

Worker-firm matching and the parenthood pay gap: Evidence from linked employer-employee data

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Abstract The parenthood pay gap is not fully explained by human capital depreciation and unobserved heterogeneity. Endogenous worker-firm matching could also account for such wage differences. This hypothesis is tested thanks to linked employer-employee data on the French private sector between 1995 and 2011. Child-birth penalties are estimated for women and for men from hourly wage equations including firm- and worker-fixed effects on top of usual measures of human capital. Though worker-firm matching explains none of the motherhood wage penalty, it plays a role in the case of fathers who do not experience any wage loss after childbirth, but do not enjoy any premium either; there is evidence of an erosion of this premium since the end of the 1990s. In a counterfactual where women do not incur any penalty after childbirth, the gender gap still amounts to 2/3 of the one that currently prevails.

Keywords High dimensional fixed effects · Worker-firm matching · Parenthood pay gap · Gender inequalities · Linked employer-employee data

JEL Classification J13 · J16 · J31

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1 Introduction

“Qui va garder les enfants ?”
 “Who’s gonna watch the kids?”

Presidential campaign, France, 2007.

As both a woman and a mother, Ségolène Royal faced numerous sexist remarks during her 2007 French presidential election campaign. Surprisingly, the toughest critiques came from her own party (the left-wing socialist party), as was noted by most political observers. The quotation above apparently refers to the fact that Royal’s companion at the time was François Hollande.¹ This shows not only how deeply grounded sexist stereotypes are but also how reluctant some individuals (presumably, but not exclusively, men) still are to be ruled by women.²

Not only do gender inequalities persist within households (in terms of the share of domestic work or bargaining power; see, e.g., Goldin 2014; Meurs and Ponthieux 2015) but they also persist within firms, which labor economists and sociologists have documented for decades. The gender pay gap, occupational gender segregation, and the glass ceiling are the most striking examples: they are both unfair and inefficient as long as they are not justified by productivity differentials. Although the gender gap has begun to decline slightly, it has not fallen to zero (OECD 2012). Composition effects tend to locate women at lower positions within the hierarchy, which contrasts with their higher average level of education—at least in most European Union member states. This discrepancy between education and occupation has been designated as a form of gender segregation. In the same vein, the glass ceiling that prevents women from reaching the top positions of the institutional hierarchy (CEOs, national presidencies, etc.) is difficult to break.

However, the most obvious gender inequality is related to childbirth. The parenthood pay gap, which accounts for hourly wage differentials following childbirth between workers who have a child and those who do not, was first documented by Waldfogel (1997). Numerous papers have since determined the magnitude of motherhood penalties. More recent papers have also assessed the existence of wage differences among men between fathers and non-fathers. Four primary theoretical explanations for post-childbirth hourly wage differentials have been proposed, namely (i) human capital depreciation, (ii) individual unobserved heterogeneity (parents would have specific average productivities), (iii) firm matching (parents would match with specific firms), and (iv) discrimination. However, the relevance of each of these explanations remains to be assessed empirically.

¹Royal is currently Minister of Ecology and Sustainable Development, while Hollande is President of France.

²The author of that quotation ignores probably that more than two centuries went by since Mary Wollstonecraft condemned the restrictive domestic sphere to which women were confined. In Wollstonecraft (1792), she praised for the same level of education for men and women and, implicitly, for no other discrimination than capacities. Ironically, the posterity has focused on her private life as storied *post mortem* by her husband William Godwin in Godwin (1798). Mary Wollstonecraft was (also) the mother of the writer Mary Shelley.

The contribution of this paper consists in testing whether the parenthood pay gap stems from selection effects of parents into firms, that is, from endogenous worker-firm matching. To the best of my knowledge, this firm matching explanation has never been investigated; this stems *a priori* both from the lack of appropriate data and from methodological issues closely related to the estimation of models with high-dimensional fixed effects. However, the typical approach (Mincer equations) might suffer from omitted variable biases due to endogenous sorting. The latter biases are important if individuals who plan to become parents move to family-friendly firms, either before or after childbirth. This is particularly likely if parents trade-off money against job amenities, including workplace nurseries, remote work, flexible work hours, and more generally any family-oriented facility at workplace (Christmas tree, family discounts for leisure activities through works council in Europe at least, etc.). Such characteristics are typically unobserved and cannot be accounted for by controlling for jobs' characteristics only. In that case, the estimated wage differentials would spuriously account for endogenous mobility instead of referring to the causal impact of children.

I resort to panel data that are also linked employer-employee data (LEED), that is, the statistical unit of which is a (individual, firm, year) triplet. LEED have proven exceptionally useful in the estimation of wage equations since Abowd et al. (1999) proposed a two-factor specification of Mincer equations, namely firm and worker fixed effects. I estimate an adjusted parenthood pay gap by employing such a two-factor model with high dimensional firm and worker fixed effects, which permits me to control for as much observed and unobserved heterogeneity as possible. Importantly, this econometric specification provides a solution to the previously mentioned omitted variable biases.

My application is based on the comprehensive DADS panel, which contains exhaustive information on French salaried employees' careers in the private sector from 1995 to 2011. This paper is the first attempt to analyze the parenthood pay gap by resorting to LEED. By disentangling the effect of childbirth from spurious correlation between parenthood and other firm-specific wage determinants, this estimation contrasts with previous analyses, which only control for individual unobserved heterogeneity. A *caveat* due to data limitations is the following: work hours have been available only since 1995, which leads me to select relatively young individuals for whom I know the complete path of hourly wages from 1995 to 2011.

After controlling for full-time and part-time experience, as well as for both firm and worker fixed effects, I still find a difference between non-mothers and mothers after childbirth that amounts to an effect of approximately -2.2% per child on the hourly wage, the latter being nonlinear in the rank of birth since the loss at the first child is about -4.7% . In the case of women, I reject the firm matching explanation, as well as the hypothesis of unobserved heterogeneity; what matters most is the absence of human capital accumulation. My results are strikingly different with respect to men. My sample of men does not experience any loss after childbirth but also does not enjoy any premium either. Moreover, the matching between workers and firms matters here, while unobserved heterogeneity and human capital explanations play no role. To explain the absence of fatherhood premia, I provide evidence of an erosion of those premia over time, such that one cannot reject the null hypothesis

during the period considered. My results are also consistent with previous findings documenting heterogeneity in those premia; overall, they indicate gender inequalities with respect to parenthood. Finally, I propose an evaluation of the contribution of the parenthood pay gap to the gender gap by simulating a counterfactual *scenario* in which women and men experience the same childbirth penalty: the former would explain approximately 1/3 of the latter in my sample of young individuals.

The remainder of the paper is organized as follows. Section 2 is devoted to a literature review and to a short presentation of the institutional background. Section 3 presents my matched employer-employee database. I describe my econometric specification in Section 4. Section 5 contains the results, namely the test of the firm matching explanation, as well as a measure of the contribution of the parenthood gap to the gender gap, and robustness checks. Section 6 concludes with some policy recommendations.

2 Literature review and institutional background

2.1 Literature review

The seminal contributions of Waldfogel (1997, 1998) document the existence of a motherhood wage penalty both in the USA and in the UK. Relying on data from 1968 to 1988, she estimates Mincer equations on log hourly wages with individual fixed effects and finds a wage loss of approximately -6% per child (see also Budig and England, 2001). Similar motherhood penalties are found in Europe in general (Davies and Pierre 2005), in Germany where the gender pay gap is very high (approximately 22%), and where the motherhood wage penalty is greater than 10% in absolute terms (see, e.g., Beblo et al. 2009; Buligescu et al. 2009; Felfe 2012), and in Denmark where an on-going work by Kleven et al. (2015) finds dynamic, long-run effects of childbirth on mothers' wages. An excellent survey of this literature can be found in Meurs and Ponthieux (2015).

Men are not absent from the picture; yet they tend rather to benefit from child-births: Lundberg and Rose (2000) find that fatherhood significantly increases the hourly wage rate. In France, Pailhé and Solaz (2007) explain that for men, a childbirth could be related to some promotion while for women childbirths were more associated with career interruptions, parental leaves, or part-time work, especially at high ranks of birth, which complements Hakim (1998)'s analysis of gender-specific preferences toward part-time work resulting in job segregation. If Meurs et al. (2010) did not find any significant motherhood penalty, they found some evidence of a fatherhood premium. Heterogeneity matters: Glauber (2008) points out that fatherhood premia are closely tied to marriage since unmarried men do not experience any premium; Killewald (2013) concludes that unmarried residential fathers, nonresidential fathers, and stepfathers experience no significant premium. The existing literature points out a gender-based inequality with respect to childbirth.

Another debate focuses on the relationship between the timing of births and the family pay gap. The idea is that women postpone having a child to accumulate more work experience and favor their career. However, identifying the causal effect from

delayed maternity on earnings is not an easy task since it requires appropriate instruments. In the USA, Miller (2011) uses biological fertility shocks as instruments for age at first birth to recover a causal impact of the timing of childbirths on wages: delaying birth by 1 year would raise earnings by up to 9/10 % and increase work hours by 6 %; it would also yield a flatter wage profile after motherhood.³

The motherhood penalty is a puzzling issue, and several theoretical explanations have been proposed to account for that wage differential. Most of the arguments below also apply to men—except the one concerning maternity leave, although in Norway for instance, welfare programs offer generous paternity leaves (14 weeks), which reduces the gender asymmetry in this respect.

First, motherhood implies some human capital depreciation due to mandatory parental leave. This “human capital deterioration” explanation dates back to Becker (1985). Human capital is a composite concept: it aggregates at least education, experience, and training. Women who wish to become mothers would rationally opt for lower educational levels. Their career has mechanically more frequent interruptions (sick leaves, either for themselves or for their children, in addition to maternity leave), which depreciates their work experience. Furthermore, the time spent out of the labor force is likely to have a negative impact on their training, especially if training is some function of continuous employment. Finally, mothers may prefer to work part-time, which further diminishes their work experience.⁴ Under the assumption of perfectly competitive labor markets, lower hourly wages must reflect a lower productivity caused by career interruptions or a lower training or education level. In Sweden, Albrecht et al. (1999) ask whether this hypothesis alone could be responsible for the parenthood pay gap; they find that human capital depreciation is not the sole explanation for the negative effect of career interruptions on subsequent wages.

Second, individual unobserved heterogeneity has been invoked to explain the wage gap between mothers and non-mothers. The former may choose careers that are more compatible with family life; this self-selection being primarily driven by preferences and/or personal abilities. The negative correlation between labor market outcomes and fertility could then reflect stronger preferences for family, domestic activities, or leisure or lower on-the-job productivity. Women endowed with such preferences and capacities would *ex ante* invest less in education and training, hence acquiring less human capital. The parenthood gap could thus reflect a different willingness to work in a competitive environment. Disentangling spurious correlation between childbirths and wages due to preferences from the causal effect of parenthood therefore requires controlling for unobserved heterogeneity, for instance that due to individual fixed effects—assuming that unobserved heterogeneity does not vary over time. Even after controlling for worker fixed effects in panel data, as is the case in most empirical papers cited previously, a substantial share of wage differences between mothers and non-mothers remains unexplained.

³See also Herr (2008, 2012), Bratti and Tatsiramos (2012) and Troske and Voicu (2013) on the same topic.

⁴One could argue that working part-time constitutes a negative signal that individuals send to their employers by reducing voluntarily their activity. However, this explanation does not belong to “human capital” theory but rather to a competing explanation, the “signaling” theory proposed by Spence (1973).

A third explanation (hereinafter “firm matching”) claims that mothers would be employed in less productive firms. To reconcile family life and careers, women who plan to be mothers would look for jobs that allow them to spend more time in child-rearing activities. For instance, they would favor jobs with flexible hours, on-site day care, jobs in which personal phone calls are authorized during work, or jobs that do not require overtime work, evening work, work during weekends, etc. In that case, in equilibrium, occupational segregation should emerge in the labor market. As a result, forward-looking women who want children would seek more convenient and less energy-intensive jobs. In the same vein, mothers may have higher search costs on the labor market, which restricts their mobility and prevents them from looking for better positions while resulting in poor job matches. Surprisingly, this explanation has received little attention thus far. Budig and England (2001) proposes controlling for as many job characteristics as possible, including part-time employment, to neutralize any “family-friendly” job feature. From the substantial literature devoted to job search, we know that there is considerable heterogeneity in the quality of the employer-employee relationship and that mobility offers potentially large wage gains. Nielsen et al. (2004) show that mothers tend to self-select into the public sector. Beblo et al. (2009) argue that the selection into private establishments has to be taken into account because it could represent up to 7pp of the parenthood pay gap in the German case (−19 % after controlling for establishment effects in a matching approach against −26 % when such effects are ignored). This explanation is also advanced by Felfe (2012), who suggests that mothers are prepared to trade off earnings against amenities and hence proposes a compensating wage differentials (CWD) explanation. Among mothers, she distinguishes those who remain in the same position after maternity leave from other mothers.⁵ She finds that the former experience a significantly smaller wage gap (−9.3 % against −24.3 %) than the latter, which supports this hypothesis. However, part of the difference stems precisely from an adjustment of work conditions, and after controlling for this adjustment, the parenthood pay gap cannot be solely explained by a CWD explanation. By the way, Datta Gupta et al. (2008) have been warning us about the “boomerang effect” of family-friendly careers: they imply some adverse effects on women’s wages with consequences for gender equality, since they hinder women’s career progression (which they refer to as a system-based glass ceiling).

Fourth and finally, a further explanation for the motherhood wage penalty is the possibility of discrimination against mothers at work. Employers could be reluctant to hire mothers-to-be or women who they expect to become mothers, which would affect mothers’ employment. Moreover, employers could also be more rigid in the wage bargaining process and offer prospective mothers fewer opportunities to distinguish themselves (through more risky assignments for instance), which would result in a less frequent attribution of irregular bonuses. Generally speaking, such discrimination could result from labor reallocation, either within firms or within

⁵Changes in both men and women’s employment after childbirth have been widely documented in Pailhé and Solaz (2007).

establishments. The strategy adopted in this paper consists in providing indirect evidence in favor of discrimination by testing for the three previous explanations and by excluding them as possibilities. At least the elimination of alternative explanations, combined with asymmetric results for women and men, suggests that gender biases are likely.

2.2 Institutional background

In France, the maternity leave is rather long. Since 1970, mothers have had the right to take a maternity leave of 16 weeks (6 weeks before the expected date of delivery and 10 weeks after) for first or second child, but 26 weeks for a third child or more; in the case of twins, the leave is 34 weeks and for triplets it is 46 weeks. Moreover, the minimal mandatory duration is 8 weeks. It is paid the equivalent of the net salary with a ceiling at 3000€ a month. Since 1946, fathers have obtained a so-called birth leave which enables them to have 3 days following a childbirth, but this leave has been extended to 11 days as a paternity leave since 2002 only. The latter extension is paid for by social security as a replacement wage, while the first 3 days are compensated directly by the employer. A parental leave was created in 1977 that permits an absence of 1 year and that is renewable up to 3 years during which the parent receives a minimal allowance (35 % of the minimum wage). Finally, employers also allow parents to have unpaid leaves to care for a sick child provided that they last less than 3 days.

As documented by Lefèvre et al. (2007), France has a long tradition of family-friendly policies at work. Nevertheless, there is some heterogeneity with respect to size and sector. Three quarter of employers with at least 20 employees are aware of the family profile of their employees and acknowledge it is a duty for them to implement such policies. In the public sector, child-related cash benefits are frequent; in the private sector, parents have access to mutual insurance fund contributions, get holiday vouchers, receive contributions to education costs as well as to bonuses. Many firms offer insurance benefits for childbirth, but it is less often to get child care, child education benefits, or holiday vouchers. About half of firms with at least 20 employees offer some childbirth-related bonus.

To promote family-friendly policies, a family tax credit was introduced from 2004, which represents 25 % of the amounts spent, up to a maximum of 500,000 € per year and per employer, and concerns four categories of expenditure: (i) day care centres for employees' children aged under three or funding of external care places; (ii) training of employees on parental leave; (iii) supplementary payments for paternity, maternity, parental, or sick child leave; and (iv) payment of employees' exceptional child care expenses due to unforeseeable work commitments outside normal working hours.

On top of those pecuniary benefits, employers manage to make working hours more flexible; for instance, 85 % of them report that work schedule can be modified for the first day of school year. However, part-time work is twice more accepted in firms with at least 50 employees. Some companies promote flexible work schedules, including telecommuting agreements (working from home), part-time work, day work with a unique constraint of core hours (4 hours a day), Wednesday for mothers, etc.

Some firms offer domestic services like a concierge (dry cleaning, shoe repair, automotive services, postal services, food shopping), access to inter-company day care, and since 2005, the creation of mini-clubs catering to employees' children within the company's premises on Wednesdays and during school holidays. Sometimes, social workers are available to inform, listen, and support employees in case of difficulties with respect to children. Many companies are also developing company or intercompany day care, recreational centers for children and cash benefits for parents (so-called CESU for Universal employment checks) to contribute to childcare costs. One in 15 people working in an establishment of at least 20 employees has access to day care center places, and 1 in 20 to a leisure center.

3 Data

3.1 Sources

My analysis is based on the merger of two French administrative datasets commonly known as the DADS-EDP panel collected by Insee.

The first source is the DADS panel, a comprehensive database of salaried employees, and the longitudinal version of the cross-sectional *Déclaration Annuelle de Données Sociales* (DADS). It is mandatory for French firms to fill in annually a DADS for every employee subject to payroll taxes, that is, for every salaried employee in the private sector or in government-owned firms, at the exclusion of civil servants and self-employed. (Another source provides me with information on the public sector, which I disregard here because the number of work hours is not available.) From 1967 to 1975, the panel does not contain any information on firms while from 1976 to 2011, the data are available at the individual-firm level. Every firm—more precisely, every establishment—has a unique identifier, the SIRET,⁶ a 14-digit number, while individuals are identified by their NIR, a social security number with 13 digits. Before 1976, an observation is made up of a unique (individual, year) pair while after 1976, it is composed of a unique (individual, firm, year) triplet, which features the data as linked employer-employee data (LEED). The DADS panel contains information about individuals born on October of even-numbered years—a representative sample of the French salaried population at rate 1/24. From 2002 onwards though, the panel has been completed with individuals born on October of odd-numbered years, which corresponds to a sampling rate of 1/12; however, the longitudinal depth is mechanically shorter for such individuals in comparison with those born on October of even-numbered years. Since filling in the DADS form is mandatory,⁷ and because of the comprehensiveness of the DADS panel with respect to individuals' careers, the data are of exceptional quality and have low measurement error in comparison with survey data. Some years are missing (1981, 1983,

⁶The SIRET is a concatenation of the SIREN, a firm identifier, and of an establishment identifier.

⁷The absence of a DADS as well as incorrect or missing answers are punished by law with fines.

and 1990) because there was no data collection by Insee during the 1982 and 1990 censuses. In 1994, 2003, 2004, and 2005, the quality is nevertheless questionable. Since overseas appeared in the panel from 2002 onwards, I restrict my attention to metropolitan France. Finally, these data contain detailed information about gross and net wages, work days, work hours, other job characteristics from 1976 onwards (like the beginning and the end of an employment's spell, seniority, a dummy for part-time employment), firm characteristics (industry, size, region), and individual characteristics (age, gender).

The second source is the *Échantillon Démographique Permanent* (hereinafter EDP). This longitudinal database covers a representative part of the population born on one of the first 4 days of October. It contains administrative registers of births and marriages⁸ from 1968 to 2011, as well as partial information on education from 1968, 1975, 1982, 1990, and 1999 censuses. However, for half of the sample, namely people born on October 2nd or 3rd, birth registers have not been properly filled in from 1983 to 1997. The information is incomplete from 1983 to 1989 and missing from 1990 to 1997. It is possible to recover part of missing data by exploiting 1990 and 1999 censuses, but at some cost (namely measurement error since the information conveyed by both censuses does not coincide perfectly). I choose the most conservative option: I rely on birth registers and keep therefore individuals born on October 1st or 4th only. This subsample is still representative of the French population⁹ at rate 2/365.

The two sources can be merged through their common individual identifier, the NIR. I exclude "wrong" NIRs that are present in the DADS panel for cross-sectional use and for statistical reasons only, as I cannot follow the careers of such individuals.¹⁰ I also exclude self-employed individuals who appeared in the panel from 2009 onwards. Finally, I eliminate observations that correspond to home-work or to unemployment amenities.

A methodological contribution of this paper consists in computing an accurate measure of salaried experience. I exploit the administrative feature of the dataset and derive experience at the individual level from the sequence of observed working times.¹¹ I count the actual (past and present) number of work hours and express it in full-time units (FTU). In France, a full-time worker used to work 2028 (1820) hours per year before (after) 2002—the mandatory working time decreased by 4 hours per week after the adoption of the Aubry laws. However, I face data limitations: work hours have been available only since 1995. Hence, I consider individuals who entered the panel after 1995 to avoid biasing my measure of experience because of missing or incomplete sequences of working time. After merging the two datasets and imposing this "entry condition," my sample includes 46,280 individuals. I proceed then to

⁸At the exclusion of contractual civil unions called PACS that have emerged in France since 1999. However, even if the number of PACS has raised dramatically since then, interestingly the number of marriages has not fallen accordingly but has rather been stable over the period, which indicates that PACS is not a perfect substitute for marriage, and that the content of marriage has been rather the same.

⁹Yet individuals born abroad are missing from the EDP.

¹⁰For instance, some of them were not born in October.

¹¹By definition, years of absence from the DADS file cannot be characterized.

further selection that is described more extensively in Appendix A: I focus on individuals aged 16 to 65 working in the private sector and whose annual wage exceeds 10 euros in 2011 terms. I disregard the years 2003 to 2005 in the analysis for the reasons mentioned above. The working sample contains 41,531 individuals, which represents 212,189 observations at the individual-year level and 301,079 observations at the individual-firm-year level. Although the sample is not representative of the entire French salaried population, the method I propose to address the omitted variable bias in the parenthood gap framework applies to any LEED sample.¹²

3.2 Descriptive analysis

Because of the entry condition mentioned above, the individuals in the sample are mechanically younger: they are aged 26.9 on average, which is relatively low and which constitutes the main empirical limitation of the current analysis. As Tables 1 and 2 show, large shares of people have not yet had their first child: 33.9 % have at least one child during the observation period (37.7 % of women and 30.5 % of men). Women experience their first childbirth at 26.1 on average, their second childbirth at 28.3, and their third childbirth at 29.7. By contrast, men are older at the time they become father for the first time (28.5); they are still older at second (30.9) and third birth (33.4). Then, 18.1 % of women (15.1 % of men) have exactly one child while 13.2 % (11 %) have exactly two children and 6.4 % (4.5 %) have three children or more. Again, the limited number of children per individual is due to the relative youth of the cohorts. Finally, 18.9 % of these individuals are married and, overall, the panel is composed of 48 % of women.

Some of these individuals worked continuously in the private sector from 1995 to 2011. The average potential experience amounts to 6.4 years, where potential experience is defined as the difference between the current year and the year the individual first appears in the panel. On average, full-time experience is 2.3 years in FTU, while part-time experience amounts to 0.8 years. Average seniority is approximately 2.5 years, where seniority is the difference between the current year and the year that an individual first appears in the current firm. The annual job duration amounts to 237 days, or 1037 h, which reflects composition effects explained by both youth insertion in the labor market and part-time activity.

The average net hourly wage amounts to slightly less than 10 euros. As I proceed to trim some very low hourly wages (see Appendix A and Section 5.3), the minimum observed hourly wage is 3.51 euros. In the labor market, women and men differ with respect to wages: women receive, on average, a lower net hourly wage (9.43 euros, which represents about 1430 euros per month) than men do (10.42 euros, which represents about 1580 euros per month), which yields an unadjusted gender pay gap of 9.6 %. Given that our sample is composed of young individuals who do not still benefit from career effects, these wages compare well to actual wages in the French

¹²As time passes, the entry condition will become less restrictive in terms of age—hence the selection will be less drastic in future works relying on the same source.

Table 1 Sample of women—descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
First year in panel	19,932	2003	4.728	1995	2011
Married	19,932	0.211	0.408	0	1
One child	19,932	0.181	0.385	0	1
Two children	19,932	0.132	0.339	0	1
Three children	19,932	0.046	0.209	0	1
Four children	19,932	0.012	0.107	0	1
Five children	19,932	0.006	0.077	0	1
Age	95,499	26.899	7.549	16	65
Potential experience	95,499	6.253	4.240	1	17
Full-time experience	95,499	2.117	2.717	0	16.9
Part-time experience	95,499	0.715	1.146	0	12.1
Seniority	95,499	2.402	2.586	0.01	17
Nb. of working days	95,499	229.4	134.1	1	360
Nb. of working hours	95,499	936	706	11	4056
Part time	95,499	0.433	0.495	0	1
Part time (FTU)	95,499	0.247	0.431	0	1
Net hourly wage	95,499	9.43	5.74	3.51	1010
Seniority	135,431	2.046	2.403	0.01	17

Sample of 19932 women working in the private sector from 1995 to 2011 (95,499 individual \times year observations, 135,431 individual \times firm \times year observations). Wages: in 2011 euros. Full-time experience: in full-time units (FTU)

private sector since they lie between the 3rd and the 4th decile of the unconditional distribution (about 50 % more than the minimum wage). Women also tend to have lower full-time experience but higher part-time experience. In FTU, 9.7 % of men's activity stems from part-time work, against 24.7 % of women's activity—the average volume of part-time activity is 15.8 %.

Turning to the issue of mobility on the labor market, I define a dummy variable for being employed by firm j on year t and by some firm $k \neq j$ on year $t + 1$. Indeed, about one tier of individuals are concerned by such transitions while the two remaining tiers are observed in the same firm j during both years. This holds regardless of the gender even if men (33.6 %) tend to be slightly more exposed than women (32.3 %) to such mobility. As suggested by the rationale, parents are also slightly less mobile according to this measure of mobility: 32.1 % against 34.2 % for non-parents; this holds both for fathers (32.6 % against 34.6 %) and for mothers (31.6 % against 33.4 %).

This issue of part-time work cannot be disregarded in an empirical analysis devoted to the effects of childbirth, as argued, for instance, by Budig and England (2001). In contrast to studies relying on full-time workers only, part-time workers are not selected out of the sample. However, the literature has not reached consensus on whether part-time work induces a penalty or a premium on the hourly wage. Using

Table 2 Sample of men—descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
First year in panel	21,599	2002	4.653	1995	2011
Married	21,599	0.168	0.374	0	1
One child	21,599	0.151	0.358	0	1
Two children	21,599	0.110	0.313	0	1
Three children	21,599	0.033	0.178	0	1
Four children	21,599	0.009	0.094	0	1
Five children	21,599	0.002	0.048	0	1
Age	116,690	26.901	7.235	16	65
Potential experience	116,690	6.598	4.242	1	17
Full-time experience	116,690	3.039	3.179	0	17
Part-time experience	116,690	0.372	0.711	0	14
Seniority	116,690	2.556	2.629	0.01	17
Nb. of working days	116,690	243.6	130.1	1	360
Nb. of working hours	116,690	1120	734	11	4400
Part time	116,690	0.243	0.429	0	1
Part time (FTU)	116,690	0.097	0.295	0	1
Net hourly wage	116,690	10.42	8.44	3.51	1760
Seniority	165,648	2.174	2.457	0.01	17

Sample of 21,599 individuals working in the private sector from 1995 to 2011 (116,690 individual \times year observations, 165,648 individual \times firm \times year observations). Wages: in 2011 euros. Full-time experience: in full-time units (FTU)

Australian data, Booth and Wood (2008) find that the negative coefficient of part-time work in a Mincerian equation on the hourly wage disappears after controlling for covariates (especially experience) and unobserved heterogeneity.¹³ I adopt their methodology and reproduce their Table 2. Hence, I specify:

$$\log W_{it} = X'_{it}\beta + \alpha P_{it} + \theta_i + \epsilon_{it}, \quad (1)$$

where W_{it} is the log hourly wage of individual i in year t (computed as the ratio between the sum of her wages and the sum of her work hours), X_{it} a set of covariates, P_{it} a dummy for part-time work, θ_i an individual fixed effect, and ϵ_{it} an error term. Table 3 reports the estimates of α under different specifications (with or without fixed effects θ_i , with or without covariates X_{it} including worker characteristics, firm characteristics, experience, etc.). In both pooled OLS and fixed effects approaches, the sign of $\hat{\alpha}$ is negative in the absence of controls, which means that part-time workers earn unconditionally less than full-time workers. However, $\hat{\alpha}$ becomes positive after

¹³To the best of my knowledge, the Australian case is not an outlier with respect to the issue of part-time employment.

Table 3 Estimates of part-time/full-time log hourly wage differential

Specification	Women		Men	
	Pooled OLS	FE	Pooled OLS	FE
(1)	-0.078*** (0.002)	-0.017*** (0.003)	-0.072*** (0.002)	-0.040*** (0.003)
(2)	-0.050*** (0.002)	0.030*** (0.002)	-0.003*** (0.002)	0.056*** (0.003)
(3)	-0.017*** (0.002)	0.034*** (0.002)	0.016*** (0.002)	0.054*** (0.003)
(4)	-0.011*** (0.002)	0.034*** (0.002)	0.027*** (0.002)	0.054*** (0.003)
(5a)	-0.003 (0.002)	0.034*** (0.002)	0.033*** (0.002)	0.054*** (0.003)
(5b)	0.013*** (0.002)	0.040*** (0.002)	0.051*** (0.002)	0.062*** (0.003)
(5c)	0.045*** (0.002)	0.052*** (0.002)	0.065*** (0.002)	0.067*** (0.003)
Observations	95,499	95,499	116,690	116,690

- (1) contains a constant
- (2) adds worker characteristics (quadratic specification in age, dummy if married)
- (3) adds firm characteristics (*département*, two-digit industry dummies and establishment size)
- (4) adds a quadratic specification in tenure
- (5a) = (4) + quadratic specification in potential experience
- (5b) = (4) + quadratic specification in experience
- (5c) = (4) + quadratic specification in full-time and part-time experience

controlling for observed and unobserved heterogeneity. This empirical result holds both for women and men.¹⁴

Another empirical issue worth examining is the relationship between family events, including childbirths and marriages, and experience. Once again, it is crucial to carefully distinguish between full-time and part-time experience. I therefore estimate:

$$\text{Exp}_{it} = X'_{it}\beta + \theta_i + \epsilon_{it}, \tag{2}$$

where Exp_{it} refers either to full-time experience (measured in FTU) or part-time experience. I do not include part-time work P_{it} as a covariate here because it would be correlated with the determinants of the dependent variable and hence endogenous in (2). Table 4 displays the results, which exhibit interesting gender differences. Women tend to accumulate less full-time experience every year than men do (.41 year as opposed to .5) but twice as much part-time experience (.12 versus .06). While women may lose up to 3 years of full-time experience after the fourth childbirth,

¹⁴In what follows, I will not distinguish P_{it} from the other covariates in X_{it} .

Table 4 Impact of childbirths on experience

Dependent	Women		Men	
	FT exp.	PT exp.	FT exp.	PT exp.
Age	0.409*** (0.007)	0.123*** (0.004)	0.500*** (0.006)	0.057*** (0.002)
First childbirth	0.183*** (0.040)	0.052* (0.021)	0.474*** (0.035)	-0.044*** (0.012)
Second childbirth	-0.237*** (0.070)	0.260*** (0.037)	0.902*** (0.057)	-0.078*** (0.020)
Third childbirth	-1.362*** (0.129)	0.339*** (0.069)	0.959*** (0.109)	-0.062 (0.039)
Fourth childbirth	-2.570*** (0.250)	0.158 (0.112)	1.072*** (0.242)	-0.123* (0.058)
Fifth childbirth	-3.530*** (0.572)	-0.047 (0.155)	-0.494 (0.485)	-0.027 (0.137)
Year dummies	Yes	Yes	Yes	Yes
Individual effects	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes
Observations	95,499	95,499	116,690	116,690
R ²	0.858	0.779	0.902	0.768

FT exp.: full-time experience, PT exp.: part-time experience

men's full-time experience never diminishes with parenthood: it appears as if men worked more to compensate for the arrival of children in the household, especially at the first and second childbirth. Women should incur a loss of experience following births because of mandatory maternity leave (8 weeks, which amounts to approximately -0.15 year of FT experience, but almost all mothers take the entire maternity leave of 16 weeks they are allowed to, hence -0.3 year of FT experience.) but they might compensate for this penalty in the long run by working more. Interestingly, after the first birth, mothers tend to work slightly more than non-mothers, on average by 0.2 year: in the long run, and accounting for the gender asymmetry of -0.3 years, this yields the same level as men ($+0.5$ year at first birth). Yet, the situation changes dramatically from the second childbirth onwards: even in the long run, mothers tend to spend less time on the labor market than non-mothers (-2.6 years at fourth childbirth). Indeed, the duration of the potential maternity leave is higher for subsequent children: 26 weeks for third-born children (or more), which explains most of the results found here. The substitution with part-time activity does allow the women to compensate for this difference: part-time experience increases little, by $+0.3$ year, after the third childbirth. Hence, labor supply decisions within households following parenthood may still be strongly biased in favor of men pursuing their labor market

activity while women reduce theirs. These results confirm the hypothesis of gender biases and suggest that remaining out of the workforce remains an option for women after the third childbirth. The parental leave might help explain those findings: it can have a total duration of 3 years, be possibly combined with part-time work and it is clearly a labor supply decision taken at the end of the maternity leave. Unfortunately, the data is not rich enough to identify such career interruptions without dealing with measurement error issues.

On top of the youth of individuals, another limitation of the present analysis has to do with the absence of civil servants, which makes my empirics conditional on choosing to work in the private sector. In particular, since Nielsen et al. (2004) have documented that mothers tend to self-select into the public sector that is on average more family-friendly, one might expect that women in my sample have observed or unobserved characteristics that make them less likely to incur high motherhood penalties; women who expected higher child-related penalties would have chosen the public sector. This may result in a downward bias of the motherhood penalty (in absolute terms).

4 Econometric specification

4.1 Worker fixed effects

The literature devoted to the parenthood pay gap has thus far focused on the estimation of Mincerian wage equations using panel data at the individual-year level. The typical dependent variable in such estimates is the logarithm of the hourly wage, which is regarded as a proxy for productivity if labor markets are perfectly competitive. Explanatory variables include quadratic specifications of age, experience, seniority, job characteristics (sector, firm size, location), potentially other controls, as well as time and individual fixed effects. In that vein, for individual i of gender $G(i)$ (either woman or man) observed in year t , I specify first:

$$\log W_{it} = X'_{it}\beta_{G(i)} + N'_{jt}\delta + V_t + \theta_i + \epsilon_{it}, \quad (3)$$

where W_{it} is the ratio between the sum of wages received by i on year t and the sum of his worked hours, X_{it} contains part-time activity, marriage and children and quadratic specifications of age, full-time experience, part-time experience, and seniority,¹⁵ N_{jt} includes firm characteristics (size, industry, *département*), and ϵ_{it} is an idiosyncratic error term, the variance of which is allowed to be individual-specific. The primary variables of interest are Childbirth_{itk} , $\forall k = 1, \dots, 5$, which are dummy variables indicating whether individual i had already experienced his k -th childbirth at time t . I allow for gender-specific effects $\beta_{G(i)}$ since the literature

¹⁵These characteristics are attached to an individual's main employment. It does not contain any information on location or distance work-home for instance. Exhaustive information on marital history is also missing.

has documented the existence of such an heterogeneity in marginal returns (for covariates in X). Moreover, N controls for numerous job characteristics that correspond to the characteristics of the individual's main employment (see Appendix A for the definition of main employment) including the firm's size, the *département* where the establishment is located, and its sector of activity. Size is coded with 12 categories, while industry is defined by the first two digits of the NACE classification and has 39 categories, including a "missing" one. Furthermore, 95 *département* dummies account for the spatial dispersion of earnings in metropolitan France. V_t is a year fixed effect that captures the contemporaneous effects affecting earnings (business cycle, macro shocks, etc.), while θ_i is an individual fixed effect that encompasses permanent unobserved heterogeneity including talent, employability, cohort effects, and initial education. The EDP provides me with a schooling variable that indicates the highest degree obtained by an individual. However, in the presence of individual fixed effects, the coefficient of education is identified provided that this variable is time-varying, which is not the case with initial formation. Finally, in the spirit of Waldfoegel (1997, 1998) and Budig and England (2001), I compare a fixed effect estimation with a first-difference estimation of model (3). While the former enables me to recover rather long-run effects, the latter accounts for a short-run effect.

4.2 Worker and firm fixed effects

Despite their qualities, previous models suffer from an omitted variable bias caused by the omission of firm fixed effects, which was first emphasized by Abowd et al. (1999). High-wage workers are expected to match with high-wage firms; more generally, the matching process may allocate specific workers to firms with specific compensation schemes. If the job matching results in important selection effects, previous estimations will suffer from endogeneity bias due to a correlation between explanatory variables such as Childbirth_{itk} , $\forall k = 1, \dots, 5$, and a firm-specific term ψ_j of the error term in (3) that would write as $\psi_j + \epsilon_{ijt}$. Again, ψ_j can recover more than observed firms' characteristics N_{jt} : it is likely to include workplace nurseries, remote work, flexible work hours, Christmas tree, family discounts for leisure activities obtained through works councils, etc. Abowd et al. (2008) explicitly model this omitted variable bias as a function of the covariance between the matrix X and the design matrix of indicator variables for the employer for which individuals work, conditional on the design matrix of individuals' indicator variables. I choose then to exploit the LEED nature of my dataset without aggregating information at the individual-year level. To the best of my knowledge, this paper is the first attempt to address the parenthood pay gap issue at the individual-firm-year level and hence avoids omitted variable bias due to endogenous matching. I specify a model *à la* Abowd et al. (1999) in which I am able to identify firm fixed effects:

$$\log w_{ijt} = x'_{ijt} \beta_{G(i)} + v_t + \theta_i + \psi_{j(i,t)} + \epsilon_{ijt}, \quad (4)$$

where w_{ijt} is the hourly wage earned by individual i working in firm j in year t , x_{ijt} contains part-time activity, marriage, and children, as well as quadratic specifications

of age, full-time experience, part-time experience, and seniority, v_t is a year fixed effect, θ_i is an individual fixed effect, $\psi_{j(i,t)}$ is a firm fixed effect, and ϵ_{ijt} an error term, the variance of which is individual-specific. I do no longer control for location, size, or industry because these covariates correspond to an aggregation of the pure firm effects $\psi_{j(i,t)}$ (Abowd et al. 2008); more precisely, they are an employment-duration weighted average of the firm effects within the *département*/size¹⁶/industry.

4.3 Identification

The identification of the model is discussed in Abowd et al. (1999) and provided in greater details in Abowd et al. (2002). It proceeds from the connectedness properties of the graph formed by individuals (let designate their number by N) and firms (let designate their number by J). Specifically, the data must be partitioned into G mutually exclusive groups of either individuals or firms such that the members of one group cannot have employed—or have been employed by—any member of another group. These G groups are the maximally connected sub-graphs of the entire graph, the vertices of which correspond to the union of the set of persons and the set of firms, while its edges are pairs of firms and persons. For each group g with N_g persons and J_g firms, $N_g - 1$ individual fixed effects and $J_g - 1$ firm fixed effects can be identified such that $N + J - G$ effects can be identified on the whole. The uniqueness of the effects within a group stems from the elimination of one person effect: it can be achieved by setting the group mean to zero as Abowd et al. (2002) suggest. Finally, the exogeneity of shocks with respect to mobility and parenthood is necessary to identify childbirth and firm effects.

The correlation between individual- and firm-fixed effects has been highly discussed in the literature. In particular, many papers conclude on French, Danish, or US data that this correlation is either null or negative, which is hard to interpret. *De facto*, I find a negative correlation of about -0.2 between θ and ψ . However, as Eeckhout and Kircher (2011) show, neither the sign nor the strength of assortative matching between firms and workers can be derived from this correlation.

4.4 Estimation

Technical details regarding the estimation of two-way high-dimensional fixed effects are provided in Abowd et al. (2002); in particular, one practical solution to cope with the inversion of large matrices consists in exploiting their sparsity. Efficient algorithms include the conjugate gradient and the “zigzag” Gauss-Seidel routine.

In practice, I estimate a unique model for both women and men and I allow for heterogeneous, gender-specific effects of covariates (firm-specific covariates and time dummies aside). First, it increases the set of observations in my sample and I need that a woman and a man working in the same firm belong to the same set to better estimate firm fixed effects, which would not be possible by considering women and

¹⁶In an abuse of terminology, size refers to all firms with a size belonging to one of the 12 previously mentioned size categories.

men separately. Second, firm-fixed effects need to be identical regardless of gender, which would not be the case if I estimated two submodels for women and for men.¹⁷ Relatively little attention has been devoted to men, and *a fortiori* to both genders simultaneously, which is another dimension in which this paper contributes to the literature.

5 Results

5.1 Testing for endogenous matching

The main results are displayed in Table 5, column 3, which reports the estimates from the model including individual- and firm-fixed effects (2FE) with $G = 8503$ groups. I estimate three different specifications: first-difference (FD) in column 1, individual fixed effects (FE) in column 2, as well as individual and firm fixed effects in column 3. As Table 6 shows, I obtain diminishing marginal returns of age, seniority, and full-time experience for both genders. I also find empirical evidence of a marital premium, which has been well documented in the literature; interestingly, this premium seems only slightly higher for men than it is for women. The part-time premium on the hourly wage which has been documented previously still holds under the 2FE specification.

Overall, and in line with previous findings, the estimations suggest the existence of a parenthood wage penalty for French women working in the private sector, of approximately -2.2% per child. As argued before, in the absence of data on the public sector, this penalty is likely to be underestimated. No significant effect is obtained for French men: in particular, I observe no fatherhood premium. The motherhood penalty exhibits some non-linearity with the rank of birth: -4.7% for the first child, -7.0% for the second child, -7.5% for the third child, and -8.5% for the fourth child. Estimates corresponding to the fifth childbirth are much more imprecise due to the low sample size. These results argue for the existence of gender bias in the relationship between children and wages. They are consistent with heterogeneity in childbirth returns (the sample comprises young individuals) and with long-run or dynamic effects of motherhood. By contrast, the FD approach tends to document a short-run effect; interestingly, the short-run motherhood penalty is systematically higher (in absolute terms) than the long-run loss measured by FE or to 2FE methods.

However, neither the FD nor FE estimates correct for the possibility of poorer job matches for parents than for non-parents, which 2FE does, and which may help explain part of the observed wage differential. The comparison of column 2 with column 3 measures precisely the share of the gap that is due to endogenous worker-firm matching and to the corresponding omitted variable bias. However, the entire motherhood penalty remains. By contrast, for men, the FE specification leads to some (small) penalties, while no significant effect of childbirth is obtained under the 2FE specification. As a result, the “firm matching” explanation is rejected in the case of

¹⁷I thank a referee for this suggestion.

Table 5 Log hourly wages (part 1)

		(1)	(2)	(3)
		FD	FE	2FE
Women	First childbirth	-0.057*** (0.004)	-0.047*** (0.004)	-0.048*** (0.005)
	Second childbirth	-0.095*** (0.005)	-0.070*** (0.006)	-0.073*** (0.008)
	Third childbirth	-0.107*** (0.008)	-0.081*** (0.011)	-0.078*** (0.014)
	Fourth childbirth	-0.161*** (0.016)	-0.099*** (0.020)	-0.089** (0.028)
	Fifth childbirth	-0.107*** (0.022)	-0.047 (0.051)	-0.080 (0.054)
Men	First childbirth	-0.005 (0.003)	-0.006 (0.004)	0.002 (0.005)
	Second childbirth	-0.007 (0.005)	-0.019** (0.006)	-0.002 (0.008)
	Third childbirth	-0.012 (0.008)	-0.039*** (0.011)	-0.008 (0.013)
	Fourth childbirth	-0.007 (0.015)	-0.014 (0.021)	0.002 (0.028)
	Fifth childbirth	-0.086** (0.033)	-0.087* (0.035)	0.015 (0.047)
Year dummies	No	Yes	Yes	
Individual effects	No	Yes	Yes	
Industry dummies	Yes	Yes	No	
Regional dummies	Yes	Yes	No	
Firm size controls	Yes	Yes	No	
Firm effects	No	No	Yes	
Observations	144,068	212,189	301,079	
Nb. individuals	33,733	41,531	41,531	
Nb. firms	44,081	62,361	84,318	
R^2	0.250	0.710	0.823	

Clustered standard errors at the individual level in parentheses

Industry dummies: 39 two-digit dummies (NACE)

Firm size controls: 12 dummies

women, while it seems to matter for men. To go further, I report several estimates of the childbirth coefficients in Tables 7 and 8, which correspond to different specifications of Eqs. 3 and 4. The coefficient of the third childbirth is nearly doubled when one fails to control for experience and for firm fixed effects. More generally,

Table 6 Log hourly wages (part 2)

		(1)	(2)	(3)
		FD	FE	2FE
Women	Age	0.048*** (0.002)	0.045*** (0.002)	0.015 (0.046)
	Age ² (1e-3)	-0.408*** (0.024)	-0.355*** (0.030)	-0.314*** (0.043)
	FT Experience	0.023*** (0.002)	0.034*** (0.002)	0.034*** (0.003)
	FT Experience ² (1e-3)	-0.533*** (0.151)	-0.977*** (0.159)	-1.023*** (0.196)
	PT Experience	-0.038*** (0.003)	-0.020*** (0.004)	-0.012* (0.005)
	PT Experience ² (1e-3)	3.499*** (0.466)	2.637*** (0.509)	2.016** (0.693)
	Marriage	0.034*** (0.004)	0.028*** (0.005)	0.025*** (0.007)
	Part-time	0.063*** (0.002)	0.057*** (0.002)	0.059*** (0.003)
	Seniority	0.018*** (0.001)	0.014*** (0.001)	0.011*** (0.001)
	Seniority ² (1e-3)	-0.995*** (0.115)	-0.978*** (0.116)	-0.707*** (0.131)
Men	Age	0.057*** (0.001)	0.058*** (0.002)	0.022 (0.046)
	Age ² (1e-3)	-0.531*** (0.023)	-0.582*** (0.031)	-0.459*** (0.045)
	FT Experience	0.019*** (0.002)	0.041*** (0.002)	0.039*** (0.003)
	FT Experience ² (1e-3)	-0.547*** (0.114)	-1.359*** (0.130)	-1.307*** (0.163)
	PT Experience	-0.027*** (0.004)	0.001 (0.007)	-0.010 (0.008)
	PT Experience ² (1e-3)	4.320*** (0.638)	-0.376 (1.362)	2.612 (1.468)
	Marriage	0.054*** (0.004)	0.045*** (0.006)	0.031*** (0.007)
	Part-time	0.066*** (0.002)	0.067*** (0.003)	0.077*** (0.004)
	Seniority	0.025*** (0.001)	0.016*** (0.001)	0.010*** (0.001)
	Seniority ² (1e-3)	-1.576*** (0.106)	-1.032*** (0.135)	-0.659*** (0.156)

Table 7 Coefficients of childbirth in Mincer equations—women

Specification	(1)	(2)	(3a)	(3b)	(3c)	(4)
First childbirth	−0.058*** (0.004)	−0.044*** (0.004)	−0.046*** (0.004)	−0.050*** (0.004)	−0.047*** (0.004)	−0.048*** (0.005)
Second childbirth	−0.080*** (0.006)	−0.081*** (0.007)	−0.087*** (0.007)	−0.079*** (0.007)	−0.070*** (0.007)	−0.073*** (0.008)
Third childbirth	−0.132*** (0.010)	−0.118*** (0.013)	−0.124*** (0.013)	−0.089*** (0.012)	−0.081*** (0.012)	−0.078*** (0.014)
Fourth childbirth	−0.174*** (0.013)	−0.162*** (0.023)	−0.170*** (0.024)	−0.100*** (0.022)	−0.099*** (0.023)	−0.089*** (0.028)
Fifth childbirth	−0.168*** (0.019)	−0.128** (0.065)	−0.136** (0.064)	−0.043 (0.055)	−0.047 (0.057)	−0.080 (0.054)
Observations	212189	212189	212189	212189	212189	301079
R ²	0.347	0.705	0.705	0.709	0.710	0.823

(1) = Pooled OLS with year and cohort dummies, and a quadratic specification in tenure

(2) = FE with year dummies and a quadratic specification in tenure

(3a) = (2) + quadratic specification in potential experience

(3b) = (2) + quadratic specification in experience

(3c) = (2) + quadratic specification in full-time and part-time experience

(4) = 2FE

these tables quantify the role played by each of the explanations in the parenthood gap previously mentioned in the literature.

The first explanation—human capital accumulation proxied by actual experience—determines up to 1/3 of the adjusted motherhood pay gap, especially from the third childbirth onwards (comparison of columns 2 and 3c). However, this result does not hold for men. Controlling for potential experience (column 3a) or failing to distinguish full-time from part-time experience (column 3b) also biases the coefficients of interest. As a *caveat*, these estimates account rather for the absence of human capital accumulation than for human capital depreciation, which could only be observed by looking at the effect of a leave period. However, in my data, I am not able to measure properly leave periods (and *a fortiori* parental leave periods).

The second explanation—individual unobserved heterogeneity—is not the major reason for lower hourly wages after childbirth, for women or men. Columns 1 and 2 exhibit some differences, but they are not significant at 5 % or, if any, their magnitude is economically small.

The third explanation—firm matching—is rejected for women, which constitutes the main result of this paper: comparing estimates from column 3c to those from column 4 does not reveal any significant difference. In other words, women who plan to become mothers would not move to firms which offer particularly lower wages—when they anticipate a pregnancy, immediately after childbirth, or subsequently. It indicates that the parenthood dimension is orthogonal to the job matching process as

Table 8 Coefficients of childbirth in Mincer equations—men

Specification	(1)	(2)	(3a)	(3b)	(3c)	(4)
First childbirth	−0.009* (0.005)	0.003 (0.004)	0.003 (0.005)	−0.006 (0.004)	−0.006 (0.004)	0.002 (0.005)
Second childbirth	0.003 (0.008)	−0.005 (0.007)	−0.005 (0.007)	−0.018** (0.007)	−0.019*** (0.007)	−0.002 (0.008)
Third childbirth	−0.034** (0.017)	−0.027** (0.012)	−0.028** (0.012)	−0.039*** (0.012)	−0.039*** (0.012)	−0.008 (0.013)
Fourth childbirth	0.006 (0.032)	−0.004 (0.024)	−0.005 (0.024)	−0.012 (0.024)	−0.014 (0.023)	0.002 (0.028)
Fifth childbirth	−0.136*** (0.048)	−0.107** (0.045)	−0.107** (0.045)	−0.083** (0.040)	−0.087** (0.039)	0.015 (0.047)
Observations	212,189	212,189	212,189	212,189	212,189	301,079
R^2	0.347	0.705	0.705	0.709	0.710	0.823

(1) = Pooled OLS with year and cohort dummies, and a quadratic specification in tenure

(2) = FE with year dummies and a quadratic specification in tenure

(3a) = (2) + quadratic specification in potential experience

(3b) = (2) + quadratic specification in experience

(3c) = (2) + quadratic specification in full-time and part-time experience

(4) = 2FE

far as women are concerned. The absence of any penalty related to family-friendly firms is rather an unexpected result. In other words, there is a family friendly job effect. On the contrary, there is a small though significant difference for men: failing to control for firm-fixed effects yields small penalties, while after including the latter the effect of childbirth is no longer significant at usual levels. Fathers would tend to move to firms that offer slightly lower compensation, and the negative coefficient in the FE specification would then account for such a spurious correlation.

As the estimates for men show, controlling not only for observed firm characteristics but also for unobserved ones matters, since there is a significant difference between both specifications at usual levels: it must therefore be concluded that firm fixed effects convey some supplementary information with respect to observed job characteristics, and this should not be ignored. There are then several competing, but not mutually exclusive explanations to previous results. The latter cannot be entirely attributed to the lack of data on the public sector: private French firms are heterogeneous enough in terms of family-friendly policies. However, the information that workers dispose on such policies may not be always available to all at the time of making career choices. A poor information level about the degree of firms' family-friendship can help explain previous results. To account for the gender asymmetry documented above, one could think that women choosing work-family reconciliation choose almost all the public sector: due to this gendered selection mechanism, the choice set of family-friendly private firms in which (young) women can work is

therefore mechanically more restricted and/or (young) women working in the private sector are more homogeneous than their male siblings with respect to work-family reconciliation. It is then consistent with finding (young) fathers moving relatively more to low-paying firms than non-fathers while it is not the case for mothers with respect to non-mothers; men may also be increasingly cautious about work-family reconciliation and more prone to incur wage penalties.

Finally, I investigate why there is no fatherhood premium in the data. Although a thorough analysis would be beyond the scope of this paper, I provide here an estimation of the evolution of that fatherhood premium over time. I estimate the same model as Eq. 4 but allow the childbirth coefficient to vary both across genders and over time. Based on the same data set, I consider a longer period (1976–2011) and hence rely on the daily wage instead of the hourly wage as the dependent variable. The corresponding sample of interest is composed of individuals working full-time in the private sector. Figure 1 displays the results, which are consistent both with the fatherhood premium reported in the literature and with my results on the effect of childbirth on men’s wages since 1995. From Fig. 1a, this premium seems to have eroded over time, from roughly 5 % at first childbirth until 1998, to almost zero at the end of the 2000s. Understanding why the fatherhood premium has disappeared is a challenging but rewarding task for applied research in this domain; it requires, however, to disentangle composition effects due to the lower participation of spouses to the labor market, from the childbirth effect. Interestingly, according to Fig. 1b, the evolution of the motherhood’s penalty from 1976 to 2011 exhibits a U-shape (in absolute terms); it sounds like recently the effect of childbirth has started to matter more, once again. There is, however, a *caveat* due to potential selection effects: the composition of full-time workers may have remained rather stable from 1976 to 2011 as far as men are concerned, but this may be less likely in the case of women.

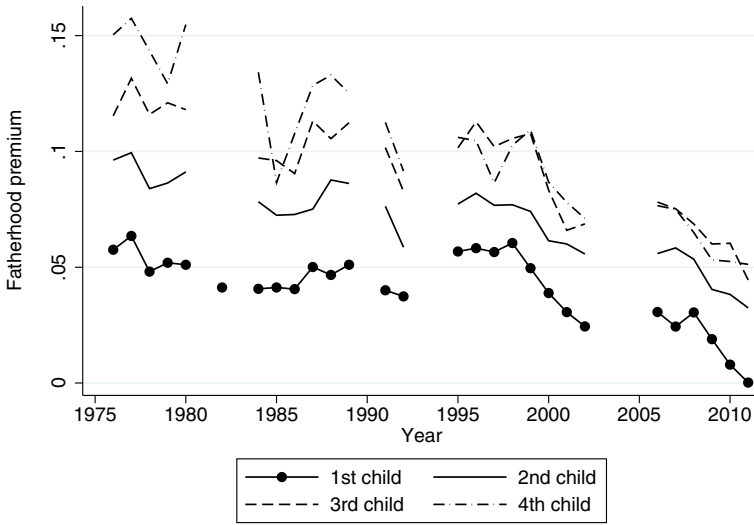
5.2 How much does the parenthood gap contribute to the gender gap?

To evaluate the contribution of this gender-biased parenthood penalty to the gender pay gap, I simulate a counterfactual *scenario* in which women would experience the same childbirth penalty as men, that is, no penalty at all. Public interventions might well consist in promoting paternity leaves, which could reduce or even eliminate such gender inequality with respect to parenthood. From women’s observed wages w_{it}^o , I compute therefore their simulated wages w_{it}^s in the case in which they face no motherhood penalty:

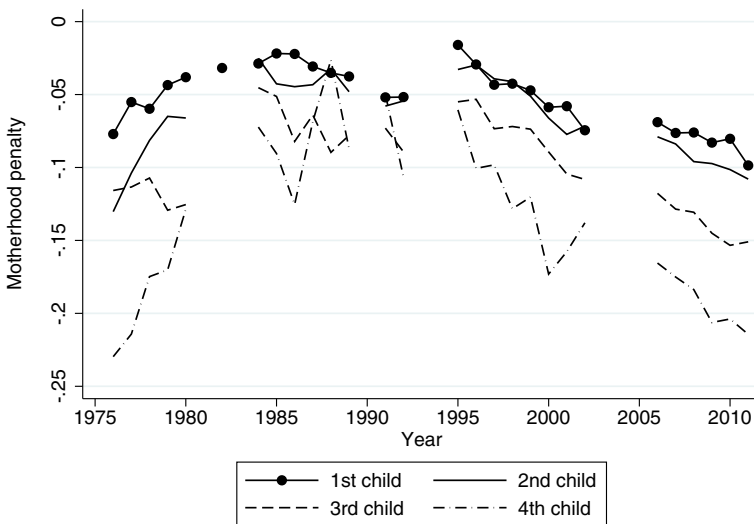
$$\log w_{it}^s = \log w_{it}^o - \sum_{k=1}^5 \beta_{ck}^{\text{Women}} \text{Childbirth}_{itk}. \tag{5}$$

From pooled cross-sectional data, I estimate annual adjusted gender pay gaps Δ_t^o and Δ_t^s on both observed and simulated wages. Denoting by G_i the gender dummy that is equal to 1 if individual i is a woman, I specify $\forall l \in (o, s)$:

$$\log w_{it}^l = \Delta_t^l G_i + X'_{it} \beta_t^l + N'_{jt} \delta_t^l + \epsilon_{it}^l. \tag{6}$$



(a) Fatherhood premium: the erosion

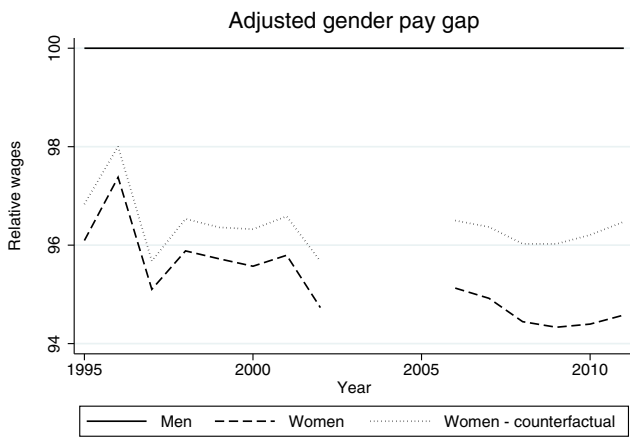


(b) Motherhood penalty: the inverted U-shape

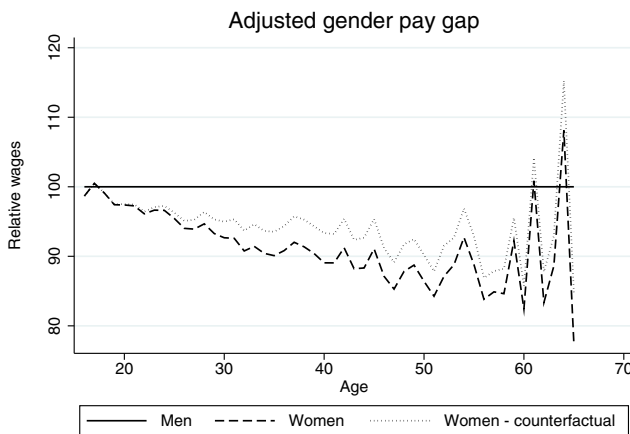
Fig. 1 The evolution of childbirth premia/penalties (daily wage – men, full-time workers, 1976–2011)

Figure 2 depicts the fraction of women’s wages in terms of men’s wages in both observed and counterfactual *scenarios*. Figure 2a displays the corresponding patterns over time, while Fig. 2b plots these patterns against age. First, the sample is composed of individuals aged 26.9 on average, hence for whom the gender gap is rather low. In 2013, the unadjusted French gender gap is almost zero for individuals aged

less than 25. Here, the adjusted gender pay gap varies from 3.5 % at the end of the 1990s to 5.5 % at the beginning of the 2010s. Second, the increase in the gender gap over time that is apparent from Fig. 2a is largely due to a composition effect: in 2011, the sample is mechanically composed of older individuals than in 1995 because of the entry condition. As Fig. 2b shows, older individuals experience a higher gender gap. Third, the sample contains few individuals aged older than 50, which makes the estimates of the gender gap very imprecise at those ages. Nevertheless, the *scenario* in which women do not encounter any motherhood penalty is still far from corresponding to equal wages between men and women. Even in 2011, the parenthood pay gap would explain at most 1/3 of the gender pay gap, even though at higher ages the



(a) Over time



(b) By age

Fig. 2 The counterfactual gender pay gap: what if women experienced the same penalty as men regarding childbirth

former could represent almost one-half of the latter (observe, for example, the result at age 35 in Fig. 2b). Investigating, in greater detail, the contribution of the motherhood penalty to the gender gap at higher ages crucially depends on the availability of appropriate data. To conclude, in this sample, one has both gender inequalities with respect to parenthood and remaining gender wage differentials.

5.3 Robustness checks

I proceed to three robustness checks of the results. First, I examine the sensitivity of the estimates to outliers. I perform several estimations with and without trimming hourly wages. Table 9 displays the corresponding results: column 1 corresponds to no trimming, column 2 corresponds to the elimination of hourly wages below a .8 minimum hourly wage (the base specification), column 3 to the elimination of hourly wages below a 1 minimum hourly wage, and column 4 further imposes a cap at 100 euros following (Felfe 2012). Overall, and although eliminating outliers tends to reduce the estimated loss, I find a limited impact on the motherhood penalty, while the absence of trimming at the bottom of the distribution leads to significant fatherhood wage penalties; no trimming at the top results in close estimates. Combining a quantile approach and two-way high-dimensional fixed effects would be needed here to guarantee further the robustness to outliers under such a specification.

Second, I investigate whether different measures of experience alter the result (Table 10). As argued above, when seriously investigating the parenthood gap issue, it is important to compute the experience covariate as accurately as possible. Resorting to administrative data is an helpful tool that enables me to provide an almost ideal variable with little measurement error. The definition of experience matters: in addition to counting the amount of time spent on-the-job, carefully distinguishing full-time from part-time experience has an impact of the estimated effect of children on wages. Childbirth coefficients differ slightly according to whether one controls for experience as a whole or for both full-time and part-time experience. Potential experience, which is a poor measure of the actual time spent in the workforce, performs worse.¹⁸ Interestingly, the comparison of column 1 with column 4 can be also seen as a test of the explanation for the parenthood pay gap based on human capital accumulation; it is reminiscent of the previous test (based on FE instead of 2FE specification) and leads to identical results.

Third, I check whether the above results are robust to the inclusion of occupational covariates in log hourly wage equations. I am reluctant to control for occupation in wage equations because it is likely to be correlated with unobserved determinants of wages including talent or productivity, hence occupation may be regarded as an endogenous variable. I nevertheless assess whether controlling for such covariates dramatically alters the conclusion, as I found no consensus in the literature on that topic. Table 11 displays the corresponding results and shows that not only do the

¹⁸In the presence of age and individual fixed effects, the slope of potential experience is not identified due to collinearity, as potential experience is defined here as the difference between the current year and the year an individual first appears in the panel.

Table 9 Sensitivity to the trimming of outliers—log hourly wages

		(1)	(2)	(3)	(4)
		2FE	2FE	2FE	2FE
Women	First childbirth	-0.065*** (0.006)	-0.048*** (0.005)	-0.034*** (0.005)	-0.035*** (0.005)
	Second childbirth	-0.104*** (0.009)	-0.073*** (0.008)	-0.053*** (0.008)	-0.053*** (0.008)
	Third childbirth	-0.127*** (0.016)	-0.078*** (0.014)	-0.057*** (0.014)	-0.057*** (0.014)
	Fourth childbirth	-0.142*** (0.033)	-0.089** (0.028)	-0.061* (0.026)	-0.059* (0.026)
	Fifth childbirth	-0.206* (0.092)	-0.080 (0.054)	-0.037 (0.061)	-0.035 (0.060)
Men	First childbirth	-0.012* (0.005)	0.002 (0.005)	0.003 (0.005)	0.003 (0.005)
	Second childbirth	-0.025** (0.009)	-0.002 (0.008)	0.001 (0.007)	0.001 (0.007)
	Third childbirth	-0.040** (0.014)	-0.008 (0.013)	-0.003 (0.013)	-0.005 (0.013)
	Fourth childbirth	-0.029 (0.030)	0.002 (0.028)	0.014 (0.027)	0.016 (0.028)
	Fifth childbirth	-0.027 (0.052)	0.015 (0.047)	0.025 (0.049)	0.040 (0.048)
Year dummies	Yes	Yes	Yes	Yes	
Individual effects	Yes	Yes	Yes	Yes	
Industry dummies	No	No	No	No	
Regional dummies	No	No	No	No	
Firm size controls	No	No	No	No	
Firm effects	Yes	Yes	Yes	Yes	
Observations	317,507	301,079	290,024	289,932	
Nb. individuals	42,513	41,531	40,953	40,950	
R^2	0.829	0.823	0.831	0.833	

Same legend as Table 5

Column (1): no trimming

Column (2): base specification (hourly wage > .8 minimum wage)

Column (3): hourly wage \geq minimum hourly wage

Column (4): hourly wage \in [minimum hourly wage; 100]

signs and significance of childbirth effects remain once occupation (namely dummies defined by the two-digit PCS-ESE French classification) has been controlled for but also their magnitude. There is hardly an attenuation in the FE specification, but no

Table 10 Sensitivity to the specification of experience—log hourly wages

		(1)	(2)	(3)	(4)
		2FE	2FE	2FE	2FE
Women	First childbirth	-0.045*** (0.005)	-0.046*** (0.005)	-0.049*** (0.005)	-0.048*** (0.005)
	Second childbirth	-0.080*** (0.008)	-0.084*** (0.008)	-0.078*** (0.008)	-0.073*** (0.008)
	Third childbirth	-0.107*** (0.014)	-0.113*** (0.014)	-0.081*** (0.014)	-0.078*** (0.014)
	Fourth childbirth	-0.142*** (0.029)	-0.149*** (0.030)	-0.089** (0.028)	-0.089** (0.028)
	Fifth childbirth	-0.147** (0.053)	-0.153** (0.053)	-0.068 (0.052)	-0.080 (0.054)
Men	First childbirth	0.010* (0.005)	0.010* (0.005)	0.002 (0.005)	0.002 (0.005)
	Second childbirth	0.009 (0.008)	0.009 (0.008)	-0.002 (0.008)	-0.002 (0.008)
	Third childbirth	0.005 (0.013)	0.005 (0.014)	-0.007 (0.013)	-0.008 (0.013)
	Fourth childbirth	0.011 (0.028)	0.011 (0.028)	0.003 (0.028)	0.002 (0.028)
	Fifth childbirth	0.008 (0.050)	0.009 (0.050)	0.013 (0.051)	0.015 (0.047)
Year dummies	Yes	Yes	Yes	Yes	
Individual effects	Yes	Yes	Yes	Yes	
Industry dummies	No	No	No	No	
Regional dummies	No	No	No	No	
Firm size controls	No	No	No	No	
Firm effects	Yes	Yes	Yes	Yes	
Observations	301,079	301,079	301,079	301,079	
Nb. individuals	41,531	41,531	41,531	41,531	
R^2	0.820	0.820	0.822	0.823	

Same legend as Table 5

Column (1): Specification without experience

Column (2): Specification with potential experience

Column (3): Specification with experience

Column (4): Specification with full-time/part-time experience

significant difference is observed in the 2FE specification between columns 3 and 4 of Tables 5 and 11.

As documented in the literature, childbirth returns are heterogeneous across individuals and depend on many factors, including the position in the wage distribution,

Table 11 Sensitivity to the inclusion of occupational covariates—log hourly wages

		(1)	(2)	(3)
		FD	FE	2FE
Women	First childbirth	−0.046*** (0.003)	−0.039*** (0.004)	−0.043*** (0.005)
	Second childbirth	−0.071*** (0.005)	−0.054*** (0.005)	−0.065*** (0.007)
	Third childbirth	−0.075*** (0.008)	−0.062*** (0.010)	−0.070*** (0.013)
	Fourth childbirth	−0.117*** (0.015)	−0.071*** (0.019)	−0.077** (0.028)
	Fifth childbirth	−0.069*** (0.020)	−0.008 (0.044)	−0.048 (0.055)
Men	First childbirth	0.000 (0.003)	0.001 (0.003)	0.005 (0.005)
	Second childbirth	0.001 (0.004)	−0.004 (0.006)	0.004 (0.007)
	Third childbirth	0.009 (0.008)	−0.008 (0.010)	0.009 (0.012)
	Fourth childbirth	0.014 (0.014)	0.019 (0.019)	0.026 (0.027)
	Fifth childbirth	−0.056 (0.031)	−0.042 (0.035)	0.049 (0.045)
Year dummies	No	Yes	Yes	
Occupational dummies	Yes	Yes	Yes	
Individual effects	No	Yes	Yes	
Industry dummies	Yes	Yes	No	
Regional dummies	Yes	Yes	No	
Firm size controls	Yes	Yes	No	
Firm effects	No	No	Yes	
Observations	144,068	212,189	301,079	
Nb. individuals	33,733	41,531	41,531	
Nb. firms	44,081	62,361	84,318	
R^2	0.354	0.750	0.837	

Same legend as Table 5

Occupational dummies: 38 two-digit dummies (PCS-ESE)

education, occupation, etc. To focus on the issue of omitted variable bias, I choose here not to investigate further these several dimensions of heterogeneity. Other dimensions may well affect this parenthood gap, such as the presence of unions, the distance between home and workplace, etc.

6 Conclusion

This paper has reexamined the parenthood pay gap by resorting to linked employer-employee data and by controlling for three explanatory factors in wage equations: experience as a proxy for human capital, worker- and firm- fixed effects. It provides a test of the firm matching explanation, according to which endogenous selection of parents into low-wage firms would spuriously explain the parenthood penalty. I estimate a linear model in the presence of two-way high-dimensional fixed effects on a sample of young French individuals working in the private sector from 1995 to 2011. I find a motherhood wage penalty of approximately -2.2% per child on the hourly wage, the effect being more pronounced at the first childbirth. By contrast, fathers do not experience any significant loss following childbirths, but they do not enjoy any premium either. While I reject the firm matching explanation as the main reason for the gender-biased parenthood penalty affecting women, mobility between firms is likely to play a role in the case of men. Moreover, I provide empirical evidence of an erosion of the fatherhood premium in France over the period from 1976 to 2011. Assessing whether these results extend to other countries, older employees or to the public sector sounds like a promising area of future research.

I also find that explanations based on human capital account for some share of gender inequalities in parenthood. The remainder of the inequality could be due to discrimination against mothers at work, which might stem from within-firm labor reallocation: mothers would be less exposed to risky assignments and thus less likely to receive bonuses or even be trapped in low-wage trajectories. A competing explanation could be underinvestment in professional life by mothers; explicitly disentangling the presence of discrimination against mothers at work from this gender-specific behavior is a promising but challenging task that should be developed in future research. However, a gender inequality (either internalized or not) is both unfair and inefficient, which legitimates further public intervention, including campaigns against discrimination, the development of on-the-job childcare, and the development/extension of paternity leave as is the case in Scandinavian countries. Since women are constrained to interrupt their career for 16 weeks at childbirth, a paternity leave of the same duration sounds necessary to force fathers to do the same and to bring down this gender gap. Moreover, the paternity leave would have a causal, positive impact on a less unequal housework division between spouses (especially as regards childcare, see Pailhé et al. 2015).¹⁹

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¹⁹This might also help explain the erosion of the fatherhood premium at the end of the 1990s. The introduction of a short paternity leave (11 days) in France dates to 2002.

Appendix A : data

Cleaning

I proceed to some cleaning of the DADS panel. First, I recode the age variable as the difference between the current year and the year of birth. The former age variable exhibits some errors due to scan problems before the numerical DADS was introduced. Second, *département* codes are sometimes one-digit instead of being two-digit; other *département* or region codes are missing. In that case, I rely on other observations in the whole database in order to recover that information.

In the EDP database, I eliminate observations for which days or months of marriage or birth are equal either to 00 or 99, as well as observations for which the year of birth is 0000.

Selection

I restrict my attention to individuals born on October of even-numbered years: careers of individuals born on October of odd-numbered years is unknown before 2002. The most important selection is dictated by the necessity of measuring experience properly (see *infra*): I focus on individuals who entered the panel after 1995, which leaves me with 46,280 individuals (338,879 observations at the individual-year level and 489,852 observations at the individual-firm-year level). We eliminate further individuals whose net annual earnings are missing or less than 10 euros in 2011 terms. I also restrict my sample to individuals aged 16 to 65, working at least 10 h a year, whose job duration is consistent with worked hours (for instance, the ratio of the latter over the former must be less than 24), which leaves me with 45,483 individuals (317,476 individual-year observations). After trimming observations with a hourly wage that is smaller than 80 % of the legal minimum wage,²⁰ and after dropping years 2003 to 2005, my estimation sample is composed of 41,531 individuals (212,189 individual-year observations and 301,079 individual-firm-year observations). Among those individuals, 19,932 are women while 21,599 are men. Last but not least, I define time-varying variables for marriage (parenthood) as the fact of being married (experiencing a childbirth) before time t for individual i .

In this sample, 62 % (69.5 %) of women (men) do not have any child yet, against 36.3 % (43.9 %) in the DADS-EDP database, the difference being mainly due to the youth of individuals in the estimation sample, and being at the source of the main empirical limit of the current analysis. Among the women in the working sample,²¹ 29.2 % leave after first birth; among the remaining mothers, 34.5 % leave after second birth, this rate being roughly the same for subsequent births. The estimation sample does not seem gender-biased with respect to the DADS-EDP database: for instance,

²⁰I proceed to robustness checks with respect to the 80 % threshold in Section 5.3.

²¹Missing years (1990, 1994, 2003–2005) for which the quality of the data on wages is questionable have been reintegrated into the sample in order to construct meaningful statistics.

54.6 % of non-parents are women in our sample, against 57.5 % in the DADS-EDP; about 47 % of parents of either one child or two children are women in our sample, against about 50 % in the DADS-EDP. Finally, individuals who work in small firms are relatively less likely in the estimation sample: in 2011, 42 % of individuals present in the DADS-EDP and working in the private sector belong to a firm with less than 20 employees, against 33 % in the estimation sample.

Definition of main employment

Aggregating data at the individual-year level requires to define for each individual her main employment in the year. I select the employment with (in successive order) the highest number of working days, the highest wage, a full-time position (if any), and the highest number of worked hours. If there are still ties after applying those criteria, I choose the job with the last SIREN in lexicographical order—to keep the code deterministic. Finally, if several observations resisted to the last iteration, I would consider them as authentic doubles and eliminate them—which does not happen here. We define job characteristics (private/public sector, industry, geographic location, firm's size, full-time/part-time, but also seniority) at the individual-year level as being related to the main employment. I sum wages and working hours, and define working days as the minimum of 360 (the annual number of working days in the DADS by convention) and the sum of working days over the whole year.

Computation of experience

Mincer (1958) demonstrated how important it is to control properly for experience and seniority in wage equations. I devote much attention to compute these variables as precisely as possible. Seniority is defined as the difference between the current date and the first appearance of a pair (individual, firm). Thanks to the comprehensive nature of the DADS panel, it is possible to reconstitute the whole salaried career of an individual, hence to compute his experience from observed working times. Experience will thus be defined as closely as possible as the amount of salaried time spent on the labor market. Since worked hours have been available from 1995 onwards only, I restrict my attention to individuals who entered the panel after 1995. I consider that workers increase their full-time/part-time experience variable every year by their share of working hours expressed in full-time units (FTU).

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