

# DISSECTING CHILD PENALTIES

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The authors relate mothers' children-related labor earnings losses, referred to here as child penalties, to their location in the distribution of potential hourly wages. Using French administrative data and based on an event study approach, the authors 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. These marked differences suggest that the pervasive part of life-cycle gender inequality associated with parenthood is related to both financial incentives and gender norms.

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**R**ecent 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 article, 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 mothers face between time spent outside the labor force,

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We thank Martin Andresen, Carole Bonnet, Thomas Breda, Alex Bryson, Bertrand Garbinti, Olivier Godechot, Libertad González, Dominique Goux, Camille Landais, Marion Leturcq, Erica Lindahl, Éric Maurin, Dominique Meurs, Ariane Pailhé, Roland Rathelot, Sébastien Roux, Anne Solaz, Andreas Steinhauer, Laurent Toulemon, Grégory Verdugo, and Josef Zweimüller for useful suggestions. We are also grateful to attendees at CASD-IAB Workshop "Advances in Social Sciences Using Administrative and Survey Data" (Paris, 2019), AFSE (Paris, 2018), EALE (Uppsala, 2019), ESEM (Cologne, 2018), ESPE (Bath, 2019), JMA (Bordeaux, 2018), JMS (Paris, 2018), LAGV (Aix-en-Provence, 2019) as well as at Ined and Insee seminars. An Online Appendix is available at <http://journals.sagepub.com/doi/suppl/10.1177/00197939251342494>. For general questions as well as for information regarding the data and/or computer programs, please contact the corresponding author at [lionel.wilner@ensae.fr](mailto:lionel.wilner@ensae.fr).

KEYWORDS: child penalties, difference-in-difference, earnings distribution, gender pay gap, childcare, labor supply

presumably devoted to child-rearing activities and home production, and their foregone labor earnings are crucial determinants of the magnitude of the child penalty. To do so, we contrast women whose opportunity cost of time spent outside the labor market differs from one another. 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 work-force after having 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 restricted 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 (10-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, and 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 method 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 *Déclarations Annuelles de Données Sociales* (DADS) panel, a comprehensive linked employer–employee data set<sup>1</sup> 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 French Permanent Demographic Sample (EDP), including longitudinal birth and marriage records at the individual level. Given the richness of the data set, we are able to consider the abovementioned control and treatment groups at specific locations of the hourly wage distribution.

### **Related Literature and Contribution**

Childbirths affect time constraints and shift women’s labor supply as well as labor market outcomes, which helps explain a substantial share of the

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<sup>1</sup>Filling out the DADS form is a mandatory part of the process of paying payroll taxes.

gender pay gap as shown by, for example, 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, et al. 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, et al. 2019).

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 et al. 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 considered. 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, that is, 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, Landais, Posch, et al. 2019), as well as comparisons of biological and adoptive families (Kleven et al. 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. 2024) all suggest that 1) holding constant norms and preferences, differences in comparative advantage do not translate into differences in child penalties; 2) conversely, holding constant the comparative advantage, heterogeneity in exposure to dissimilar norms is strongly correlated with differences in child penalties. An exception to this trend would be Angelov, Johansson, and Lindahl (2016) who found substantial heterogeneity in child penalties depending on parents’ relative potential earnings.

This article is also related to a few additional studies that have contrasted child penalties among individuals characterized by mothers’ labor market opportunities. First, following Goldin (2014), Bütkofer, Jensen, and Salvanes (2018) compared child penalties across occupations among top earners. Bazen, Joutard, and Périvier (2025) and de Quinto, Hospido, and

Sanz (2021) compared child penalties across education levels in France and Spain. Rabosto and Bucheli (2023) and Andrew, Bandiera, Costa-Dias, and Landais (2021) estimated heterogeneity in child penalties by hourly wages in Uruguay and in the United Kingdom. Both papers found that the effects on labor market outcomes are much larger in the bottom of the wage distribution. We go a step further on French data by showing that the effect of hourly wages remains when the impact of education is netted out, hence somehow disentangling both dimensions, and 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. Second, a small body of sociological literature has been devoted to the distributional impact of the child penalty, following Budig and Hodges (2010). Because of methodological issues regarding the interpretation of quantile regression coefficients, however, it remains difficult to identify the main lessons from this literature (see Killewald and Bearak 2014; Budig and Hodges 2014; England, Bearak, Budig, and Hodges 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, et al. (2019) compared ordinary least squares (OLS) and instrumental variables (IV) methods to address that concern. It turns out that the causal effect of the third childbirth, estimated by sex-mix instruments, differs little from an OLS estimate based on an event-study approach. In this article, we rely to some extent on this result to advocate for our difference-in-difference strategy, and we develop additional tests that enable us to show that endogenous fertility decisions likely do not affect our results.

Finally, this article 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) pointed out that vertical segregation, that is, 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; because of 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).

## Data and Institutional Background

### 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,<sup>2</sup> French firms have to fill out the DADS form—an annual form that is the analogue of the W-2 form in the United States—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 a rate of 4.4%. Because of the comprehensiveness of the panel with respect to individuals' careers, the data are of exceptional quality and have low measurement error in comparison with survey data, in addition to comprising a large sample size and having no top-coding. Also, 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,<sup>3</sup> other job characteristics (the beginning, duration, and end of a period of employment, seniority, and part-time employment), firm characteristics (industry, size, and region), and individual characteristics (age and gender). This information is available as long as individuals work in the private sector. We do not observe business income or earnings from self-employment. We are also able to determine the numbers of male and female employees at each firm by accessing the cross-sectional version of the DADS and using the linked employer–employee data set (LEED). Our main variables of interest are 1) net real annual labor earnings defined as the sum of all salaried earnings over all employers, 2) time worked, measured as the number of paid hours as well as the number of days worked, and 3) hourly wages defined as the ratio of annual earnings and time worked. In Appendix A, we provide further details on the measurement of earnings and time worked. The main point is that, with few exceptions, 1) maternity leave allowances paid by social security are not included in our measure of earnings, 2) mothers are considered to be salaried employees during the entire duration of their maternity leaves, 3) the number of hours worked during the maternity leave is equal to 0, and 4) the number of hours worked (respectively, hourly wages) is overestimated (respectively, 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 merging 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 child-birth is missing before 2002 for individuals born in January, April, or July.

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<sup>2</sup>The absence of DADS as well as incorrect or missing answers are subject to fines.

<sup>3</sup>This information has been available since 1995 only.

For this reason, we first consider individuals born on October 1–4. Additionally, some childbirth-related data are 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.<sup>4</sup> Finally, partial data on education are available in this data set (see Charnoz, Coudin, and Gaini 2011), and they indicate the highest degree obtained at the end of studies.

Our working sample is composed of salaried male and female employees in the private sector with the exclusion of agricultural workers and household employees.<sup>5</sup> We restrict our analysis to individuals aged 20 to 60 living in mainland France<sup>6</sup> between 1998 and 2015. This approach requires us 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 a rate of 0.5%.<sup>7</sup>

The empirical analysis described in the Results section below 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  $t - 1$ .<sup>8</sup> For individuals with very low labor participation, we considered an individual as 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 (respectively, greater than) 1/100 of (respectively, 100). Our working sample has approximately 1.4 million individuals-years of observations, corresponding to nearly 155,000 workers.

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<sup>4</sup>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.

<sup>5</sup>During her career, an individual may work in the public sector, be self-employed, or an “hourly” worker at some point, though. In such cases, we only avail of information related to employment spells when 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.

<sup>6</sup>At the exclusion of the five French overseas departments (French Guiana, Guadeloupe, Martinique, Mayotte, and Réunion).

<sup>7</sup>On top of this longitudinal sample, we also rely on a comprehensive version of the DADS data set 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.

<sup>8</sup>The core results of this article 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 Appendix Figures F.9 and F.10.

## 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 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. Appendix Figure G.1 displays the number of childbirths both in the raw EDP data set and in our final sample.<sup>9</sup> Because we focus on childbirths that occur after individuals have experienced rather stable employment for several years in a row, and because our data cover only salaried employment in the private sector, numerous childbirths are not included in our final sample: We disregard about half of the women who experienced childbirth between 1998 and 2015. These proportions amount to roughly 60% for men during the same period.

## Institutional Background

Family-friendly policies in France have a long history (see Rosental 2010) that dates back at least to pro-natalist concerns during the interwar period between World War I and World War II (Huss 1990). These policies rely on 1) tax cuts, especially the *quotient familial* introduced in 1945, whereby the income tax rate depends on the number of children in a household; 2) various child benefits; and 3) some other welfare benefits, such as bonuses included in retirement pensions that depend on realized fertility and/or housing allowances. In France, income is taxed jointly within households; this scheme is the source of strong incentives toward 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

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<sup>9</sup>The raw EDP data set itself is not perfectly representative of all childbirths that occur in France because it provides information on fertility for only the individuals who have appeared at least once in labor market data, the sample of which has varied over time.

Table 1. Sample Selection

	Base sample				Censoring				Final sample			
	Women		Men		Women		Men		Women		Men	
	# Observations	907,202	1,166,815	778,320	1,062,183	581,921	68,235	832,477	87,148			
# Individuals	97,388	110,818	88,015	104,322								
	Frequency (in %)	Average hourly wages (2015 €)										
<b>Ages</b>												
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
<b>Industry</b>												
Construction	1.7	12.1	11.5	12.9	1.9	12.3	12.0	12.6	2.0	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	16.0	13.3	20.4	10.9	16.2	14.0
<b>Education</b>												
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 dropouts	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

*Notes:* Base sample includes all individuals aged 20 to 60 that have positive employment in the private sector at time  $t$ . Censoring excludes individuals who work less than 45 days a year, less than 1/8 of the legal duration a week, or are paid less than 90% of the minimum hourly wage. Final sample includes only individuals who are over this threshold at time  $t$ ,  $t - 1$ , and at least twice between  $t - 5$  and  $t - 2$ . Figures for the final sample are computed at time  $t - 1$ .

workers by social insurance from 1970 onward. Since 1980, the arrival of the first two children granted a woman a 16-week maternity leave consisting of six 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 eight weeks, consisting of two weeks before childbirth and six weeks after. By contrast, paternity leaves have granted fathers an 11-day leave since 2002 only.

Paternity leaves came into force in 2002 in addition to birth leaves that amounted to three 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 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, various parental allowances 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 de 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)). These child benefits date back to 1985 and appeared with the creation of *Allocation Parentale d'Éducation* (APE), which was initially restricted to mothers of three or more children. APE was extended to mothers of two 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 six months. The CLCA was replaced in 2015 by PreParE, which introduced incentives to split the leave between parents; it amounted to approximately €400 (equivalent to approximately \$440) 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, Pailhé, Recotillet, and Solaz 2013). Another means-tested benefit, the *Complément familial*, is attributed to families with three children or more and amounts to slightly less than €300 monthly.

By 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-minders, and nannies;<sup>10</sup> family structure; and geographic location); nonetheless, they always depend on earnings in a way that makes mothers at the bottom of the wage distribution more likely to stop or reduce their employment activity (see, e.g., Givord and Marbot 2015).

Considering labor supply, the current family insurance program 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 onward; 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. By contrast, the combination of PreParE benefits (€200) with a reduction of childcare expenditures is worth considering for low-earnings mothers: For example, 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. Therefore, 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: For example, that list includes the PPA (*Prime pour l'activité*), a French equivalent of the US earned income tax credit;<sup>11</sup> the RSA (*Revenu de solidarité active*), which is a minimum income<sup>12</sup> that is an important part of the social safety net; pension bonuses granted to parents;<sup>13</sup> and various housing allowances.<sup>14</sup> Moreover, parents eligible for means-tested allowances are entitled to borrow at reduced rates. Last, family-friendly policies may be available within firms; for example, 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.

<sup>10</sup>This very choice itself depends on parents’ earnings; affluent households are more likely to opt for nannies, whereas poor households more often choose child-minders or day nurseries, though there is variation in this respect.

<sup>11</sup>With a typical phasing out from €595.25 for monthly earnings of €687.35 to €173.22 at €1,398 monthly.

<sup>12</sup>The RSA equals €607.75 monthly without a child, along with a supplementary bonus of €200 per child.

<sup>13</sup>The *Majorations de Durée d'Assurance* (MDA) provides extra quarters of coverage.

<sup>14</sup>In the private housing sector, this refers to the *Aide Personnalisée au Logement* (APL), and the *Allocation de Logement Social* (ALS) concerns social housing. The typical bonus amounts to €100 per child.

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 will be heterogeneous along the hourly wage distribution, namely monotone: They should decrease, in absolute, along that ladder.

### 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;<sup>15</sup>  $\tilde{h}_{it}$  denotes the average number of paid hours per day during year  $t$ ; and  $\tilde{w}_{it}$  is the average hourly wages of individual  $i$  during year  $t$ . Hence,

$$(1) \quad y_{it} = d_{it}\tilde{h}_{it}\tilde{x}_{it}\tilde{w}_{it}.$$

### Normalization

Providing estimates of the causal effect of childbirth by comparing parents and non-parents requires netting out other life cycle effects as confounding factors; for example, the number of childbirths an individual has experienced is a nondecreasing function of age. We choose to net out life cycle 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, Karahan, Ozkan, and Song (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:

$$(2) \quad \log(\tilde{z}_{it}) = \sum_c \lambda_c^z 1_{cohort_i=c} + \sum_a \mu_a^z 1_{age_i=a} + \sum_T \nu_T^z 1_{t=T} + \epsilon_{it}^z$$

The identification of age-period-cohort models can be achieved at the cost of normalizations, which we detail in Appendix C. In this article, the choice of normalization is insignificant, given that we rely on the sum

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<sup>15</sup>The number of days in a year is capped at 360 in DADS.

$\hat{\lambda} + \hat{\mu} + \hat{\nu}$  and never 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

$$(3) \quad z_{it} = \frac{\tilde{z}_{it}}{\exp(\lambda_c^z + \mu_a^z + \nu_T^z)}$$

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

$$(4) \quad y_{it} = d_{it} x_{it} h_{it} w_{it}$$

### 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. Consequently, our analysis relies on the definition of such groups based on a measure of recent hourly wages:

$$(5) \quad w_{i,t-1} = \frac{\sum_{\tau=t-5}^{t-1} d_{i\tau} \tilde{w}_{i\tau}}{\sum_{\tau=t-5}^{t-1} d_{i\tau} \exp(\widehat{\lambda_{cohort_i}^w} + \widehat{\mu_{age_{i\tau}}^w} + \widehat{\nu_\tau^w})}$$

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  $\times$  year cell, we rank workers according to their recent wages,  $w_{i,t-1}$ . We use this ranking to divide the sample into five groups depending on their location in that distribution. Hence, we assume that workers within each age  $\times$  year  $\times$  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 numerous at the bottom and relatively less numerous at the top of the distribution, which merely reflects the existence of a gender gap in hourly wages (see Table 1).<sup>16</sup>

One may be concerned by sample selection arising, for instance, due to the wage profile increasing with age within our sample (see Table 1). This outcome could indeed bias our results in favor of larger estimated child penalties if high-income individuals delayed the timing of their first birth, for example. 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

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<sup>16</sup>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.

hourly wages measured between ages 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 because of 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 the Heterogeneous Consequences of Childbirth section [Table 4]), we neutralize the effect of worker age at childbirth, which further mitigates such concerns.

### 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, et al. 2019). We discuss the plausibility of the assumption that fertility decisions are exogenous, and we devise additional tests of its validity in Appendix G as well as concerns about mean reversion driving our results.

The treatment corresponds to the arrival of one's first child during year  $t$ . Our control group is composed of individuals of the same gender without a 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  $t - 1$ .

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.<sup>17</sup> Given 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.

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<sup>17</sup>We thus restrict our attention to the first three childbirths, namely 96% of childbirths.

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 the following:  $b_{it}$  is a dummy for the arrival of the first child during year  $t$ ,  $c_i$  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.<sup>18</sup>

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

Notably, we do not 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 in the first stage in our empirical approach.

Considering the causal impact of childbirth  $\beta^{y,k}$  being identified on a subset of time periods that depends on  $k$ , we assume that treatment effects are time homogeneous, that is, having a  $k$ -year-old first child bears the same consequences if the child were born in 1998 as it does if she were 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 of 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.

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<sup>18</sup>Given the time period that our data set covers, this implies  $\mathcal{T}_k = [1998, 2015 - k]$ .

Decomposition (7) states that average normalized earnings growth can be represented as a sum of its four components, plus a selection term given 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:

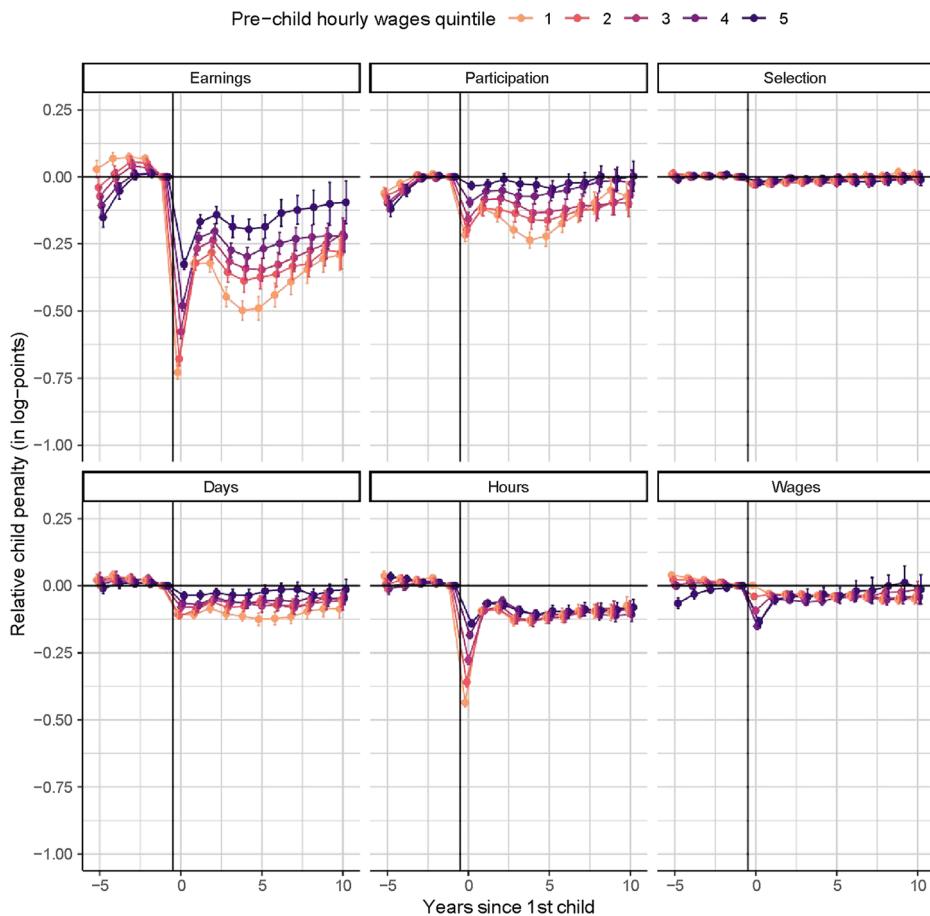
$$\begin{aligned}
 (7) \quad & \underbrace{\log\left(\frac{E[y_{i,t+k}]}{E[y_{i,t-1}]}\right)}_{\text{Labor Earnings Changes}} = \underbrace{\log(P(d_{i,t+k} = 1))}_{\text{Participation}} \\
 & + \underbrace{\log\left(\frac{E[y_{i,t-1}|d_{i,t+k} = 1]}{E[y_{i,t-1}]}\right)}_{\text{Selection}} \\
 & +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

This decomposition is made in an accounting sense.<sup>19</sup> 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  $\beta^s$  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).

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<sup>19</sup>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 us to quantify, in an accounting sense, the contribution of the extensive margin of employment, which is relevant in this particular setting.

*Figure 1.* Consequences of First Childbirth for Women's Labor Outcomes



*Notes.* 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.

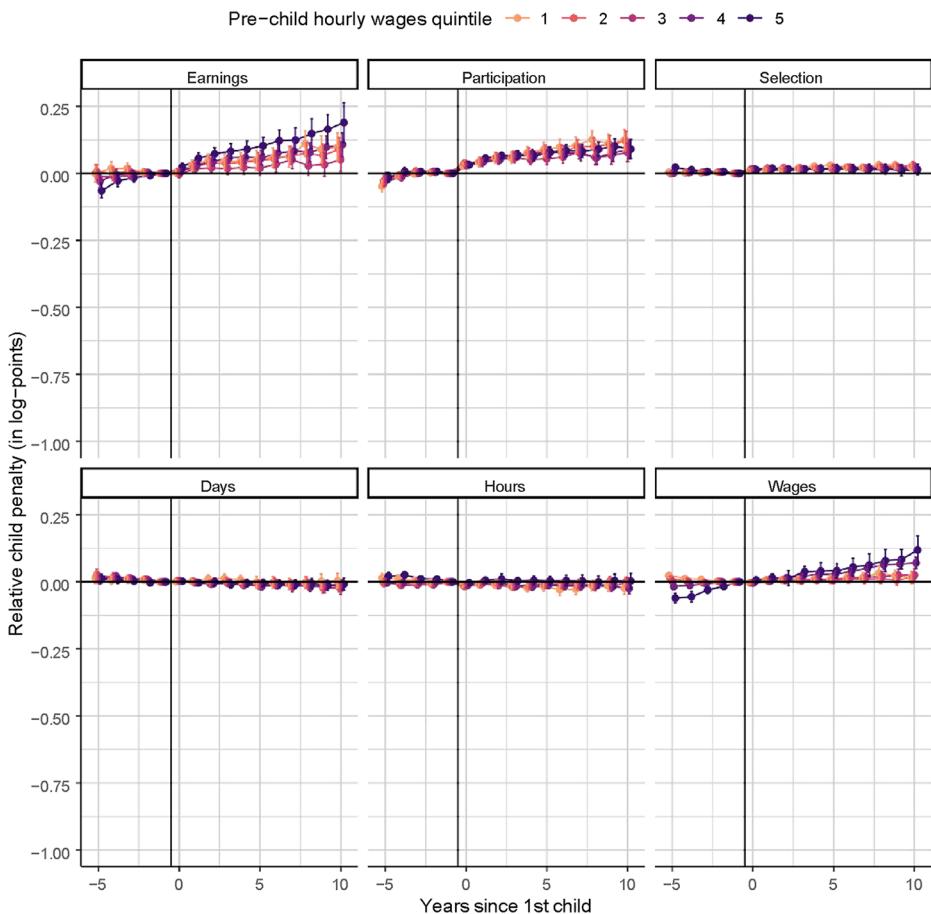
## Results

### 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  $t + k \in \{t - 5, \dots, t + 10\}$ . Tables 2 and 3 display the corresponding estimates one year, five years, and 10 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. On 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

*Figure 2.* Consequences of First Childbirth for Men's Labor Outcomes



*Notes:* 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.

least 10 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) estimated that the child penalty amounts to 40% in the short-run and to 30% in the long-run.

Of more interest, 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

*Table 2.* Relative Child Penalty: Impact of the First Child on Mothers' Labor Outcomes, in Log-Points

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

*Notes:* 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.

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%) 10 years after the arrival of a child. By 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 child-birth reduces by 12 log-points (11%) the probability that women are

*Table 3.* Relative Child Penalty: Impact of the First Child on Fathers' Labor Outcomes, in Log-Points

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

*Notes:* 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.

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 of 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 10 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,<sup>20</sup> 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. These differences are less than 12 log-points (1%), however, 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 wages. Two competing explanations may prevail, though: 1) 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;<sup>21</sup> and 2) 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 still would require within-firm mobility by both parents and non-parents, which is less likely in smaller firms due to the limited mobility bias. Again, monotonicity suggests that the underlying mechanism has to do with the opportunity cost of time, although there is *a priori* no particular reason why this form of employer 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.

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<sup>20</sup>This assumption deals with trends in potential outcomes (absent childbirth) after childbirth.

<sup>21</sup>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. Our approach compares individuals who have children at some point in time with others who do not. For the former group, the age at which they have their first child is their age at childbirth. For the latter group, the age at childbirth cannot be defined: The age at which we consider such individuals is an age at *counterfactual* childbirth.

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 from 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 et al. (2025) and de Quinto et al. (2021). We find smaller child penalties for more-educated mothers (see Appendix 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.<sup>22</sup> Table 4 displays the results we obtain when replicating previous analysis on reweighted data. Though slightly less heterogeneity is 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.

### 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

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<sup>22</sup>To that end, we consider 1) education, measured by the highest degree obtained at the end of studies, as an 8-level variable; 2) recent labor participation at all margins between year  $t - 5$  and  $t - 1$ ; 3) the share of females working part-time for the main employer of each individual at time  $t - 1$ ; and 4) 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 is written  $P(R = r|G = g)|P(R = r|G = g, X = x_{it})$ , where  $x_{it}$  corresponds to the observed variables upon which our reweighting procedure is based. Specifically, we take as  $P(R = r|G = g, X = x_{it})$  the predicted probability of belonging to rank  $r$  in the distribution based on an ordered logit.

*Table 4.* Relative Child Penalty: Impact of the First Child on Mothers' Labor Earnings, in Log-Points (Estimates Based on Different Reweighting Approaches)

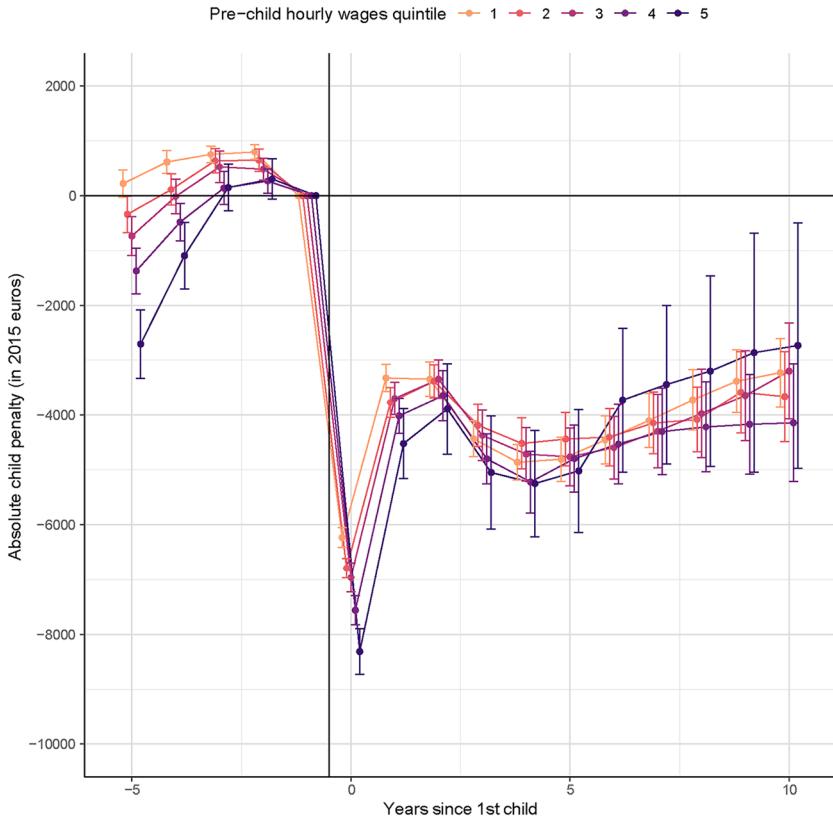
<i>Pre-child hourly wages quintile</i>	<i>Baseline</i>	<i>Reweighted on education</i>	<i>Reweighted on pre-birth observables</i>	<i>Reweighted on all observables</i>
One year after first child's birth				
1	-0.32 (0.03)	-0.30 (0.02)	-0.30 (0.02))	-0.30 (0.02)
2	-0.32 (0.01)	-0.32 (0.02)	-0.32 (0.02)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

*Notes:* 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 fertility-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.

use surveys (Champagne, Pailhé, and Solaz 2015), in 1999 French women devoted 82 minutes per day to childcare, on average, approximately 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.

To the best of our knowledge, this simple exercise is rather new to the literature (although Kleven, Landais, and Leite-Mariante [forthcoming] adopted a similar approach on the employment rate), and it sheds interesting insights on the underlying mechanisms driving observed behavior on the

*Figure 3.* Consequences of First Childbirth for Women's Labor Outcomes  
(in Absolute Terms)



*Notes:* 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.

labor market. Previous heterogeneity partly reflects the smaller base for low-wage women. Those women may be unwilling to spend time in the labor market given the magnitude of that nominal penalty, hence the effects at both extensive and intensive margins documented before. By contrast, high-wage women incur a higher opportunity cost when not spending time in 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.

## Conclusion

This article investigates whether mothers with labor market opportunities that differ from one another have different children-related labor market outcomes, which ultimately translate into earnings losses. To study this trend, 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 children have a large and negative impact on

mothers' labor earnings, regardless of their wages, 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 from the workforce and/or 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 approximately €5,000 annually. By contrast, fathers' labor market outcomes seem almost independent of this cost. This finding 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 2024).

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