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Mothers' quits at childbirth and firm level responses

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Mothers' quits at childbirth and firm level responses

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Mothers' quits at childbirth and firm level responses[*]

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Abstract

This paper investigates how firms respond to mothers' exit from the labour market at childbirth. We exploit a reform that increased the duration of the unemployment benefit as exogenous variation in mothers' quits. In Italy, mothers are eligible for unemployment benefits within 12 months from childbirth not only in case of dismissal, but also in that of resignation. Relying on the heterogeneous change in the duration of the benefit, which depends on the previous contributory history, we employ a triple difference model to causally estimate the effects of the reform on maternal employment outcomes and on firms employing new mothers. We find that mothers more affected by the reform have a higher probability of quitting and of being non-employed, while the probability of being laid-off is unchanged. Firms employing them increase hires of young men but not those of young women. The result is consistent with statistical discrimination in the external labour market, with negative effects on women's employment and careers.

Keywords: quits, hirings, separations, unemployment benefits, statistical discrimination, triple difference

JEL Classification: J16, J23, J21, J38, J65

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Le dimissioni delle madri lavoratrici e i loro effetti sulle imprese

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Sintesi non tecnica

Questo studio analizza gli effetti sulle scelte occupazionali delle imprese associati all'uscita delle madri dal mercato del lavoro alla nascita del primo figlio. Il lavoro sfrutta l'entrata in vigore della Nuova Assicurazione Sociale per l'Impiego (NASpI; d. lgs. 22 del 4 marzo 2015) che ha determinato un aumento significativo della durata del sussidio di disoccupazione, eccezionalmente concesso nel nostro ordinamento alle madri anche in caso di dimissioni volontarie entro il primo anno di vita del bambino (è invece escluso per le dimissioni in via generale). Tale modifica normativa potrebbe aver indotto più madri a dimettersi, almeno nel breve periodo, comportando una riduzione del loro tasso di occupazione; a questi effetti si assocerebbero costi attesi più elevati per le imprese che assumono donne in età fertile, a causa della maggiore necessità di sostituire le lavoratrici che lascerebbero l'azienda in prossimità della maternità.

Per indagare tali impatti, lo studio si avvale dei dati amministrativi dell'INPS sull'universo dei lavoratori nel settore privato non agricolo, abbinati alle informazioni sulle imprese in cui essi sono occupati. L'analisi si concentra sulle aziende con meno di 35 dipendenti, per le quali i costi connessi con le dimissioni sono più elevati.

I risultati principali mostrano che:

- Le madri più esposte alla riforma – ossia le donne con storie contributive relativamente più continue, per le quali la durata del sussidio ù aumentata maggiormente dal 2015 – mostrano tassi di dimissioni e di non-occupazione piè elevati intorno alla nascita del primo figlio (rispettivamente del 2,9 e 4,8 per cento per 100 giorni in più di durata potenziale del sussidio). Il tasso di licenziamento rimane invece invariato, suggerendo che nelle imprese non si riscontra, a seguito della riforma, una riduzione dei licenziamenti a compensazione dell'aumento delle dimissioni volontarie delle madri.

- L'aumento delle dimissioni comporta un incremento del tasso di turnover nelle imprese. Quelle in cui sono occupate le madri più esposte, dove quindi si è verificato un numero maggiore di dimissioni, aumentano le assunzioni di dipendenti uomini nella classe di età 20-45 anni (dello 0,4 per cento per 100 giorni in più di durata del sussidio della madre). Inoltre, le imprese riducono lievemente le cessazioni delle donne tra 20 e 45 anni (dello 0,4 per cento). Questo suggerisce che le aziende, a fronte del maggiore turnover delle madri, tenderebbero a preferire l'assunzione di uomini quando si rivolgono al mercato del lavoro esterno, mentre per le sostituzioni interne ricorrerebbero alle donne. Il risultato sarebbe in parte attribuibile al fatto che la riforma analizzata, aumentando

il costo atteso per le imprese associato alla genitorialità per le madri e non per i padri, avrebbe ampliato la rilevanza delle asimmetrie informative sul grado di attaccamento al lavoro delle donne, per cui le imprese preferirebbero manodopera interna laddove disponibile.

- L'aumento della propensione ad assumere uomini in risposta alle maggiori dimissioni delle madri in seguito alla nascita del primo figlio segnalerebbe quindi la presenza di fenomeni di discriminazione statistica nei confronti delle donne in età fertile.

Parole chiave: dimissioni, cessazioni, sussidio di disoccupazione, discriminazione statiatistica, differenza tripla

1 Introduction

Despite the progress registered in the last decades in labour market participation of women and in their career prospects, substantial gender gaps remain. Although motherhood has always been recognised as a key experience influencing female labour market participation (Becker, 1985, 1991), the literature has documented that it still plays an important role in explaining the remaining gaps. There is evidence showing that mothers suffer from the so-called child penalty (Kleven et al., 2019; de Quinto et al., 2020; Kleven et al., 2019; Casarico and Lattanzio, 2023a). When they continue working after childbirth, they tend to earn less than women without children or than fathers, mainly due to the lower number of hours worked. They are also more likely to enter non-employment, since motherhood increases their exit rate from the labour force (De Philippis and Lo Bello, 2022).

This paper investigates how firms respond to mothers' exit from the labour market at childbirth. To address this question, we exploit a policy that increased the incentives for mothers to quit their job at childbirth. In Italy, mothers who voluntarily quit their job within the first 12 months from childbirth are entitled to receive the unemployment benefit (UB), the access to which is thus not limited to the case of dismissal. Law decree 22/2015 (i.e., the *Jobs Act*) increased the duration of the unemployment benefit from 8 months to up to 24 months, depending on the number of weeks of contribution before the job separation.

From the theoretical point of view, we might expect different effects of the policy change both on mothers and on firms, depending on the time period we look at. Focusing on the short run, for mothers, the provision of a generous income support for a longer time period increases the expected utility from being unemployed and, thus, the incentives to resign the job contract. Maternal employment reduces. For firms, in the presence of search frictions, the higher turnover of mothers at childbirth increases the expected labour costs of employing childbearing age women, since in case of resignation firms need to incur in hiring and training costs, with possible negative consequences on productivity. This might induce firms to shy away from hiring women at childbearing age. If this mechanism is at play, the policy would trigger statistical discrimination against women at childbearing age.[1]

We investigate the effects of changes in the incentives to quit at the individual and firm level in the short run, leveraging on monthly administrative data from the Italian National Social Security Institute (INPS) on employees in the private non-farm sector. Relying on matched employer-employee data and exploiting information on childbirths, we focus on mothers who gave birth between January 2013 and June 2017, and firms employing them. For identification of causal effects, we exploit a reform of the Italian UB system, effective

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[1]In the long run effects might be different. On the one hand, the negative effects detected in the short term might be even reinforced in the long term, if quitting mothers do not come back to work and show lower labour market attachment, with adverse consequences on their human capital and future employment prospects. On the other hand, mothers, thanks to the policy change, can exploit a longer search period to get a better worker-employer match, with positive effects on earnings and/or employment. On the firm side, the improved reallocation of workers across firms can have a positive influence on firm productivity.

from May 2015, that changed the way in which the benefit duration is computed. Before the reform, it was 8 months for all workers below 50 years old. After the reform, the duration is set equal to 50% of the number of weeks of contribution paid in the last 4 years before the job separation. The heterogeneity in the number of weeks of contribution across individuals (mothers in our case) generates variation in their exposure to the reform, depending on their previous work experience. In the case of careers without interruptions in the 4 years before the job termination, the duration of UB is 24 months. For these individuals, the reform has therefore tripled the potential duration of the UB. We use this heterogeneous change in the UB potential duration as an exogenous shifter for new mothers' labour supply. We estimate, in an event-study around childbirth, a difference-in-differences model, which compares the outcomes of new mothers (or firms employing new mothers) with a more/less marked increase in the potential duration of UB, before and after the reform. We study the effects of the policy on maternal employment outcomes (probability of quitting, of being non-employed and of being laid-off) and on firms' margins (i.e., hirings, separations and types of contract).

We find that the longer duration of UB increases mothers' quit rate after 10 months from childbirth, approximately when both the mandatory maternal and the voluntary parental leave expire. As a consequence, employment probability declines and is still lower 18 months after childbirth. The effects are concentrated among mothers with permanent contracts, as the reform changed very little the incentives for workers with temporary contracts, who cannot voluntarily quit their jobs (unless in special cases justified by specific reasons) and are less likely to be eligible for the unemployment benefits when the contract expires. We do not detect any impact on layoffs, indicating that firms employing new mothers more exposed to the reform faced a higher turnover and were not replacing layoffs with voluntary quits. In response to the increased turnover of new mothers, we observe that in most affected firms (i.e., those that employ most affected mothers at the time of childbirth), the number of net hires among male workers goes up, while that among female workers does not change significantly. Effects are concentrated among young workers, who are more likely to be substitutes for new mothers because of their age proximity. Looking at the margins of adjustment separately, more exposed firms increase hires of young male workers, while they reduce separations of young female workers (the new mother is excluded). This suggests that, in response to higher turnover, firms rely on the external market to employ young men, while they resort to the internal market for young women. This would imply that the reform has deterred the hires of childbearing-age women, limiting their employment and career opportunities. It also provides direct evidence of statistical discrimination against mothers.

Our paper contributes to several strands of the economic literature. First, the policy that we study can be seen as a significant extension in job leave rights, with no job protection attached, but with a high replacement rate (75% of previous earnings). Thus, we speak to the very large literature studying the effects of the characteristics of parental leaves (e.g.,

duration, replacement rate and degree of job protection) on maternal employment and wages (see, among others, Lalive and Zweimüller (2009); Kluve and Tamm (2013); Schönberg and Ludsteck (2014)). The reform we focus on is the same as the one in Zurla (2022), but the identification strategy adopted to study its effects on mothers' choices is different and the focus on firms differentiates ours from her paper. The literature focusing on the impact of leave policies on firms and coworkers is more limited and with contrasting results on the presence and costs for firms of mothers' absence. Gallen (2019) provides evidence from a parental leave reform in Denmark, that extended the period of paid parental leave reserved to mothers. She finds that firms employing women exposed to the reform (i.e., giving birth after the reform was passed) have significantly lower survival rates and retention of mothers. There is, however, no effect on coworkers' labour market earnings. Huebener et al. (2022) look at a parental leave extension that took place in Germany and focus on firms with up to 50 employees. They find small negative effects on firms' employment and total wage bill during the extended parental leave period, but not afterwards. Moreover, firms do not change their propensity to hire women of childbearing age. While these studies find negligible impacts of parental leave on firms and coworkers, Ginja et al. (2023) show that firms bear adjustment costs to cope with workers' absence. Exploiting a parental leave expansion of three months in Sweden, they find that firms incurred in additional wage costs, whose size depends on the availability of workers' substitutes. The contrasting effects may depend on different labour markets and welfare state institutions, or on different social and gender norms. While these papers look at extensions in parental leaves, which usually bring about a temporary absence of mothers from their jobs, the policy that we study incentivises mothers' quits, implying that mothers permanently separate from their jobs. The effects at the firm level can therefore be different from the ones observed for (changes to) parental leave policies. Finally, we separately look at the effects on different margins of employment adjustments by gender, which is not the main focus of most of the above-mentioned papers.

Second, we contribute to the small literature estimating how public policies can have unintended consequences and strengthen, rather than reduce, statistical discrimination.[2] Statistical discrimination is usually difficult to detect since expectations of all firms are equally affected by a policy change and causal research designs may fail to estimate such an effect. Fernández-Kranz and Rodríguez-Planas (2021) analyse the impact of a policy aimed at protecting jobs and granting more flexibility to parents (the right to work part-time), and show that, given the gendered take-up, the policy backfired. Thomas (2021) finds that mandated maternity leave increased the likelihood of mothers remaining employed, but reduced their probability of being promoted. Although we focus on a policy that supports income but does not protect mothers' jobs, we are equally interested in studying the link between the policy and statistical discrimination in the data. In this respect, we also add to the literature that looks at unintended consequences of family policies for women's careers

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[2]Bjerk and Han (2007) and Casarico et al. (2023) develop theoretical models in which they show how cash transfers to families or maternity leaves, respectively, can reinforce statistical discrimination.

([Blau and Kahn, 2017]). Moreover, our estimates highlight a relevant role for firms in determining gender employment and pay gaps ([Card et al., 2016]; [Casarico and Lattanzio, 2023b]).

Lastly, there is a growing literature studying employers' ability to substitute workers when they leave the firm. [Sauvagnat and Schivardi] (2023) focus on the substitution of managers; [Jäger and Heining] (2022) look at workers more broadly. Both papers exploit sudden deaths to measure productivity and substitutability. We look at mothers' quits associated to childbirth. Although they bring about a permanent separation from the employer (like deaths), birth-related absences/quits can be anticipated by firms, allowing them to plan and react earlier at lower costs. We show, instead, that firms face costs when dealing with mothers' quits. We also shed light on the substitutability between men and women, and on the mechanisms through which this substitution takes place (internal vs. external labour market).

The rest of the paper is organized as follows. In Section 2 we describe the policies intended to support new mothers, the UB system and the reform used in the empirical analysis. Sections 3 and 4 describe the data and explain our empirical strategy, respectively. Section 5 reports our individual and firm-level results. Finally, Section 6 concludes.

2 The institutional setting

2.1 The parental leave and childcare systems

In Italy, mothers have compulsory maternity leave entitlements for 5 months, which can be used either 2 months before childbirth and 3 months after, or 1 month before and 4 months after.[3] Compulsory maternity leave is paid at 80% of last earnings by the National Security Institute (INPS) and many collective agreements envisage that employers pay the remaining 20%.

Once the mandatory maternity leave has expired, parents are entitled to a voluntary parental leave of 10-11 months, 6 of which are paid at 30% of last earnings and the remaining ones at zero.[4] Each parent can take a maximum of 6 months of leave, which becomes 7 for fathers taking at least 3 months of leave — for a total of 11 months considering both parents. The leave is paid by INPS and some collective agreements establish that employers have to guarantee a top-up. According to data provided by OECD (2023), the use of parental leave is rather low in Italy in comparison with other developed countries; approximately 80% of users are women. The low replacement rate contributes to explaining both the low take-up and the fact that the leave is mostly used by women. Indeed, it is more convenient

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[3]In the most recent years, not included in our empirical analysis, there is also the option of using all the 5-month entitlements after childbirth.

[4]These are the rules in place between 2013 and 2018, the period we analyse. In August 2022, the number of months of paid leave increased to 9: 3 months for each parent that cannot be transferred to the other, and 3 months that can be shared between parents.

to renounce to the lower wage in the family, usually that of women, since they are more likely to be secondary earners.

A compulsory paternity leave was introduced in 2012: it was just 1 day in 2013, and it increased to 4 days in 2018 (currently it is 10 days). The length of the leave is much lower compared to those adopted in other developed countries.

Finally, the supply of childcare facilities addressed to 0-2 year-old children is rather low in Italy. According to Istat (2022), the average coverage rate — the number of (both public and private) childcare places for 100 0-2 year-olds — is 27.2%, still below the target of 33% set by the European Council in 2002. Moreover, there is large geographical heterogeneity in the supply of childcare services: the coverage rate in Northern and Central regions is more than double the one registered in Southern regions. It ranges from the highest value (40.8) recorded in Emilia-Romagna to the lowest in Campania (11.0).[5]

2.2 The unemployment benefit system

The Italian UB system has been profoundly revised in the last decade. The main goals of these reforms were to guarantee universal access to UB on the basis of contribution criteria and to increase their generosity. While access to UB is granted only in case of dismissal, since 1971 for mothers there is a specific provision according to which they can access UB not only in case of dismissal, but also in case of resignation within the first year of the child. The availability of unemployment benefits in case of voluntary resignation is not specific to Italy. Other countries (like Germany, Sweden and Denmark) provide unemployment benefits to workers — not only mothers — in case they quit for family-related reasons, without reductions or penalties in the amount or in the period over which the benefit is paid (Venn, 2012; Langenbucher, 2015).

The option for mothers to resign and access UB has not been changed by the reforms that took place, but the economic convenience of taking advantage of it obviously depends on the generosity of the UB transfer. Before the latest reforms of the UB, the system covered all employees in the non-agricultural sector and the duration of the benefit depended on workers' age. Workers fired before turning 50 years old were eligible for 8 months, while workers fired after turning 50 could receive the benefit for up to 12 months. The replacement rate was 60% of the average wage in the last 3 months before the layoff for the first 6 months of UB, 50% in the 2 two months for workers below 50, 40% in the final 4 months for workers older than 50. UB eligibility required: *i)* to initiate contributing to the social security system at least 2 years before the start of the UB spell; *ii)* to have worked at least 52 weeks in the last 2 calendar years before the layoff.

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[5]Fees are also rather high. According to some recent estimates, in Italy an average family composed of two working adults and one child aged less than 3 pays 303 euros per month for a full-time place in a public nursery (Cittadinanzattiva, 2018); the cost in private nurseries is higher, on average approximately 500 euros per month. Private places represent around 50% of the overall supply of childcare services. Also for tariffs, there is a large geographical heterogeneity, which reflects the North-South divide.

The first reform took place in 2012 and was implemented starting from January 1st, 2013. The UB took the name of ASpI.[6] Access to ASpI was granted also to workers whose contract had expired and to trainees, who were previously excluded. The reform did not significantly change the eligibility criteria, but it increased the replacement rate to 75% of the average wage in the last 2 years for the first 6 months of UB, and 60% for the remaining months. The main innovation was the introduction of a UB subsidy (Mini-ASpI) for workers with less experience, who had worked for only 13 weeks in the last 12 months before the layoff. The replacement rate was set to 75% of the average wage in the last 2 years and the duration at 50% of weeks worked in the last 12 months before the separation.

The second reform of UB, named NASpI,[7] is the one we rely on in the empirical strategy, and took place in 2015. It changed several features of ASpI and Mini-ASpI. First, the system envisaged only one instrument to address unemployment, and eligibility criteria were relaxed, requiring 13 weeks of work in the last 4 years and 30 days of work in the last 12 months before the contract termination. Second, duration was set equal to 50% of weeks of contribution in the last 4 years before the job termination, up to 24 months. This significant extension in the duration of UB increases mothers' expected value of unemployment, raising incentives to quit their jobs. This might affect firm turnover and the related costs.

3 The data

We leverage monthly administrative data provided by INPS on the universe of employees in the private (non-farm) sector, for the period 2013-2018. In particular, we merge different data sources: *i)* monthly contribution records on workers' histories (information on wages, weeks worked, occupation, contract's start and end dates and reason for termination), and demographic characteristics (gender, age, region); *ii)* monthly records on firms (hires, separations, sector, size, types of contracts);[8] *iii)* information on childbirth episodes for working mothers. We do not have any further family-related information (e.g., the number of children, and the partner).

We select a sample of women who gave birth to their first child between January 2013 and June 2017, and follow them 6 months before and 18 months after childbirth. We keep a balanced sample of mothers (and firms employing them), including periods of employment and non-employment. We do not go further than 18 months in order to have enough observations in the balanced sample.[9] In the firm-level analysis, we consider firms in which

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[6]*Assicurazione Sociale per l'Impiego* enacted by Law n. 92 of 28th June 2012.

[7]*Nuova Assicurazione Sociale per l'Impiego* enacted by Law Decree n. 22 of 4th March 2015.

[8]The availability of the reason for contract termination is crucial for our purposes, as it allows us to distinguish voluntary quits and layoffs. We nonetheless also exploit the panel structure of the data and identify non-employment spells as months in which a worker is not employed by any firm. A dummy for workers quitting their job and for those in non-employment are the main outcomes of the worker-level analysis, together with a dummy for whether the worker is laid off.

[9]As we analyse a balanced sample, looking at, e.g., 36 months ahead would limit the sample of mothers under consideration. Given the data cut-off in December 2018, we would need to trace back mothers who

the selected mothers — those who gave birth between January 2013 and June 2017 — worked 4 months before childbirth. We focus on firms with less than 35 employees, which represent around 97% of firms and employ 48% of workers in the Italian private sector. The restriction to small firms is not uncommon in the literature that investigates the effects of employees' turnover on firm outcomes (e.g., Brenøe et al., 2020; Jäger and Heining, 2022). There are two main reasons to focus on firms with less than 35 employees. First, most mothers' quits in any given year happen in small firms.[10] Second, the effect of an employee's separation on coworker outcomes decreases with firm size, so it would be difficult to detect it in larger employers.

4 Empirical Strategy

4.1 Reform induced variation in the duration of UB

We exploit the 2015 reform that considerably increased the duration of UB as an exogenous shifter for maternal labour supply. Specifically, for each mother with a child less than 1 year old (new mother) in the sample, we compute the change in the potential duration of the UB due to the 2015 reform. In other terms, based on the information on the working history of new mothers, we measure the exposure to the reform with the time-invariant variable Z_i, i.e.:

$$Z_i = D_i^{Post} - D_i^{Pre}, \tag{1}$$

where D_i is the UB potential benefit duration for mother i 4 months before childbirth, computed according to the post and pre-reform rules.[11] We fix the variable at event time -4 because this is when mothers usually leave work for the compulsory maternity leave, allowing for some measurement error as the precise timing is between event times -3 and -2 from childbirth.

The heterogeneous change in potential duration Z_i across new mothers depends on their weeks of contribution. Figure 1 reports the distribution of Z_i. For most mothers, the reform results in a large expansion in the duration of the UB.

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gave birth at most by December 2015 (i.e., 36 months before the data cut-off). In addition, only those giving birth between May and December 2015 would be treated by the reform. Our choice of considering a post-childbirth window of 18 months, therefore, reflects the trade-off between sample size and an analysis of a sufficiently lengthy time period.

[10]As an example, the report for the year 2016 by the Italian Labour Inspectorate on voluntary resignations after childbirth shows that close to 72% of resignations happen in firms with less than 50 employees (and 55% in firms with less than 15 employees).

[11]Specifically, $D_i^{Pre} = 208$ days (8 months considering a working month of 26 days). $D_i^{Post} = 0.5 \times \widetilde{L}$, where $\widetilde{L}$ is the number of days worked in the prior 4 years.

4.2 Empirical model

We estimate a triple difference model, comparing the probability of quitting of new mothers around childbirth, before and after the UB reform for more or less treated mothers (we consider a continuous treatment in the change in UB duration). In other words, we employ an event-study specification around childbirth and compute the difference in the evolution of outcomes of mothers more or less treated before and after the UB reform. The reduced-form specification is as follows:

$$
\begin{aligned}
y_{ist} =& \boldsymbol{\beta} \boldsymbol{D}_{ist}^{Event} \cdot Post_t \cdot Z_i + \boldsymbol{\alpha}^P \boldsymbol{D}_{ist}^{Event} \cdot Post_t + \boldsymbol{\alpha}^Z \boldsymbol{D}_{ist}^{Event} \cdot Z_i \\
&+ \boldsymbol{\alpha} \boldsymbol{D}_{ist}^{Event} + \boldsymbol{\eta} Post_t \cdot Z_i + \boldsymbol{\gamma} \boldsymbol{D}_{ist}^{Ind} + \boldsymbol{\delta} \boldsymbol{D}_{ist}^{Time} + \zeta \cdot X_{it} + \omega_{ist},
\end{aligned}
\tag{2}
$$

where y_{ist} is a dummy variable equal to 1 if mother i quits/is non-employed/is laid-off in calendar month t at time-to-childbirth s; $Post_t$ indicates whether time t is after the introduction of the NASpI (May 2015); Z_i is the time-inviariant measure of the shock (the change in potential UB duration); $\boldsymbol{D}_{ist}^{Event}$ is a full set of event time dummies from month $s = -6$ to $s = 18$ relative to childbirth, excluding period $s = -1$; $\boldsymbol{D}_{ist}^{Ind}$ and $\boldsymbol{D}_{ist}^{Time}$ are full sets of individual and calendar time dummies; X_{it} are pre-determined worker characteristics (i.e., part-time contract, occupation) interacted with calendar time dummies; ω_{ist} is an error term clustered at the individual level.

Our coefficients of interest are in the vector $\boldsymbol{\beta}$, and estimate the difference in quit (non-employment/lay-off) rates between mothers at each event time around childbirth, after the reform relative to the period before the reform, and experiencing an increase equal to 100 days in the potential duration of the UB (which corresponds to approximately one standard deviation). We are primarily interested in the effects on mothers' voluntary quits. We also examine the impact on the probability of layoffs to investigate whether voluntary quits are just a replacement for layoffs in the absence of the reform. We also examine the non-employment probability to understand whether quits are associated with the exit from employment or with transitions to a different job.

When analysing firm responses, we consider the same estimating equation (2) but for different outcome variables computed at the firm level. In the firm-level analysis, the "event" is employing a woman who gave birth in the period January 2013–June 2017. In this case the firm-level outcomes $y_{j(i)st}$ are the cumulative number of hires, separations, net hires, permanent contracts in firm j where the new mother i worked 4 months before childbirth, in calendar month t at the time-to-childbirth s. To compute firm-level variables, we consider all workers in the firm where the new mother works, excluding the new mother i. The coefficients of interest $\boldsymbol{\beta}$ estimate the difference in the outcomes between firms employing new mothers more or less affected by the UB reform, before and after childbirth and before and after the reform.

Pre-trends Unless otherwise specified, we follow the approach of Gruber et al. (2021), and report coefficients net of a linear trend, fitted separately for new mothers before and after the reform, more or less exposed to the increase in UB duration, *before* childbirth. In other terms, in the period before childbirth, i.e., for $s < 0$, we estimate:

$$
\begin{aligned}
y_{ist} =& \boldsymbol{\theta} F_t \cdot Post_t \cdot Z_i + \boldsymbol{\zeta}^P F_t \cdot Post_t + \boldsymbol{\zeta}^Z F_t \cdot Z_i \\
& + \boldsymbol{\zeta} F_t + \boldsymbol{\xi} Post_t \cdot Z_i + \boldsymbol{\varsigma} \boldsymbol{D}_{ist}^{Ind} + \boldsymbol{\nu} \boldsymbol{D}_{ist}^{Time} + \epsilon_{ist},
\end{aligned}
\tag{3}
$$

where all variables are defined as in equation (2) and F_t is a linear event time trend.

We then compute the predicted values $\widehat{y}_{ist}$, and subtract them from the dependent variables $\widetilde{y}_{ist} = y_{ist} - \widehat{y}_{ist}$. The outcomes netted of the linear pre-trend are, therefore, the outcomes of our regressions. We follow the same approach when considering firm-level outcomes, $y_{j(i)st}$.[12]

4.3 Descriptive statistics

Table 1 shows that women giving birth in the periods pre and post reform do not show remarkable differences in observable variables. It is important to note that 95% of the selected sample of new mothers is employed under a permanent contract, reflecting the positive effect that job stability has on fertility (De Paola et al., 2021; Clark and Lepinteur, 2022). Moreover, our focus on small firms, that are more concentrated in Southern regions,[13] may explain why the employment rate of new mothers in the South is relatively higher than that observed for the overall female population. As for the outcome variables, we observe an increase in the separation rate in the post-reform period, driven by the increase in the quit rate.

Table 2 shows the outcome variables relative to workers in firms employing new mothers. Among female workers, we exclude the new mothers. Outcome variables look very similar in the pre and post-reform periods.

5 Results

5.1 Maternal outcomes

Probability of quitting, of being non-employed and of being laid-off Figure 2 depicts the estimated coefficient of the triple difference around childbirth. Mothers' probability of quitting rises at 10 months after childbirth (approximately when both the compulsory maternal and the voluntary parental leaves are usually exhausted) and remains higher until 18 months from childbirth. The quitting probability following childbirth increases

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[12]See also Freyaldenhoven et al. (2021) for a formal illustration of this approach.

[13]In the South 66.7% of firms have less than 35 employees, against 42.5% in the North and 47.6% in the Centre.

by 0.46 percentage points or by 2.9% relative to the pre-reform average quit rate (0.046/0.16, see Table 1) when the UB duration increases by 100 days. This is associated with an increase in the non-employment probability. Over the same period, we do not observe any significant effect on layoffs of most affected mothers (Figure 3), excluding the possibility that firms are replacing layoffs with mothers' voluntary quits through collusion.

Heterogeneity   Effects are concentrated among new mothers with permanent contracts, being 95% of the selected sample as shown in subsection 4.3. They are slightly larger for mothers with part-time contracts and with low job experience (Figure 4 in panel a. and b., respectively), who are less likely to be affected by caps to UB payments.[14] Moreover, effects are larger among mothers employed in the manufacturing sector, typically characterised by less flexibility in the working time schedule (Figure 5 panel a.); similarly, effects are slightly more pronounced for blue collars, who more often perform jobs requiring night shifts and physical strength (Figure 5 panel b.). We do not detect any heterogeneity by macroarea and firm size (Figure 6, panel a. and b., respectively).

5.2   Firm level responses

Aggregate effects on net hires   Figure 7 shows the estimated coefficient of the triple interaction for cumulative net hires — the number of positions in the firm. After childbirth, firms employing mothers more exposed to the reform increase the number of male workers and, to a non-significant level, the number of female workers.

Effects by age   Figure 8 shows that the effects are concentrated among workers aged 20-45, who are more likely substitutes for new mothers, while no action is detected for workers older than 45.

Hires and separations   As for the margins of adjustment, more exposed firms increase hires of young men (Figure 9) and reduce separations of young women (Figure 10), suggesting that more exposed firms rely on the external labour market when employing young men, and on the internal one in the case of women. While young men are not exposed to the higher incentives to quit, young women do, and firms prefer to retain those female workers who have already shown labour market attachment or taken fertility decisions, for which there is less asymmetric information.

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[14]The UB payment is equal to 75% of average monthly earnings in the last 4 years, divided by the number of weeks of paid contribution and multiplied by 4.33. This payment, if larger than a given minimum amount, is equal to the sum of 75% of average monthly earnings and 25% of the difference between average monthly earnings and the minimum amount. The UB payment cannot be larger than the maximum amount that is defined by INPS each year.

Effects by type of contract Figure 11 shows that the number of permanent contracts in firms more strongly affected by the reform increases, which is consistent with the fact that the policy mainly affected new mothers with a permanent position.

Other margins We do not detect any effect of the policy on wages or on the intensive margin (number of days of work per month).

Quantification of the effects We quantify the magnitude of the effects by combining the results at the individual and firm levels. The policy induced a 2.9% increase in the quit rate of mothers by the end of the observation window, at $s = 18$. Using as an example male hires, we estimate that, over the same period, firms increase hires of men by .06 units, or by 1.1% relative to the pre-reform average male employment (.06/5.17). Hence, we estimate that the reform induced an increase of 0.4% in male hirings (1.1/2.9). Similar calculations yield that female separations decreased by 0.4%. The magnitudes for the main outcomes are depicted in Figure 14, which also reports calculations using the individual-level effect on non-employment rather than on quits.

Discussion We interpret our overall set of results as evidence that firms have an optimal share of permanent contracts — that is not affected by the policy change — and tend to replace workers for whom the turnover rate increased with employees whose quitting behaviour did not change. Indeed, the reform that we have analysed increased the quit rate of mothers with permanent contracts and firms replaced them along two margins. When firms need to rely on the external labour market, firms are more likely to hire young men relative to young women in response to the policy change. When, instead, internal substitutes are available, firms tend to reduce separations among young women. The most likely channel is the transformation of temporary contracts into permanent ones. This suggests that firms, when employing women, select those who already have some experience in the firm — for whom asymmetric information is lower. This is because the policy increased the expected cost of parenthood for mothers but not for fathers, amplifying the importance of asymmetric information on the degree of labour market attachment when hiring women. On top of this, firms may tend to reduce more separations among young women since they are more likely hired under temporary contracts (see Table 2) and easier to transform into permanent positions; moreover, incumbent men may perform tasks and have different skills from those of women that need to be replaced.

Overall, the higher hiring rate of young men in response to the reform suggests that the policy, by increasing mothers' incentives to quit at childbirth, has created a penalty on the hires of young women, worsening statistical discrimination against women at childbearing age. These women, then, are penalized when looking for a new job. Despite women already employed in the firm do not seem to face adverse effects associated with the reform — if anything, they are more likely to get a permanent position —, limited hiring opportunities

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have negative consequences on employment of out-of-work women and on female workers' careers and wages, since most of the wage increases take place with job-to-job transitions (Cahuc et al. (2006); see Berson et al. (2020) for evidence on Italy). Finally, no action is detected when looking at older workers, suggesting that substitutability holds within given age classes.

6   Conclusions

This paper studies how mothers' exit at childbirth affects firms. We exploit a reform that heterogeneously increased the duration of the Italian unemployment benefit as a shifter for maternal labour supply, and show that the reform increased quit rates around childbirth for mothers experiencing a longer extension in the duration of the unemployment benefit. Firms more impacted by the increased job turnover react by hiring young men, and reducing the separation rate for young women already employed in the firm, who plausibly have already had children and showed attachment to the firm. That young women are not among the newly hired by firms more strongly affected by the reform indicates that the reform has worsened statistical discrimination against women of childbearing age. The provision according to which mothers can access unemployment benefits also in case of voluntary resignation, combined with more generous unemployment benefits, may protect mothers' income in the short run, but it overall reduces their employment and job opportunities. The availability of unemployment benefits in case of voluntary resignation is not specific to Italy. Other countries provide unemployment benefits to workers in case they quit for family-related reasons, without reductions or penalties in the amount or in the period over which the benefit is paid (Venn, 2012; Langenbucher, 2015). This reinforces the external validity of our analysis. In addition, the results of our paper can inform the discussion on increasing or lifting sanctions for voluntary unemployment. They can also inform the assessment of all policies that reduce labour market attachment of women, such as home care subsidies, child-related transfers whose amounts decrease with family income, or increases in the price of childcare.

One limitation of this paper is the evaluation of the effects on a short-term horizon. Due to data availability, we cannot analyse new mothers' labour market behaviour once the maximum duration of UB has expired (approximately from 34 months after childbirth). Thus, we cannot detect whether the policy has helped new mothers find better job matches due to the longer search period, with cascade effects on firm productivity. This limitation hinders a proper welfare analysis of the effects of the policy.

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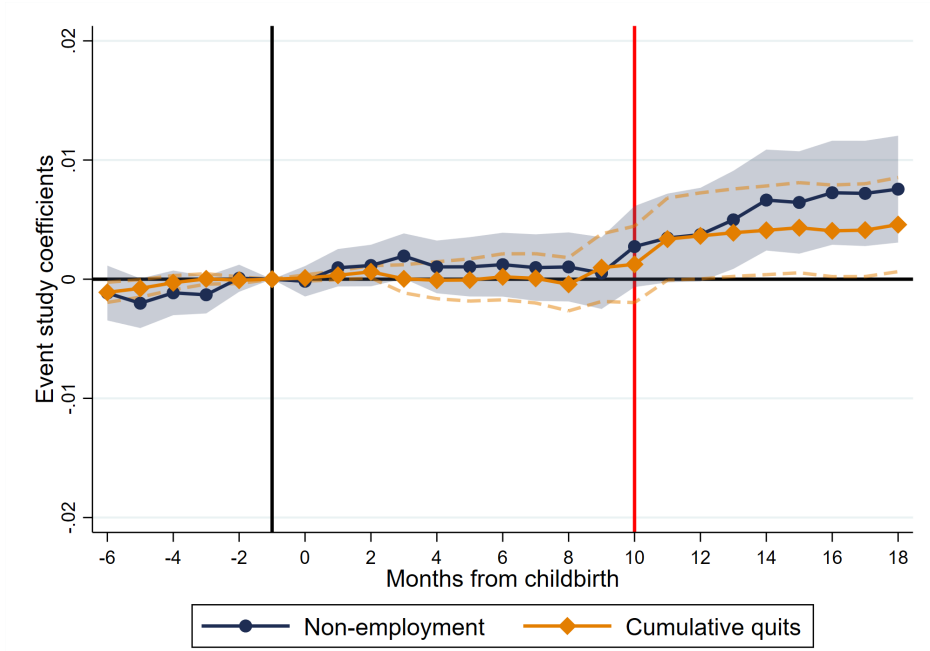
Figures and Tables

Figure 1: Distribution of the shock in the duration of UB according to post and pre 2015 reform rules
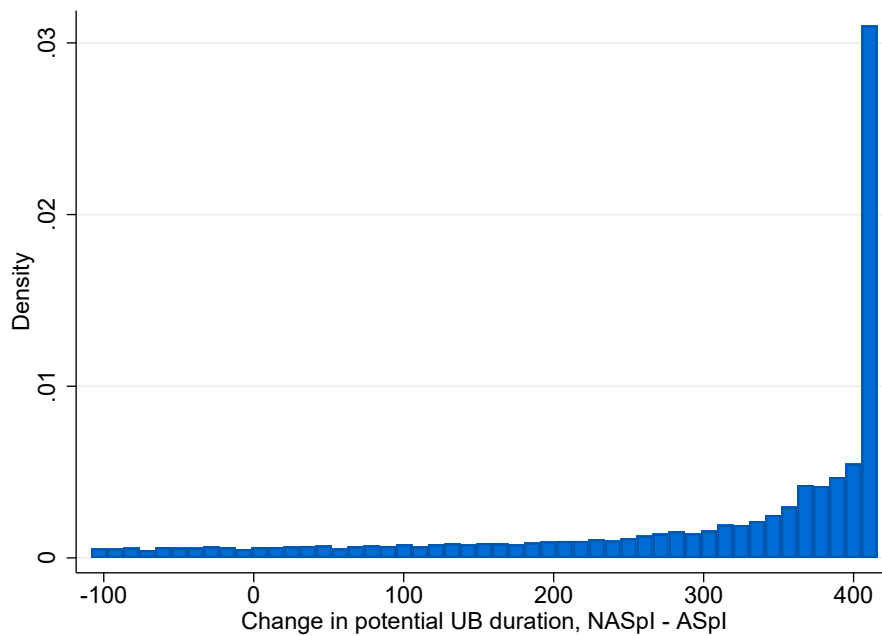

Notes: Social security data, mothers who gave birth between January 2013 and June 2017.

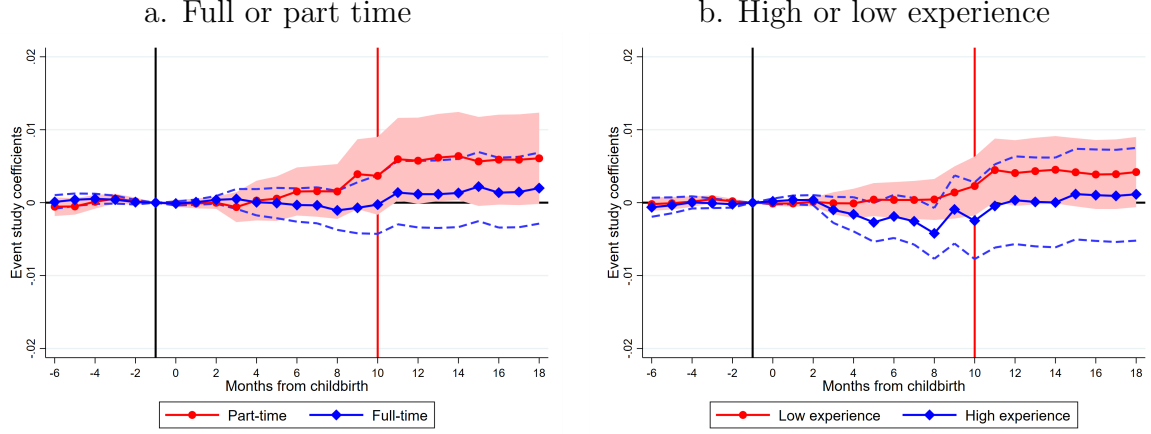Figure 2: Maternal employment outcomes: quit and non-employment probabilities

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the quit/non-employment probability of new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level. The regression also controls for dummies for whether the mother works in a white-collar job and with a part-time contract in the 4th month before childbirth, interacted with event time dummies.

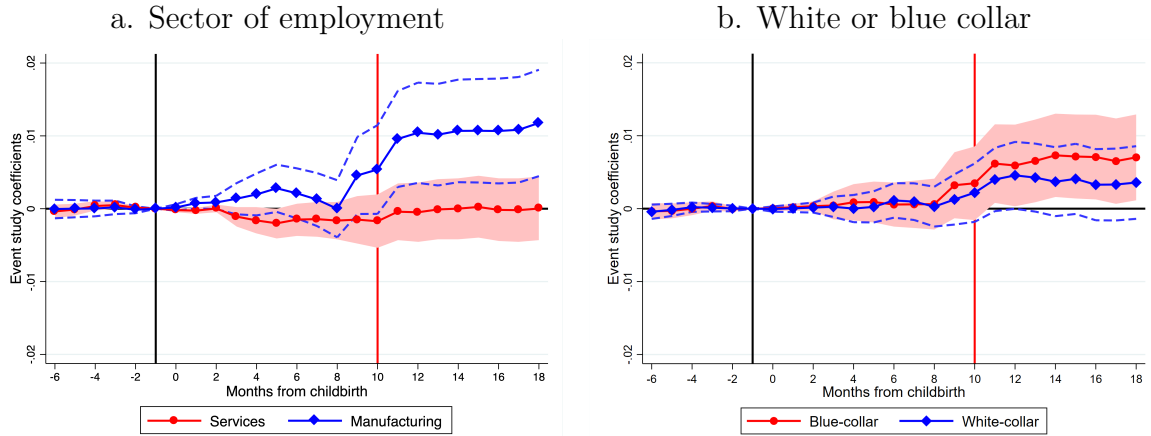Figure 3: Maternal employment outcomes: layoff probability
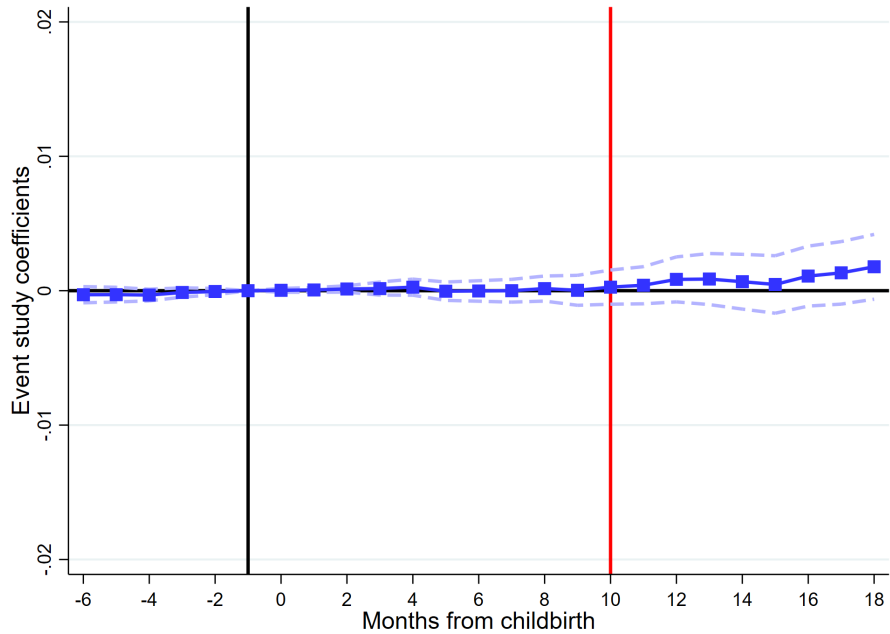
Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the layoff probability of new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level. The regression also controls for dummies for whether the mother works in a white-collar job and with a part-time contract in the 4th month before childbirth, interacted with event time dummies.

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Figure 4: Maternal employment outcomes: quitting probability by job contract and working experience
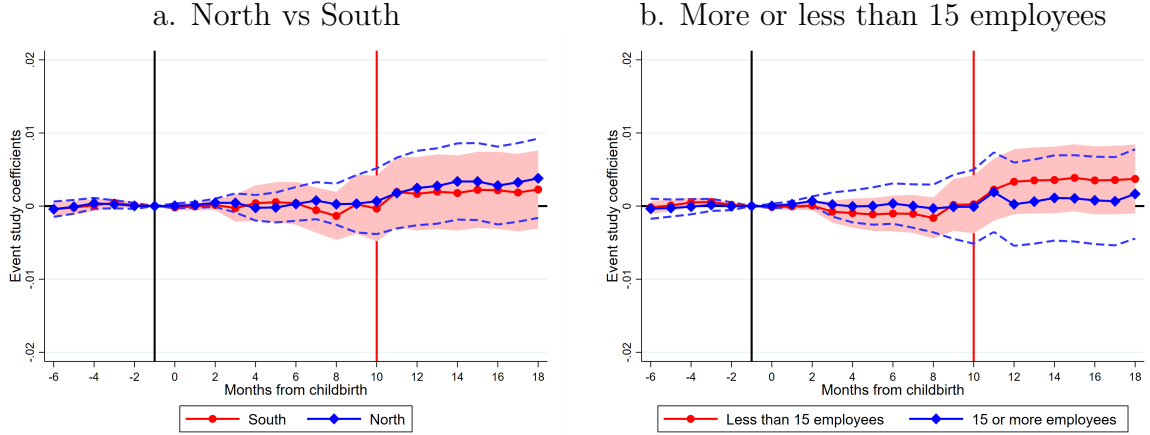
a. Full or part time

b. High or low experience

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the quitting probability of new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level. The regression also controls for dummies for whether the mother works in a white-collar job and with a part-time contract in the 4th month before childbirth, interacted with event time dummies.

Figure 5: Maternal employment outcomes: quitting probability by sector and type of labour

a. Sector of employment

b. White or blue collar

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the quitting probability of new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level. The regression also controls for dummies for whether the mother works in a white-collar job and with a part-time contract in the 4th month before childbirth, interacted with event time dummies.

Figure 6: Maternal employment outcomes: quitting probability by macro-area and firm size
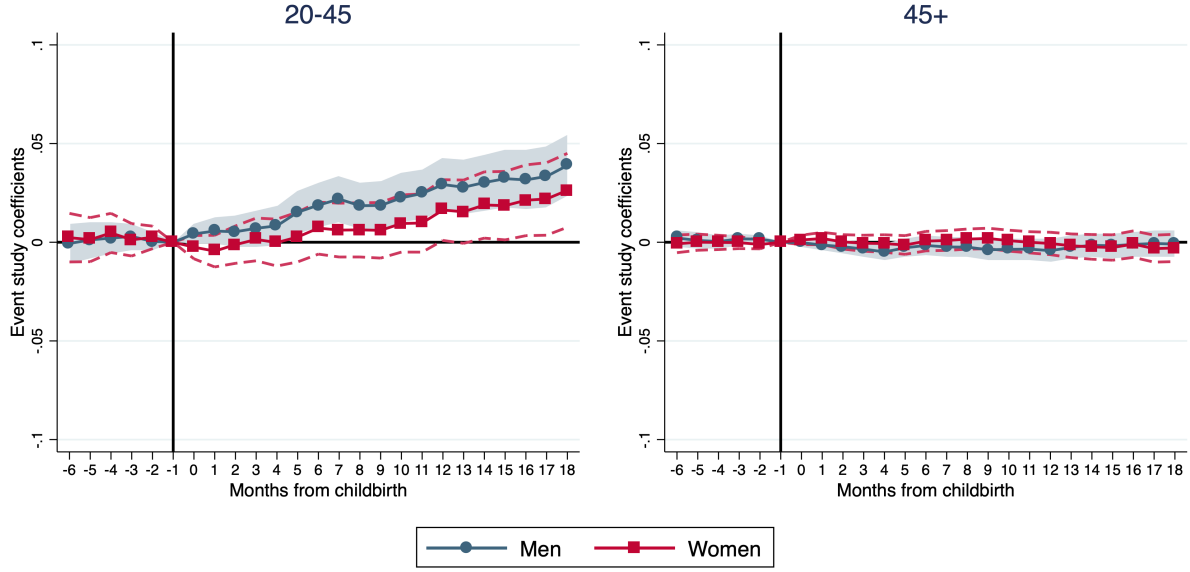
a. North vs South

b. More or less than 15 employees

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the quitting probability of new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level. The regression also controls for dummies for whether the mother works in a white-collar job and with a part-time contract in the 4th month before childbirth, interacted with event time dummies.

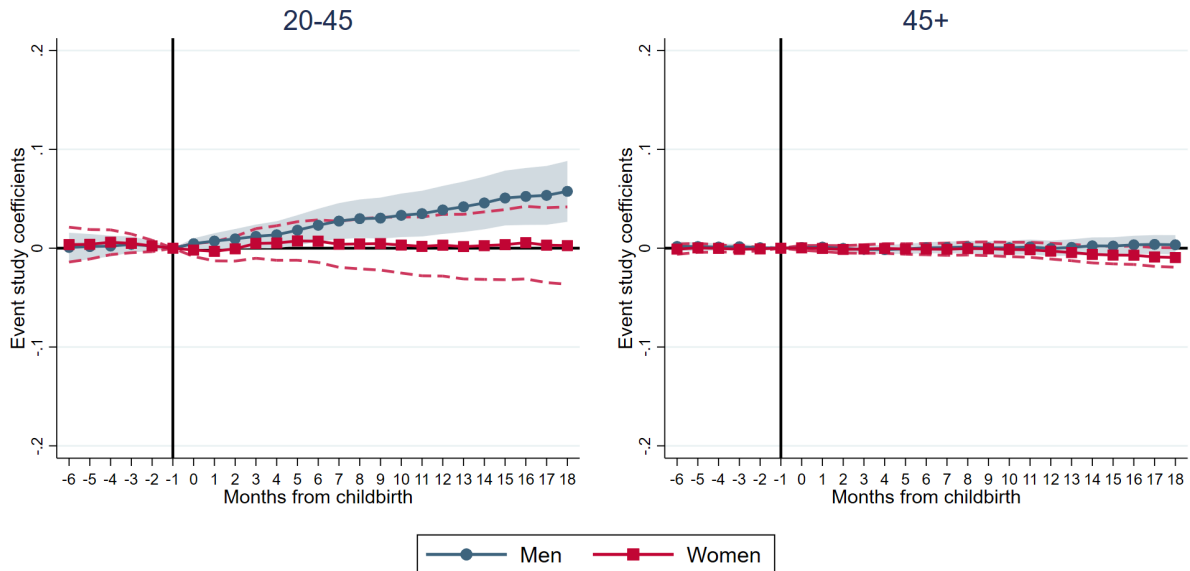## Figure 7: Firm-level outcomes: cumulative net hires
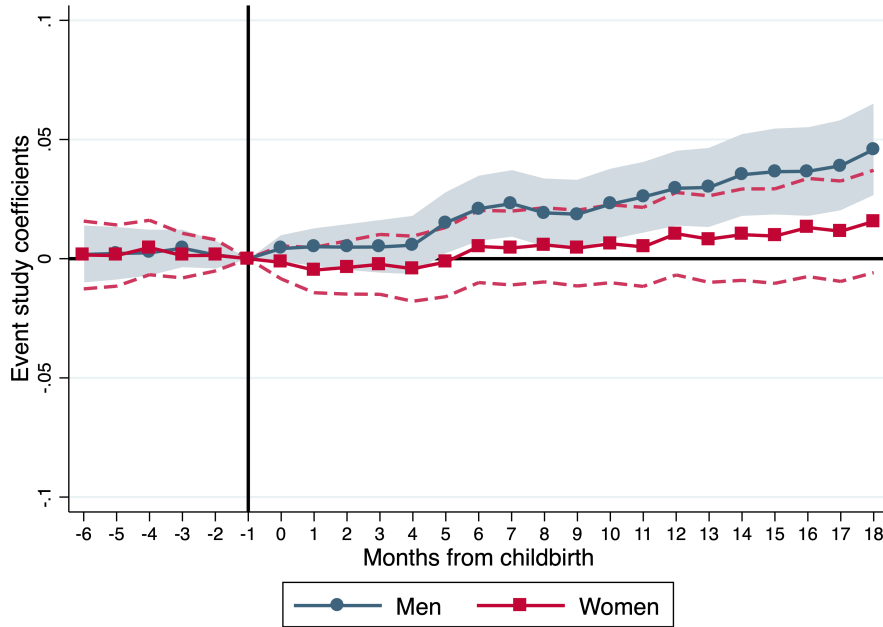
Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the number of male and female cumulative net hires in firms employing new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level.

Figure 8: Firm-level outcomes: cumulative net hires by age

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the number of male and female cumulative net hires, by age group, in firms employing new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level.
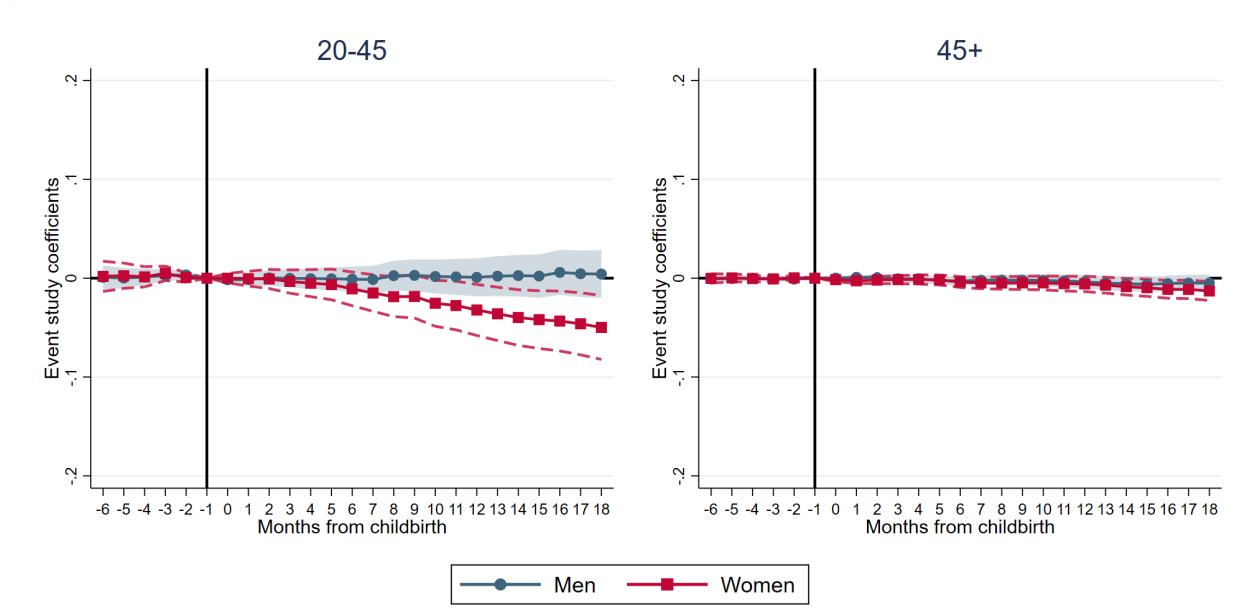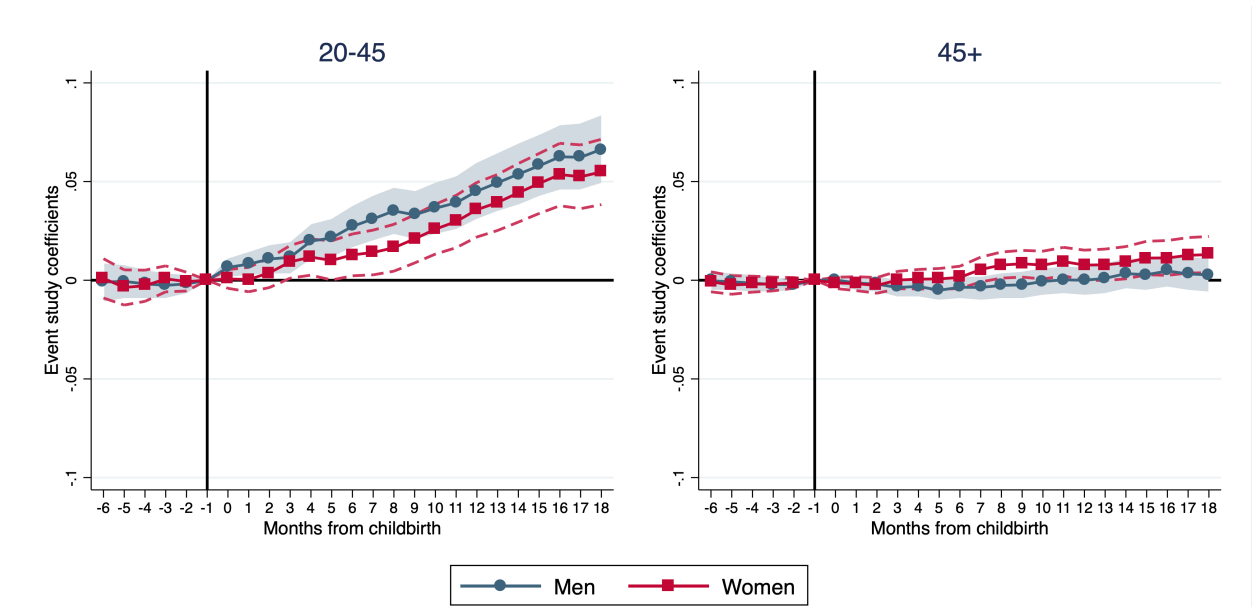
Figure 9: Firm-level outcomes: cumulative hires by age

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the number of male and female cumulative hires, by age group, in firms employing new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level.

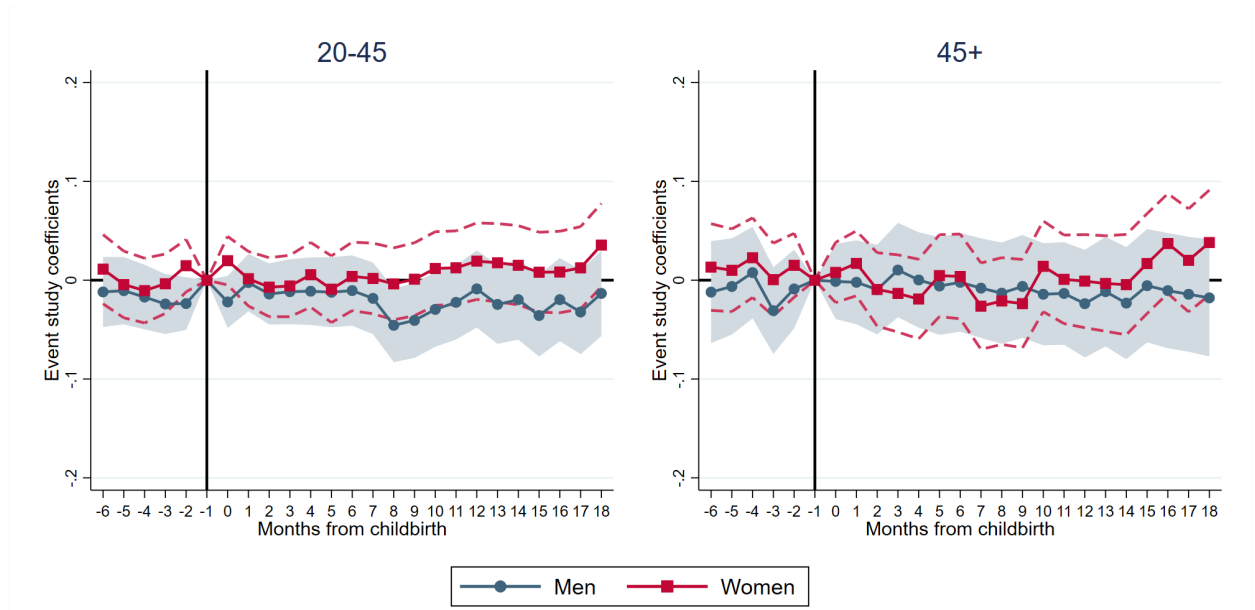## Figure 10: Firm-level outcomes: cumulative separations by age

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the number of male and female cumulative separations, by age group, in firms employing new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level.

Figure 11: Firm-level outcomes: number of permanent contracts by age

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in the number of male and female workers with permanent contracts, by age group, in firms employing new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level.

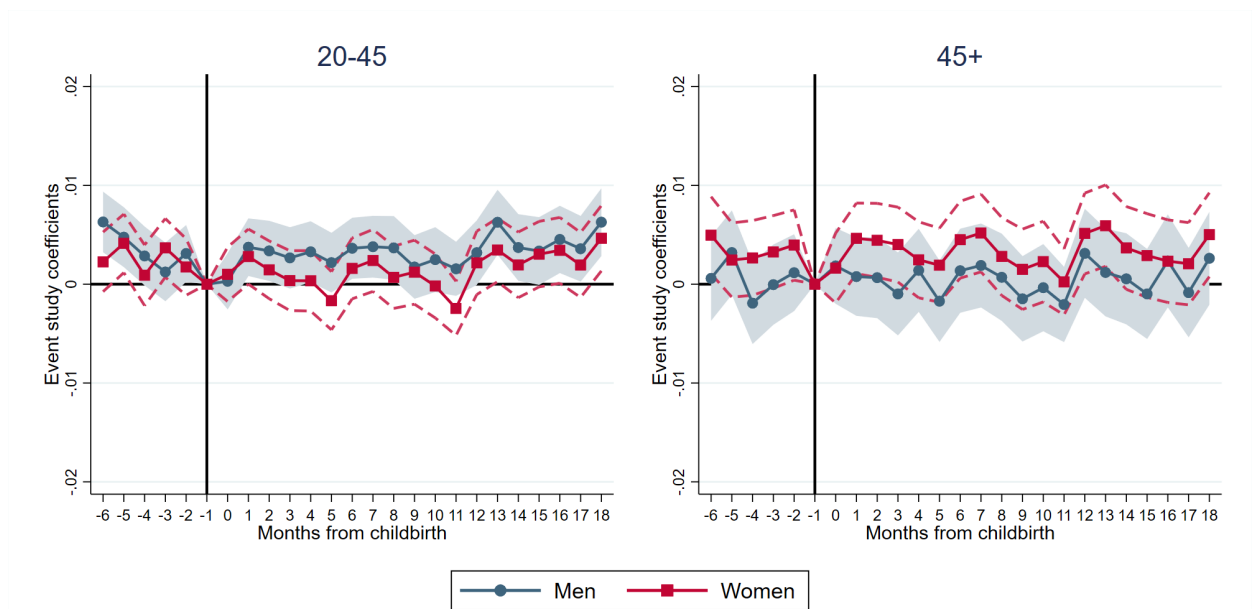Figure 12: Firm-level outcomes: average number of days worked per month

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in male and female average days worked, by age group, in firms employing new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level.

Figure 13: Firm-level outcomes: daily wages

Notes: The figure reports the event-study coefficients β from equation (2), alongside 95% confidence intervals. Each dot represents the change in male and female average log daily wages, by age group, in firms employing new mothers having their child after the UB reform relative to the period before and exposed to a 100 days increase in their potential UB duration, between the 6 months before until 18 months after childbirth (event time -1 is the excluded dummy). The confidence intervals are obtained from cluster-robust standard errors at the individual level.

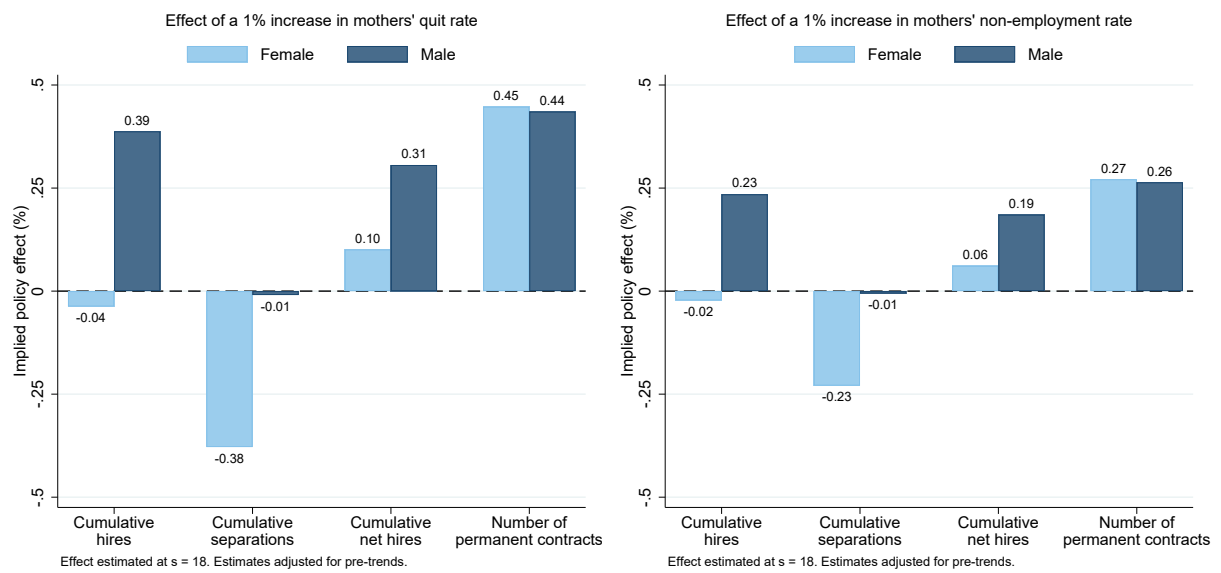Figure 14: Quantification of the effects

Table 1: Summary statistics of new mothers

	(1) Pre-reform	(2) Post-reform
South	0.47	0.48
Industry	0.37	0.35
Services	0.63	0.65
Experience	10.71	10.74
Full-time	0.70	0.66
Permanent	0.95	0.95
White-collar	0.59	0.56
Manager	0.01	0.01
Monthly wage	1272.07	1177.84
Daily wage	83.44	77.76
Days worked	18.73	18.55
Quit rate	0.16	0.21
Firing rate	0.06	0.05
Separation rate	0.27	0.31
N mothers	41398	57576

Notes. Observables measured 4 months before childbirth. Quit rates, firing rates and separation rates are computed as the quit/firing/separation probability over the entire period between 6 months before and 18 months after childbirth.

Table 2: Summary statistics of firms employing new mothers

	(1) Male workers Pre-reform	(2) Post-reform	(3) Female workers Pre-reform	(4) Post-reform
Employment	5.17	5.03	5.34	5.28
Cumulative hires	0.47	0.61	0.66	0.82
Cumulative separations	0.40	0.44	0.51	0.53
Avg. log monthly wages	7.53	7.45	7.29	7.23
Avg. days worked	22.57	21.84	19.34	18.84
Share part-time	0.15	0.22	0.41	0.46
Share white-collar	0.34	0.34	0.54	0.52
Share temporary	0.10	0.10	0.14	0.14
Observations	36897	50116	36897	50116

Notes. Observables measured for all coworkers of mothers who gave birth before June 2015, 4 months before childbirth.