Dissecting child penalties*

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Abstract

We relate mothers' children-related labor earnings losses, child penalties, to their location in the distribution of potential hourly wages. Using French administrative data and based on an event study approach, we show that the magnitude of these earnings losses decreases steeply along that distribution. This heterogeneity is the result of low-wage mothers leaving the labor market and more frequently reducing their working hours. By contrast, fathers' labor market outcomes do not vary upon the arrival of children, regardless of their location in the distribution of potential hourly wages.

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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 in developed countries (Juhn and McCue, 2017; Kleven, Landais, and Søgaard, 2019). Surprisingly, reproductive biology explains actually very little of these penalties (Kleven, Landais, and Søgaard, 2021). Indeed, for this explanation to hold would require women to be much more productive than men in child-rearing activities, in ways totally unrelated to reproductive biology.

In this paper, we emphasize women's *absolute* labor market productivity, as opposed to their *relative* within-household productivity, as a key determinant of their child-related labor market outcomes. Specifically, we show that the trade-off that mothers face between time spent outside the labor force, presumably devoted to child-rearing activities and home production, and their foregone labor earnings is a crucial determinant of the magnitude of the child penalty. To do so, we contrast women whose opportunity cost of time spent outside the labor market is very different. These differences in opportunity costs are well approximated by differences in post-childbirth potential hourly wages. However, post-childbirth potential hourly wages are not observed for mothers who choose to leave the workforce due to children. Additionally, these wages could themselves be affected by children-related labor market outcomes. This would be the case if time spent outside the labor market translates into a slower accumulation of labor market-specific human capital. As a result, we consider instead pre-childbirth hourly wages, averaged over several years. Because (potential) hourly wages are strongly correlated over time when restricting to a single worker, this gives us a reasonable proxy of potential post-childbirth hourly wages that is affected by neither the sample selection nor the simultaneity bias. In the end, we estimate the heterogeneity of the consequences of childbirth along the distribution of pre-childbirth wages.

We consider the short-run (one-year) to long-run (ten-year) impacts on several labor outcomes: total labor earnings, hourly wages, and annual working hours decomposed into two margins, the number of working days and the number of working hours per day. Our empirical strategy embeds a nonlinear difference-in-difference framework within a nonparametric ranking of individuals along the hourly wage distribution; the latter aims precisely at depicting the heterogeneity in individual labor market trajectories along the wage distribution. Our treatment group consists of parents with n children, while our control group contains parents with exactly $n-1$ children. Our difference-in-difference approach is nonlinear: it is set in a multiplicative form. This makes it possible to decompose, in an accounting sense, the causal effect of parenthood on labor earnings as the sum of adjustments on different margins of labor market outcomes, plus the changes in the wage rate.

We apply this method to French administrative data, namely, the DADS panel, a comprehensive linked employer-employee dataset[1] that covers the period from 1995 to 2015 and contains information on individuals' labor earnings and paid hours. This panel is merged with the 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 the above-mentioned control and treatment groups at specific locations of the hourly wage distribution.

2 Related literature and contribution

Childbirths tighten time constraints and shift women's labor supply as well as labor market outcomes, which helps explain a substantial share of the gender pay gap as shown by, 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, 2019). More generally, childbirths have been shown to explain a significant share of the aggregate gender gap, though there is no consensus on the exact share or 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, 2019).

[1] Filling out the DADS form is a mandatory part of the process of paying payroll taxes.

The most prominent contribution to child penalties likely stems from children-induced career interruptions and adjustments in labor supply, which in turn results in human capital depreciation (Meurs, Pailhé, and Ponthieux, 2010; Ejrnæs and Kunze, 2013; Adda, Dustmann, and Stevens, 2017). Other channels involve reduction in work effort (Becker, 1985; Hersch and Stratton, 1997) and mothers having a strong preference for time flexibility (Anderson, Binder, and Krause, 2003; Goldin, 2014), which can generate 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 to the causes of such decisions, two views can be contrasted. The first builds on the model of time allocation proposed by Becker (1981), based on the comparative advantage between the labor market and home production, i.e., on specialization. The second view, related to preferences and norms, refers to the identity model of Akerlof and Kranton (2000) 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 the sense that is compatible with gender-specific prescriptions.

Cross-country comparisons (Kleven et al., 2019), as well as comparisons of biological and adoptive families (Kleven, Landais, and Søgaard, 2021), different- and same-sex couples (Andresen and Nix, 2021), or families belonging to different linguistic groups (Steinhauer, 2018), and lastly careful investigations of numerous family policy reforms (Kleven et al., 2020) all suggest that: (i) holding constant norms and preferences, differences in comparative advantage do not translate into differences in child penalties; (ii) conversely, holding constant the comparative advantage, heterogeneity in exposure to different norms is strongly correlated with differences in child penalties. An exception to this trend would be Angelov, Johansson, and Lindahl (2016) who find substantial heterogeneity in child penalties depending on parents' relative potential earnings.

This paper is also related to a few additional studies that have contrasted child penalties among individuals characterized by mothers' labor market opportunities. Firstly, following Goldin (2014), Bütikofer, Jensen, and Salvanes (2018) compare child penalties across occupations among top earners. Bazen and Périvier (2022) and de Quinto, Hospido, and Sanz (2021) compare child penalties across different education levels in France and Spain. Rabosto and Bucheli (2021) and Andrew et al. (2021) estimate heterogeneity in child penalties by hourly wages category in Uruguay and in the UK. Both papers find that the effects on labour market outcomes are much larger in the bottom of the wage distribution. We go a step further on French data (i) by showing that the effect of hourly wages remains when the impact of education is netted out, hence somehow disentangling both dimensions, and (ii) by estimating child penalties in absolute terms (rather than relative to the counterfactual earnings level). The latter is important because income sounds like a more policy-relevant dimension than education, thinking of means-tested policies, for instance. Secondly, a small body of sociological literature has been devoted to the distributional impact of the child penalty, following Budig and Hodges (2010). Due to methodological issues regarding the interpretation of quantile regression coefficients, it, however, remains difficult to identify the main lessons from this literature (see Killewald and Bearak, 2014; Budig and Hodges, 2014; England et al., 2016).

The above empirical strategies rely on an evaluation of the causal impact of parenthood on labor outcomes, which requires overcoming the issue of endogeneity of fertility decisions (see e.g. Lundberg and Rose, 2000; Miller, 2011). Kleven, Landais, and Søgaard (2019) compare OLS and IV methods to address that concern: it turns out that the causal effect of the third childbirth, estimated by 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, and develop additional tests that enable us to show that endogeneous fertility decisions likely do not affect our results.

Lastly, this paper is relevant to the analysis of 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 sole explanation: child penalties are, if anything, smaller at the top of the distribution. Yet vertical segregation may result from (even small) motherhood penalties: due to statistical discrimination, the generosity of parental leave systems may cause employers to place fewer women in top positions (Datta Gupta, Smith, and Verner, 2008; Albrecht, Thoursie, and Vroman, 2015).

3 Data and institutional background

3.1 Data

Our analysis is based on a large panel of French salaried employees, namely, the longitudinal version of the *Déclarations Annuelles de Données Sociales* (DADS). By law,[2] French firms have to fill out the DADS form – an annual form that is the analogue of the W-2 form in the US – for every employee subject to payroll taxes. Starting from 1967, the panel covers individuals born in October of even-numbered years. As of 2002, the panel contains information on individuals born on January 2-5, April 1-4, July 1-4 and October 1-4 regardless of the parity of their year of birth; these (more or less) first four days of each quarter correspond to the birthdays of individuals for whom we obtain census records in addition to labor market characteristics. 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 individuals' careers, the data is of exceptional quality and has low measurement error in comparison with survey data, in addition to a large sample size and no top-coding. Interestingly, administrative data enable us to follow individuals who move, for instance.

The database contains detailed information about gross and net wages, days worked, paid hours,[3] other job characteristics (the beginning, duration and end of a period of employment, seniority, and part-time employment), firm characteristics

[2] The absence of DADS as well as incorrect or missing answers are punished with fines.
[3] This information has been available since 1995 only.

(industry, size, and region) and individual characteristics (age and gender). This is true as long as individuals work in the private sector: in particular, we neither observe business income nor earnings from self-employment. We are also able to recover the numbers of male and female employees at each firm by resorting to the cross-sectional version of the DADS to this end and using the linked employer-employee dataset (LEED) dimension. Our main variables of interest are (i) net real annual labor earnings defined as the sum of all salaried earnings over all employers, (ii) time worked, measured as the number of paid hours as well as the number of days worked, and (iii) hourly wages defined as the ratio of annual earnings and time worked. In Appendix A, we provide some further details on the measurement of earnings and time worked. 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) mothers are considered to be salaried employees during the entire duration of their maternity leaves; (iii) the number of hours worked during the maternity leave is equal to 0, and (iv) the number of hours worked (resp., hourly wages) is overestimated (resp., underestimated) for workers that are not paid by the hour in years in which they take maternity leave.

Individuals are identified by their NIR, a 13-digit social security-like number that allows to merge the DADS panel with *Échantillon démographique permanent*. The latter is a longitudinal version of the census that includes births and marriage registers as of 1968. However, information on childbirth is missing before 2002 for individuals born in January, April or July. For this reason, we consider first individuals born on October 1-4. Additionally, some childbirth-related data is available in administrative birth registers for individuals born October 2-3; however, it was incomplete during the 1990s (for details, see Wilner, 2016): as a result, for these individuals we rely on the census rather than birth records.[4] Finally, partial data 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 salaried male and female employees in the

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

private sector at the exclusion of agricultural workers and household employees.[5] We restrict our analysis to individuals aged 20 to 60 living in mainland France[6] between 1998 and 2015. This requires to restrict our attention to individuals born on even-numbered years, given that individuals born on odd-numbered years are not covered by the panel before 2002. We are therefore relying on a representative sample at rate 0.5%.[7]

The empirical analysis described in Section 4 requires selecting individuals with a strong attachment to the labor market. We specify that these individuals be employed in the private sector for at least two years between $t-5$ and $t-2$ in addition to being present in $t-1$.[8] To deal with individuals with very low labor participation, an individual is considered employed at t if her paid hours exceed 1/8 of the annual duration of work (1,820 hours as of year 2002), if her total employment duration exceeds 45 days per year and if her hourly wage exceeds 90% of the minimum wage. We also winsorize labor earnings at the quantile of order 0.99999 to avoid outliers. We exclude individuals for which one observation has the ratio of net labor earnings to gross labor earnings less than (resp., greater than) 1/100 of (resp., 100). Our working sample has approximately 1.4 million individuals-years of observations, corresponding to nearly 155,000 workers.

3.2 Summary statistics

Table 1 provides several statistics for the selection process. First, censoring of observations with low numbers of paid hours or low employment duration is illustrated. Second, the restriction to individuals for whom data are available for two

[5]During her career, an individual may work in the public sector, be self-employed or 'hourly' worker at some point, though. In such cases, we only avail of information related to employment spells where she belongs to the private sector. In particular, we do not necessarily select those individuals out of our sample, except when they do not meet criteria detailed just below.

[6]At the exclusion of the 5 French overseas departments (French Guiana, Guadeloupe, Martinique, Mayotte, and Réunion).

[7]On top of this longitudinal sample, we also rely on a comprehensive version of the DADS dataset that allows us to track all salaried employees from one year to the next to devise additional tests of our identifying assumption: see Appendix G.

[8]The core results of this paper rely on years t from 1998 to 2015. As a result, because data are only available from 1995, the inclusion condition is slightly stronger for years 1998 and 1999. However, dropping these years and focusing only on years 2000 to 2015 does not change our estimates, as shown in Figures F.9 and F.10.

years between $t-5$ and $t-2$ in addition to years $t-1$ and t is applied. As expected, both steps increase average hourly wages within a given gender, age group and industry. The selection is harsher for women than it is for men, as women are more likely to experience career interruptions. Censoring reduces the share of younger workers slightly, which is consistent with entry into the workforce through shorter and non-full-time employment spells; selection has the same effect for the same reason. Censoring reduces the share of workers in the service industry who are more likely to have short employment spells and to work part-time. Selection also reduces the share of service industry workers among men and the share of trade industry workers among women, as these individuals have less stable employment histories than those of their counterparts working in other industries.

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

Figure G.1 displays the number of childbirths both in the raw EDP dataset and in our final sample.[9] Because we focus on childbirths that occur after individuals have experienced rather stable employment for 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 disregard about a half of women who experienced childbirth between 1998 and 2015. These proportion amount to roughly 60% for men during the same period.

3.3 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, whereby the income tax rate depends on the number of children in a household, (ii) various child benefits, and (iii) some other welfare benefits, such as bonuses included in retirement pensions that depend on realized fertility,

[9]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.

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; they were first unpaid, and subsequently became 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 granted a woman a 16-week maternity leave consisting of 6 weeks before childbirth and 10 weeks after. Starting from the arrival of the third child, the total duration becomes 26 weeks (8+18), and maternity leave duration may increase to 46 weeks in the case of multiple births. Maternity leaves also have a minimum duration of 8 weeks, consisting of 2 weeks before childbirth and 6 weeks after. By contrast, paternity leaves have granted fathers an 11-day leave since 2002 only.

Paternity leaves came in force in 2002 in addition to birth leaves that amounted to 3 consecutive days following childbirth. Such a leave grants a father an 11-day leave that is fully covered, up to some threshold, by social insurance. Its duration may reach 18 days in the case of multiple births but always includes weekends and public holidays. The idea of extending that duration has recently attracted some attention; the French government has asked for an internal *ex ante* evaluation, and in 2020 the decision was made to extend the paternity leave to 25 calendar days.

In addition to the above leaves, there are various parental allowances that were merged in 2004 into the *Prestation d'Accueil du Jeune Enfant* (PAJE). 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* (CMG)), and some child benefits granted when parents interrupt their careers or work part-time (previously *Complément Libre Choix d'Activité* (CLCA) and now *Prestation Partagée d'Éducation de l'enfant* (PreParE)). In fact, these child benefits date back to 1985 and appeared with the creation of *Allocation Parentale d'Éducation* (APE) initially restricted to mothers of 3 or more children. APE was extended to mothers of 2 children in 1994, and was replaced by the CLCA in 2004, becoming effective with the first childbirth and providing a fixed not-means-tested amount for the maximum duration of 6 months. The CLCA was replaced in 2015 by PreParE that introduced incentives to split the leave between parents; it amounted to approximately €400 per month in the case of career interruption and to nearly €200 in the case of 80% part-time work. Several papers have shown that these benefits induce mothers to reduce their labor supply (Choné, Le Blanc, and Robert-Bobée, 2004; Piketty, 2005; Lequien, 2012; Joseph et al., 2013). Another means-tested benefit, the *Complément familial*, is attributed to families with 3 children or more; it amounts to slightly less than €300 monthly.

In contrast, other policies favor participation in the labor force by decreasing the cost of childcare; an example of such a policy is CMG, namely *Complément de libre choix du mode de garde*, which is not means-tested, and entails payroll tax cuts or income tax credits. A typical tax credit amounts to 50% of childcare expenditures up to some threshold that depends on the type of chosen daycare. The annual threshold is €2,300 for childcare providers or wet nurses, but it may be as high as €13,500 (€16,500 in the first year) for nannies employed at home. 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,[10] family structure and geographic location) but 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).

Considering labor supply, the current family insurance scheme therefore provides contradictory incentives: on the one hand, PreParE should reduce labor supply after childbirth, as well as the *Complément familial* from the third childbirth onwards; on the other hand, CMG should preserve it. Determining which effect dominates is an empirical task; yet the answer to that question depends crucially on the location in the wage distribution. Mothers at the top of the wage distribution will not be particularly responsive to PreParE since career interruption and part-time employment are more costly for them. In contrast, the combination of PreParE benefits (€200) with a reduction of childcare expenditures

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

is worth considering for low-earnings mothers: e.g., at the minimum wage (slightly above €1,200 per month), a switch to 80% part-time work means a monthly cut of approximately €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 to mothers located at the bottom of the wage distribution.

On top of previous family benefits, other means-tested welfare benefits increase with the number of children: not claiming to be exhaustive, that list includes the PPA or *Prime pour l'activité*, a French equivalent of the U.S. EITC,[11] the RSA (*Revenu de solidarité active*), i.e. a minimum income[12] that is an important part of the social safety net, pensions bonuses granted to parents[13] and various housing allowances[14]. Moreover, parents eligible to means-tested allowances are entitled to borrow at reduced rates. Lastly, family-friendly policies may be available within firms; e.g., employers may provide childcare services to 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.

As a result of this overall family-oriented social insurance scheme, low-wage women are more likely to reduce their labor supply following childbirth than mothers at the top of the wage distribution, for instance by entering part-time employment. The family insurance scheme and childcare subsidies are indeed designed in such a way that they magnify those financial incentives. In sum, we expect that labor supply responses to childbirth be heterogeneous along the hourly wage distribution, namely monotone: they should decrease, in absolute, along that ladder.

[11]With a typical phasing out from €595.25 for monthly earnings of €687.35 to €173.22 at €1,398 monthly.

[12]€607,75 monthly without child, and a supplementary bonus of €200 per child.

[13]MDA stand for *Majorations de Durée d'Assurance* and consist in extra quarters of coverage.

[14]In the private housing sector, this refers to the APL, standing for *Aide Personnalisée au Logement*, while the ALS, or *Allocation de Logement Social*, concerns social housing. The typical bonus amounts to €100 per child.

4 Empirical analysis

Our main outcome of interest is total annual labor earnings of individual i during year t; we denote such earnings by $\tilde{y}_{it}$. We decompose them into four components: d_{it} is a dummy variable for participation; $\tilde{x}_{it}$ represents the employment duration in days, and is between 0 and 360;[15] $\tilde{h}_{it}$ denotes the average number of paid hours per day during year t, and lastly $\tilde{w}_{it}$ is the average hourly wages of individual i during year t. Hence

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

4.1 Normalization

Providing estimates of the causal effect of childbirth by comparing parents and non-parents requires netting out other lifecycle effects as confounding factors; e.g., 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 in response to fertility decisions, and hence should be taken into account as part of child penalties instead of being controlled for. As a result, the first step of our empirical framework derived from that of Guvenen et al. (2021) consists of normalizing earnings and each of earnings' components with respect to age, cohort and period. Let $\tilde{z}$ denote either labor earnings or one of its components 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 cost of normalizations, which we detail in Appendix C. In this paper, the choice of normalization is insignificant, given that we rely on the sum $\hat{\lambda} + \hat{\mu} + \hat{\nu}$ and never

[15]The number of days in a year is capped at 360 in DADS.

use these components separately. Note that our estimation sample includes people with and without children, all of them contributing to the identification of age, period, and cohort effects.

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

$$z_{it} = \frac{\tilde{z}_{it}}{\exp(\hat{\lambda}^z_{cohort_i} + \hat{\mu}^z_{age_{it}} + \hat{\nu}^z_t)} \tag{3}$$

An accounting decomposition similar to that of (1) is used for normalized earnings:

$$y_{it} = d_{it} x_{it} h_{it} w_{it} \tag{4}$$

4.2 Ranks in the hourly wage distribution

Our empirical strategy embeds a difference-in-difference setting within a framework that aims at modeling heterogeneity in the consequences of childbirth along the hourly wage distribution. To this end, 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 such groups based on a measure of recent hourly wages:

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

We compute this measure for individuals who participate in year $t-1$ and at least twice between years $t-5$ and $t-2$ (i.e., provided that $d_{i,t-1} \sum_{\tau=t-5}^{t-1} d_{i\tau} \geq 3$). Within each age × year cell, we rank workers according to their recent wages $W_{i,t-1}$. We use this ranking to divide the sample into 5 groups depending on their location in that distribution. Hence we assume that workers within each age × year × recent wage 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 Table 1).[16]

One may be concerned by sample selection arising for instance due to the wage profile increasing with age within our sample (Table 1). This could indeed bias our results in favor of larger estimated child penalties if high-income individuals delayed the timing of their first birth, for instance. To alleviate this concern, we first provide a robustness check (see Appendix Figures E.3 and E.4) in which we assign wage bins based on the rank in the hourly wage distribution at age 26, which is itself based on hourly wages measured between 20 and 25, rather than relative to the year of childbirth. The chosen threshold, 26, is an empirical choice guided by the fact that youngest individuals are mechanically selected out of our sample due to our inclusion criteria (our individuals being rather strongly attached to the labor market). Reassuringly, our results remain mostly unaltered by this methodological choice. Second, in a reweighing exercise (see section 5.1), we somehow neutralize the effect of age at childbirth, which further mitigates such concerns.

4.3 Difference-in-difference strategy

Our estimates of the consequences of childbirth are based on a difference-in-difference approach. The endogeneity of fertility decisions is often regarded as a key issue, but recent results suggest that it is not an empirical problem (Kleven, Landais, and Søgaard, 2019). We discuss the plausibility of the assumption that fertility decisions are exogenous, and devise additional tests of its validity in Appendix G as well as concerns about mean reversion driving our results.

The binary treatment consists in the arrival of one's first child during year t. Our control group for is composed of individuals of the same gender without child. The main identifying assumption is that, absent the children, the evolution of labor outcomes among parents would have paralleled that of labor outcomes of individuals who remain without children. Year $t-1$ is regarded as the reference year; by construction, all individuals participate in the labor market during year $t-$

[16] We nevertheless provide a robustness check in which we rank observations into gender-specific wage bins instead, see Appendix Figures E.5 and E.6.

1.

Our quantity of interest corresponds to the effect of parenthood, which begins with the arrival of the first child but also encompasses the consequences of higher-order births. Appendix I further investigates the impact of second and third children by comparing parents with n children to those with exactly $n-1$ children.[17]

Due to the omission ("right-censoring") of unknown but relevant data on fertility decisions taken after 2015, individuals belonging to the control group may actually have children born after 2015; we address this issue in Appendix F.

The same childless individual intervenes multiple times in our estimation because she belongs to the control group all along her life-cycle. 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 who belong to the same quintile group 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.

The impact of children on earnings k years after the arrival of the first child for individuals of gender g at rank r in the recent wage distribution is given by

$$\beta_{g,r}^{y,k} = \underbrace{\log\left(\frac{\mathbb{E}[y_{i,t+k}|b_{it}=1, r_{it}=r, g_i=g, t \in \mathcal{T}_k]}{\mathbb{E}[y_{i,t-1}|b_{it}=1, r_{it}=r, g_i=g, t \in \mathcal{T}_k]}\right)}_{\text{Treated}} - \underbrace{\log\left(\frac{\mathbb{E}[y_{i,t+k}|c_i=0, r_{it}=r, g_i=g, t \in \mathcal{T}_k]}{\mathbb{E}[y_{i,t-1}|c_i=0, r_{it}=r, g_i=g, t \in \mathcal{T}_k]}\right)}_{\text{Control}} \tag{6}$$

where b_{it} is a dummy for the arrival of the first child during year t, c_{it} is a dummy for individuals who remain childless 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.[18]Notably, we do not

[17]We thus restrict our attention to the first three childbirths, namely 96% of childbirths.

[18]given the time-period that our dataset covers, this implies $\mathcal{T}_k = [\![1998, 2015-k]\!]$.

match treatment and control groups according to age, period and cohort; this is made possible by our normalization of the data with respect to these dimensions that comes as a first stage in our empirical approach.

Considering the causal impact of childbirth $\beta_{g,r}^{y,k}$ 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 first child bears the same consequences if the child was born in 1998 as it does if she was born in 2015. We assess the plausibility of this assumption, among others, in Appendix G. Importantly, considering $k < -1$ allows us to verify that trends are parallel before childbirth.

Several econometricians have recently warned that frequently used approaches to difference-in-difference with multiple treated groups, known as two-way fixed effects regressions, may result in biased results (see de Chaisemartin and D'Haultfœuille, 2022, for a survey of this literature). In our setting, this issue would arise because two-way fixed effects regressions rely on comparisons between parents who have their children at different times, not only when some have had their first child and the others are yet to have theirs, but also when both have children. The latter comparison is only informative as to the consequences of children under an additional (often implausible) assumption that these consequences are the same regardless the timing of childbirths. However, our approach does not rely on two-way fixed effects regressions: instead, our estimates are directly obtained from the comparison of mean outcomes across groups, which allows us to explicitly rule out previous (and so-called) forbidden comparisons.

Decomposition (7) states that average normalized earnings growth can be represented as a sum of its four components, plus a selection term due to the fact that individuals who participate in the labor market in year $t+k$ may not have the exact same past earnings $y_{i,t-1}$ as those who do not participate:

$$
\begin{aligned}
\underbrace{\log\left(\frac{\mathbb{E}\left[y_{i,t+k}\right]}{\mathbb{E}[y_{i,t-1}]}\right)}_{\text{Labor earnings changes}} = & \underbrace{\log\left(\mathbb{P}(d_{i,t+k}=1)\right)}_{\text{Participation}} \\
& + \underbrace{\log\left(\frac{\mathbb{E}[y_{i,t-1}|d_{i,t+k}=1]}{\mathbb{E}[y_{i,t-1}]}\right)}_{\text{Selection}} \\
& + \underbrace{\log\left(\frac{\mathbb{E}\left[\boldsymbol{x_{i,t+k}} h_{i,t-1} w_{i,t-1}|d_{i,t+k}=1\right]}{\mathbb{E}\left[x_{i,t-1} h_{i,t-1} w_{i,t-1}|d_{i,t+k}=1\right]}\right)}_{\text{Employment Duration Changes}} \\
& + \underbrace{\log\left(\frac{\mathbb{E}\left[\boldsymbol{x_{i,t+k}} \boldsymbol{h_{i,t+k}} w_{i,t-1}|d_{i,t+k}=1\right]}{\mathbb{E}\left[\boldsymbol{x_{i,t+k}} h_{i,t-1} w_{i,t-1}|d_{i,t+k}=1\right]}\right)}_{\text{Hours-per-day Changes}} \\
& + \underbrace{\log\left(\frac{\mathbb{E}\left[\boldsymbol{x_{i,t+k}} \boldsymbol{h_{i,t+k}} \boldsymbol{w_{i,t+k}}|d_{i,t+k}=1\right]}{\mathbb{E}\left[\boldsymbol{x_{i,t+k}} \boldsymbol{h_{i,t+k}} w_{i,t-1}|d_{i,t+k}=1\right]}\right)}_{\text{Hourly Wage Growth}}
\end{aligned}
\tag{7}
$$

This decomposition is made in an accounting sense.[19] Specifically, a causal interpretation of this decomposition would require employment decisions to be mean independent of changes in the wage rate, which seems unlikely. In Appendix D, we detail the computation 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 impact of childbirth on earnings; we write it as $\beta^y = \beta^s + \beta^d + \beta^x + \beta^h + \beta^w$, where β^s stands for the selection term, and the four other terms correspond to each component of labor earnings (for readability, we omit all other unnecessary indices).

[19]This decomposition is akin to the accounting decomposition of log-earnings changes as the sum of log-hourly wages and log-hours worked changes that is commonly used in labor economics (see e.g. Lachowska, Mas, and Woodbury, 2020), while having the advantage of not conditioning on positive earnings. As a result, it allows to quantify, in an accounting sense, the contribution of the extensive margin of employment, which is relevant in this particular setting.

5 Results

5.1 Heterogeneous consequences of childbirth

First, we assess the consequences of childbirth on labor outcomes of men and women by relying on the accounting framework. Our estimates of the impact of parenthood on individuals' total labor earnings are shown in Figure 1 for women and in Figure 2 for men. We plot those estimates for $t + k \in \{t - 5, ..., t + 10\}$. Tables 2 and 3 display the corresponding estimates one year, five years and ten years after the arrival of children.

Mothers experience large earnings losses after childbirth relative to women who earned similar hourly wages a few years before. In average, earnings losses due to the arrival of a first child amount to approximately 40 log-points (33%) five years after her birth. This decline persists up to at least ten years after the arrival of children: on average, earnings losses amount to approximately 21 log-points (20%) by this time. All components contribute to these losses: after the arrival of a child, mothers are more likely to leave employment, work fewer days, work fewer hours per day and earn lower hourly wages than women belonging to our control groups. Nevertheless, adjustments in participation and working hours seem to be driving these large earnings losses. This empirical evidence is consistent with previous findings in the literature: Meurs and Pora (2019) estimate that the child penalty amounts to 40% in the short-run and to 30% in the long-run.

More interestingly, children-related earnings losses display substantial heterogeneity: low-wage women experience far larger relative earnings losses than do high-wage women. At the bottom of the distribution, women's earnings losses amount to 32 log-points (27%) one year after childbirth, remain at 49 log-points (39%) five years after the arrival of a child, and up to 29 log-points (25%) ten years after. In contrast, women in the top 20% of the hourly wage distribution experience earnings losses of 17 log-points (16%), 19 log-points (17%) and less than 9 log-points (9%), respectively. Our main result is thus that child penalties decrease along the wage distribution as pre-childbirth hourly wage increases.

The decomposition of annual earnings growth into each of its components helps clarify the channels that contribute the most to this pattern. Previous heterogeneity is primarily driven by working time: a childbirth reduces by 12 log-points (11%) the probability that women are employed one year after the arrival of their first child at the bottom of the distribution, but does not decrease that probability by more than 3 log-points (3%) at the top of the distribution. The same holds as time goes by: low-wage women see their salaried employment rate decline, whereas their high-wage counterparts have theirs virtually unaffected by children. Similar differences are observed in terms of days worked, which suggests infra-annual transitions in and out salaried employment being much more frequent among low-wage mothers.

By contrast, working hours responses look much more similar across the hourly wage distribution. Motherhood wage penalties are also much more homogeneous one to ten years after childbirth, roughly amounting to 4 log-points (4%).

Our approach enables us to verify that trends of the treated and control groups before treatment are parallel. While observing parallel trends before treatment is not sufficient to assess the credibility of our identifying assumption,[20] observing large differences in trends between treated and control groups before treatment would cast doubt as to the validity of our design. We observe small differences between groups' earnings in years $t-5$ and $t-2$ with respect to year $t-1$. The difference is slightly positive when considering the arrival of the first child: mothers had slightly slower earnings growth than did non-mothers prior to the first childbirth. However, these differences are less than 12 log-points (1%), which is not much in comparison with earnings differences after childbirth (up to 73 log-points, i.e. 51%). More importantly, these differences vary little along the wage distribution, which is reassuring as far as the identification of heterogeneity of the impact of childbirth on women's labor outcomes is concerned.

Financial incentives provided by the French family insurance scheme, especially through means-tested childcare benefits, are consistent with previous results. It is all the more likely that the estimated impact of birth on labor market outcomes is strictly monotone along the distribution of pre-birth wage. Two competing expla-

[20]This assumption deals with trends in potential outcomes (absent childbirth) after childbirth.

nations may prevail, though: (i) a selection story, namely a correlation between productivity and preference for leisure; but this is less likely after we have controlled for individual fixed effects;[21] and (ii) a demand-based explanation, namely a correlation between productivity and job security such that low-income mothers are more likely to be fired by their employers. Ruling this explanation out could require including firm-parental status fixed effects in our equations. Identification yet would require within-firm mobility by both parents and non parents, which is less likely in smaller firms, and hurts to a limited mobility bias. Again, monotonicity suggests that the underlying mechanism has to do with the opportunity cost of time, while there is a priori no particular reason why this form of employers discrimination should be targeted against low-productivity women only. Also notable is the homogeneity of the estimated labor market penalty across the distribution, in the price sense (i.e., on hourly wages). By contrast, the penalty on working time is heterogeneous, which likely reflects the role of financial incentives on supply-side decisions.

When it comes to men, our estimates suggest that childbirths increase labor earnings slightly, especially through higher participation and hourly wages. The increase in participation is slightly more pronounced for fathers at the top of the wage distribution. An interpretation of previous results is that families consider replacing mothers' contributions to childcare by market services, but not fathers' contributions, possibly because the latter correspond to less routine tasks or to more recreational activities (Craig and Mullan, 2011; Raley, Bianchi, and Wang, 2012) that are harder to externalize.

We then show that additional sources of heterogeneity due to past human capital decisions, which could affect pre-childbirth hourly wages and stem from childcare-related preferences, do not drive our results. First, we replicate our analysis by ranking individuals according to education, hereby estimating the heterogeneity of child penalties in that dimension as in Bazen and Périvier (2022) and

[21] Moreover, in our reweighing exercise below, we do our best to neutralize the impact of observed variables including education but also recent labor participation and age at (counterfactual) childbirth, which thus suggests that these covariates do not act as confounding factors for our estimated childbirth penalties.

de Quinto, Hospido, and Sanz (2021). We find smaller child penalties for more educated mothers (see Figures E.1 and E.2). Second, we estimate child penalties in a counterfactual population, in which the rank in the wage distribution is as much unrelated to education (as well as to past labor participation, firm choice and age at childbirth) as possible, by an appropriate reweighting of the data.[22] Table 4 displays the results we obtain when replicating previous analysis on reweighted data. Though there is slightly less heterogeneity along the wage distribution than in the baseline analysis, the patterns are still extremely similar. This effort to somehow neutralize the impact of education enables us to claim that we have estimated the effect of income net of education on child penalties.

5.2 Absolute child penalties

Previous estimates of child penalties are *relative* in the sense that they correspond to a fraction of pre-birth wages. Converting those penalties into absolute terms can be done by simply multiplying our estimates by average counterfactual earnings in each quintile of the pre-birth wage distribution, which yields Figure 3. Absolute child penalties look much more homogeneous. Three years after childbirth, a short run child penalty of about €5,000 is incurred by all mothers, almost regardless of their pre-birth wages. A possible interpretation is related to the market-valued cost of childcare, which is quite independent from parents' characteristics. According to time use surveys (Champagne, Pailhé, and Solaz, 2015), in 1999 French women devoted 82 minutes per day to childcare, on average, i.e. 500 hours per year. Given the hourly minimum wage rate amounts to €9.23, this monetized time precisely represents about €4,600 annually, a figure that compares well with the nominal child penalty.

[22] To that end, we consider (i) education, measured by the highest degree obtained at the end of studies, as an 8-level variable; (ii) recent labor participation at all margins between year $t-5$ and $t-1$; (iii) the share of females working part-time for the main employer of each individual at time $t-1$; and (iv) the age at (counterfactual) childbirth. We rely on these variables to reweight the data so that within each treatment/control group, the composition does not vary across the recent wage distribution. In this setting, the weight of the observation that corresponds to individual i at time t, who belongs to the treatment (control) group g, and is ranked r in the hourly wage distribution writes $\frac{\mathbb{P}(R=r|G=g)}{\mathbb{P}(R=r|G=g,X=x_{it})}$ where x_{it} corresponds to the observed variables upon which our reweighting procedure is based. Specifically, we take as $\mathbb{P}(R=r|G=g,X=x_{it})$ the predicted probability of belonging to rank r in the distribution based on an ordered Logit.

To the best of our knowledge, this simple exercise is rather new to the literature (Kleven, Landais, and Leite-Mariante (2023) adopt a similar approach on the employment rate, though), and sheds interesting insights on the underlying mechanisms driving observed behavior on the labor market. Previous heterogeneity partly reflects the smaller base for low-wage women. Those women may be unwilling to spend time on the labor market due to the magnitude of that nominal penalty, hence the effects at both extensive and intensive margins documented before. In contrast, high-wage women incur a higher opportunity cost when not spending time on the labor market, and are thus incentivized to maintain both their participation, at the extensive margin, and their number of hours worked, at the intensive margin.

6 Conclusion

This paper investigates whether mothers with different labor market opportunities have different children-related labor market outcomes, which in the end translate into earnings losses. To do so, we contrast the causal effect of children, identified thanks to a difference-in-difference approach, along the pre-childbirth wage distribution. We show that while, regardless of their wages, children have a large and negative impact on mothers' labor earnings, the magnitude of this impact is much larger for those with low potential hourly wages than it is for those with high potential hourly wages. The reason for this is that the former are much more likely than the latter to retreat for the workforce, and to decrease their hours worked in the labor market. By contrast, fathers are very unlikely to change their hours worked upon the arrival of children, regardless of the wage rate.

Differences in potential hourly wages reflect the heterogeneity in the opportunity cost of time, a key determinant of children-related labor market outcomes. This opportunity cost sums up the trade-off between the income generated by the time mothers spend on the market and the costs incurred, namely mothers' foregone contribution to child-rearing. High-wage mothers being much less likely to work less than their low-wage counterparts thus suggests that the former can compensate the latter. Observed behavior is consistent with families willing to resort to market solutions that substitute for maternal childcare, provided that the cost remains lower than about €5,000 annually. By contrast, fathers' labor market outcomes seem almost independent of this cost. This would indicate that in their time allocation problem, families do not view fathers' contribution to child-rearing as a possible substitute for mothers' contribution. Overall, this interpretation helps rationalize why mothers of young children respond strongly to reforms that make child-related career breaks more or less costly (Piketty, 2005; Lequien, 2012; Joseph et al., 2013), while recent reforms that specifically target fathers have close to no impact on their behavior (Périvier and Verdugo, 2021).

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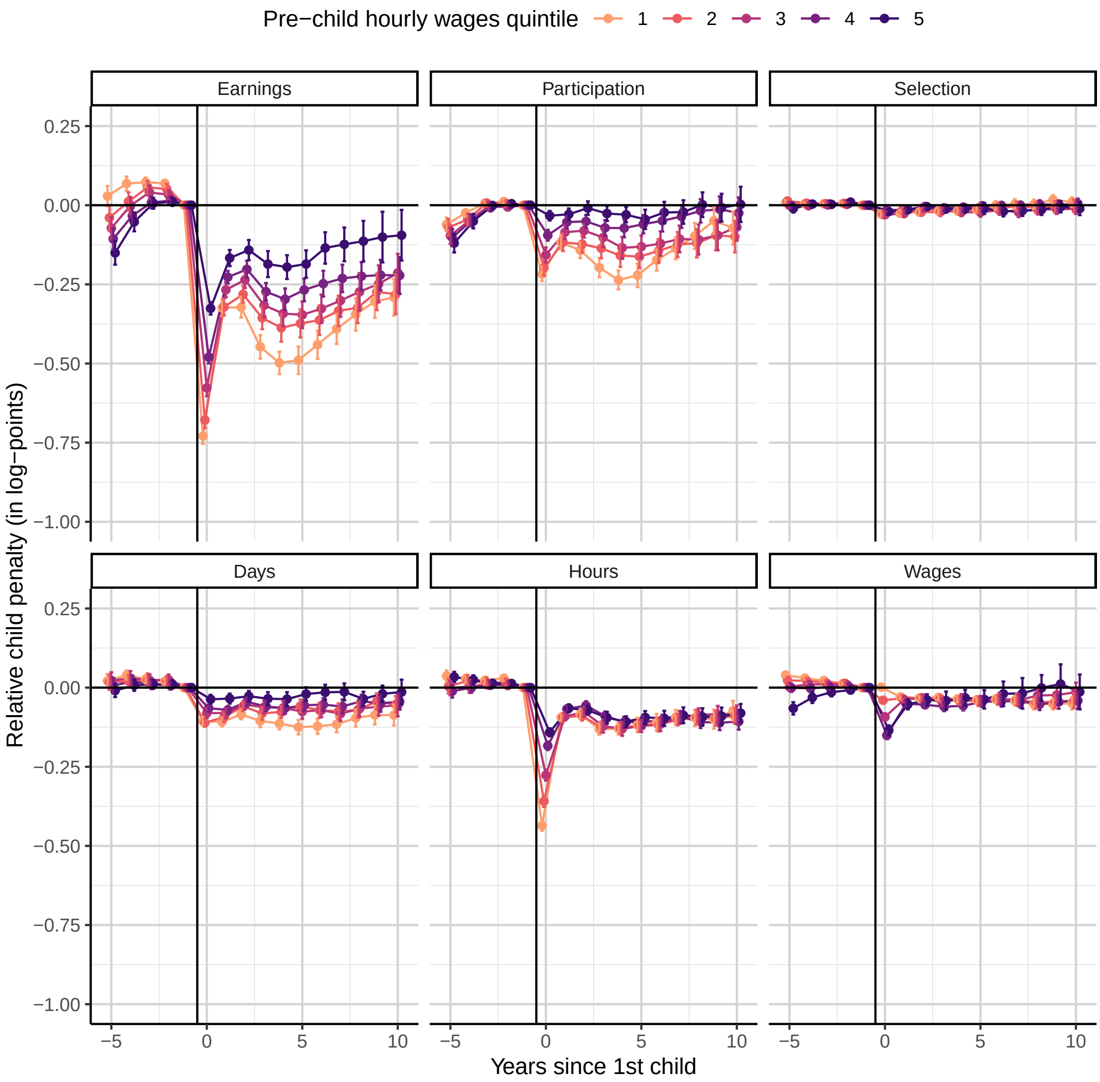
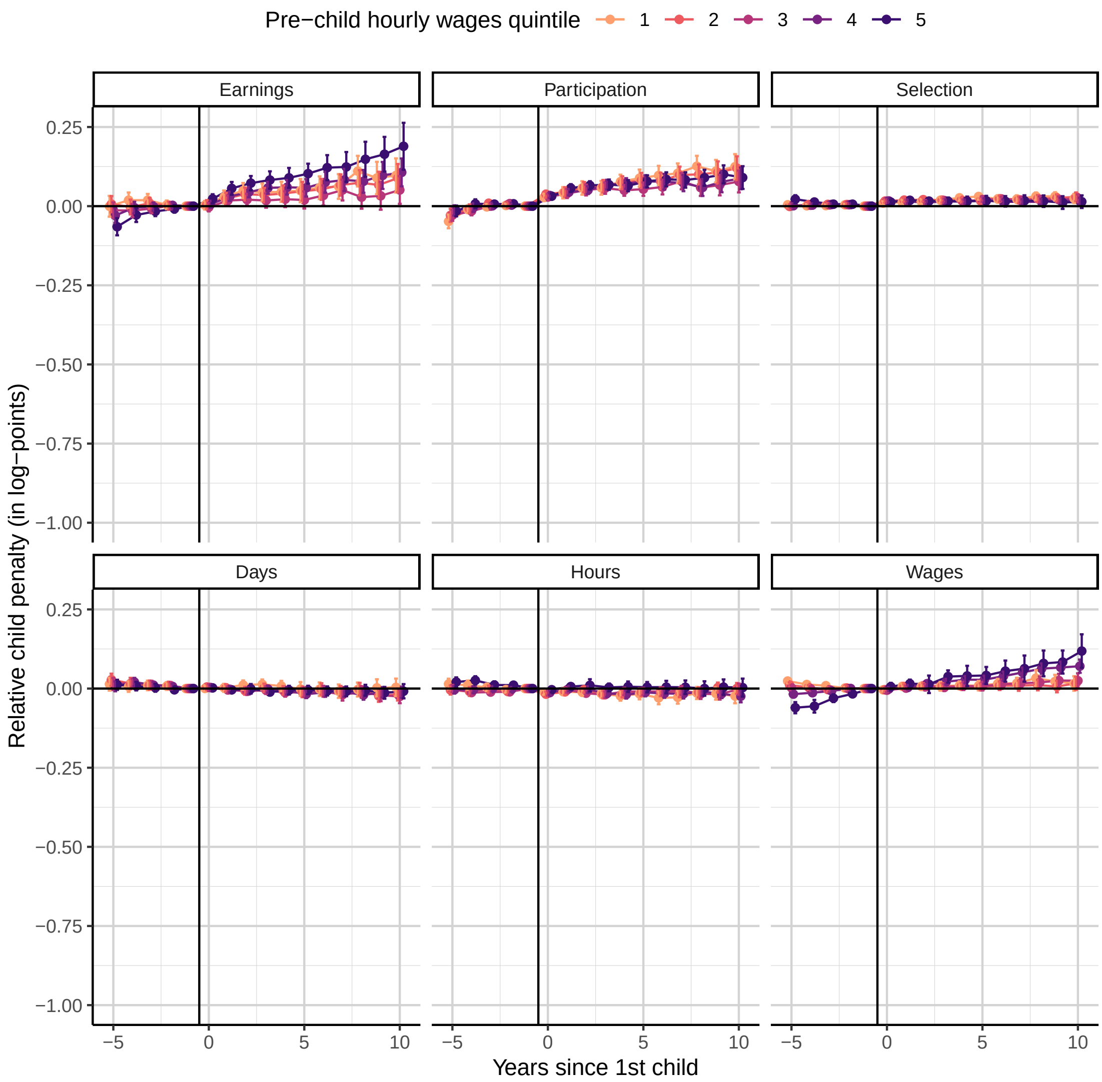
Figures

Figure 1 – Consequences of first childbirth for women's labor outcomes



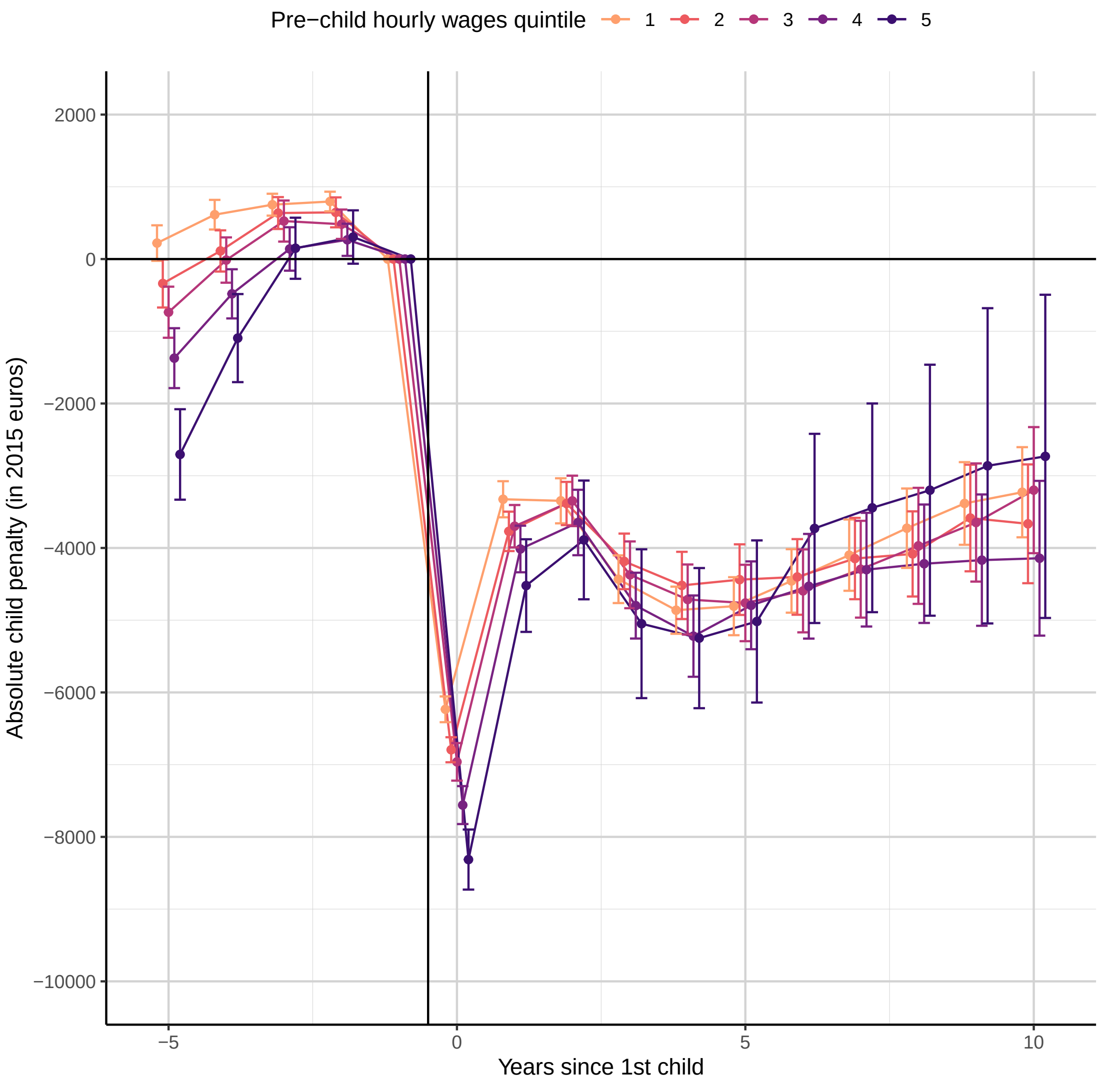
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure 2 – Consequences of first childbirth for men's labor outcomes



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure 3 – Consequences of first childbirth for women's labor outcomes (in absolute terms)



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Tables

Table 1 – Sample selection

	Base sample				Censoring				Final sample			
	Women		Men		Women		Men		Women		Men	
# Obs.	907 202		1 166 815		778 320		1 062 183		581 921		832 477	
# Ind.	97 388		110 818		88 015		104 322		68 235		87 148	
	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												
23-30	23.9	10.0	23.3	10.8	22.7	9.9	21.3	10.7	19.7	10.2	18.7	11.0
30-39	30.0	11.9	30.9	14.2	30.1	11.7	31.2	13.6	30.1	12.2	31.6	14.0
40-49	27.4	12.6	27.2	16.3	28.0	12.4	27.7	15.9	29.1	12.8	28.6	16.2
50-59	18.7	13.2	19.6	18.0	19.2	13.0	19.8	17.8	21.1	13.2	21.1	18.0
Industry												
Construction	1.7	12.1	11.5	12.9	1.9	12.3	12	12.6	2	12.9	12.3	13.3
Manufacturing	13.7	12.3	25.3	15.1	14.8	12.1	26.8	14.8	15.7	12.6	28.4	15.2
Services	64.8	11.8	47.6	14.5	62.9	11.8	45.1	14.6	61.8	12.5	43.2	15.6
Trade	19.8	10.5	15.6	13.9	20.4	10.4	16	13.3	20.4	10.9	16.2	14
Education												
College graduates	10.5	17.3	9.6	25.2	10.4	17.0	9.5	25.3	10.6	18.4	9.7	27.1
High-school graduates	32.3	12.2	24.1	16.3	33.5	12.2	24.4	15.6	35.1	12.7	25.0	16.4
High-school drop-outs	46.3	10.1	56.3	12.1	46.3	10.0	57.1	12.1	46.6	10.4	58.1	12.5
Unknown	10.9	10.7	9.9	12.0	9.8	10.9	8.9	12.2	7.7	12.0	7.2	13.3

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 $t-1$.

Table 2 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	-0.32	-0.12	-0.03	-0.11	-0.09	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
2	-0.32	-0.12	-0.02	-0.09	-0.09	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
3	-0.27	-0.09	-0.03	-0.08	-0.09	-0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.23	-0.05	-0.01	-0.07	-0.07	-0.05
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
5	-0.17	-0.03	-0.01	-0.03	-0.07	-0.05
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
			Five years after first child's birth			
1	-0.49	-0.22	-0.01	-0.13	-0.12	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.37	-0.16	-0.01	-0.06	-0.12	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.35	-0.13	-0.02	-0.08	-0.12	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.27	-0.06	-0.00	-0.06	-0.11	-0.04
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.19	-0.04	-0.01	-0.02	-0.09	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
			Ten years after first child's birth			
1	-0.29	-0.07	0.01	-0.09	-0.07	-0.05
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
2	-0.28	-0.10	-0.00	-0.05	-0.09	-0.04
	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.21	-0.07	-0.01	-0.06	-0.09	-0.01
	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
4	-0.22	-0.02	0.00	-0.04	-0.11	-0.04
	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.09	0.00	-0.01	-0.01	-0.08	-0.01
	(0.04)	(0.03)	(0.01)	(0.02)	(0.01)	(0.03)

Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 3 – Relative child penalty: impact of the first child on fathers' labor outcomes, in log-points

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	0.03	0.04	0.01	0.00	-0.01	0.01
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
2	0.02	0.04	0.02	0.00	-0.01	0.01
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
3	0.02	0.04	0.01	-0.00	-0.00	0.00
	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)
4	0.03	0.04	0.01	-0.00	-0.00	0.00
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
5	0.06	0.06	0.02	-0.00	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
			Five years after first child's birth			
1	0.05	0.09	0.03	-0.00	-0.02	0.01
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
2	0.05	0.08	0.02	-0.01	-0.01	0.01
	(0.02)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
3	0.02	0.05	0.02	-0.01	-0.01	0.01
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
4	0.06	0.08	0.02	-0.02	-0.01	0.03
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
5	0.10	0.08	0.02	-0.00	0.00	0.04
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
			Ten years after first child's birth			
1	0.10	0.12	0.02	0.00	-0.02	0.02
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
2	0.09	0.12	0.03	-0.02	0.00	0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	0.05	0.08	0.02	-0.02	-0.00	0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
4	0.11	0.09	0.02	-0.01	-0.02	0.07
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	0.19	0.09	0.01	-0.01	0.00	0.12
	(0.04)	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)

Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 4 – Relative child penalty: impact of the first child on mothers' labor earnings, in log-points – estimates based on different reweighting approaches

Pre-child hourly wages quintile	Baseline	Reweighted on education	Reweighted on pre-birth observables	Reweighted on all observables
		One year after first child's birth		
1	-0.32	-0.30	-0.30	-0.30
	(0.03	(0.02)	(0.02))	(0.02)
2	-0.32	-0.32	-0.32	-0.32
	(0.01)	(0.02)	(0.02)	(0.02)
3	-0.27	-0.27	-0.27	-0.27
	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.23	-0.24	-0.24	-0.24
	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.17	-0.19	-0.18	-0.19
	(0.02	(0.02)	(0.02))	(0.01)
		Five years after first child's birth		
1	-0.49	-0.47	-0.47	-0.48
	(0.03)	(0.03)	(0.03)	(0.03)
2	-0.37	-0.37	-0.37	-0.38
	(0.00)	(0.02)	(0.03)	(0.03)
3	-0.35	-0.35	-0.35	-0.35
	(0.03)	(0.02)	(0.02)	(0.02)
4	-0.27	-0.29	-0.29	-0.29
	(0.03)	(0.02)	(0.02)	(0.02)
5	-0.19	-0.20	-0.21	-0.21
	(0.02)	(0.03)	(0.03)	(0.03)
		Ten years after first child's birth		
1	-0.29	-0.29	-0.28	-0.28
	(0.04)	(0.04)	(0.05)	(0.05)
2	-0.28	-0.29	-0.30	-0.29
	(0.02)	(0.03)	(0.03)	(0.03)
3	-0.21	-0.22	-0.22	-0.21
	(0.03)	(0.04)	(0.03)	(0.03)
4	-0.22	-0.24	-0.24	-0.24
	(0.03)	(0.03)	(0.04)	(0.04)
5	-0.09	-0.14	-0.15	-0.15
	(0.08)	(0.05)	(0.05)	(0.05)

Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Pre-birth variables include education, share of female part-time workers in the firm, pre-child days and hours worked, and pre-child labor market attachment. Additional feritlity related observables include age at first child and second and third child's arrival. Bootstrapped standard errors using 100 replications are clustered at the individual level.

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 approximately equal to the minimum wage) but excludes stock-options. Social security contributions, public pension schemes, unemployment benefits and other contributions including two flat-rate taxes on earned 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, be paid 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 reimbursed by the Social Security administration. The employer subsequently subtracts the maternity leave allowances that the employer advanced from the measure of earnings. Because the reimbursement occurs after the maternity leave itself, the decline in earnings may occur a few weeks after the maternity leave. 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 in the 2nd quarter of the year that are immune to this issue (see Appendix Figures F.7 and F.8).

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

A.2 Days

In the DADS dataset, days worked refer to the duration during which an employee is part of the 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 at 360.

A.3 Hours

In our dataset, hours worked refer to hours for which the worker is paid according to the labor contract. The data on hours is reported by employers when they fill out 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 of 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 a fixed-price basis.

If one of these conditions does not hold, 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.

As to workers whose compensation does not depend on the time worked, but who do not belong to one of the above-mentioned occupations, i.e., typically managers ("forfait-jour"), employers provide 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 hours worked but is instead paid by the Social Security Administration (and possibly receives a complementary payment from her employer), hours worked 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 days worked, which do not vary during maternity leaves. As a result, the DADS dataset overestimates hours worked – and underestimates hourly wages – for such workers during years when they give birth to children. In general, these are qualified workers that belong to the upper part of the hourly wage distribution, so 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 obtained from administrative birth records with census data to deal with the incompleteness of the former for individuals born October 2 and 3 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 censuses of 1990 and 1999 to fill the gap.

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

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

Information from birth records has been available since 1967 only, which results in left-censoring. However, because we are mostly interested in individuals giving birth between 1998 and 2015, we do not 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 our sample size substantially.

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:

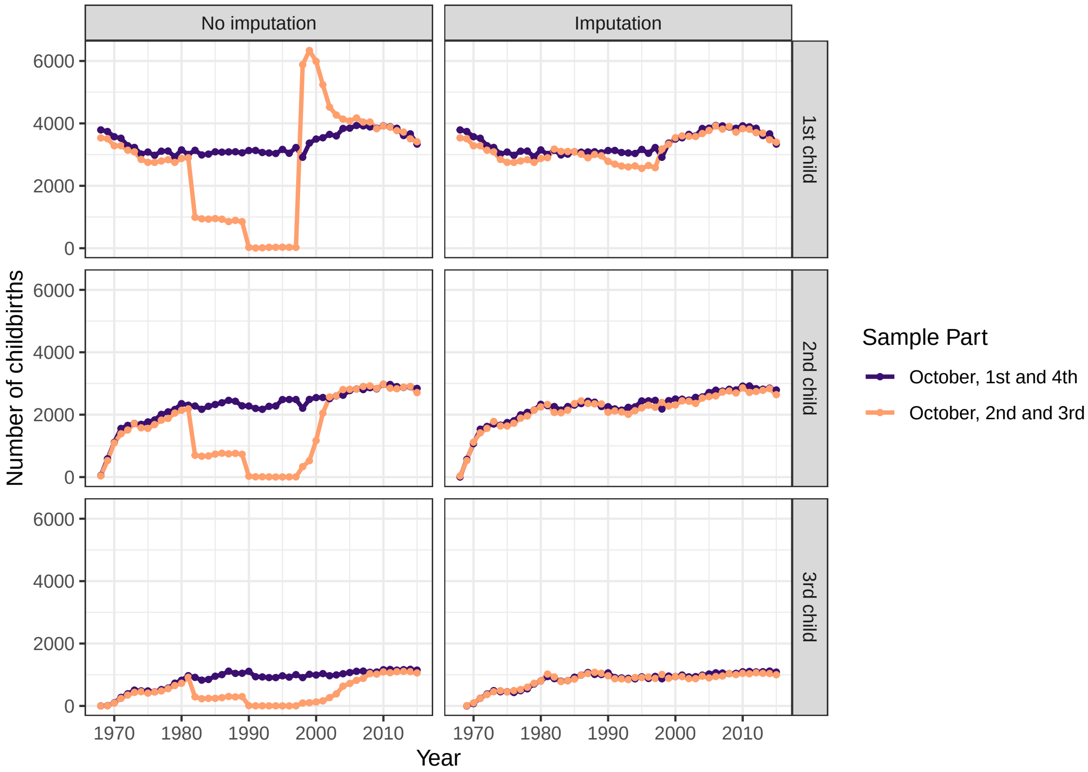
- if the first childbirth in birth records occurs before 1982, we regard it as the first childbirth;
- else,
  - if the earliest of years of childbirth she declared in the 1990 census is after 1982, we consider the earliest of these years and the year of the first childbirth as it appears in birth records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

- else,
  - if the earliest of years of childbirth she declared in the 1999 census is after 1982, we consider the earliest of these years and the year of the first childbirth as it appears in birth records as the year of the first childbirth;
  - else,
    - if birth records indicate that she has children, we consider the year of the first childbirth in birth records as the year of first childbirth;
    - else, we assume that she has no children.

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 the $n-1$th childbirth.

This approach does not take multiple births into account; more generally, it does not account for individuals who experience more than one childbirth per year. Despite this *caveat*, our approach matches the historical pattern in the complete half of the sample quite well. Figure B.1 plots the number of childbirths by rank of childbirth for each year since 1968 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 that occur in the beginning of the 1980s or in the late 1990s in the incomplete half of the sample, our approach matches reasonably well the patterns observed in the complete half of the sample.

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



C Age-Period-Cohort models

The major challenge in the simultaneous identification of λ, μ and ν stems from collinearity between age, cohort and period: age is equal to the current period less the year of birth. Several solutions have been explored in the sociological literature; e.g., Mason et al. (1973) assume that any two ages, periods or cohorts have the same effect, in addition to removing one dummy in each dimension. Deaton and Paxson (1994) and Deaton (1997) suggest a transformation of period effects to meet two requirements: (i) that time effects sum to zero, and (ii) that 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 $\sum_{t=1}^{T} \nu_t (t-1) = 0$.

Because we rely on a sample that only contains individuals born on even-numbered years, we have to impose one additional normalization (see Pora and Wilner, 2020, on this matter). Indeed, without additional normalizations the model is underidentified: during even-numbered years, individuals could face a systematic shock that is exactly offset by the fact that their ages are also even-numbered. Specifically, the two models:

$$\tilde{y}_{it} = \sum_c \lambda_c \mathbb{1}_{cohort_i = c} + \sum_a \mu_a \mathbb{1}_{age_{it} = a} + \sum_j \nu_j \mathbb{1}_{t=j} + \epsilon_{it}$$

and

$$\tilde{y}_{it} = \sum_c \lambda_c \mathbb{1}_{cohort_i = c} + \sum_a \left(\mu_a \mathbb{1}_{age_{it} = a} + \xi \mathbb{1}_{age_{it} \equiv 0[2]} \right) + \sum_j \left(\gamma_j \mathbb{1}_{t=j} - \xi \mathbb{1}_{t \equiv 0[2]} \right) + \epsilon_{it}$$

are observationally equivalent regardless of the identification of cohort coefficients.

This limitation leads us to further impose that both odd-year and even-year time effects sum to zero, i.e., to consider two restrictions: $\sum_{t>0} \nu_t = 0$ where $t = 2j$ and $\sum_t \nu_t = 0$ where $t = 2j+1$ The corresponding transformation of time dummies

$d_T = \mathbb{1}_{t=T}$ is written as follows:

$$d_t^* = \begin{cases} d_t - \left[\frac{t}{2}d_3 - \frac{t-2}{2}d_1\right] & t = 2j, j > 1 \\ d_t - \left[\frac{t-3}{2}d_3 + d_2 - \frac{t-3}{2}d_1\right] & t = 2j+1, j > 1 \end{cases} \tag{8}$$

where $d_1^* = d_2^* = d_3^* = 0$. 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^*, d_2^* and d_3^* in the regression.

D Accounting decomposition

The log-change in total labor earnings between time $t-1$ and time $t+k$ is written as

$$\Delta y_{t+k} = \log\left(\mathbb{E}[y_{i,t+k}]\right) - \log\left(\mathbb{E}[y_{i,t-1}]\right) \tag{9}$$

Δy_{t+k} can also be rewritten as

$$\Delta y_{t+k} = \log\left(\frac{\mathbb{E}[\frac{y_{i,t+k}}{y_{i,t-1}} y_{i,t-1}]}{\mathbb{E}[y_{i,t-1}]}\right) \tag{10}$$

This formulation is particularly relevant here since we require that all individuals be employed at time $t-1$, so $y_{i,t-1} > 0$. 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

$$\begin{aligned}\mathbb{E}[y_{i,t+k}] =& \mathbb{P}(d_{i,t+k} = 0)\mathbb{E}[y_{i,t+k}|d_{i,t+k} = 0] \\ &+ \mathbb{P}(d_{i,t+k} = 1)\mathbb{E}[y_{i,t+k}|d_{i,t+k} = 1]\end{aligned} \tag{11}$$

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

$$\begin{aligned}\Delta y_{t+k} =& \quad \log(\mathbb{P}(d_{i,t+k} = 1)) + \log(\mathbb{E}[y_{i,t+k}|d_{i,t+k} = 1]) - \log(\mathbb{E}[y_{i,t-1}]) \\ =& \quad \underbrace{\log(\mathbb{P}(d_{i,t+k} = 1))}_{\text{Participation}} + \underbrace{\log(\mathbb{E}[y_{i,t-1}|d_{i,t+k} = 1]) - \log(\mathbb{E}[y_{i,t-1}])}_{\text{Selection}} \\ &+ \underbrace{\log(\mathbb{E}[y_{i,t+k}|d_{i,t+k} = 1]) - \log(\mathbb{E}[y_{i,t-1}|d_{i,t+k} = 1])}_{\Delta y_{t+k}^{\text{Participants}}}\end{aligned} \tag{12}$$

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

are observed in the data. Then,

$$\Delta y_{t+k}^{\text{Participants}} = \underbrace{\log\left(\frac{\mathbb{E}\left[\frac{w_{i,t+k}}{w_{i,t-1}} x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k}=1\right]}{\mathbb{E}\left[x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k}=1\right]}\right)}_{\text{Hourly wages growth}}$$
$$+ \log\left(\frac{\mathbb{E}\left[x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k}=1\right]}{\mathbb{E}[x_{i,t-1} h_{i,t-1} w_{i,t-1} | d_{i,t+k}=1]}\right) \tag{13}$$

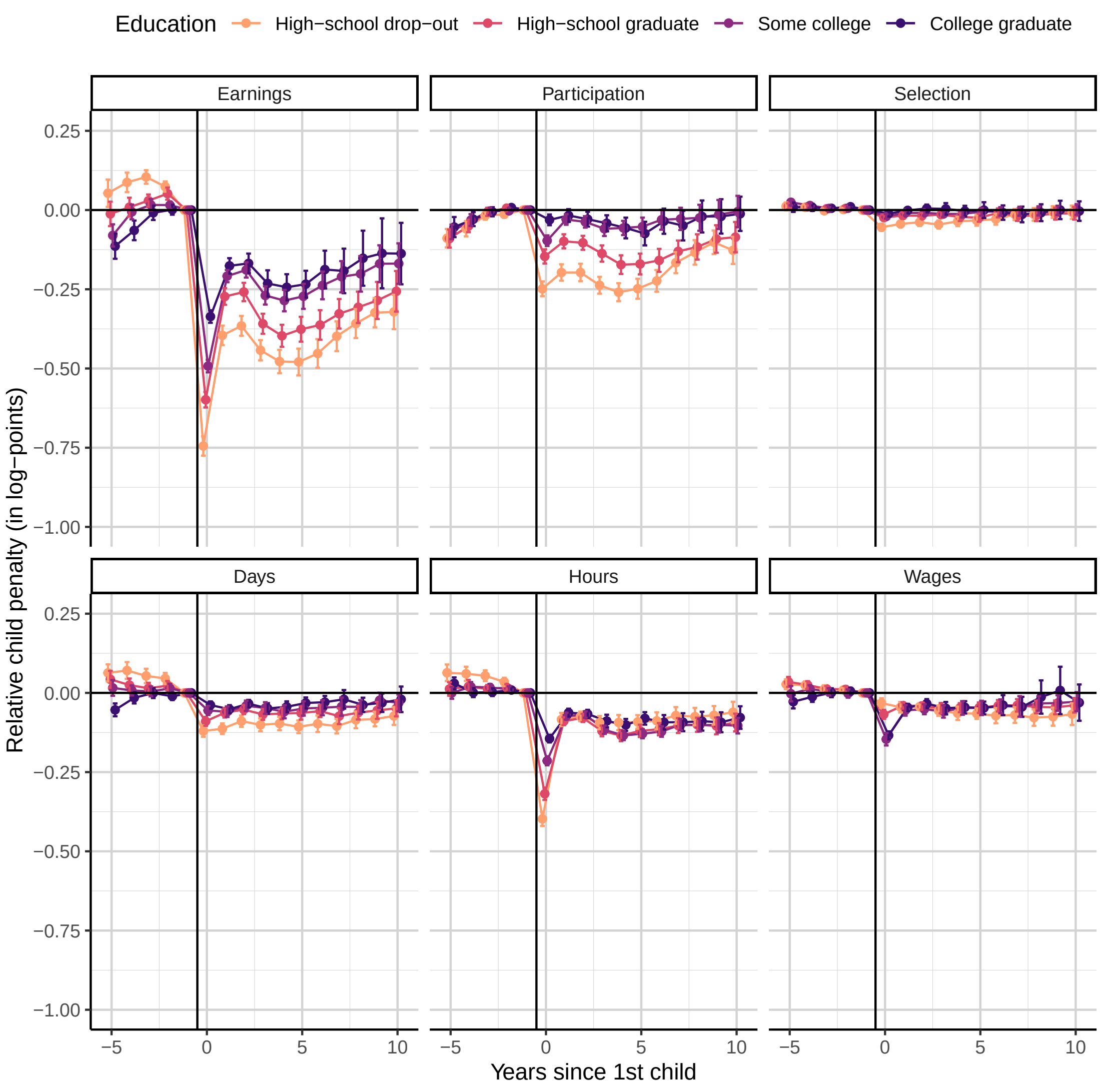
We continue to perform similar substitutions in the second term with respect to the two remaining components (hours and days). It follows that

$$\underbrace{\Delta y_{t+k}}_{\text{Labor earnings changes}} = \underbrace{\log\left(\mathbb{P}(d_{i,t+k}=1)\right)}_{\text{Participation}}$$
$$+ \underbrace{\log\left(\frac{\mathbb{E}[y_{i,t-1} | d_{i,t+k}=1]}{\mathbb{E}[y_{i,t-1}]}\right)}_{\text{Selection}}$$
$$+ \underbrace{\log\left(\frac{\mathbb{E}\left[\frac{x_{i,t+k}}{x_{i,t-1}} x_{i,t-1} h_{i,t-1} w_{i,t-1} | d_{i,t+k}=1\right]}{\mathbb{E}\left[x_{i,t-1} h_{i,t-1} w_{i,t-1} | d_{i,t+k}=1\right]}\right)}_{\text{Changes in Days Worked}}$$
$$+ \underbrace{\log\left(\frac{\mathbb{E}\left[\frac{h_{i,t+k}}{h_{i,t-1}} x_{i,t+k} h_{i,t-1} w_{i,t-1} | d_{i,t+k}=1\right]}{\mathbb{E}\left[x_{i,t+k} h_{i,t-1} w_{i,t-1} | d_{i,t+k}=1\right]}\right)}_{\text{Changes in Hours Per Day}}$$
$$+ \underbrace{\log\left(\frac{\mathbb{E}\left[\frac{w_{i,t+k}}{w_{i,t-1}} x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k}=1\right]}{\mathbb{E}\left[x_{i,t+k} h_{i,t+k} w_{i,t-1} | d_{i,t+k}=1\right]}\right)}_{\text{Hourly Wage Growth}} \tag{14}$$

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 for each component, and a selection term.

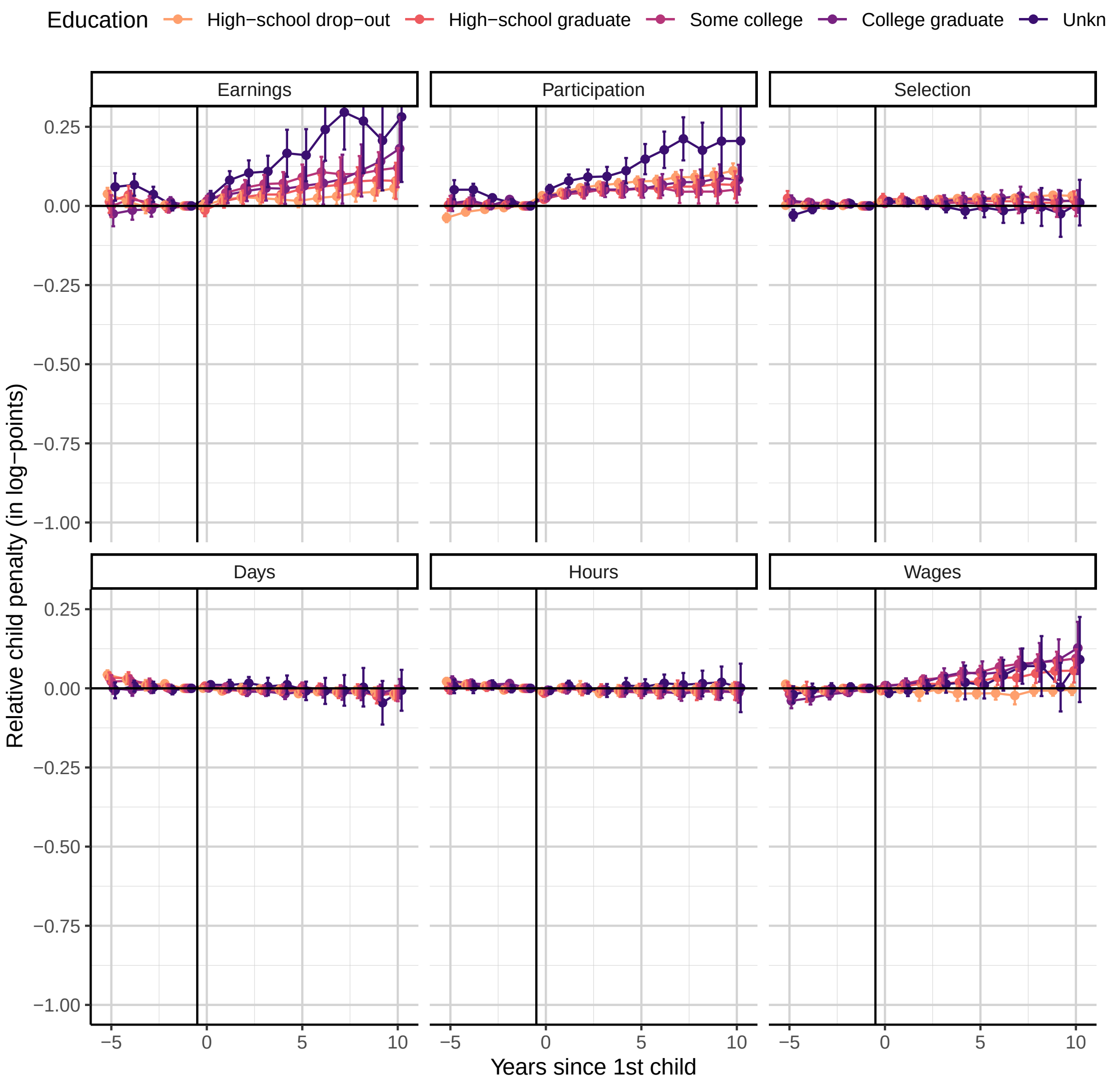
E Robustness checks (Alternate rankings)

Figure E.1 – Consequences of first childbirth for women's labor outcomes: education-based ranking



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure E.2 – Consequences of first childbirth for men's labor outcomes: education-based ranking



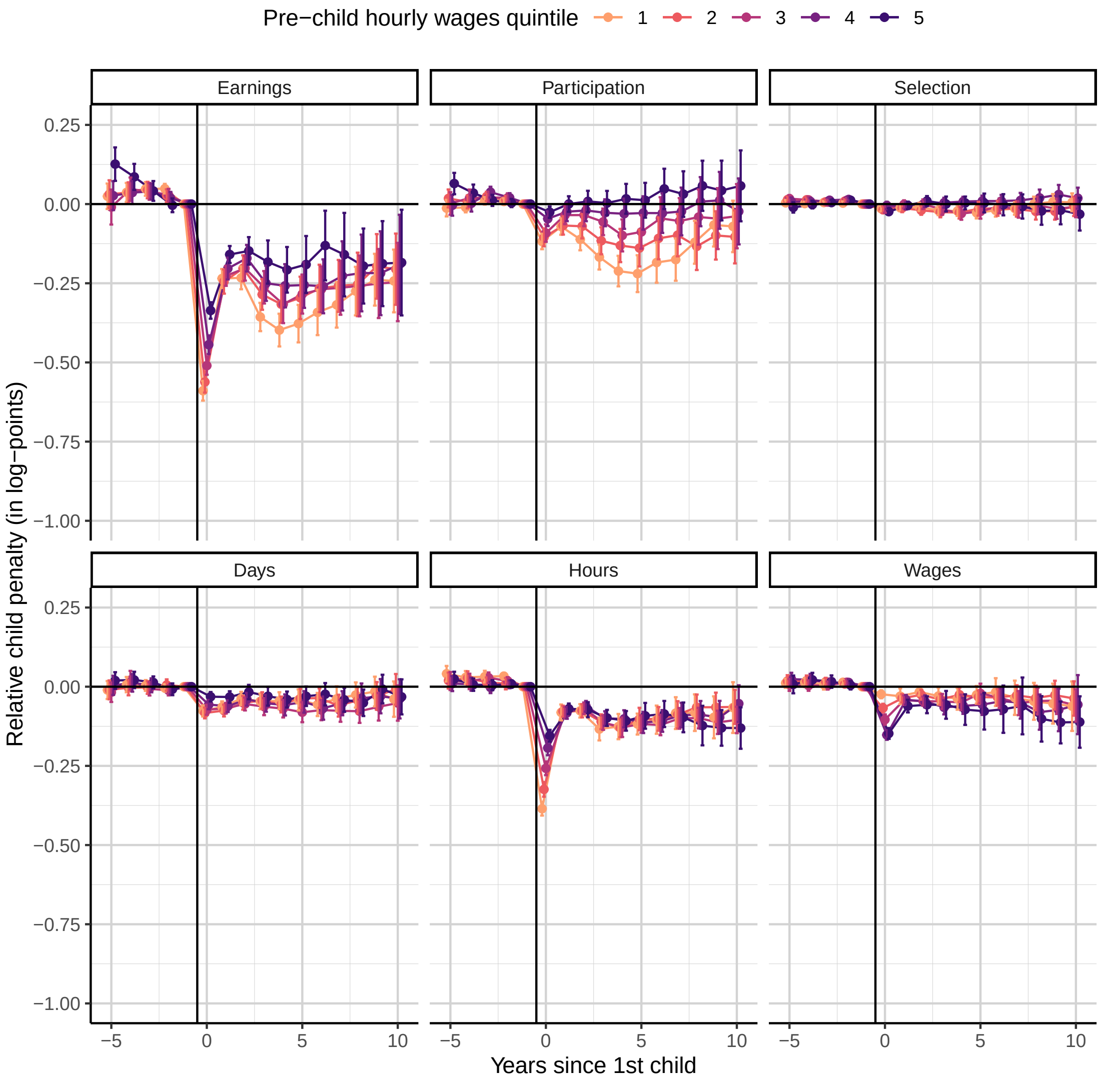
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 5 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
One year after first child's birth						
High-school drop-out	-0.40	-0.20	-0.04	-0.11	-0.08	-0.05
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
High-school graduate	-0.27	-0.10	-0.02	-0.06	-0.09	-0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Some college	-0.21	-0.03	-0.01	-0.06	-0.07	-0.06
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
College graduate	-0.18	-0.02	-0.00	-0.05	-0.06	-0.05
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Unknown	-0.27	-0.07	-0.04	-0.09	-0.09	-0.05
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Five years after first child's birth						
High-school drop-out	-0.48	-0.25	-0.03	-0.11	-0.09	-0.07
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
High-school graduate	-0.38	-0.17	-0.02	-0.06	-0.12	-0.05
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Some college	-0.27	-0.05	-0.01	-0.05	-0.13	-0.05
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
College graduate	-0.23	-0.07	-0.00	-0.03	-0.08	-0.05
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Unknown	-0.24	-0.08	-0.05	-0.04	-0.14	-0.03
	(0.06)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)
Ten years after first child's birth						
High-school drop-out	-0.32	-0.13	-0.01	-0.07	-0.06	-0.07
	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
High-school graduate	-0.26	-0.09	-0.01	-0.05	-0.09	-0.04
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Some college	-0.17	-0.00	0.00	-0.03	-0.10	-0.03
	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)
College graduate	-0.14	-0.01	-0.00	-0.02	-0.08	-0.03
	(0.05)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
Unknown	-0.26	0.00	-0.03	-0.10	-0.14	-0.05
	(0.10)	(0.07)	(0.04)	(0.05)	(0.05)	(0.03)

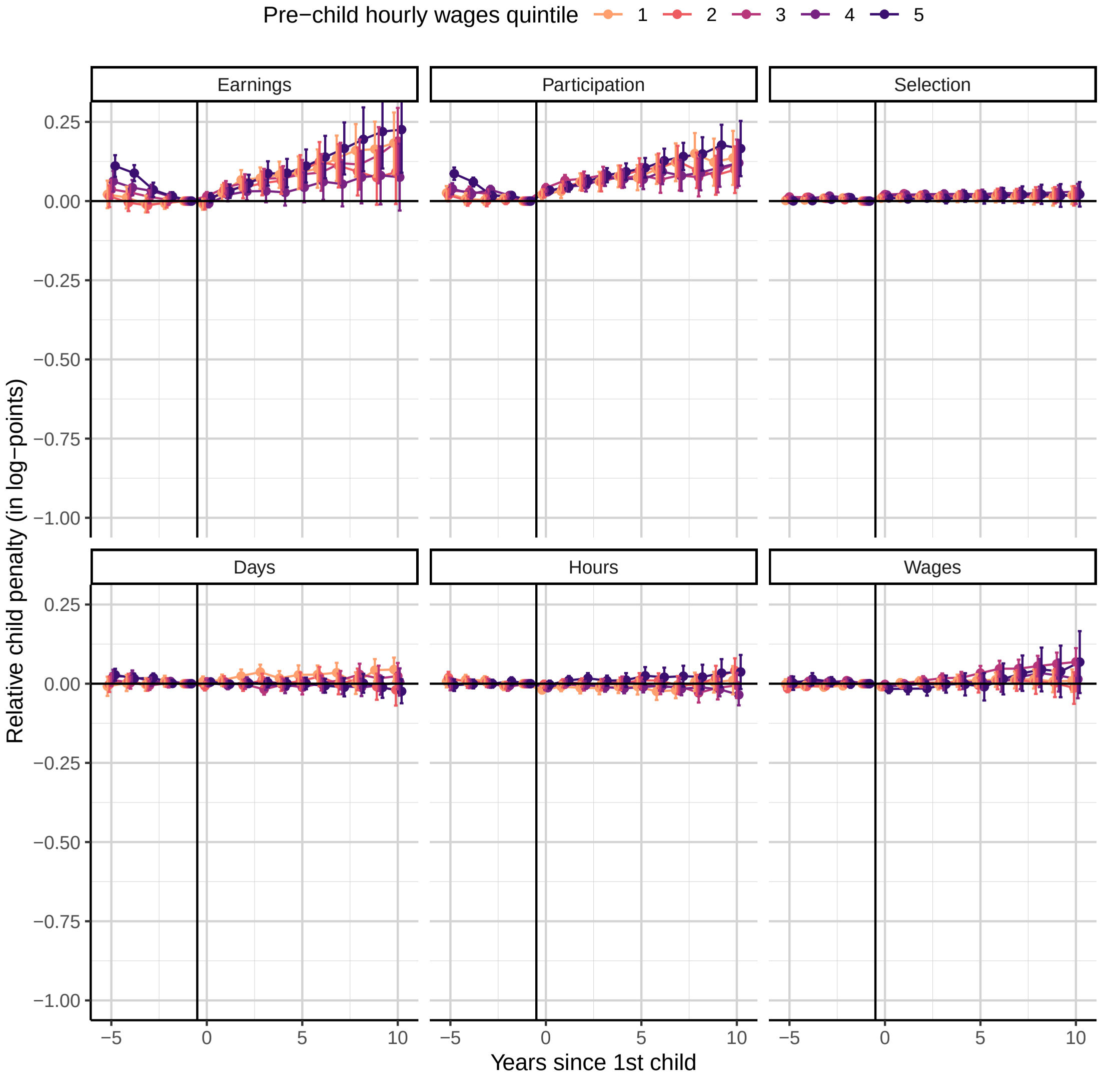
Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure E.3 – Consequences of first childbirth for women's labor outcomes: ranking based on rank at age 26 (hourly wages observed between ages 20 and 25)



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The ranking is based on average normalized hourly wages at 26, which aggregates hourly wages observed between 20 and 25. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure E.4 – Consequences of first childbirth for men's labor outcomes: ranking based on rank at age 26 (hourly wages observed between ages 20 and 25)



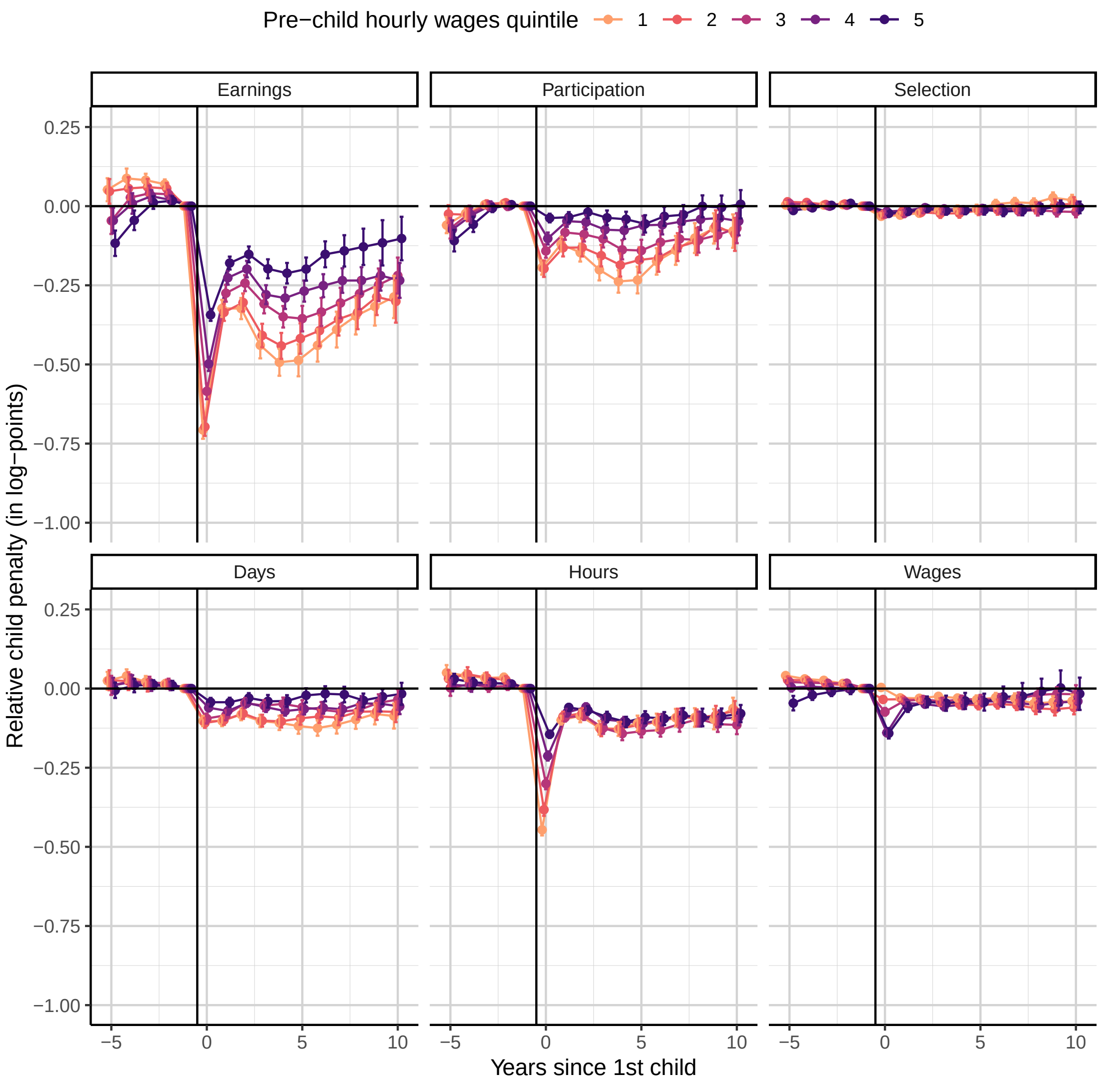
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The ranking is based on average normalized hourly wages at 26, which aggregates hourly wages observed between ages 20 and 25. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 6 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points. Ranks based on hourly wages before age 26

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	-0.23	-0.07	-0.01	-0.06	-0.08	-0.03
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.24	-0.07	-0.01	-0.08	-0.08	-0.04
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.23	-0.03	-0.00	-0.07	-0.08	-0.04
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.20	-0.02	-0.01	-0.06	-0.08	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.16	0.00	-0.00	-0.03	-0.07	-0.06
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
			Five years after first child's birth			
1	-0.38	-0.22	-0.03	-0.04	-0.12	-0.03
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
2	-0.30	-0.14	-0.01	-0.04	-0.10	-0.03
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
3	-0.28	-0.09	-0.02	-0.08	-0.12	-0.02
	(0.03)	(0.03)	(0.01)	(0.02)	(0.01)	(0.02)
4	-0.26	-0.03	0.01	-0.05	-0.11	-0.06
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
5	-0.19	0.01	0.00	-0.03	-0.09	-0.08
	(0.05)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
			Ten years after first child's birth			
1	-0.24	-0.07	0.01	-0.04	-0.07	-0.06
	(0.05)	(0.04)	(0.01)	(0.03)	(0.04)	(0.04)
2	-0.21	-0.10	-0.01	-0.01	-0.06	-0.04
	(0.06)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)
3	-0.25	-0.04	-0.01	-0.05	-0.11	-0.06
	(0.06)	(0.05)	(0.02)	(0.03)	(0.02)	(0.03)
4	-0.19	-0.02	0.02	-0.04	-0.05	-0.06
	(0.08)	(0.05)	(0.02)	(0.03)	(0.03)	(0.05)
5	-0.18	0.06	-0.03	-0.03	-0.13	-0.11
	(0.09)	(0.06)	(0.03)	(0.03)	(0.03)	(0.04)

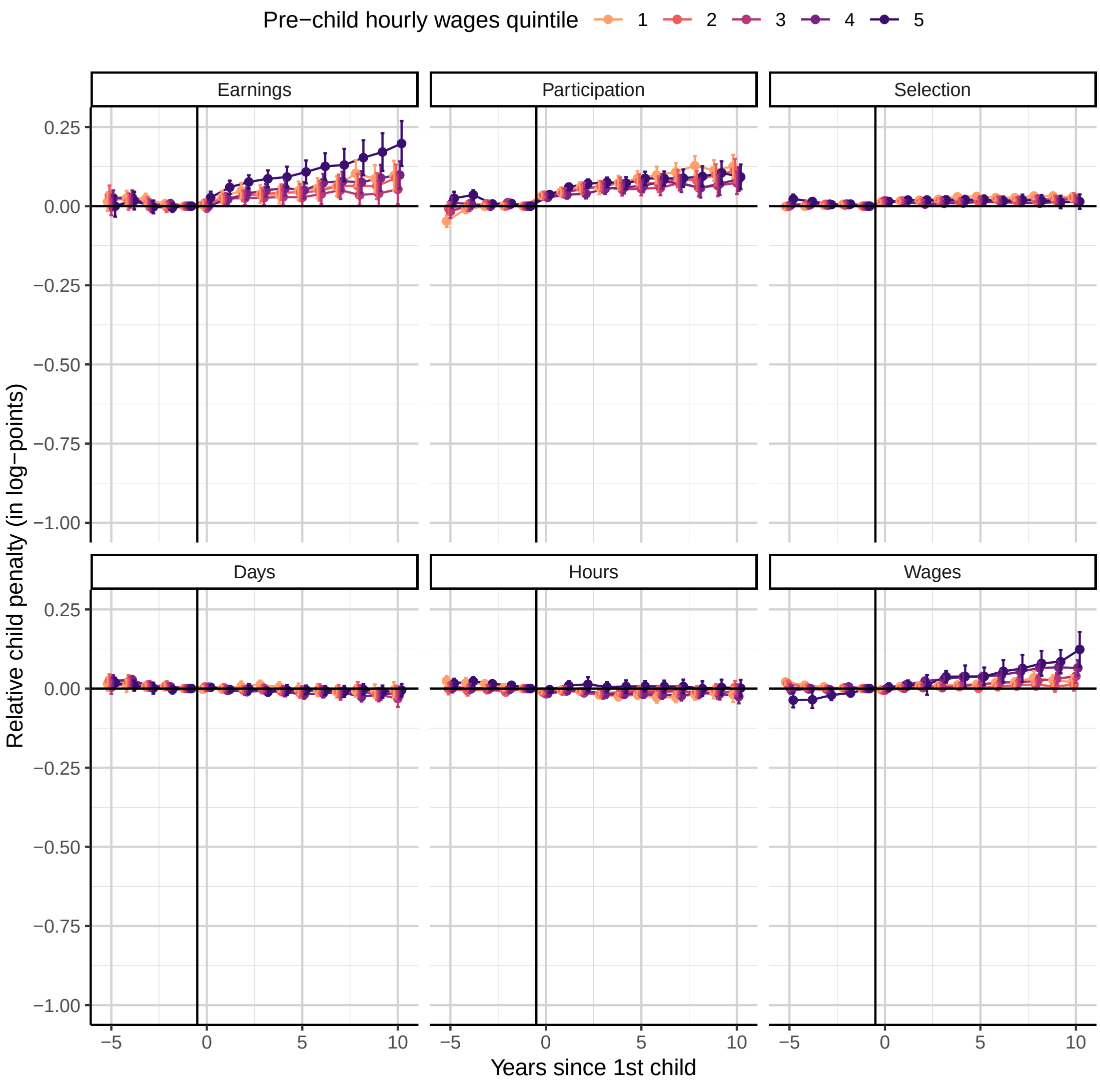
Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The ranking is based on hourly wages measured between age 20 and age 25, and the sample is restricted to (counterfactual) events that occured from age 26. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure E.5 – Consequences of first childbirth for women's labor outcomes: gender-specific ranking



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. A gender-specific ranking is based on average normalized hourly wages measured between $t-5$ and $t-1$. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure E.6 – Consequences of first childbirth for men's labor outcomes: gender-specific ranking



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. A gender-specific ranking is based on average normalized hourly wages measured between $t-5$ and $t-1$. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 7 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points. Ranks based on within-gender comparisons

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
		One year after first child's birth				
1	-0.32	-0.12	-0.03	-0.10	-0.10	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
2	-0.34	-0.13	-0.02	-0.10	-0.09	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
3	-0.28	-0.08	-0.02	-0.08	-0.09	-0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
4	-0.23	-0.05	-0.01	-0.07	-0.08	-0.05
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
5	-0.18	-0.04	-0.02	-0.04	-0.06	-0.06
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
		Five years after first child's birth				
1	-0.49	-0.23	-0.01	-0.12	-0.11	-0.04
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.42	-0.17	-0.01	-0.09	-0.12	-0.05
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.36	-0.14	-0.01	-0.06	-0.13	-0.03
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.27	-0.06	-0.01	-0.06	-0.11	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.20	-0.06	-0.01	-0.02	-0.09	-0.04
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
		Ten years after first child's birth				
1	-0.29	-0.08	0.02	-0.09	-0.06	-0.04
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
2	-0.30	-0.09	0.01	-0.07	-0.07	-0.06
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
3	-0.22	-0.07	-0.02	-0.03	-0.12	-0.02
	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
4	-0.23	-0.05	-0.00	-0.06	-0.09	-0.04
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.10	0.01	-0.00	-0.02	-0.08	-0.02
	(0.04)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)

Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The ranking is based on quintiles that are conditional on gender. Bootstrapped standard errors using 100 replications are clustered at the individual level.

F Right-censoring and measurement error

Our definition of control and treatment groups, despite being practical, raises some issues. First, due to right-censoring, individuals in our control group are not of 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 lifetime; this is true for old cohorts, but our estimate for younger cohorts might be spuriously affected by selection bias, namely, differences between parents of k children who experience childbirths quite early and parents who eventually have k children but do so later in life. Third, the definition of our treatment as experiencing the kth childbirth during year t might be blurred by the timing of working hours mainly because women are entitled a maternity leave that begins several weeks before childbirth and ends several months after. Choosing year $t-1$ as a reference for labor market outcomes may therefore lead to biases with respect to childbirths that occur in 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 control group as individuals without children as of 2015 according to the data.We take them at an age randomly drawn from the empirical distribution of age at the 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 made all of their fertility decisions by year 2015 (see Figures F.3 and F.4).

3. We define our control group as childless individuals according to the data as of time t, who do have children as of 2015, and who do not experience any childbirth between $t-1$ and $t+k$ (see Figures F.5 and F.6). This strategy is

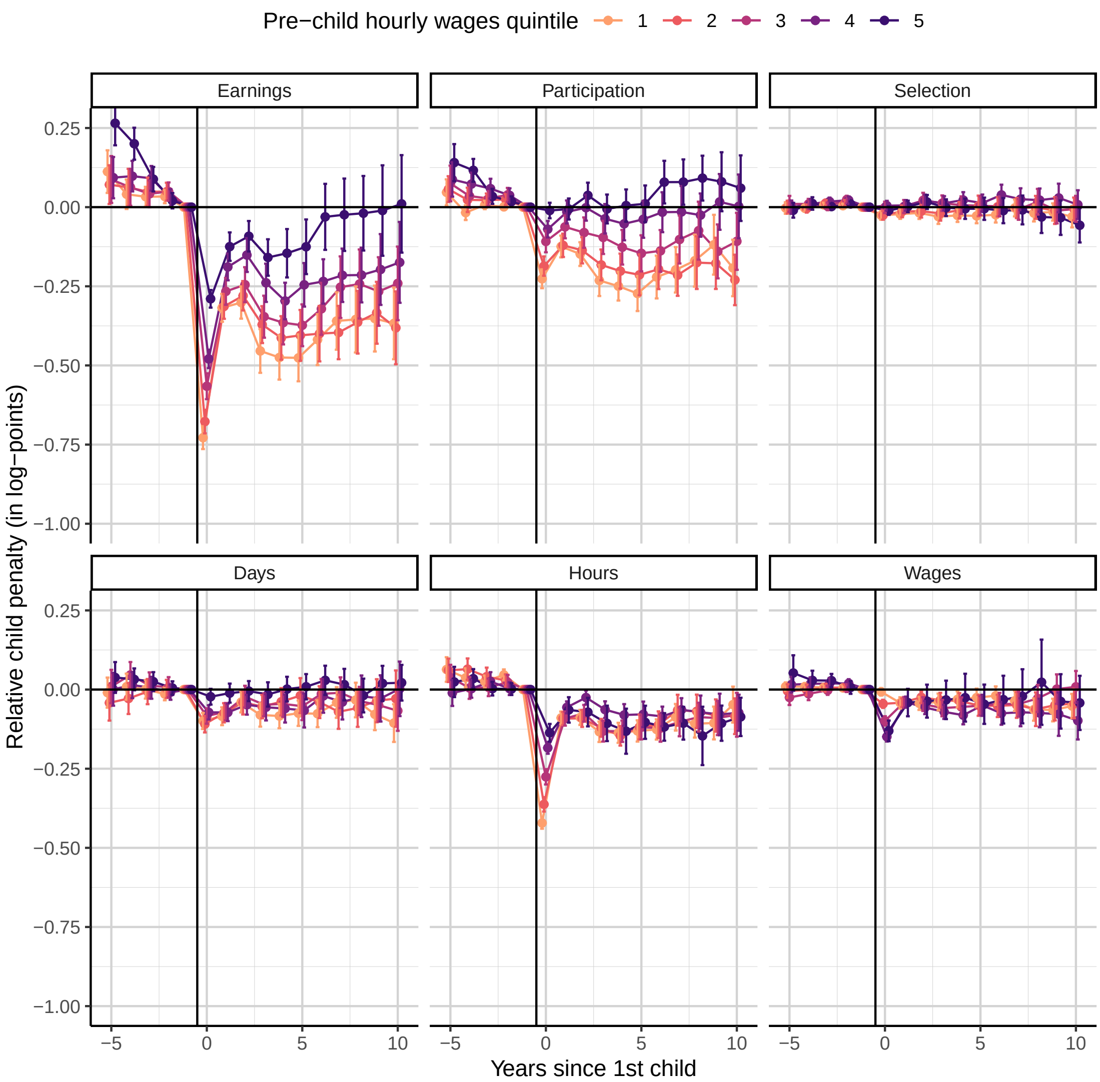
closer to that of Kleven, Landais, and Søgaard (2019) in that it relies on the timing of the first childbirth among those who indeed have children.

4. We restrict our treatment group to individuals who experience their first 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.

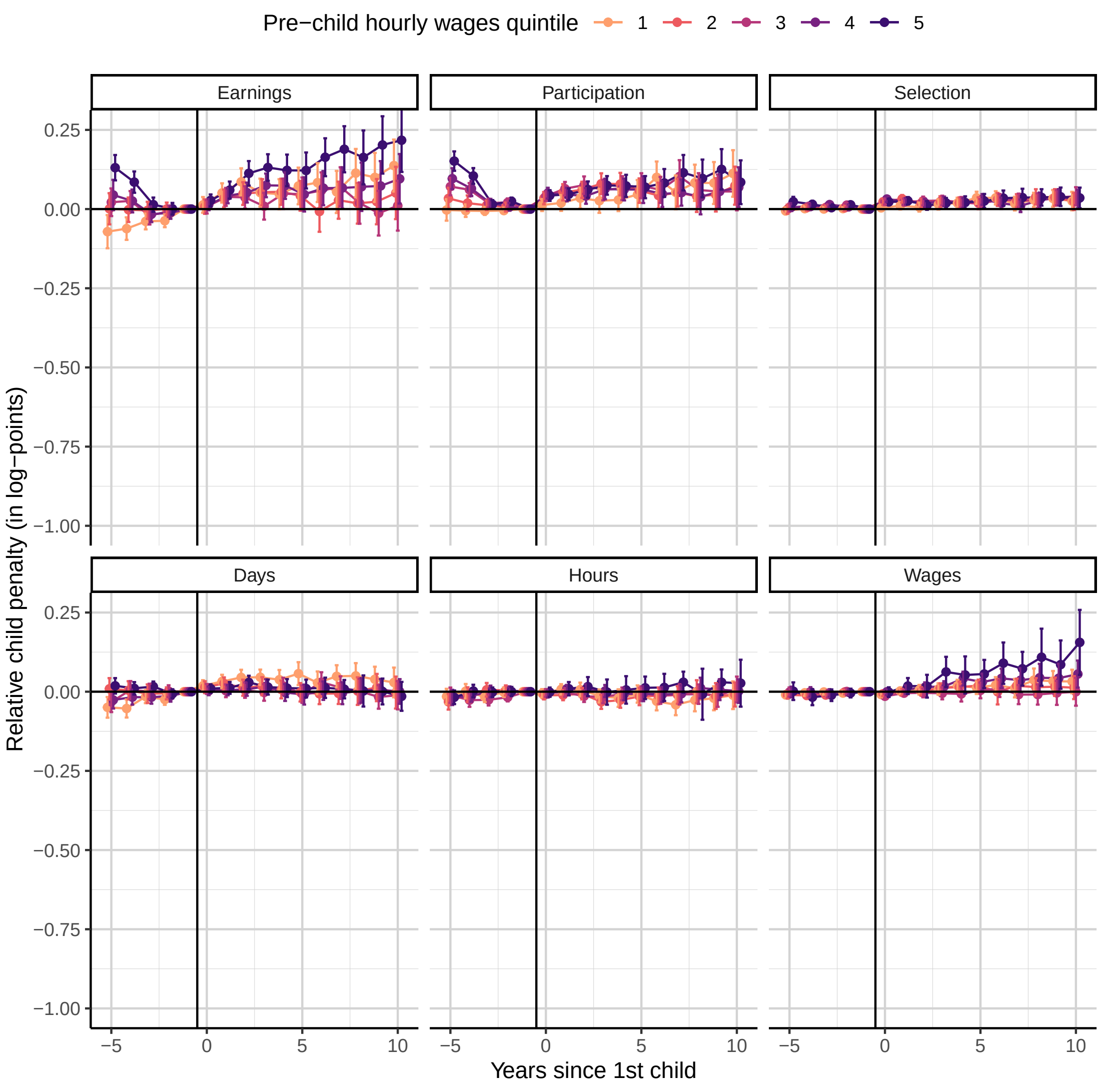
Additionally, our measure of the causal impact of childbirth rests on the assumption that treatment effects are time-homogeneous, i.e., that childbirths occurring in 1998 have the same causal impact as those that occurred in 2015 if they are considered after the passage of the same amount of time. This assumption justifies reliance of our estimates on a time-varying window: the impact of childbirth at time t is estimated on all childbirths from 1998 to 2015, while our estimate of the impact of childbirths at time $t + 10$ only relies on childbirths that occurred before 2016. The credibility of this assumption might be problematic given that the institutional background varied over the time period, e.g. the PAJE reform took place in 2004, and another change in parental leave rules, which was a slight change in the incentives to split parental leave between parents, happened in 2015. Nevertheless, we replicate our analysis while restricting it to childbirths during 2000-2005; hence this compositional change does not distort our estimates of dynamic treatment effects: all treatment effects for all durations of time to childbirth are computed on the very same sample. Additionally, by choosing 2000 as the beginning of the estimation timespan, we can ignore the fact that the selection condition in our sample is harsher for childbirths in 1998-1999 due to left-censoring issues in the data. Figures F.9 and F.10 display our estimates and show that our approach is completely robust with respect to these concerns.

Figure F.1 – Consequences of first childbirth for women's labor outcomes: a comparison with a control group determined at the imputed age of childbirth



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The control group is determined at age randomly drawn from the distribution of age at the first childbirth within gender × cohort × education cells. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure F.2 – Consequences of first childbirth for men's labor outcomes: a comparison with a control group determined at the imputed age of childbirth



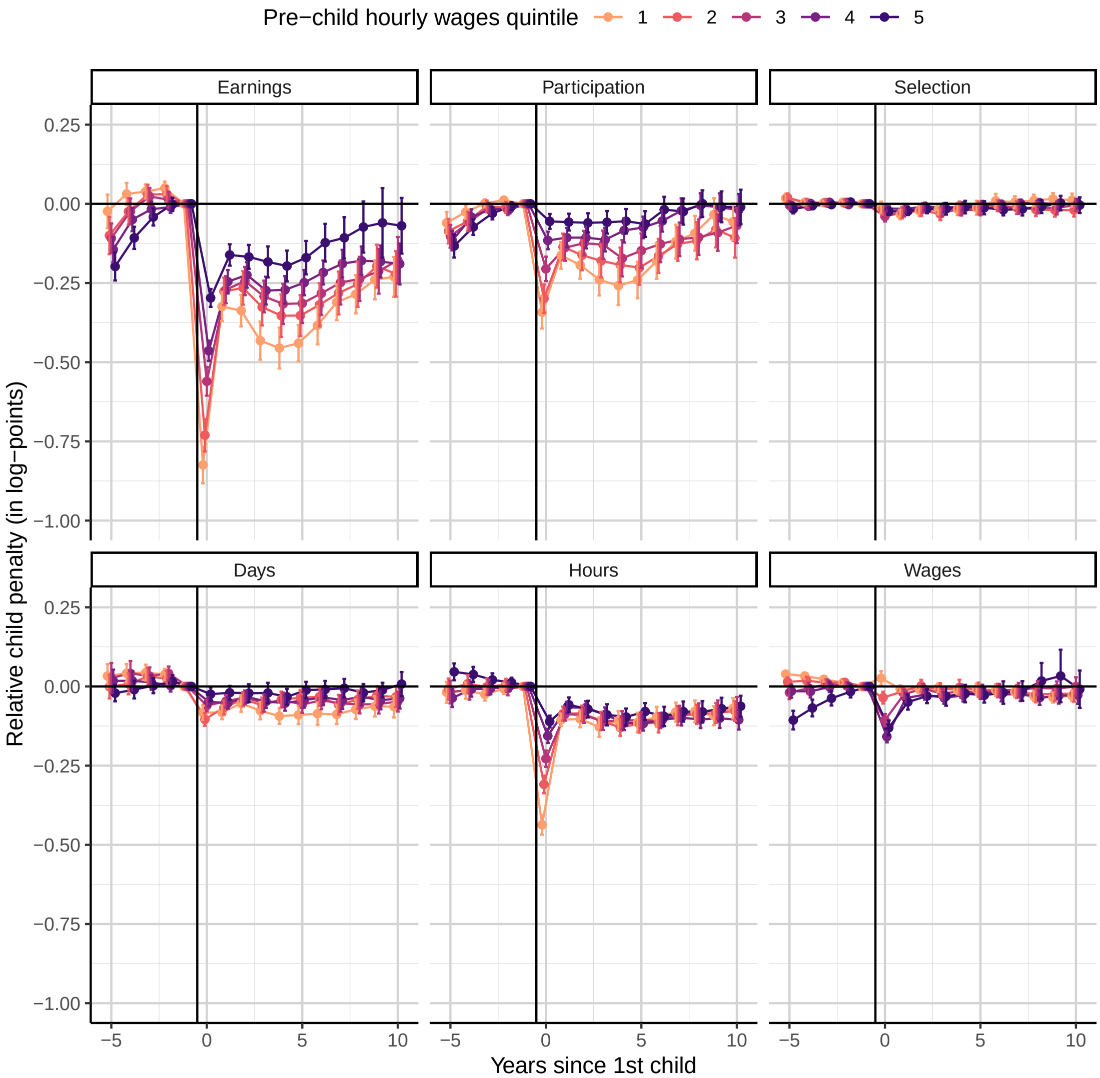
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The control group is determined at age randomly drawn from the distribution of age at the first childbirth within gender × cohort × education cells. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 8 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points. Estimates based on imputed counterfactual childbirths

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	-0.32	-0.12	-0.02	-0.08	-0.09	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.31	-0.12	-0.01	-0.07	-0.09	-0.04
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
3	-0.27	-0.06	0.00	-0.07	-0.09	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.19	-0.02	0.01	-0.07	-0.06	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.12	-0.01	0.00	-0.01	-0.06	-0.04
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
			Five years after first child's birth			
1	-0.48	-0.27	-0.03	-0.07	-0.13	-0.03
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
2	-0.41	-0.21	0.00	-0.02	-0.12	-0.05
	(0.04)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)
3	-0.37	-0.15	0.00	-0.05	-0.12	-0.05
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
4	-0.25	-0.04	0.01	-0.07	-0.08	-0.05
	(0.04)	(0.03)	(0.01)	(0.03)	(0.02)	(0.02)
5	-0.13	0.01	-0.01	0.01	-0.10	-0.05
	(0.04)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
			Ten years after first child's birth			
1	-0.37	-0.19	-0.03	-0.11	-0.05	-0.05
	(0.06)	(0.05)	(0.02)	(0.03)	(0.03)	(0.02)
2	-0.38	-0.23	-0.01	-0.03	-0.08	-0.04
	(0.06)	(0.04)	(0.02)	(0.05)	(0.03)	(0.02)
3	-0.24	-0.11	-0.01	-0.07	-0.08	0.01
	(0.06)	(0.05)	(0.02)	(0.03)	(0.04)	(0.03)
4	-0.17	0.00	0.01	0.00	-0.07	-0.10
	(0.07)	(0.05)	(0.02)	(0.04)	(0.03)	(0.03)
5	0.01	0.06	-0.06	0.02	-0.09	-0.04
	(0.08)	(0.05)	(0.03)	(0.03)	(0.03)	(0.04)

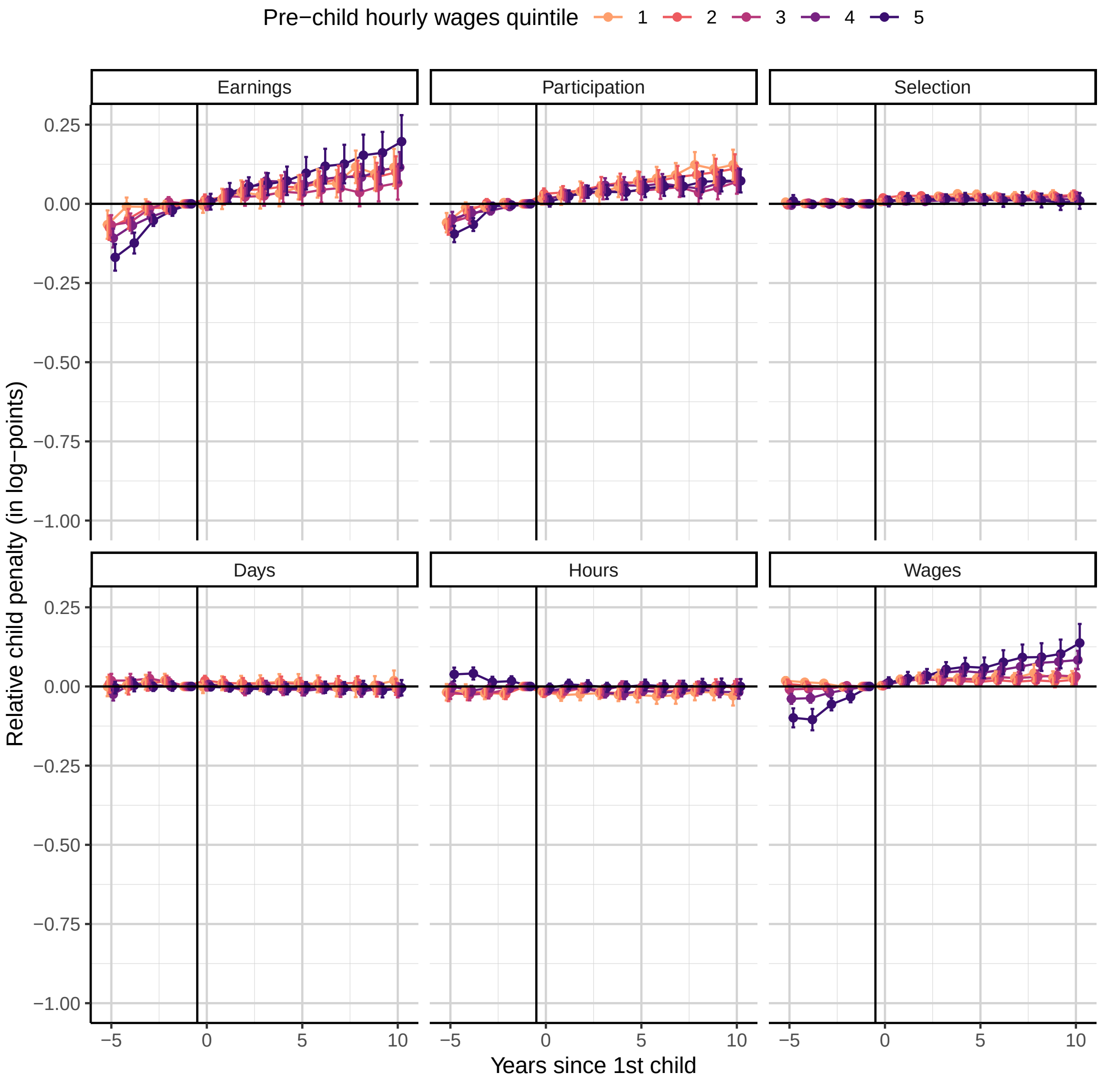
Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The treatment group is defined thanks to counterfactual childbirths, randomly imputed based on gender, year of birth and education. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure F.3 – Consequences of first childbirth for women's labor outcomes: restriction to older cohorts that have made complete fertility decisions



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The sample is restricted to individuals born in 1975 or earlier. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure F.4 – Consequences of first childbirth for men's labor outcomes: restriction to older cohorts that have made complete fertility decisions



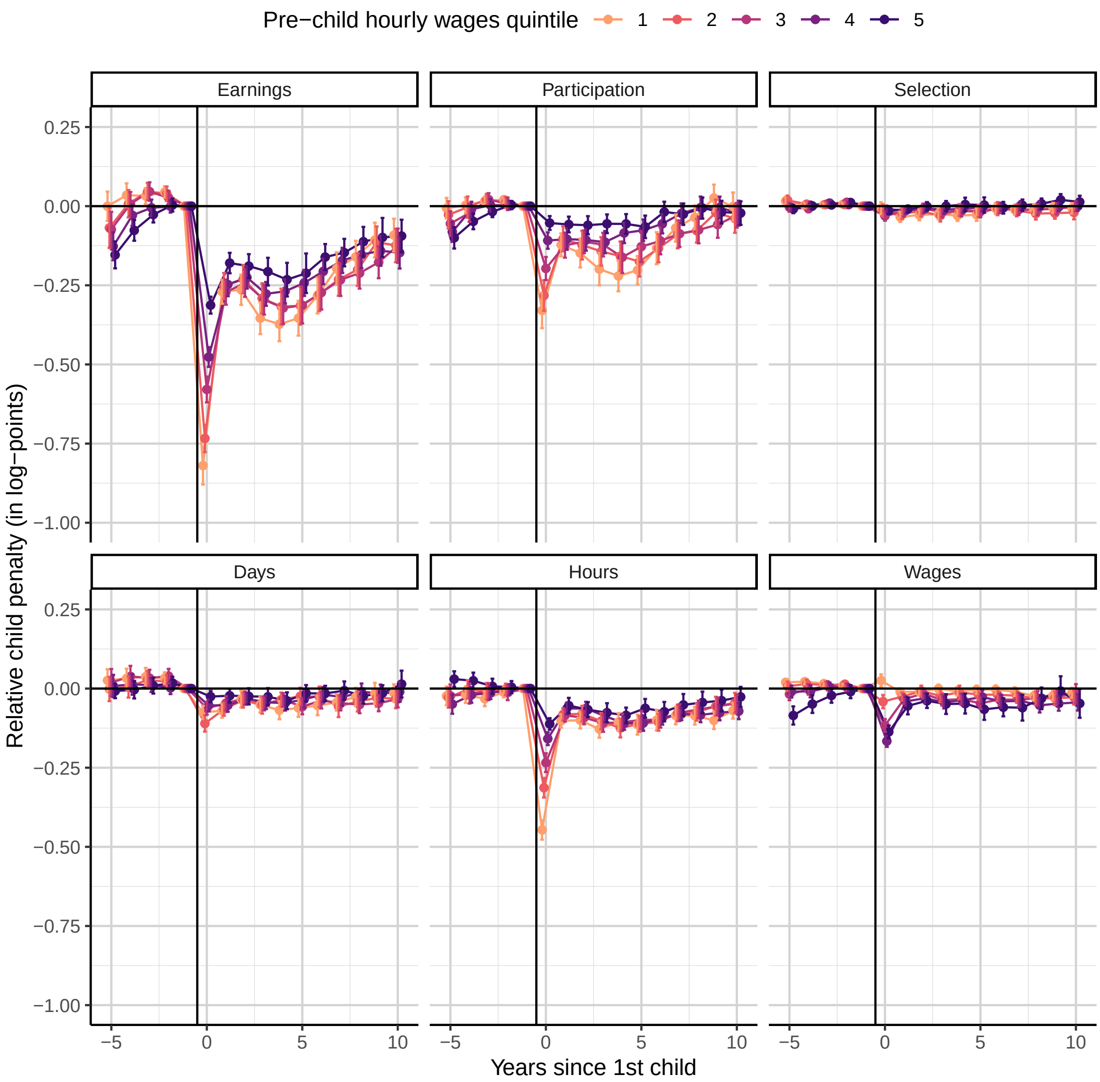
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The sample is restricted to individuals born in 1975 or earlier. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 9 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points. Estimates based on older cohorts

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	-0.32	-0.16	-0.03	-0.08	-0.11	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.00)
2	-0.28	-0.14	-0.03	-0.07	-0.09	-0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.27	-0.14	-0.03	-0.05	-0.09	-0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.25	-0.11	-0.02	-0.05	-0.07	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.16	-0.06	-0.02	-0.02	-0.06	-0.05
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
			Five years after first child's birth			
1	-0.44	-0.24	-0.01	-0.09	-0.11	-0.01
	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)
2	-0.35	-0.20	-0.01	-0.04	-0.12	-0.01
	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.31	-0.15	-0.02	-0.06	-0.11	-0.02
	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.25	-0.07	-0.00	-0.04	-0.12	-0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.17	-0.06	-0.01	-0.01	-0.08	-0.03
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
			Ten years after first child's birth			
1	-0.23	-0.06	0.01	-0.06	-0.07	-0.03
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
2	-0.22	-0.11	-0.02	-0.03	-0.08	-0.02
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
3	-0.18	-0.07	-0.00	-0.05	-0.07	-0.00
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
4	-0.19	-0.02	0.00	-0.04	-0.11	-0.03
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
5	-0.07	-0.01	-0.00	0.01	-0.06	-0.01
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)

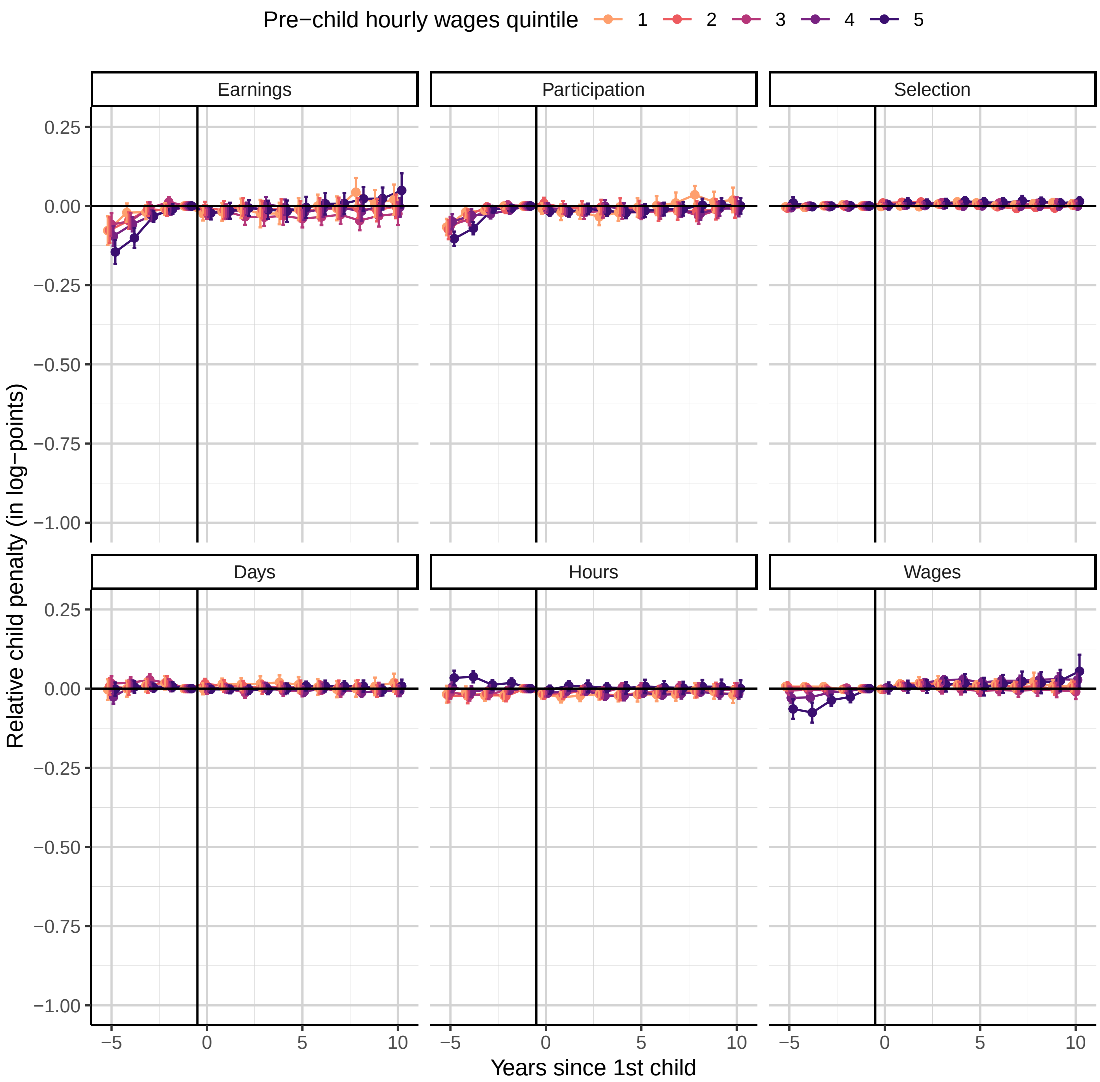
Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The sample is restricted to parents born before 1976. Bootstrapped standard errors using 100 replications are clustered at the individual level.

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



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The control group is made up of individuals with children whose first child is born later than $t + k$. Bootstrapped standard errors using 100 replications are clustered at the individual level.

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



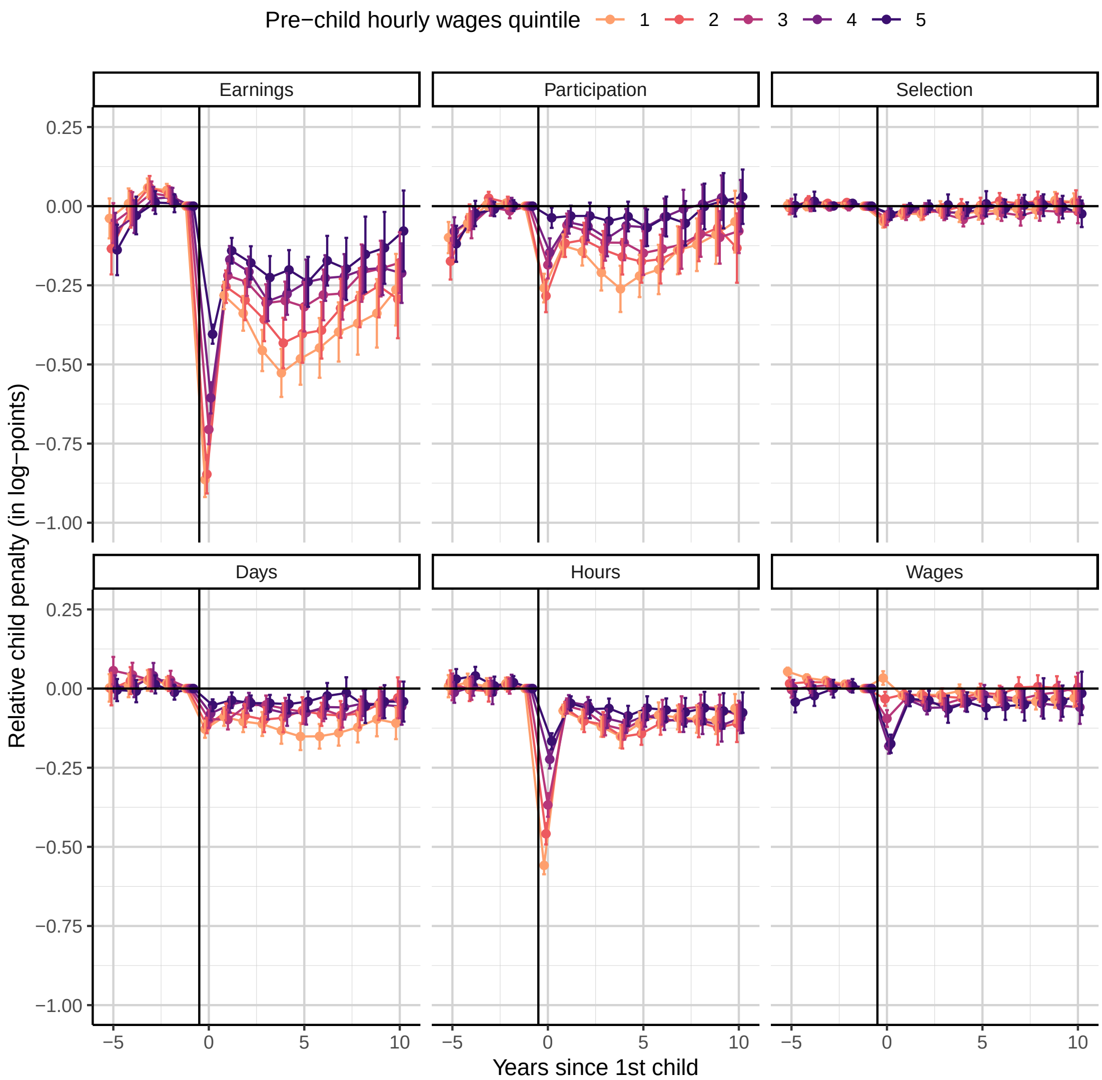
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The control group is made up of individuals with children whose first child is born later than $t + k$. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 10 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points. Estimates based on older cohorts

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	-0.27	-0.12	-0.03	-0.07	-0.10	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.25	-0.10	-0.02	-0.06	-0.08	-0.03
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.27	-0.12	-0.02	-0.05	-0.08	-0.03
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.25	-0.10	-0.01	-0.05	-0.06	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.18	-0.06	-0.01	-0.02	-0.05	-0.05
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
			Five years after first child's birth			
1	-0.35	-0.20	-0.03	-0.06	-0.11	-0.00
	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
2	-0.31	-0.18	-0.02	-0.02	-0.11	-0.02
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.31	-0.13	-0.02	-0.06	-0.10	-0.05
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.24	-0.08	-0.00	-0.03	-0.11	-0.03
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.21	-0.07	0.00	-0.02	-0.06	-0.07
	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
			Ten years after first child's birth			
1	-0.09	-0.00	-0.01	-0.01	-0.07	-0.01
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.12	-0.04	-0.02	-0.03	-0.05	-0.03
	(0.03)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
3	-0.12	-0.03	-0.00	-0.03	-0.05	-0.02
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
4	-0.15	-0.02	-0.00	-0.01	-0.07	-0.04
	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
5	-0.09	-0.02	0.01	0.01	-0.03	-0.05
	(0.03)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)

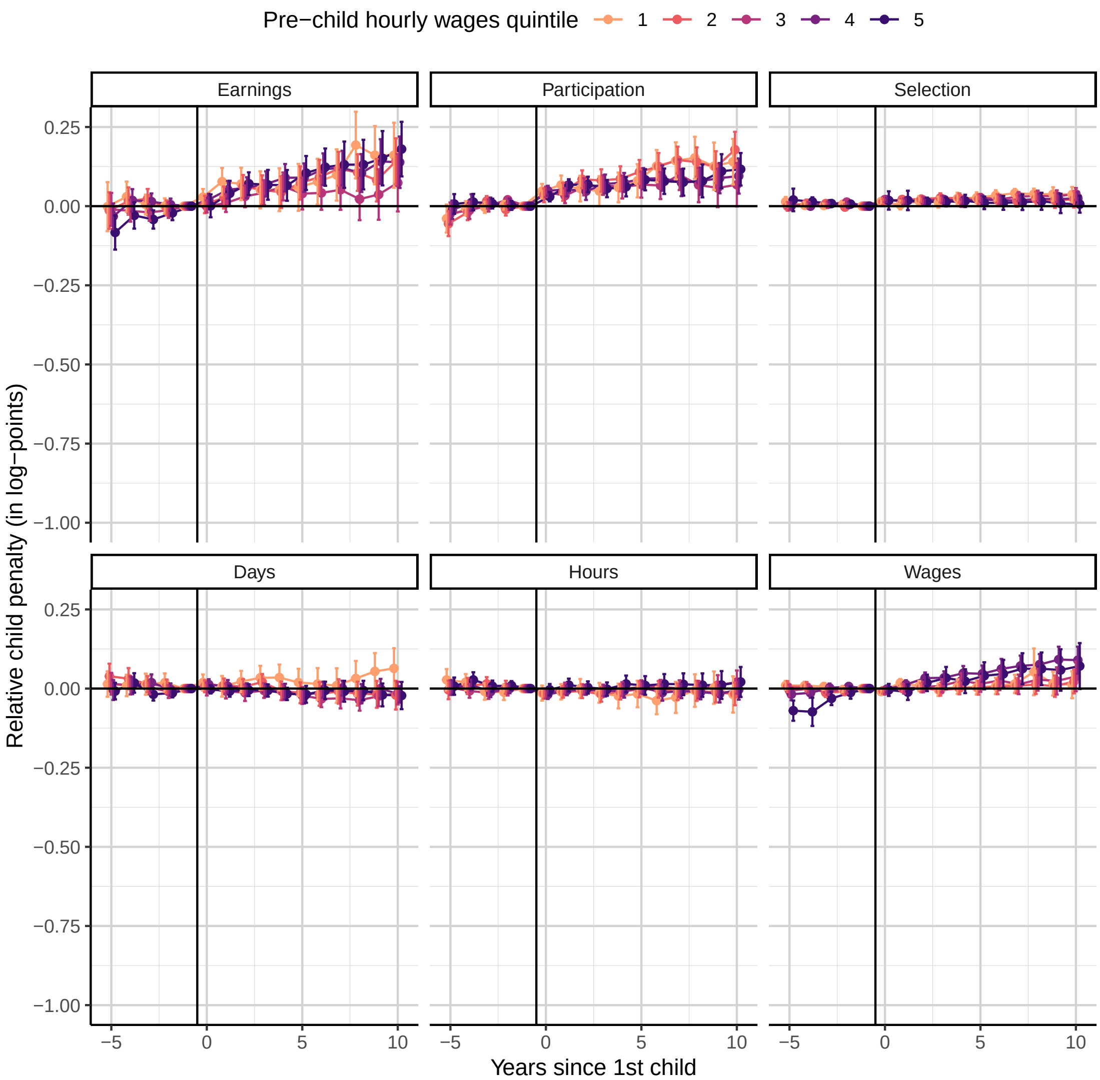
Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The treatment group correspond to parents who are yet to have their first child. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure F.7 – Consequences of first childbirth for women's labor outcomes: restriction to childbirths in the second quarter



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The treated group is restricted to individuals that experience the nth childbirth during the second quarter of year t. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure F.8 – Consequences of first childbirth for men's labor outcomes: restriction to childbirths in the second quarter



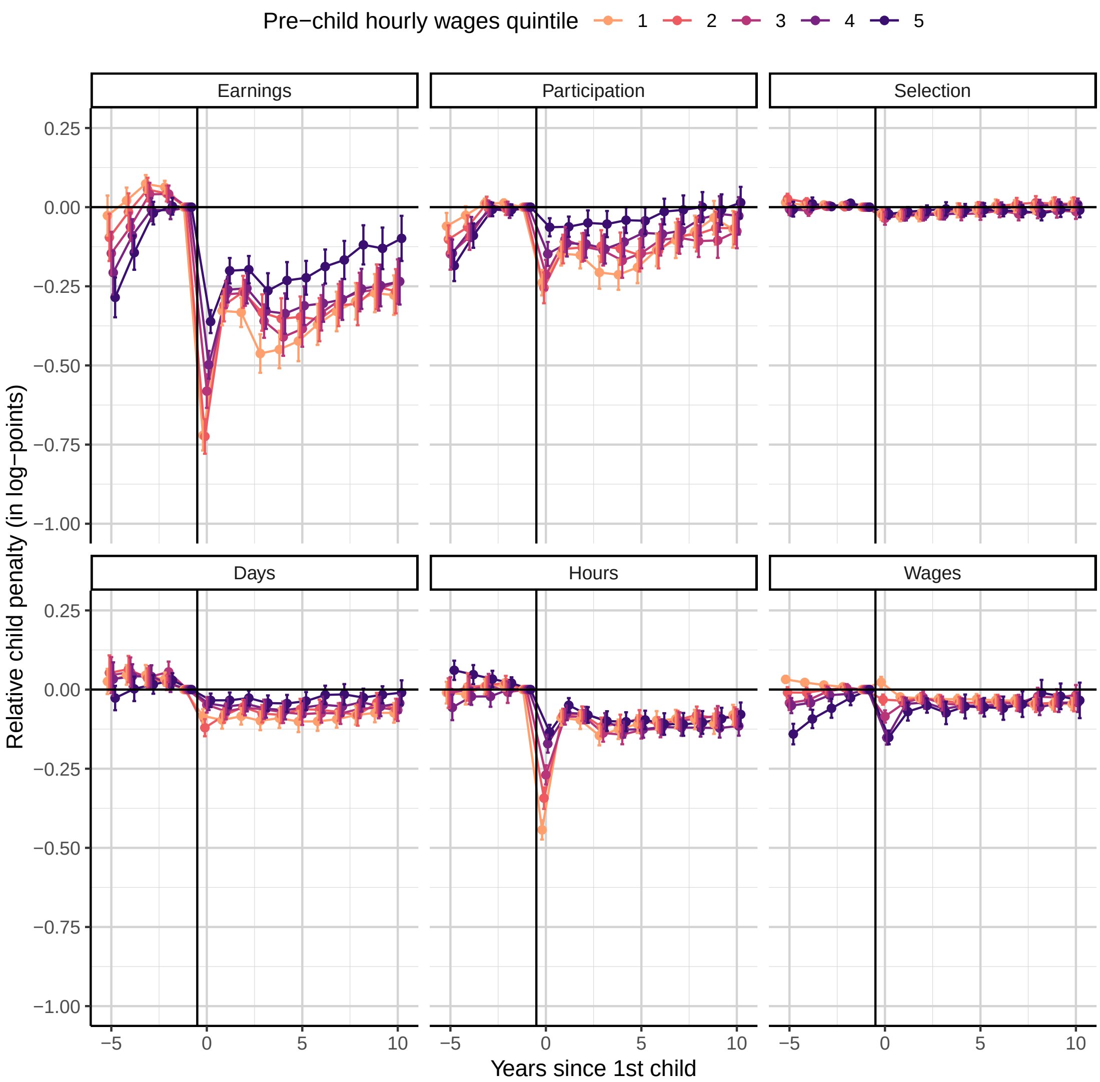
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The treated group is restricted to individuals that experience the nth childbirth during the second quarter of year t. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Table 11 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points. Estimates based on older cohorts

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	-0.28	-0.12	-0.02	-0.09	-0.07	-0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.25	-0.12	-0.02	-0.07	-0.06	-0.02
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.22	-0.06	-0.02	-0.10	-0.05	-0.03
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
4	-0.17	-0.05	-0.01	-0.05	-0.04	-0.03
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.14	-0.03	-0.01	-0.04	-0.05	-0.03
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
			Five years after first child's birth			
1	-0.48	-0.22	-0.02	-0.15	-0.11	-0.02
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
2	-0.40	-0.18	0.00	-0.07	-0.14	-0.01
	(0.05)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
3	-0.32	-0.15	-0.03	-0.07	-0.10	-0.03
	(0.04)	(0.04)	(0.01)	(0.02)	(0.02)	(0.01)
4	-0.24	-0.07	-0.01	-0.08	-0.09	-0.02
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
5	-0.24	-0.07	0.01	-0.04	-0.06	-0.06
	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
			Ten years after first child's birth			
1	-0.26	-0.05	0.01	-0.11	-0.06	-0.03
	(0.06)	(0.05)	(0.01)	(0.03)	(0.02)	(0.01)
2	-0.29	-0.13	0.01	-0.03	-0.11	-0.01
	(0.06)	(0.06)	(0.02)	(0.03)	(0.03)	(0.02)
3	-0.18	-0.08	-0.02	-0.04	-0.09	0.00
	(0.05)	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)
4	-0.21	0.00	0.00	-0.06	-0.09	-0.06
	(0.05)	(0.04)	(0.01)	(0.03)	(0.02)	(0.03)
5	-0.08	0.03	-0.02	-0.04	-0.08	-0.02
	(0.07)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)

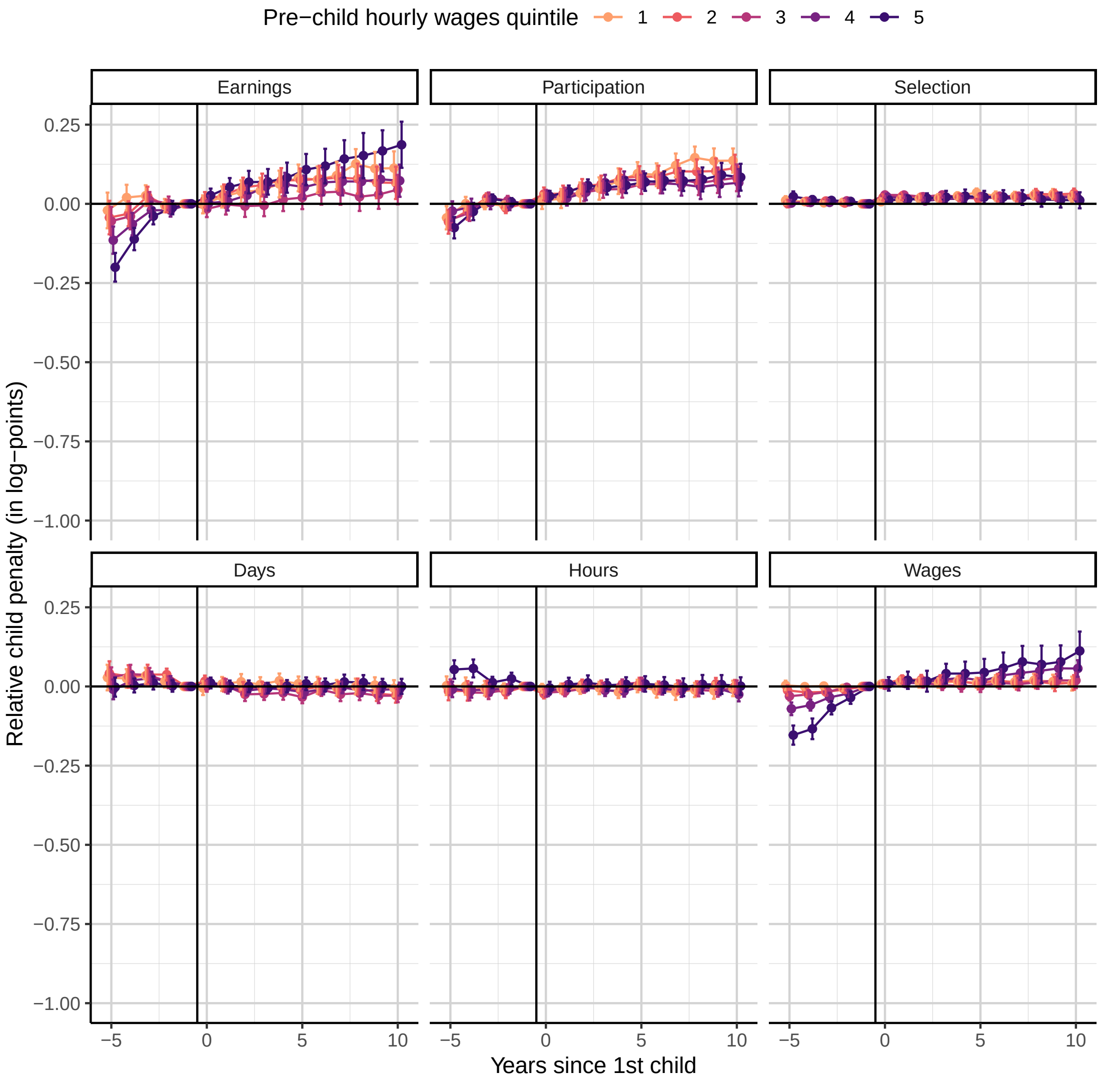
Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The sample is restricted to parents whose child is born on the 2nd quarter. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure F.9 – Consequences of first childbirth for women's labor outcomes: restriction to childbirths in 2000-2005



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The treatment time is restricted to years 2000 to 2005. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure F.10 – Consequences of first childbirth for men's labor outcomes: restriction to childbirths in 2000-2005



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see 6) for various values of time-to-childbirth expressed in years. The treatment time is restricted to years 2000 to 2005. Bootstrapped standard errors using 100 replications are clustered at the individual level.

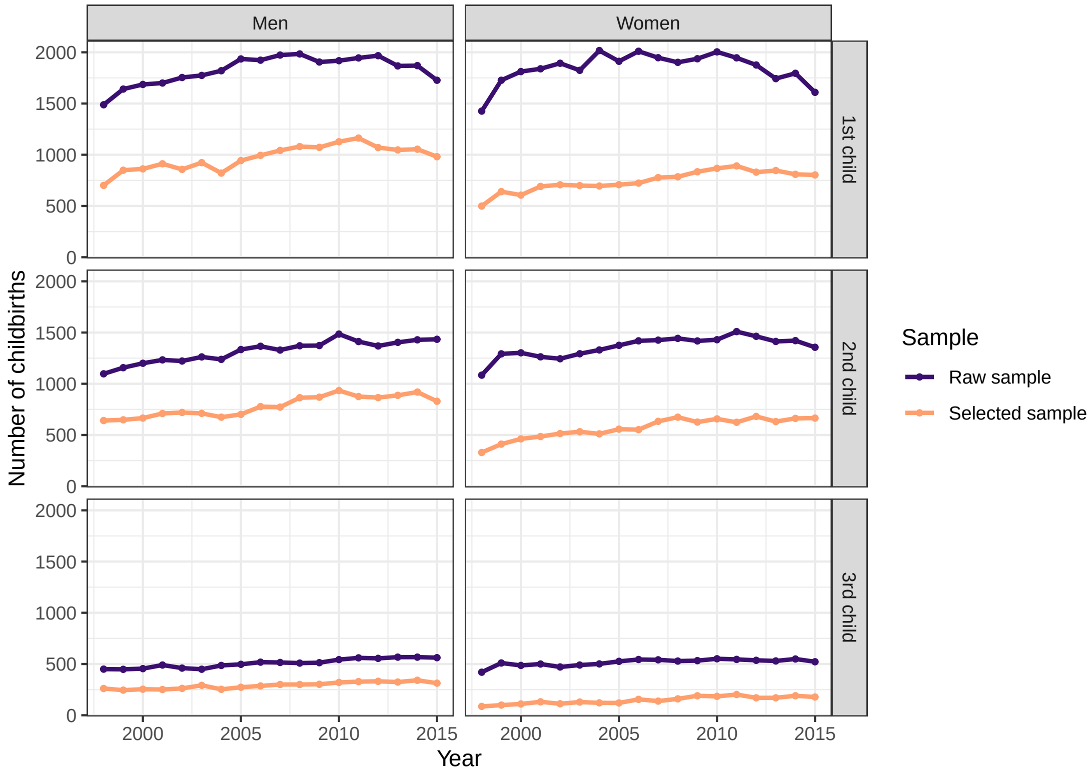
Table 12 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points. Estimates based on older cohorts

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	-0.28	-0.12	-0.02	-0.09	-0.07	-0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.25	-0.12	-0.02	-0.07	-0.06	-0.02
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.22	-0.06	-0.02	-0.10	-0.05	-0.03
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
4	-0.17	-0.05	-0.01	-0.05	-0.04	-0.03
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.14	-0.03	-0.01	-0.04	-0.05	-0.03
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
			Five years after first child's birth			
1	-0.48	-0.22	-0.02	-0.15	-0.11	-0.02
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
2	-0.40	-0.18	0.00	-0.07	-0.14	-0.01
	(0.05)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
3	-0.32	-0.15	-0.03	-0.07	-0.10	-0.03
	(0.04)	(0.04)	(0.01)	(0.02)	(0.02)	(0.01)
4	-0.24	-0.07	-0.01	-0.08	-0.09	-0.02
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
5	-0.24	-0.07	0.01	-0.04	-0.06	-0.06
	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
			Ten years after first child's birth			
1	-0.26	-0.05	0.01	-0.11	-0.06	-0.03
	(0.06)	(0.05)	(0.01)	(0.03)	(0.02)	(0.01)
2	-0.29	-0.13	0.01	-0.03	-0.11	-0.01
	(0.06)	(0.06)	(0.02)	(0.03)	(0.03)	(0.02)
3	-0.18	-0.08	-0.02	-0.04	-0.09	0.00
	(0.05)	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)
4	-0.21	0.00	0.00	-0.06	-0.09	-0.06
	(0.05)	(0.04)	(0.01)	(0.03)	(0.02)	(0.03)
5	-0.08	0.03	-0.02	-0.04	-0.08	-0.02
	(0.07)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)

Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The sample is restricted to parents whose child is born on the 2nd quarter. Bootstrapped standard errors using 100 replications are clustered at the individual level.

G Threat to identification: Possible endogeneity of fertility decisions

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



Fertility decisions may well depend on labor earning which can be decomposed into the product of two terms, working time and hourly wages. In what follows, we provide empirical evidence which limits concerns about each of these terms being a strong predictor of fertility: these two checks make us then confident that, respectively, our estimates are causal within each wage bin, and that these causal estimates do not correspond to specific wage bins.

Does exogenous future earnings loss predict fertility? A first threat to identification indeed stems from possible violations of the common trend assumption upon which our child penalties estimates are based. This assumption would not stand if individuals made their fertility decisions based on unobserved shocks common to both potential treated and untreated labor outcomes. Specifically, this would be the case if women expecting large earnings losses (due to dismissals for instance, or to cuts in the number of paid hours) to occur in the near future were more likely to have children. The parallel trend assumption would therefore not apply post-treatment, which would lead us to inflate the detrimental consequences of children.

In the absence of plausible exogenous shocks to fertility decisions, there is no simple way of quantifying this potential source of bias. However, a recent empirical study by Kleven, Landais, and Søgaard (2019) investigates this issue and observes that, for the third childbirth, child penalties estimated through event study methods do not differ from those obtained by using a sex-mix instrument. Additionally, if high-wage women responded to expected future shocks to their labor outcomes the same way as low-wage women did, this source of bias 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 at the top.

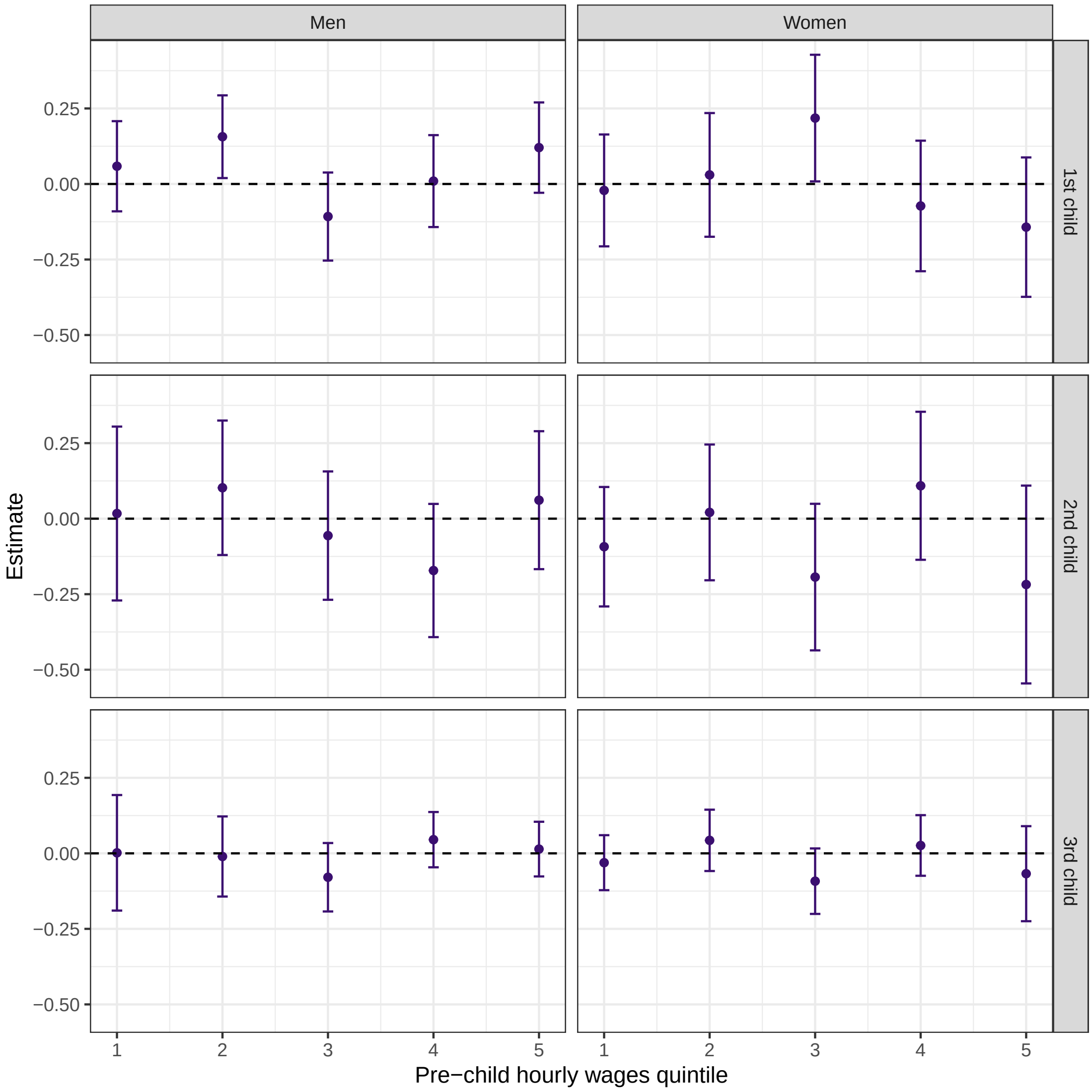
In addition to these arguments, we provide direct evidence that plausible sources of negative shocks to labor outcomes do not trigger problematic fertility responses. First, we estimate how macro-level shocks in the labor market affect fertility decisions.

Within the population of eligible individuals, i.e., those with exactly $n-1$ children at $t-1$, we wonder how much b_t^n, the probability of birth of their nth child at time t, depends on the business cycle:

$$b_{it}^n = \eta^n\{\log(GDP_t) - \log(GDP_{t-1})\} + \kappa_{age_{i,t}}^n + \pi^n t + \xi_{it} \tag{15}$$

where all coefficients are indexed by rank in the recent wage distribution and gender. Coefficients η account for the sensitivity of fertility decisions to macro-level shocks. An endogeneity problem would arise if those coefficients were estimated to be significantly negative, especially at the bottom of the wage distribution. According to Figure G.2, with very few exceptions, this is not the case. This empirical evidence seems apparently at odds with the one documented in the U.S. that points out to rather strong ties between economic conditions and fertility choices (see, e.g., Dehejia and Lleras-Muney (2004)). However, in Europe, such a relationship, if any, sounds looser and more temporary (Bellido and Marcén, 2019; Hofmann and Hohmeyer, 2016). Demographic studies suggest that one hardly observed any immediate decline in fertility consecutive to the Great Recession from 2007-2008: see for instance Figure 2 in Pison (2013), and this was later confirmed by Insee, the French National Institute in charge of Statistics and Economic Studies, in Masson (2015) as far as France is concerned.

Figure G.2 – Probability of having children (sensitivity to the business cycle)



Estimates of coefficients related to log-GDP growth between times $t-1$ and t in a linear probability model with rank in the recent wage distribution × age fixed effects (Equation (15)). The outcome is a dummy variable for having the nth childbirth at time t. Standard errors are clustered at the individual level. The sample includes individuals up to age 60 at time t.

Second, we ask whether micro-level shocks generate such fertility responses. We build on Huttunen and Kellokumpu (2016), who show that job displacement triggers negative fertility responses. We rely on the linked employer-employee nature of our data to identify plausible mass layoff episodes. Namely, we assume that individual i is subject to a firm-level shock f_{it} at time t if more than 25% of individuals working for the main employer[23] of i at time $t-1$, but who are not individual i herself, leave the firm at time t.[24] Within each eligible subpopulation, these firm-level shocks indeed correlate with job losses. We estimate a linear model for the probability l_{it} of being jobless at time t,

$$l_{it} = \rho^n f_{it} + \sigma^n_{age_{it},t} + \upsilon_{it}, \tag{16}$$

where all coefficients depend on gender and the rank in the wage distribution, omitting once again the index n.

Figure G.3 displays the estimates of coefficients ρ, and shows that it is plausible that exogenous firm-level shocks are felt at the individual level: firm-level shocks only moderately increase job loss chances, by about +7pp, regardless of the location in the earnings distribution. We then estimate the probability of having the nth child at time t in a reduced-form fashion:

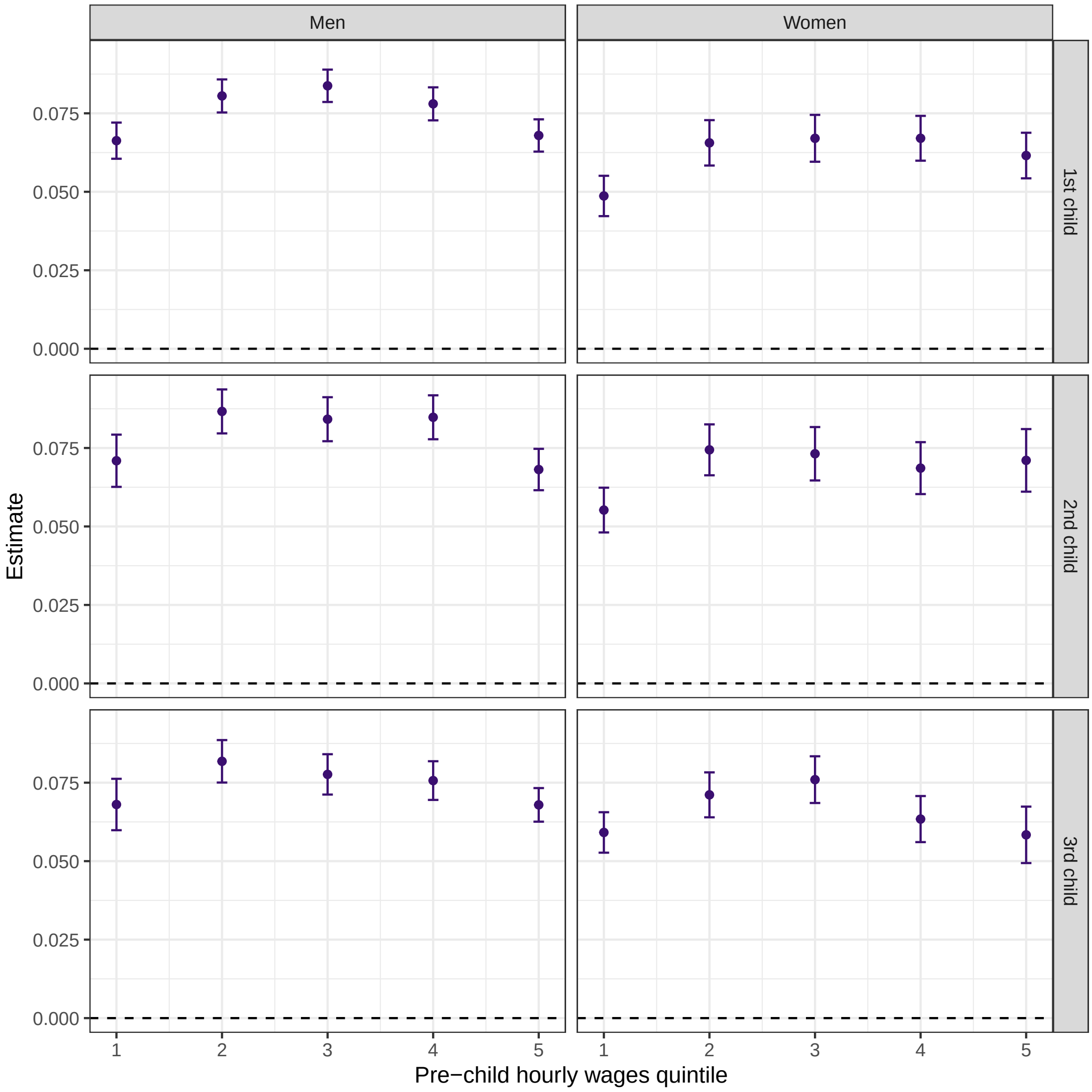
$$b^n_{it} = \phi^n f_{it} + \psi^n_{age_{it},t} + \omega_{it} \tag{17}$$

Figure G.4 displays the corresponding estimates of ϕ. In most cases, we cannot reject the null hypothesis that these coefficients are equal to 0, which suggests that firm-level employment shocks do not trigger positive fertility responses that would

[23]The main employer of an individual is defined as the firm that pays that individual the largest earnings during a given year.

[24]These firm-level shocks are identified from a comprehensive version of the DADS data that allows to track all salaried employees, and not only those included in the DADS panel, from year $t-1$ to year t.

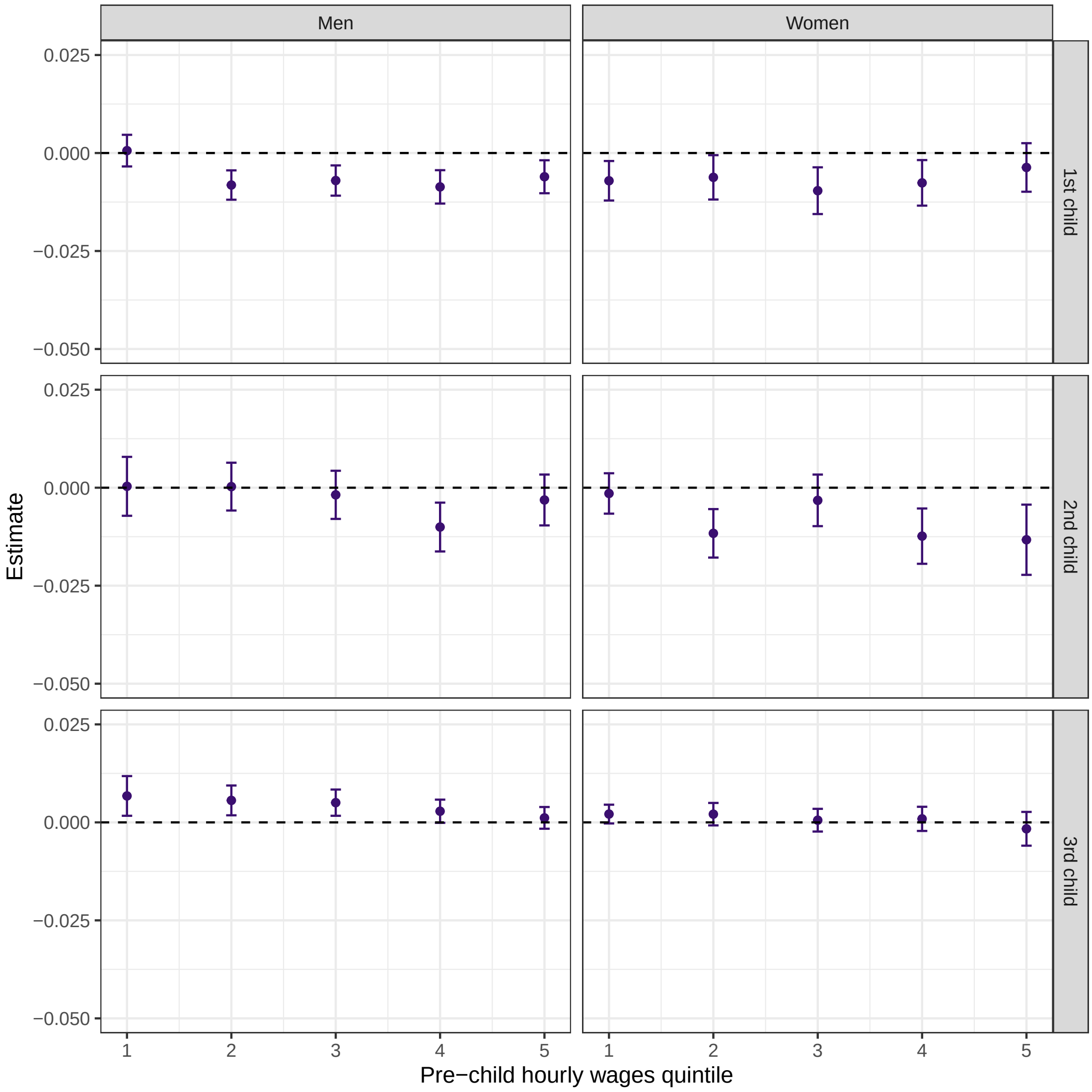
Figure G.3 – Probability of losing one's job (sensitivity to firm-level shocks)



Estimates of coefficients related to firm-level shocks in a linear probability model with rank in the recent wage distribution × age × year fixed effects (Equation (16)). The outcome is a dummy variable for being jobless at time t. Standard errors are clustered at the individual level. The sample includes individuals up to age 60 at time t.

render our estimates of child penalties meaningless.[25]

Figure G.4 – Probability of having children (sensitivity to firm-level shocks)



Estimates of coefficients related to firm-level shocks in a linear probability model with rank in the recent wage distribution × age × year fixed effects (Equation (17)). The outcome is a dummy variable for having the nth childbirth at time t. Standard errors are clustered at the individual level. The sample includes individuals up to age 60 at time t.

[25]This estimation builds upon the comparison of those who worked in an affected firm, regardless of whether they lost their job or not, with those who did not work in an affected firm. As such, it does not assume that those who worked in an affected firm but did not lose their jobs did not experience a decline in their future earnings prospects. On the contrary, they form part of the treated group in this analysis, which aims at capturing the impact of decreased earnings prospects on fertility decisions.

Figure G.5 – Unconditional probability of having a 2nd child (along the wage distribution)

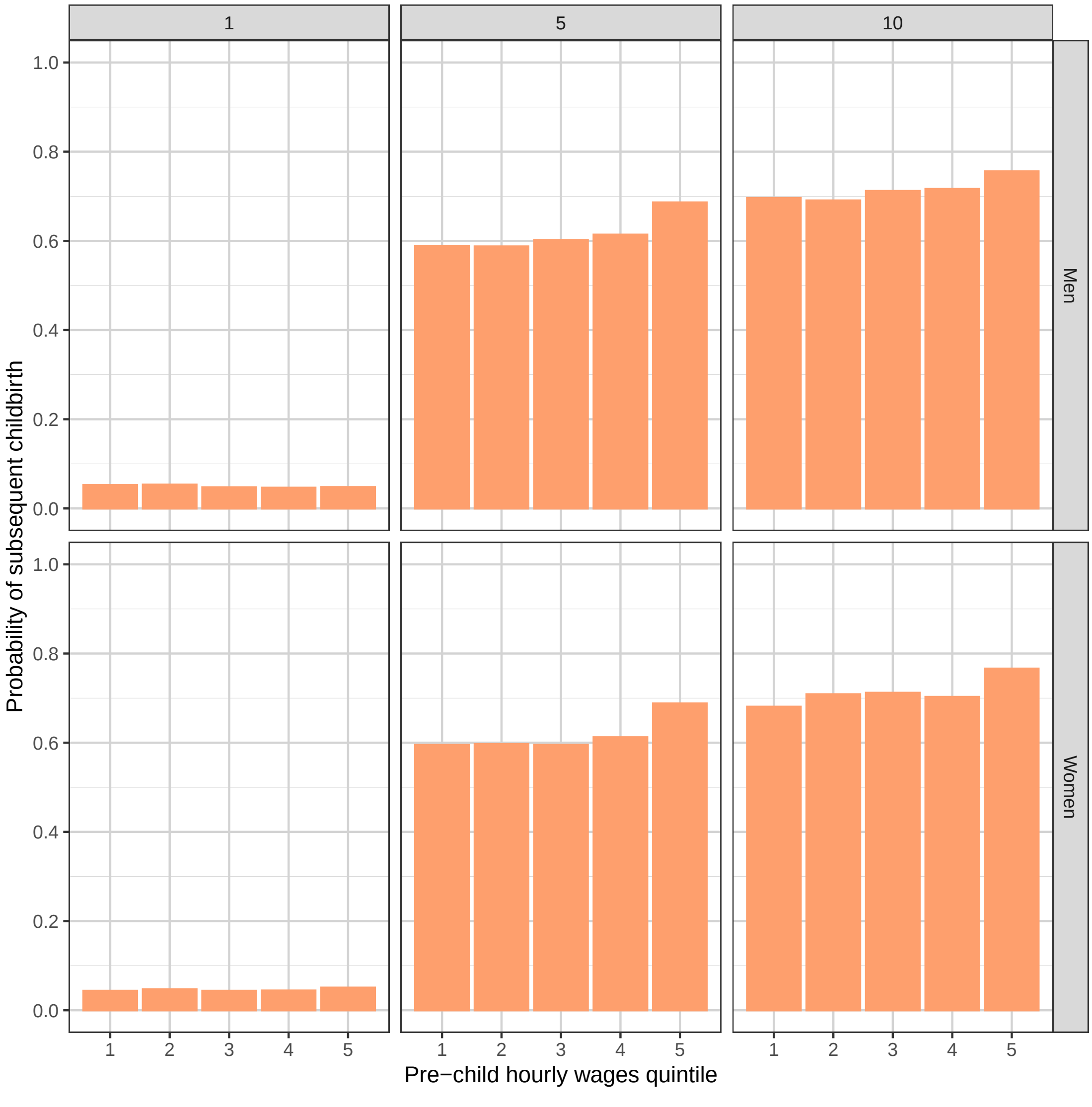


Figure G.6 – Probability of having children

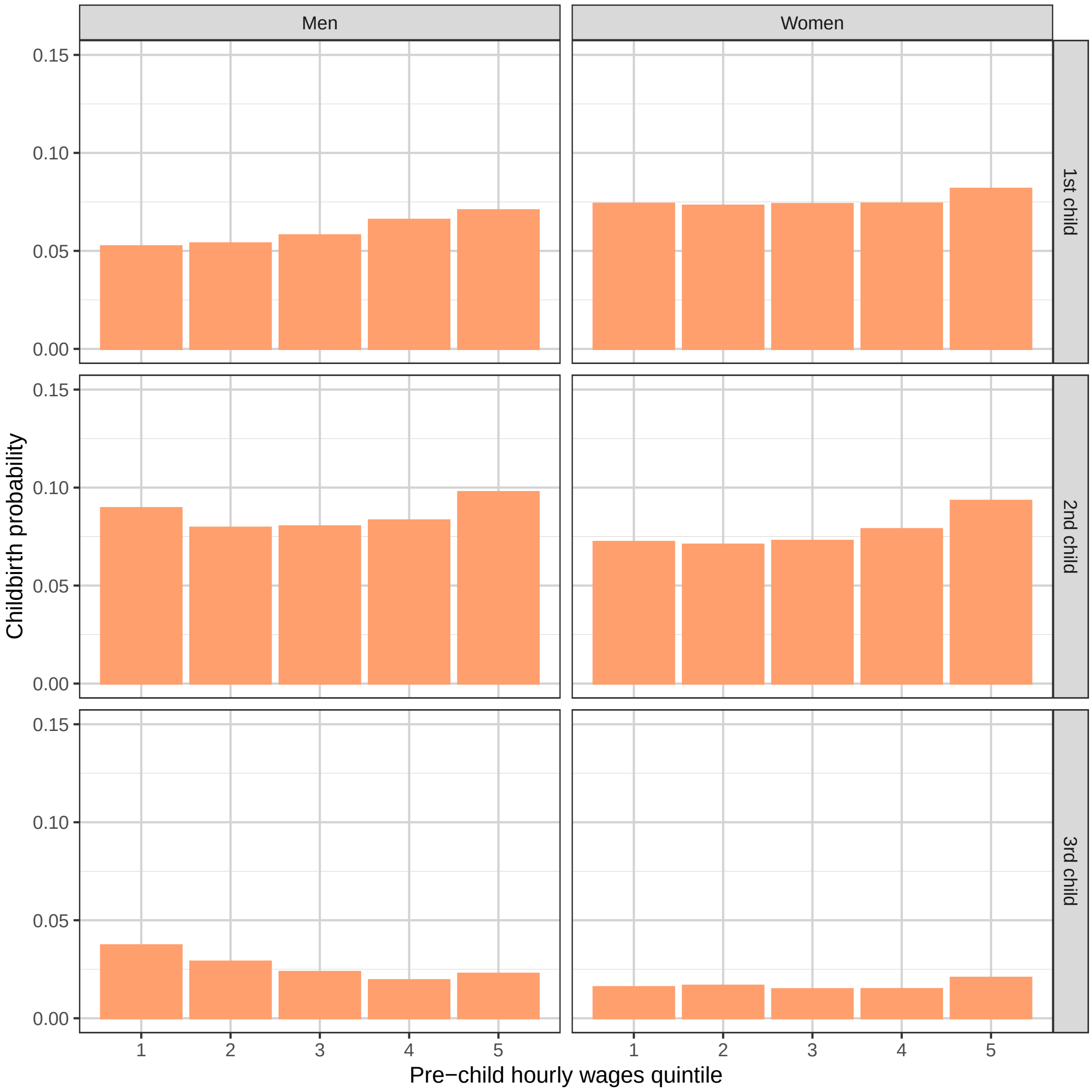
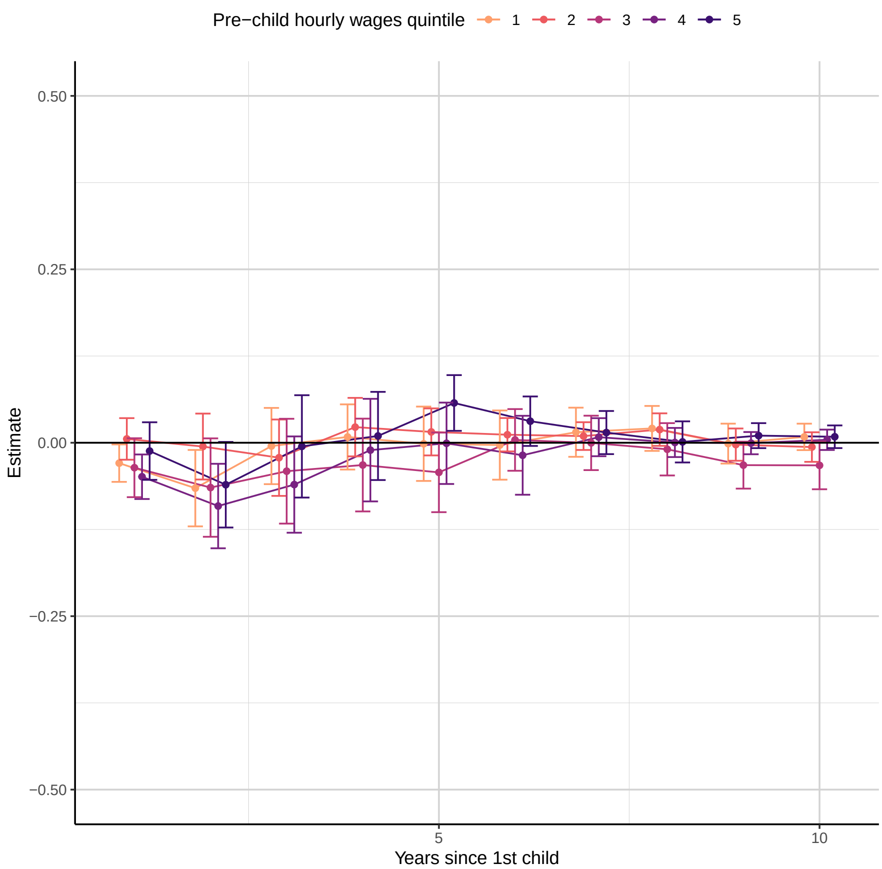


Figure G.7 – Investigating for possible endogeneity in subsequent fertility decisions



Estimates of the probability of having a second child for various values of time-to-first-childbirth expressed in years, net of controls.

Do wages predict fertility? Having shown that within hourly wages categories, a decline in future labor earnings prospects does not result in being more likely to have children, we now turn to differences in fertility decisions across wage levels. This investigation is relevant to this paper for two reasons:

- Our quantity of interest is the impact of parenthood, which begins with the arrival of one's first child (the extensive margin of fertility, i.e. whether to have children or not), but encompasses all potential subsequent children (the intensive margin of fertility, i.e. how many children conditional on having at least one). A a result, differences in (long-run) consequences of parenthood along the wage distribution could partly reflect differences in fertility decisions at the intensive margin. For instance, low-earnings mothers have usually more children than their high-earnings counterparts.

- In the presence of heterogeneous treatment effects, difference-in-difference estimates identify the average treatment effect on the treated (ATT). As a result, treatment effect estimates may be heterogeneous even if high-wage women (as defined by their *potential hourly wages in the absence of children*) incur the same detrimental consequences of fertility, on average (for this pattern to arise, it is indeed sufficient that, among high-wage women, those who face the largest career costs choose not to have children).

To check that the heterogeneity in child penalties along the wage distribution is not driven by high-achieving mothers being more prone to restrict their fertility at the intensive margin, we compute the probability of having at least a second child a few years after the first childbirth. Figure G.5 displays the corresponding estimates. For both men and women, this probability is a non-decreasing function of pre-childbirth hourly wages. In other words, up to 10 years after the first childbirth, parents at the top of the pre-childbirth wage distribution are, if anything, less likely than their counterparts from the bottom of the distribution to restrict themselves to one child only. Hence our results on the heterogeneity of child penalties are not driven by mothers with high hourly wages having less children.

We then assess whether high-wage women are more likely to opt out of parenthood by estimating the probability of child arrival on year t among those eligible, i.e., those who already have $n-1$ children in year $t-1$, along the entire recent wage distribution. Figure G.6 displays our estimates. They suggest that this explanation is not entirely convincing. Indeed, the estimated probabilities are very similar along the wage distribution. If anything, when they are without children, high-wage women are more likely to become mothers in the near future than their low-wage counterparts. As a result, it seems unlikely that their smaller child penalties result from them being more likely to opt out of parenthood if they feel the career consequences are too harsh.

Last, we investigate whether parents who experienced the largest short-run earnings decline following parenthood are also more likely to have additional children in the long run, and we proceed to such an analysis within each quintile wage group. To do so, we regress a dummy that indicates whether an individual has at least two children at time $t+k$ on (i) the individual relative earnings change between $t-1$ and t, and (ii) a set of age $\times$ calendar year dummies. Figure G.7 displays our estimates in the case of women. Overall, they point out to very a weak correlation between having additional children and the short-run effect of becoming a mother. In the end, it seems unlikely that those who experience the most detrimental short-run effects of motherhood select into having more children in the future.

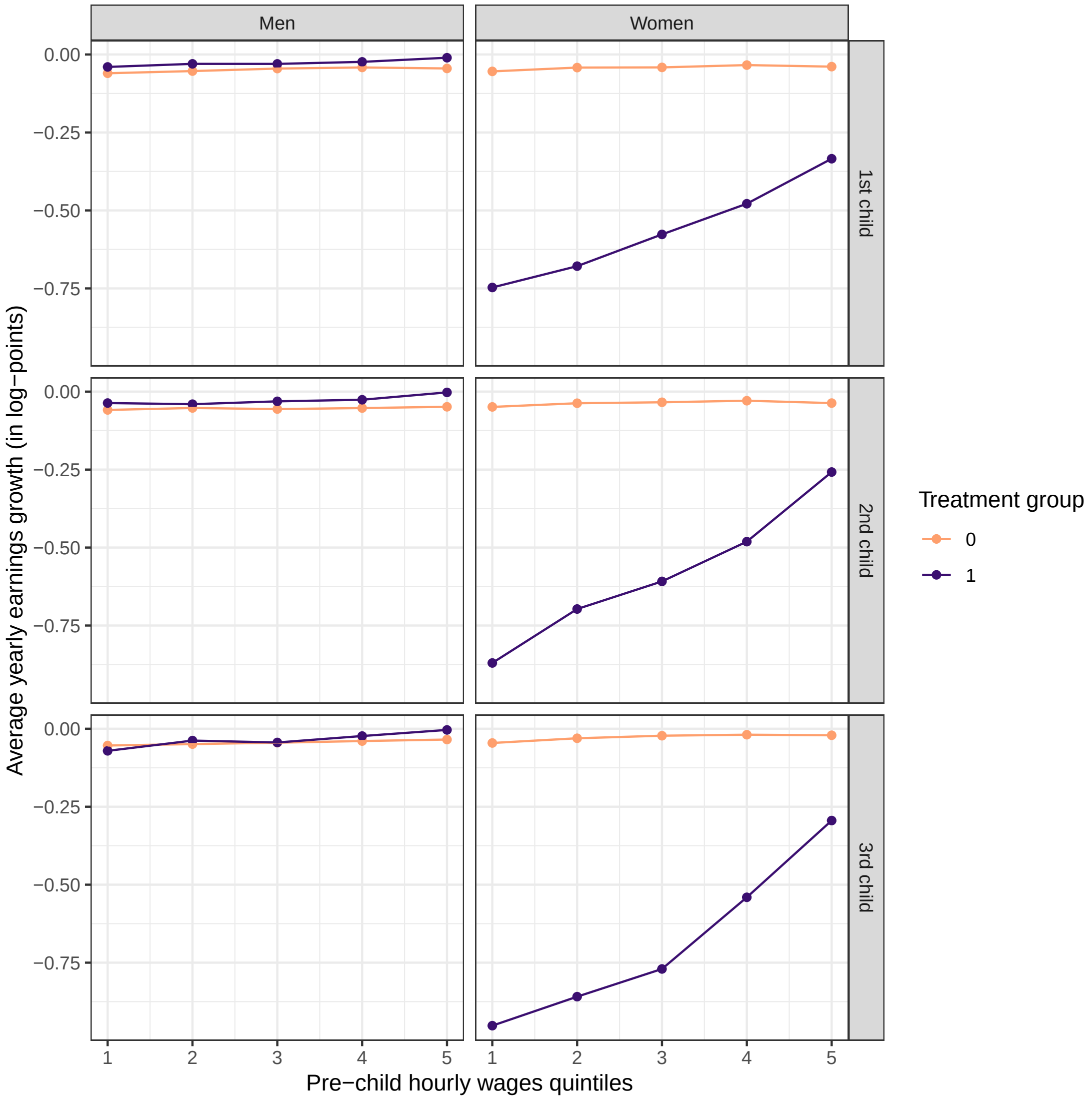
H Threat to identification: Mean reversion

We here address the issue of mean reversion. Our approach that aims at comparing child penalties across the hourly wages distribution requires splitting the sample according to pre-event hourly wages. A potential shortcoming of this approach is that, by doing so, we may end up introducing mean reversion in the estimation: if initial labor outcomes incorporate both a permanent and a transitory component, then women with high initial earnings would be mechanically more likely to experience relative earnings fall in subsequent years. In other words, the assumption that we compare women with different opportunity costs fails if we condition on short-term shocks.

To check that this does not drive our estimates, we first display changes between the last year before the (counterfactual) childbirth in both treatment and control groups, and the (counterfactual) childbirth year. Figure H.1 display our results. Firstly, it makes it clear that heterogeneity in the child penalties stems from the heterogeneity in the earnings changes in the treated group, not in the control group as would be the case if mean reversion were at play. Secondly, the heterogeneity in the earnings changes of the control group does not suggest that we are conditioning on short-term shocks that would entail mean reversion: individuals at the bottom of the distribution are more likely to experience earnings losses, which is consistent with them being more exposed to unemployment risk (Guvenen et al., 2021).

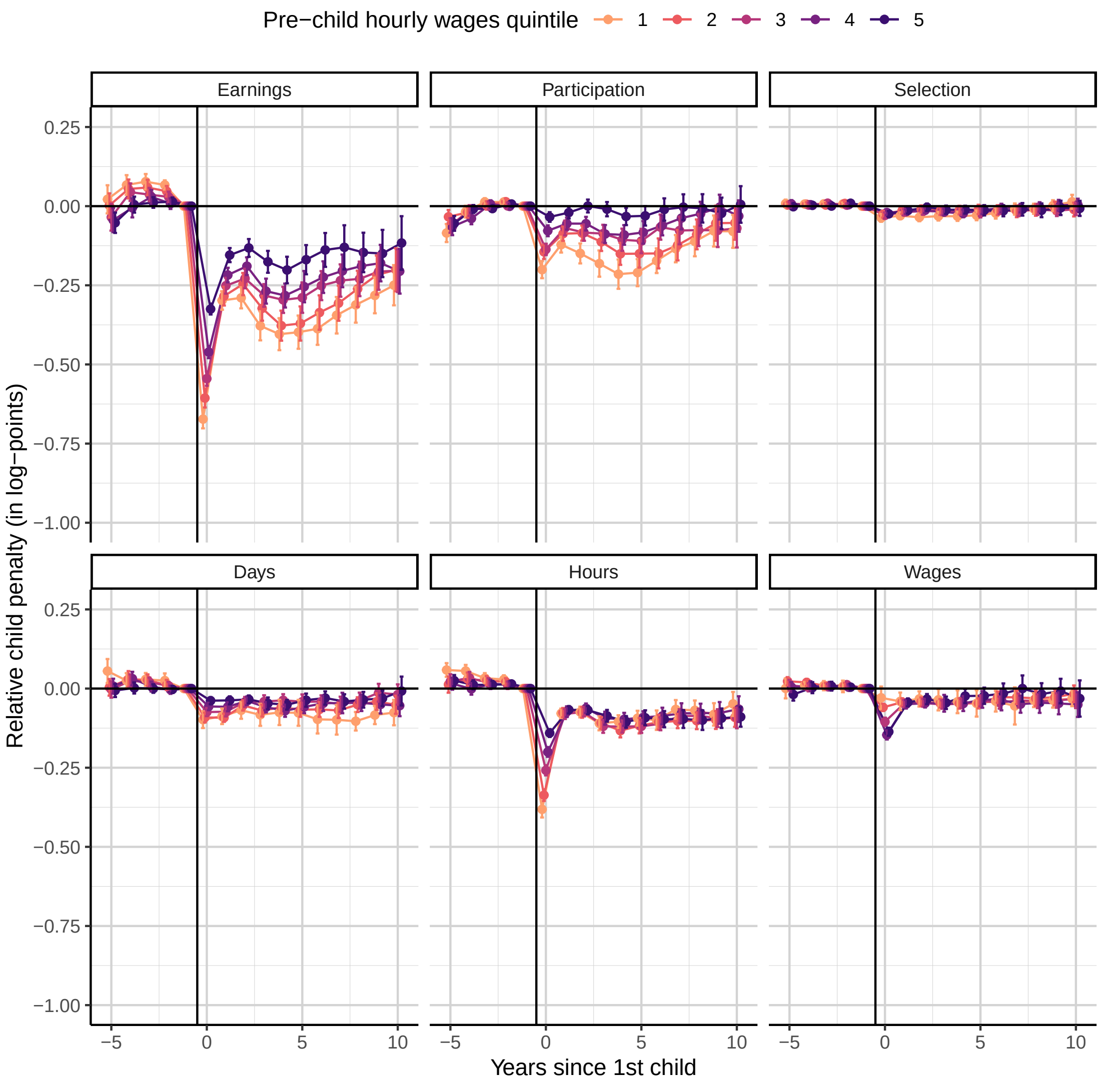
To further insure against mean reversion driving our estimates, we replicate our approach while conditioning on hourly wages as observed 3 to 7 years before childbirth, as opposed to 1 to 5 years before childbirth (Figures H.2 and H.3). The results remain completely similar to our baseline estimates.

Figure H.1 – Underlying earnings changes



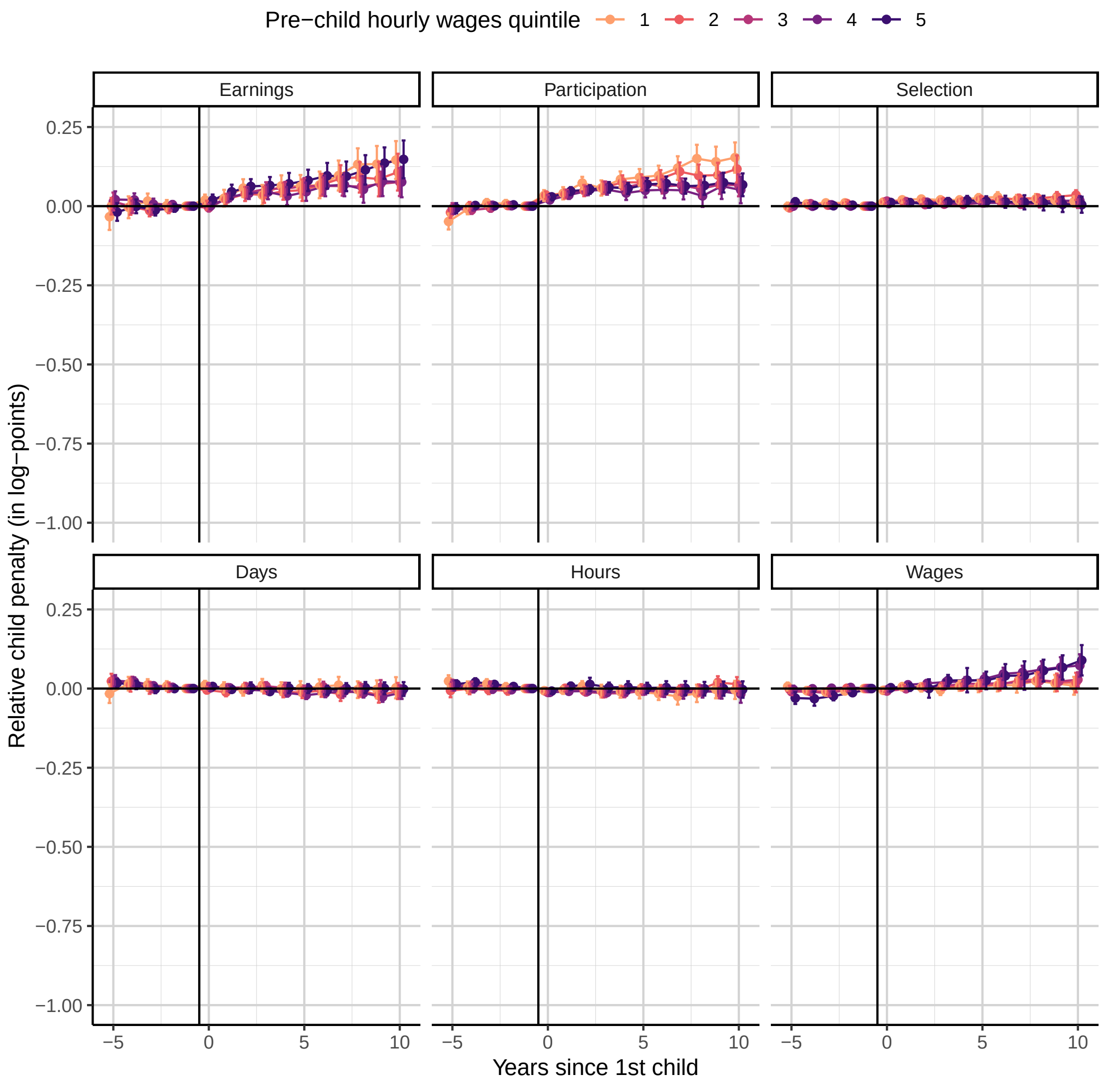
Log-average earnings changes between $t-1$ and t by gender and treatment status, used in Figures 1 and 2.

Figure H.2 – Consequences of first childbirth for women's labor outcomes: alternate ranking



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The ranking is based on average normalized hourly wages as measured between $t-7$ and $t-3$. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure H.3 – Consequences of first childbirth for men's labor outcomes: alternate ranking



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The ranking is based on average normalized hourly wages as measured between $t-7$ and $t-3$. Bootstrapped standard errors using 100 replications are clustered at the individual level.

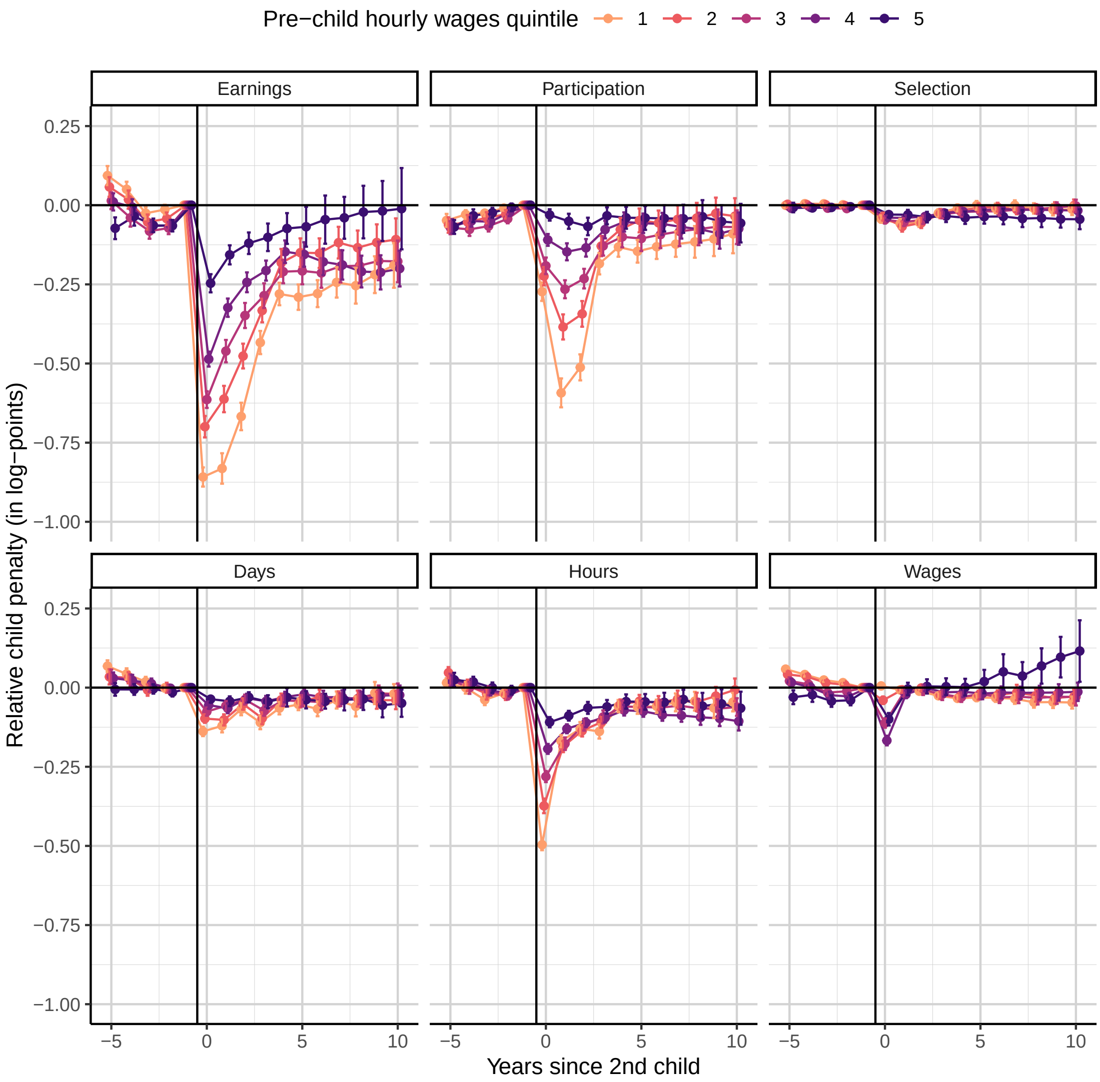
Table 13 – Relative child penalty: impact of the first child on mothers' labor outcomes, in log-points. Ranking based on wages between 7 and 3 years before

Pre-child hourly wages quintile	Earnings	Part.	Selection	Days	Hours	Wages
			One year after first child's birth			
1	-0.30	-0.12	-0.03	-0.09	-0.08	-0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
2	-0.28	-0.09	-0.01	-0.09	-0.08	-0.04
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.25	-0.07	-0.02	-0.07	-0.08	-0.05
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.22	-0.06	-0.02	-0.06	-0.07	-0.05
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
5	-0.15	-0.02	-0.01	-0.04	-0.07	-0.04
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
			Five years after first child's birth			
1	-0.40	-0.21	-0.03	-0.08	-0.09	-0.05
	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
2	-0.37	-0.15	-0.01	-0.07	-0.12	-0.05
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	-0.29	-0.11	-0.02	-0.04	-0.12	-0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
4	-0.25	-0.08	-0.01	-0.06	-0.09	-0.03
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
5	-0.17	-0.03	-0.01	-0.04	-0.09	-0.02
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
			Ten years after first child's birth			
1	-0.25	-0.08	0.01	-0.08	-0.05	-0.03
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
2	-0.20	-0.05	-0.01	-0.05	-0.09	-0.02
	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)
3	-0.20	-0.07	-0.01	-0.02	-0.09	-0.03
	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
4	-0.21	-0.03	0.00	-0.05	-0.06	-0.05
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
5	-0.12	0.01	-0.01	-0.01	-0.09	-0.03
	(0.04)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)

Estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. The ranking is based on hourly wages measured between 7 and 3 years before the (counterfactual) event. Bootstrapped standard errors using 100 replications are clustered at the individual level.

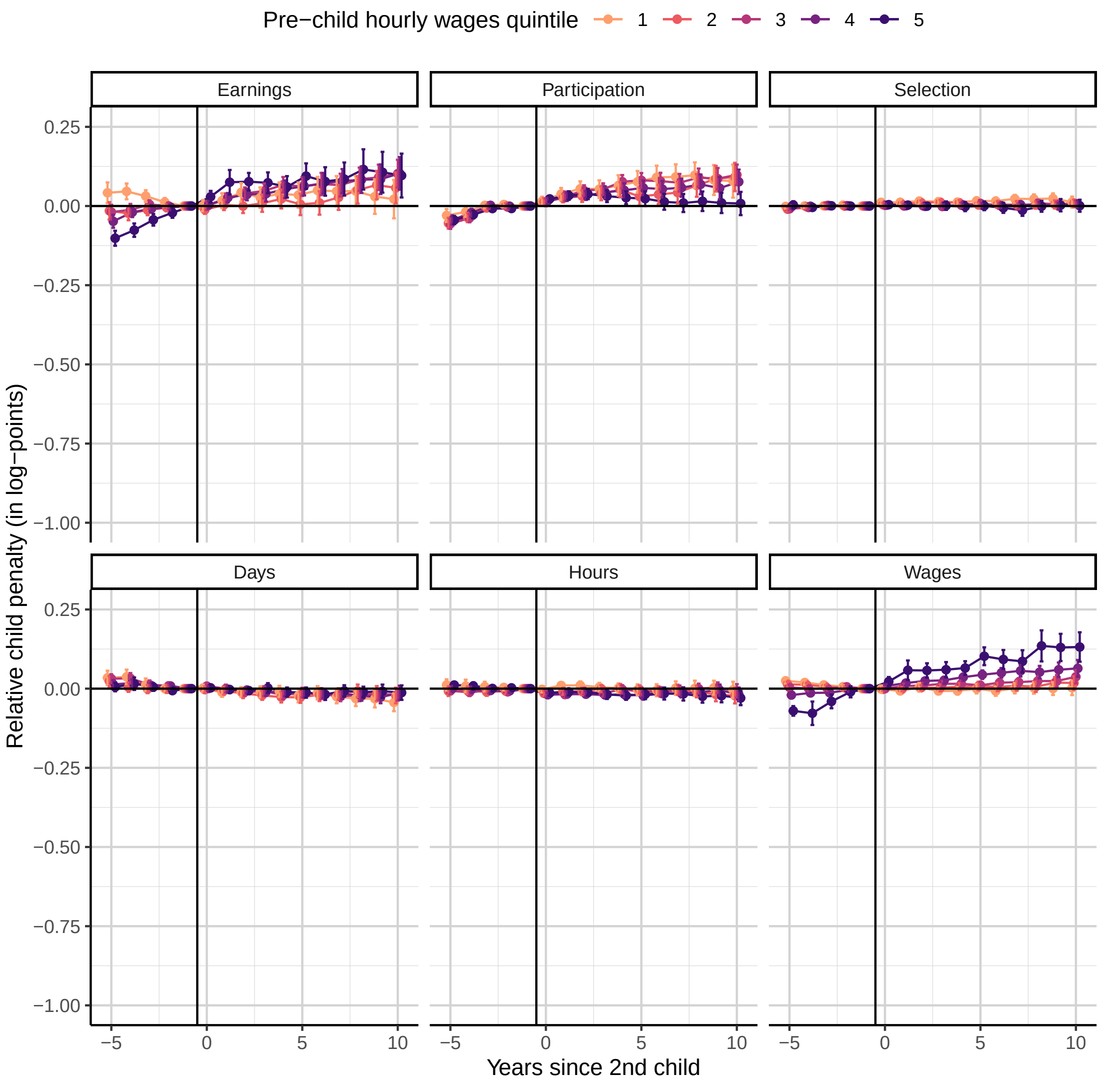
I Child penalties: Later births

Figure I.1 – Consequences of second childbirth for women's labor outcomes



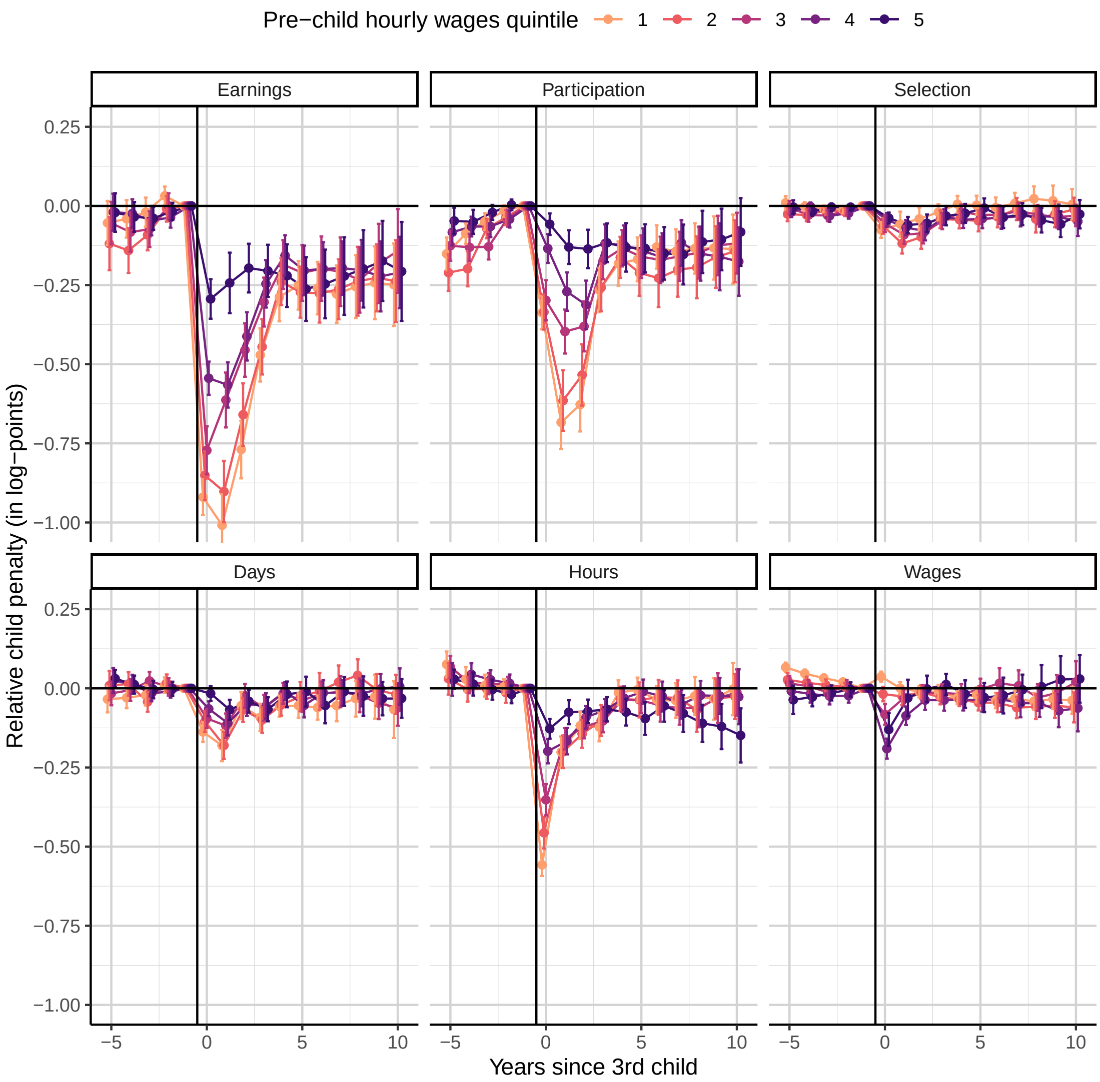
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure I.2 – Consequences of second childbirth for men's labor outcomes



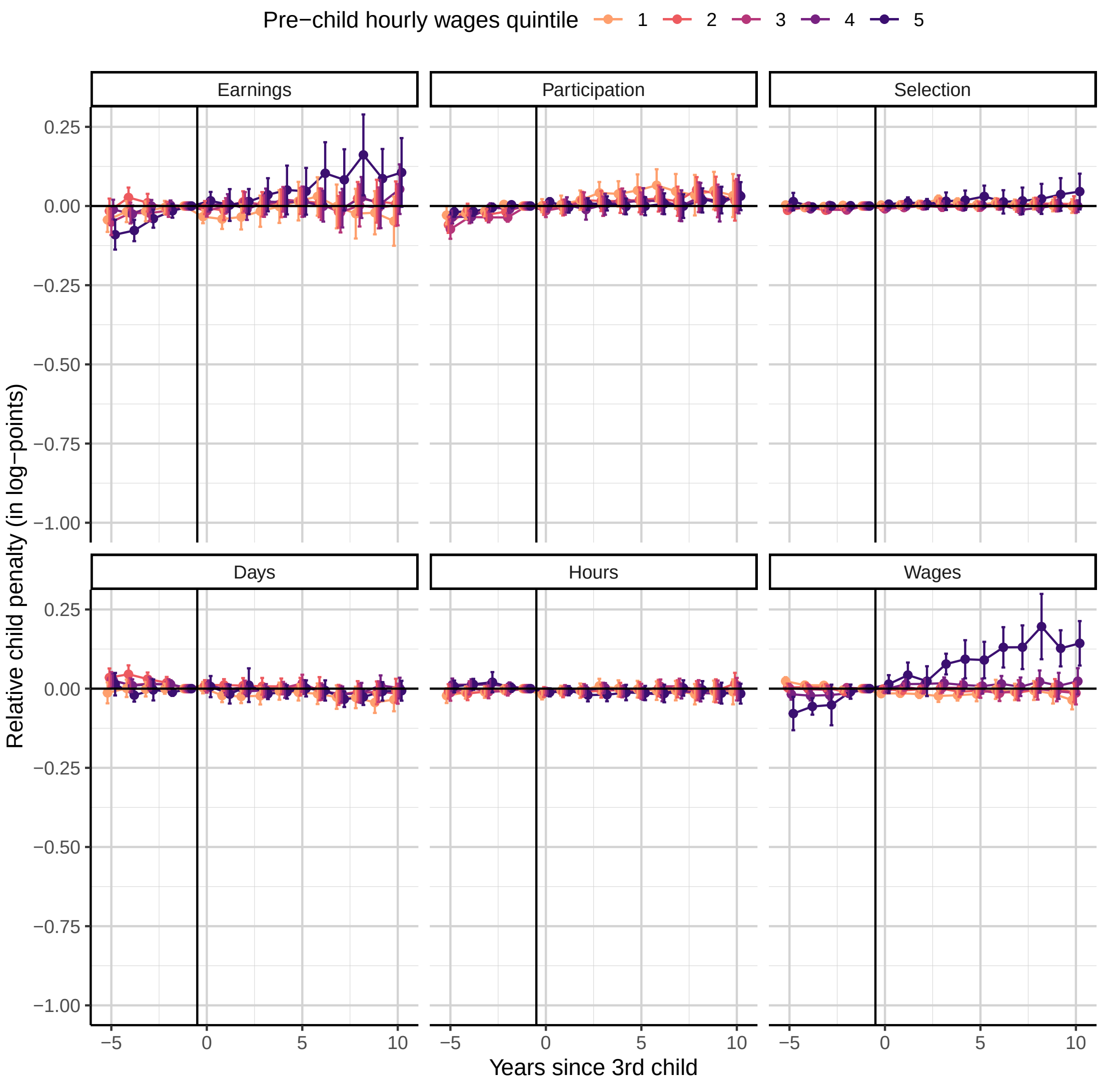
Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure I.3 – Consequences of third childbirth for women's labor outcomes



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.

Figure I.4 – Consequences of third childbirth for men's labor outcomes



Each panel displays the estimates of child penalties obtained by the difference-in-difference method (see Equation (6)) for various values of time-to-childbirth expressed in years. Bootstrapped standard errors using 100 replications are clustered at the individual level.