40) and all men employed (column (c)); and the total number of employees (column (d)) in firm i in year t. The mean sickness leave duration for women aged below 40 years is 98 days, while it is 120 days for older women all men. All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level.
* $p < 0.01$.

Table B.3: The Effect of Sickness Leave Absence on Employment Flows, Women Aged 40 or Less

	Inflows		Outflows	
	Headcount	Share	Headcount	Share
	(a)	(b)	(c)	(d)
Panel A: 2SLS Estimates				
$\widehat{Absence}^{SL}{}_{i,l,j,t}$	0.000	0.000	0.000	-0.000
	(0.001)	(0.000)	(0.001)	(0.000)
Panel B: Reduced Form Estimates				
$B^{SL}_{i,l,j,t}$	-0.000	-0.001	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.000)
Outcome Mean	0.157	0.221	0.407	0.145
Observations	218,617	20,531	218,617	106,230

Notes: The dependent variables are: the number of newly hired women aged 40 or less (column (a)); the share of newly hired women aged 40 or less among all new hires (column (b)); he number of separations involving women aged 40 or less (column (c)); and the share of separations involving women aged 40 or less among all separations in plant i in year t (column (d)). For other details, see the notes to Appendix Table B.2.

Table B.4: The Effect of Sickness Leave Absence on Part-Time Employment, Women Aged 40 or Less

	Stock		Inflow	
	Headcount	Share	Headcount	Share
	(a)	(b)	(c)	(d)
Panel A: 2SLS Estimates				
$\widehat{Absence}^{SL}{}_{i,l,j,t}$	0.00369**	-0.00004	0.00007	-0.00002
	(0.00153)	(0.00005)	(0.00024)	(0.00008)
Panel B: Reduced Form Estimates				
$B^{SL}_{i,l,j,t}$	-0.00352*	0.00004	-0.00007	0.00018
	(0.00202)	(0.00004)	(0.00023)	(0.00061)
Outcome Mean	0.696	0.018	0.058	0.095
Observations	218,617	218,617	218,617	20,531

Notes: The dependent variables are: the number of women aged 40 or less employed on a part-time contract (column (a)); the within-plant share of women aged 40 or less employed on a part-time contract (column (b)); the number of newly hired women aged 40 or less working on a part-time contract (column (c)); and the share of newly hired women aged 40 or less working on a part-time contract among all new hires in firm i in year t (column (d)). Part-time employment is defined as working 30 or fewer hours per week. For other details, see the notes to Table 9 and Appendix Table B.2.
** $p < 0.05$, * $p < 0.10$.

C Additional Results

Table C.1: The 2SLS Effect of Parental Leave Related Absence on Employment, Men Aged 40 or Less (Stocks): 1994–2013

	Headcount	Share
	(a)	(b)
$\widehat{Absence}_{i,l,j,t}$	-0.01311***	-0.00066***
	(0.00172)	(0.00006)
Outcome Mean	3.08053	0.12509
Observations	224,885	224,885

Notes: The dependent variables are: the number of men aged 40 or less (column (a); the share of men aged 40 or less employed in firm i in year t (column (b)). Both regressions control for firm, year, and industry×local labor market×year fixed effects. The estimates are obtained from 2SLS regressions. Standard errors are clustered at the local labor market level.
*** $p < 0.01$.

Table C.2: The Effect of Parental Leave Related Absence on Plant Closures: 1994–2013

	Full Sample		Corp. Outcomes Sample	
	(a)	(b)	(c)	(d)
Panel I: 2SLS				
$\widehat{Absence}_{i,l,j,t}$	-0.00001	-0.00003***	0.00001	-0.00004***
	(0.00001)	(0.00001)	(0.00003)	(0.00001)
Panel II: Reduced Form				
$B_{i,l,j,t}$	-0.00002	-0.00004***	0.00002	-0.00004***
	(0.00003)	(0.00001)	(0.00003)	(0.00001)
Outcome Mean	0.03346	0.03346	0.03346	0.03346
Observations	224,865	225,146	137,652	137,652
Firm FE	YES	NO	YES	NO

Notes: The dependent variable is a binary indicator of plant closure of firm i in year $t+1$. Columns (a) and (c) control for plant, year, and industry×local labor market×year fixed effects. Columns (b) and (d) control for year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level.
*** $p < 0.01$.

Table C.3: The 2SLS Effect of Parental Leave Related Absence on Three Measures of Wages, Women Aged More than 40 and All Men

	Income	Earnings	Wage
	(a)	(b)	(c)
$\widehat{Absence}_{i,l,j,t}$	0.000***	0.000***	-0.000
	(0.000)	(0.000)	(0.000)
Outcome Mean	12.498	12.583	9.959
Observations	222,279	221,938	113,235

Notes: The dependent variables are: log of mean annual total pay net of transfers and parental leave benefits (column (a)); log of mean annual total taxable earnings comprising all types of formal remuneration from work, including overtime pay, performance-related pay and bonuses, and benefit income (column (b)); and log of mean total monthly income adjusted for hours of work (column (c), where 'FTE' stands for full-time equivalent). Each outcome is the within-plant mean computed over all female employees aged more than 40 and male employees of all ages in plant i in year t and is inflation-adjusted using the Norwegian consumer price index with 2000 as base year. The sample period for income and earnings is 1994—2013 (columns (a) and (b), and for wage is 1997—2013 (column (c)), due to data availability. The estimates are obtained from 2SLS regressions. Standard errors are clustered at the local labor market level. All regressions control for plant, year, and industry×local labor market×year fixed effects.
*** $p < 0.01$.

Table C.4: The 2SLS Effect of Parental Leave Related Absence on Three Measures of Wages, Men Aged 40 or Less

	Income	Earnings	Wage
	(a)	(b)	(c)
$\widehat{Absence}_{i,l,j,t}$	-0.00002	-0.00001	-0.00008
	(0.00005)	(0.00003)	(0.00009)
Outcome Mean	12.50646	12.57537	9.97714
Observations	94,626	94,433	33,485

Notes: The dependent variables are as in Appendix Table C.3 but computed on men aged 40 or less. The estimates are obtained from 2SLS regressions. Standard errors are clustered at the local labor market level. For other details, see the notes to Appendix Table C.3.

Table C.5: The 2SLS Effect of Parental Leave Related Absence on the Wage Gap between Labor Inputs, (f versus o)

	Income	Earnings	Wage
	(a)	(b)	(c)
$\widehat{Absence}_{i,l,j,t}$	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Outcome Mean	0.354	0.270	0.185
Obsservations	81,171	80,164	32,984

Notes: The dependent variables are: within-firm income gap (column (a)); within-firm earnings gap (column (b)); and within-firm wage gap (column (c)) among older women and all men (labor input o) and women aged 40 or less (input f) employed in firm i in year t. The estimates are obtained from 2SLS regressions. Standard errors are clustered at the local labor market level. All regressions control for plant, year, and industry×local labor market×year fixed effects. For other details, see the notes to Appendix Table C.3.

Table C.6: The 2SLS Effect of Parental Leave Related Absence on the Wage Gap between Men (of All Ages) and Women Aged 40 or Less

	Income	Earnings	Wage
	(a)	(b)	(c)
$\widehat{Absence}_{i,l,j,t}$	-0.00020*	-0.00011*	-0.00007
	(0.00011)	(0.00006)	(0.00008)
Outcome Mean	0.51216	0.42546	0.33042
Observations	71,067	70,414	27,592

Notes: See the notes to Appendix Table C.5.
* $p < 0.10$.

Table C.7: The 2SLS Effect of Parental Leave Related Absence on Three Measures of Wages without Firm FE, Women Aged 40 or Less

	Income	Earnings	Wage
	(a)	(b)	(c)
$\widehat{Absence}_{i,l,j,t}$	-0.000	-0.001***	-0.002***
	(0.000)	(0.000)	(0.001)
Outcome Mean	12.073	12.234	9.742
Observations	84,188	83,178	35,698

Notes: For details, see the notes to Table 5 or Appendix Table C.3.
*** $p < 0.01$.

Table C.8: The 2SLS Effect of Parental Leave Related Absence on Three Measures of Wages, 25^{th} and 75^{th} Percentiles

	Income		Earnings		Wage	
	25p	75p	25p	75p	25p	75p
	(a)	(b)	(c)	(d)	(e)	(f)
$\widehat{Absence}_{i,l,j,t}$	-0.0002*	0.0000	-0.0001**	-0.0000	-0.0003**	-0.0002*
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0001)
Outcome Mean	12.183	12.652	12.336	12.707	9.753	10.077
Observations	224,249	224,311	223,975	223,975	114,445	114,453

Notes: The dependent variables are: log of mean annual total pay net of transfers and parental leave benefits (columns (a)–(b)); log of mean annual total taxable earnings comprising all types of formal remuneration from work, including overtime pay, performance-related pay and bonuses, and benefit income (columns (c)–(d)); and log of mean total monthly income adjusted for hours of work (columns (e)–(f), where 'FTE' stands for full-time equivalent). Each outcome is the within-plant bottom (25p) and top (75p) quartile computed over all employees in plant i in year t and is inflation-adjusted using the Norwegian consumer price index with 2000 as base year. The sample period for income and earnings is 1994–2013 (columns (a)–(d)), and for wage is 1997–2013 (columns (e)–(f)), due to data availability. All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.
** $p < 0.05$, * $p < 0.10$.

Table C.9: The 2SLS Effect of Parental Leave Related Absence on Three Measures of Wages of Young Women, 25^{th} and 75^{th} Percentiles

	Income		Earnings		Wage	
	25p	75p	25p	75p	25p	75p
	(a)	(b)	(c)	(d)	(e)	(f)
$\widehat{Absence}_{i,l,j,t}$	0.0007*	0.0004*	0.0002	0.0002	0.0003	0.0009
	(0.0004)	(0.0002)	(0.0002)	(0.0001)	(0.0007)	(0.0011)
Outcome Mean	11.828	12.217	12.092	12.336	9.626	9.824
Observations	83,164	83,198	82,201	82,201	34,125	34,129

Notes: Each outcome is the within-plant bottom (25p) and top (75p) quartile computed over all female employees aged 40 or less in plant i in year t and is inflation-adjusted using the Norwegian consumer price index with 2000 as base year. For other details, see the notes to Appendix Table C.8.
* $p < 0.10$.

Table C.10: The 2SLS Effect of Parental Leave Related Absence on Short Part-time Employment of Women Aged 40 or Less, 1994–2013

	Stock		Inflow	
	Headcount	Share	Headcount	Share
	(a)	(b)	(c)	(d)
$\widehat{Absence}_{i,l,j,t}$	0.00609***	0.00014***	0.00074***	0.00094***
	(0.00043)	(0.00003)	(0.00009)	(0.00016)
Outcome Mean	0.338	0.009	0.036	0.399
Observations	224,885	224,885	224,885	20,716

Notes: The dependent variables are: the number of women aged 40 or less employed on a short part-time contract (column (a)); the within-plant share of women aged 40 or less employed on a short part-time contract (column (b)); the number of newly hired women aged 40 or less working on a short part-time contract (column (c)); and the share of newly hired women aged 40 or less working on a short part-time contract among all new hires in firm i in year t (column (d)). Short part-time employment is defined as working 20 or fewer hours per week. All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. For other details, see the notes to Table 9.
*** $p < 0.01$.

Table C.11: The 2SLS Effect of Parental Leave Related Absence on Part-time Employment of Older Women and All Men, 1994-2013

	Stock		Inflow	
	Headcount	Share	Headcount	Share
	(a)	(b)	(c)	(d)
$\widehat{Absence}_{i,l,j,t}$	0.00470***	-0.00006**	0.00010	-0.00017***
	(0.00106)	(0.00002)	(0.00010)	(0.00005)
Outcome Mean	2.482	0.073	0.072	0.164
Observations	224,885	224,885	224,885	20,716

Notes: Part-time employment is defined as working 30 or fewer hours per week. For other details, see the notes to Table 9.
*** $p < 0.01$, ** $p < 0.05$.

Table C.12: First-Stage Estimates (Shorter Time Horizon), 1994–2004

	Total PLdays	Mean PLdays	Median PLdays	PL (0,1)	B with s_t
	(a)	(b)	(c)	(d)	(e)
$B_{i,l,j,t}$	2.673***	0.585***	0.618***	0.001***	0.632***
	(0.773)	(0.164)	(0.178)	(0.000)	(0.081)
F-test	11.941	12.697	12.120	9.603	60.943
Outcome Mean	120.339	45.028	44.785	0.264	44.919
Observations	145,022	145,022	145,022	145,022	145,022

Notes: The dependent variables are: the number of total parental leave days taken by all employees in plant i in year t (column (a)); the mean parental leave days taken by employees in plant i in year t (column (b)); the median parental leave days taken by employees in plant i in year t (column (c)); whether any parental leave was taken in plant i in year t (=1, if yes, and =0, otherwise; column (d)); and a Bartik-type measure of parental leave exposure computed using contemporaneous (time t) labor shares rather than historical shares (column (e)). All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. 'Outcome Mean' refers to baseline figures, while 'Observations' refers to the number of plant×year observations.
*** $p < 0.01$.

Table C.13: The Effect of Parental Leave Related Absence on Firm Employment Stocks (Shorter Time Horizon), 1994–2004

	Women Aged 40 or Less (input f)		Other Workers (input o)	All Workers (both f and o)
	Headcount	Share	Headcount	Headcount
	(a)	(b)	(c)	(d)
Panel A: 2SLS Estimates				
$\widehat{Absence}_{i,l,j,t}$	0.03453***	0.00091***	-0.01142	0.02312
	(0.00153)	(0.00015)	(0.01753)	(0.01815)
Panel B: Reduced Form Estimates				
$B_{i,l,j,t}$	0.09230***	0.00242***	-0.03051	0.06178
	(0.02446)	(0.00033)	(0.05024)	(0.04558)
Outcome Mean	3.45004	0.12898	42.17983	45.62987
Observations	145,022	145,022	145,022	145,022

Notes: The dependent variables are: the number of employed women aged 40 or less (column (a), input f); the share of employed women aged 40 or less (column (b)); the number of older women (aged more than 40) and all men (column (c), input o); and the total number of employees in plant i in year t (column (d)). All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. 'Outcome Mean' refers to baseline figures, while 'Observations' refers to the number of plant×year observations.
*** $p < 0.01$.

Table C.14: The 2SLS Effect of Parental Leave Related Absence on Firm Employment Flows, Women Aged 40 or Less (Shorter Time Horizon, 1994–2004)

	Inflows		Outflows	
	Headount	Share	Headcount	Share
	(a)	(b)	(c)	(d)
$\widehat{Absence}_{i,l,j,t}$	0.004***	0.000***	0.007***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Outcome Mean	0.242	0.274	0.625	0.194
Observations	145,022	14,837	145,022	77,994

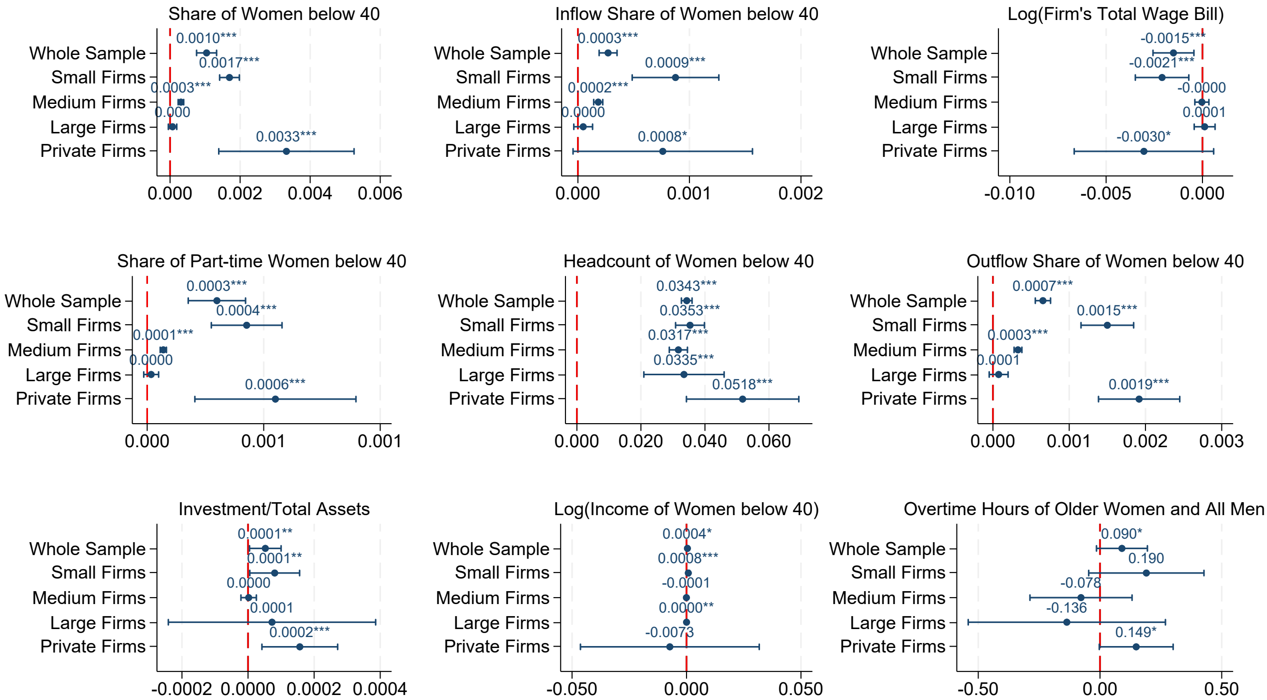
Notes: The dependent variables are: the number of newly hired women aged 40 or less (column (a)); the share of newly hired women aged 40 or less among all new hires (column (b)); the number of separations involving women aged 40 or less (column (c)); and the share of separations involving women aged 40 or less among all separations in plant i in year t (column (d)). The estimates are obtained from 2SLS regressions. Standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.
*** $p < 0.01$.

Table C.15: The 2SLS Effect of Parental Leave Related Absence on Part-Time Employment, Women Aged 40 or Less (Shorted Time Horizon, 1994–2004)

	Stock		Inflow	
	Headcount	Share	Headcount	Share
	(a)	(b)	(c)	(d)
$\widehat{Absence}_{i,l,j,t}$	0.01140***	0.00036***	0.00172***	0.00010*
	(0.00102)	(0.00009)	(0.00023)	(0.00005)
Outcome Mean	1.054	0.028	0.089	0.118
Observations	145,022	145,022	145,022	14,837

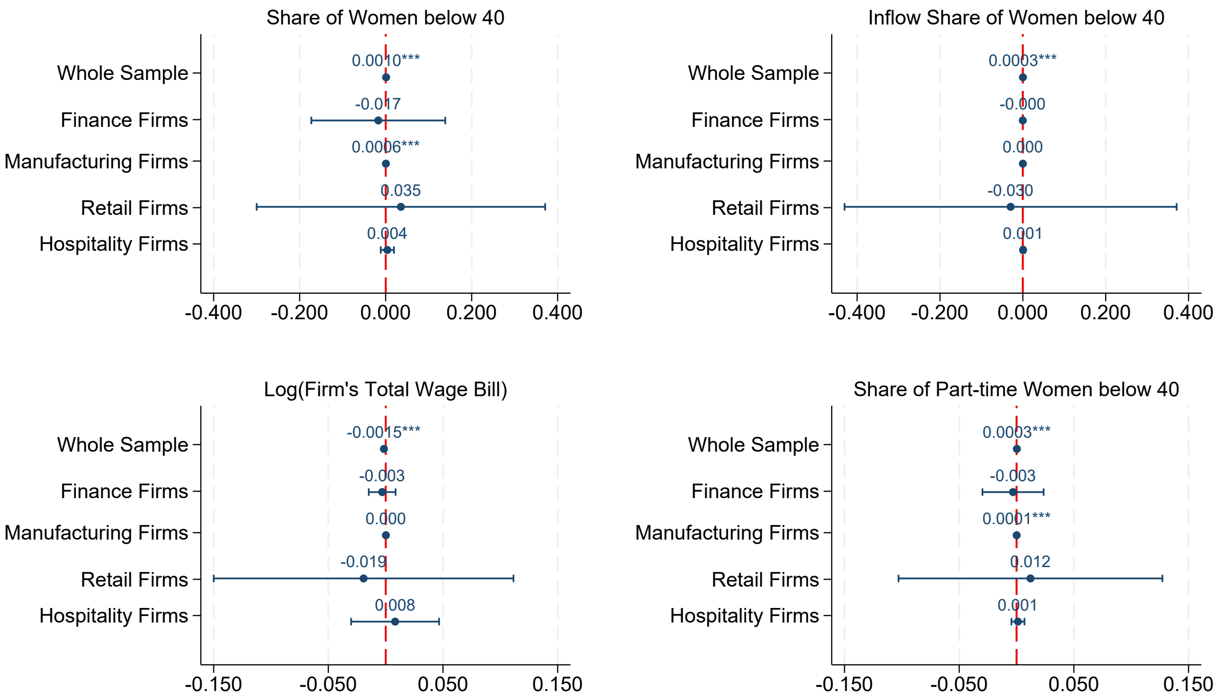
Notes: The dependent variables are: the number of women aged 40 or less employed on a part-time contract (column (a)); the within-plant share of women aged 40 or less employed on a part-time contract (column (b)); the number of newly hired women aged 40 or less working on a part-time contract (column (c)); and the share of newly hired women aged 40 or less working on a part-time contract among all new hires in firm i in year t (column (d)). Part-time employment is defined as working 30 or fewer hours per week. The analysis sample in column (d) only includes firms that have new hires. All regressions control for plant, year, and industry×local labor market×year fixed effects. The estimates are obtained from 2SLS regressions. Standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.
*** $p < 0.01$, * $p < 0.10$.

Figure C.1: Heterogeneity by Firm Size and Sector



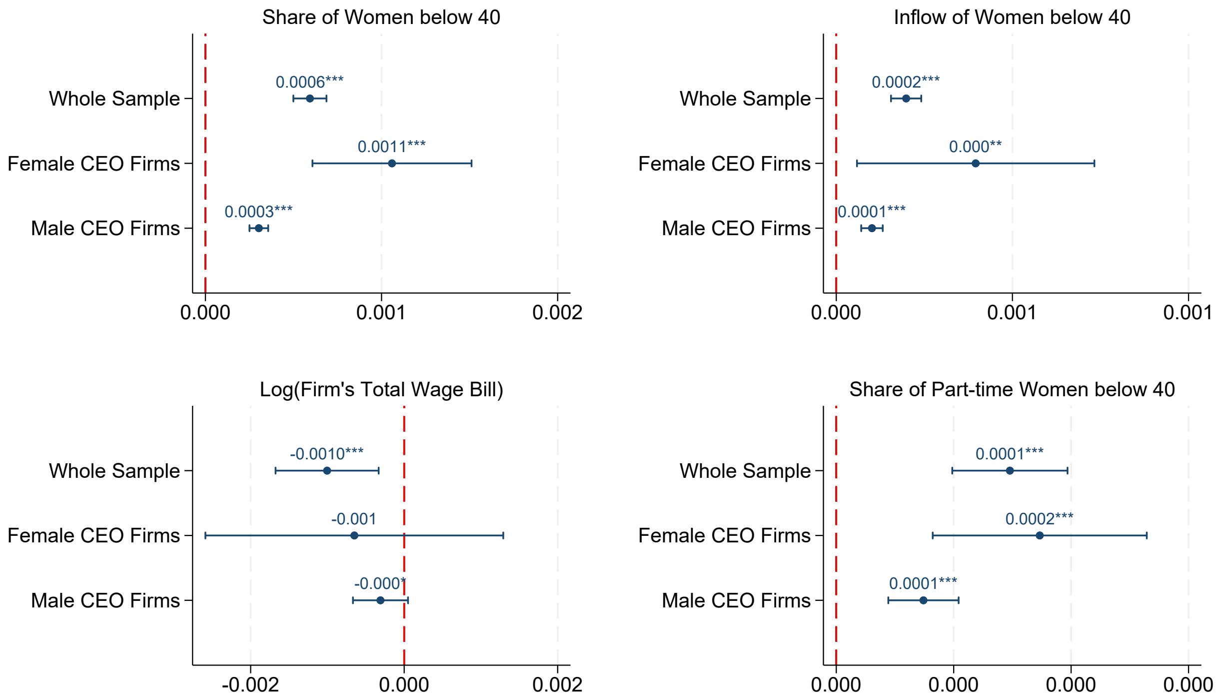
Notes: The figure plots the 2SLS estimates of $\widehat{Absence}_{i,l,j,t}$ (β) on selected outcomes. Firm size refers to pre-reform firm size, measured in 1986. Firm size categories are defined as: small (50 or fewer employees), medium (51-250 employees), and large (>250 employees).

Figure C.2: Heterogeneity by Industry



Notes: The figure plots the 2SLS estimates of $\widehat{Absence}_{i,l,j,t}$ (β) on selected outcomes.

Figure C.3: Heterogeneity by CEO Gender



Notes: The figure plots the 2SLS estimates of $\widehat{Absence}_{i,l,j,t}$ (β) on selected outcomes. CEO gender refers to the gender of the highest paid employee in the firm. All analyses exclude micro-firms (i.e., companies with fewer than 10 employees), based on firm size measured in 1986.

D Robustness Exercise: LIML Estimates

Table D.1: The LIML Effect of Parental Leave Related Absence on Firm Employment Stocks, 1994–2013

	Women Aged 40 or Less (input f)		Others Workers (input o)	All Workers (both f and o)
	Headcount (a)	Share (b)	Headcount (c)	Headcount (d)
$\widehat{Absence}_{i,l,j,t}$	0.03436***	0.00105***	-0.00508	0.00033*
	(0.00284)	(0.00021)	(0.02530)	(0.00019)
Observations	224,869	224,869	224,869	224,869

Notes: The dependent variables are: the number of employed women aged 40 or less (column (a), input f); the share of employed women aged 40 or less (column (b)); the number of older women (aged more than 40) and all men (column (c), input o); and the total number of employees in plant i in year t (column (d)). All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. 'Observations' refers to the number of plant×year observations.
*** $p < 0.01$, * $p < 0.10$.

Table D.2: The LIML Effect of Parental Leave Related Absence on Firm Employment Flows, Women Aged 40 or Less (1994–2013)

	Inflows		Outflows	
	Headcount (a)	Share (b)	Headcount (c)	Share (d)
$\widehat{Absence}_{i,l,j,t}$	0.003***	0.000*	0.006***	0.000***
	(0.001)	(0.000)	(0.001)	(0.000)
Observations	224,869	20,725	224,869	107,940

Notes: The dependent variables are: the number of newly hired women aged 40 or less (column (a)); the share of newly hired women aged 40 or less among all new hires (column (b)); the number of separations involving women aged 40 or less (column (c)); and the share of separations involving women aged 40 or less among all separations in plant i in year t (column (d)). All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level.
*** $p < 0.01$, * $p < 0.10$.

Table D.3: The LIML Effect of Parental Leave Related Absence on Corporate Outcomes, 1995–2013

	(a) Operating Profits/ Total Assets	(b) Investment/ Total Assets	(c) Income from Sales/ Total Assets	(d) log(Value Added)	(e) log(Total Wage Bill)
$\widehat{Absence}_{i,l,j,t}$	0.0001	0.0001*	0.0014	-0.0019**	-0.0019**
	(0.0012)	(0.00006)	(0.0080)	(0.0009)	(0.0009)
Observations	137,628	137,628	137,628	135,683	136,167

Notes: The dependent variables are: operating profits divided by total assets (column (a)); investment divided by total assets (column (b)); income from sales divided by total assets (column (c)); log of value added, computed as the difference between operating profits and total payroll (column (d)); and log of total payroll of firm i in year t (column (e)). All regressions control for plant, year, and industry×local labor market×year fixed effects. Standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.
** $p < 0.05$, * $p < 0.10$.

Table D.4: The LIML Effect of Parental Leave Related Absence on Three Measures of Wages of Women Aged 40 or Less

	Income (a)	Earnings (b)	Wage (c)
$\widehat{Absence}_{i,l,j,t}$	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Observations	83,198	82,201	34,131

Notes: The dependent variables are: log of mean annual total pay net of transfers and parental leave benefits (column (a)); log of mean annual total taxable earnings comprising all types of formal remuneration from work, including overtime pay, performance-related pay and bonuses, and benefit income (column (b)); and log of mean total monthly income adjusted for hours of work (column (c), where 'FTE' stands for full-time equivalent). Each outcome is the within-plant mean computed over all female employees aged 40 or less in plant i in year t and is inflation-adjusted using the Norwegian consumer price index with 2000 as base year. The sample period for income and earnings is 1994–2013 (columns (a) and (b), and for wage is 1997–2013 (column (c)), due to data availability. All regressions control for plant, year, and industry×local labor market×year fixed effects. In all regressions, standard errors are clustered at the local labor market level.

Table D.5: The LIML Effect of Parental Leave Related Absence on Part-time Employment of Women Aged 40 or Less, 1994–2013

	Stock		Inflow	
	Headcount (a)	Share (b)	Headcount (c)	Share (d)
$\widehat{Absence}_{i,l,j,t}$	0.01144***	0.00030***	0.00116***	0.00001
	(0.00140)	(0.00006)	(0.00035)	(0.00001)
Observations	224,869	224,869	224,869	20,725

Notes: The dependent variables are: the number of women aged 40 or less employed on a part-time contract (column (a)); the within-plant share of women aged 40 or less employed on a part-time contract (column (b)); the number of newly hired women aged 40 or less working on a part-time contract (column (c)); and the share of newly hired women aged 40 or less working on a part-time contract among all new hires in firm i in year t (column (d)). Part-time employment is defined as working 30 or fewer hours per week. The analysis sample in column (d) only includes firms that have new hires. All regressions control for plant, year, and industry×local labor market×year fixed effects. The estimates are obtained from 2SLS regressions. Standard errors are clustered at the local labor market level. For other details, see the notes to Table 5 in the text.
*** $p < 0.01$.

Table D.6: The LIML Effect of Parental Leave Related Absence on Contractual Hours and Overtime Hours

	Women Aged 40 of Less (input f)		Other Workers (input o)	
	Weekly Hours (a)	Annual Overtime Hours (b)	Weekly Hours (c)	Annual Overtime Hours (d)
$\widehat{Absence}_{i,l,j,t}$	0.001	0.050	0.059	0.136
	(0.003)	(0.404)	(0.456)	(0.331)
Observations	10,473	784	88,986	25,470

Notes: The dependent variables are: mean (contractual) weekly working hours for women aged 40 or less (column (a)); mean yearly overtime hours for women aged 40 or less (column (b)); mean (contractual) weekly working hours for women aged above 40 and all men (column (c)); and and mean yearly overtime hours for women age above 40 and all men employed in plant i in year t (column (d)). All regressions control for plant, year, and industry×local labor market×year fixed effects. The sample period in columns (a) and (c) is 2004–2013, while in columns (b) and (d) it is 2009–2013, due to data availability. In all regressions, standard errors are clustered at the local labor market level. For other details, see the notes to Table 5.

E Additional Details on the Data

Here we provide more details on the original data and the construction of the estimating sample. We use multiple administrative registers that cover the universe of Norwegian firms and workers. In a nutshell, we merge the registers of employment statistics, earnings histories, demographics, births, and parental leave duration (available only from 1992) using unique person and plant identifiers, to generate plant specific variables for a panel dataset of plants.

Specifically, we start with the employment statistics for 1983 to 1994 and 1995 to 2014 to create an annual employer-employee matched register data set containing the information on unique person identifiers, the main job, and the main employer. Employment statistics are often referred to as the employer-employee matched data registers, since they contain each contract (or employment spell) for a worker identified by a unique individual identifier with an employer, who is also identified by a unique identifier. We select the main job as either the one with the largest weekly hours or the highest income for each individual in a given year. For the construction of employment shares, it is crucial to have information on the total number of days of absence. We observe unique employer identifiers since 1983. This defines the first year of our sample period. We can then follow employees and employers until 2013.

We should note that the data released by Statistics Norway have a structural break in the definition of establishment identifiers in 1995, which makes it challenging to trace plants back before 1995. We have therefore coded a flow routine that generates new establishment identifiers consistently defined over the entire time period 1983 to 2013. In particular, we use worker flows in the population data of employees from 1994 to 1995 to identify the same establishments both in 1994 and 1995, which allows us to create a new establishment identifier that is then consistently applied across the entire period from 1983 to 2013. It is then straightforward to use this new dataset to our annual employer-employee matched dataset and measure worker mobility across plants.[27]

To the basic employer-employee matched dataset we can merge individual characteristics from several registers. To measure duration of absence from work due to parental leave, we use the welfare registers, which starting from 1992 contain exact information on the total number of days of worker's absence due to paid parental leave and payout of parental leave benefits for each parent. The welfare registers also have data on the number of days of long sickness absence at the individual level (available since 1992). Long sickness absence is registered when it exceeds 10 days. All births are observed in the birth registers, which give us information on birth dates and parent-child linkages. The demographic registers provide us with the information needed to measure workers' age and gender. These variables are key to constructing our shift-share instrument.

We also use information on hours of work brackets and exact hours worked. From 1997 onward, the wage statistics register provides data on exact (contractual) hours of work and overtime hours. Data on hours brackets are available before 1997, which can be used to assess differences in the intensive margin over a longer time period than the two more recent measures. From the education registers, we construct our education variable, defined by three categories: It is equal to 1 for elementary/compulsory education, 2 for high school/some college education, and 3 for university or greater qualifications. Work experience is calculated on the cumulative number of years of positive earnings above basic

[27] The routine is available on request.

income and obtained from the register of taxable earnings, which goes back to 1967. Firm tenure is calculated as the cumulative number of years with the same plant identifier.

To measure income from work, we make use of a three measures available from different administrative records on wage payments from firms to workers. These are all at the worker level. The first is annual earnings reported as of 31 December in each year, which include taxable labor income and benefit transfers (excluding child benefits). The second is annual income, which from 1993 onward includes only labor income, while parental leave benefits are excluded. Thus, this is arguably a more precise measure of the wage cost faced by employers (and it goes to zero during parental leave from the firm's viewpoint). The third measure is gross monthly earnings, which is a more comprehensive measure of pay that includes basic remuneration, additional allowances and bonuses, but excludes overtime pay. It is available only from 1997 from the wage statistics register, and it tends to exclude very small plants in the private sector.

Using the highest paid employee in a plant in a given year, we generate a proxy to identify the CEO of the plant. Based on this variable and gender, we then can distinguish plants that have a female or male CEO. This variable is used in the heterogeneity analysis in Section 7. To perform other analyses in the paper, we need local labor market characteristics, such as unemployment rates and population by gender, which we extracted from the municipality database.

In the final step, we collapse the data to generate a plant-year panel dataset. For each plant, we additionally have data on sector (public or private), industry at the 3-digit level, and local labor market as defined by Statistics Norway (2009). From the balance sheets data available at the enterprise-year level from 1995 onward, we have information on profits, sales, assets, equity, and total wage bill. Approximately 20% of companies in our dataset are multi-plant firms. After several sensitivity checks, we decided to present results only on single-plant enterprises with clean balance sheet information. All monetary variables are inflation-adjusted using the Norwegian consumer price index with 2000 as the base year.

In the analysis, the baseline period refers to pre-reform years, 1983 to 1986, and we can go as far back as 1983 to compute the shares for the Bartik instrument. We use all firms but exclude plants with three or fewer employees at baseline, in part because in this way we avoid an overrepresentation of companies with employment shares at the two extremes of the distribution, and in part because the vast majority of very small employers never hire. Plants are followed until the end of the sample period in 2013. Plants that close down before are followed until the year prior to their death. Due to the construction of the shifts, firms are required to survive at least until 1995, two years after the last reform under analysis. Companies that shut down before 1995 are excluded from the sample. We do not expect this to be a problem, given our focus on firms' long-term responses. Similarly, companies born after 1986 cannot contribute to our analysis by definition, as we cannot construct the share components pre-reform for them.