

The effect of gender equality bargaining on women's careers

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Abstract: This paper investigates the effects of firm-level gender equality bargaining on women's careers using French administrative data. To address the endogeneity of gender equality bargaining, we exploit a 2010 reform that introduced financial penalties for firms with 50 or more employees that failed to comply with bargaining requirements. The reform led to a marked increase in gender equality bargaining among firms subject to the mandate. Using a fuzzy difference-in-differences approach, we estimate the causal effects of gender equality bargaining on firms' workforce composition and the motherhood earnings penalty. We find limited effects on workforce composition and no evidence of improvements in women's labor market outcomes. However, we find some evidence that gender equality bargaining may widen the motherhood earnings penalty in the years following childbirth. While the estimated effect on mothers' earnings is close to zero immediately after the birth of their first child, it becomes significantly negative five years later. Our findings suggest that some measures included in gender equality agreements and action plans, particularly those aimed at facilitating work-life balance, may have unintended consequences for mothers' labor market outcomes.

JEL Classification: J64; J68; H43

Keywords: gender inequalities, motherhood penalty, collective gender equality bargaining, event study

The gender earnings gap has narrowed over recent decades, reflecting a broad convergence between women and men in education, labor force participation, occupations, and working hours (Goldin, 2014). Nevertheless, a substantial gap persists, driven in large part by women's shorter working hours and more frequent career interruptions (Bertrand et al., 2010). In several countries, collective bargaining has become an important part of the “regulatory toolkit” for addressing gender inequalities in pay and working conditions (Milner et al., 2019). A substantial literature has examined the factors that shape gender equality bargaining (Hoque & Noon, 2004; Milner & Gregory, 2014). Yet few quantitative studies have assessed whether such bargaining is effective in reducing gender inequalities¹.

France provides a particularly relevant setting in which to study this question, as it was the first country to mandate bargaining on gender equality (Bruno et al., 2021). Since 2001, French firms with 50 or more employees have been required to take steps to reduce gender inequalities through collective agreements on gender equality. Although this requirement has been in place since 2001, financial penalties for non-compliance were introduced only in 2010. We exploit this reform as a quasi-natural experiment to estimate the effects of gender equality bargaining on firms' workforce composition, including the share of women in the workforce, their representation across occupations, and their representation among part-time workers². We also examine whether gender equality bargaining affects the motherhood earnings penalty. Our analysis combines French administrative data from a demographic panel matched with employer earnings declarations and data from the French Ministry of Labor on collective agreements and action plans.

Our results show no significant effect of either the 2010 reform or effective coverage by a gender equality agreement or action plan on firm-level outcomes, except for a temporary increase in the share of women in intermediate occupations in the years following the reform. This limited response

¹ To our knowledge, Coly (2022) is the only paper that investigates this issue.

² The effect of this reform is also studied by Coly (2022), who finds no effect on the gender pay gap. Furthermore, our exploratory analysis shows that few firms allocate financial resources to reducing the gender pay gap through gender equality bargaining. For these reasons, we do not include the gender pay gap among the firm-level outcomes examined in this paper.

suggests that financial penalties for non-compliance had little effect on workforce composition or working hours, consistent with previous evidence on the gender wage gap (Coly, 2022). More generally, we find no evidence that the reform improved women's labor market outcomes. At the individual level, however, the reform is associated with less favorable outcomes for mothers following the birth of their first child. In particular, mothers' total wage income falls by about 4.2% in the years following childbirth, while we find no comparable effect for fathers. Our estimates imply an effect equivalent to approximately 9.7% of average pre-birth income among mothers employed in firms subject to the bargaining requirement. Our analysis of potential mechanisms shows that gender equality bargaining increases the probability that mothers work part-time by 15.9 percentage points (p.p.) in the year following childbirth. However, we find no robust evidence of a persistent reduction in annual working hours or of improvements in women's labor market outcomes more generally. Finally, our results suggest heterogeneous effects across the earnings distribution, with low- and high-earning mothers exhibiting different adjustment patterns following childbirth.

This article makes three main contributions. First, it contributes to the literature on gender equality bargaining by providing, to our knowledge, the first quantitative assessment of its causal effects on firms' workforce composition and on labor market outcomes following motherhood. Second, it contributes to the literature on the labor market consequences of parenthood by showing that the motherhood earnings penalty may vary with firms' exposure to gender equality bargaining. Third, it contributes to the emerging literature on the role of firms in shaping gender inequalities by examining whether workplace measures intended to promote gender equality can affect mothers and fathers differently following childbirth. The next section discusses these contributions in relation to the existing literature.

Background

Related literature

This article first contributes to the literature on gender equality bargaining. Building on the seminal work of Dickens (1998), a substantial literature has examined the factors that shape gender equality bargaining, including the economic context, bargaining relationships between the parties, negotiators' characteristics, and the legislative framework (Gregory & Milner, 2009; Hoque & Noon, 2004; Milner & Gregory, 2014; Williamson & Baird, 2014). Other studies have examined the content and implementation of gender equality bargaining agreements. Typically relying on small, non-random samples of agreements, these studies conclude that such agreements often resemble "empty shells", with limited substantive content and serving primarily an image-management function (Hoque & Noon, 2004; Laufer & Silvera, 2006; Pochic et al., 2019). To our knowledge, no quantitative study has estimated the causal effect of gender equality bargaining on firms' workforce composition or on the motherhood earnings penalty.

Second, this article contributes to the literature on the labor market consequences of parenthood. Changes in women's labor supply and labor market outcomes following the birth of their first child account for a substantial share of the gender pay gap (Bertrand et al., 2010; Kleven, Landais, & Søgaaard, 2019; Waldfogel, 1998; Wilner, 2016)³. A growing literature quantifies the effects of parenthood on employment and earnings. Earlier studies focusing on specific groups of highly educated individuals find large earnings penalties for women—but not for men—following the birth of the first child (Azmat & Ferrer, 2017; Bertrand et al., 2010). More recent studies use event-study approaches to examine the effects of parenthood in broader populations across countries and time periods and consistently document substantial motherhood earnings penalties (Angelov et al., 2016; Kleven, Landais, Posch, et al., 2019; Kleven, Landais, & Søgaaard, 2019). These studies show that women's labor market

³ Kleven, Landais, & Søgaaard (2019) estimate that about 80% of the gender earnings gap in Denmark in recent years is attributable to child penalties. Wilner (2016) finds a large earnings loss associated with motherhood in France and a much smaller loss associated with fatherhood.

trajectories are similar before parenthood but diverge sharply following the arrival of children⁴. We contribute to this literature by examining whether the motherhood earnings penalty varies with firms' exposure to gender equality bargaining.

Third, this article relates to a growing literature on the role of firms and workplace practices in shaping gender inequalities. Given the limited effect of public policies such as expansions of parental leave or childcare subsidies on women's labor market outcomes (Kleven et al., 2024), recent research has increasingly examined firms' internal policies and practices (Goldin et al., 2020). Drawing on a case study of a large French firm, Lucifora et al. (2021) suggest that gender-specific workplace practices, such as granting additional leave or childcare-related paid leave exclusively to mothers, may not directly affect earnings but may influence managers' assessments of employees' career prospects. Measures intended to facilitate work-life balance may therefore have unintended consequences for women's careers relative to men's, potentially placing mothers on a slower career track— the "mommy track". We contribute to this literature by examining whether measures introduced through gender equality bargaining affect mothers' and fathers' labor market outcomes differently.

Institutional background

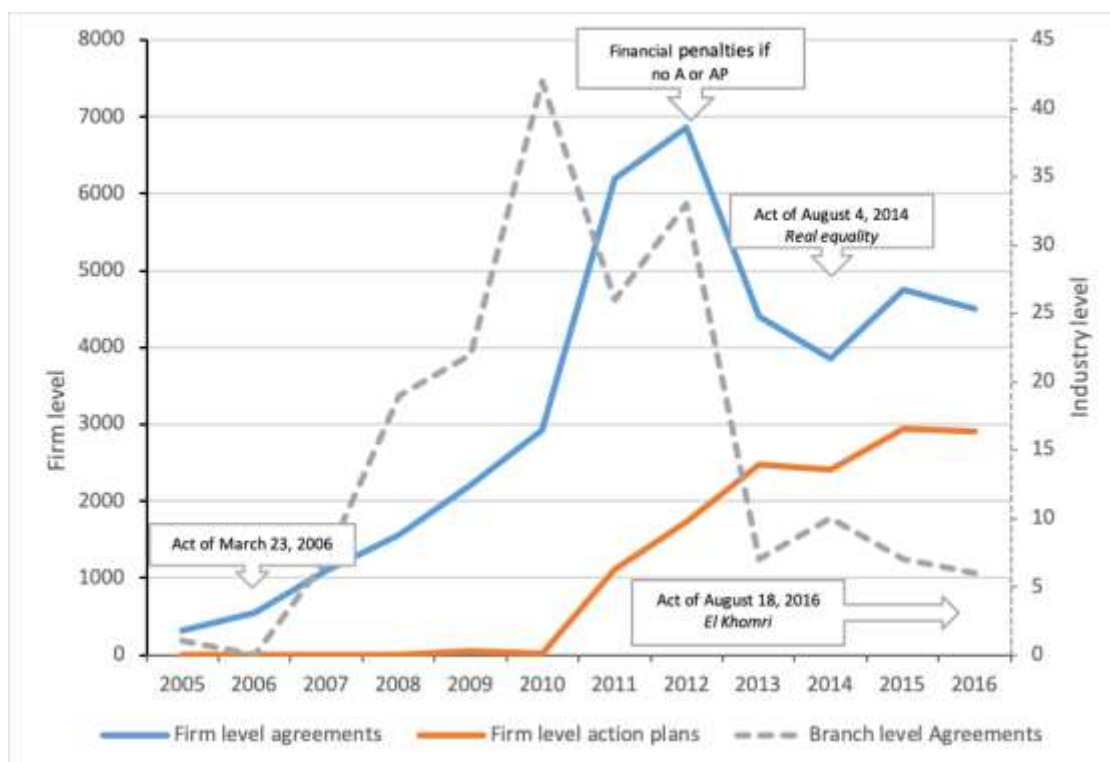
In France, gender equality in the workplace is promoted in part through firm-level collective bargaining, which is intended to involve firms directly in advancing equal employment opportunities and equal pay. Collective bargaining on gender equality has developed gradually since the adoption of the *Roudy Act* in 1983. The Act required employers to submit to the works council a written report — the *Rapport de situation comparée* — comparing the employment situation of women and men within the firm. It also introduced, for the first time, the possibility for employers and union representatives

⁴ Comparing child penalties across six countries, Kleven, Landais, Posch, et al. (2019) show that the child penalty in earnings ranges from 21% in Denmark to 61% in Germany. Using the same event-study approach on a representative panel of French employees, Meurs & Pora (2019) find an earnings penalty for women of around 30% over the five years following the birth of their first child.

to negotiate measures aimed at reducing gender inequalities in the workplace. In 2001, the *Géniſson* Act made such bargaining mandatory, requiring negotiations on firm's objectives regarding gender equality at work and the measures needed to achieve them. In addition to these mandatory gender equality negotiations, the Act of 23 March 2006 required firms to address measures aimed at eliminating gender pay inequalities as part of mandatory annual wage negotiations.

Firm-level collective bargaining on gender equality increased substantially after 2010 (see Figure 1). This expansion was strongly encouraged by public authorities through a series of legislative reforms that introduced sanctions for non-compliance and specified the procedures and scope of gender equality bargaining. This institutional framework has been described as "*bargained public policy*" (Groux, 2005) or "*state-managed bargaining*" (Mias et al., 2016). Most importantly for our empirical strategy, the Act of 9 November 2010 introduced financial penalties of up to 1% of payroll for firms with 50 or more employees that failed to comply with the requirement to be covered by a collective agreement on gender equality or, in the absence of such an agreement, by a unilateral action plan. These financial penalties came into force on 1 January 2012. Non-compliance was subsequently also sanctioned through restrictions on bidding for public contracts.

Figure 1 – Number of firm- and industry (or branch)-level collective agreements and action plans on gender equality



For firms with 50 to 299 employees, the action plan — whether negotiated or adopted unilaterally — must cover at least two areas. For firms with 300 or more employees, it must cover at least three of the following eight areas: hiring, training, promotion, qualifications, job classification, working conditions, pay and benefits, and work-life balance. Gender equality measures must be renewed annually when implemented through an action plan and every three years when established through a collective agreement.

The Decree of 18 December 2012 made pay a mandatory area of gender equality bargaining. The *Real Equality Act* (2014) subsequently streamlined the existing requirements into a single negotiation covering gender equality at work and equal pay. The *Rebsamen Act* (2015) further consolidated bargaining requirements by combining gender equality bargaining with negotiations on quality of working life. Finally, the *El Khomri Act* (2016) introduced further changes aimed at expanding collective bargaining and modernize social dialogue.

Milner et al. (2019) characterize the French institutional framework as a distinctive model of "bargaining equality", resulting from increasingly stringent employer obligations and compliance requirements. Another distinctive feature of the French model is the legal principle of complementarity between collective bargaining at the industry (branch) and firm levels. Bargaining is mandatory at both levels. Industry-level agreements establish provisions on issues such as pay scales, job classifications, and joint initiatives addressing industry-wide concerns, while firm-level bargaining allows these provisions to be complemented by measures tailored to gender equality issues specific to the workplace.

An exploratory analysis of gender equality agreements and action plans

To shed light on the measures introduced through gender equality bargaining, we conduct an exploratory analysis of 590 gender equality agreements and action plans adopted by firms between 2006 and 2014. Although this sample is relatively small, it is larger than those used in previous studies examining the content of gender equality bargaining (Laufer & Silvera, 2006; Pochic et al., 2019). This limited sample size reflects data availability: only a subset of agreements and action plans adopted during this period is publicly available, as the publication of firm-level agreements became mandatory only on September 1, 2017.

Table A.1 in the Appendix reports the measures identified in the agreements and action plans in our sample and their frequency. The list is not exhaustive, as we exclude measures appearing in fewer than 5% of the documents, such as "efforts to promote gender balance in employee representation" or "a one-hour daily reduction for breastfeeding". We classify the measures according to their area of action and group these areas into four broad categories. The first, "Gender balance in employment", includes measures aimed at increasing gender diversity within the workforce. The second, "Reducing gender pay gaps", includes measures aimed at addressing pay inequalities. The third, "Promoting work-life balance", encompasses measures intended to reconcile work and family responsibilities, particularly the return to work following childbirth. The fourth, "Improving access to training", includes measures

designed to improve women's access to training opportunities, particularly following career interruptions for family reasons.

Measures aimed at promoting work-life balance appear in 56.8% of the agreements and action plans in our sample. The most common measure in this category is a dedicated interview with the human resources (HR) department for employees returning from maternity or parental leave, which appears in 44.1% of the documents. Such interviews are often accompanied by measures intended to improve access to training, including systematically informing employees about training opportunities (27.6%) and giving priority to training requests from employees returning from maternity or parental leave (24.9%). Because access to training tends to be greater among higher-skilled workers, these measures may disproportionately benefit higher-skilled and higher-earning women.

Another common work-life balance measure is the promotion and facilitation of part-time work, which appears in 30.8% of the documents. Some agreements and action plans monitor access to part-time work using an indicator comparing the number of requests received with the number accepted. In almost all such cases, the document also states that working part-time should not hinder promotion or slow wage growth. By contrast, measures facilitating a return to full-time work are less than half as common, appearing in 13.9% of the documents.

In addition, 20.8% of the agreements and action plans state that meetings, training sessions, and seminars should be scheduled at times compatible with employees' personal lives. Some also specify recommended days, times, or locations, or encourage the use of videoconferencing. As Pochic et al. (2019) note, such measures regulating meeting schedules primarily concern executives and managers and may facilitate women's access to these positions. Another measure potentially relevant to high-earning women is giving priority to internal female candidates for management positions, which appears in 19.2% of the documents.

Using the typology proposed by Bruno et al. (2024), we find that adjustment measures — such as facilitating part-time work or adjusting working hours at the beginning of the school year — are more common than corrective actions, such as allocating a specific budget to reduce gender pay inequalities.

In our sample, 49% of the agreements and action plans include adjustment measures, compared with 30% that include corrective measures. In particular, only 20% allocate a budget to reducing gender pay inequalities.

Measures aimed at promoting gender balance in employment are also common, appearing in 53.1% of the agreements and action plans. These primarily involve efforts to use gender-neutral language in job advertisements (44.7%) and to raise awareness of anti-discrimination legislation in recruitment (41.7%).

This exploratory analysis motivates four hypotheses about the potential effects of gender equality bargaining on firms' workforce composition, working hours, or the motherhood earnings penalty:

H1. Gender equality bargaining may increase gender balance in firms' workforces, particularly in higher-skilled occupations.

H2. Gender equality bargaining may increase the share of women among part-time workers, given the prevalence of measures facilitating part-time work and promoting work-life balance.

H3. Gender equality bargaining may affect mothers' labor supply following childbirth by encouraging reductions in working hours.

H4. The effects of gender equality bargaining on mothers' labor market outcomes may vary across the earnings distribution.

The remainder of the paper empirically examines these four hypotheses.

Data

Our analysis combines several French administrative data sources covering the period 2004-2019. First, we use the longitudinal version of the *Déclarations Annuelles de Données Sociales* (DADS; Annual Declarations of Social Data)⁵, a panel subsample of French salaried employees drawn from the exhaustive DADS database for research purposes. Since 2004, the DADS panel has included individuals born in October and therefore represents one-twelfth of the French salaried population. The data provide detailed information on gross and net earnings, paid hours, individual characteristics (age, gender, nationality, and seniority), and job characteristics (contract type, full- or part-time status, and occupation).

We also use the DADS position files (*fichiers postes*) to obtain firm-level information on industry, workforce composition, and wages. These data allow us to measure the share of women in the workforce and in each occupation, as well as the share of women among part-time workers.

To identify gender equality bargaining at the firm level, we use data from the French Ministry of Labor covering the period 2005-2019. Information from documents related to collective bargaining is standardized by the Ministry and compiled into D@ccord, a longitudinal firm-level database. D@ccord provides detailed information on the type of document (agreement, disagreement, amendment, or unilateral action plan), the bargaining unit (establishment, firm, group, or industry), the topics covered by the negotiation (wages, bonuses, employment, working hours, working conditions, and gender equality), the signatories, and the unions involved in the negotiation.

To estimate the effects of gender equality bargaining on the child penalty, we link a subset of workers in the DADS panel to the *Echantillon Démographique Permanent* (EDP, Permanent Demographic Sample), a large-scale sociodemographic panel containing information on births, marriages, and deaths⁶. The EDP covers approximately 4% of the population living in France, with inclusion in the

⁵ French firms are required to report information on each employee's earnings annually for payroll and tax purposes.

⁶ Information on births, marriages, and deaths has been collected from civil registration records since 1968, together with information from the 1968, 1975, 1982, 1990 and 1999 censuses.

sample determined by date of birth. The matched DADS-EDP sample includes individuals born between October 1 and 4, who account for 13% of individuals in the DADS panel.

For each individual, we use information from their main employer⁷. We restrict the sample to individuals aged 20-59 who live in mainland France and work in the private sector prior to the birth of their first child. We exclude homeworkers, farmers, CEOs, trainees, apprentices, and members of the clergy. We construct a balanced panel in which parents are observed every year from two years before the birth to five years after the birth of their first child and restrict the sample to individuals who were employed one year before the birth. Our final sample consists of 52,810 parents, 26,404 mothers and 26,406 fathers, whose first child was born between 2006 and 2014.

This sample forms the basis of the empirical analyses presented in the remainder of the paper. We first examine the effects of gender equality bargaining on firms' workforce composition and women's working hours, using the firms employing individuals in the panel described above. We then estimate the effects of gender equality bargaining on the child penalty by comparing individuals who became parents before the period surrounding the 2010 reform with those who became parents after it.

The effect of gender equality bargaining on firms' workforce structure and women's working hours

Empirical strategy

In this section, we investigate the effects of gender equality bargaining on firms' workforce composition, focusing on the share of women in the workforce, their representation across occupations, and their representation among part-time workers. We consider a firm to be treated in year t if it is covered by a gender equality agreement or action plan in that year. A simple comparison of outcomes between treated and untreated firms would identify the causal effect of gender equality bargaining only if treatment status were independent of observed and unobserved determinants of these outcomes. This assumption is unlikely to hold. For instance, employers that are more committed

⁷ The main employer of an individual is the firm that pays her the highest labor earnings during a given year.

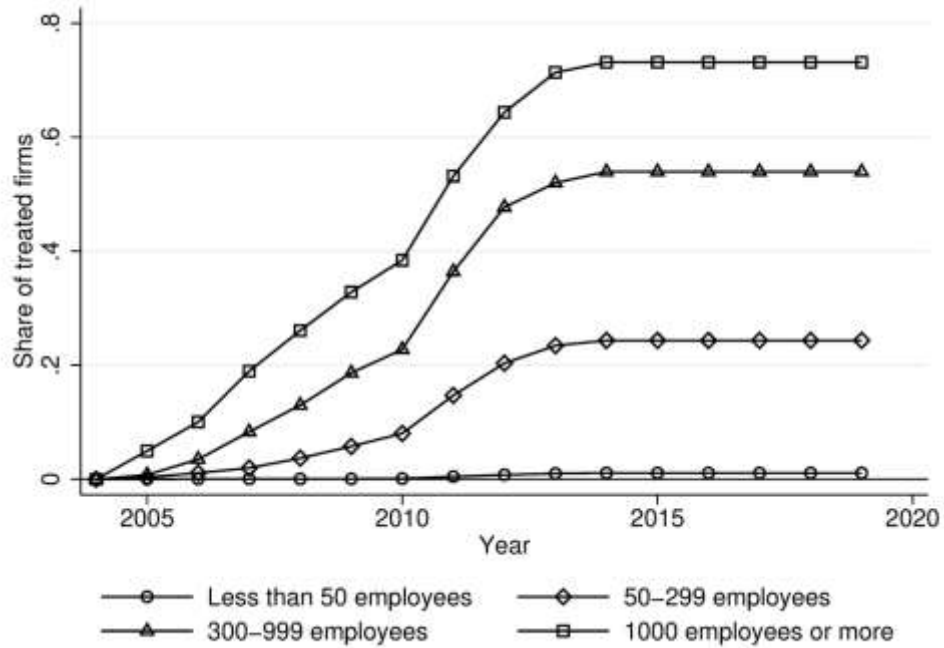
to gender equality may be both more likely to engage in gender equality bargaining and more likely to employ women or have greater female representation across occupations.

To address this potential endogeneity, we exploit the 2010 reform as a quasi-natural experiment. The reform introduced financial penalties for firms that failed to comply with the requirement to be covered by a collective agreement on gender equality or, in the absence of such an agreement, by a unilateral action plan. The reform applied to firms with 50 or more employees but not to firms below this threshold. Let G_i be an indicator equal to one if firm i has 50 or more employees in 2007, and let $Post_t$ be indicator indicating for the post-reform period. We use the interaction $G_i \times Post_t$ as an instrument for treatment.

A first requirement of our identification strategy is that the instrument strongly predicts treatment. Figure 2 provides graphical evidence of the first stage. Before 2010, the share of treated firms with fewer than 50 employees remained consistently below 1%, whereas treatment was more prevalent and increased over time among larger firms. More generally, the incidence of gender equality bargaining increased with firm size.

The 2010 reform was followed by a sharp acceleration in gender equality bargaining among firms with 50 or more employees, while no comparable change occurred among smaller firms. Among firms with 50-299 employees, for example, the share of treated firms increased from 1.1% in 2006 to 8% in 2010 and then to 24.3% in 2019. The increase was even more pronounced among firms with 300-999 employees: the share rose from 0% in 2004 to 22.8% in 2010 and reached 53.9% in 2019. By contrast, the share of treated firms with fewer than 50 employees remained below 2% throughout the period. These patterns provide evidence of a strong first-stage relationship between the reform and gender equality bargaining.

Figure 2 – Share of treated firms by firm size, 2004-2019



Source: DADS Fichiers postes; D@ccord database.

Note: A firm is considered treated in year t if it is covered by a gender equality agreement or action plan in that year.

Following de Chaisemartin & D’Haultfoeuille (2018), we estimate the causal effect of gender equality bargaining using a fuzzy Difference-in-Differences (DiD) design. This approach is appropriate because the share of treated firms increased sharply among firms with 50 or more employees following the 2010 reform, but compliance remained well below 100%. The fuzzy DiD estimator identifies the causal effect of gender equality bargaining under a set of assumptions. The first two are analogous to those required in a standard DiD design with a binary, absorbing treatment and full compliance. First, the parallel-trends assumption requires that, in the absence of the reform, outcomes would have evolved similarly in firms below and above the 50-employee threshold. Second, firms must not have anticipated the reform before its announcement.

Additional assumptions are required in a fuzzy DiD setting. In particular, the average treatment effect among treated firms must remain stable over time within both groups, and the treatment effect must be the same across the two groups. These additional assumptions are required, however, only when

treatment intensity also changes in the control group following the reform. As shown in Figure 2, the share of treated firms with fewer than 50 employees remains close to 0 both before and after the reform. Concerns arising from heterogeneous treatment effects between smaller and larger firms are therefore likely to be limited in our setting.

Figure 2 also shows that treatment adoption was gradual rather than immediate following the reform. We are therefore in a staggered adoption design where the intensity of treatment increases with firm size and over time. Although the reform was enacted in November 2010, compliance increased progressively as firms adjusted to the new legal requirements. We therefore allow for a transition period and define 2013 as the beginning of the post-reform period. We also exclude 2008 and 2009 from the pre-reform period to avoid potential confounding effects associated with the economic crisis. Firms are assigned to the treatment-eligible and control groups according to whether they had at least 50 employees or fewer than 50 employees in 2007 ($G \in \{0,1\}$), respectively. Our main specification thus compares the pre-reform (2004-2007) with the post-reform period (2013-2019), $T \in \{0,1\}$. To examine how the effects evolve over time, we further divide the post-reform period into 2013-2016 and 2017-2019.

To estimate the Local Average Treatment Effect (LATE), i.e. the effect of gender equality bargaining for compliers, we use the following Wald-DiD estimator:

$$W_{DID} = \frac{E(Y_{11}) - E(Y_{10}) - \{E(Y_{01}) - E(Y_{00})\}}{E(D_{11}) - E(D_{10}) - \{E(D_{01}) - E(D_{00})\}} \quad (1)$$

where Y_{gt} and D_{gt} denote, respectively, the outcome Y and the treatment dummy D for firms belonging to the group $G = g$ at $T = t$. The resulting estimate is a local effect: it captures the effect of gender equality bargaining on firms that complied with the legal requirement in response to the reform. The numerator of the Wald-DiD estimator is the Intention-To-Treat (ITT) effect of the reform and therefore captures the effect of exposure to the 2010 reform on firms' workforce composition and working hours. The denominator is the first-stage effect of the reform on treatment. We assess the strength of the first stage by testing the relationship between treatment status and the interaction between eligibility based on firm size and the post-reform period.

As shown by de Chaisemartin & D'Haultfoeuille (2018), the specification can also incorporate observed covariates. Let X denote a vector of covariates. The extended Wald-DiD estimator is defined as:

$$W_{DID,OLS}^X = \frac{\sum_{i \in \mathcal{J}_{11}} (Y_i - X_i' \widehat{\beta}_{10}^Y - X_i' \widehat{\beta}_{01}^Y + X_i' \widehat{\beta}_{00}^Y)}{\sum_{i \in \mathcal{J}_{11}} (D_i - X_i' \widehat{\beta}_{10}^D - X_i' \widehat{\beta}_{01}^D + X_i' \widehat{\beta}_{00}^D)} \quad (2)$$

where $\mathcal{J}_{11}$ stands for the group of firms with 50 employees or more, β_{gt}^Y and β_{gt}^D denote, respectively, the coefficients on X from OLS regressions of Y on X , and D on X , among firms belonging to the group g in $T = t$. Standard errors are obtained using 250 bootstrap replications.

Results

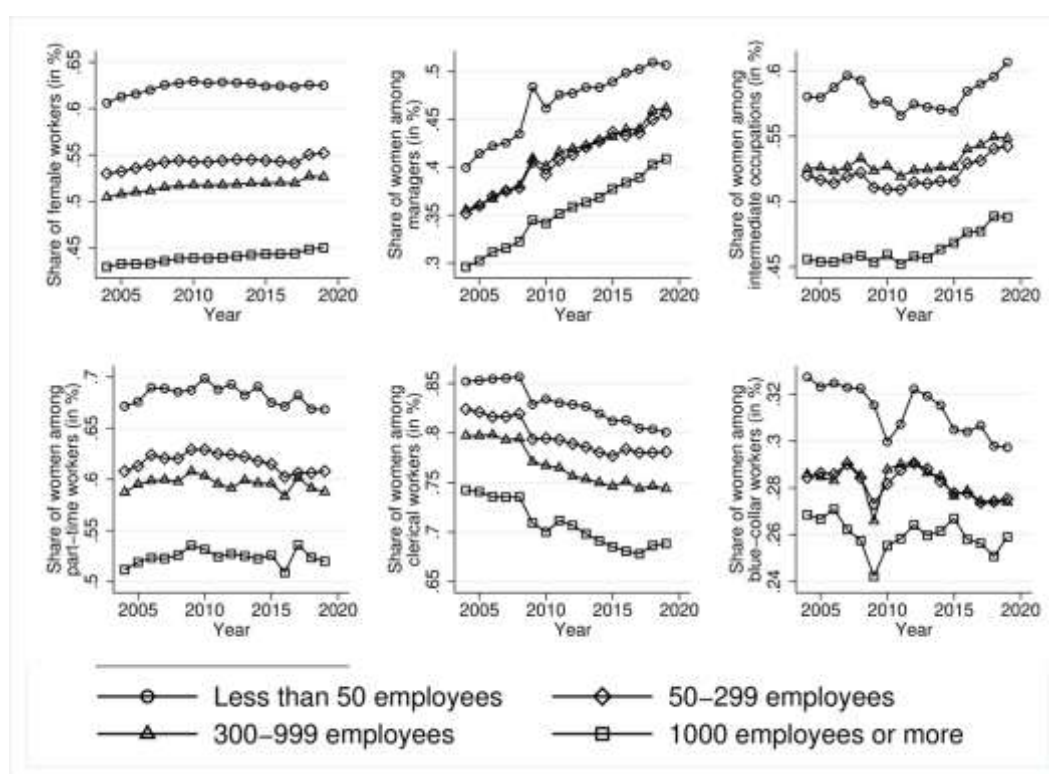
Figure 3 plots the evolution of firm-level outcomes by firm size. The share of women in the workforce follows a similar upward trend across all four firm-size groups, with no visible break around the 2010 reform among firms subject to the new financial penalties. The share of women among managers also increases throughout the period, more sharply than the overall share of women in the workforce, with no clear differences in trends across firm-size groups⁸.

More pronounced differences across firm sizes emerge for the share of women in intermediate occupations. In particular, the increase begins earlier among firms with 1,000 or more employees than among smaller firms. While the share increases across all firm-size groups from 2015 onward, the upward trend begins as early as 2013 among firms with 1,000 or more employees. By contrast, the share of women among clerical workers declines after 2010 across all firm sizes, with a more pronounced decrease among larger firms. The share of women among blue-collar workers also declines toward the end of the period, although year-to-year fluctuations are more pronounced, making differences in trends across firm sizes difficult to identify. Finally, the share of women among

⁸ The sharp increase observed in 2009 across all firm-size groups is partly attributable to a change in the coding of occupational categories that year, from a two-digit to a four-digit classification, which temporarily reduced the quality of the classification. Similar sharp changes are observed in 2009 in the shares of women in other occupational categories.

part-time workers decreases over time, particularly in firms with fewer than 300 employees. As with the other outcomes, there is no clear break in the trend following the reform among firms subject to the new financial penalties.

Figure 3 – Evolution of firm-level outcomes, 2004-2019



Source: DADS Fichiers postes

Table 3 and 4 report the ITT and LATE estimates, respectively. Table 3 reports the ITT effects of the 2010 reform on firm-level outcomes, comparing the post-reform period (2013-2019) with the pre-reform period (2004-2007). Panel A reports estimates without covariates, while Panel B includes industry controls⁹. To examine whether the effects vary over time, we also report estimates separately for 2013-2016 and 2017-2019.

⁹ We control for industry differences using 10 indicator variables: 1. Manufacturing industry, 2. Construction, 3. Motor vehicle retail sales, 4. Transports, 5. Hotels and restaurants, 6. Information, communication, 7. Banking, finance, insurance, real estate, 8. Business services, 9. Non-commercial services, 10. Household services.

As a placebo test of the parallel-trends assumption, Panel A also reports estimates comparing changes in outcomes between 2004-2005 and 2006-2007, two periods preceding the reform. The absence of significant differential changes in this placebo specification provides support for the parallel-trends assumption.

Table 4 reports the corresponding LATE estimates of the effect of gender equality bargaining, without and with industry controls. The first stage is strong: the interaction between eligibility based on firm size and the post-reform period increases the probability of treatment by 35.4 percentage points. The F-statistic for the excluded instrument is 2212.3, providing strong evidence against a weak first stage. The LATE estimates capture the local causal effect of gender equality bargaining for firms that complied with the bargaining requirement in response to the 2010 reform.

Consistent with the patterns shown in Figure 3, we find no significant effect of either the 2010 reform (ITT) or gender equality bargaining (LATE) on the overall share of women in the workforce, with or without covariates. More generally, both the ITT and LATE estimates are close to zero for most firm-level outcomes. The main exception is the share of women in intermediate occupations. The reform increases this share by approximately 2 p.p. on average over the 2013-2019 period. The effect is concentrated in 2013-2016 and is no longer statistically significant in 2017-2019. The pattern is similar with and without covariates. The LATE estimates are larger: gender equality bargaining increases the share of women in intermediate occupations by approximately 6 p.p. in 2013-2016. As with the ITT estimates, however, the effect does not persist in 2017-2019.

Table 3 - Effects of the 2010 reform on firm-level outcomes: ITT estimates

	Share of women among:					
	(1) All workers	(2) Managers	(3) Intermediate occupations	(4) Clerical workers	(5) Blue-collar workers	(6) Part-time workers
Panel A: Without covariates						
Post-reform period : 2013-2019	0.000 (0.007)	0.000 (0.010)	0.019** (0.008)	-0.004 (0.006)	0.002 (0.012)	0.000 (0.008)
Post-reform period : 2013-2016	-0.002	-0.004	0.021**	-0.008*	0.012	-0.003

	(0.007)	(0.012)	(0.009)	(0.004)	(0.013)	(0.008)
Post-reform period : 2017-2019	0.003	0.003	0.013	0.003	0.022*	0.004
	(0.007)	(0.012)	(0.009)	(0.007)	(0.011)	(0.008)
Placebo test: 2004-2005 vs. 2006-2007	-0.003	0.000	-0.013	-0.008	0.006	-0.008
	(0.007)	(0.009)	(0.012)	(0.006)	(0.012)	(0.009)
Panel B: With covariates						
Post-reform period : 2013-2019	0.008	0.002	0.021**	0.000	-0.004	0.006
	(0.007)	(0.009)	(0.009)	(0.006)	(0.010)	(0.007)
Post-reform period : 2013-2016	0.006	-0.001	0.024***	-0.005	0.001	-0.003
	(0.007)	(0.010)	(0.009)	(0.005)	(0.011)	(0.008)
Post-reform period : 2017-2019	0.008	0.003	0.013	0.007	-0.010	0.009
	(0.007)	(0.010)	(0.010)	(0.006)	(0.013)	(0.009)
Number of observations	20,448	17,805	18,961	20,094	15,833	20,178

Source: DADS Fichiers postes, D@ccord database.

Notes: Each column reports the estimated effect of the 2010 reform on a firm-level outcome. All dependent variables are expressed as shares. The reference period is 2004-2007. Panel A reports Intention-to-Treat (ITT) estimates from the Wald-DiD estimator without covariates, corresponding to the numerator of Equation (1). Panel B reports ITT estimates from the Wald-DiD estimator with covariates, corresponding to the numerator of Equation (2). The covariates include 10 industry dummies. The placebo test reported in Panel A compares 2004-2005 and 2006-2007, two periods preceding the reform. Standard errors, reported in parentheses, are obtained using 250 bootstrap replications. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 4 - Effects of gender equality bargaining on firm-level outcomes: LATE estimates

	Share of women among:					
	(1)	(2)	(3)	(4)	(5)	(6)
	All workers	Managers	Intermediate occupations	Clerical workers	Blue-collar workers	Part-time workers
Panel A: Without covariates						
First-stage : coef.	0.354***	0.354***	0.354***	0.354***	0.354***	0.354***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
F-test for excluded instruments	2212.3	2212.3	2212.3	2212.3	2212.3	2212.3
Post-reform period : 2013-2019	0.001	0.000	0.054**	-0.013	0.005	-0.001
	(0.019)	(0.030)	(0.024)	(0.018)	(0.031)	(0.022)
Post-reform period : 2013-2016	-0.006	-0.012	0.060**	-0.023*	0.033	-0.008
	(0.019)	(0.035)	(0.025)	(0.012)	(0.035)	(0.023)
Post-reform period : 2017-2019	0.010	0.010	0.037	0.007	0.0588*	0.010
	(0.019)	(0.032)	(0.024)	(0.020)	(0.029)	(0.024)
Panel B: With covariates						
Post-reform period : 2013-2019	0.023	0.007	0.060*	-0.001	-0.011	0.018
	(0.018)	(0.026)	(0.028)	(0.018)	(0.028)	(0.022)
Post-reform period : 2013-2016	0.018	-0.003	0.067***	-0.015	0.003	0.010
	(0.018)	(0.028)	(0.025)	(0.015)	(0.030)	(0.025)
Post-reform period : 2017-2019	0.024	0.009	0.036	0.020	-0.027	0.024
	(0.020)	(0.027)	(0.027)	(0.020)	(0.036)	(0.025)

Number of observations	20,448	17,805	18,961	20,094	15,833	20,178
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Source: DADS Fichiers postes, D@ccord database.

Notes: Each column reports the estimated effect of gender-equality bargaining on a firm-level outcome. All dependent variables are expressed as shares. The reference period is 2004-2007. Panel A reports Local Average Treatment Effects (LATE) estimates from the Wald-DiD estimator without covariates, corresponding to Equation (1). Panel B reports LATE estimates from the Wald-DiD estimator with covariates, corresponding to Equation (2). The covariates include 10 industry dummies. The first-stage coefficient is 0.354 (SE = 0.00753), and the F-statistic for the excluded instrument is 2212.3. Standard errors, reported in parentheses, are obtained using 250 bootstrap replications. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Overall, these results provide little evidence that the 2010 reform or gender equality bargaining substantially affected firms' workforce composition or the representation of women among part-time workers in the years following the reform. This finding is consistent with Coly (2022), who finds no effect of the reform on the gender wage gap. The results therefore provide little support for H1, according to which gender equality bargaining may increase gender balance in firms' workforces, or H2, according to which it may increase the share of women among part-time workers. Although we find limited effects on firm-level outcomes, gender equality bargaining may still affect women and men differently following the birth of a child. We examine this possibility in the second part of the paper.

The effect of gender equality bargaining on the child penalty

Empirical strategy

In this section, we estimate the causal effect of gender equality bargaining on the child penalty in earnings at each event time e following the birth of the first child. We first focus on mothers, as previous research shows that fathers' earnings are largely unaffected by parenthood (Kleven, Landais, & Søgaaard, 2019). Following the child penalty literature (Kleven et al., 2025; Kleven, Landais, & Søgaaard, 2019), we use $e = -1$, i.e. one year before the birth of the first child, as the reference year. The dependent variable is the change in a mother's total wage income between $e = -1$ and event time e , denoted $\Delta Y_{i,e} = Y_{i,e} - Y_{i,-1}$ for mother i .

Let D_i be a treatment indicator equal to one if mother i is employed, in the year of her first child's birth, in an establishment that has already signed a gender equality agreement or action plan. Let $\Delta Y_{i,e}^1$ denote the potential outcome at event time e under treatment and $\Delta Y_{i,e}^0$ the corresponding potential

outcome in the absence of treatment. Our parameter of interest is the Average Treatment Effect on the Treated (ATT) at event time e : $ATT_e = E(\Delta Y_{i,e}^1 - \Delta Y_{i,e}^0)$. A simple comparison of earnings changes between treated and untreated mothers would identify the causal effect of gender equality bargaining only if treatment status were independent of observed and unobserved determinants of these changes. This assumption is unlikely to hold. For example, firms with stronger social dialogue or a greater commitment to gender equality and work-life balance may be more likely to adopt a gender equality agreement or action plan and may also offer working conditions that affect mothers' earnings trajectories following childbirth.

To address this potential endogeneity, we again exploit the 2010 reform introducing financial penalties for firms that failed to comply with gender equality bargaining requirements. Let G_i be an indicator variable equal to one if mother i was employed in a firm with 50 or more employees one year before the birth of her first child, and let $Post_i$ indicate that her first child was born in the post-reform period. We use the interaction $G_i \times Post_i$ as an instrument for treatment.

Table 5 reports the first-stage estimates. The interaction between employment in a firm with 50 or more employees and belonging to the post-reform cohort increases the probability of treatment by 43.4 percentage points. The F-statistic for the excluded instrument is 1500.88, providing strong evidence against a weak first stage.

Table 5 - First-stage estimates

First-stage : coef.	0.434*** (0.006)
F-test for excluded instruments	1500.88

Source: DADS-EDP panel, D@ccord database.

Note: The table reports OLS estimates from the first-stage regression of the treatment indicator on an indicator for employment in a firm with 50 or more employees one year before the birth of the first child, an indicator for belonging to the post-reform cohort, and their interaction. The interaction term is the excluded instrument. Standard errors are reported in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We divide mothers into two groups according to the size of the firm in which they were employed one year before the birth of their first child: those employed in firms with fewer than 50 employees ($G = 0$) and those employed in firms with 50 or more employees ($G = 1$). We also distinguish two cohorts based on the timing of the first birth relative to the reform, $T \in \{0,1\}$. The reform was enacted on November 9, 2010, and financial penalties came into force in 2012. Because compliance increased gradually, we treat 2010-2012 as a transition period and define mothers whose first child was born in 2013-2014 as the post-reform cohort. To avoid potential confounding effects associated with the 2008 economic crisis, we define mothers whose first child was born in 2006-2007 as the pre-reform cohort. The estimation sample consists of 11,677 mothers, of whom 2,444 are treated and 9,233 are untreated. The pre-reform cohort includes 6,135 mothers and the post-reform cohort 5,542.

To estimate the Local Average Treatment Effect (LATE) at each event time e , i.e. the effect of gender equality bargaining on compliers e years after the birth of their first, we use the following Wald-DiD estimator:

$$W_{DiD}^e = \frac{E(\Delta Y_{11}^e) - E(\Delta Y_{10}^e) - \{E(\Delta Y_{01}^e) - E(\Delta Y_{00}^e)\}}{E(D_{11}) - E(D_{10}) - \{E(D_{01}) - E(D_{00})\}} \quad (3)$$

where ΔY_{gt}^e denotes the motherhood effect on outcome Y between $e = 1$ and event time e for mothers in group $G = g$ and whose first child was born in $T = t$. D_{gt} stands for the treatment dummy variable for individuals of the group $G = g$ and whose first child was born in $T = t$. The resulting LATE captures the causal effect of gender equality bargaining on the child penalty among mothers employed in firms that complied with the bargaining requirement in response to the 2010 reform. The numerator of the Wald-DiD estimator is the Intention-to-Treat (ITT) effect of the reform. It therefore captures the effect of exposure to the reform on mothers' earnings trajectories following childbirth among firms subject to the bargaining requirement.

As in the previous section, we estimate an extended specification that includes a parsimonious set of covariates. Because characteristics measured after the reform may themselves be affected by the

reform or by subsequent behavioral responses, we restrict the covariates to three characteristics measured one year before the birth of the first child: the mother's age, labor market experience, and occupational level. This specification is similar to a Mincerian wage equation (Mincer, 1974), with occupational level used as a proxy for educational attainment. The extended Wald-DiD estimator is defined as:

$$W_{DID,OLS}^{X,e} = \frac{\sum_{i \in \mathcal{J}_{11}} (\Delta Y_i^e - X_i' \widehat{\beta}_{10}^{\Delta Y^e} - X_i' \widehat{\beta}_{01}^{\Delta Y^e} + X_i' \widehat{\beta}_{00}^{\Delta Y^e})}{\sum_{i \in \mathcal{J}_{11}} (D_i - X_i' \widehat{\beta}_{10}^D - X_i' \widehat{\beta}_{01}^D + X_i' \widehat{\beta}_{00}^D)} \quad (4)$$

where $\mathcal{J}_{11}$ denotes individuals employed in firms with 50 employees or more ($G = 1$) whose first child was born after the reform ($T = 1$), and $\beta_{gt}^{\Delta Y^e}$ and β_{gt}^D are the coefficients on X from OLS regressions of ΔY_i^e and D on X , respectively, among workers in group g whose first child was born in $T = t$. Standard errors are obtained using 250 bootstrap replications. To account for dependence across observations within firms, we implement a block bootstrap at the firm level.

Identification additionally requires that the instrument affect mothers' earnings trajectories following childbirth only through its effect on gender equality bargaining. A potential concern is that earnings dynamics or gender inequalities may evolve differently across firms of different sizes (Card et al., 2016). Our design accounts for permanent differences by firm size and common differences across cohorts, while identification comes from their interaction. We assess whether differential trends by firm size could drive our results using a placebo test based entirely on pre-reform cohorts. Specifically, we redefine the two cohorts using mothers whose first child was born in 2006 ($T = 0$) and 2007 ($T = 1$) and re-estimate the ITT effect. Because both cohorts precede the 2010 reform, a statistically significant placebo estimate would raise concerns about the identifying assumption.

Results

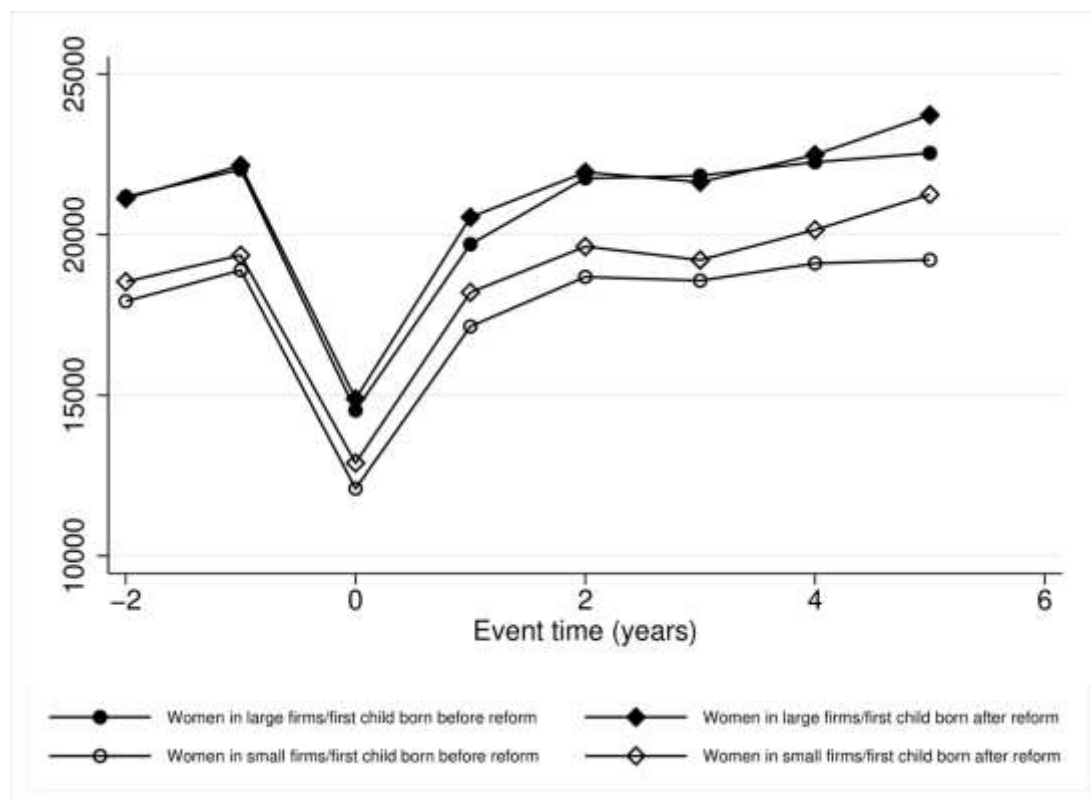
The effect of gender equality bargaining on mothers' total wage income

Figure 4 plots mothers' total wage income around the birth of their first child, separately by firm size and birth cohort. We distinguish mothers employed in firms with fewer than 50 employees from those employed in firms with 50 or more employees, and mothers whose first child was born before the reform from those whose first child was born after the reform. Total wage income is measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, paid overtime, benefits in kind, bonuses, but excludes maternity leave allowances.

Before the birth of the first child, total wage income follows similar trends across firm-size groups within each cohort. For the pre-reform cohort, total wage income trajectories following childbirth are also very similar between mothers employed in smaller and larger firms. For the post-reform cohort, however, the trajectories begin to diverge several years after childbirth: between the third and fifth years after the first birth, mothers' total wage income increases less in firms with 50 or more employees than in smaller firms. This descriptive evidence suggest that the reform may have affected mothers' earnings trajectories in the years following childbirth.

Table A.2 in the External Appendix reports total wage income values for each group of mothers and changes relative to the year preceding the first birth. In the pre-reform cohort, total wage income five years after the first birth is 513 euros higher than one year before the birth among mothers employed in larger firms, compared with 323 euros among those employed in smaller firms. In the post-reform cohort, the corresponding increases are 1,571 euros and 1,895 euros, respectively. The resulting difference-in-differences estimate five years after the first birth is therefore approximately -514 euros.

Figure 4 - Evolution of mothers' total wage income around the birth of the first child, by firm size and birth cohort



Source: DADS-EDP panel; D@ccord database.

Note: Total wage income is measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, paid overtime, benefits in kind, bonuses, and indemnities, but excludes maternity leave allowances. The pre-reform cohort consists of mothers whose first child was born between 2004 and 2007, while the post-reform cohort consists of mothers whose first child was born between 2013 and 2014. Small firms have fewer than 50 employees, and large firms have 50 or more employees.

Figure 5 presents the Wald-DiD estimates at each event time, together with 95% confidence intervals.

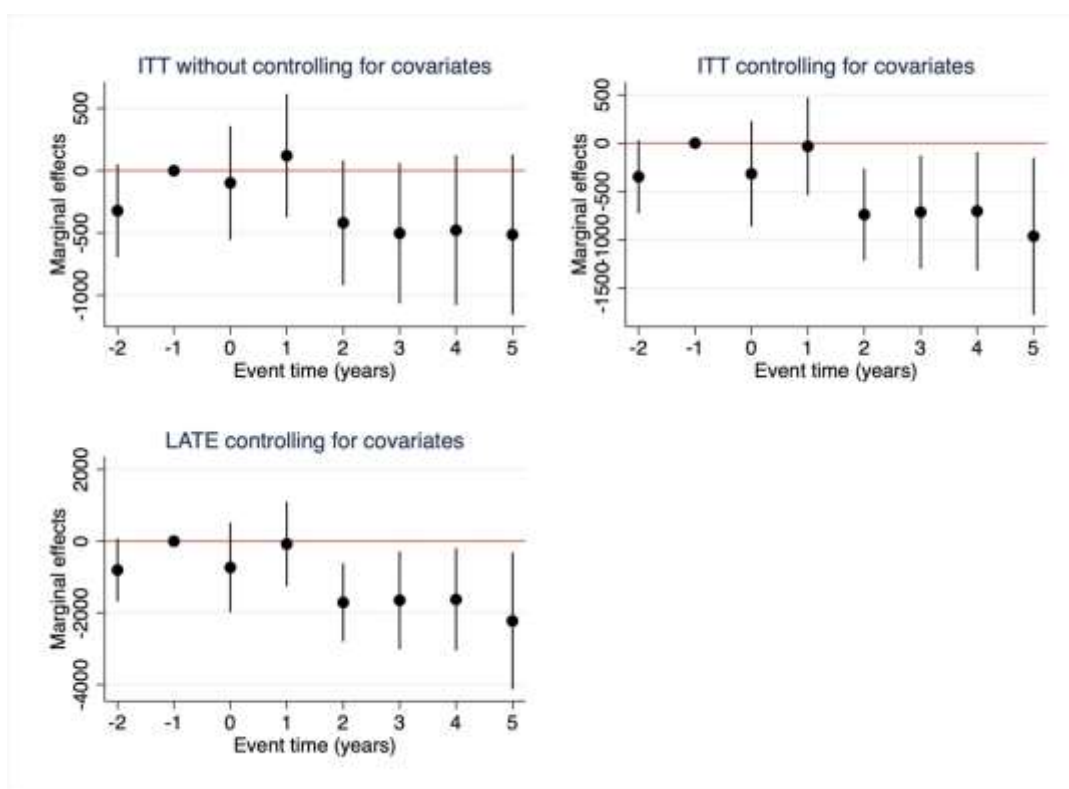
The top-left panel reports the ITT estimates without covariates, the top right panel reports the ITT estimates with covariates, and the bottom-left panel reports the LATE estimates with covariates. The corresponding point estimates and standard errors are reported in Table A.3 in the Appendix. The specifications with covariates control for mothers' age, labor market experience, and occupational level one year before the birth of their first child.

Without covariates, the ITT estimates are close to zero immediately following the first birth and become negative two to three years later, reaching approximately -400 to -500 euros. These estimates are statistically significant at the 10% level in the second and third years after childbirth but are no longer statistically significant thereafter. The results are more pronounced when covariates are included. From the second through the fifth year after the first birth, the ITT estimates are negative and statistically significant at the 5% level. They are approximately -700 euros between the second and fourth years and reach -964 euros five years after the first birth. Relative to average total wage income of approximately 23,000 euros among mothers employed in firms with 50 or more employees one year before childbirth, the estimate at event time $e = 5$ corresponds to approximately 4.2% of annual income.

The LATE estimates show a similar pattern but are larger in magnitude. Five years after the first birth, the estimated effect of gender equality bargaining on total wage income among compliers is -2,224 euros. This corresponds to approximately 9.7% of average total wage income among mothers employed in firms subject to the reform one year before childbirth. Taken together, the ITT and LATE estimates provide evidence that the reform and the resulting increase in gender equality bargaining reduced mothers' total wage income in the years following the birth of their first child, with the effects emerging two years after childbirth and becoming larger over time. These findings are consistent with the possibility that some workplace measures associated with gender equality bargaining may affect mothers' career trajectories following childbirth. They also relate to Lucifora et al. (2021), who show that some child-related workplace policies may adversely affect women's access to internal promotions¹⁰.

¹⁰ The increase in gender equality bargaining following the 2010 reform was more pronounced among firms with 300 or more employees than among firms with 50-299 employees. We therefore assess the robustness of our results by restricting the treated group to mothers employed in firms with 300 or more employees. In this specification, the first-stage coefficient increases from 0.434 to 0.483. Figure A.1 reports the corresponding ITT and LATE estimates. The results are similar but somewhat larger in magnitude. Five years after the first birth, the ITT estimate with covariates is approximately -1,000 euros, compared with -965 euros in the baseline specification.

Figure 5 – Effects of the 2010 reform and gender equality bargaining on mothers' total wage income



Source: DADS-EDP panel; D@ccord database.

Note: Total wage income is measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, paid overtime, benefits in kind, bonuses, and indemnities, but excludes maternity leave allowances. Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. Estimates are based on 11,677 mothers, of whom 2,444 are treated. The top-left panel reports the Intention-To-Treat (ITT) estimates without covariates (numerator of Equation 3). The top-right panel reports the ITT estimates with covariates (numerator of Equation 4). The bottom-left panel reports the Local Average Treatment Effect (LATE) with covariates (Equation 4). Covariates include mothers' age, labor market experience, and occupational level measured one year before the first birth. The vertical bars indicate 95% confidence intervals based on 250 bootstrap replications, with resampling performed at the firm level.

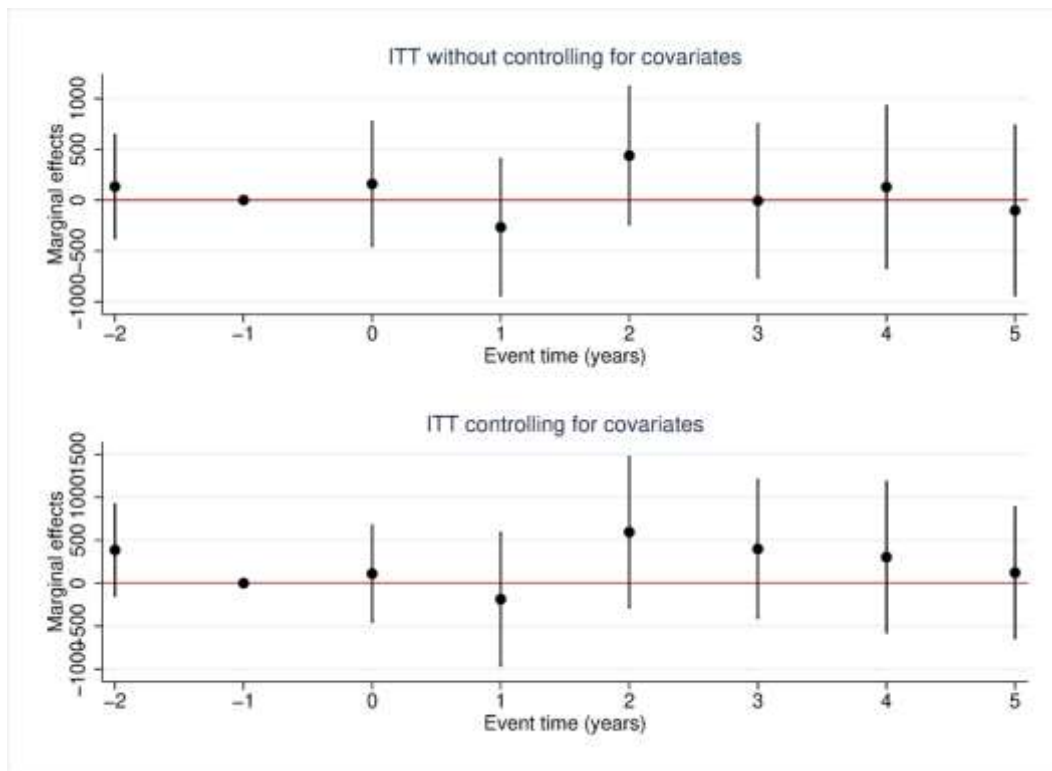
Robustness checks

Placebo test using pre-reform birth cohorts

We first conduct a placebo test to assess whether our main results could be driven by differential earnings trends between mothers employed in smaller and larger firms. We reproduce the ITT analysis using two cohorts whose first child was born before the 2010 reform. Specifically, we define mothers whose first child was born in 2006 as the placebo pre-period cohort and those whose first child was born in 2007 as the placebo post-period cohort. Because both cohorts precede the reform, a statistically significant placebo estimate would raise concerns that differential earnings dynamics by firm size, rather than the reform, drive our main results.

Figure 6 reports the placebo ITT estimates with and without covariates. We do not report LATE estimates because the interaction between firm size and the placebo post-period cohort is not expected to generate a meaningful first-change in treatment. The ITT estimates remain close to zero throughout the event window and are not statistically significant, with or without covariates. In particular, we find no evidence of a differential change in total wage income five years after childbirth. These results provide additional support for our identification strategy by showing no evidence of differential earnings trajectories between mothers employed in smaller and larger firms when the same design is applied to two pre-reform cohorts.

Figure 6 – Placebo ITT estimates using pre-reform birth cohorts



Source: DADS-EDP panel; D@ccord database.

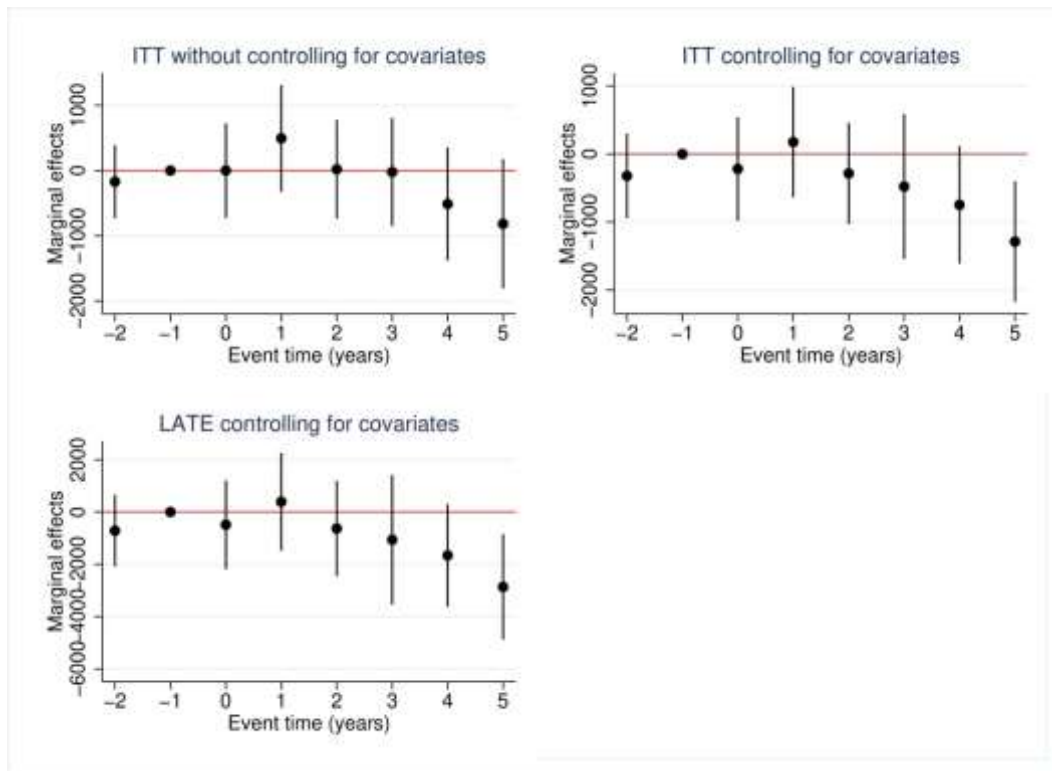
Note: The dependent variable is total wage income, measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, paid overtime, benefits in kind, bonuses, and indemnities, but excludes maternity leave allowances. Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. For this placebo test, mothers whose first child was born in 2006 constitute the placebo pre-period cohort, while those whose first child was born in 2007 constitute the placebo post-period cohort. The top panel reports the Intention-To-Treat (ITT) estimates without covariates (numerator of Equation 3). The bottom panel reports the ITT estimates with covariates (numerator of Equation 4). Covariates include mothers' age, labor market experience, and occupational level measured one year before the first birth. The vertical bars indicate 95% confidence intervals based on 250 bootstrap replications, with resampling performed at the firm level.

Results for one-child mothers

Our baseline estimates may capture not only the effect associated with the birth of the first child but also, for some mothers, changes associated with subsequent births. This is potentially relevant because our event window extends to five years after the first birth. To assess whether subsequent births drive our results, we re-estimate our specifications on the subsample of mothers who have only one child over the observation period. This restriction yields a sample of 4,744 mothers, of whom 922 are treated.

Figure 7 reports the ITT estimates with and without covariates and the LATE estimates with covariates for this subsample. The results exhibit a pattern similar to that obtained for the full sample. In particular, the estimated effect of gender equality bargaining on total wage income remains negative several years after the first birth. Five years after the birth, the LATE estimate with covariates is approximately -2,860 euros. The persistence of the negative effect in this subsample indicates that our main findings are not driven solely by the occurrence of subsequent births during the five-year follow-up period.

Figure 7 – Effects of the 2010 reform and gender equality bargaining on total wage income among one-child mothers



Source: DADS-EDP panel, D@ccord database.

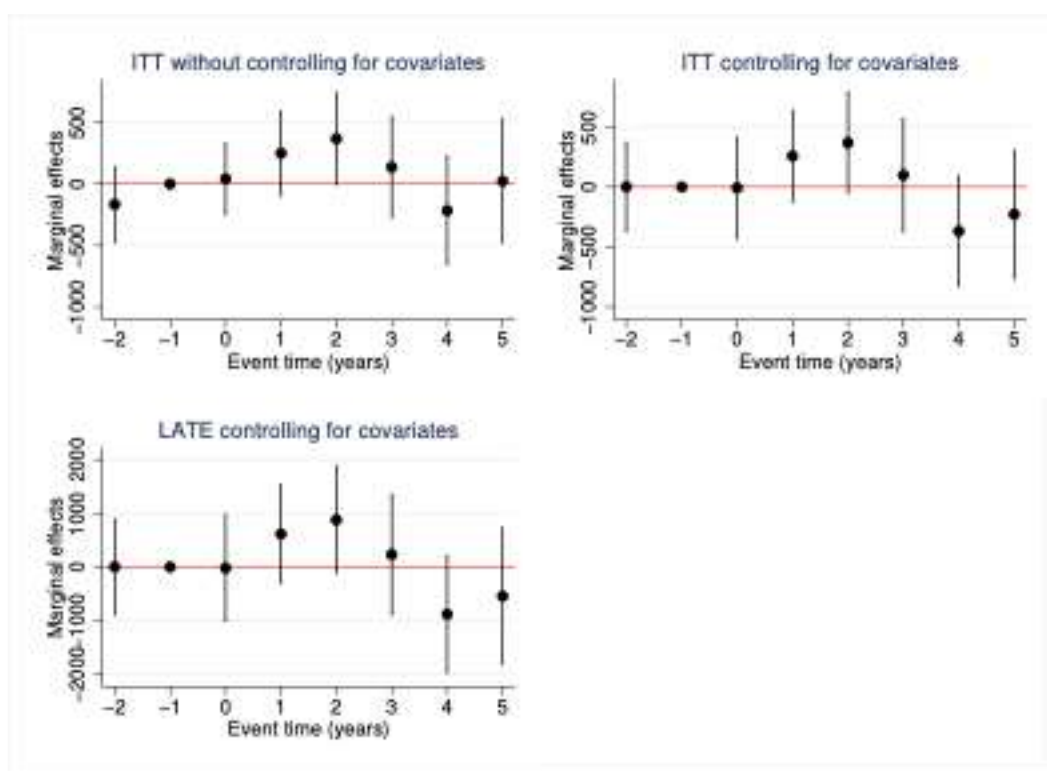
Note: The dependent variable is total wage income, measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, paid overtime, benefits in kind, bonuses, and indemnities, but excludes maternity leave allowances. Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. Estimates are based on 4,744 mothers who have only one child over the observation period, of whom 922 are treated. The top-left panel reports the Intention-To-Treat (ITT) estimates without covariates (numerator of Equation 3). The top-right panel reports the ITT estimates with covariates (numerator of Equation 4). The bottom-left panel reports the Local Average Treatment Effect (LATE) with covariates (Equation 4). Covariates include mothers' age, labor market experience, and occupational level measured one year before the first birth. The vertical bars indicate 95% confidence intervals based on 250 bootstrap replications, with resampling performed at the firm level.

Results for fathers

Our baseline analysis focuses on mothers because the child penalty is primarily concentrated among women. As an additional robustness check, we examine whether similar effects are observed among fathers. This comparison is informative because the negative effects estimated for mothers could potentially reflect broader changes affecting all workers in firms subject to the reform rather than changes specifically related to mothers' labor market outcomes. For example, if the implementation of gender equality measures temporarily disrupted firm organization or reduced productivity, earnings effects might also be observed among fathers.

We therefore replicate our analysis using a sample of 21,441 fathers, of whom 4,112 are treated. Figure 8 reports the ITT estimates with and without covariates and the LATE estimates with covariates. In contrast to the results for mothers, we find no statistically significant effect on fathers' total wage income following the birth of their first child. In particular, we find no evidence of a negative effect five years after the first birth. These results suggest that the negative earnings effects documented for mothers are not part of a comparable decline in fathers' earnings in firms affected by the reform. They are therefore difficult to reconcile with mechanisms that would generate similar earnings effects for both mothers and fathers, such as a general deterioration in firm performance.

Figure 8 – Effects of the 2010 reform and gender equality bargaining on fathers' total wage income



Source: DADS-EDP panel; D@ccord database.

Note: The dependent variable is total wage income, measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, paid overtime, benefits in kind, bonuses, and indemnities, but excludes maternity leave allowances. Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. Estimates are based on 21,441 fathers, of whom 4,112 are treated. The top-left panel reports the Intention-To-Treat (ITT) estimates without covariates (numerator of Equation 3). The top-right panel reports the ITT estimates with covariates (numerator of Equation 4). The bottom-left panel reports the Local Average Treatment Effect (LATE) with covariates (Equation 4). Covariates include mothers' age, labor market experience, and occupational level measured one year before the first birth. The vertical bars indicate 95% confidence intervals based on 250 bootstrap replications, with resampling performed at the firm level.

Mechanisms

Decomposing the effect on total wage income

The child penalty in earnings may operate through several channels, including changes in labor supply along both the intensive and the extensive margins and changes in hourly wages (Cortés & Pan, 2019). On the intensive margin, mothers may reduce their working hours or shift toward more regular work schedules following childbirth (Azmat & Ferrer, 2017; Bertrand et al., 2010; Bolotnyy & Emanuel, 2022;

Cha & Weeden, 2014; Cook et al., 2020; Goldin, 2014; Goldin & Katz, 2016). On the extensive margin, mothers may temporarily or permanently leave employment following the birth of their first child (Olivetti & Petrongolo, 2017; Périvier & Verdugo, 2024). Career interruptions and reductions in working hours may also have persistent consequences for subsequent wage growth (Adda et al., 2017; Blundell et al., 2016; Lucifora et al., 2021).

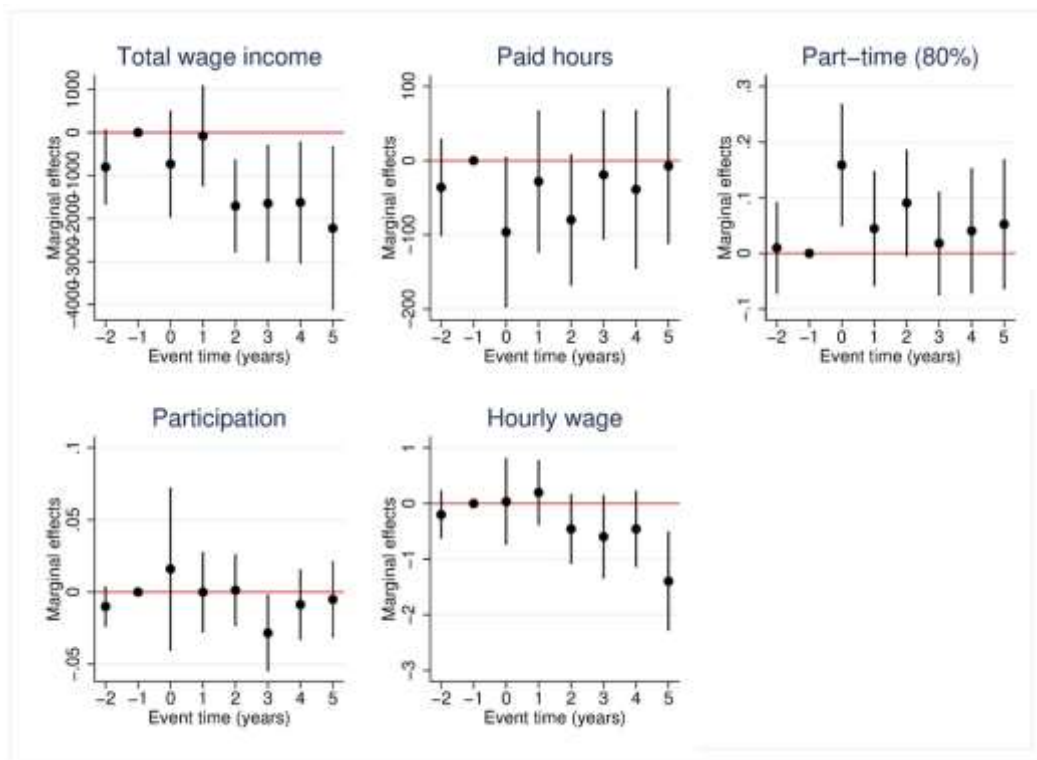
Motivated by this literature, we examine three potential channels through which gender equality bargaining may affect mothers' total wage income: (i) annual paid hours¹¹, (ii) labor market participation, and (iii) hourly wages. Examining these outcomes separately allows us to assess whether the negative effect on total wage income documented above is associated primarily with changes in labor supply or with changes in hourly wages.

To further characterize adjustments along the intensive margin, we construct an indicator equal to one if annual paid hours fall below 1,440. Relative to 1,800 annual hours for a full-time worker, this threshold corresponds to a reduction in working time of at least 20%.

Figure 9 reports the LATE estimates and corresponding 95% confidence intervals for each outcome at each event time e . The corresponding ITT and LATE estimates are reported in Table A.4. As in Table A.3, Column 2 reports the ITT without covariates, Column 3 reports the ITT estimates controlling for mothers' age, labor market experience, and occupational level, and Column 4 reports the corresponding LATE estimates.

¹¹ As Pora & Wilner (2024) note, hours worked during maternity leave are recorded as zero for hourly workers. During this period, employees receive payments from the Social Security Administration rather than their employer. However, for workers who are not paid by the hour, employers record the number of days worked, which remains constant during maternity leave. For these workers, paid hours are derived from paid days and are therefore non-zero. As a result, the DADS dataset overestimates the number of hours worked in the year of childbirth.

Figure 9 – Effects of gender equality bargaining on mothers' total wage income and its components



Source: DADS-EDP panel; D@ccord database.

Note: Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. Estimates are based on 11,677 mothers, of whom 2,444 are treated. Each panel reports LATE estimates from the extended Wald-DiD specification, controlling for mothers' age, labor market experience, and occupational level measured one year before the first birth. The vertical bars indicate 95% confidence intervals based on 250 bootstrap replications, with resampling performed at the firm level.

Gender equality bargaining is associated with a reduction in mothers' annual paid hours in the year of their first birth ($e = 0$). The ITT estimates range from -42.1 to -44.5 hours, while the corresponding LATE estimate is -96.4 hours. However, these estimates are not statistically significant, and therefore

provide no conclusive evidence that gender equality bargaining reduces annual paid hours in the year of childbirth.

By contrast, gender equality bargaining significantly increases the probability of part-time employment in the year following the first birth. This result indicates that gender equality bargaining affects mothers' employment arrangements shortly after childbirth. However, we find no evidence that this effect is accompanied by a persistent reduction in annual paid hours: the estimated effects on annual paid hours from the second year after childbirth onward are not statistically significant. Overall, these results provide partial support for *H3: Gender equality bargaining may influence mothers' labor supply shortly after childbirth, encouraging them to reduce their working hours*. Gender equality bargaining appears to affect mothers' labor supply shortly after childbirth through an increase in part-time employment, but we find no robust evidence of a sustained reduction in annual paid hours.

A different pattern emerges for hourly wages. Five years after the first birth, gender equality bargaining reduces mothers' hourly wages by approximately 1.4 euros, and the estimate is statistically significant. This result suggests that the negative effect on total wage income observed several years after childbirth cannot be explained solely by contemporaneous reductions in annual paid hours or labor market participation. One possible interpretation is that employment adjustments following childbirth, including greater use of part-time work, may have consequences for subsequent wage progression. This interpretation is consistent with Lucifora et al. (2021), who discuss how family-related workplace practices may contribute to slower career progression for mothers. However, our estimates do not allow us to identify directly whether the hourly wage effect operates through promotion, occupational mobility, wage growth within jobs, or other mechanisms.

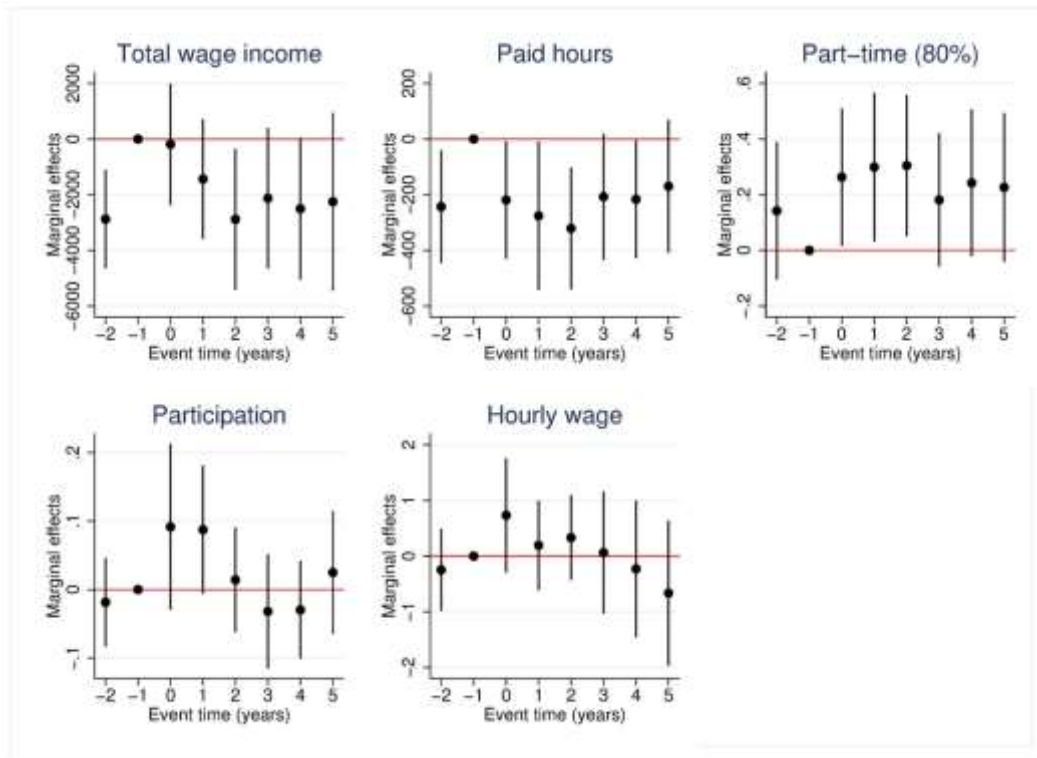
Finally, the estimated effects on paid hours, labor market participation, and hourly wages at $e = -2$ are small and statistically insignificant. This provides reassuring evidence that differential changes in these outcomes are not already apparent before the birth of the first child.

Heterogeneity across the earnings distribution

Previous research documents substantial heterogeneity in child penalties across the earnings distribution (Angelov et al., 2016; Pora & Wilner, 2025). We therefore examine whether the effects of gender equality bargaining on the child penalty differ according to mothers' pre-birth earnings.

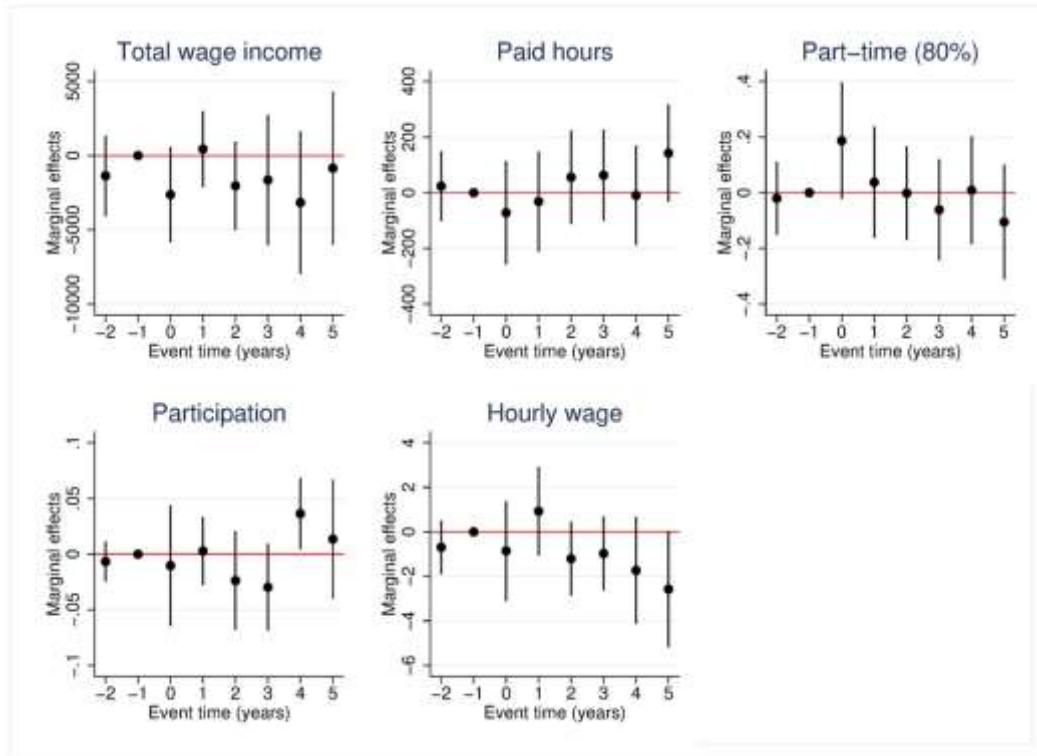
We separately estimate the effects for mothers in the first and fourth quartiles of the pre-birth total wage income distribution. The first-quartile sample contains 2,957 mothers, of whom 473 are treated, while the fourth-quartile sample contains 2,912 mothers, of whom 772 are treated. Figures 10 and 11 report the LATE estimates for total wage income and its components for mothers in the first and fourth quartiles, respectively. These specifications include the same covariates as the baseline specification, except for occupational level.

Figure 10 – Effects of gender equality bargaining on total wage income and its components: first quartile of pre-birth earnings



Note: Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. Estimates are based on 2,957 mothers in the first quartile of the pre-birth earnings distribution, of whom 473 are treated. Each panel reports LATE estimates from the extended Wald-DiD specification, controlling for mothers' age and labor market experience measured one year before the first birth. The vertical bars indicate 95% confidence intervals based on 250 bootstrap replications, with resampling performed at the firm level.

Figure 11 – Effects of gender equality bargaining on total wage income and its components: fourth quartile of pre-birth earnings



Note: Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. Estimates are based on 2,912 mothers in the fourth quartile of the pre-birth earnings distribution, of whom 772 are treated. Each panel reports LATE estimates from the extended Wald-DiD specification, controlling for mothers' age and labor market experience measured one year before the first birth. The vertical bars indicate 95% confidence intervals based on 250 bootstrap replications, with resampling performed at the firm level.

Among mothers in the first quartile of the pre-birth earnings distribution, gender equality bargaining significantly reduces annual paid hours during the first two years following childbirth. The estimated reduction reaches approximately 330 hours per year. Consistent with this result, bargaining also increases the probability of working substantially fewer hours. These effects are concentrated in the first two years following childbirth and are no longer statistically significant from the third year onward. Total wage income also declines by approximately 2,000 euros, although this estimate is statistically significant only at the 10% level.

The pattern differs among mothers in the fourth quartile. Gender equality bargaining is associated with a reduction of approximately 60 annual paid hours in the first year following childbirth, but the estimate is not statistically significant. We find no evidence of a persistent reduction in annual paid hours in subsequent years. By contrast, a significant negative effect on hourly wages emerges several

years after childbirth. Five years after the first birth, the estimated LATE on hourly wages is approximately -2.2 euros and is statistically significant at the 5% level.

Taken together, these results indicate substantial heterogeneity in the channels through which gender equality bargaining affects mothers following childbirth. Among lower-earning mothers, the effects are concentrated primarily in labor supply adjustments during the first two years after childbirth. Among higher-earning mothers, we find little evidence of persistent reductions in annual paid hours, but a negative hourly wage effect emerges several years after childbirth. These results provide support for H4, according to which the effects of gender equality bargaining on the child penalty may vary across the earnings distribution.

Conclusions

This paper examines the effects of firm-level gender equality bargaining on firms' workforce composition and the child penalty. We combine an exploratory analysis of gender equality agreements and action plans with causal evidence from French administrative data. To address the endogeneity of gender equality bargaining, we exploit the 2010 reform introducing financial penalties for firms with 50 or more employees that failed to comply with bargaining requirements. Using a fuzzy difference-in-differences design, we estimate both the effect of exposure to the reform and the effect of gender equality bargaining among firms that complied with the requirement in response to the reform.

Our results show limited effects on firms' workforce composition. We find no significant effect on the overall share of women in the workforce or on their representation among part-time workers. The main exception is a temporary increase in the share of women in intermediate occupations, concentrated in the first years following the reform. Overall, these findings provide little support for H1 and H2 and suggest that the substantial increase in gender equality bargaining following the reform did not translate into broad changes in firms' workforce composition over 2013-2019.

At the individual level, however, we find evidence that gender equality bargaining affects mothers' labor market outcomes following childbirth. The estimated effect on mothers' total wage income is close to zero immediately after the birth of the first child but becomes negative from the second year onward. Five years after the first birth, the estimated ITT effect of the reform is approximately -964 euros, while the estimated LATE of gender equality bargaining among compliers is approximately -2,224 euros. We find no comparable negative effect for fathers. Although the results for fathers do not rule out all firm-level mechanisms, they are difficult to reconcile with explanations that would generate similar earnings effects for mothers and fathers.

Our analysis of potential mechanisms provides further insight into these results. Gender equality bargaining increases the probability that mothers work part-time shortly after childbirth, but we find no robust evidence of a persistent reduction in annual paid hours. Several years after childbirth, however, a negative effect emerges on mothers' hourly wages. The effects also differ across the pre-birth earnings distribution. Among lower-earning mothers, the adjustment is concentrated primarily in reduced working hours during the first two years following childbirth. Among higher-earning mothers, we find little evidence of persistent reductions in annual paid hours, but a significant negative effect on hourly wages emerges five years after the first birth. These findings provide partial support for H3 and support H4 by showing that gender equality bargaining can affect mothers through different channels across the earnings distribution.

Taken together, our findings show that increasing compliance with gender equality bargaining requirements does not necessarily translate into improvements in women's labor market outcomes. Our exploratory analysis helps provide a possible interpretation. Gender equality agreements and action plans frequently include measures intended to facilitate work-life balance, such as facilitating part-time work and supporting employees returning from maternity or parental leave. Such measures may help mothers adjust their employment following childbirth, but they may also have unintended consequences for subsequent earnings trajectories. Our results are consistent with this possibility,

although our empirical design does not allow us to identify which specific provisions of gender equality agreements and action plans generate the estimated effects.

More broadly, our findings qualify the characterization of gender equality agreements as "empty shells" found in parts of the previous literature. The agreements and action plans in our exploratory sample contain concrete measures addressing gender balance, pay inequalities, training, and work-life balance. The key issue may therefore be not only whether firms adopt gender equality measures, but also which measures they adopt and how these measures affect women and men following childbirth. Our results suggest that policies intended to promote gender equality should be assessed not only in terms of formal compliance or the adoption of gender equality measures, but also in terms of their effects on labor market outcomes and the potential for unintended consequences.

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External appendix

Table A.1 – Measures included in gender equality agreements and action plans

Measure	All texts
Gender balance in employment	0.531
Non-gendered title of job offers	0.447
Raising awareness about non-discrimination legislation in hiring process	0.417
Tracking % of men and women in promotions/hiring/staff	0.478
Quantified target for increasing female representation in promotions/hiring/staff	0.225
Communication actions towards educational organizations and institutions	0.231

Improve working conditions to enable more women to fill certain positions	0.0580
Encourage internal promotion	0.290
Priority to internal female candidates for management positions	0.192
Reducing gender wage gaps	0.415
Tracking pay gaps by gender, age and position	0.383
Quantified target for reducing the gender pay gap	0.158
Envelope devoted to potential corrective measures	0.200
Promoting work-life balance	0.568
Meeting with HR before/after maternity/parental leave	0.441
Sending information during maternity/parental leave	0.205
Equal treatment between full-time and part-time	0.300
Promote part-time work	0.308
Promote transition from part-time to full-time on request	0.139
Reservation of slots in inter-firm nurseries	0.0980
Child care benefits	0.0880
Wage increase upon return to work beyond the legal framework	0.161
Promotion of paternity leave	0.193
Paternity leave considered as actual working time	0.203
Meeting time in a limited time slot	0.208
Reduction in daily working hours from the 4th month of pregnancy for identical pay	0.137
Possibilities of work arrangements during pregnancy on request	0.136
Adjustment of timetables for the start of the school year	0.153
Sick child leave	0.198
Improving access to training	0.434
Systematic proposal/information to women regarding training	0.276
Priority access to training for employees returning to work following parental leave	0.249
Monitoring training rates by gender	0.375
Quantified target for reducing training gap	0.0760
Refund of part of the childcare costs for attending training involving significant travel	0.132
Priority to training in the workplace or via e-learning	0.175
Number of observations	590

Note: The table reports the share of gender equality agreements and action plans in the exploratory sample that include each measure. Measures appearing in fewer than 5% of the documents are excluded. The sample consists of 590 agreements and action plans adopted between 2006 and 2014.

Table A.2 — Mothers' total wage income by firm size and birth cohort

Outcomes	Pre-reform cohort			Post-reform cohort			Diff.-in-diff.
	Small firms	Large firms	Diff.	Small firms	Large firms	Diff.	
Total wage income (in euros)							
One year before the first child birth	18,894.55	22,028.76	3,134.21	19,360.66	22,155.96	2,795.3	-338.91
One year after the first child birth	17,145.89	19,703.87	2,557.98	18,206.91	20,546.27	2,339.36	-218.62
Five years after the first child birth	19,217.68	22,541.76	3,324.08	21,255.33	23,726.9	2,471.57	-852.51
Motherhood effect							
One year after the first child birth	-1,748.66	-2,324.89	-576.23	-1,153.75	-1,609.69	-455.94	120.29
Five years after the first child birth	323.13	513	189.87	1,894.67	1,570.94	-323.73	-513.6

Source: DADS-EDP panel, D@ccord database.

Note: The table reports mothers' average total wage income by firm size and birth cohort, together with changes relative to one year before the first birth. Total wage income is measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, paid overtime, benefits in kind, bonuses, and indemnities, but excludes maternity leave allowances. The pre-reform cohort consists of mothers whose first child was born between in 2006-2007, while the post-reform cohort consists of mothers whose first child was born in 2013-2014. Small firms have fewer than 50 employees, and large firms have 50 or more employees.

Table A.3 – Effects of the 2010 reform and gender equality bargaining on mothers' total wage income

	(1)	(2)	(3)
	ITT	ITT	LATE
Event-time	without covariates	with covariates	with covariates
-2	-321.821* (186.183)	-348.737* (187.948)	-803.836* (435.081)
-1	(ref.)	(ref.)	(ref.)

0	-98.929 (229.764)	-317.620 (273.105)	-732.113 (623.464)
1	120.299 (248.864)	-33.762 (253.753)	-77.820 (587.307)
2	-418.761* (250.695)	-740.839*** (237.691)	-1707.629*** (541.885)
3	-502.548* (284.015)	-714.915** (294.186)	-1647.874** (683.195)
4	-477.992 (301.925)	-705.179** (307.548)	-1625.434** (712.877)
5	-513.605 (324.423)	-964.981** (410.628)	-2224.276** (959.274)
Number of observations	11,677	11,677	11,677

Source: DADS-EDP panel; D@ccord database.

Note: The dependent variable is total wage income, measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, any paid overtime, benefits in kind, bonuses, and indemnities, but excludes maternity leave allowances. Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. Column (1) reports the Intention-To-Treat (ITT) estimates without covariates (numerator of Equation 3). Column (2) reports ITT estimates with covariates (numerator of Equation 4). Column (3) reports LATE estimates with covariates (Equation 4). Covariates include mothers' age, labor market experience, and occupational level measured one year before the first birth. Standard errors, reported in parentheses, are based on 250 bootstrap replications, with resampling performed at the firm level. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table A.4 – Effects of the 2010 reform and gender equality bargaining on the components of mothers' total wage income

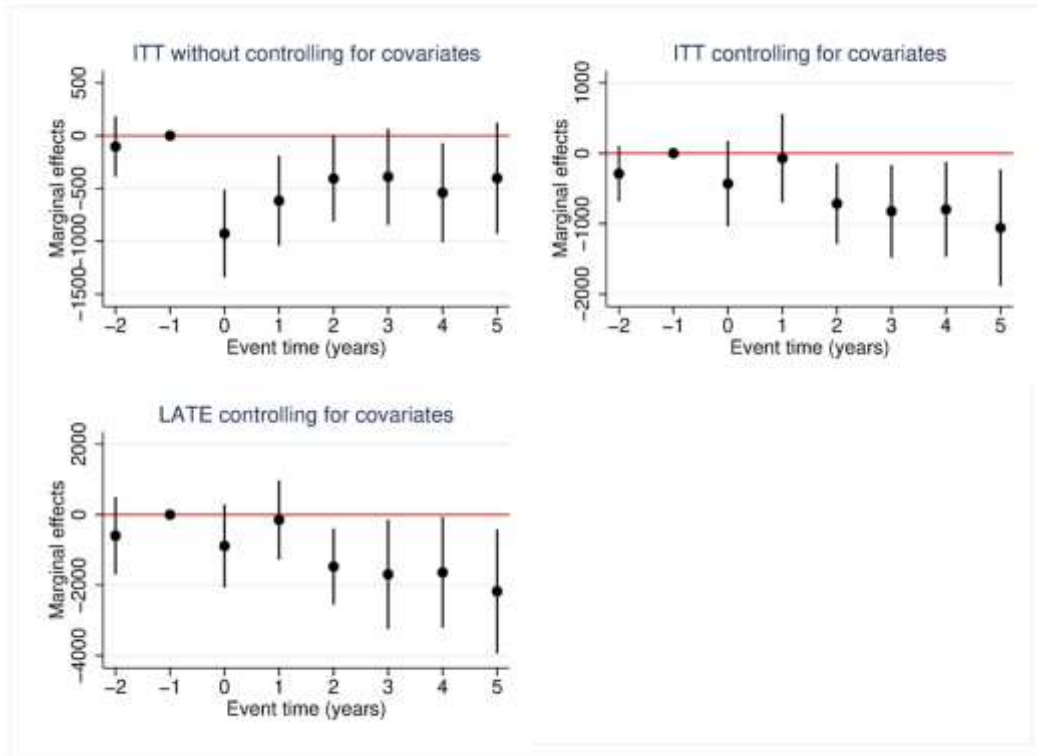
Event-time	ITT	ITT	LATE
	without covariates	with covariates	with covariates
Panel A: Annual paid hours			
-2	-12.617 (14.529)	-15.635 (14.274)	-35.993 (32.838)
-1	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
0	-44.482** (19.842)	-42.060* (22.720)	-96.355* (51.119)
1	-3.140 (18.318)	-12.246 (20.526)	-28.178 (48.218)
2	-29.310* (18.318)	-34.553* (18.318)	-79.833* (48.218)

	(17.397)	(19.475)	(44.657)
3	-8.054	-8.257	-19.082
	(18.755)	(18.927)	(44.006)
4	-10.321	-16.775	-38.879
	(19.196)	(23.033)	(53.899)
5	0.826	-3.123	-7.252
	(19.465)	(21.265)	(52.938)
Panel B: Probability of working at least 20% fewer annual hours			
-2	0.000	0.004	0.010
	(0.016)	(0.018)	(0.041)
-1	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
0	0.072***	0.069***	0.159***
	(0.022)	(0.024)	(0.055)
1	0.014	0.019	0.045
	(0.020)	(0.022)	(0.052)
2	0.037*	0.039*	0.091*
	(0.019)	(0.021)	(0.048)
3	0.010	0.008	0.018
	(0.020)	(0.020)	(0.047)
4	0.007	0.017	0.040
	(0.021)	(0.025)	(0.057)
5	0.012	0.023	0.052
	(0.021)	(0.024)	(0.059)
Panel C: Participation			
-2	-0.005	-0.004	-0.010
	(0.004)	(0.003)	(0.007)
-1	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
0	-0.000	0.007	0.016
	(0.011)	(0.012)	(0.029)
1	-0.002	-0.000	-0.000
	(0.006)	(0.006)	(0.014)
2	0.005	0.001	0.001
	(0.005)	(0.005)	(0.012)
3	-0.012**	-0.012**	-0.028**
	(0.005)	(0.006)	(0.013)
4	-0.006	-0.004	-0.009
	(0.005)	(0.005)	(0.012)
5	-0.001	-0.002	-0.005
	(0.005)	(0.006)	(0.013)
Panel D: Hourly wage			
-2	-0.086	-0.087	-0.201
	(0.079)	(0.092)	(0.212)
-1	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
0	0.221*	0.015	0.033
	(0.131)	(0.171)	(0.389)
1	0.062	0.085	0.196
	(0.098)	(0.125)	(0.289)
2	-0.107	-0.199	-0.459
	(0.106)	(0.134)	(0.311)
3	-0.168	-0.259	-0.598
	(0.123)	(0.163)	(0.374)
4	-0.073	-0.197	-0.457
	(0.152)	(0.144)	(0.342)
5	-0.385**	-0.601***	-1.396***
	(0.150)	(0.189)	(0.446)
Number of observations	11677	11677	11677

Source: DADS-EDP panel, D@ccord database.

Note: Event time is measured from the first child's birth; the reference year is one year before the first child's birth. Column 2 presents the intention-to-treat (ITT) effect of a simple Wald-DiD estimator without covariates (numerator of Equation 3). Column 3 presents the ITT with covariates (numerator of Equation 4). Column 4 presents the local average treatment effect (LATE) with covariates (estimates from Equation 4). In Columns 3 and 4, we control for mothers' age, professional experience, and occupational level one year before their first child's birth. Standard errors (in brackets) are obtained via a bootstrap procedure with 250 replications. To account for potential within-firm dependence across observations, we use a block-bootstrap at the firm-level. Significance levels: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Figure A.1 — Effects of the 2010 reform and gender equality bargaining on mothers' total wage income: restricted sample



Source: DADS-EDP panel, D@ccord database.

Note: The dependent variable is total wage income, measured as annual net earnings reported by employers to the tax authorities. It includes wages and salaries, paid overtime, benefits in kind, bonuses, and indemnities, but excludes maternity leave allowances. Event time is measured relative to the birth of the first child, with one year before the birth as the reference year. The restricted sample excludes treated mothers employed in firms with 50-299 employees. Estimates are based on 9,122 mothers, of whom 1,987 are treated. The top-left panel reports the Intention-To-Treat (ITT) estimates without covariates (numerator of Equation 3). The top-right panel reports ITT estimates with covariates (numerator of Equation 4). The bottom-left panel reports the Local Average Treatment Effect (LATE) estimates with covariates (Equation 4). Covariates include mothers' age, labor market experience, and occupational level measured one year before the first birth. The vertical bars indicate 95% confidence intervals based on 250 bootstrap replications, with resampling performed at the firm level.