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Does gender equality bargaining reduce child penalty? Evidence from France

Pierre-Jean Messe* Jeremy Tanguy^{†‡}

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Abstract

This paper investigates the effects of firm-level gender equality bargaining on the motherhood penalty using French administrative data. To tackle the endogeneity issue, we exploit the 2010 reform that introduced financial penalties for firms with 50 employees or more not complying with their obligation of negotiating on gender equality. This change led to a strong acceleration of gender equality bargaining after 2010 but only for firms with 50 employees or more. As a consequence, women who had their first child in concerned firms after 2010 are more likely to be employed in firms covered by a text related to gender equality. Controlling for firms' size effect and time trends as well as a set of other individuals' and firms' characteristics, we identify the causal effect of gender equality bargaining on earnings impact of motherhood. Our estimates show that forcing firms to promote measures related to gender equality has reinforced the motherhood penalty. While the causal effect of this reform is close to zero just after the first child birth, it turns out to be significantly negative 5 years after. Our results suggest that some measures mentioned in GE texts, especially those favouring work-life balance, may act as an indirect discrimination towards mothers.

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

JEL Classification: J13, J16, J22, J31, J52, J71

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1 Introduction

The gender gap in earnings has narrowed over recent decades thanks to a broad convergence in education, labor market participation, occupations and working hours between women and men (Blau and Kahn 2017; Goldin 2014)¹. Though, there remains a gender gap in earnings, that mostly comes from fewer work hours and more career discontinuities among women (Bertrand et al. 2010). In France in 2018, for every day with at least one hour of work, women earn on average 24% less than men (Breda et al. 2021). This difference can be explained by fewer hours of work for women when they work and a lower remuneration per hour of work. Once working time differences are controlled for, the gender wage gap is reduced by about 10pp. For the same volume of work, French women earn 16.8% less than their male colleagues (Georges-Kot 2020). At the same age, job and firm, there remains a gender wage gap of around 5% in France (Breda et al. 2021). A central explanation to this remaining gender wage gap is parenthood, *i.e.* the effect of children on the careers of women relative to men (Kleven et al. 2019b; Meurs and Pora 2019), alongside more marginal factors such as discrimination (Altonji and Blank 1999; Blau and Kahn 2017) or gender differences in wage bargaining (*e.g.* Exley et al. 2020). The arrival of a child leads to a lasting loss of wage income for the mother but not for the father, contributing to increasing the wage gap between women and men.

In this paper, we aim at evaluating the extent to which decentralized collective bargaining on gender equality (GE hereafter) contributes to reduce this motherhood penalty in France. As in several other countries, collective bargaining is in France an important part of the “regulatory toolkit” to fight against gender inequalities in wages and working conditions (Milner et al. 2019). In France, firms are required to commit to the reduction of gender inequalities by negotiating collective agreements covering several areas of action, including work-life balance.² While this obligation has existed since 2001, non-compliance is sanctioned only since January 1st, 2012, in application of a law passed in 2010. Not all firms had complied with the law before the introduction of sanctions but also, to a lesser extent, after (Milner et al. 2019). Therefore, only a fraction of workers are covered today by a collective agreement on GE. We analyze the trajectory of total wage income and its components (paid hours, participation, hourly wage) of young mothers depending on whether they worked in an establishment covered by a GE text (collective agreement or action plan) one year before having their first child.

Our empirical approach is based on the combination of individual- and firm-level administrative data from employers. We use data from a demographic panel survey matched with employer earnings declarations in combination with data provided by the French Ministry of Labor on collective

1. See also Meurs and Pora (2019) for France.

2. The agreement must set progress targets and actions to achieve them covering at least three of the following areas of action in firms with less than 300 employees, four in firms with 300 employees and more: hiring, training, career advancement, qualifications, classification, working conditions, safety and health at work, effective remuneration, and work-life balance.

bargaining texts (collective agreements and unilateral employer action plans). These different data allow us to follow a sample of young mothers (parents) over time, from three years before to five years after the birth of their first child. About 20% of these mothers worked in a firm covered by a text on GE the year before the birth of their first child. We estimate the effect of the first child birth on four labor market outcomes (total wage income, paid hours, participation, hourly wage) separately for these “treated” mothers and for the other “untreated” mothers of our sample, working in firms not covered by a text on GE.

A simple comparison of the effects of the first child birth between treated mothers and untreated mothers in an event-study approach *à la* Kleven et al. (2019b) does not give the causal effect of collective GE bargaining on motherhood penalty. Indeed, specific characteristics of these mothers’ firms may affect at the same time the treatment and the outcomes. We propose two strategies to take into account differences in characteristics between treated and non-treated firms. We first use a reweighting procedure based on the propensity score to make the treated and non-treated groups comparable in terms of individuals’ and firms’ characteristics. This strategy allows to address the selection bias on observables but there remains a bias if some omitted variables influence GE bargaining outcomes and earnings dynamics. To tackle this endogeneity issue, we then use an instrumental variable (IV) strategy, that exploits the exogenous change in the treatment status of firms with 50 employees and more following the 2010 law that introduced financial penalties for non-complying firms of this size.

A pure event-study analysis suggests that collective bargaining on GE makes it possible to reduce the motherhood penalty, as the drop in total wage income following the first child birth is lower for mothers working in treated firms than for those working in untreated firms. When controlling for differences in observable characteristics between treated and untreated mothers with the reweighting procedure, we fail to find any positive and persistent effect of GE bargaining on the four outcomes considered. This suggests that the motherhood penalty differences observed between treated and untreated mothers are due to differences in their employers’ characteristics (*e.g.* firm size, industry, socio-professional and gender composition of the workforce) other than collective bargaining on GE .

When we correct for the omitted variable bias with our IV strategy, we find that the motherhood penalty is rather reinforced in the treated firms. The fall in total wage income following the first child birth is greater for mothers employed in firms covered by a GE text than for those employed in non-covered firms. This main result holds when restricting only the sample to mothers who have had only one child over the period of analysis. It is also robust even if we reduce the range of firms’ size or if we adopt an alternative definition of the treatment, focusing on texts that are only related to GE. Though surprising at first glance, this strengthening of the motherhood penalty associated to collective bargaining on GE is nevertheless consistent with Lucifora et al. (2021), who conclude

that not gender-neutral firm-level policies on child-related leaves can exacerbate the motherhood penalty by leading mothers down a slower, less steep career path – a “mommy track”.

We contribute to three streams of the literature.

This paper is first related to the literature on the labor market outcomes of fertility decisions. Shifts in women’s labor supply and labor market outcomes explain a large share of the gender pay gap (Waldfogel 1995, 1997, 1998), although there is no consensus about the exact size of this share or its evolution over time in developing countries (Adda et al. 2017; Bertrand et al. 2010; Juhn and McCue 2017; Kleven et al. 2019b; Wilner 2016).³ There is a recent literature quantifying the employment and earnings effect of parenthood. While focusing on specific groups of highly-educated individuals, earlier papers find large earnings penalties for women, not for men, following the first child birth (Azmat and Ferrer 2017; Bertrand et al. 2010). More recent papers consider the effect of parenthood on the whole population in different countries and over different time periods using an event-study approach, and reveal also a motherhood penalty in earnings (Angelov et al. 2016; Fitzenberger et al. 2013; Goldin and Mitchell 2017; Kleven et al. 2019a,b). Labor market trajectories of men and women diverge sharply with the arrival of children, while similar in the years preceding parenthood. Comparing child penalties across six countries, Kleven et al. (2019a) 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 and Pora (2019) find a child penalty in earnings among women of around 30% over the five years following the first child birth.⁴ We get a similar average child penalty in women’s earnings on more recent and slightly more selected data from this panel in France. We further show that this child penalty varies significantly depending on the outcomes of GE collective bargaining in the firm as well as other characteristics of the firm such as size, industry or workforce composition.

This paper is also related to a recent literature that investigates the impact of policies, implemented by governments or firms, to reduce the motherhood penalty. Public policies include incentives for fathers to take up some of the parental leave through the introduction of “daddy quotas”. While most fathers tend to take up their quota in Scandinavian countries and Quebec (Dahl et al. 2014; Ekberg et al. 2013; Patnaik 2019), consequences for mothers’ labor market outcomes are not always positive⁵ Another common public policy is subsidized or publicly provided child care

3. Kleven et al. (2019b) estimate that about 80% of the total gender wage gap in Denmark in recent years is attributable to child penalties. Wilner (2016) finds a large wage loss associated to motherhood in France and a much smaller loss associated to fatherhood.

4. Other studies in France apply the same approach to more specific data. Using data from a large French firm, Lucifora et al. (2021) find a child penalty in earnings of about 9%, that increases by including bonuses. Using data from the French generation survey on the early part of careers, Bazan et al. (2021) find a child penalty for mothers in monthly earnings of 23% overall, 35% for those with secondary education only.

5. While some studies find that such policies improve labor market outcomes (see Andersen 2018 for Denmark; Johansson 2010 for Sweden; Patnaik 2019 for Quebec), others find no effect (see Ekberg et al. 2013 for Sweden; Dunatchik and Özcan 2021 for Quebec) or negative effects (see Cools et al. 2015 for Norway).

(*e.g.* in United States, Canada, Norway, France). As Olivetti and Petrongolo (2016) point out, this type of policy has essentially positive effects on mothers’ labor market outcomes in countries where the cost of child care is high (*e.g.* United States, Canada).

Given the limited role played by public policies – such as parental leave and child care subsidies – in improving mothers’ labor market outcomes (Kleven et al. 2020), very recent works are focusing on the role played by individual firms in the provision of extended paid parental leave (PPL) and paid family leave (PFL). Both extended PPL and PFL are usual employee-specific family benefits provided by employers in addition to family benefits guaranteed by a country’s legislation (Lucifora et al. 2021). Some of these benefits are gender-neutral but, in most cases, more PPL and PFL are offered to mothers than to fathers (Goldin et al. 2020). Thomas (2020) shows that, in a world where statistical discrimination exists, extended maternal leave policies exacerbate and widen the motherhood gap in promotions. Similarly, Lucifora et al. (2021) suggest that the non-gender-neutrality of some of their company’s internal policies, such as guaranteeing extra-weeks of leave or paid child-related leave to mothers only, despite non directly affecting their earnings, influence their managers’ assessment in terms of chances of career advancement. These results are in line with our findings. Some measures regarding work-life balance mentioned in GE texts may discriminate indirectly women against men and contribute to this “mommy track”.

This paper also contributes to the literature on gender equality bargaining. Starting from seminal work of Dickens (1998), a bulk of papers aim at characterizing the main factors that shape GE bargaining such as the economic context, the bargaining relationship between parties, the characteristics of the negotiators or the legislative framework (Berg and Piszczek 2014; Gerstel and Clawson 2001; Gregory and Milner 2009; Hoque and Noon 2004; Milner and Gregory 2014; Rigby and O’Brien-Smith 2010). Other studies analysed the process of GE bargaining through analysis of GE texts. These studies usually rely on small and non-random samples of agreements or on a reasoned selection of sectors. The seminal work of Laufer and Silvera (2006) for instance deals with 40 agreements signed in France between 2001 and 2004 at a time when only a small number of firms complied with their legal obligations, making them necessarily specific. After reading this small sample of texts, they conclude that most of the time they look like “empty shells”: a simple exercise in image management but nothing of substance. Subsequent analyzes of GE texts converge towards this same conclusion for the French case (Grésy 2009; Pochic et al. 2019) or for the UK case (Hoque and Noon 2004). However, as far as we know, no quantitative study has assessed the effectiveness of gender equality bargaining outcomes to tackle gender inequalities. Our paper fills this gap exploring the effect of gender equality bargaining on the motherhood penalty.

The remainder of the paper is organized as follows. Section 2 provides some institutional background on GE collective bargaining in France. Section 3 describes the different data used in the

paper and provides some descriptive evidence on differences in parents' labor market outcomes depending on the state of GE collective bargaining in the firms where they are employed. We explain the econometric approach in Section 4, we present the main results in Section 5 and we conduct several robustness checks in Section 6. Section 7 concludes. Additional results are relegated to an extended appendix.

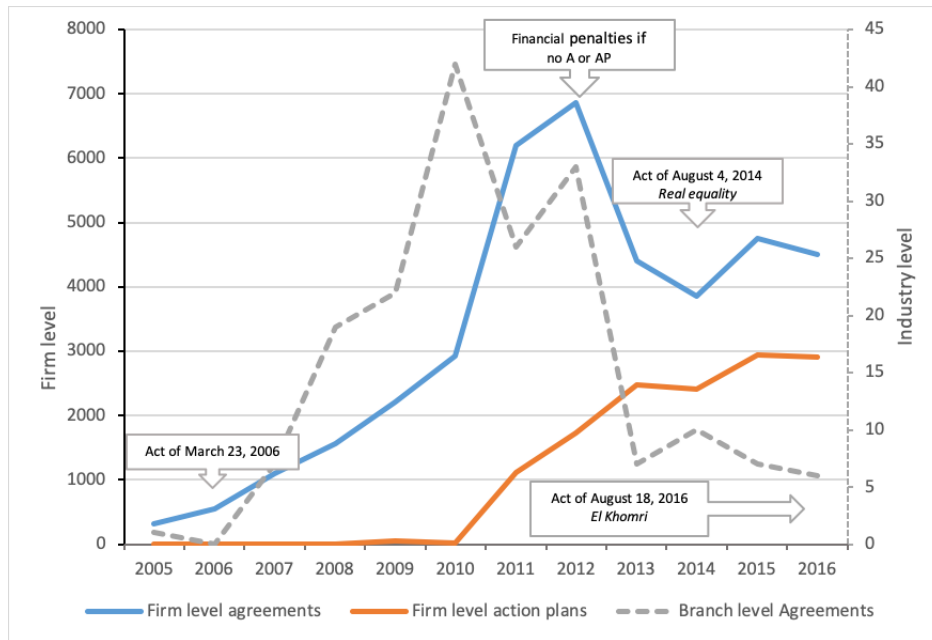
2 Background

In France, it is through the negotiation of firm-level agreements that the legislator seeks to engage firms to act for equal employment and equal pay. Collective bargaining on GE in firms has gradually developed since the implementation of the *Roudy* Act in 1983, which introduced the obligation for employers to submit a written report – called *Rapport de situation comparée* – on the comparative situation of women and men to the works council. This act also gave, for the first time, the opportunity for employers to bargain with union delegates measures to reduce gender inequalities within the firm. In 2001, the *Génisson* Act turned this opportunity to bargain into an obligation, specifically on the firm's objectives in the area on GE at work and on the actions to achieve them. In addition to this mandatory negotiation on GE, the Act of March 23, 2006 initiated the definition and implementation of measures to remove gender wage inequality in the mandatory annual negotiations on the wage policy.

The period from 2010 has been marked by an acceleration of firm-level collective bargaining on GE (see Figure 1). This has been strongly promoted by the public authorities through a set of laws imposing sanctions but also setting the various procedures and parameters of the negotiations. It then became common practice to refer to *bargained public policy* (Groux 2005) or *state managed bargaining* (Mias et al. 2016). The Act of November 9, 2010 introduced for the first time financial penalties (up to 1% of payroll) for firms with 50 employees and more not complying with the obligation to be covered by a collective agreement or, failing that, by a unilateral employer decision (action plan) on GE. Negotiations on GE have then to be renewed after one year in case of an action plan, three years in case of a collective agreement. These sanctions were implemented from January 1st, 2012 and have since been reinforced by the prohibition to bid on public contracts. The Decree of December 18, 2012 made bargaining on the effective compensation area compulsory. Finally, the *Real equality* Act (2014) streamlined obligations into a unique global negotiation on gender professional and pay equality, the *Rebsamen* Act (2015) has consolidated and streamlined the obligations of negotiation, merging equality bargaining with quality of working life and the *El Khomri* Act (2016) has taken further disposition to develop collective bargaining and modernize social dialogue.

Milner et al. (2019) argue that in France, the “development of increasingly strong employer duties and complex compliance requirements has [...] created a distinctive model of ‘bargained equality’” (p. 277-278). Another feature of the French negotiated equality model is that it is based

Figure 1 – Number of texts on GE bargained at the firm and industry level



on a principle of complementarity, enshrined in law, between industry- and firm-level collective bargaining. Both levels have an obligation to negotiate and bargaining at the firm level is supposed to add to agreements set at the industry level (on pay scales, classifications, joint initiatives on key issues of the industry) provisions tailored to address gender equality issues which are specific to the workplace.

3 Data and descriptive statistics

Data description The analysis is based on administrative data that combine several administrative registers containing rich data over the period 2005-2018. First, we use the longitudinal version of the *Déclarations Annuelles de Données Sociales* (DADS, Annual Declarations of Social Data)⁶, a panel subsample of French salaried employees extracted from exhaustive DADS database for research purposes. Since 2002, the DADS panel subsample gathers information on individuals born in October and therefore is a representative sample of the French salaried population at the 1/12. The data set contains rich information on gross and net wages, paid hours, individual characteristics (age, gender, nationality, seniority) and job characteristics (type of contract, full- or part-time job, occupation).

We estimate child penalty on annual net earnings, that correspond to the wage information reported by firms to the fiscal services for income tax purposes. This measure includes all wages

6. French firms have to report annually the wage information of each employee for payroll and fiscal purposes.

and salaries, any paid overtime, benefits in kind, all bonuses and indemnities (including shift work), including those paid once a year and those paid after contract termination if they exceed the industry-negotiated levels. However, it does not include maternity-leave allowances. We also decompose the effect of first childbirths on net earnings into three components: the number of paid hours, the labour force participation and the hourly wage, defined as the ratio of net earnings and paid hours. We then identify the change in labor supply at the intensive and extensive margins. Note that the number of paid hours can be overestimated for workers who are not paid by the hours during their maternity leave (Meurs and Pora 2019).

We also use the position files (*fichiers postes*) of the DADS data to get further information on the structure of the workforce and the wages at the establishment-level. For employed individuals, we control for several characteristics of the employer establishment such as the size, the sector, the wage distribution, the average working hours, the occupational structure, the number of occupational titles, the share of part-time workers, the share of female workers, the share of foreign-born workers or the growth rate of employment.

In addition to this information on wages and job characteristics, we use the individual information on some of these workers which is provided in the *Echantillon Démographique Permanent* (EDP, Permanent Demographic Sample), a large-scale socio-demographic panel gathering information on all births, marriages and deaths⁷ The sample corresponds to a 4% survey of the population living in France. As for the DADS panel, selection into the EDP is based on the birth date. The linkage with the DADS panel concerns individuals born on October 1-4, representing 13% of the DADS panel.

To have information on gender equality bargaining at the firm-level, we exploit data provided by the French Ministry of Labor over the period 2005-2018. Information on all texts related to collective bargaining is standardized by the Ministry of Labour to build a longitudinal firm-level data set: the D@ccord database. The data provide rich information on the type of text (agreements, disagreements, amendments or unilateral employer decisions), the signatory unit (establishment, firm, group, branch), the topics of the negotiation (wages, bonuses, employment, hours, labour conditions, gender equality), the signatories and the unions present at the negotiation.

Sample selection We consider for each individual information provided by her main employer⁸. We keep only individuals aged 20-59, living in mainland France and working in the private sector at least until the first child birth. In addition, we exclude home workers, farmers, company heads, trainees and apprentices (and clergy). We consider a balanced panel in which we observe parents every year between three years before having their first child and five years after. We restrict our

7. This information is collected since 1968 from the registry office, along with census information from 1968, 1975, 1982, 1990 and 1999.

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

sample to individuals employed one year before their first child birth. We end up with a balanced sample of 39,676 parents, 14,331 mothers and 25,345 fathers, whose first child is born between 2008 and 2013.

Definition of the treatment We consider that an individual is treated if she worked in an establishment covered by a text (collective agreement or action plan) related to GE one year before having her first child. Note that a text may have several motives, including GE. In our baseline definition of the treatment, we consider all texts including those that do not exclusively deal with GE issues. Among the 14,331 mothers in our sample observed every year, 2,848 ($\approx 19.9\%$) are treated. Our main analysis is based on this definition of treatment that does not account for the quality of texts. We conduct in section 6 several robustness checks using alternative definitions of the treatment.

Descriptive statistics We estimate the impact of children on mothers' and fathers' labor market outcomes using an event study around the first child birth. We use the event study specification proposed by Kleven et al. (2019b). For each parent in the data, event time t is indexed relative to the year of the first child birth.

We run the following regression separately for treated and non-treated women and men:

$$Y_{ist}^g = \sum_{j \neq -1} \alpha_j^g \cdot \mathbf{I}[j = t] + \sum_k \beta_k^g \cdot \mathbf{I}[k = \text{age}_{is}] + \sum_y \gamma_y^g \cdot \mathbf{I}[y = s] + \nu_{ist}^g \quad (1)$$

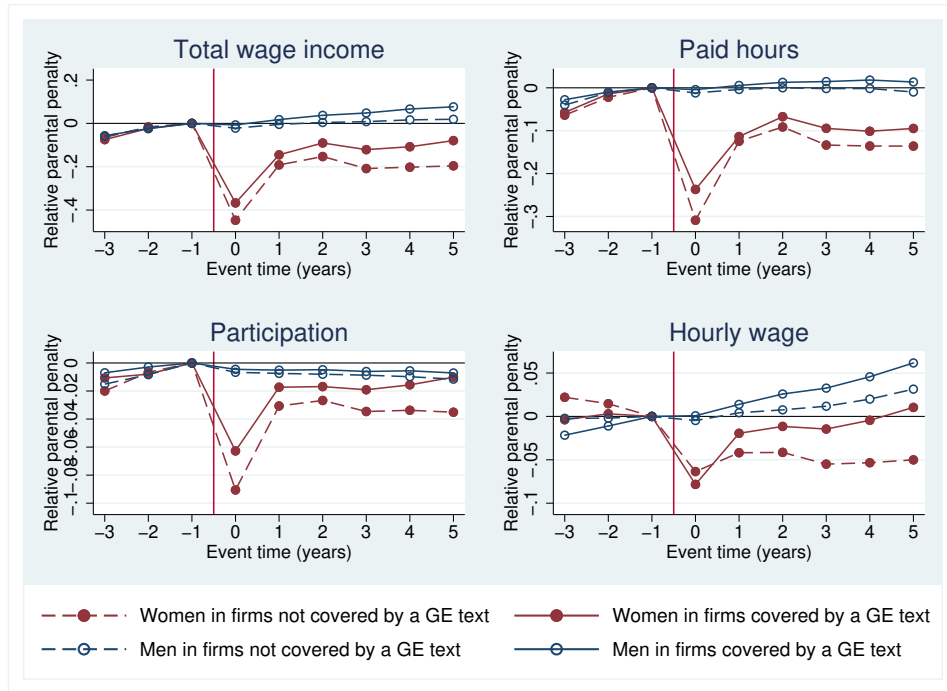
where Y_{ist}^g the outcome for individual i of gender g in year s and at event time t . On the right-hand side, the first term includes event time dummies, the second term includes age dummies, and the third term includes year dummies. Age dummies allow us to control for life-cycle trends, while year dummies control for time trends. We omit the event time dummy at $t = -1$, so that the event time coefficients capture the impact of children relative to the year just before the first child birth. We are able to identify the effects of all three sets of dummies because, conditional on age and year, there is variation in event time driven by variation in the age at which individuals have their first child (Kleven et al. 2019a)

Figure 2 shows the impact of parenthood on annual net earnings of women and men according to whether they were employed in an establishment covered by a GE text one year before their first child birth. For each gender, each dot of the continuous and the dotted lines gives the percentage impact on the outcome at event time t respectively for treated and not treated individuals. Regardless of the treatment, the arrival of a first child generates a drop in annual net earnings of women by around 40% at event time 0 and a subsequent decline in their earnings by 30%. In parallel, annual net earnings of

men are not affected by the first child birth. These results are the same as in Meurs and Pora (2019).

Consider then the role of gender equality bargaining. The earnings of individuals employed in an establishment covered by a GE text at event time -1 evolve in parallel with those employed in an establishment not concerned by any GE text before the first child birth. After having children, mothers earnings profile diverge sharply but the negative impact is somewhat smaller for the treated. This is also true for each component of the annual earnings. This suggests that the child penalty would be lower in establishments covered by a GE text one year before the first child birth. This attenuation effect would pass through a lower drop in the number of paid hours (intensive margin), a lower use of parental leave (extensive margin) and a higher hourly wage rate.

Figure 2 – Impact of the first child birth on parents’ total wage income and its components by GE text coverage (one year before childbirth)



Sources: DADS-EDP panel, D@ccord database. Notes: The figure displays the estimated changes in parents’ total wage income and its components (paid hours, participation, hourly wage) around the first child birth using the event-study specification of Kleven et al. (2019b). The vertical red lines indicate the timing of the first child birth.

But these differences in motherhood penalties between treated and non-treated women do not necessarily result from gender equality bargaining. Indeed, the characteristics of these two groups are strongly different, especially regarding their workplaces’ attributes. Table 1 provides descriptive statistics in the samples of treated and non-treated mothers. This clearly shows that treated women are employed in workplaces strongly different than non-treated ones. For example, estab-

lishments covered by a GE text are on average larger (990 employees) than non-covered ones (234 employees) and they offer higher mean annual wages (24,375 euros in covered establishments and 19,395 euros in non-covered ones). They are characterized by a higher proportion of managers and professionals (21.8% versus 18.4%), a lower proportion of part-time workers (17.5% versus 21.8%) and a lower proportion of female workers (53% versus 63.3%). The incidence of gender equality bargaining strongly differs across industries. For example, the banking/finance or the transport sectors are over-represented among treated individuals. These facts are in line with the literature on the determinants of gender equality bargaining (Bruno et al. 2021; Dickens 1998; Heery 2006; Milner and Gregory 2014).

In addition, our data set does not contain any information about the quality of the social dialogue in the workplace or the bargaining relationship between parties that may drive the incidence of gender equality bargaining and its impact on females' earnings. So, to measure the causal effect of gender equality bargaining on child penalty we have to deal with this selection bias on observables and on unobservables.

4 Empirical strategy

4.1 An Inverse Probability Weighting method

Our goal is to estimate the causal effect of gender equality bargaining on earnings impact of parenthood, t years after the first child birth. Since we have seen that fathers' careers are not affected by the treatment, we will focus on the sample of mothers. We take event time -1 as a reference year. For each outcome, the dependent variable is its absolute change between -1 and t . This difference for mother i is denoted by $\Delta Y_{it} = Y_{it} - Y_{i-1}$.

Let D_i be the treatment dummy equal to one if the woman is employed in an establishment covered by a GE text at event time -1 . Let ΔY_{it}^1 the motherhood effect t years after first child birth for woman i when treated and ΔY_{it}^0 its counterpart in absence of treatment. The parameter of interest is the Average Effect of Treatment on the Treated $ATT_t = E(\Delta Y_{it}^1 - \Delta Y_{it}^0)$.

If selection into treatment does not depend on observed or unobserved variables, the comparison of earnings changes after motherhood between treated and control women will provide a measure of the gender equality bargaining effect. For each event time t , the ATT would write as:

$$ATT_t = E(\Delta Y_{it}|D_i = 1) - E(\Delta Y_{it}|D_i = 0)$$

This effect is obtained from a least squares regression of the change in outcomes between event time -1 and t on the treatment dummy D_i . However, in the previous section, we have highlighted strong

Table 1 – Descriptive statistics

Characteristics	Untreated	Treated	Observations
Individual characteristics			
Foreign born	0.0500	0.0740	14,331
Age	29.33 (4.336)	29.90 (4.292)	14,331 .
Vocational basic or less	0.169	0.149	11881
Vocational advanced	0.269	0.215	11881
Undergraduate	0.342	0.332	11881
Graduate	0.220	0.304	11881
Experience	7.551 (3.809)	7.927 (3.889)	14,331 .
Seniority in the firm	2.966 (2.271)	3.572 (2.492)	14,331 .
Permanent worker	0.942	0.949	14,331
Part-time	0.113	0.102	14,331
Work hours, % of full-time	0.794 (0.147)	0.825 (0.129)	14,331 .
Annual labor income	20535 (8407.078)	24090 (9472.496)	14,331 .
Manager or professional	0.147	0.237	14,331
Intermediate occupation	0.280	0.300	14,331
Clerical, sales or service worker	0.514	0.372	14,331
Blue-collar worker	0.0580	0.0910	14,331
Workplaces' attributes			
Mean wages	19395 (9280.445)	24375 (11062.534)	14,331 .
Mean work hours, % of full time	0.794 (0.147)	0.825 (0.129)	14,331 .
Share of managers and professionals	0.184	0.218	14,331
Share of intermediate occupations	0.272	0.245	14,331
Share of clerical, sales or service workers	0.304	0.300	14,331
Share of blue-collar workers	0.141	0.157	14,331
Number of occupational titles	3.282 (1.219)	3.783 (1.027)	14,331 .
Wage dispersion p90/p50	1.954 (2.443)	1.796 (1.145)	14,331 .
Wage dispersion p90/p10	13.10 (80.009)	11.32 (90.616)	14,331 .
Share part-time workers	0.218	0.175	14,331
Share female workers	0.633	0.530	14,331
Share workers foreign born	0.0740	0.0670	14,331
Number of employees (workplace)	233.9 (1128.024)	990.0 (2446.339)	14,331 .
Number of employees (firm)	1812 (7327.667)	17913 (38409.609)	14329 .
Single establishment	0.537	0.146	14,331
Growth rate	0.144 (3.900)	0.0810 (2.264)	14,331 .
Manufacturing industry	0.120	0.194	14,331
Construction	0.0230	0.0160	14,331
Motor vehicle retail sales	0.244	0.159	14,331
Transport	0.0360	0.107	14,331
Hotels and restaurant	0.0390	0.0180	14,331
Information,communication	0.0410	0.0470	14,331
Banking, finance, insurance, real estate	0.0950	0.217	14,331
Business services	0.185	0.142	14,331
Administration, education	0.0280	0.0110	14,331
Health, social action	0.129	0.0810	14,331
Others	0.0590	0.008	14,331
Number of mothers	11,483	2,848	14,331

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

differences in the composition of the sample between treated and non-treated mothers. This may affect the dynamics of outcome after the first child birth. To account for such differences, we apply a weighting scheme that depends on the values of the propensity score $P(D_i = 1|X_i)$ where X_i includes a set of characteristics of each woman i and of her employer one year before the first child birth and year dummies to control for time trends. The motherhood effect ΔY_{it} is reweighted for non-treated mothers, with low weights being given to low values of propensity scores. This Inverse Probability Weighting method accounts for imbalances in the distribution of X_i between the treated and the control group of mothers. The ATT is measured as the following weighted average:

$$ATT_t = E(\Delta Y_{ist}|D_i = 1) - \frac{P(D_i = 1|X_i)}{1 - P(D_i = 1|X_i)} E(\Delta Y_{ist}|D_i = 0)$$

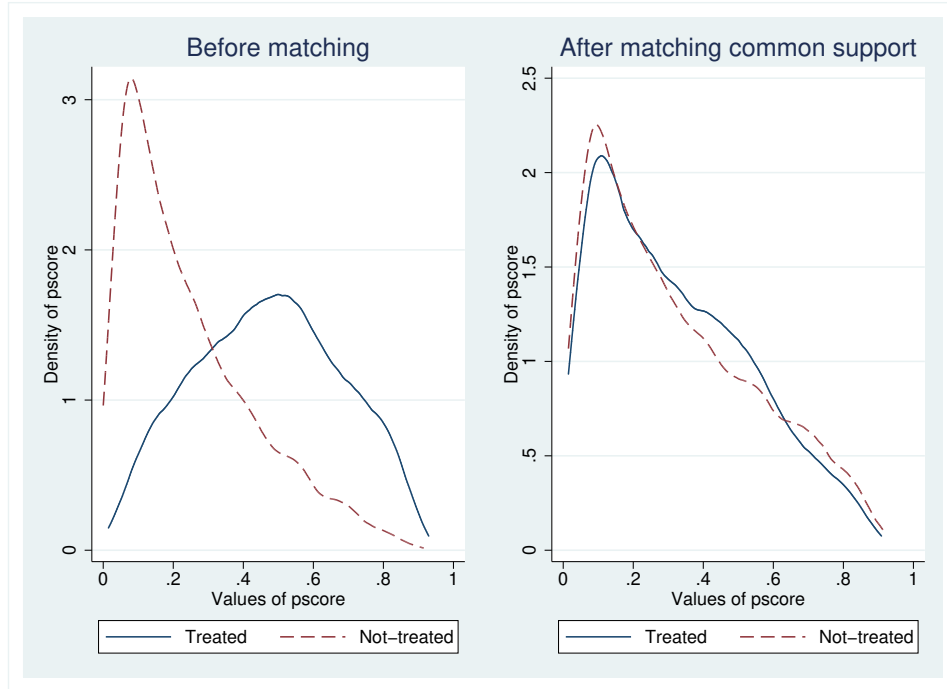
This parameter is identified under two assumptions. First, the Conditional Independence Assumption (CIA) states that outcome dynamics should be independent from treatment given the set of covariates X_i . Second, the common support states that the distribution of estimated propensity scores between the treated and the control group sufficiently overlap. One issue is that gender equality bargaining strongly depends on the size of the firm. The share of treated individuals is almost nil in firms with less than 50 employees while it is 66.4% in those with 1,000 employees or more. To ensure the validity of the common support assumption, we restrict our sample to individuals employed in firms with 50 employees or more one year before having their first child.

We use a two-step strategy. First, we estimate the propensity score $P(D_i = 1|X_i)$ using a rich set of covariates observed one year before the first child birth, including all the characteristics reported in Table 1 and year dummies⁹. To check whether the distribution of the propensity scores sufficiently overlap between treated and non-treated mothers, we plot in Figure 3 the density of estimated propensity scores in each sample before and after reweighting. The common support is defined by a Min-Max method (Dehejia and Wahba 1999). Restricting the sample to the common support does not lead to a strong loss of observations: we only drop 1 treated and 32 untreated mothers.

To check whether this method allows to make both groups, treated and control, comparable in terms of characteristics, we plot in Figure 4 the standardized differences in means between both groups of each characteristic before reweighting (black circles) and after reweighting (black crosses). For the sake of readability, we only report in Figure 4 the standardized differences in means of the main determinants of GE bargaining. We observe that the strong differences that prevailed before reweighting drop to almost zero after implementing this correction.

9. Coefficients associated to covariates in our propensity score model are available upon request.

Figure 3 – Kernel density of the propensity score before and after matching



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

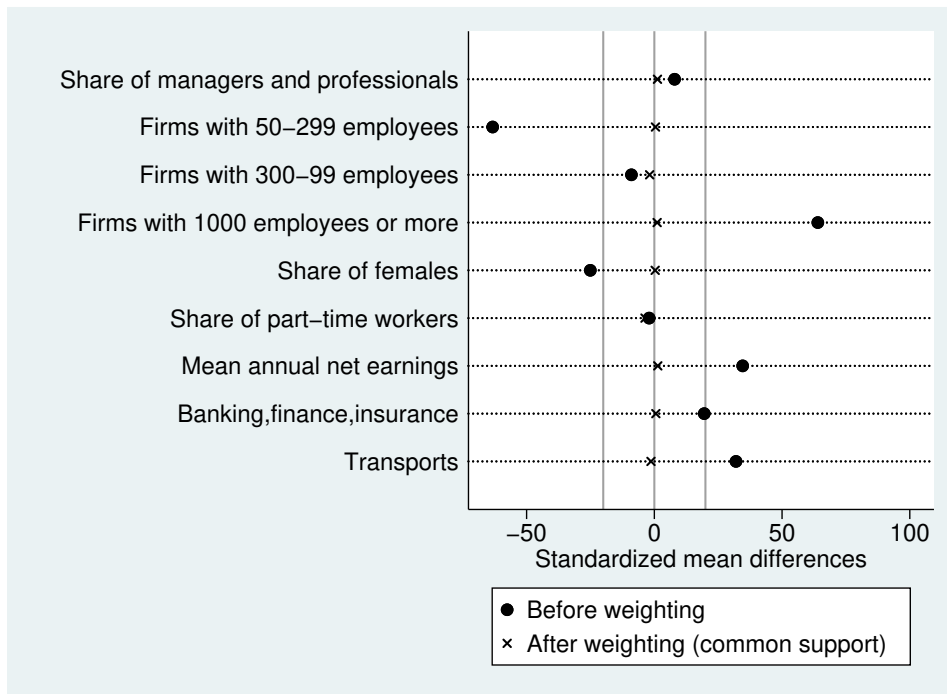
4.2 An Instrumental Variable approach

The reweighting procedure described above yields a consistent estimator of the ATT only if the CIA is valid. However, we could suspect that some variables omitted in our specification, such as the quality of social dialogue within the individual’s workplace may influence the treatment and the motherhood effect. In that case, our treatment is endogenous and the bias cannot be removed by an Inverse Probability Weighting method. To account for this potential endogeneity bias, we use an Instrumental Variable (IV) approach.

We use the 2010 reform, that introduced financial penalties for firms not complying with the obligation to be covered by a collective agreement or by a unilateral action plan for GE, as a quasi-natural experiment. This reform was targeted on firms with 50 employees or more, but did not concern those with less than 50 employees. Let G_i be a dummy variable equal to one if the woman i is employed in a firm with 50 employees or more one year before the birth of her first child. Let $Post_i$ be a dummy variable indicating whether the woman i ’s first child is born in 2012 or later. We use the interaction term between G_i and $Post_i$ as an instrument of the treatment.

For our identification strategy to be relevant, we check first whether this instrument is a strong predictor of the treatment. A visual inspection of the first stage is given by Figure 5. We depict

Figure 4 – Balancing property



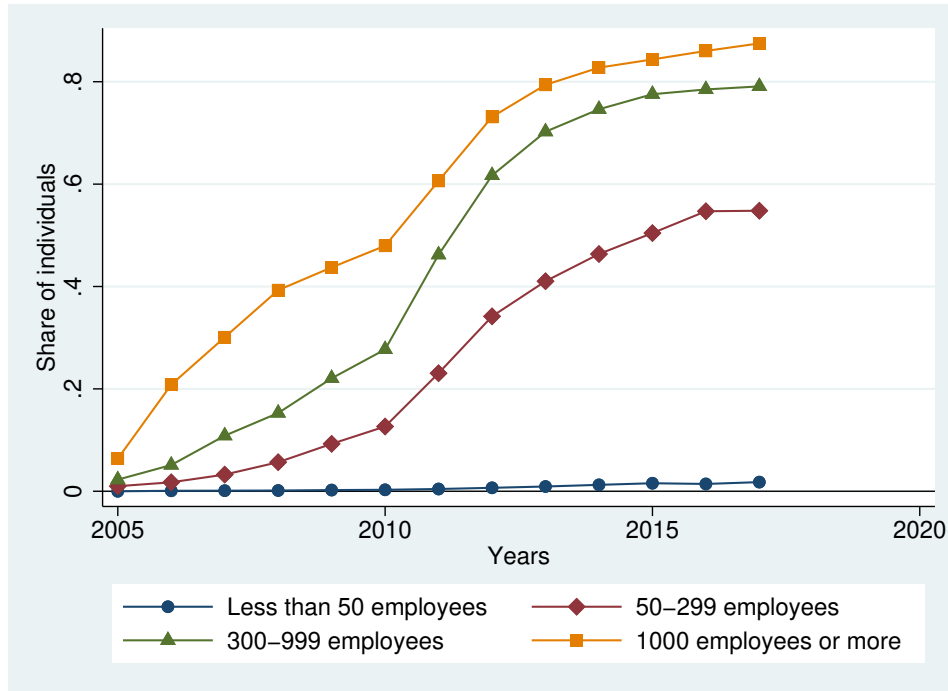
Sources: DADS-EDP panel, D@ccord database.

the evolution of the proportion of treated mothers according to the size of their firm. Until 2010, the share of treated mothers in firms with less than 50 employees is almost 0 while its counterpart in firms with 50 employees or more slightly increases over time. Not surprisingly, the larger the firm, the steeper the rise in the incidence of GE bargaining within the workplace. However, the 2010 reform has led to a strong acceleration of gender equality bargaining but only within firms with 50 employees or more. For example, between 2005 and 2010, the proportion of treated mothers within firms of 50-299 employees rose from almost 1% to 12.7%. Then, between 2010 and 2015, this share rose from 12.7% to nearly 50%. At the same time, the share of treated mothers in smaller firms remains close to zero. This ensures that our instrument is a strong predictor of the treatment¹⁰.

Our instrument is valid only if it does not affect the motherhood effect through another channel than gender equality bargaining. Our strategy consists in comparing earnings impact of parenthood in small and large firms before and after 2010. It is well acknowledged that earnings dynamics strongly differ by firm size and have been strongly affected by the 2010 economic crisis. However, our instrument is the interaction term between a firm size dummy variable and a year dummy vari-

10. After regressing the treatment dummy variable D_i on the set of characteristics X_i described in the previous section and on the excluded instrument, *i.e.* the interaction term between G_i and $Post_i$, we find that the latter increases by 23.4 percentage points the probability of being treated. The associated F-test for excluded instruments amounts to 445.71 so we can reject the null hypothesis of weak instrument.

Figure 5 – Proportion of treated mothers according to firms’ size over the period 2005-2018



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

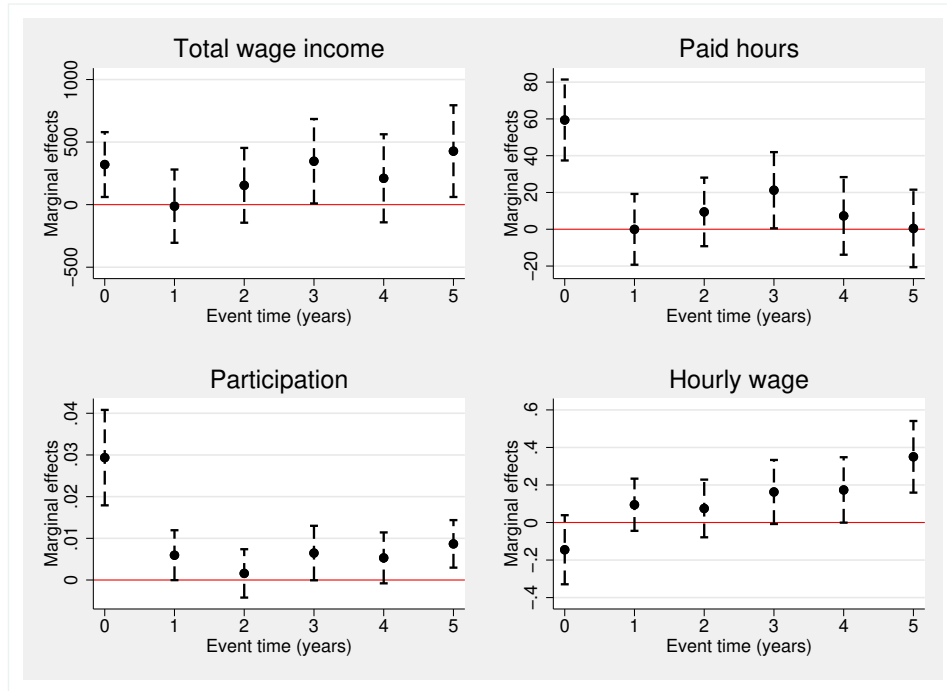
able, and we control for firm size and year dummy variables in our IV model. At first sight, there is no reason to observe any correlation between the instrument and the outcome dynamics once controlling for size effects and time trends. However, one could suspect that earnings dynamics may have diverged before and after 2010, *i.e.* in the aftermath of the economic crisis, and that the latter has had differential effects on mothers’ earnings according to the size of the firm. This may be particularly true when confronting wage dynamics before and after crisis between small firms and very large firms with 1,000 employees or more. To test the validity of this approach, we will conduct a robustness check narrowing the range of firm size and restricting our sample only to individuals employed at event time -1 in firms with less than 300 employees.

Note that the causal effect estimated through the Inverse Probability Weighting or the IV approach are not the same. The former measures the ATT every year after the first child birth and concerns only mothers employed in firms with 50 employees or more. The latter captures a local effect, *i.e.* the impact of gender equality bargaining on earnings impact of motherhood in firms complying with the legal obligation after 2010.

5 Results

We explore first the effects of gender equality bargaining on motherhood penalty without accounting for the endogeneity of the treatment. In Figure 6, we present estimates of the ATT every year after the first child birth without reweighting by the propensity score and the corresponding 95% confidence intervals. We do not exclude women employed in firms with less than 50 employees nor restrict our sample to the common support. Each dot actually measures the difference between the continuous line and the dotted line observed for women in Figure 1. It allows to quantify the attenuation effect presented in the descriptive statistics.

Figure 6 – Effects of gender equality bargaining on mothers’ annual net earnings and its components without reweighting by the propensity score



Sources: DADS-EDP panel, D@ccord database. Notes: The figure displays the estimated effects of gender equality bargaining on changes in mothers’ total wage income and its components (paid hours, participation and hourly wage) from the first child birth (0) to five years after (5) with their 95% confidence intervals.

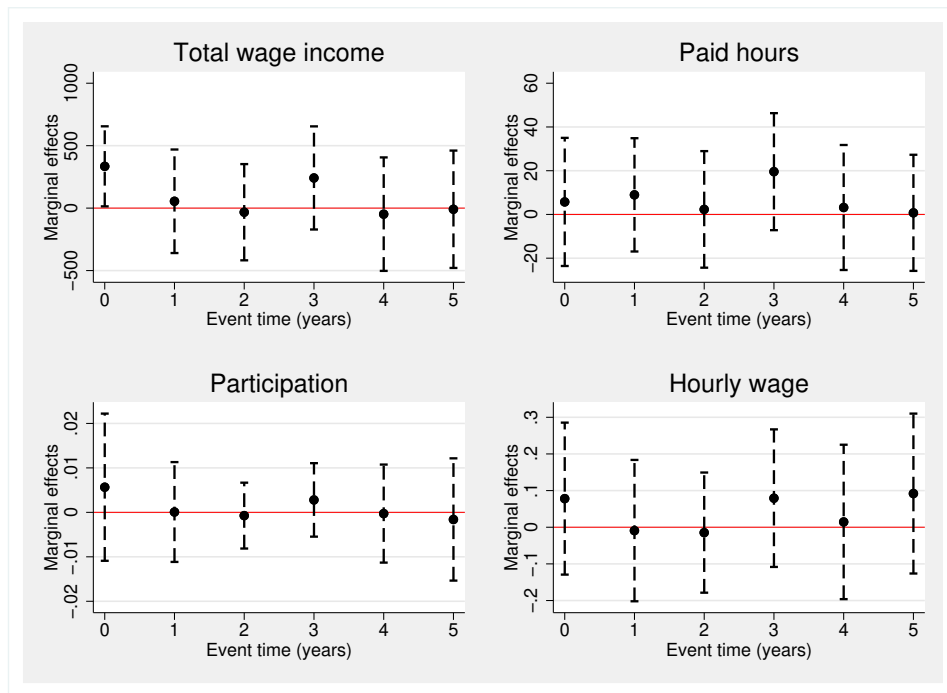
The ATT is always positive and even significant at a 5% level at event time 0, 3 and 5. This suggests a positive association between GE bargaining and earnings dynamics after the first child birth. More precisely, at the year of first child birth, being employed in an establishment covered by a GE text one year before is associated with a significant attenuation of the motherhood penalty in earnings. The average drop in annual net earnings for mothers turns around 7,000 euros¹¹, so this

11. Recall that maternity allowances are not accounted in our measure of earnings. Since maternity leave in France

attenuation effect is small in magnitude (less than 5% of the drop in earnings just after the birth) but significant and it persists over time. This effect would mainly pass through an increase in paid hours, a higher participation rate and a higher hourly wage rate.

Then we present the same graphs after excluding women employed in firms with less than 50 employees, restricting the sample to the common support given the distribution of propensity scores and reweighting by the latter. The estimates of corresponding ATT and confidence intervals are depicted in Figure 7. Once controlling for the selection bias on observables, we fail to find any significant positive effect of gender equality bargaining on earnings impact of motherhood, except at the event time 0. The attenuation effect persistent over time seems to be mainly driven by differences in workplace characteristics such as firms' size, occupational structure or sector.

Figure 7 – Effects of gender equality bargaining on mothers' annual net earnings and its components using an Inverse Probability Weighting method

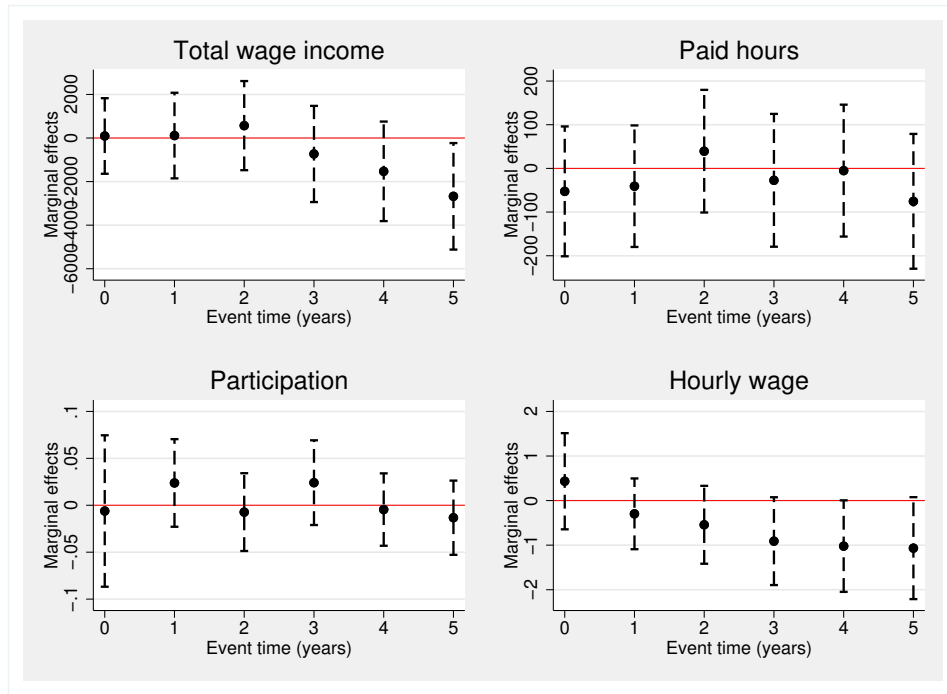


Sources: DADS-EDP panel, D@ccord database. Notes: The figure displays the estimated effects of gender equality bargaining on changes in mothers' total wage income and its components (paid hours, participation and hourly wage) from the first child birth (0) to five years after (5) with their 95% confidence intervals.

Once addressing the potential endogeneity bias, we draw different conclusions. ATT estimates obtained from the IV model described in the previous section are reported in Figure 8. The local effect of GE bargaining on earnings impact of motherhood estimated through an IV model is almost lasts for 16 weeks, this does not reflect a strong drop in mothers' income just after the birth.

nil just after the first child birth but tend to be negative a couple of years after. This negative effect even turns out to be significant 5 years after the birth. This suggests that forcing firms to negotiate favorable terms for women and gender equality has led to an increase in the motherhood penalty in these firms. This negative effect would mainly pass through a gradual reduction in hourly wage rates. This result is consistent with findings of Lucifora et al. (2021) that put forward a negative effect of some firm-level child-related policies on females’ access to internal promotions. These measures would lead to a “mommy track” *i.e.* a less steep career path than childless women.

Figure 8 – Effects of gender equality bargaining on mothers’ annual net earnings and its components using an Instrumental Variable model



Sources: DADS-EDP panel, D@ccord database. Notes: The figure displays the estimated effects of gender equality bargaining on changes in mothers’ total wage income and its components (paid hours, participation and hourly wage) from the first child birth (0) to five years after (5) with their 95% confidence intervals.

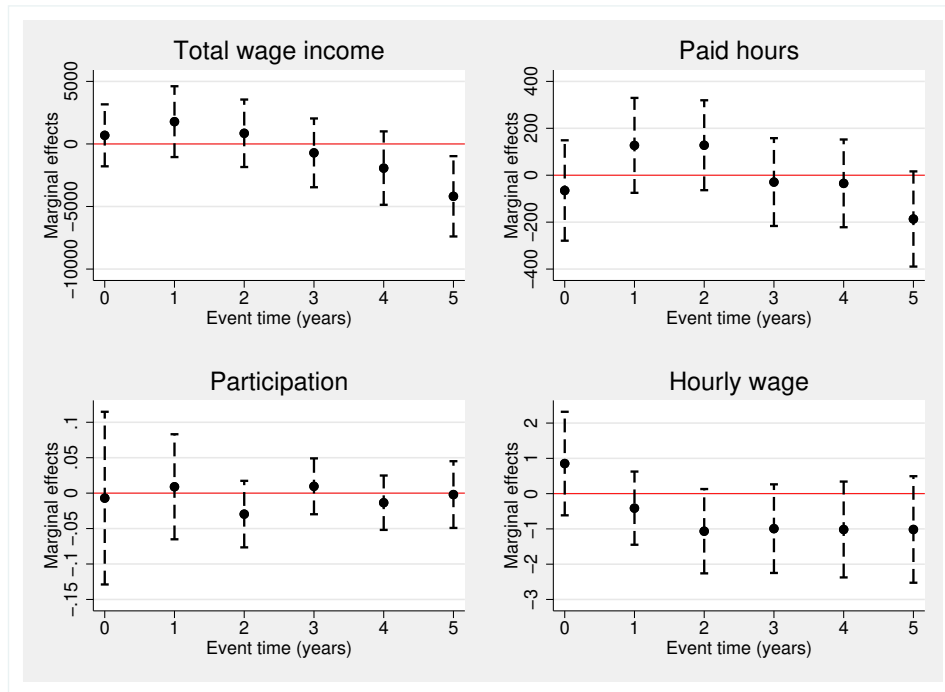
6 Robustness checks

One could argue that all these graphs capture not only the earnings impact of the first child birth but also, in some cases, the effect of the second child birth. Indeed, the average time duration between the first and the second child turns around three years. Since our follow-up period lasts for five years, we do not isolate the proper effect of the first child birth. To address this issue, we estimate the same effects restricting our sample of mother to those having only one child over the period. This reduces our sample because we end up with only 5,708 one-child mothers. We expect

that this restriction will lower the significance of the estimated coefficients. However, it ensures to capture the causal effect of gender equality bargaining on the earnings impact of the first child birth.

We present the results from the IV model run on this subsample of one-child mothers in Figure 9. We still observe that the treatment exacerbates the motherhood penalty, even though the precision of estimated effects is lower. This supports our baseline estimates.

Figure 9 – Effects of gender equality bargaining on mothers’ annual net earnings and its components using an Instrumental Variable model for one-child mothers only



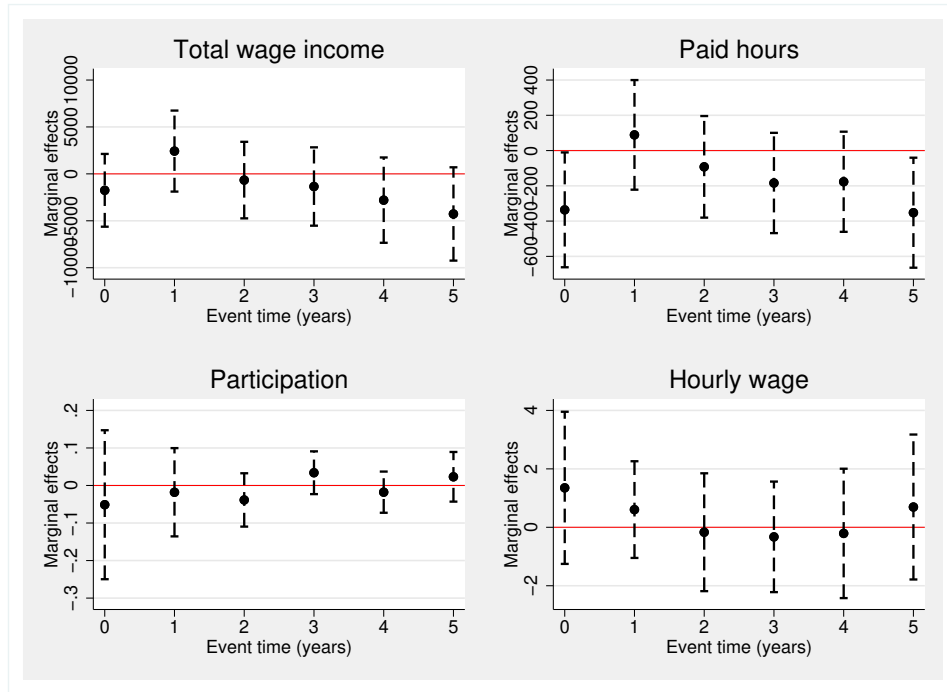
Sources: DADS-EDP panel, D@ccord database. Notes: The figure displays the estimated effects of gender equality bargaining on changes in mothers’ total wage income and its components (paid hours, participation and hourly wage) from the first child birth (0) to five years after (5) with their 95% confidence intervals.

We can also have some doubts about the relevance of our instrument, as it consists in comparing earnings dynamics in small and very large firms before and after 2010. We could suspect that the economic crisis has had differential effects on earnings dynamics according to the size of the firms. To test whether our IV estimates are robust to this criticism, we restrict our sample to one-child mothers employed in firms with less than 300 employees. If our effect would be driven by difference in trends of earnings before and after 2010 between small and large firms, it should be strongly affected when narrowing the range of firm size. This subsample is made up of 3,311 one-child mothers.

The corresponding estimates presented in Figure 10 support our main findings. Even though

the precision of our estimates is smaller, given the small number of mothers in our subsample, we find the same pattern of the ATT over the time span after the first child birth. Starting at almost zero, this effect turns out to be negative and almost significant at a 5% level five years after the birth.

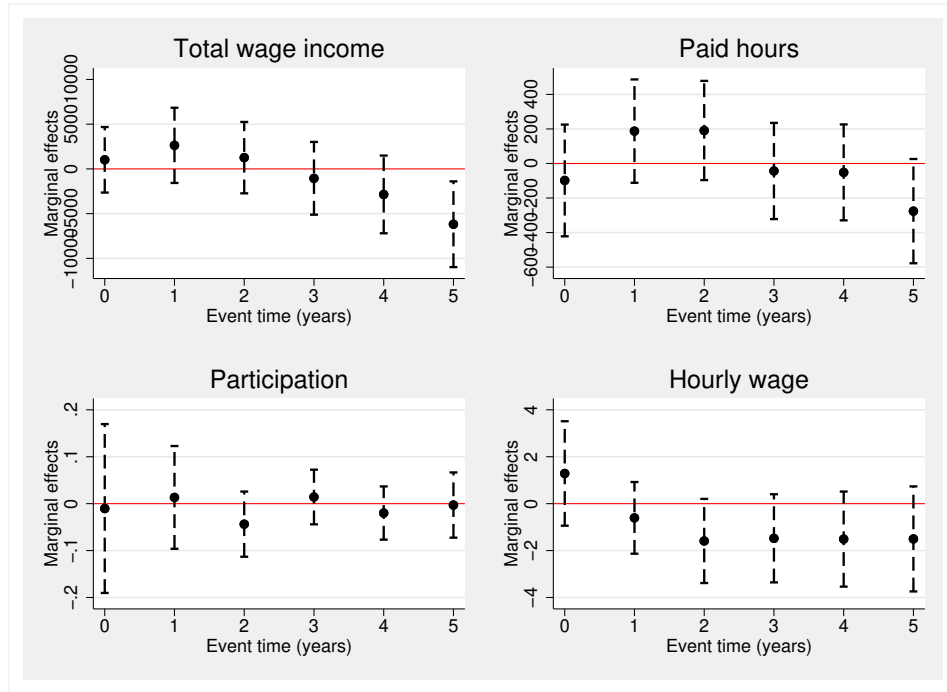
Figure 10 – Effects of gender equality bargaining on mothers’ annual net earnings and its components using an Instrumental Variable model for one-child mothers only employed in firms with less than 300 employees



Sources: DADS-EDP panel, D@ccord database. Notes: The figure displays the estimated effects of gender equality bargaining on changes in mothers’ total wage income and its components (paid hours, participation and hourly wage) from the first child birth (0) to five years after (5) with their 95% confidence intervals.

We also check whether these results are robust when using alternative definitions of the treatment. Our baseline definition included all texts that mention GE issues, even though it is not the only one object of the negotiation. To test the sensitivity of our results to the type of gender equality outcomes, we consider that a women is treated if she is employed in an establishment covered by a text that exclusively deals with gender equality. This corresponds to a more restrictive definition of the treatment. The share of treated is therefore lower (14.82%) than in our baseline definition of the treatment (19.87%). Figure 11 presents the ATT when using this alternative definition of the treatment. We still find this decline in the ATT over time until the marginal effect of gender equality bargaining on earnings dynamics turns out to be negative and significant five years after the first child birth. Our baseline results are therefore robust to the definition of the treatment.

Figure 11 – Effects of gender equality bargaining on mothers’ annual net earnings and its components using an Instrumental Variable model for one-child mothers only considering a restrictive definition of the treatment



Sources: DADS-EDP panel, D@ccord database. Notes: The figure displays the estimated effects of gender equality bargaining on changes in mothers’ total wage income and its components (paid hours, participation and hourly wage) from the first child birth (0) to five years after (5) with their 95% confidence intervals.

7 Conclusion

Using a unique combination of French administrative data, we explore the effect of GE bargaining on earnings impact of motherhood. We compare earnings dynamics after the first child birth of mothers according to whether they are employed in a firm covered by a GE text one year before having their first child. To tackle the endogeneity bias, we exploit the 2010 reform that introduced financial sanctions to firms with 50 employees or more not complying with their obligation of negotiating on GE issues as a quasi-natural experiment. Indeed, women employed in concerned firms whose first child is born after the reform have a probability of working in a firm covered by a GE text 23.4 points higher than those employed in firms with less than 50 employees and whose first child is born before 2010.

A simple comparison of earnings dynamics between treated and untreated mothers using an event-study approach would suggest that GE bargaining attenuates the motherhood penalty. Such descriptive statistics show a lower drop in mothers’ earnings after the first child birth in firms covered by a GE text. However, our IV estimates yield a very different picture. We show that GE

bargaining reinforced the earnings penalty for mothers. While the causal effect of GE bargaining on mothers' earnings is almost nil just after the first child birth, it turns out to be significantly negative 5 years after. This provides empirical evidence of a higher drop in annual net earnings for mothers employed in firms covered by a GE text.

These findings are in line with the results of Lucifora et al. (2021) that exhibit a slower and less steep career path for mothers in a large French firm. This firm had adopted some measures related to GE especially favoring work-life balance, such as extended child related leaves, that indirectly discriminate mothers. Our study generalizes this result showing that forcing firms to promote such measures may have deteriorated the mothers' career. Note that the effect we found is only local and concerns the compliers, *i.e.* the firms with 50 employees or more who started negotiations on GE after the 2010 reform. It does not say anything on the effect of GE texts adopted by firms who complied with their bargaining obligations just after the 2001 reform. We could expect that negotiations on GE that only resulted from the state intervention without any real favorable bargaining environment are more likely to generate empty shell agreements, not followed by adjustment measures introducing corrections in gender wage gap.

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