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Effets de la parentalité le long de la distribution des salaires : le rôle des incitations financières

Résumé

Nous relions les pertes de revenu salarial consécutives à l'arrivée d'un enfant pour les femmes à leur position dans la distribution de salaire horaire avant cette naissance. En mobilisant des données administratives françaises couvrant la période 2005-2015, nous montrons que ces pertes de revenu salarial s'atténuent nettement le long de la distribution. Au contraire, les pertes de salaire horaire sont relativement homogènes. Les mères dont les salaires horaires sont les plus bas avant la naissance interrompent leur carrière ou réduisent leurs heures de travail bien plus fréquemment que les autres. L'ampleur de ces décisions d'offre de travail est strictement monotone le long de la distribution de salaire horaire. Ce fait stylisé souligne le rôle des incitations financières à l'activité pour les mères, et suggère que les pertes de revenu salarial dues aux naissances découlent de décisions fondées sur les gains économiques de la spécialisation au sein des ménages, plutôt que de préférences différentes entre femmes et hommes, ou de normes de genre.

Mots-clés : écarts de rémunération femmes-hommes ; parentalité ; offre de travail ; différence-de-différence ; distribution de salaires.

Child Penalties and Financial Incentives: Exploiting Variation along the Wage Distribution

Abstract

We relate women's labor earnings losses due to motherhood to their pre-childbirth rank in the distribution of hourly wages. Using French administrative data from 2005 to 2015, we show these "child penalties" to decrease steeply along the distribution; by contrast, related hourly wage losses are pretty homogeneous. Low-wage mothers opt out or decrease their working hours more frequently; the magnitude of such responses is completely monotone along the distribution. This empirical evidence highlights the relevance of financial incentives and suggests that child penalties arise from decisions based on the gains of specialization, rather than on gender differences in preferences or on gender norms.

Keywords: Gender pay gap; child penalties; labor supply; difference-in-difference; wage distribution.

Classification JEL : J13, J16, J22, J31.

1 Introduction

Recent research has highlighted that women's earnings losses due to motherhood, referred to as child penalties, have become the main driver of gender inequality in the labor market among developed countries (Kleven, Landais, and Søgaard, forthcoming; Juhn and McCue, 2017). From this perspective, the key question is therefore: which channels do generate such child penalties? On the supply side of the labor market, two plausible settings may entail such earnings losses. On the one hand, the arrival of a child is likely to emphasize within-household *specialization* towards the labor market and home production: by increasing the need for home production, and especially childcare, the arrival of a child would make women's comparative advantage in home production more salient, and thus lead to decreases in female labor supply and labor earnings. On the other hand, households can change their time allocation when they have children, either because women have intrinsically stronger taste for childcare (*preferences*), or because households penalize choices that deviate from traditional gender roles that are followed by their peers (*norms*).

Disentangling among specialization, preferences and norms is crucial to the design of public policies that aim at promoting gender equality, because different responses to policy instruments are expected depending on which channel prevails. For instance, usual policy tools which target work disincentives for mothers, either by reforming the allocation of family-related benefits, or by expanding the provision of external childcare are likely to alleviate child penalties, and thus gender inequality in the labor market if the specialization channel dominates. Conversely, if the preferences and norms channel dominates, reforms that aim at changing the way household perceive gender roles, such as the introduction of paternity leaves or/and parental leaves with mandatory splitting, may have a higher impact.

This paper addresses these issues by contrasting the effect of the arrival of a child across groups of workers who face different trade-offs as regards labor market *versus* home production and childcare. Under the specialization hypothesis, the responses of the concerned workers to the arrival of a child should be very heterogeneous, a pattern that the preferences and norms channel is less likely to account for. Specifically, the specialization hypothesis predicts that women with the largest returns to time spent on the labor market are much less likely to decrease their labor supply due to children. The financial incentives related to the labor market *versus* home production and childcare trade-off are therefore driven by the opportunity cost of time, a key parameter in the time-allocation problem faced by households. We propose to approximate this cost by relying on potential hourly wages, which we measure *before* the arrival of a child. As a result, we document here the heterogeneity of the consequences of childbirth along the distribution of pre-childbirth wages.

We consider the short-run (one-year) to medium-run (five-year) consequences on a bunch of labor outcomes: total labor earnings, hourly wages, labor supply at both extensive and intensive margins of employment. Our empirical strategy embeds a difference-in-difference method within a nonparametric ranking of individuals along the hourly wage distribution *à la* Guvenen et al. (2016); the latter framework aims precisely at depicting heterogeneity in individual labor market trajectories along the wage distribution. Our treatment group is made up of parents with n children while our control group is composed of parents with exactly $n-1$ children. We provide additional evidence to support the validity of our approach: first, in terms of identification of child penalties since we show that it is plausibly unaffected neither by measurement error, nor by potential endogeneity of fertility decisions with respect to labor outcomes; second, in terms of reverse causality, so that the correlation found between child penalties and financial incentives likely reflects a causal mechanism –rather than heterogeneity in norms and preferences being a confounder. We implement this method on French administrative data, namely the DADS panel, a comprehensive linked employer-employee dataset[1] from 2005 to 2015 - a peaceful period in terms of policy changes - that contains information on individual's labor earnings and paid hours. This panel is merged with census data from the EDP including longitudinal birth and marriage records at the individual level. Due to the richness of the dataset, we are able to consider such

[1] Filling up the DADS form is mandatory for payroll taxes.

control and treatment groups at fine locations of the hourly wage distribution.

Our main results are the following: (i) high-wage women experience much smaller labor earnings losses due to childbirth than their lower paid counterparts; (ii) they are much less likely to interrupt their careers or reduce their paid hours; (iii) importantly, the magnitude of the latter two effects exhibits monotonic behavior along the hourly wage distribution; (iv) hourly wage losses look rather homogeneous along the pre-childbirth hourly wage distribution. We relate the monotone patterns obtained to the increasing opportunity cost of time spent outside the labor workforce or/and increasing returns of experience along the wage distribution. These results strongly suggest that the specialization channel is at play: mothers whose financial incentives to remain in the labor workforce are the strongest, due to a high hourly wage and hence to a high cost of career interruption are very unlikely to opt out or decrease their working hours. Reversely, those for whom work disincentives are much larger, because their current hourly wage make child benefits that compensate a decrease in labor supply worth hesitating upon (on top of that, at the minimum wage level for instance, career interruptions do not affect much future career prospects) are likely to leave the labor workforce, or at least to decrease their labor supply at the intensive margin.

Overall, by stressing the importance of the specialization channel, as opposed to the role of norms and preferences, these results suggest that public policies aimed at increasing incentives for women to remain in employment after childbirth are instrumental in reducing gender inequality on the labor market. They draw special attention to the financial incentives generated by parental leave allowances: for instance, those studied by Piketty (2005); Lequien (2012); Joseph et al. (2013) provide parents who interrupt their careers or reduce their working hours to take care of their children with a fixed income that does not depend on their pre-childbirth hourly wage. By decreasing the opportunity cost of career interruptions for low-wage mothers, they may lead part of them to get stuck between low labor force participation and low hourly wages. Our results additionally imply that increasing the provision of external childcare and decreasing its cost are likely to be efficient in decreasing gender gaps on the labor market.

Literature This paper is primarily related to the vast literature devoted to the consequences of fertility decisions on labor market outcomes. Childbirths tighten time constraints, shift women's labor supply and labor market outcomes, which helps explain a substantial share of the gender pay gap: see, e.g., the seminal contributions on the "motherhood penalty" by Waldfogel (1995, 1997, 1998). Recent empirical evidence suggests that motherhood not only explains a large part of the gender gap in labor earnings, but also accounts for a growing share of this gap in developed countries (Kleven, Landais, and Søgaard, forthcoming). More generally, childbirths have been found to explain a significant share of the aggregate gender gap, though there is no consensus in how much exactly, or on whether this contribution is increasing over time (Bertrand, Goldin, and Katz, 2010; Wilner, 2016; Adda, Dustmann, and Stevens, 2017; Juhn and McCue, 2017; Kleven, Landais, and Søgaard, forthcoming).

Given these findings, pinning down the channels that generate such child penalties is a key issue. While the existence of some employers exerting discrimination towards mother cannot be dismissed easily, the most likely channels lie with the supply side of the labor market. The most prominent contribution to child penalties plausibly stems from children-induced career interruptions and adjustments in labor supply, which results in human capital depreciation (Meurs, Pailhé, and Ponthieux, 2010; Ejrnæs and Kunze, 2013; Adda, Dustmann, and Stevens, 2017). Others channels involve decrease in work effort (Becker, 1985; Hersch and Stratton, 1997) and mothers having strong demand for time flexibility (Goldin, 2014), which in turn generates compensating wage differentials, or lead mothers to work in family-friendly firms that are likely to exert monopsony power (Coudin, Maillard, and Tô, 2018).

As regards the causes of such decisions, two views can be contrasted. The first builds on Becker (1981)'s model of time allocation based on comparative advantage between the labor market and home production, namely on specialization. The second view, related to preferences and norms, refers to Akerlof and Kranton (2000)'s model of identity and suggests that childbirth enhances the perception of oneself and her spouse as belonging to one gender or another, which distorts households' time-allocation decisions in a sense that is compatible with gender-specific prescriptions.

Disentangling among these two channels is an empirical question. A first strategy requires a structural model of fertility and labor supply in the vein of Adda, Dustmann, and Stevens (2017) –even if the identity channel is absent from their framework. The second relies on policy changes that affect exogeneously either the labor market vs. home production trade-off – to identify the specialization channel – or gender identity – to identify the norms and preferences channel. In Austria, Kleven et al. (2019a) take advantage of various parental leave reforms and childcare expansions, and provide evidence that they do not lead to substantial changes in the long-run consequences of women's fertility decisions; they conclude that the gender norms and preferences are the prominent channel. A last strategy contrasts child penalties across groups of individuals who are exposed to either heterogeneous labor market vs. home production trade-offs, or to different gender prescriptions. On the one hand, Angelov, Johansson, and Lindahl (2016) connect the impact of childbirth on labor earnings to within-couples pre-childbirth gender gap, and stress the specialization channel. Bütikofer, Jensen, and Salvanes (2018)'s comparison of child penalties across occupations among top earners is devised as an empirical assesment of Goldin (2014)'s theory of the contribution of non-linear wage structures to the gender pay gap. On the other hand, Steinhauer (2018), Nix and Andresen (2019) and Kleven et al. (2019b) rely either on heterogeneity across linguistic groups in Switzerland, on differences between same-sex and heterosexual couples or on cross-country comparisons to emphasize the role of gender norms.

All these empirical strategies rely on an evaluation of the causal impact of parenthood on labor outcomes, which requires to overcome the issue of endogeneity of fertility decisions. For instance, Lundberg and Rose (2000) rely on twin sisters and Miller (2011) exploits biological fertility shocks as instruments for age at first birth, few paper. However, Kleven, Landais, and Søgaard (forthcoming) emphasizes that, empirically, correcting for potential endogeneity does not make too much of a difference: the causal effect of third childbirth, estimated thanks to sex-mix instruments, does not differ much from an OLS estimate based on an event-study approach. In this paper, we rely to some extent on this result to advocate for our difference-in-difference strategy. We develop additional tests, especially one based on Huttunen and Kellokumpu (2016)'s investigation of the impact of job displacement on fertility decisions, to show that endogeneous fertility decisions likely do not affect our empirical strategy in ways that would render our approach spurious.

By contrasting child penalties among individuals characterized by their pre-childbirth ranks in the wage distribution, this paper is also related to the few studies that have investigated the distributional impact of the arrival of a child. A small sociological literature is devoted to this question, following Budig and Hodges (2010). Due to methodological issues regarding the interpretation of quantile regression coefficients, it remains however difficult to assess what the main lessons from this literature are (see Killewald and Bearak, 2014; Budig and Hodges, 2014; England et al., 2016). Among economists, Ejrnæs and Kunze (2013) rely on policy changes in Germany to estimate the impact on wages of one additional year spent outside the labor market due to childbirth; they find that these wage losses are far more substantial for the most highly skilled mothers. While this is a close, but different question from the impact of childbirth *per se*, these findings can easily be reconciled with ours.

Lastly, this paper is relevant to the analysis of the heterogeneity of the gender pay gap along the wage distribution (e.g. Albrecht, Björklund, and Vroman, 2003; Arulampalam, Booth, and Bryan, 2007; Gobillon, Meurs, and Roux, 2015). In particular, Fortin, Bell, and Böhm (2017) points out that vertical segregation, i.e. women being underrepresented at the very top of the distribution, can account for a large share of the aggregate gender gap in earnings. Our results suggest that while child penalties may well contribute to this underrepresentation at the top, it is not the main explanation: child penalties are, if anything, smaller at the top of the distribution. Albrecht, Thoursie, and Vroman (2015) argue that the generosity of the Nordic parental leave system makes employers place less women in top positions (cf. statistical discrimination, Phelps, 1972). In a self-confirming belief equilibrium where women go for family-friendly jobs, both vertical segregation and motherhood penalties prevail.[2] A somewhat similar argument is provided by Datta Gupta, Smith, and Verner (2008) who suggest generous parental leave policies may generate a welfare state-based glass ceiling in Nordic countries.

The rest of the paper is organised as follows. Next section presents our data and the institutional setting. In section 3, we describe our empirical approach. Section 4 displays our results, section 5 discusses the validity of our identification strategy, and section 6 concludes.

2 Data and institutional background

2.1 The DADS-EDP panel

Our analysis is based on a large panel of French salaried employees, the longitudinal version of the *Déclarations Annuelles de Données Sociales* (DADS). By law,[3] French firms have to fill in the DADS – an annual form that is the analogue of the W-2 form in the US – for every employee affected by payroll taxes. As of year 2002, the panel contains information on individuals born on January, 2nd to 5th, April, 1st to 4th, July, 1st to 4th and October, 1st to 4th; these (more or less) first four days of each quarter correspond to the birthdays of individuals for whom we dispose of census records on top of labor market characteristics (see *infra*). This panel is therefore a representative sample of the French salaried population at rate 4.4%. Because of the comprehensiveness of the panel with respect to individual's careers, the data is of exceptional quality and has low measurement error in comparison with survey data, on top of a large sample size and no top-coding.

The database contains detailed information about gross and net wages, work days, paid hours, other jobs characteristics (the beginning, the duration and the end of an employment's spell, seniority, part-time employment), firm characteristics (industry, size, region) and individual characteristics (age, gender). We are also able to recover the number of male and female employees working in each

[2] In practice, the Swedish glass ceiling tends to be higher at the top and to increase with age, which is consistent with the previous argument. Moreover, one half of the gender pay gap is present before first childbirth, as if the two explanations were equally important.

[3] The absence of a DADS as well as incorrect or missing answers are punished with fines.

firm, resorting to the cross-sectional version of the DADS for this purpose and exploiting the linked employer-employee dataset (LEED) dimension here. Our main variables of interest are: (i) net real annual labor earnings defined as the sum of all salaried earnings over all employers, (ii) working time measure in paid hours as well as in work days, and (iii) hourly wages defined as the ratio of annual earnings over working time. In Appendix A we provide some further details as to the measurement of earnings and working time. The main point is that, with few exceptions: (i) maternity leave allowances paid by Social Security are not included in our measure of earnings; (ii) the duration of maternity leave in days is taken into account in work days; (iii) working hours during the maternity leave are equal to 0; (iv) working hours (resp. hourly wages) are overestimated (resp. underestimated) for workers that are not paid by the hour for years in which they take maternity leave.

Individuals are identified by their NIR, a social security number with 13 digits that enables the researcher to merge the DADS panel with the *Échantillon démographique permanent*. The latter is a longitudinal version of census that includes births and marriage registers as of year 1968. However, information on childbirth is missing before 2002 for individuals born January, April or July. For this reason, we consider first individuals born October 1st to 4th. Besides, some childbirth-related information is available in administrative birth registers for individuals born October 2nd to 3rd but it was incomplete during the 1990s (see Wilner, 2016, on that topic): as a result, for these individuals we rely on the census rather than on birth records.[4] Finally, partial information on education is available in this dataset (see Charnoz, Coudin, and Gaini, 2011) that indicates the highest degree obtained at the end of studies.

Our working sample is composed of male and female salaried employees working in the private sector at the exclusion of agricultural workers and household employees. We restrict our attention to individuals aged 20 to 60 living in metropolitan France between 2005 and 2015.

The empirical analysis described in Section 3 requires to select individuals with

[4]Appendix B explains how we recover such an information, the quality of which is comparable with that of individuals born October 1st or 4th for whom birth records are available.

a strong attachment to the labor market. We impose that these individuals are employed in the private sector at least two years between $t-5$ and $t-2$ on top of being present in $t-1$.[5] To deal with individuals for which labor participation is very low, an individual is considered employed at t when her paid hours exceed 1/8 of the annual duration of work (1,820 hours as of year 2002), when her total employment duration is higher than 45 days a year and when her hourly wage exceeds 90% of the minimum wage. We also winsorize labor earnings at quantile of order 0.99999, in order to avoid outliers. We drop individuals for which one observation has the ratio of net labor earnings over gross labor earning inferior (resp. superior) to 1/100 of (resp. 100 times) her gross labor earnings. Our working sample has about 1.9 million individuals-years observations, corresponding to nearly 270 000 workers. Appendix C provides with summary statistics on the sample selection process, both in terms of labor outcomes and in terms of fertility decisions.

2.2 Institutional background

Family-friendly policies in France have a long-lasting history (see Rosental, 2010) that dates back at least to pro-natalist concerns during the interwar period (Huss, 1990). These policies rely on (i) tax cuts, especially the *quotient familial* introduced in 1945 and by which the income tax rate depends on the number of children in a household; (ii) various child benefits; (iii) some other welfare benefits, such as bonuses for retirement pensions that depend on realized fertility, or housing allowances. In France, income is taxed jointly within households: this scheme is the source of strong incentives towards within-household specialization.

Maternity leaves were created in 1909, first being unpaid, then being fully covered, up to some threshold, for all salaried workers by social insurance from 1970 onwards. Since 1980, the arrival of the first two children has granted women with a 16 weeks maternity leave, 6 weeks before childbirth and 10 weeks after. From the arrival of the third child, the total duration is 26 weeks (8+18), and

[5]The core results of this paper rely on years t from 2005 to 2015. As a result, because data is only available from 2002, the inclusion condition is slightly stronger for years 2005 and 2006. However, dropping these years and only focusing on years 2007 to 2015 does not change our estimates: see Figures F.9 and F.10.

maternity leave duration may go up to 46 weeks in the case of multiple births. Maternity leaves also come with a minimum duration of 8 weeks, 2 weeks before childbirth and 6 weeks after.

Paternity leaves were enforced in 2002 on top of birth leaves that amount to 3 consecutive days following childbirth. It grants fathers with a 11-day-long leave that is fully covered, up to some threshold, by social insurance. The duration can go up to 18 days in the case of multiple births, but includes weekends and public holidays in any case. The idea of extending that duration has recently received some attention: the French government asked for an internal *ex ante* evaluation but no decision has be taken yet.

On top of these leaves come various parental allowances that were merged in 2004 into the PAJE (*Prestation d'Accueil du Jeune Enfant*). It comprises a one-shot means-tested bonus at childbirth (*prime de naissance*), monthly means-tested benefits (*allocations familiales*), a childcare subsidy (*Complément libre choix du Mode de Garde* or CMG), and some child benefits that are granted when parents interrupt their careers or work part-time (previously *Complément Libre Choix d'Activité* or CLCA and now *Prestation Partagée d'Éducation de l'enfant* or PreParE).

These child benefits date back to 1985 with the creation of the APE (*Allocation Parentale d'Éducation*) that was initially restricted to mothers of 3 children or more. In 1994, the APE was extended to mothers of 2 children , and was replaced in 2004 by the CLCA, from the 1st child onwards , which provides with a fixed amount that is not mean-tested, for a maximal duration of 6 months. Lastly, the CLCA was replaced in 2015 by PreParE to which fathers become eligible; it amounts to roughly €400 per month in case of a career interruption and to nearly €200 for a 80% part-time. Several papers have shown these benefits to induce mothers to decrease their labor supply (Choné, Le Blanc, and Robert-Bobée, 2004; Piketty, 2005; Lequien, 2012; Joseph et al., 2013).

By contrast, other policies favor participation to the labor force by decreasing the cost of childcare, like the CMG, which is not means-tested, payroll tax cuts or income tax credits.[6] It is not straightforward to determine the exact scheme of financial incentives provided by such childcare subsidies because they depend on numerous dimensions (the type of childcare chosen among day nurseries, child-minder and nannies;[7] family structure; geographic location) but they always depend on earnings in a way that makes mothers at the bottom of the wage distribution more likely to stop or reduce their activity (see, e.g., Givord and Marbot, 2015).

As far as labor supply is concerned, the current family insurance scheme provides therefore contradictory incentives: on the one hand, PreParE should decrease labor supply after childbirth; on the other hand, the CMG should preserve it. Determining which effect dominates is an empirical question; the answer to that question depends on the location in the wage distribution. Mothers at the top of the wage distribution won't be too much responsive to PreParE since career interruption or, more likely, part-time employment is particularly costly for them. By contrast, the combination of PreParE benefits (€200) with a reduction of childcare expenditures is worth hesitating for mothers with low wages: for instance, at the minimum wage (slightly above €1,200 per month), a switch to 80% part-time means a wage diminution of about €240, hence a net monetary loss of €40 only. Hence the current system including family allowances and childcare subsidies is more likely to make the "mommy track" all the more attractive than the mother is located at the bottom of the wage distribution.

Lastly, other welfare benefits including bonuses for pensions and housing allowances depend on the number of children, too. Other family-friendly policies may be available within firms, such as employers providing childcare services to their employees. These firm-specific family policies can be subject to further tax reductions or credits, such as the *Crédit d'impôt famille* created in 2004.[8]

[6]The typical tax credit amounts to 50% of childcare expenditures up to some threshold that depends on the type of day care chosen. The annual threshold is €2,300 for childcare providers or wet nurses but as high as €13,500 (€16,500 on the first year) for nannies employed at home.

[7]This very choice depends itself on parents' earnings; affluent households are more likely to opt for nannies while poor households will choose child-minders or day nurseries more often, though there is variation in this respect.

[8]To the best of our knowledge, there is no empirical evaluation of the effect of such policies, to this date.

3 Empirical analysis

Our main outcome of interest is total annual labor earnings of individual i during year t, that we denote as $\tilde{y}_{it}$. We decompose it into four components: d_{it} a dummy for participation, $\tilde{x}_{it}$ the employment duration in days, comprised between 0 and 360[9], $\tilde{h}_{it}$ the average number of paid hours per day during year t, and lastly $\tilde{w}_{it}$ the average hourly wages of individual i during year t. Hence:

$$\tilde{y}_{it} = d_{it}\tilde{x}_{it}\tilde{h}_{it}\tilde{w}_{it} \tag{1}$$

3.1 Normalization

Providing estimates of the causal effect of childbirth by comparing parents and non-parents requires to net out other lifecycle effects as confounding factors: for instance, the number of childbirths an individual has experienced is a nondecreasing function of age. We choose to net out lifecycle and business cycle effects only; many other factors that determine labor outcomes could be adjusted as a response to fertility decisions, so that they should be taken into account as part of child penalties, rather than controlled for. As a result, the first step of our empirical framework derived from that of Guvenen et al. (2016) consists in normalizing earnings and each of its components with respect to age, cohort and period. Let $\tilde{z}$ denote either labor earnings or one of its component, with the exception of the participation dummy. We start by regressing the logarithm of $\tilde{z}_{it}$ on a set of cohort (year of birth), age and period dummies. We estimate the following pooled, cross-sectional regression:

$$\log(\tilde{z}_{it}) = \sum_{c} \lambda_c^z \mathbb{1}_{cohort_i=c} + \sum_{a} \mu_a^z \mathbb{1}_{age_{it}=a} + \sum_{T} \nu_T^z \mathbb{1}_{t=T} + \epsilon_{it}^z \tag{2}$$

The identification of age-period-cohort (APC) models can be achieved at the

[9]The number of days in a year is capped to 360 in the DADS.

cost of normalizations.[10] In this particular paper, the choice of normalization is innocuous given that we rely on the sum $\hat{\lambda}+\hat{\mu}+\hat{\nu}$ and never use these components separately.

Previous estimates enable us to define the normalized component z_{it} as:

$$z_{it} = \frac{\tilde{z}_{it}}{\exp(\hat{\lambda}^z_{cohort_i} + \hat{\mu}^z_{age_{it}} + \hat{\nu}^z_t)} \tag{4}$$

An accounting decomposition similar as (1) stands for normalized earnings:

$$y_{it} = d_{it}x_{it}h_{it}w_{it} \tag{5}$$

3.2 Ranks in the hourly wages distribution

Our empirical strategy embeds a difference-in-difference setting within a framework that aims at depicting heterogeneity in the consequences of childbirth along the hourly wage distribution. For this purpose, we rely on comparisons both within groups of workers with similar hourly wages and across these groups. Hence our analysis relies on the definition of those groups, which is based on a measure of recent hourly wages:

$$W_{i,t-1} = \frac{\sum_{\tau=t-5}^{t-1} d_{i\tau}\tilde{w}_{i\tau}}{\sum_{\tau=t-5}^{t-1} d_{i\tau}\exp(\hat{\lambda}^w_{cohort_i} + \hat{\mu}^w_{age_{i\tau}} + \hat{\nu}^w_\tau)} \tag{6}$$

We compute this measure for individuals who participate in $t-1$ and at least twice between $t-5$ and $t-2$ (i.e., provided that $d_{i,t-1}\sum_{\tau=t-5}^{t-1} d_{i\tau} \geq 3$). Within each

[10] The major threat to the simultaneous identification of λ, μ and ν stems from colinearity between age, cohort and period: age is equal to current period minus year-of-birth. Several solutions have been investigated in the sociological literature, e.g., Mason et al. (1973) who assume that any two ages, periods or cohorts have the same effect, on top of removing one dummy in each dimension. Deaton and Paxson (1994) and Deaton (1997) suggest a transformation of period effects in order to meet two requirements: (i) time effects sum to zero, and (ii) they are orthogonal to a time trend, so that age and cohort effects capture growth while year dummies account for cyclical fluctuations (or business cycle effects) that average to zero over the long-run. Hence the parameters of the model (λ, μ, ν) are identified provided that $\lambda_{\underline{c}} = 0$ and $\sum_{t=1}^{T} \nu_t(t-1) = 0$. The corresponding transformation of time dummies $d_T = \mathbb{1}_{t=T}$ writes as follows:

$$d^*_T = d_T - [(T-1)d_2 - (T-2)d_1] \tag{3}$$

with $d^*_1 = d^*_2 = 0$. In practice, it is convenient to include all age dummies, all cohort dummies but the first, and all transformed dummies d^*_T but d^*_1 and d^*_2 in the regression.

age × year cell, we rank workers according to their recent wages $W_{i,t-1}$. We use this ranking to create 20 cells: P0-P5, P5-P10, ..., P90-P95 and P95-P100. Hence we consider that workers within each age × year × recent wages cell are if not identical, at least *ex ante* similar with respect to their hourly wage levels before year t. Ranks are not conditional on gender: within these cells, men and women have approximately the same recent wages. As a result, women are more (resp. less) numerous at the bottom (resp. top) of the distribution, which merely reflects the existence of a gender gap in hourly wages (see Appendix C).

This depiction of heterogeneity along the wages distribution yields conceptually different estimates than a quantile approach would do. Such approaches would be based, by definition, on the rank in the (potential) outcome distribution, which is not what we want here. There is no particular reason why ranks in the wage distribution would coincide with ranks in the labor supply distribution, unless strong assumptions are made. However, both approaches provide with complement insights: for instance, Albrecht, Thoursie, and Vroman (2015) find that the effect of a parental leave is higher at the top of the distribution.

3.3 Difference-in-difference strategy

Our estimates of the consequences of childbirth are based on a difference-in-difference. The endogeneity of fertility decisions is often regarded as a key issue but recent empirical results suggest that it is not so much of a problem (Kleven, Landais, and Søgaard, forthcoming). We discuss the plausibility of the assumption that fertility decisions are exogeneous, and devise additional tests of its validity in Section 5.

We define N treatments where the nth treatment consists in experiencing nth childbirth during year t. Our control group for nth childbirth is composed of individuals of the same gender with $n-1$ children who never had a nth child. Due to right censoring of the data in 2015, individuals belonging to the nth control group may experience nth childbirth after 2015: we address this issue *infra*. In practice, we restrict our attention to the first three childbirths, which represents 96% of childbirths. Year $t-1$ is taken as the reference year: by construction, all individuals participate in the labor market at $t-1$.

Due to multiple treatments, the same individual may intervene several times in our estimation, though at different dates, either as a member of a treated or a control group. Proper inference has to take this issue into account: we therefore cluster standard errors at the individual level (Bertrand, Duflo, and Mullainathan, 2004).

This difference-in-difference approach is embedded in our ranking along the hourly wage distribution. Our control groups are therefore restricted to individuals with the same rank in the recent hourly wage distribution as our treated individuals. Moreover, the effect of childbirth is allowed to vary along that distribution of recent wages.

We now propose two distinct implementations of this approach.

3.3.1 Accounting framework

First, we rely on an accounting framework to provide estimates of childbirth on labor market outcomes and labor supply. Our estimate of the consequences of nth childbirth on earnings k years after childbirth for individuals of gender g at rank r in the recent wages distribution writes:

$$\beta_{g,r}^{y,n,k} = \underbrace{\log\left(\frac{\mathbb{E}[y_{i,t+k}|b_{it}^{n}=1, r_{it}=r, g_i=g, t\in\mathcal{T}_k]}{\mathbb{E}[y_{i,t-1}|b_{it}^{n}=1, r_{it}=r, g_i=g, t\in\mathcal{T}_k]}\right)}_{\text{Treated}} \\ - \underbrace{\log\left(\frac{\mathbb{E}[y_{i,t+k}|c_{it}^{n}=1, r_{it}=r, g_i=g, t\in\mathcal{T}_k]}{\mathbb{E}[y_{i,t-1}|c_{it}^{n}=1, r_{it}=r, g_i=g, t\in\mathcal{T}_k]}\right)}_{\text{Control}} \tag{7}$$

where b_{it}^{n} is a dummy for experiencing nth childbirth during year t, c_{it}^{n} is a dummy for belonging to the nth control group at time t, i.e., having $n-1$ children at time t but never experiencing nth childbirth according to the data, and $\mathcal{T}_k$ is the set of time periods for which $t-3$ to $t+k$ are observed in the data.

The causal impact of childbirth $\beta_{g,r}^{y,n,k}$ being identified on a subset of time periods that depends on k, we assume that treatment effects are time-homogeneous, i.e., that having a k-year old nth child bears the same consequences if the child was born in 2005 than it does if she was born in 2015. We discuss and assess the plausibility of this assumption, among others, in Section 5. Importantly, considering $k < -1$ allows us to check that trends are parallel before childbirth.

The overall impact of childbirth on the gender gap in pay can be obtained directly as the difference between the impact on men's labor outcomes, and that on women's labor outcomes, both computed by difference-in-difference. It thus writes (omitting the indices y, k, r, z for the sake of clarity):

$$\beta_{\text{gap}} = \beta_f - \beta_m \tag{8}$$

Decomposition (9) states that average normalized earnings growth can be decomposed in a sum of its four components, plus a selection term which arises from the fact that individuals who participate in $t+k$ may not have the exact same past earnings $y_{i,t-1}$ as those who do not participate:

$$\begin{aligned}
\underbrace{\log\left(\frac{\mathbb{E}\left[y_{i,t+k}\right]}{\mathbb{E}[y_{i,t-1}]}\right)}_{\text{Labor earnings changes}} = & \underbrace{\log\left(\mathbb{P}(d_{i,t+k} = 1)\right)}_{\text{Participation}} \\
& + \underbrace{\log\left(\frac{\mathbb{E}[y_{i,t-1}|d_{i,t+k} = 1]}{\mathbb{E}[y_{i,t-1}]}\right)}_{\text{Selection}} \\
& + \underbrace{\log\left(\frac{\mathbb{E}\left[x_{i,t+k}h_{i,t-1}w_{i,t-1}|d_{i,t+k} = 1\right]}{\mathbb{E}\left[x_{i,t-1}h_{i,t-1}w_{i,t-1}|d_{i,t+k} = 1\right]}\right)}_{\text{Employment Duration Changes}} \\
& + \underbrace{\log\left(\frac{\mathbb{E}\left[x_{i,t+k}h_{i,t+k}w_{i,t-1}|d_{i,t+k} = 1\right]}{\mathbb{E}\left[x_{i,t+k}h_{i,t-1}w_{i,t-1}|d_{i,t+k} = 1\right]}\right)}_{\text{Hours Per Day Changes}} \\
& + \underbrace{\log\left(\frac{\mathbb{E}\left[x_{i,t+k}h_{i,t+k}w_{i,t+k}|d_{i,t+k} = 1\right]}{\mathbb{E}\left[x_{i,t+k}h_{i,t+k}w_{i,t-1}|d_{i,t+k} = 1\right]}\right)}_{\text{Hourly Wages Growth}}
\end{aligned} \tag{9}$$

In Appendix D, we clarify the interpretation of this decomposition, showing that it can be rewritten in terms of expected values of changes in labor outcomes, up to some reweighting. This decomposition of labor earnings growth allows us to consider separately each component of the consequences of childbirth on earnings: $\beta^y = \beta^s + \beta^d + \beta^x + \beta^h + \beta^w$ where β^s stands for the selection term and the four others correspond to each component of labor earnings (we omit all other unnecessary indices for the sake of readability). This decomposition is made in an accounting sense. A causal decomposition would require a precise modeling of the causal links between components of labor earnings, such as labor supply being taken on the basis of the wage rate offered in the labor market, and hourly wages depending on past labor supply for instance through human capital accumulation; however, such a modeling is beyond the scope of this paper.

3.3.2 Regression framework

Second, we implement an alternate specification of the same approach. Here, the decomposition of earnings stands at the individual rather than at the aggregate level, and we are able to control for additional covariates to get a better sense of the channels that generate child penalties. Our estimate of the consequences of nth childbirth on earnings k years after childbirth for individuals of gender g at rank r in the recent wages distribution now writes:

$$\begin{aligned} \theta^{y,n,k}_{g,r} = & \underbrace{\mathbb{E}[\log(y_{i,t+k}) - \log(y_{i,t-1}) | b^n_{it} = 1, r_{it} = r, g_i = g, t \in \mathcal{T}_k]}_{\text{Treated}} \\ & - \underbrace{\mathbb{E}[\log(y_{i,t+k}) - \log(y_{i,t-1}) | c^n_{it} = 1, r_{it} = r, g_i = g, t \in \mathcal{T}_k]}_{\text{Control}} \end{aligned} \tag{10}$$

Given a component z[11] of the decomposition (5), we consider its growth between $t-1$ and $t+k$: $\delta^k z_{it} = \log(z_{i,t+k}) - \log(z_{i,t-1})$, which is defined for all individuals working in the private sector on year $t+k$. We estimate the following

[11] when focusing on participation, we use $d_{i,t+k}$ as the outcome. By construction, $d_{i,t-1} = 1$ hence $d_{i,t+k}$ accounts for changes in labor supply at the extensive margin.

$(k+1)$-difference regression by OLS for each component z, gender g, rank r and whatever the duration k (we omit the indices g, k, r, z for the sake of clarity):

$$\delta^k z_{it} = \alpha + \sum_n \gamma^n (b_{it}^n + c_{it}^n) + \sum_n \theta^n b_{it}^n + \zeta X_{it} + u_{it} \tag{11}$$

where X_{it} is a vector of either invariant or time-varying covariates and u_{it} some idiosyncratic error term.[12]

Parameters of interest, namely the treatment effects θ, tell us how parents' outcomes change k years after childbirth with respect to their siblings, i.e., non-parents of the same gender and with similar hourly wages. Interestingly, our approach enables us to recover an impact of childbirth that varies all along the recent wages distribution in a non-parametric fashion.

4 Results

4.1 Heterogeneous consequences of childbirth

First, we assess the consequences of childbirth on men's and women's labor outcomes, relying on the accounting framework. Our estimates of the impact of the first three childbirths on individuals' total labor earnings are depicted by Figure 1 for women and by Figure 2 for men. We plot those estimates for $t+k \in \{t-3, ..., t+5\}$ at the exception of $t-1$ since it is the reference year (our estimates are hence all equal to zero at that date).

Mothers experience large earnings losses after childbirth relative to women who earned similar hourly wages few years before. All components contribute to these losses: after the arrival of a child, mothers are more likely to leave employment,

[12] An alternate specification of the same regression is:

$$\delta^k z_{it} = (\alpha + \alpha_{\text{gap}} g_i) + \sum_n (\gamma^n + \gamma_{\text{gap}}^n g_i)(b_{it}^n + c_{it}^n) + \sum_n (\theta^n + \theta_{\text{gap}}^n g_i) b_{it}^n + (\zeta X_{it} + \zeta_{\text{gap}} X_{it} g_i) X_{it} + u_{it}$$

Here θ^n corresponds to the impact of childbirth on fathers' labor outcomes while θ_{gap}^n gives us information as to how mothers' outcomes shift with respect to those of fathers. Once again, the comparison holds for individuals with similar recent hourly wages, having already controlled for the gendered divergence among non parents: hence it measures directly how childbirths contribute to the gender gap in labor outcomes. To this respect, θ_{gap}^n results from a triple-difference estimation.

work fewer days, work fewer hours per day and earn lower hourly wages than women belonging to our control groups. Nevertheless, in the short to medium run, labor supply decisions seem to be driving these large earnings losses. Moreover, the consequences of childbirth on women's labor outcomes increase, in absolute, with the rank of the child. This empirical evidence is consistent with previous findings in the literature.

More interestingly, children-related earnings losses display substantial heterogeneity: low-wage women experience far larger earnings losses than high-wage women. At the very bottom of the distribution, women's losses amount to 70 log-points the year they first give birth, 37 log-points one year after childbirth, and still 47 log-points 5 years after the arrival of a child.[13] By contrast, women ranked in the top 5% of the hourly wages distribution experience corresponding losses of 22 log-points, 9 log-points and less than 5 log-points respectively. The main result is that child penalties are decreasing along the wage distribution as regards earnings.

The decomposition of annual earnings growth into each of its components helps clarify the channels that most contribute to this pattern. Previous heterogeneity is primarily driven by labor supply decisions at the extensive margin: childbirth reduces by 15 log-points (resp. 60 and 85 log-points) the probability that women are employed one year after the arrival of their first (resp. second and third) child at the bottom of the distribution, but does actually not decrease this probability in the top 5% of the distribution. Once again, one obtains monotonic behavior of labor supply responses in the ranking within the hourly wage distribution: this striking monotony suggests that financial incentives matter, and gives some credit to the specialization channel, as opposed to the preferences and norms channel.

Conversely, while hourly wage losses display a U-shape pattern along the distribution the year of childbirth, which may be driven by some problems regarding the measurement of hours during maternity leaves for workers that are not paid by the hours, which are more numerous in the upper part of the hourly wage distribution (see Subsection 2.1 and Appendix A), those motherhood wages penalties look

[13] By definition and by law, year t mixes both maternity leave and employment periods, see Subsection 2.1.

much more homogeneous one to five years later; they amount to approximately 5 log-points for the first child, and even less for subsequent children.[14]

A nice feature of this approach is that it enables us to verify that trends are parallel between the treated and control groups before treatment, an assumption upon which the difference-in-difference methodology rests. Under this assumption, there should be no difference between treated and control groups before $t-1$. Formally, this assumption is rejected by the data: we find small differences between groups' earnings in $t-3$ and in $t-2$ with respect to $t-1$. The difference is slightly positive (negative) when considering the arrival of the first (second) child: mothers had slightly slower (faster) earnings growth than non-mothers (mothers of one child) prior to first (second) childbirth. However, these differences are less than 10 log-points, which is not much by comparison with earnings differences after childbirth (up to 130 log-points). More importantly, these differences vary little along the wage distribution, which is reinsuring as far as the identification of the heterogeneity of consequences of childbirth on women's labor outcomes is concerned. In Section 5, we nevertheless discuss the credibility of the parallel trend assumption *post-treatment*, which is crucial to our identification strategy.

When it comes to men, our estimates suggest childbirths increase slightly labor earnings, especially through higher participation and hourly wages. The increase in participation is slightly more pronounced for fathers at the top of the wage distribution.

Additionally, Figure 3 displays our estimates of the impact of the first childbirth on the gender gap in labor earnings, participation, hours and hourly wages, for individuals who belong either to the very bottom or the very top of the distribution. These are merely triple-difference estimates, i.e., the difference between the effect estimated for women minus that estimated for men. In particular, it makes it very clear that (i) the gender gap in earnings widens much more at the bottom than it does at the top of the wage distribution; (ii) this pattern is nearly entirely driven by differences in the impact on participation rather than on other components of labor earnings. It remains however difficult to assess whether these differences

[14]Additionnally, this U-shape pattern may be due to the institutional setting: the maternity leave compensation scheme involves various thresholds and depends on its duration.

maintain themselves in the long run, as standard errors become large past the first seven years after the arrival of a first child, due to small sample size.[15]

4.2 Motherhood penalties and fatherhood premias five years after

We now focus solely on the consequences on hourly wages five years after childbirths and rely here on our difference-in-difference approach to compare hourly wage growth between $t-1$ and $t+5$ for individuals who experienced childbirth during year t with that of invididuals belonging to the adequate control group. Our choice to focus on $t+5$ stems from the measure of hourly wages at time t being biased due to the mismeasurement of working hours during maternity leaves, which creates spurious patterns in the data that we do not want to confound with the effect of childbirth (see Subsection 2.1 and Appendix A). Figures 4 and 5 display our estimates for women and men.

The arrival of the first child has a negative, significant impact on women's hourly wages five years after her birth, about -5 log-points for the largest part of the distribution (Model 1). The difference in the effect along a large part of the distribution is not significant: the effect is mostly homogeneous. However, both ends of the distribution figure as exceptions: the consequences of childbirths are slightly less harsh for women that earned either low or high wages before childbirth. Controlling for horizontal segregation (occupation, industry and firm composition) does not alter much the estimates (Model 2). However, controlling for experience, mobility and career interruptions lowers the effect (Model 3), which confirms that post-birth labor supply decisions are a key driver of motherhood penalties, implying less human capital accumulation for instance. A second child does not lead to statistically significant motherhood penalties with respect to mothers of one, but the confidence intervals are large and we cannot reject the hypothesis that it generates economically significant wages losses.

[15] In Appendix E, we investigate the impact on hourly wages in the longer run; we find that hourly wage penalties are plausibly persistent all along the distribution, and may increase over time at the bottom.

In the upper half of the distribution, fathers experience faster hourly wage growth after first childbirth than their counterparts without child (Model 1); this fatherhood premium amounts to 6 log-points at the very top of the distribution. Controlling for horizontal segregation (Model 2) as well as for experience accumulation and job mobility (Model 3) attenuates the estimates, which suggests that faster human capital accumulation due to increased labor supply may be at play. A second childbirth does not generate significant fatherhood premias, at the exception of the very top of the distribution; once again, the confidence intervals are large so that we cannot reject economically significant effects.

The consequences of childbirths on the gender pay gap are estimated by triple difference and displayed by Figure 6. Consistently with previous results, the first childbirth has a significant, negative impact on the gender pay gap: it widens this hourly wage differential for all workers but for those belonging to the lowest part of the distribution. Our estimates suggest that the first childbirth leads to a larger gap among top-earners, up to 8 log-points five years after birth, than among workers with lower wages, for whom the effect amounts to 5 log-points only.

We also document how the effect of covariates varies along the distribution: Figure 7 displays our estimates of the coefficients related to actual experience,[16] career interruptions,[17] job mobility[18] and firm composition[19] in Model 3. Hourly wage growth is much more positively (negatively) correlated with experience (career interruptions) at the top of the wage distribution, which is consistent with Dustmann and Meghir (2005) who find that returns to experience are higher among skilled workers.

These results are consistent with Ejrnæs and Kunze (2013) who find one additional year spent outside the labor market to be much more detrimental to the most highly skilled mothers, or with Adda, Dustmann, and Stevens (2017) who

[16]as opposed to *potential* experience. Experience is computed as the sum of worked hours between t and $t + 5$ divided by the median duration of work for individuals employed full-time one year without interruption (namely 1820 hours).

[17]Career interruptions are proxied by a dummy for spending at least one year between t and $t + 5$ outside employment in the private sector.

[18]Job mobility is measured by a dummy for having different main employers at time $t - 1$ and $t + 5$. A main employer is the firm that pays him the highest labor earnings in a year.

[19]Firm composition is measured by the share of part-time working women among employees of the same firm as i at time $t - 1$.

show that the human capital depreciation due to time spent outside the labor market is much more pronounced in abstract occupations. Our estimates should be interpreted with caution since, as opposed to Ejrnæs and Kunze (2013) who rely on policy changes to identify the effect of exogeneous decisison, here experience and past career interruptions reflect past labor supply decisions that were made based on expected future wages. Nevertheless, they do convey some information. Firstly, this substantial heterogeneity helps rationalize why low earning women do not encounter larger hourly wage penalties, while being more likely to reduce labor supply at the arrival of a child.[20] Secondly, it concurs with the argument based on the opportunity cost of career interruptions (and its heterogeneity along the distribution), which is key to the understanding of mothers' labor supply decisions. Forward-looking mothers base such decisions not only on their current wage, but also on their expected future wage, which is more contingent on current labor supply decisions at the top of the distribution. Moreover, these decisions depend also on the financial incentives provided by maternity leave allowances and childcare subsidies. Overall, the heterogeneity at stake here sounds pretty consistent with mothers adjusting their labor supply according to financial incentives.

Finally, switching from one firm to another coincides with hourly wage gains among workers in the bottom of the distribution and with negative wage growth in the top. Workers from firms that exhibit high shares of part-time working women tend to have slower hourly wage growth, which might indicate that the sorting dimension investigated by Card, Cardoso, and Kline (2016) or by Coudin, Maillard, and Tô (2018) affects hourly wages not only in levels, but also in terms of progressions.

5 Threats to identification

In this section, we address various threats that could affect the empirical validity of our identification strategy. These threats stem firstly from the fact that our

[20] Another institutional explanation for that heterogeneity is merely the high level of the minimum wage in France; as a result, hourly wage losses at the bottom of the distribution can never be too large.

treated and control groups are defined based on realized fertility as observed in 2015, which creates some right-censoring issue; we also investigate other sources of measurement error. A second issue has to do with the endogeneity of fertility decision as regards potential labor outcomes. A last concern is the non-random assignment to pre-childbirth wage groups based on expected children-related labor supply decisions; this last source of endogeneity would not affect our estimates of the heterogeneity of child penalties *per se* but rather their causal interpretation, and thus questions our ability to disentangle the specialization channel from the influence of preferences and norms.

5.1 Right-censoring and measurement error

Altough practical to handle, our definition of control and treatment groups raises some issues. First, due to right-censoring, individuals in our control group are not the same age as those in our treatment group. Second, and for the same reason, our treatment effect estimate corresponds to the difference in labor market outcomes between parents with k children and individuals with $k-1$ children over the lifecycle; this is true for old cohorts but as far as younger cohorts are concerned, our estimate might be spuriously affected by some selection bias, namely differences between parents with k children who experience childbirths quite early and parents who will eventually have k children but later in their lifecycle. Third, the definition of our treatment as experiencing kth childbirth during year t might be blurred by the timing of labor supply decisions, mainly because women are entitled a maternity leave that begins several weeks before childbirth and ends several months after. Choosing the year $t-1$ as a reference for labor market outcomes is therefore likely to generate biases with respect to childbirths that occur at the very beginning of the year, since part of the childbirth effect might already have happened.

We address all three issues by providing several estimations based on alternative definitions of control and/or treatment groups:

1. We define our nth control group as individuals that experience $n-1$ childbirth in the data, taken at an age randomly drawn in the empirical distribution of age at nth childbirth within education × cohort cells. This allows us to assess

robustness with respect to the age difference between control and treatment groups (see Figures F.1 and F.2).

2. We restrict our analysis to individuals born in 1975 or before: such individuals are most likely to have taken all their fertility decisions by year 2015 (see Figures F.3 and F.4).

3. We define our nth control group as individuals who have n children in the data, taken at a time t when they do not experience any childbirth between $t-1$ and $t+k$ (see Figures F.5 and F.6). This strategy is closer to that of Kleven, Landais, and Søgaard (forthcoming) in that it relies on the timing of nth childbirth among those who do have n children.

4. We restrict our nth treatment group to individuals who experience nth childbirth during the second quarter, i.e., between April and June: their maternity leaves do not begin before January and do not end after December (see Figures F.7 and F.8).

Our findings prove robust to these alternative definitions.

Additionally, our measure of the causal impact of childbirth rests on the assumption that treatment effects are time-homogeneous, i.e., that childbirths occurring in 2005 have the same causal impact as those which occurred in 2015 when considered at the same time-distance. This assumption legitimates the fact that our estimates rely on a time-varying window: the impact of childbirth at time t is estimated on all childbirths from 2005 to 2015, while our estimate of the impact of childbirths at time $t+5$ only relies on childbirths that occured before 2011. The credibility of this assumption rests on our choice to focus on a peaceful period in terms of family policy changes: the PAJE reform took place in 2004, and the only other change in parental leave rules, which is a slight change in the incentives to split parental leave between parents, happened in 2015 so that it only concerns a small part of the sample. Nevertheless, we replicate our analysis while restricting to the 2007-2010 childbirths, so that this compositional change does not pollute our estimates of the dynamic treatment effects: all treatment effects for all time-distances to childbirth are computed on the exact same sample. Additionally, by choosing 2007 as the beginning of the estimation time-span, we can abstract from the fact that the selection condition in our sample is harsher for 2005-2006 childbirths, due to left-censoring issues in the data. Figures F.9 and F.10 display our estimates: they show that our approach is completely robust with respect to these concerns.

5.2 Selection into treatment

5.2.1 Comparing treatment effects between subpopulations

In the presence of heterogeneous treatment effects and under the parallel trend assumptions, i.e., assuming that the difference in average *actual* outcomes for the control group between the pre-treatment period and the post-treatment period is equal to the difference in average *potential* outcomes in the absence of treatment for the treated group, this strategy yields the average treatment effect on the treated (ATT). As a result, the estimates provided by such an approach within two subpopulations may differ due to two distinct channels: (i) low-wage women's may suffer from more detrimental consequences of children on the labor market; (ii) high-wage women may suffer from the same detrimental consequences of fertility, but, among them, those who face the largest career costs would choose not to have children. For the second channel to be ruled out requires either to assume that treatment effects are homogeneous within groups, or that treatment effects are mean-independent of actual fertility decisions within groups. In both cases, our approach would yield average treatment effects (ATE) on the whole population, which would ensure that the comparison across wage groups is relevant.

None of these assumptions is testable nor plausible. However, assuming the distribution of treatment effects is constant across wages groups (that is, if treatment effects were heterogeneous, but independent of the pre-childbirth rank in the wage distribution), selection into treatment would imply that high-wage women are much less likely to experience childbirth than their low-wage counterparts. We can therefore assess the plausibility of this assumption by computing the probability to give birth to her nth child during year t among those eligible, i.e., those who already have $n-1$ children in $t-1$, all along the recent wage distribution. Figure 8 shows that among women, the probability to give birth does not vary much along the wage distribution, and that high-wage women are, if anything, in fact more likely to have children than their low-wage counterparts. As a result, this kind of selection into treatment does not seem too much of a problem here.

5.2.2 Endogeneity of fertility decisions

A second kind of selection into treatment could affect our results. Here the problem would not merely to compare ATTs across subgroups rather than ATEs, but related to the very possibility of identifying ATTs (including on the whole population). This could happen if individuals made their fertility decisions based on an unobserved shock common to both potential treated and untreated labor outcomes, and not on the difference between potential treated and untreated labor outcomes, as we investigated before. For instance, if women expected large earnings losses, especially through dismissals or paid hours cuts, to happen in the near future, women would be more likely to have children. This kind of endogeneity has been long investigated by the maternal labor supply literature which has resorted to various instruments to identify the consequences of exogeneous fertility decisions (see e.g. Rosenzweig and Wolpin, 1980; Korenman and Neumark, 1992; Angrist and Evans, 1998). It leads to a violation of the parallel trend assumption post-treatment[21] and in our case it would lead us to inflate the detrimental consequences of children.

In the absence of plausible exogeneous shocks to fertility decisions, there is no simple way to quantify this potential source of bias. However, recent empirical research by Kleven, Landais, and Søgaard (forthcoming) investigates this issue and finds that child penalties estimated through simple event-studies do not differ from those obtained thanks to a sex-mix instrument, as far as the third childbirth is concerned. Additionally, if high-wage women respond to expected future shocks on their labor outcomes the same way as low-wage women do, this source of bias

[21] The parall trend assumption holding before treatment is not sufficient to rule out this possibility.

would be constant along the distribution, and would not affect our claim that child penalties are larger at the bottom of the wage distribution than they are at the top.

On top of these arguments, we provide direct evidence that plausible sources of negative shocks to labor outcomes do not trigger problematic fertility responses. Firstly, we estimate how macro-level shocks on the labor market translate in terms of fertility decisions. We wonder whether the Great Recession has altered the probability of having children. Figure 9 suggests that this is not the case.

Then we document more precisely the effect of macro-shocks. Within the population of eligible individuals, i.e., those with exactly $n-1$ children at $t-1$, we estimate the probability to give birth to their nth children at time t along the business cycle based on a linear probability model:

$$b_{it}^n = \eta^n\{\log(GDP_t) - \log(GDP_{t-1})\} + \kappa_{age_{i,t}}^n + \pi^n t + \xi_{it} \tag{12}$$

where all coefficients are indexed by rank in the recent wage distribution and gender. The η^n coefficients account for the sensitivity of fertility decisions to macro-level shocks. An endogeneity problem would arise if those coefficients were estimated significantly negative, especially at the bottom of the wage distribution. According to Figure 10 and with very few exceptions, this is not the case.

Secondly, we ask whether micro-level shocks tend to generate this kind of fertility responses. We build on Huttunen and Kellokumpu (2016) who show that job displacement triggers negative fertility responses (as opposed to positive fertility responses that would be problematic here). We rely here on the linked-employer-employee nature of our data, which we have not exploited to this point, to identify plausible mass layoff episodes, as frequently done in the job displacement literature (e.g. Jacobson, LaLonde, and Sullivan, 1993). Namely, we consider that individual i is subject to a firm-level shock f_{it} at time t if more than 25% of individuals with the same main employer[22] as i at time $t-1$, but who are not individual i herself, leave the firm at t. First, within each eligible subpopulation, these firm-level shocks

[22] The main employer of an individual is defined as the firm-identifier that pays him the largest earnings for a given year.

do correlate with job losses. We estimate a linear model on the probability l_{it} of being jobless at t:

$$l_{it} = \rho^n f_{it} + \sigma^n_{age_{it},t} + \upsilon_{it}, \tag{13}$$

where all coefficients depend on gender and the rank in the wage distribution. Figure 11 displays the estimates of the ρ^n coefficients, and shows that plausibly exogeneous firm-level shocks do transmit to the individual level. Second, we estimate the probability of having the nth child at time t:

$$b^n_{it} = \phi^n f_{it} + \psi^n_{age_{it},t} + \omega_{it} \tag{14}$$

Figure 12 displays the corresponding estimates of ϕ^n. In most cases, we cannot reject the null hypothesis that ϕ^n coefficients are equal to 0, which suggests that firm-level employment shocks do not trigger positive fertility responses that would render our estimates of child penalties meaningless. Of course, there could be other kinds of unobserved shocks upon which individuals may base their fertility decisions in ways that could bias our estimated child penalties. Nevertheless, neither macro-level nor firm-level negative shocks triggering positive fertility responses supports the credibility of our identification of the consequences of children on labor outcomes.

5.3 Endogeneity of pre-childbirth wages

Even with properly identified child penalties along the pre-childbirth wage distribution, there could still be some doubts with respect to the random assignment of individuals to wage groups. The ideal design to document specialization would be to assign individuals exogeneously to various incentives to remain in the labor force, i.e., to varying wage levels, and to contrast child penalties across those groups. However, pre-childbirth wages reflect individuals' decisions that likely depend on their preferences or on the gender norms they are exposed to. Namely, when considering forward-looking individuals who differ in their unobserved preferences over family and career, an intertemporal human capital accumulation model with two sectors *à la* Becker (1981) predicts that those with higher tastes for family over career will invest less in the acquisition of labor market valued skills, and therefore earn lower hourly wages prior to childbirth. These individuals may for instance choose to work in firms that are more prone to favor family-career conciliation, but also to pay lower wages (Card, Cardoso, and Kline, 2016; Coudin, Maillard, and Tô, 2018). This would generate some reverse causality bias which forbids to interpret the variation of child penalties with the rank in the wage distribution as a causal relationship.

We assess how much that channel is likely to explain our results by interacting our difference-in-difference method not only with the rank in the recent wage distribution, but also with other variables that proxy human capital investment as well as preferences over family and career before childbirth. We consider education, measured by the highest degree obtained at the end of studies, as an 8-level variable, as well as the rank in the distribution of labor supply at the intensive margin, conditionally on age and year, from $t-5$ to $t-1$: to this aim, we compute a measure of average paid hours per day H_{it} in the same spirit as $W_{i,t-1}$:

$$H_{i,t-1} = \frac{\sum_{\tau=t-5}^{t-1} d_{i\tau}\tilde{h}_{i\tau}}{\sum_{\tau=t-5}^{t-1} d_{i\tau}\exp(\hat{\lambda}^h_{cohort_i} + \hat{\mu}^h_{age_{i\tau}} + \hat{\nu}^h_{\tau})} \tag{15}$$

and rank individuals according to this measure within age × year cells in order to group them into 20 bins. Within each cell, we also rank individuals according to the share of female working part-time in the main employer of each individual at time $t-1$, which leads us to consider further 20 firm-composition related groups. In the end, we estimate (omitting the indices g,k and z for the sake of clarity):

$$\delta^k z_{it} = (\bar{\alpha}_{r_{it}}+\bar{\alpha}_m M_{it})+\sum_n(\bar{\gamma}^n_{r_{it}}+\bar{\gamma}^n_m M_{it})(b^n_{it}+c^n_{it})+\sum_n(\bar{\theta}^n_{r_{it}}+\bar{\theta}^n_m M_{it})b^n_{it}+(\bar{\zeta}_{r_{it}}+\bar{\zeta}_m M_{it})X_{it}+u_{it} \tag{16}$$

Here heterogeneity in the $\bar{\beta}^n_r$ stems from variations along the wage distribution of childbirth-related changes in z *within* groups of individuals with similar M, i.e., within groups of individuals with similar education, labor force attachment and firm composition as measured in $t-1$. While this is certainly not sufficient to capture all the variation that arises from different human capital investment and sorting due for instance to heterogeneous preferences over family and career, finding substantial heterogeneity in the $\bar{\theta}_r^n$ along the wage distribution, i.e., with respect to the rank r, can be viewed as suggestive evidence for contemporary hourly wages being a financial incentive that drives much of childbirth-related labor decisions.

We operationalize the approach presented in (16) by considering the probability of remaining in employment one year after childbirth as the outcome. Figure 13 reports the coefficients that depict heterogeneity along the recent wage distribution, first in the case where labor supply decisions are allowed to vary depending on recent hourly wages only (first and second panels), and then in the case where they can also differ depending on education, recent paid hours and firm composition (third and fourth panels).[23]

First, consistently with Figure 1, low-wage women are far more likely to interrupt their careers after the arrival of a child than their high-wage counterparts, when solely interacting our difference-in-difference with the recent wage distribution. The corresponding difference between those at both ends of the distribution amounts to 14 probability points at first chilbirth, 40 points at second childbirth and 43 points at third childbirth.

Second, when labor supply decisions in $t+1$ are allowed to be contingent not only on recent wages, but also on education, past paid hours and firm composition, we still find substantial remaining heterogeneity along the distribution. Within groups of women with similar education, similar paid hours between $t-5$ and $t-1$ and who worked in similar firms with respect to female labor supply in $t-1$, those at the bottom of the distribution are 10 (resp. 29 and 25) log-points less likely to remain in employment one year after the arrival of the first (resp. the second and the third) child than those who are ranked at the top of the distribution. Hence differences in education and full-time status as well as differential sorting account for a pretty low share of the heterogeneity in labor supply responses to childbirth at the extensive margin. While this contribution is not negligible, roughly one third of the initial estimate, it tends to rule out the idea that mothers at the bottom of the wage distribution being more likely to interrupt their careers is primarily

[23] The coefficients related to the heterogeneity along the education dimension, past labor supply decisions and firm composition are shown in Appendix G.

driven by unobserved preferences.

6 Conclusion

This investigation of gender differences in career progressions pays a special attention to the heterogeneity of the effect of children on their parents' labor outcomes along the wage distribution. Childbirths have a large, negative impact on their mothers' labor earnings, but coincide with slightly faster labor earnings growth in the case of high-earnings fathers. In the short to medium run, this is primarily the result of labor supply decisions, and not so much of hourly wages. Moreover, this effect is heterogeneous along the hourly wage distribution: low-wage mothers are more likely to leave the labor market or to reduce their paid hours. Overall, this striking monotony along the wage distribution is consistent with the specialization channel, and with the financial incentives provided by the French family insurance scheme. While intrinsic preferences towards family and career are certainly at play, the observed patterns are likely to be driven by the opportunity cost of career interruptions.

By contrast, the effect of childbirth on their mothers' hourly wages is quite homogeneous along the hourly wage distribution (slightly larger, if any, for mothers with high pre-childbirth wages). Top male earners may also experience a slight fatherhood premium. In the medium run, the gender gap in hourly wages tends to widen more among top earners than among low earners. As a result, the consequences of childbirths contribute to some extent to the women's underrepresentation at the top of the distribution, hence to the glass ceiling. This empirical finding may seem at odds with the fact that career interruptions and labor supply reductions among mothers are less frequent at the top of the distribution. However, we show that both returns to experience and hourly wage losses due to career interruptions are presumably also larger, which both explains why mothers with high pre-childbirth wages would be more reluctant to spend time outside the labor workforce and why them spending less time outside employment has still more serious consequences on their hourly wages. This is consistent both with the view that occupations with better compensations have more non-linear pay structures (Goldin, 2014), so that conciling career and family concerns is more difficult and leads to larger motherhood penalties (Bütikofer, Jensen, and Salvanes, 2018), and with higher human capital depreciation due to time spent outside the labor force for high-skilled workers (Adda, Dustmann, and Stevens, 2017).

Children-related labor supply decisions of mothers being seemingly driven by the economic gains of within-household specialization and financial incentives suggests that reshaping the design of parental leave allowances (Piketty, 2005; Lequien, 2012; Joseph et al., 2013), including childcare subsidies (Givord and Marbot, 2015), would have first-order consequences on their career progressions and therefore on the gender pay gap. Since current parental leave allowances are mainly a lump-sum transfer that does not depend on the hourly wage, mothers with low (potential) wages are more likely to be pulled out of the labor workforce, and therefore to experience lower wage growth; the sticky floor is consistent with this financial scheme in this regard.

As far as childbirths are concerned, another policy instrument includes extended and mandatory paternity leaves: the French ministry in charge of promoting gender equality has recently asked for an *ex ante* evaluation on this topic. A recent working paper by Nix and Andresen (2019) exploiting a large Norwegian reform that expanded access to childcare suggests that subsidized early childcare is more efficient at reducing the child penalty than paternity leaves, which would nevertheless be useful in balancing childcare within the household according to Pailhé, Solaz, and Tô (2018).

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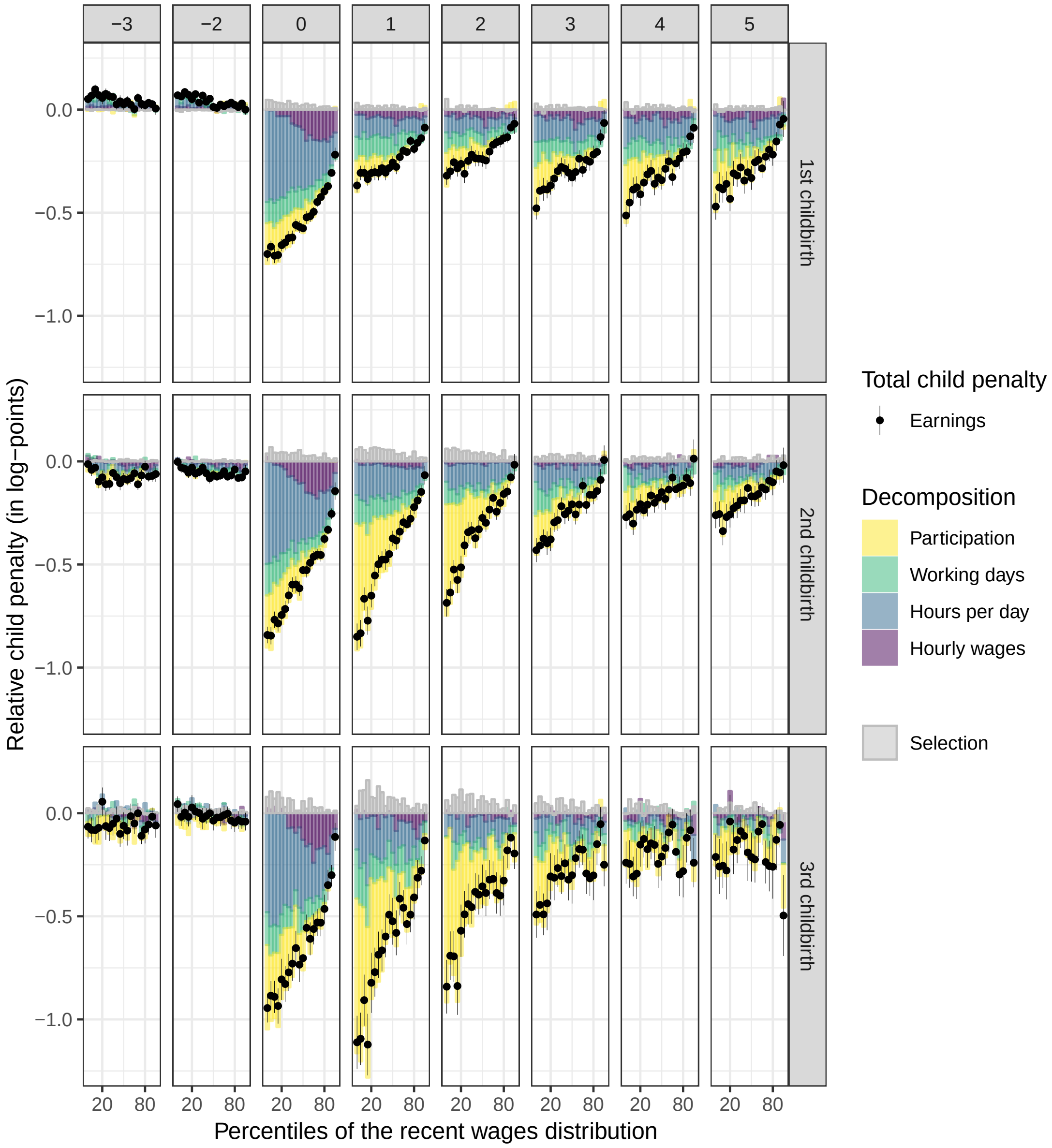
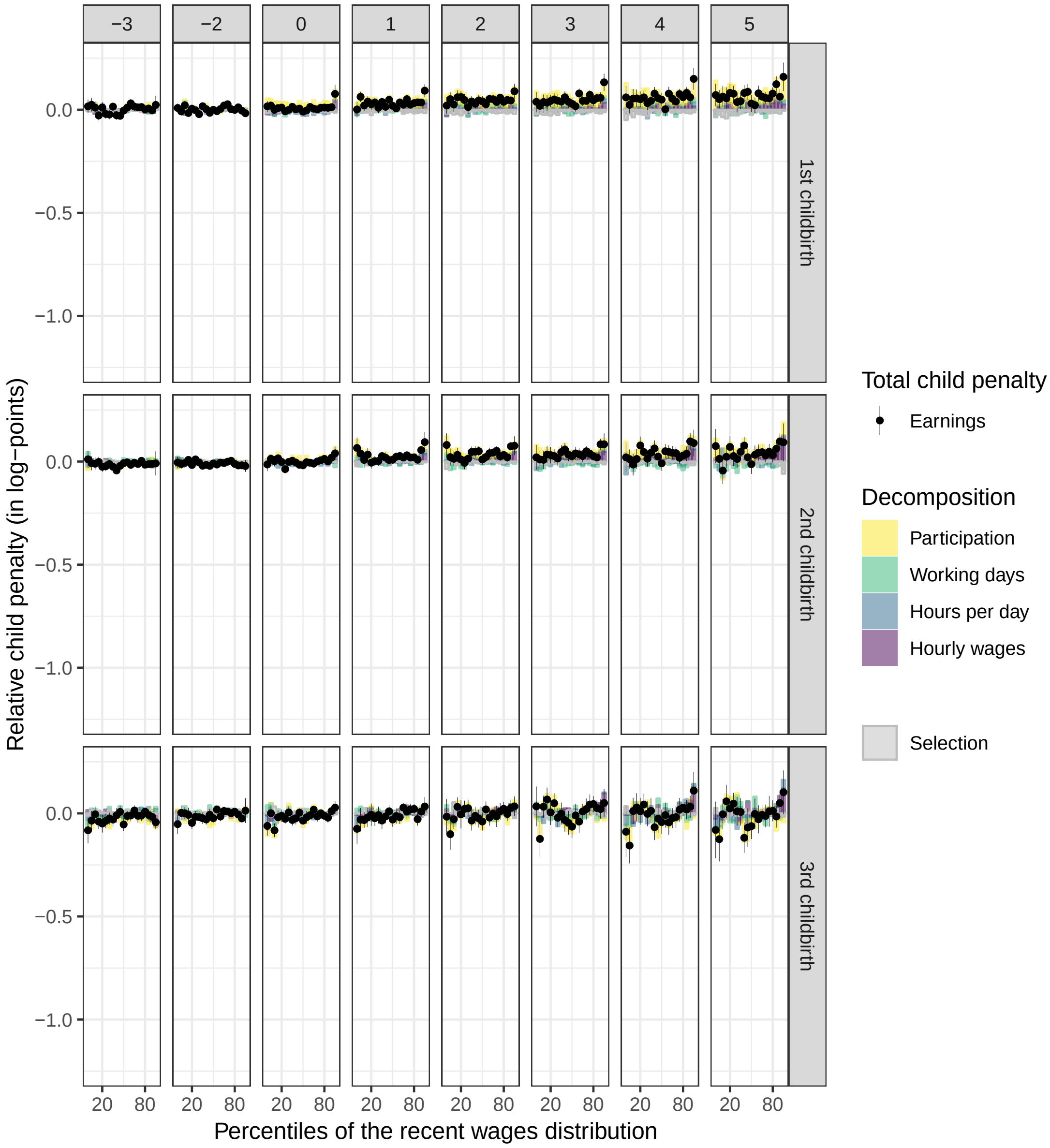
Figures

Figure 1 – Consequences of childbirth on women's labor outcomes



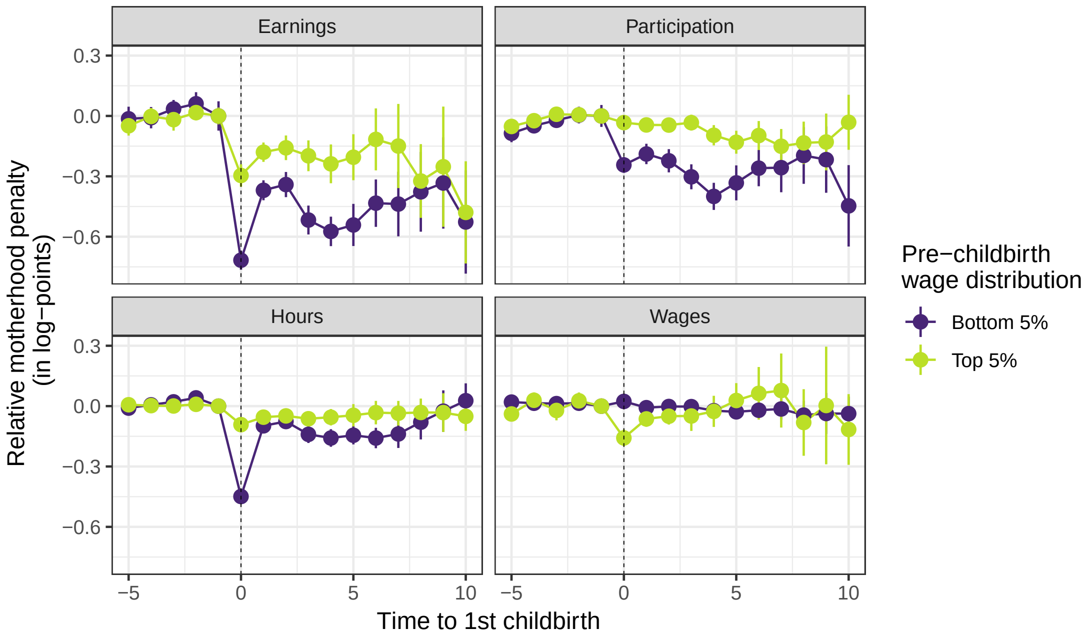
Each panel displays the estimates of child penalties obtained by difference-in-difference (see Equation (7)) for a different time-to-childbirth expressed in years. Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure 2 – Consequences of childbirth on men's labor outcomes



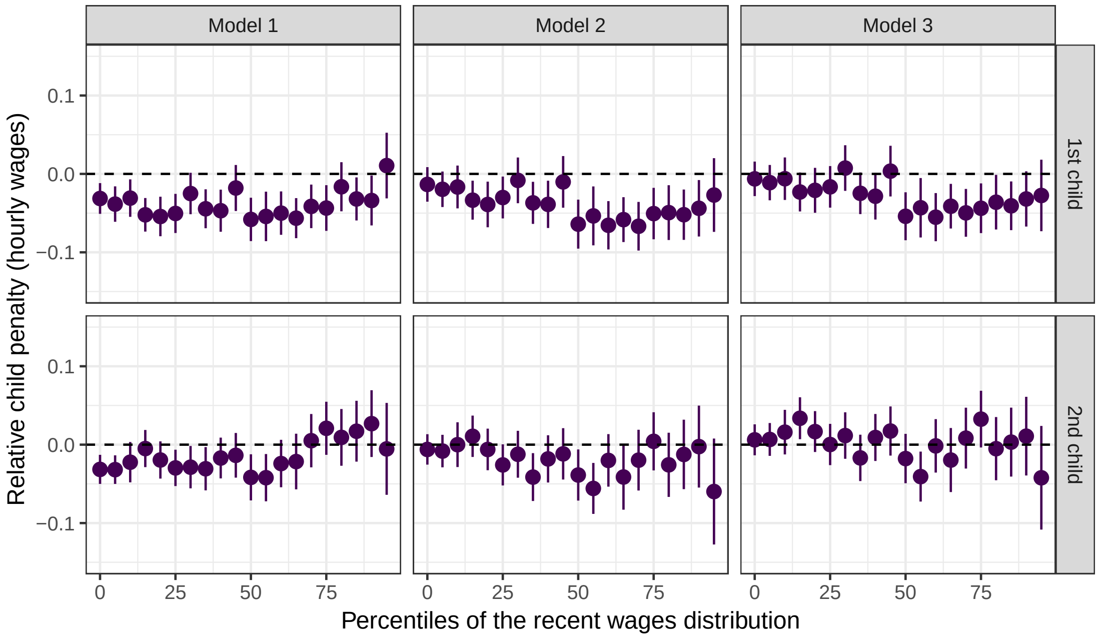
Each panel displays the estimates of child penalties obtained by difference-in-difference (see Equation (7)) for a different time-to-childbirth expressed in years. Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure 3 – Consequences of first childbirth on the gender gap in earnings and labor outcomes



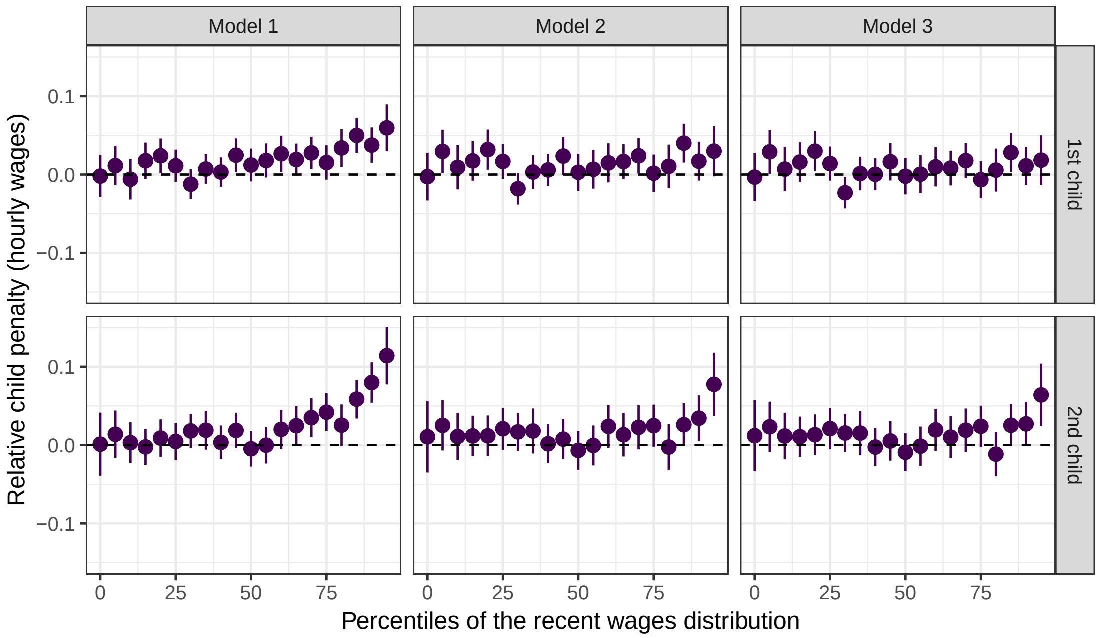
Estimates of the effect of 1st childbirth on the gender gap in pay, obtained by difference-in-difference-in-difference (see Equation (8)). Standard errors are clustered at the individual level and computed by bootstrap (100 replicates).

Figure 4 – Medium-run consequences of childbirth on hourly wages (women)



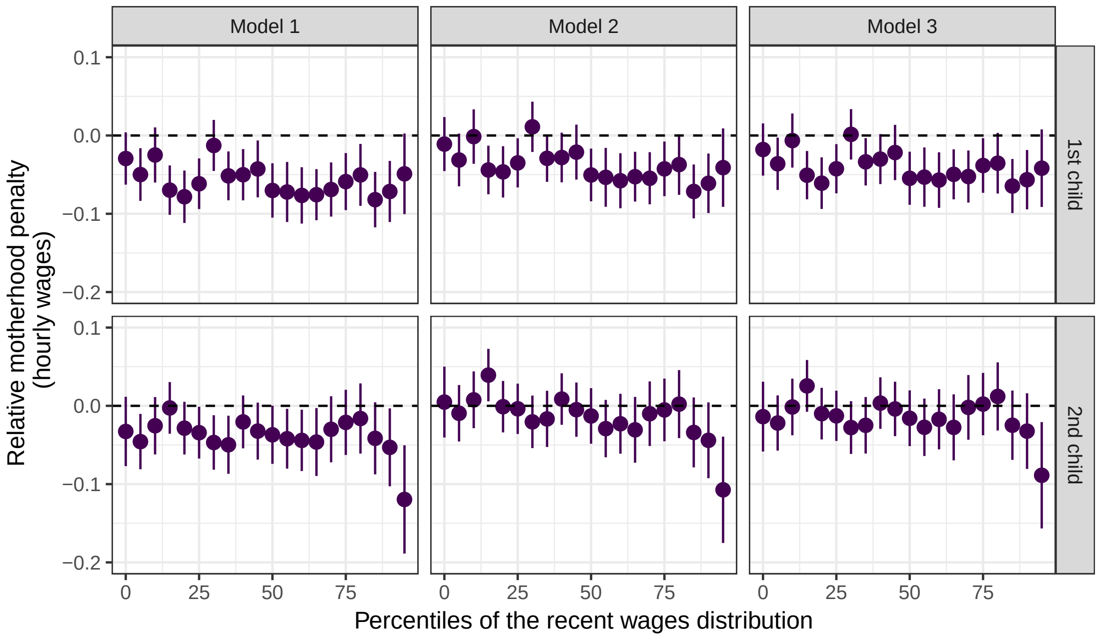
Estimates of the coefficients related to childbirth for women in hourly wages growth model that interacts a double-difference setting with gender and rank in the recent wages distribution (11). Outcome is a (log) hourly wages growth between $t-1$ and $t+5$. Model 1 includes not controls. Model 2 controls for year, age, industry, firm composition (share of part-time working women) and 1-digit occupation within each gender × recent wages cell. Model 3 includes all these controls plus experience between t and $t+5$, a dummy for having spent at least one year outside private sector employment, and having changed firm between $t-1$ and $t+5$. Standard errors are clustered at the individual level. Sample includes individuals up to age 55 at time t.

Figure 5 – Medium-run consequences of childbirth on hourly wages (men)



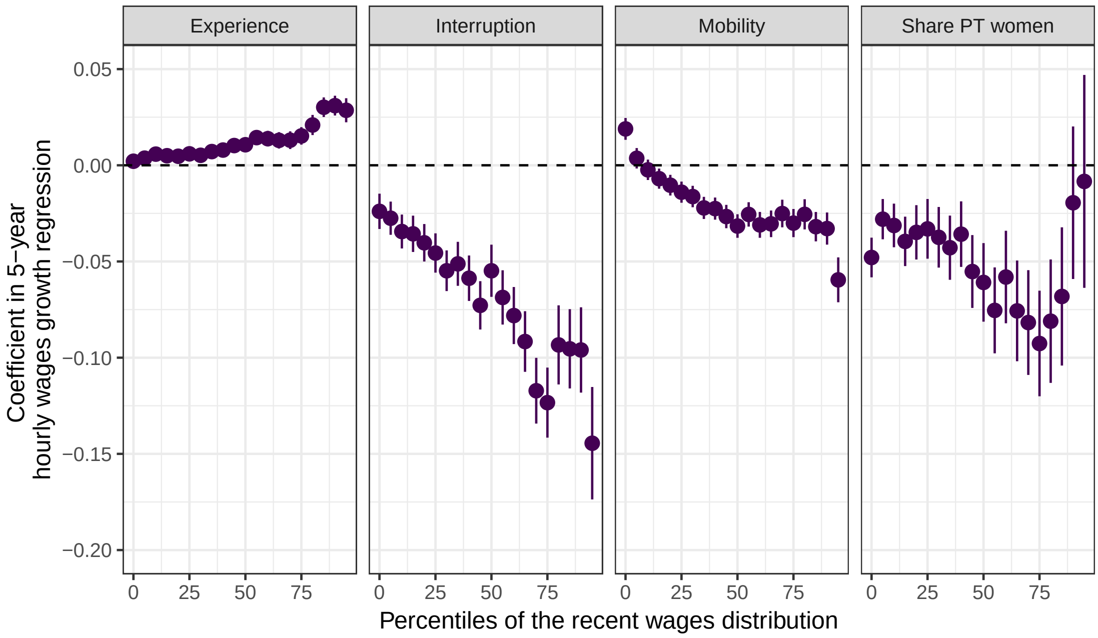
Estimates of the coefficients related to childbirth for men in hourly wages growth model that interacts a double-difference setting with gender and rank in the recent wages distribution (11). Outcome is a (log) hourly wages growth between $t-1$ and $t+5$. Model 1 includes not controls. Model 2 controls for year, age, industry, firm composition (share of part-time working women) and 1-digit occupation within each gender × recent wages cell. Model 3 includes all these controls plus experience between t and $t+5$, a dummy for having spent at least one year outside private sector employment, and having changed firm between $t-1$ and $t+5$. Standard errors are clustered at the individual level. Sample includes individuals up to age 55 at time t.

Figure 6 – Medium-run consequences of childbirth on hourly wages (gender gap)



Estimates of the coefficients related to childbirth for women (men taken as a reference) in hourly wages growth model that interacts a double-difference setting with gender and rank in the recent wages distribution (12). Outcome is a (log) hourly wages growth between $t-1$ and $t+5$. Model 1 includes not controls. Model 2 controls for year, age, industry, firm composition (share of part-time working women) and 1-digit occupation within each recent wages cell. Model 3 includes all these controls plus experience between t and $t+5$, a dummy for having spent at least one year outside private sector employment, and having changed firm between $t-1$ and $t+5$. Standard errors are clustered at the individual level. Sample includes individuals up to age 55 at time t.

Figure 7 – Heterogeneity in returns to experience, career interruptions, firm composition and between-firm mobility



Estimates of the coefficients related to experience and career interruptions in hourly wages growth model that interacts a double-difference setting with gender and rank in the recent wages distribution (12). Outcome is a (log) hourly wages growth between $t-1$ and $t+5$. Model controls for year, age, industry, firm composition (share of part-time working women), 1-digit occupation, experience between t and $t+5$, a dummy for having spent at least one year outside private sector employment, and having changed firm between $t-1$ and $t+5$. Standard errors are clustered at the individual level. Sample includes individuals up to age 55 at time t.

Figure 8 – Probability to have children

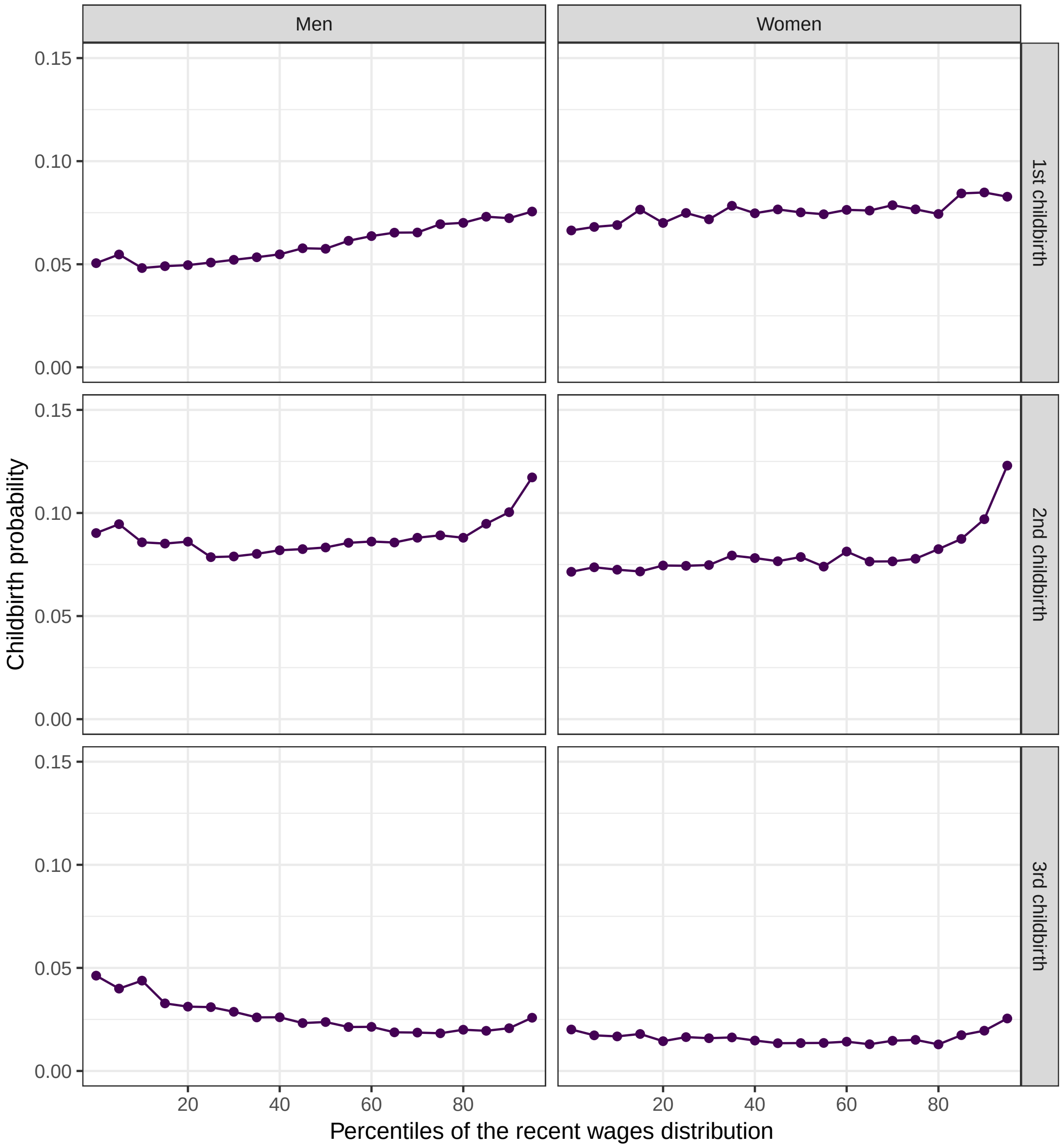


Figure 9 – Probability to have children (by subperiod)

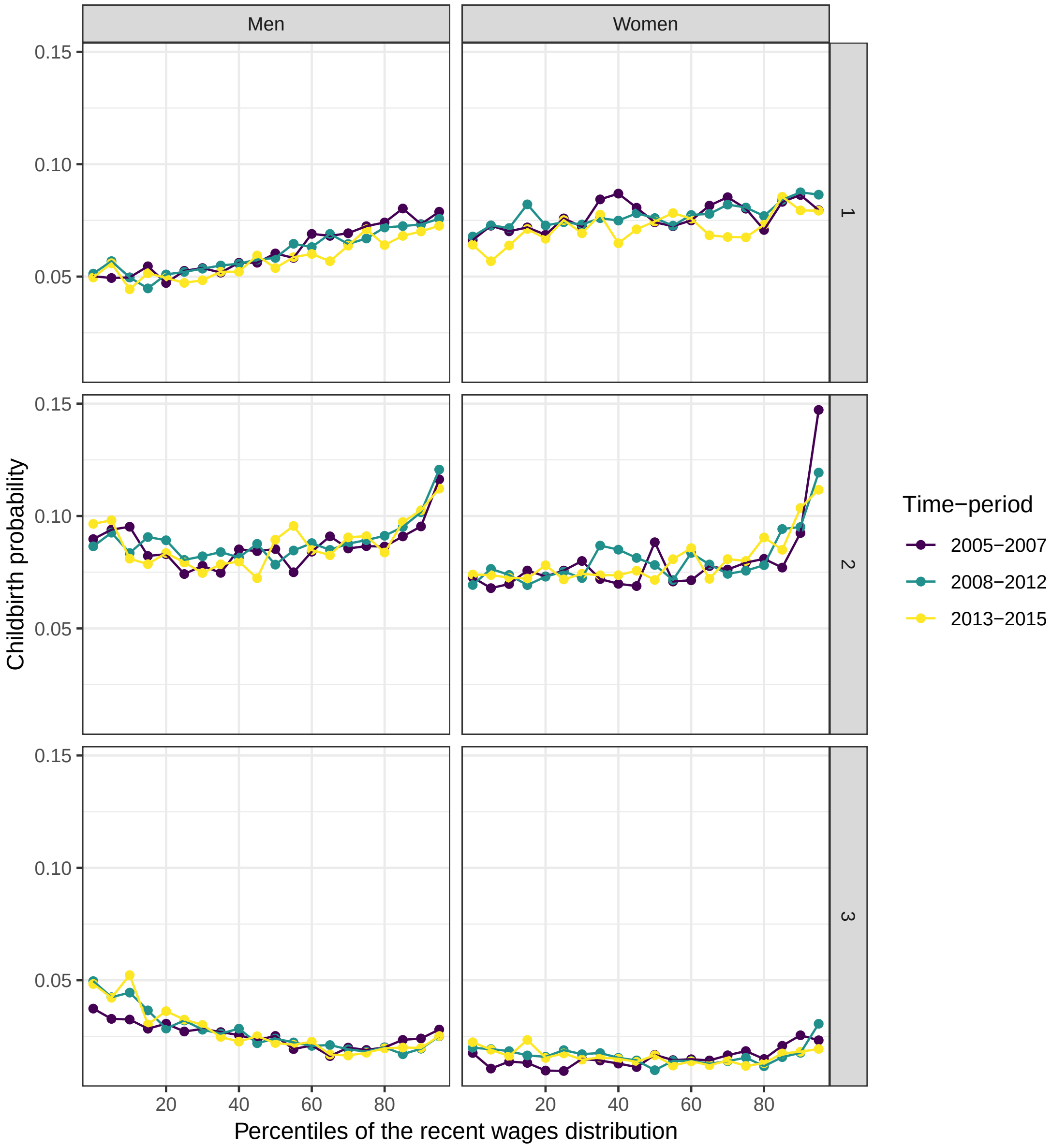
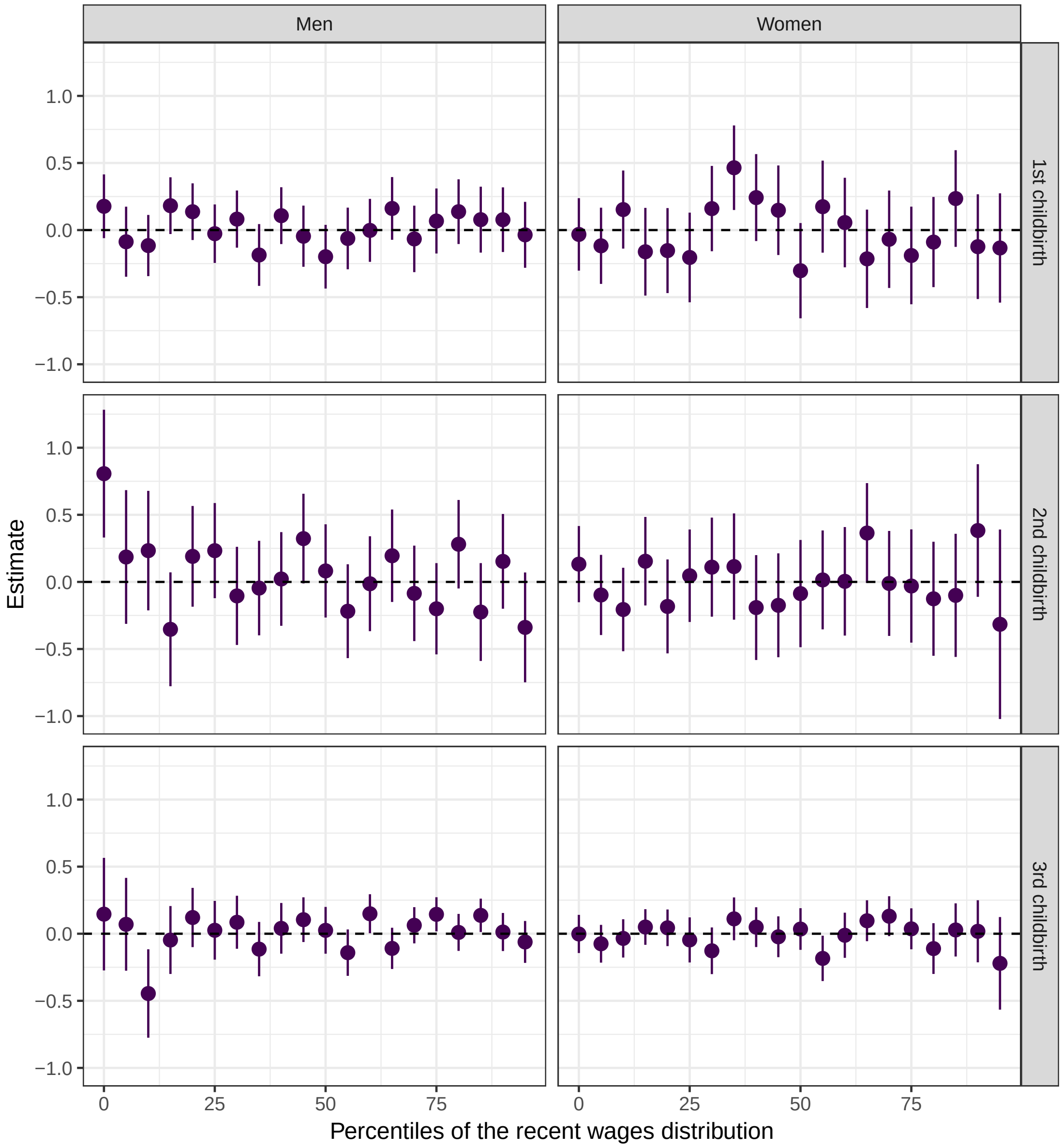
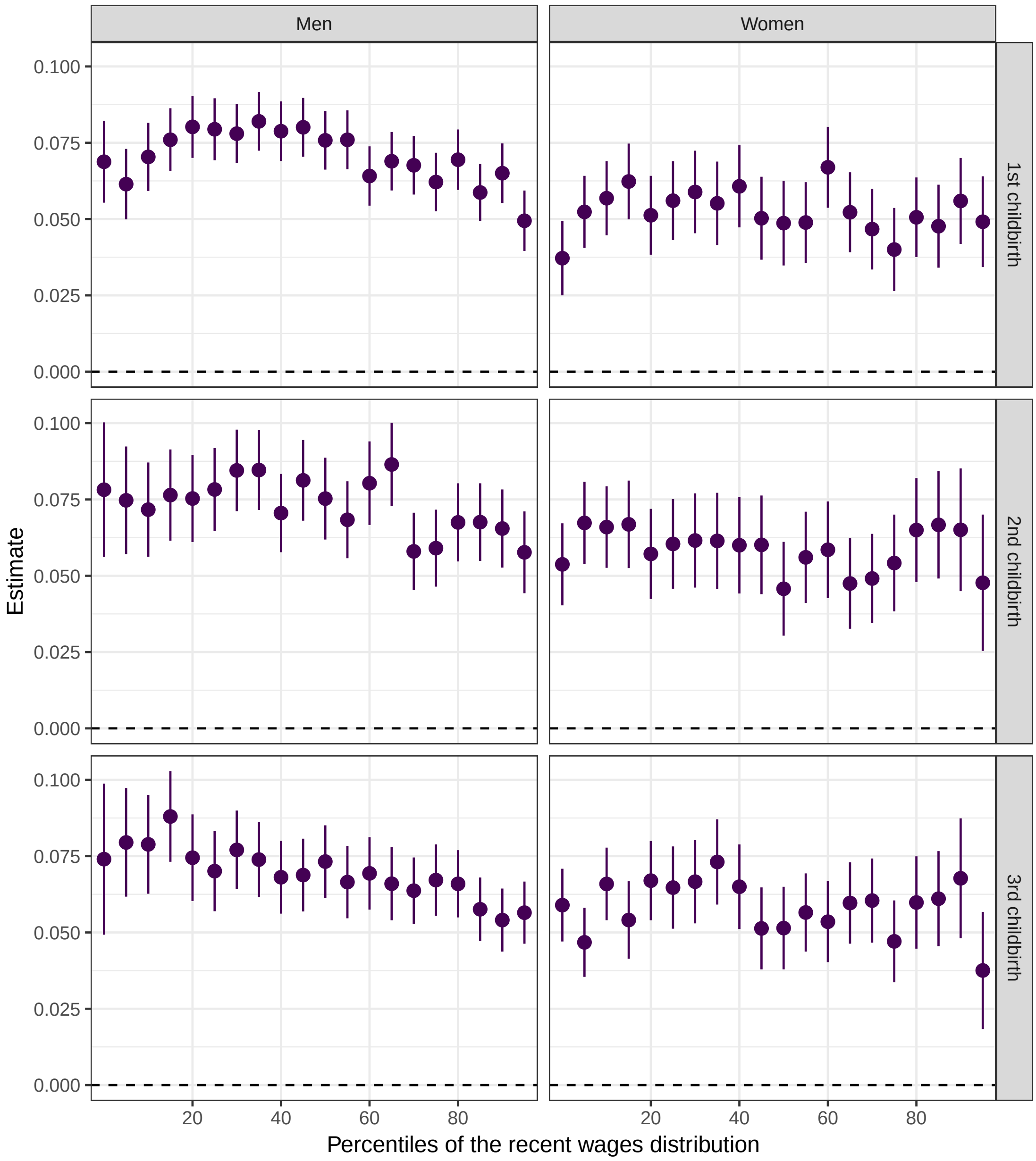


Figure 10 – Probability to have children (sensitivity to the business cycle)



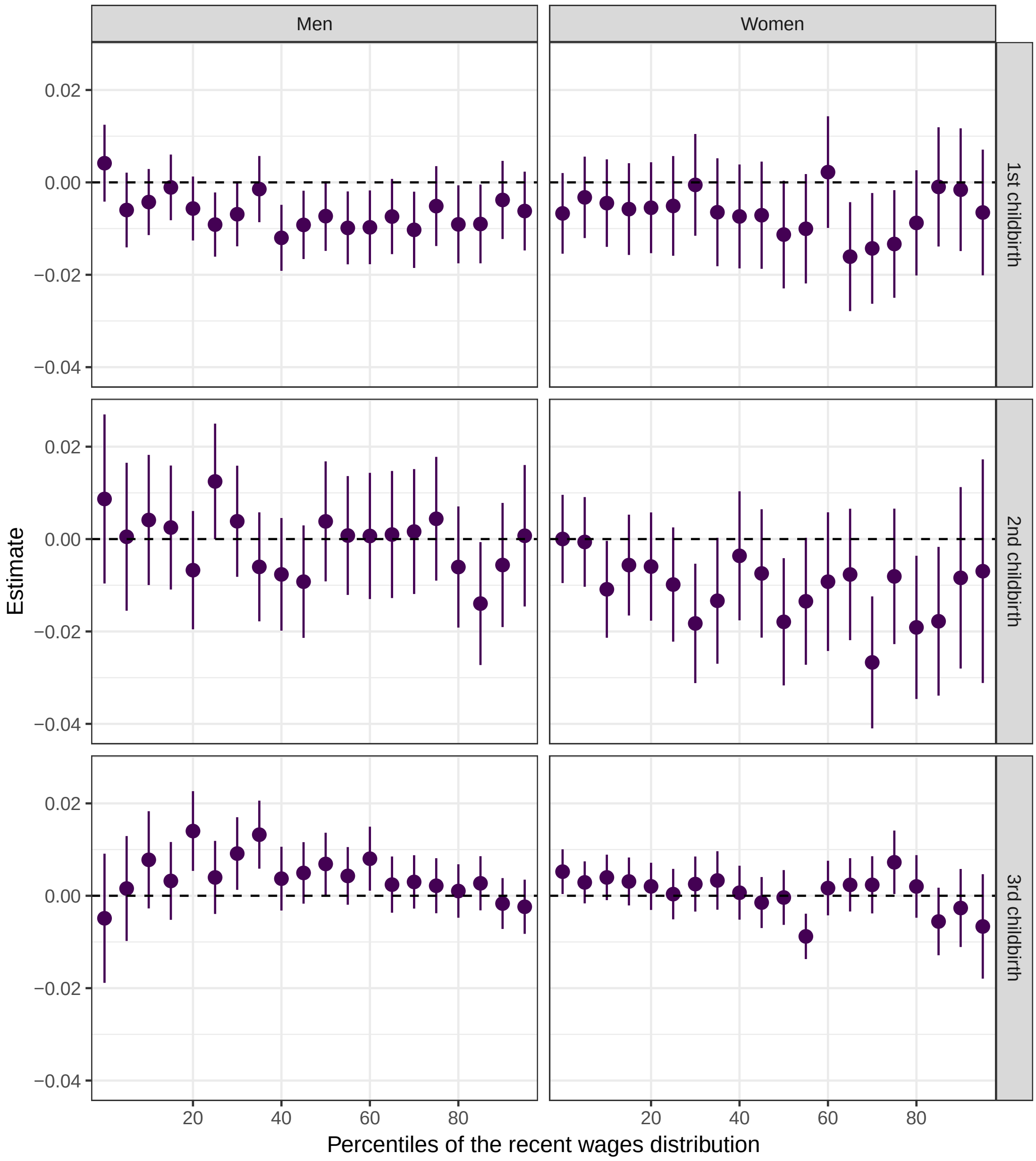
Estimates of the coefficients related to log-GDP growth between $t-1$ and t in a linear probability model with rank in the recent wages distribution × age fixed-effects (12). Outcome is a dummy for having nth childbirth at time t. Standard errors are clustered at the individual level. Sample includes individuals up to age 60 at time t.

Figure 11 – Probability to lose one's job (sensitivity to firm-level shocks)



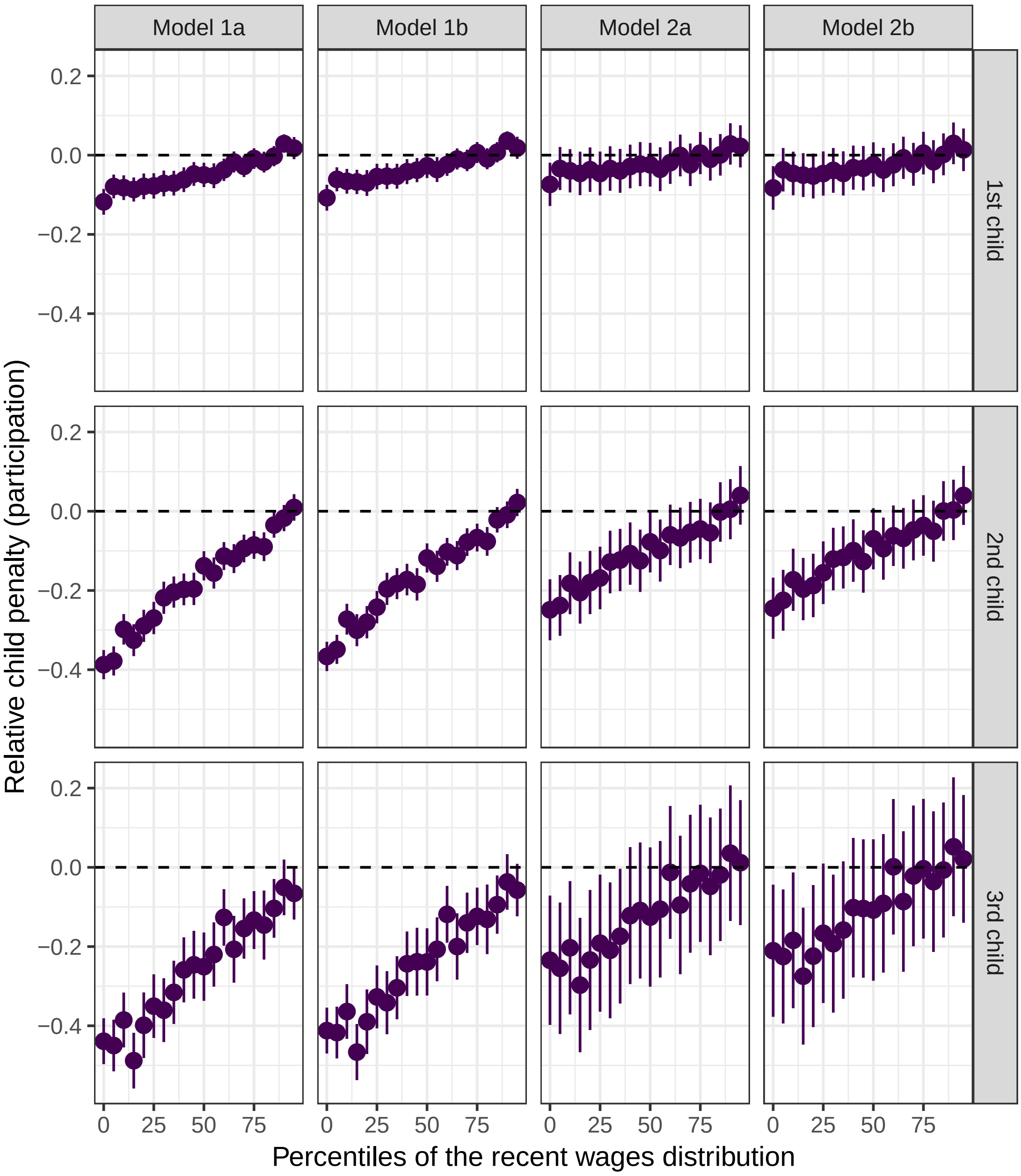
Estimates of the coefficients related to firm-level shocks in a linear probability model with rank in the recent wages distribution × age × year fixed-effects (13). Outcome is a dummy for being jobless at time t. Standard errors are clustered at the individual level. Sample includes individuals up to age 60 at time t.

Figure 12 – Probability to have children (sensitivity to firm-level shocks)



Estimates of the coefficients related to firm-level shocks in a linear probability model with rank in the recent wages distribution × age × year fixed-effects (14). Outcome is a dummy for having nth childbirth at time t. Standard errors are clustered at the individual level. Sample includes individuals up to age 60 at time t.

Figure 13 – Heterogeneity in the probability to remain in employment one year after childbirth: allowing for additional sources of heterogeneity



Estimates of the coefficients related to childbirth for women in a linear probability model that interacts a double-difference setting with gender and rank in the recent wages distribution (16). Outcome is a dummy for participating in the labor market at time $t+1$. In Model 1, the difference-in-difference is only interacted with the recent wages distribution; in Model 2 it is also interacted with education, rank in the distribution of recent paid hours and rank in the distribution of firm composition. Models 1a and 2a include no controls; models 1b and 2b control for year, age, industry and 1-digit occupation within each cell. Standard errors are clustered at the individual level. Sample includes individuals up to age 55 at time t.

For Online Publication - Appendix

A Earnings and working time measures

A.1 Earnings

Our measure of labor earnings relies on net annual earnings. This measure aggregates all wages paid to an individual, including performance pay and bonuses, paid vacations, in-kind benefits, the share of severance payments that exceeds the legal minimum, and early retirement benefits (to the extent that these benefits exceed an amount roughly equal to the minimum wage) but it excludes stock-options. Social security contributions, public pensions schemes, unemployment benefits and other contributions including two flat taxes on labor income (CSG and CRDS) are subtracted to this amount to compute our measure of net annual earnings. In that sense, we measure earnings before income taxes but after some transfers.

Maternity leave allowances are paid by the Social Security administration, and as such are not part of our measure of earnings. They may however transit through the employer (*subrogation*): in this setting, the employer pays the employee the equivalent of maternity leave allowances during her maternity leave, and is later refunded by the Social Security administration. The employer then substracts the maternity leave allowances that they advanced from the measure of earnings. Because the reimbursement occurs later than the maternity leave itself, the drop in earnings may occur a few weeks later than the maternity leave itself. Because we consider annual earnings, this problem is restricted to childbirths that occur at the end of the calendar year. Our results are however very robust to considering only childbirths that occur at the beginning of the year, which are immune to this issue (see Subsection 5.1).

Lastly, in some firms, the employer may be bound by collective agreement to complement earnings during maternity or sick leaves on top of Security provided allowances. This complement is part of labor earnings as measured by the DADS.

A.2 Days

In the DADS dataset, working days refer to the duration during which an employee is part of the labor workforce of a firm within a given year. As a result, maternity and sick leaves, or paid vacations are part of this measure of days, whereas a period of unemployment between two distinct employment spells is not. Additionally, this measure of days is capped to 360.

A.3 Hours

In our dataset, working hours refer to hours for which the worker is paid for according to the labor contract. The information on hours is reported by employers when they fill in payroll tax forms. Before making the data available, Insee performs three checks:

- the total number of hours for a given individual × employer × year observation should not exceed an industry-specific threshold: 2,500 hours per year in a small subset of industries (mostly manufacturing industries, transportation, hotels and restaurants), and 2,200 hours per year in the rest of the private sector;
- the implied hourly wages should exceed 80% of the minimum wage;
- the total number of hours should be positive with the exception of a narrow subset of occupations (mostly journalists and salespersons) working on an fixed-price basis.

If one of these conditions does not hold, then Insee ascribes hours to the observation to make the hourly wage consistent within narrow cells defined by 4-digit occupation, full-time or part-time status, age and gender.

When it comes to workers whose compensation does not depend on their working time, but who do not belong to one of the previous occupations, i.e., typically managers (”forfait-jour”), employers fill in the number of days only. A number of hours is ascribed to these observations based on the legal duration of work for full-time workers, the number of work days, and the implied hourly wages..

Because during a maternity leave, an employee is not paid by her employer for any working hour, but by the Social Security administration (+ the possible complement provided by her employer), working hours during a maternity leave are equal to 0. Workers who are not paid by the hour are an exception to this rule, because their hours are imputed based on their working days, which do not vary during maternity leaves. As a result, the DADS overestimates working hours – and underestimates hourly wages – for these workers for the years when they give birth to a child. In general, these are qualified workers that belong the the upper part of the hourly wages distribution, so that the decomposition of earnings penalties into hours and hourly wages may be biased at the top of the distribution for the specific year workers take maternity leaves.

B Childbirth imputation

We combine data issued from administrative birth records with census data in order to deal with the incompleteness of administrative birth records for individuals born October 2nd and 3rd in our dataset. Specifically, (part of) birth records are missing for these individuals between 1982 and 1997. Our strategy is to take information from the 1990 and 1999 censuses in order to fill the gap.

For each individual in our sample, our data provides us with:

- the years of birth for 1st to 12th child appearing in birth records as of 1967;
- the years of birth for 1st to 12th child as declared in the 1990 census;
- the years of birth for 1st to 12th child as declared in the 1999 census.

Information from birth records has been available since 1967 only, which generates left-censoring. However, because we are mostly interested in individuals giving birth between 2005 and 2015, we don't try to deal with this issue. Our goal is to fill the gap in administrative records between 1982 and 1997 for half of the sampled individuals, which increases substantially our sample size.

For each individual i belonging to the incomplete half of the sample, we impute first the year of first childbirth according to the following principles:

- if the first childbirth in birth records occurs before 1982, we consider it as the first childbirth;
- else:
  - if the minimum of years of childbirth she declared in the 1990 census is after 1982, we consider the minimum of these years and of the year of first childbirth as it appears in birth records as the year of first childbirth;
  - else:
    * if the minimum of years of childbirth she declared in the 1999 census is after 1982, we consider the minimum of these years and of the

year of first childbirth as it appears in birth records as the year of first childbirth;

  * else:

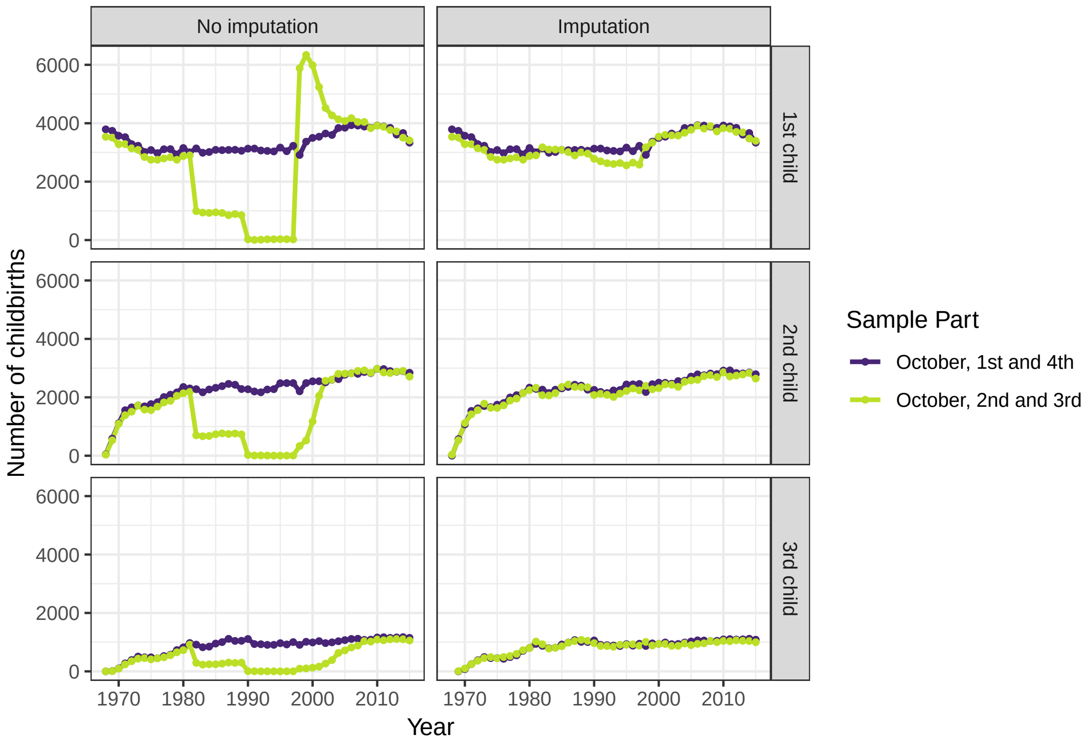
    · if birth records indicate she has children, we consider the year of the first childbirth in birth records as the year of first childbirth;

    · else we consider she has no child.

We then consider the nth childbirth with $n > 1$ as the minimum of years of childbirth within both birth records and censuses among years of birth that follow the computed year of $n - 1$th childbirth.

This approach does not take multiple births into account; more generally, it does not enable individuals to experience more than one childbirth per year. Despite that *caveat*, our approach matches the historical pattern in the complete half of the sample quite well. Figure B.1 plots the number of childbirths for each year since 1968, by rank of childbirth, for both parts of the sample, relying on birth records only (left panel) and on our approach (right panel). While we still slightly underestimate first childbirths which occur at the beginning of the 1980s or in the late 1990s in the incomplete half of the sample, our approach does a reasonable job at matching the patterns observed in the complete half of the sample; this is especially true as far as the 2005-2015 period on which our analysis focuses is concerned.

Figure B.1 – Imputation of childbirths for individuals born October 2nd and 3rd



C Summary statistics

Table 1 provides with some statistics on the selection process. First comes the censoring of observations with low paid hours or low employment duration. Second comes the restriction to individuals that were present two year between $t-5$ and $t-2$, on top of being present in $t-1$ and t. As expected, both steps increase average hourly wages within gender, age groups and industry. The selection is harsher for women than it is for men, women being more likely to experience career interruptions. The censoring decreases slightly the share of younger workers, which is consistent with entry in the labor workforce through shorter and non-full time employment spells –so does the selection, for the same reason. The censoring decreases the share of workers in the service industry who are more likely to have short employment spells and to work part-time. The selection also decreases the share of service industry workers among men and the share of trade industry workers among women: these individuals have more unstable employment histories than their counterparts working in other industries.

Both within our base sample (after the censoring) and within our selected sample, the gender gap in hourly wages is larger among older workers than it is among their younger counterparts.

Figure C.1 displays the number of childbirths both in the raw EDP dataset and in our final sample.[24] Because we focus on childbirths that occur after individuals have experienced quite stable employment several years in a row, and because our data only covers salaried employment in the private sector, numerous childbirths are not included in our final sample: we dispose of between one third (third childbirth) and one half (first childbirth) of women who experience childbirth between 2005 and 2015. These proportions amount to 50% and 60% for men during the same period.

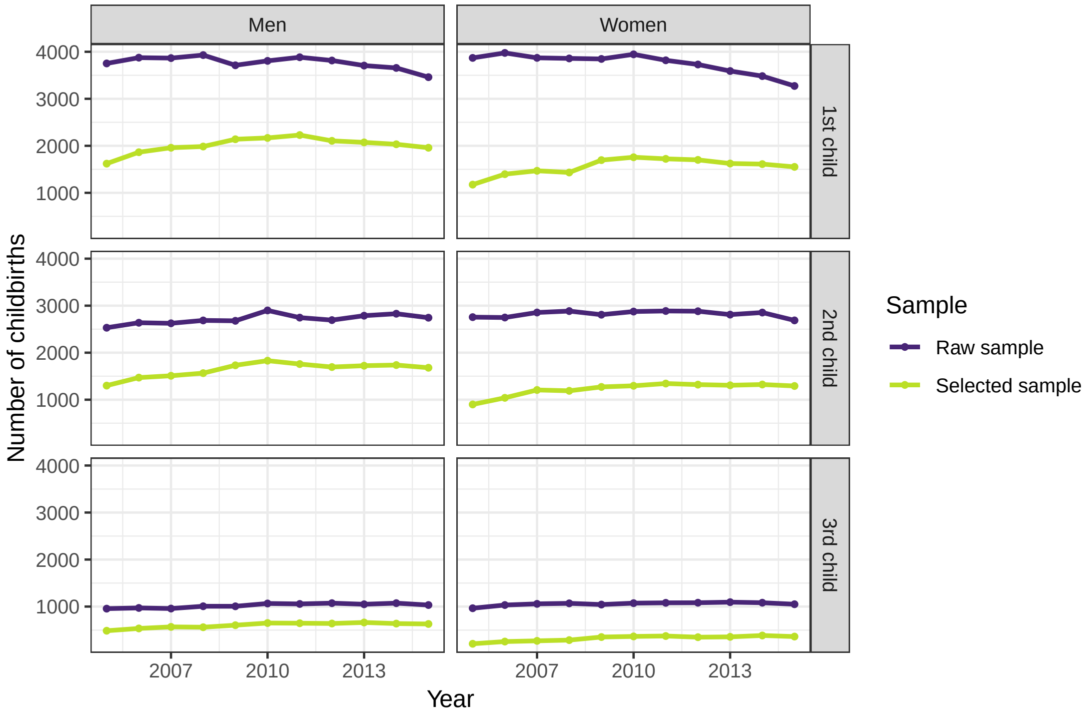
[24]The raw EDP dataset itself is not perfectly representative of all childbirths that occur in France because it only provides information on fertility of individuals that have appeared at least once in labor market data, the sample of which has varied over time.

Table 1 – Sample selection

	Base sample				Censoring				Final sample			
N_{it}	2 851 895				2 568 211				1 886 171			
N_i	362 637				334 811				267 632			
	Women		Men		Women		Men		Women		Men	
N_{it}	1 264 722		1 587 173		1 107 859		1 460 352		795 133		1 091 038	
N_i	169 670		192 967		153 268		181 543		118 260		149 372	
	Frequency (in %)	Average hourly wages (2015 €)	Frequency (in %)	Average hourly wages (2015 €)	Frequency (in %)	Average hourly wages (2015 €)	Frequency (in %)	Average hourly wages (2015 €)	Frequency (in %)	Average hourly wages (2015 €)	Frequency (in %)	Average hourly wages (2015 €)
Age												
23-24	6,8	9,4	6	9,9	6,1	9,4	5,5	9,9	4,4	9,3	4,1	10
25-29	16	10,6	15,4	11,7	15,7	10,6	15	11,5	14,8	10,7	14,2	11,7
30-34	14,8	11,8	15,3	13,7	14,8	11,9	15,4	13,4	14,6	12,2	15,3	13,6
35-39	14,3	12,5	15	15,1	14,4	12,5	15,1	14,8	14,4	12,9	15,3	15
40-44	14,2	12,8	14,4	16,2	14,5	12,8	14,7	15,9	14,8	13,1	15,1	16,1
45-49	13,5	12,9	13,2	16,9	13,7	12,9	13,4	16,6	14,4	13,2	14	16,9
50-54	11,7	13	11,7	17,3	12	13,1	11,9	17,3	12,8	13,3	12,4	17,5
55-59	8,7	13,6	9	18,5	8,8	13,4	9	18,4	9,8	13,6	9,5	18,6
Industry												
Construction	1,8	12,7	11,8	13,4	1,9	12,7	12,3	13,1	2,1	13,3	12,6	13,7
Manufacturing	12,5	12,8	23,7	15,6	13,5	12,8	25	15,4	14,3	13,2	26,3	15,7
Services	66	12	48,9	14,8	64,2	12,1	46,6	14,9	63,2	12,7	44,8	15,7
Trade	19,7	10,7	15,6	13,8	20,3	10,7	16,1	13,6	20,4	11,2	16,2	14,1

Base sample includes all individuals aged 20 to 60 that have positive employment in the private sector at time t. Censoring excludes individuals that that work less than 45 days a year, less than 1/8 of the legal duration a week, or paid less than 90% of the minimum hourly wage. Final sample includes only individual that are over this threhold at time t, $t-1$ and at least twice between $t-5$ and $t-5$. Figures for the final sample are computed at time $t-1$.

Figure C.1 – Consequences of sample selection with respect to childbirths



D Accounting decomposition

The log-change in total labor earnings between time $t-1$ and time $t+k$ writes:

$$\Delta y_{t+k} = \log\left(\mathbb{E}[y_{i,t+k}]\right) - \log\left(\mathbb{E}[y_{i,t-1}]\right) \tag{17}$$

Δy_{t+k} can also be rewritten as:

$$\Delta y_{t+k} = \log\left(\frac{\mathbb{E}[\frac{y_{i,t+k}}{y_{i,t-1}} y_{i,t-1}]}{\mathbb{E}[y_{i,t-1}]}\right) \tag{18}$$

This writing is particularly relevant here since we impose that all individuals have positive employment at time $t-1$, so that $y_{i,t-1} > 0$. Hence Δy_{t+k} is simply the log-average of individual changes $y_{i,t+k}/y_{i,t-1}$ weighted by initial earnings $y_{i,t-1}$.

Next we use an accounting decomposition of labor earnings at the individual level. First, using the law of iterated expectations yields:

$$\begin{aligned}\mathbb{E}[y_{i,t+k}] =& \mathbb{P}(d_{i,t+k}=0)\mathbb{E}[y_{i,t+k}|d_{i,t+k}=0] \\ &+ \mathbb{P}(d_{i,t+k}=1)\mathbb{E}[y_{i,t+k}|d_{i,t+k}=1]\end{aligned} \tag{19}$$

Since $d_{i,t+k} = 0 \Rightarrow y_{i,t+k} = 0$, the first term vanishes:

$$\begin{aligned}\Delta y_{t+k} =& \log(\mathbb{P}(d_{i,t+k}=1)) + \log(\mathbb{E}[y_{i,t+k}|d_{i,t+k}=1]) - \log(\mathbb{E}[y_{i,t-1}]) \\ =& \underbrace{\log(\mathbb{P}(d_{i,t+k}=1))}_{\text{Participation}} + \underbrace{\log(\mathbb{E}[y_{i,t-1}|d_{i,t+k}=1]) - \log(\mathbb{E}[y_{i,t-1}])}_{\text{Selection}} \\ &+ \underbrace{\log(\mathbb{E}[y_{i,t+k}|d_{i,t+k}=1]) - \log(\mathbb{E}[y_{i,t-1}|d_{i,t+k}=1])}_{\Delta y^{\text{Participants}}_{t+k}}\end{aligned} \tag{20}$$

We are thus left with the decomposition of the latter term $\Delta y^{\text{Participants}}_{t+k}$; for these participants, all components of labor earnings – days, hours and hourly wages –

are observed in the data. Then,

$$\Delta y_{t+k}^{\text{Participants}} = \underbrace{\log\left(\frac{\mathbb{E}\left[\frac{w_{i,t+k}}{w_{i,t-1}} x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k} = 1\right]}{\mathbb{E}\left[x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k} = 1\right]}\right)}_{\text{Hourly wages growth}} + \log\left(\frac{\mathbb{E}\left[x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k} = 1\right]}{\mathbb{E}[x_{i,t-1} h_{i,t-1} w_{i,t-1} | d_{i,t+k} = 1]}\right) \tag{21}$$

and we keep introducing similar substitutions in the second term with respect to the two remaining components (hours and days). It follows that:

$$\begin{aligned} \underbrace{\Delta y_{t+k}}_{\text{Labor earnings changes}} &= \underbrace{\log\left(\mathbb{P}(d_{i,t+k} = 1)\right)}_{\text{Participation}} \\ &+ \underbrace{\log\left(\frac{\mathbb{E}[y_{i,t-1} | d_{i,t+k} = 1]}{\mathbb{E}[y_{i,t-1}]}\right)}_{\text{Selection}} \\ &+ \underbrace{\log\left(\frac{\mathbb{E}\left[\frac{x_{i,t+k}}{x_{i,t-1}} x_{i,t-1} h_{i,t-1} w_{i,t-1} | d_{i,t+k} = 1\right]}{\mathbb{E}\left[x_{i,t-1} h_{i,t-1} w_{i,t-1} | d_{i,t+k} = 1\right]}\right)}_{\text{Working days Changes}} \\ &+ \underbrace{\log\left(\frac{\mathbb{E}\left[\frac{h_{i,t+k}}{h_{i,t-1}} x_{i,t+k} h_{i,t-1} w_{i,t-1} | d_{i,t+k} = 1\right]}{\mathbb{E}\left[x_{i,t+k} h_{i,t-1} w_{i,t-1} | d_{i,t+k} = 1\right]}\right)}_{\text{Hours Per Day Changes}} \\ &+ \underbrace{\log\left(\frac{\mathbb{E}\left[\frac{w_{i,t+k}}{w_{i,t-1}} x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k} = 1\right]}{\mathbb{E}\left[x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k} = 1\right]}\right)}_{\text{Hourly Wages Growth}} \end{aligned} \tag{22}$$

This accounting identity clarifies that the (reweighted) log-average of individual earnings changes can be decomposed into the sum of (reweighted) log-average of individual changes relative to each component, plus a selection term.

E Additional results

E.1 Long-run child penalties

Our results suggest that the arrival of a child does generate a short-run shift in hourly wages for mothers at the top of the wage distribution, after which a catch-up may occur but is not sufficient for women to recover by comparison with their male counterparts (Figures 1 and 2). We further investigate the impact of children on hourly wages in the longer run, to gain a better sense of how the arrival of a child impacts the wage rate past the first few years after childbirth. The motivation here has to do with the possibility that mothers recover at least partly from the negative hourly wage shock that childbirth generates. It could also be that there is in fact no recovery from childbirths, i.e., that the latter are the source of some permanent hourly wage shift, and that afterwards men and women experience similar career progressions, i.e., have parallel wages growth. A last possibility corresponds to men and women having diverging hourly wage levels due to the arrival of children, so that in the long-run the child penalty increases over time, for instance because mothers spend less time on the labor market, so that they experience slower wage growth than their male counterparts.

We now focus on the gender gap in hourly wage growth among parents of children aged 6 or more and compare it to the one that prevails among non-parents. After age 6, most children attend school, which alleviates time constraints. We resort to the same methodology as before, our control group being composed of non-parents; yet the analogue of the time dimension in a standard difference-in-difference is replaced here by the gender dimension. Figure E.1 displays the results from an OLS estimation, the outcome being 1-year hourly wage growth.[25] We resort to this strategy because there are much fewer observations for which we observe both pre-childbirth wage levels and long-run hourly wages: typically our previous approach yields only 10-year child penalties for individuals who had a childbirth in 2005; small sample size generates large standard errors and renders

[25] The gender gap in hourly wage growth among non-parents, our control group, is displayed in Appendix E.2.

the results uninterpretable (see Figure 3).

In the lowest part of the distribution, the gender gap is similar or slightly larger among parents, about 0.3 log-point per year. Among individuals belonging to the highest half of the distribution, the sign is reversed, which suggest that some part of high-achieving mothers may recover some part of their children-related wage losses (Model 1). These results suggest therefore the possibility of long-run effects of childbirths on their mothers' career progressions at the bottom of the distribution. Controlling for horizontal segregation somehow lowers the estimates (Model 2), even though the difference is not statistically significant. This empirical evidence is consistent with the arrival of a child generating gender differentials in sorting across firms (Coudin, Maillard, and Tô, 2018), which is a plausible explanation to this observed gap. Surprisingly, controlling further for both experience and job mobility widens this gap (Model 3).

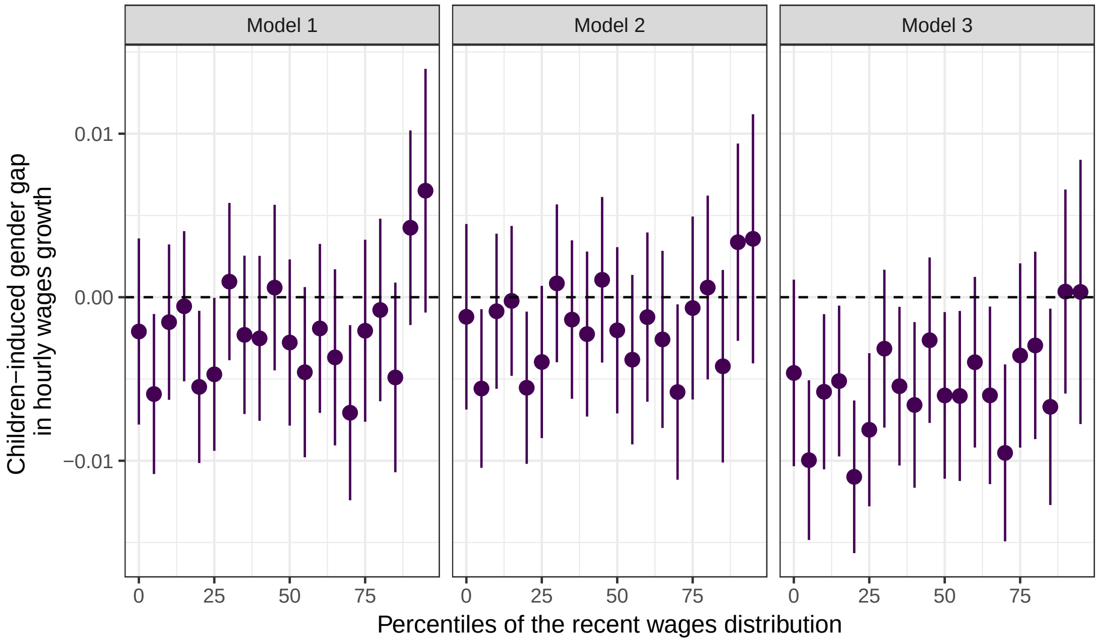
To sum up, even if childbirths don't generate large, short-run losses in hourly wages at the bottom of the distribution, which stems most likely from the minimum wage (and more generally from the institutional setting), they may generate a sticky floor pattern in the long-run. By contrast, at the top of the distribution, mothers may well experience short-run shifts in their hourly wages, but several years after their last child is born they experience slightly better career progession as their male counterparts do, which may enable them to partly catch-up with men.

E.2 Career progressions among nonparents

We show that men and women still have different career progressions even when they do not have children. We focus on individuals with no observed child in the data; these individuals could experience childbirth after 2015. We estimate gender differences in hourly wage growth all along the recent wages distribution, as Figure E.2 shows.

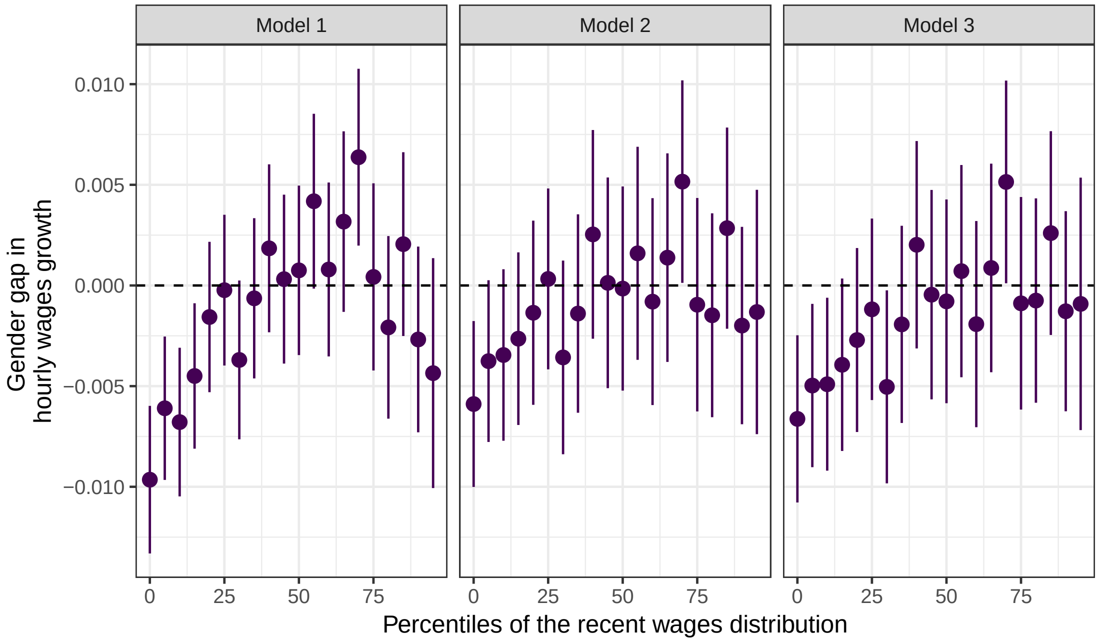
We obtain a U-shaped pattern: at both ends of the distribution, women experience slower hourly wage growth than their male counterparts, while among median workers gender differences are not statistically significant (Model 1). Though the difference is not large (about 1 log-point), such differentials cumulate over time and are the source of substantial wage gaps. Controlling for horizontal segregation (Model 2) as well as for experience and job mobility (Model 3), the difference shrinks at both ends of the distribution. As far as low earners are concerned, the gap remains barely significant even in the full control specification: the sticky floor effect would not merely be the consequence of children only and leads the concerned women to be progressively left apart in the bottom of the distribution.

Figure E.1 – Children-induced gender differences in hourly wages growth among parents of older children



Estimates of the coefficients related to gender (female dummy) × having all children aged more than 6, relative to gender × never having children, interacted with location in the recent wages distribution, in a hourly wages growth model. Outcome is a (log) hourly wages growth between $t-1$ and t. Model 1 includes not controls. Model 2 controls for year, age, industry, firm composition (share of part-time working women) and 1-digit occupation within recent wages cell. Model 3 includes all these controls plus experience between t and t and having changed firm between $t-1$ and t. Standard errors are clustered at the individual level. Sample includes individuals up to age 60 at time t.

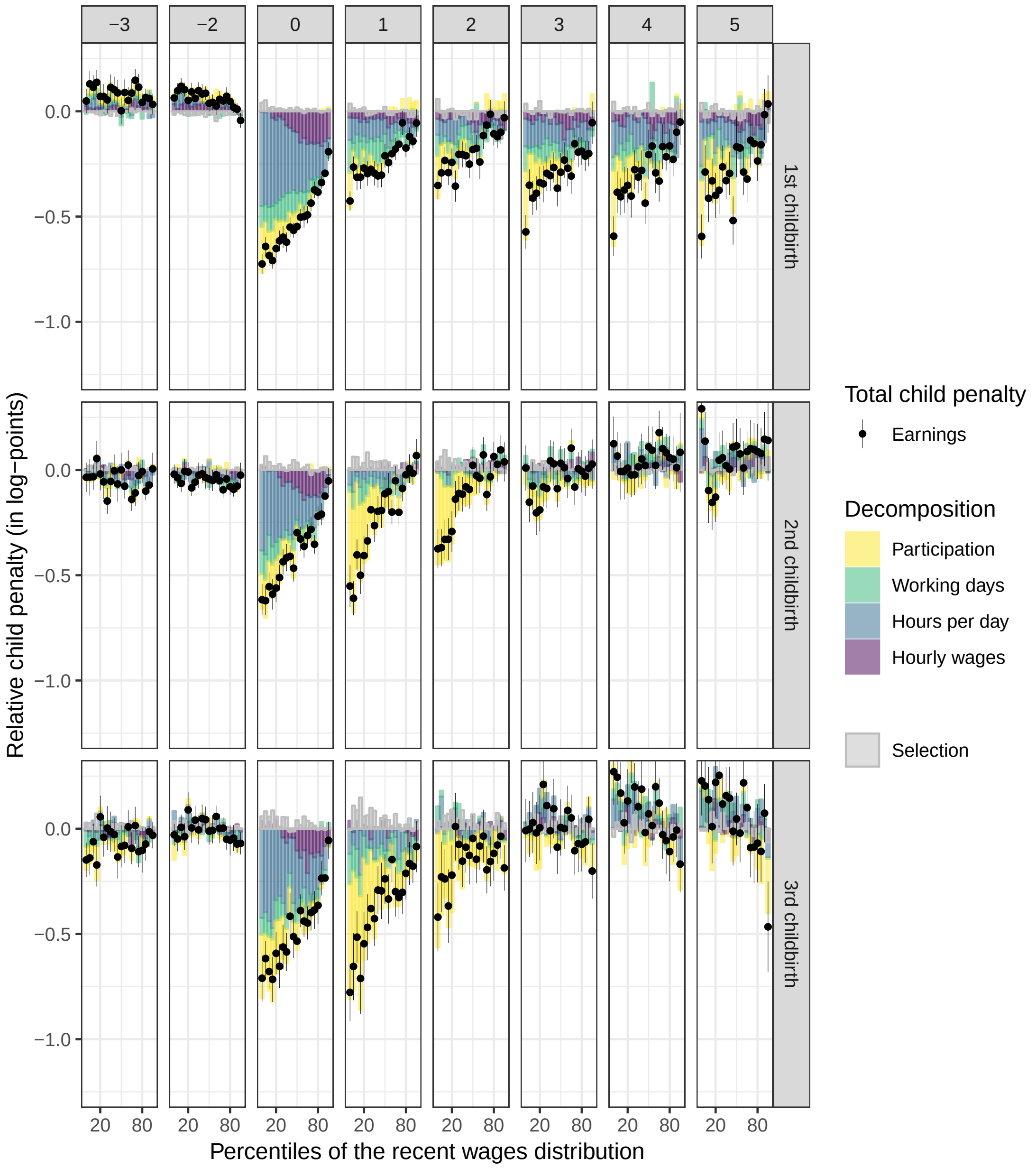
Figure E.2 – Gender differences in hourly wages growth among nonparents



Estimates of the coefficients related gender (female dummy) hourly wages growth model that interacts gender and rank in the recent wages distribution. Outcome is a (log) hourly wages growth between $t-1$ and t. Model 1 includes not controls. Model 2 controls for year, age, industry, firm composition (share of part-time working women) and 1-digit occupation within recent wages cell. Model 3 includes all these controls plus experience between t and t and having changed firm between $t-1$ and t. Standard errors are clustered at the individual level. Sample includes individuals up to age 60 at time t.

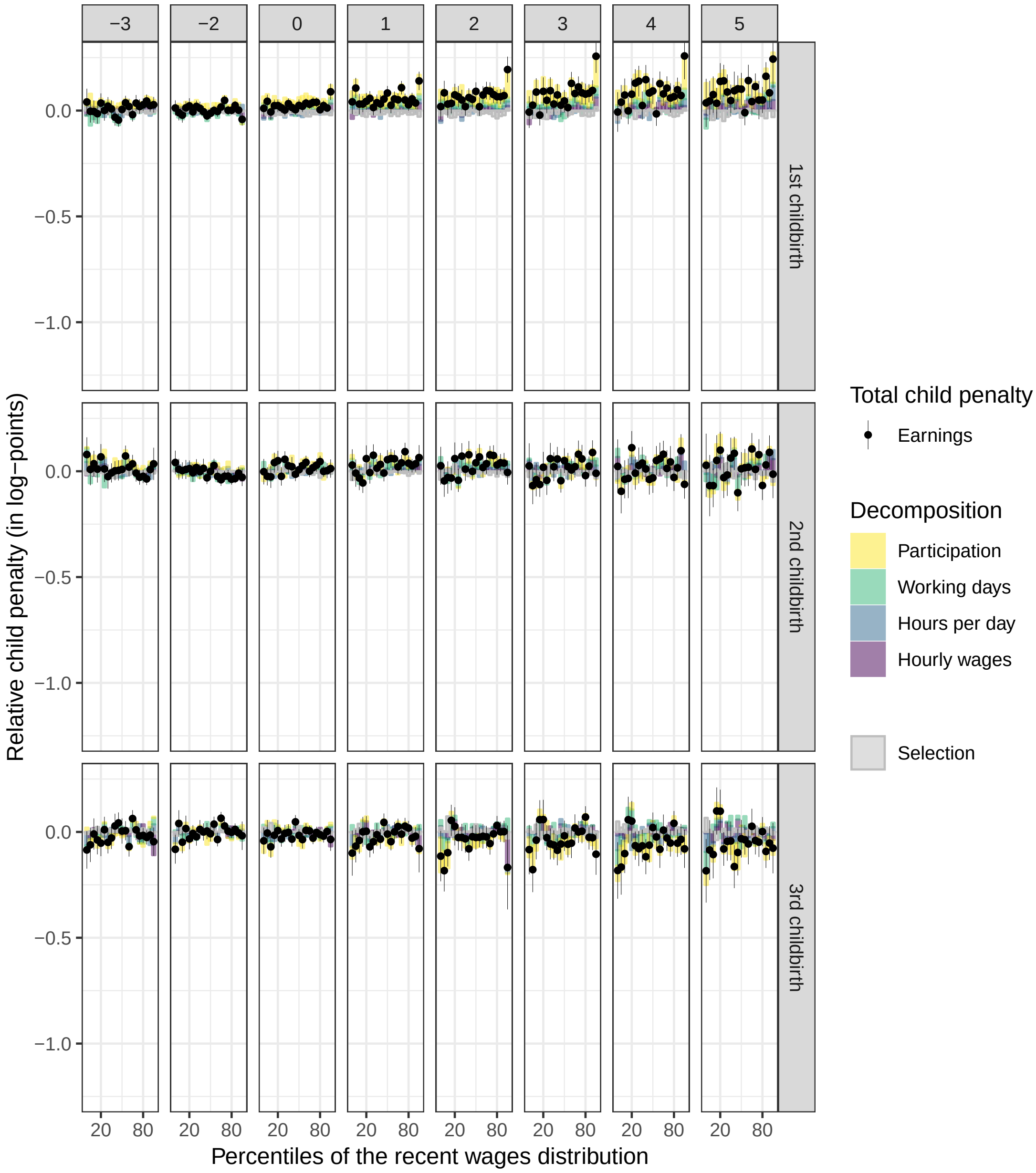
F Robustness checks

Figure F.1 – Consequences of childbirth on women's labor outcomes: with control group taken at imputed age of childbirth



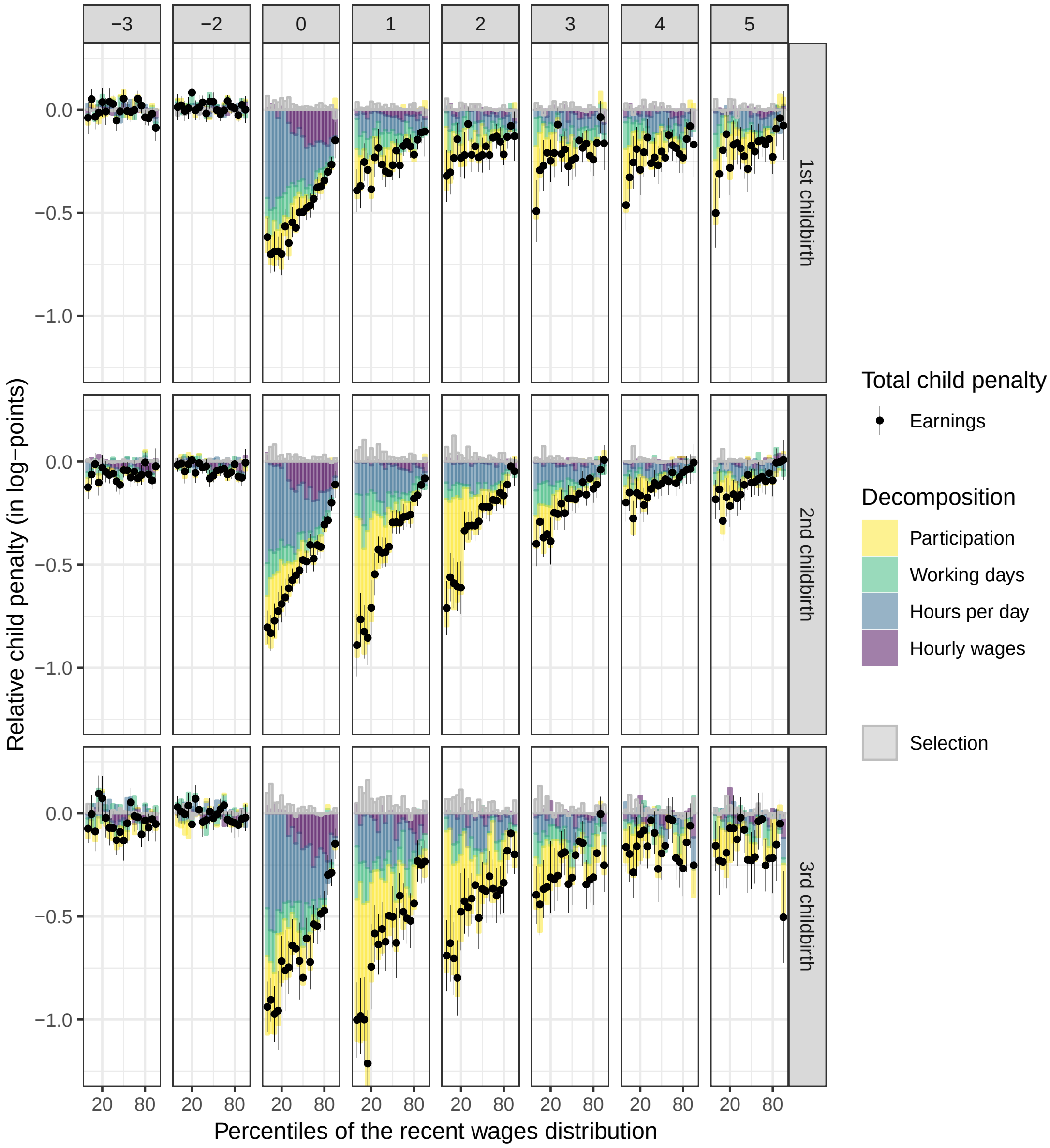
Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Control group is taken at age randomly drawn in the distribution of age at nth childbirth within gender × cohort × education cells (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure F.2 – Consequences of childbirth on men's labor outcomes: with control group taken at imputed age of childbirth



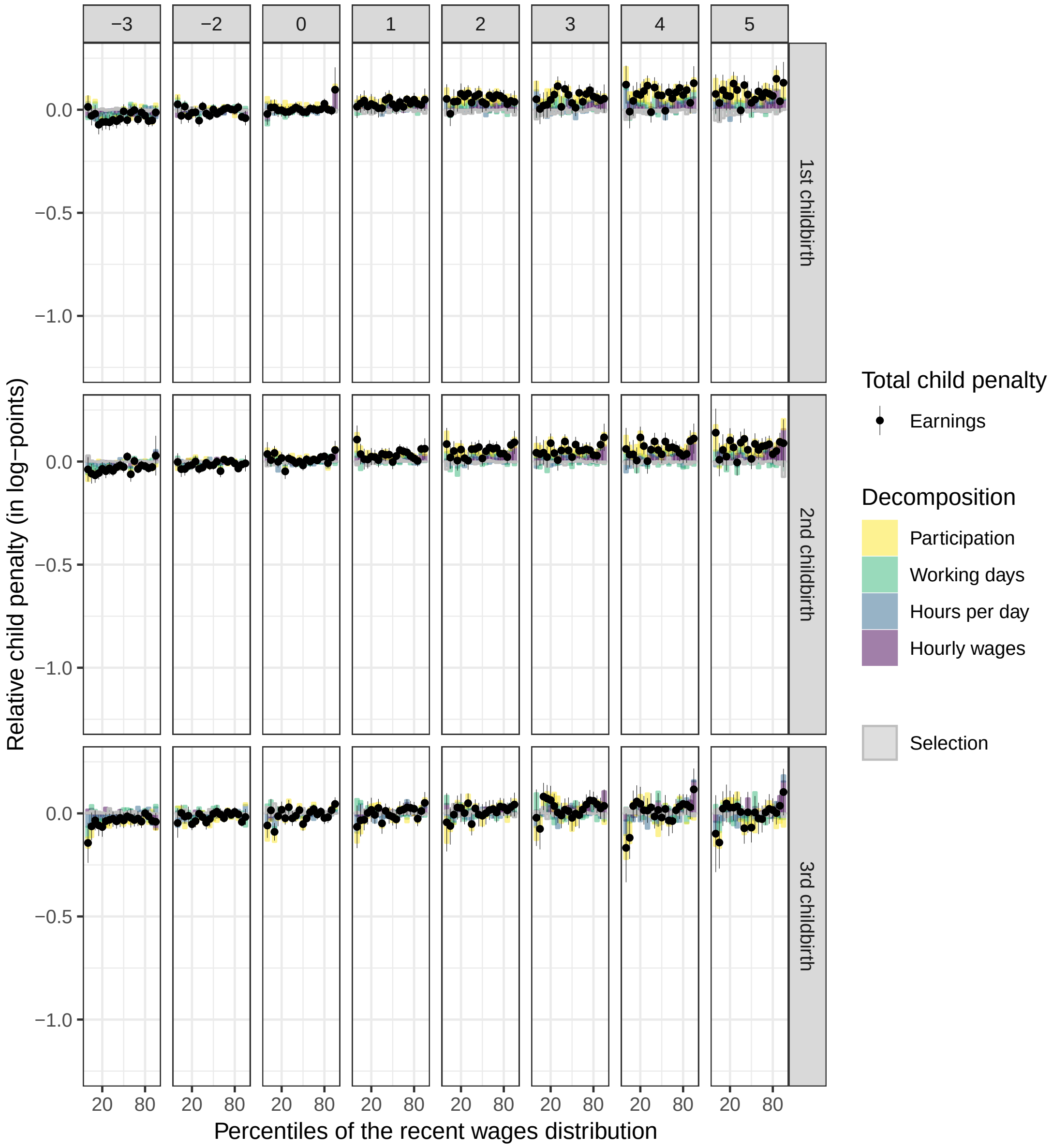
Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Control group is taken at age randomly drawn in the distribution of age at nth childbirth within gender × cohort × education cells (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure F.3 – Consequences of childbirth on women's labor outcomes: restriction to older cohorts that have completed fertility decisions



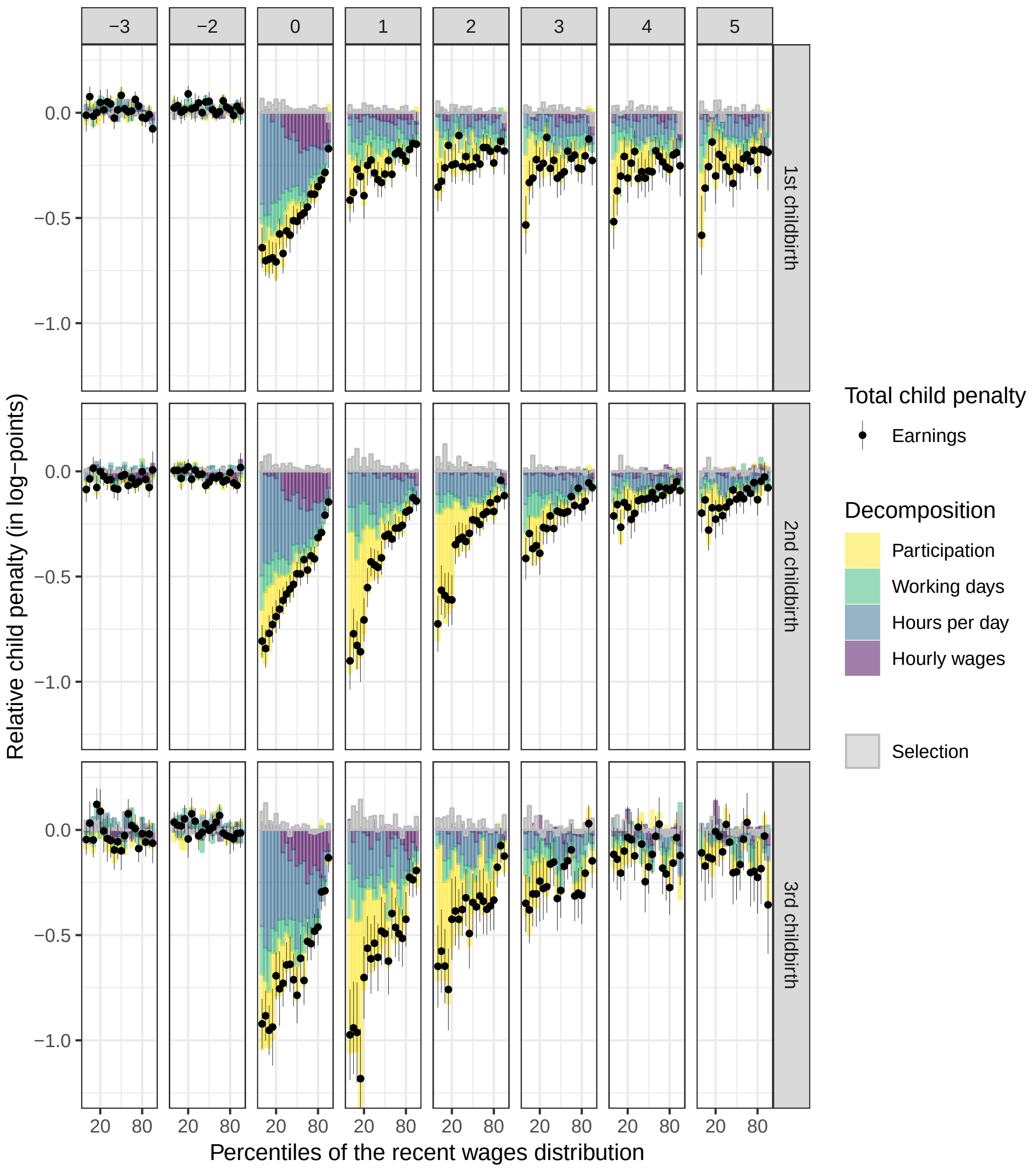
Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Sample is restricted to individuals born before in 1975 or before (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure F.4 – Consequences of childbirth on men's labor outcomes: restriction to older cohorts that have completed fertility decisions



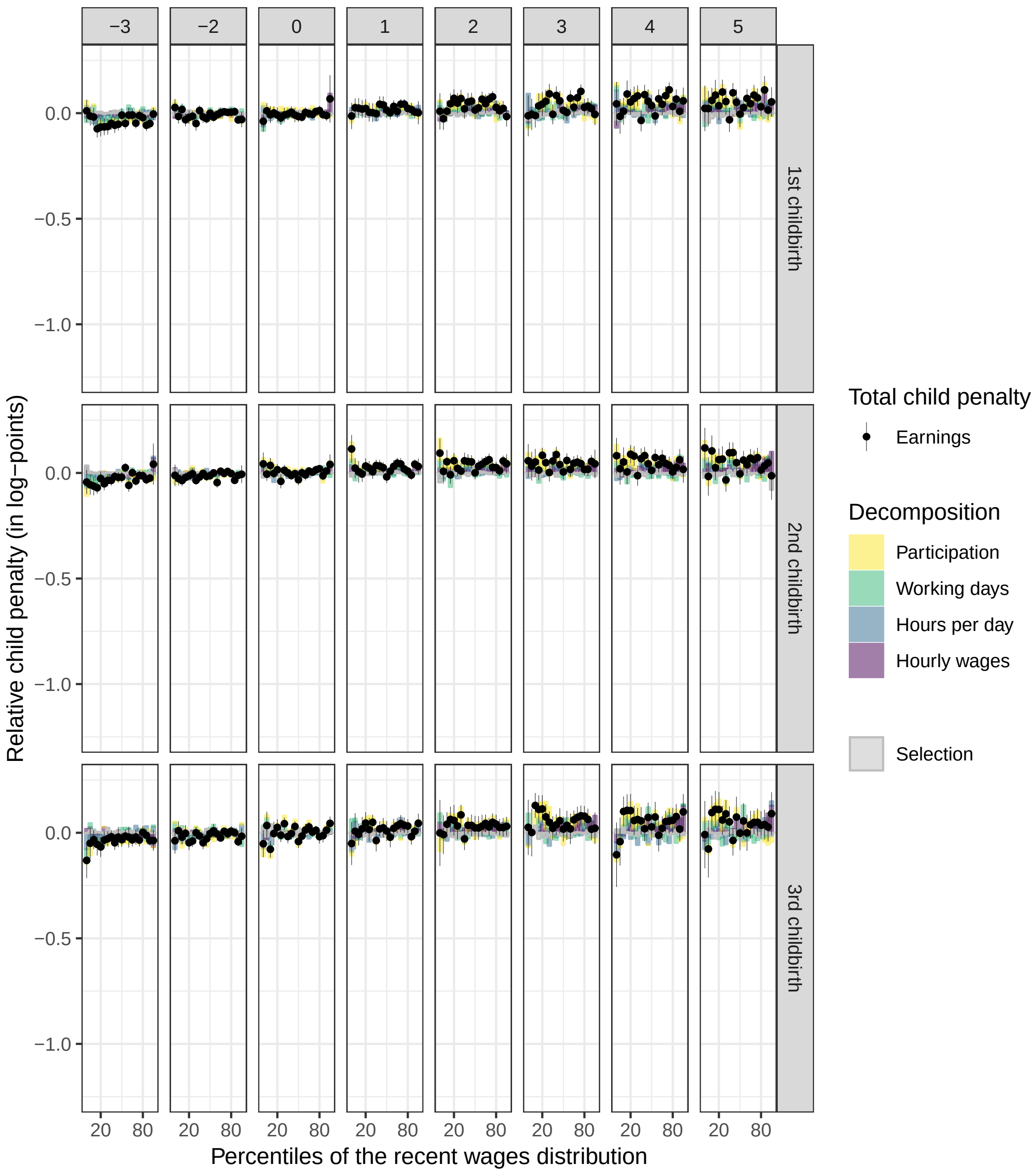
Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Sample is restricted to individuals born before in 1975 or before (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure F.5 – Consequences of childbirth on women's labor outcomes: identification based on the timing of kth childbirth



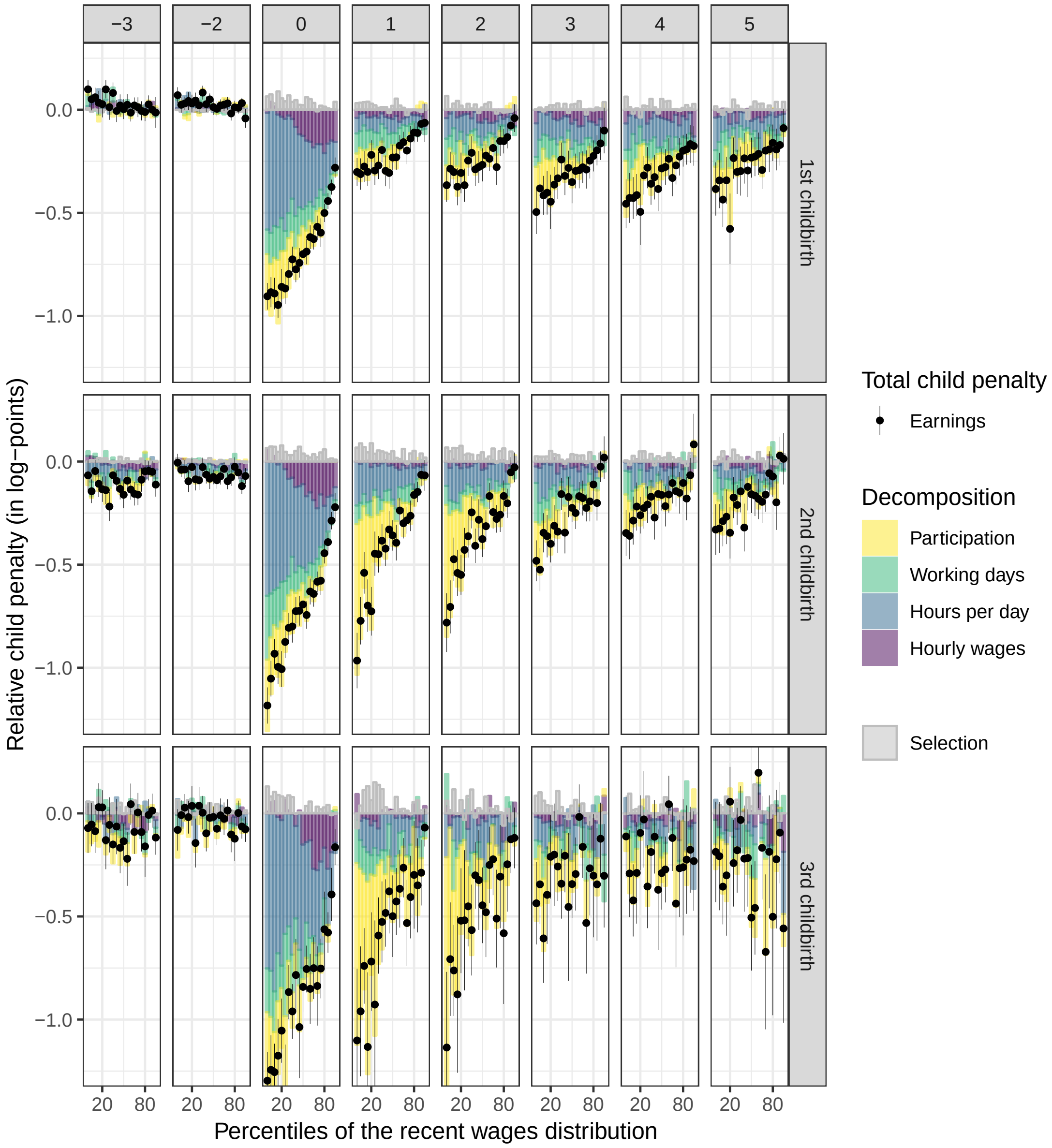
Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Control group includes individuals with excactly n children in 2015, that do not experience childbirth between $t-3$ and $t+5$ (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure F.6 – Consequences of childbirth on men's labor outcomes: identification based on the timing of kth childbirth



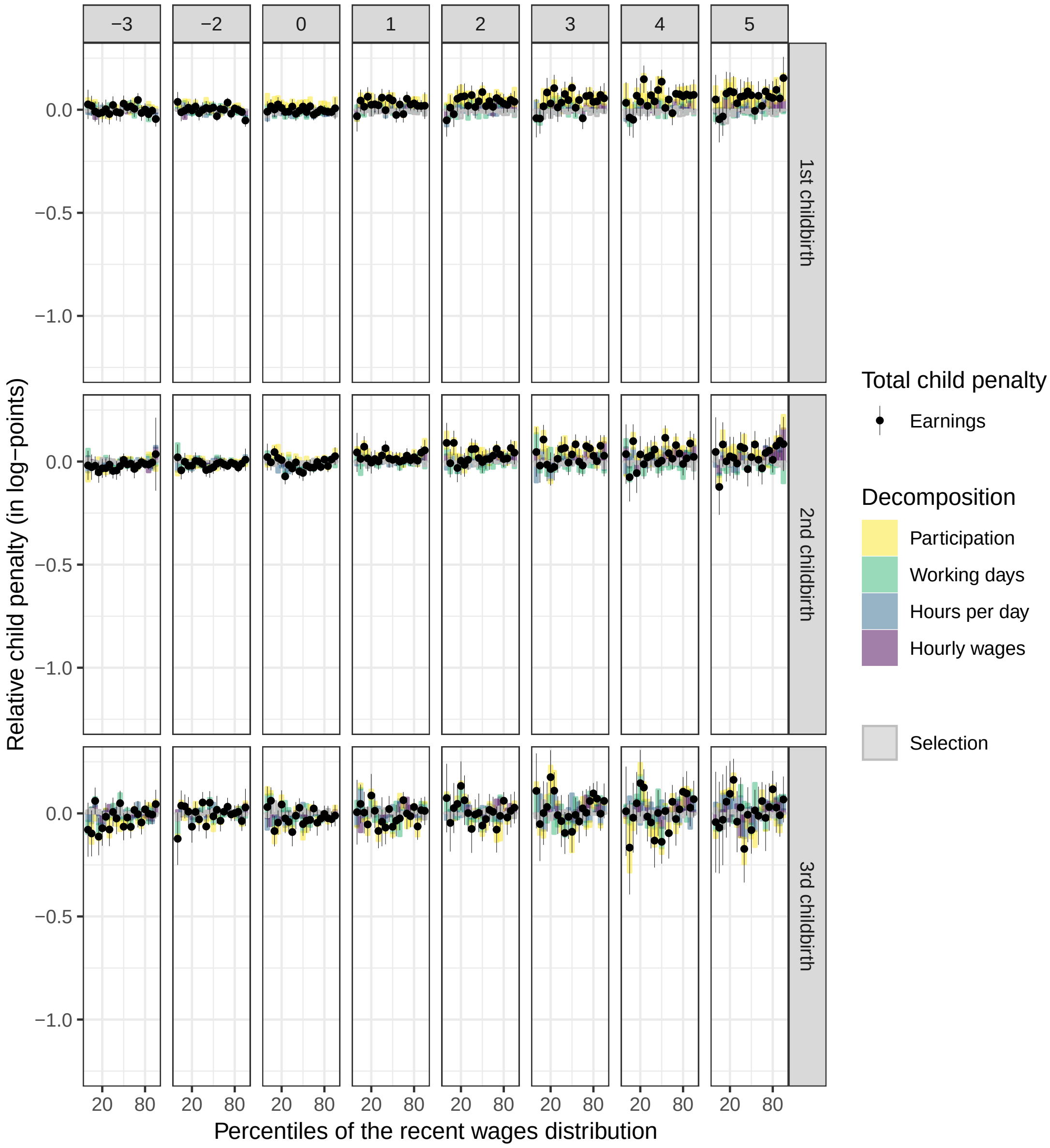
Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Control group includes individuals with excactly n children in 2015, that do not experience childbirth between $t-3$ and $t+5$ (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure F.7 – Consequences of childbirth on women's labor outcomes: restriction to childbirths of the 2nd quarter



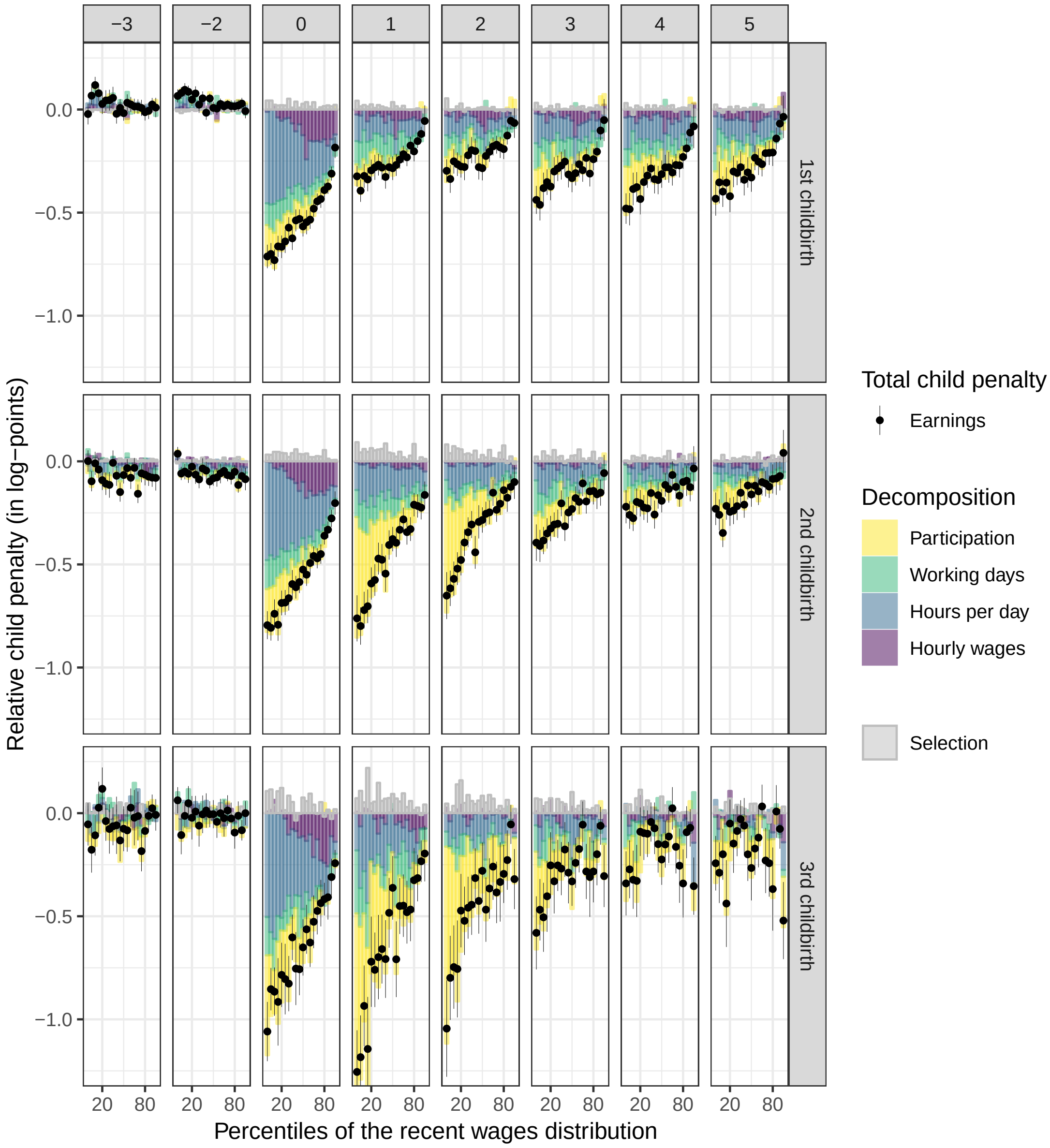
Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Treated group is restricted to individuals that experience nth childbirth during the second quarter of year t (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure F.8 – Consequences of childbirth on men's labor outcomes: restriction to childbirths of the 2nd quarter



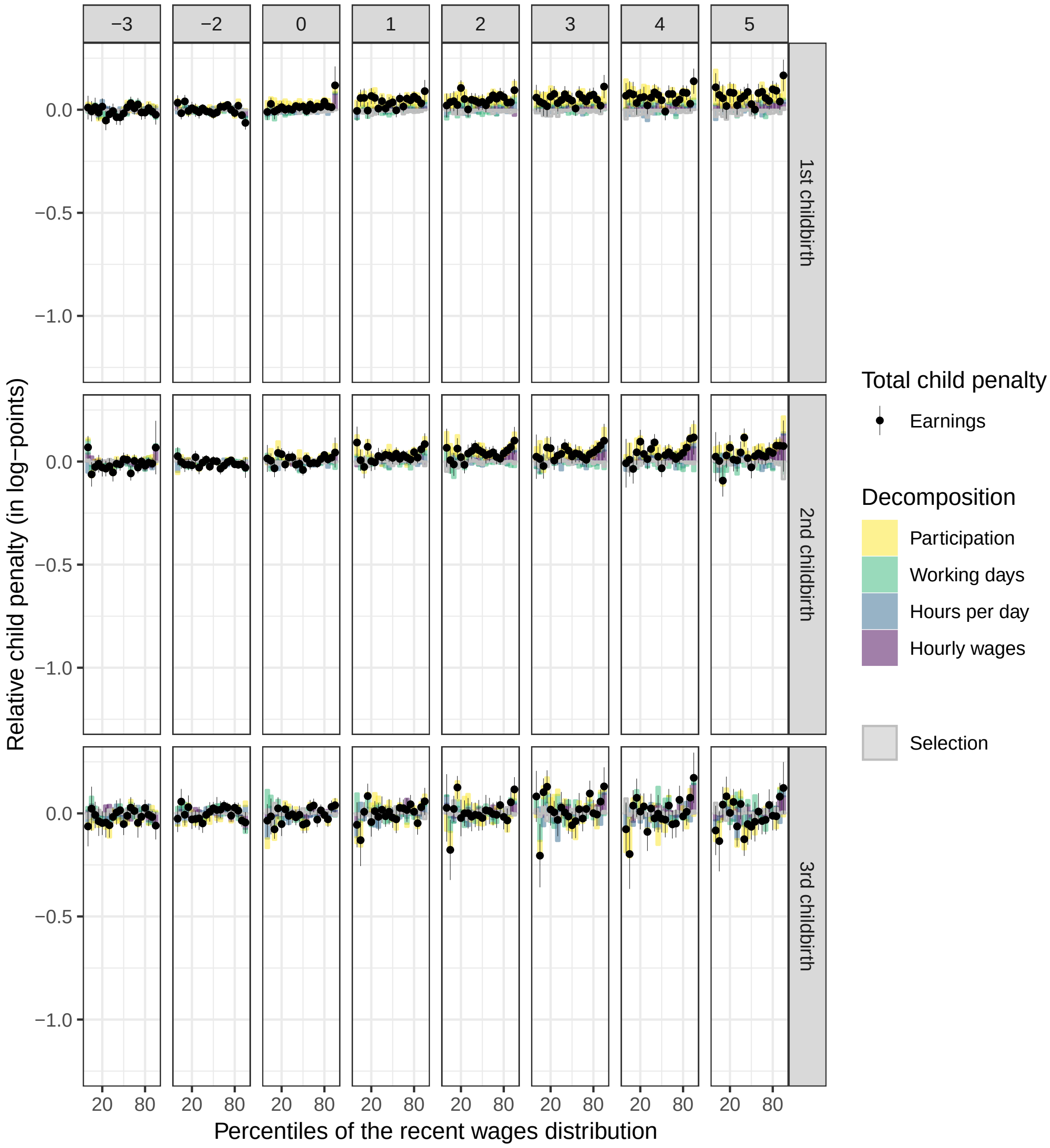
Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Treated group is restricted to individuals that experience nth childbirth during the second quarter of year t (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

Figure F.9 – Consequences of childbirth on women's labor outcomes: restriction to 2007-2010 childbirths



Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Treatment time is restricted to years 2007 to 2010 (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

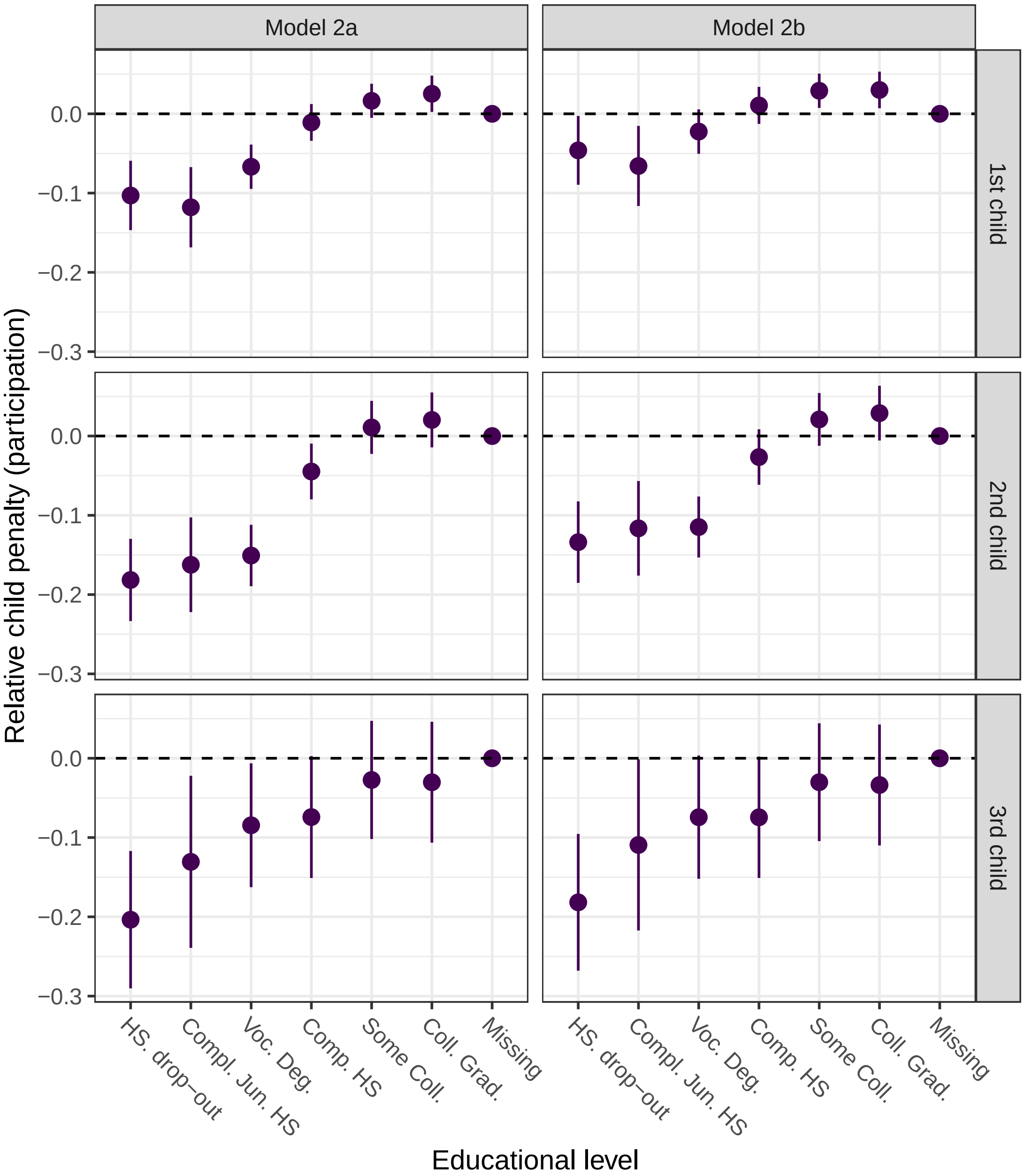
Figure F.10 – Consequences of childbirth on men's labor outcomes: restriction to 2007-2010 childbirths



Each panel displays the estimates of child penalties obtained by difference-in-difference (see 7) for a different time-to-childbirth expressed in years. Treatment time is restricted to years 2007 to 2010 (see 5.1). Standard errors are clustered at the individual level and computed by boostrap (100 replicates).

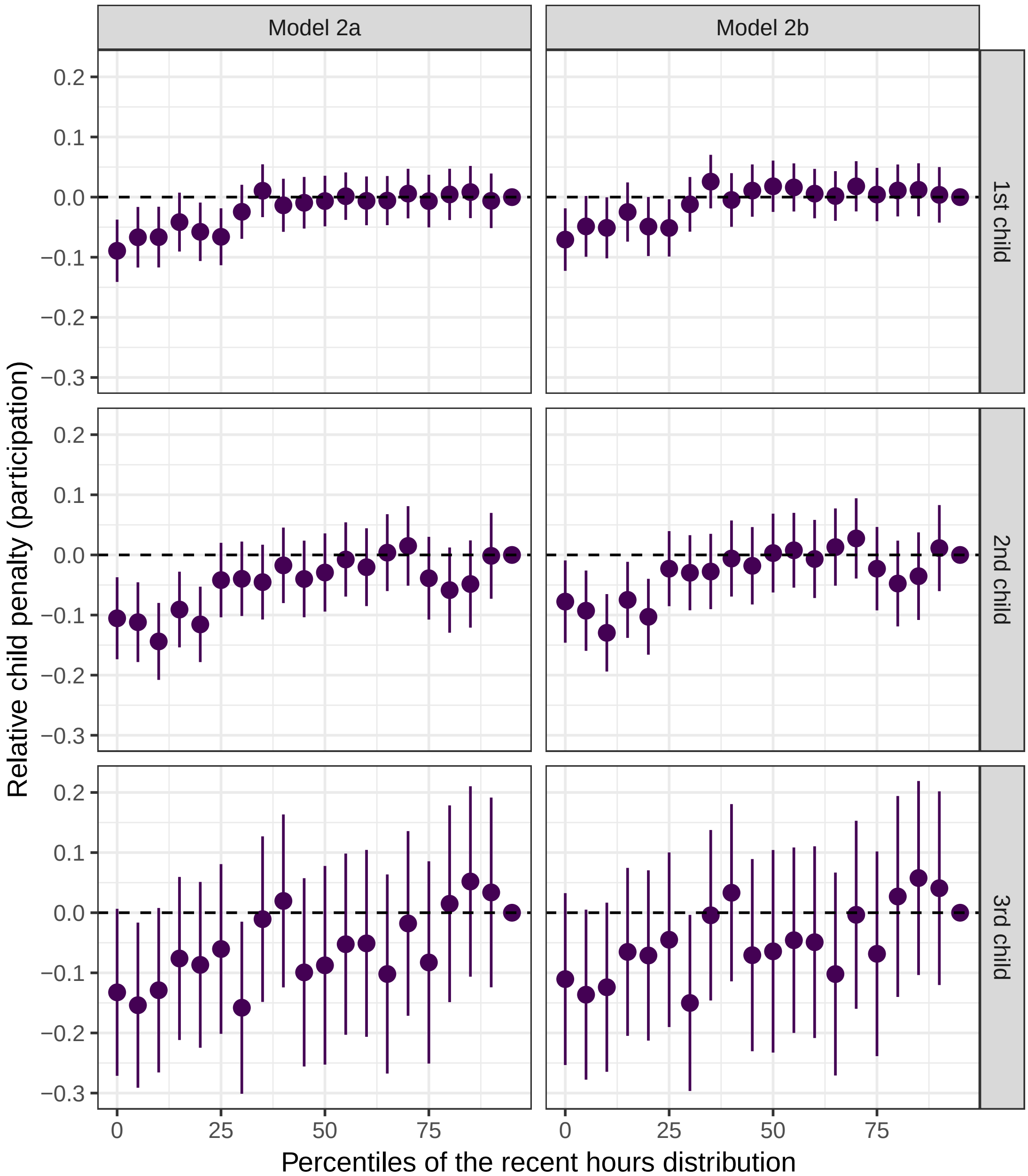
G Other sources of heterogeneity

Figure G.1 – Heterogeneity in the probability to remain in employment one year after childbirth: education



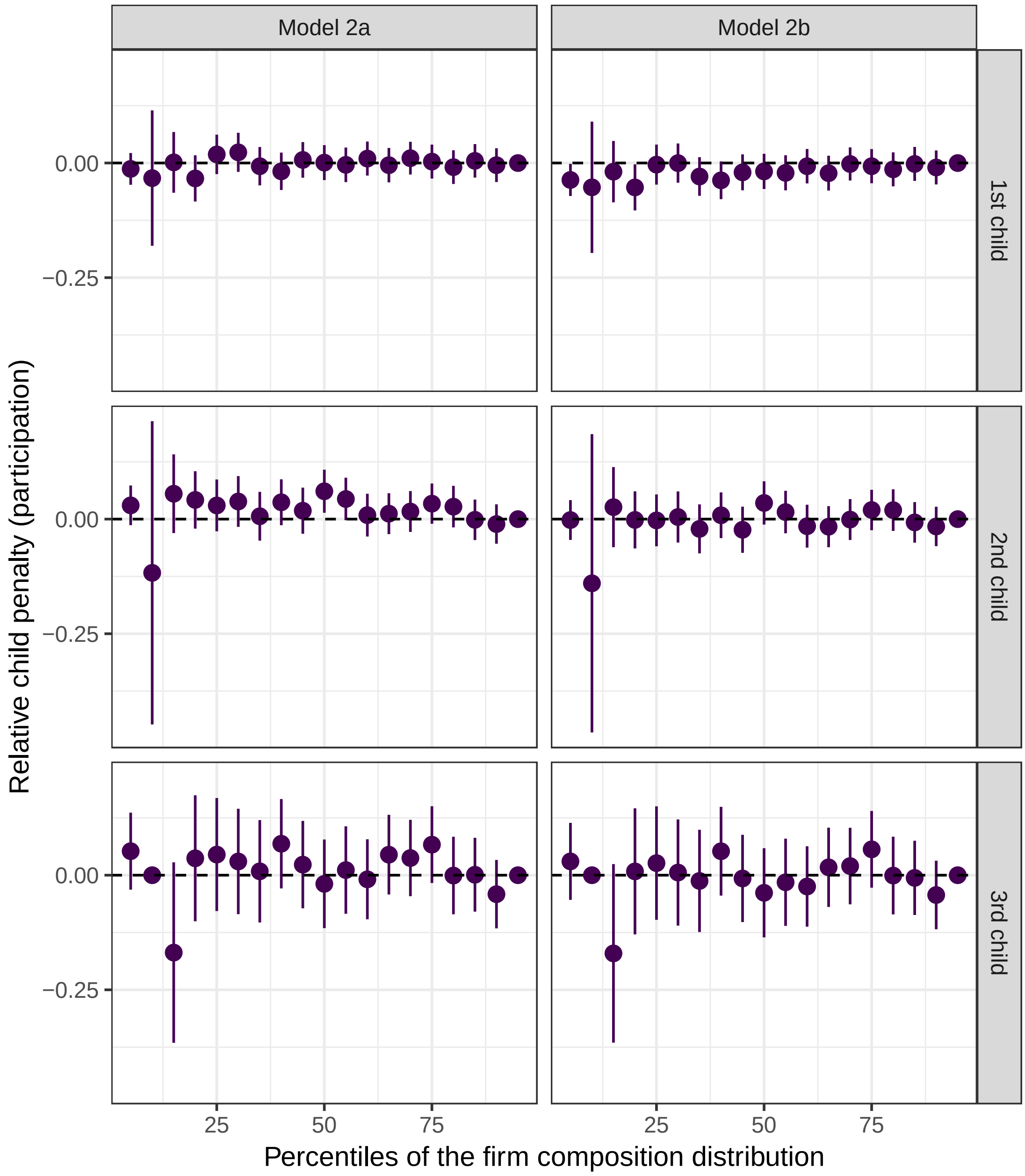
Estimates of the coefficients related to childbirth for women in a linear probability model that interacts a double-difference setting with gender and rank in the recent wages distribution, education, in the distribution of recent paid hours and in the distribution of firm composition (16). Individuals with missing data regarding education are taken as the reference. Outcome is a dummy for participating in the labor market at time $t+1$. Model 2a includes no controls; model 2b controls for year, age, industry and 1-digit occupation within each cell. Standard errors are clustered at the individual level. Sample includes individuals up to age 55 at time t.

Figure G.2 – Heterogeneity in the probability to remain in employment one year after childbirth: past labor supply decisions



Estimates of the coefficients related to childbirth for women in a linear probability model that interacts a double-difference setting with gender and rank in the recent wages distribution, education, rank in the distribution of recent paid hours and rank in the distribution of firm composition (16). Individuals that belong to the highest percentile group of recent paid hours are taken as the reference. Outcome is a dummy for participating in the labor market at time $t+1$. Model 2a includes no controls; model 2b controls for year, age, industry and 1-digit occupation within each cell. Standard errors are clustered at the individual level. Sample includes individuals up to age 55 at time t.

Figure G.3 – Heterogeneity in the probability to remain in employment one year after childbirth: firm composition



Estimates of the coefficients related to childbirth for women in a linear probability model that interacts a double-difference setting with gender and rank in the recent wages distribution, education, rank in the distribution of recent paid hours and rank in the distribution of firm composition (16). Individuals whose main employer in $t-1$ was in the highest percentile group in terms of the share of part-time working women among its employees are taken as the reference. Outcome is a dummy for participating in the labor market at time $t+1$. Model 2a includes no controls; model 2b controls for year, age, industry and 1-digit occupation within each cell. Standard errors are clustered at the individual level. Sample includes individuals up to age 55 at time t.

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