The Shock of a Birth: What Makes Babies So Expensive?*

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This paper estimates the costs of having children and presents the trade-offs that household make to fund these additional costs. We use monthly household expenditures linked to tax declarations to document a household's consumption trajectory starting right after a member becomes pregnant until 20 months after childbirth. We find that household consumption rises 18.6 % after childbirth but that this increase is accompanied by strong substitution effects, with spending usually directed toward transportation, recreational activities, and hotels and restaurants shifting toward healthcare, childcare and retail trade. Mothers' labor market participation comes with higher childcare expenditures: households with employed mothers spend 20 p.p. more on childcare than those with nonemployed mothers. We also estimate the opportunity costs of childbirth in terms of earnings, showing that mothers primarily bear this cost. We establish a link between expenditures and this opportunity cost: mothers more likely to have access to nonmarket childcare exhibit smaller labor market gaps, which suggests that the size of the child penalty on women's labor market outcomes varies with the direct cost of having and caring for a child. Finally, we discuss how groups with different fertility choices have different returns to their labor market experience, which, if unaccounted for, might bias standard child penalty estimates.

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1 Introduction

Children are an *expensive* blessing. But what exactly *are* the costs of having children? These extend beyond just the monetary expenditures involved in having a child (*"direct"* costs). They also encompass time and effort, influencing household consumption behavior and the distribution of activity inside and outside the household (*"indirect"* costs). From fertility choices to labor market dynamics, particularly those of women, having children has a profound impact on earnings and family life. Moreover, an understanding of the degree of association of the direct and indirect costs can offer valuable insights into household behavior and provide opportunities for more effective policy design.

This paper estimates the direct and indirect costs incurred by a household when a couple has its first child; we focus on the period from conception onward and present the trade-offs involved in funding these additional costs. That said, identifying these costs presents significant challenges. Let us start by considering the direct cost of childbirth, which entails a rise in household expenditures. Traditional methods, relying on expenditure surveys, have limitations tied to the nature of the data. Surveys are cross-sectional, which restricts the researcher's ability to track household expenditures over time and account for changes in household expenditure behavior. Additionally, the level of detail can be constrained by survey design. To address these limitations, existing methods focus on the demand for specific goods and normalize with respect to the consumption of households without children. Considering the indirect costs, researchers generally proxy them by the *child penalty*, usually computed as the differential earnings around childbirth relative to the earnings of households with different fertility choices. However, the validity of this measure is questionable if parents and nonparents have different career trajectories, which potentially affects their earnings and expenditure behavior. As a result, current estimates of the costs of having a child—both direct and indirect—might be biased.

In the first part of this paper, we use data on monthly household consumption expenditures from an electronic invoicing program in Portugal, linked to information on household structure and dates of birth from personal income tax declarations. We provide a consumption distributional analysis around a child's birth, comparing changes in the size and composition of household expenditures across three distinct stages: pregnancy, pregnancy confirmation, and the first 20 months of the child's life. By exploiting the panel dimension of our data, we can directly compare spending patterns during these critical periods to those in the prepregnancy period. This approach effectively controls for household-specific preferences, enabling us to isolate the precise change in expenditure behavior attributable to the new baby.

We define the cost of having a child by considering the changes relative to spending in the prepregnancy period. This method avoids normalizing the estimates with respect to an external reference group, which could introduce bias arising from inherent differences in the latter's spending, earnings, risk tolerance, time discounting, or product quality preferences. By leveraging the panel structure of our data, we effectively control for these underlying household preferences, yielding a more accurate estimation of the direct costs of raising a child. We find a recomposition of expenditure at different times. The analysis reveals a sizeable increase in total household expenditures beginning six months prior to childbirth. On average, these expenditures are 18.6% above their prepregnancy levels. Nevertheless, we find large substitution effects: there is an expenditure recomposition driven by a reallocation of resources from transportation (-19.5%), recreational activities (-15.3%), and hotels and restaurants (-21.6%) toward healthcare (+29.1%) and retail trade (+37.3%). There are further adjustments in spending patterns following childbirth, with a rise in utility bills and a later surge in expenditures related to childcare and education. We compare the results by mother's labor force participation, finding that households with employed mothers spend by approximately 20 p.p. more on childcare than those with nonemployed mothers. These findings highlight the different ways in which families adapt their spending habits to accommodate the changing needs and life stages associated with childbirth.

Next, we estimate the indirect costs of having a child, which we proxy by the child penalty following the standard approach of Kleven *et al.* (2019b). Research suggests this penalty can be substantial, ranging from 20% to 60% of women's earnings and contributing to the overall gender pay gap (Kleven *et al.*, 2019a, 2023; Casarico and Lattanzio, 2023). The impact extends beyond wages, affecting all labor market outcomes—including hours worked, total earnings, and even the decision to participate in the workforce altogether. We use an event-study design relying in detailed data on work histories linked to information on births. Our analysis reveals a 17% decrease in earnings following childbirth for mothers in Portugal relative to the earnings of nonmothers. This decline is almost entirely driven by reduced female labor force participation. Moreover, we observe minimal changes in hours worked and little adjustment in productivity, reflected in a small decrease in hourly wages of approximately 3%. We further conduct a heterogeneity analysis that considers worker characteristics, firm attributes, income levels, and household composition. These results consistently emphasize the significance of labor force participation, with the penalty being larger for women with less workplace flexibility, and driven by both tails of the distribution.

To further investigate the link between direct and indirect costs, we examine the role of childcare supply in mitigating the child penalty. Building on our initial approach, we

divide the sample into two groups: mothers with and without access to nonmarket childcare. In particular, we identify mothers with a retired parent or parent-in-law (her child's grandparent), who are a potential source of informal childcare. We find a 30% reduction in the child penalty for mothers who have access to a nonmarket caregiver compared to the penalty suffered by those who do not. While both groups experience a similar initial decline in earnings immediately following childbirth, the decrease persists for mothers without access to grandparental care. This result suggests that the ability to rely on grandparents for childcare allows mothers to maintain higher labor force participation, thereby mitigating the long-term impact of the child penalty. This is especially relevant because it shows the relationship between the child penalty, childcare costs, and female labor force participation. In all, the size of the child penalty to women's labor market outcomes seems to be proportional to how expensive having and caring for a baby is.

In the last part of the paper, we deal with a key concern in constructing costs estimates: the comparability of the reference group. Similar returns to labor market experience across fertility groups would suggest that their career trajectories are comparable within and between periods. Conversely, if returns to experience differ, particularly with respect to the wage growth of nonparents, child penalty estimates could be overstated. By combining work histories and births, we are able to track the impact of childbirth on careers from the time of labor force entry. We employ a novel approach that combines the method of Manning and Swaffield (2008), which accounts for career gaps and the consequent differential labor market participation, with the approach of Di Addario *et al.* (2023), which considers differences in pay premia from job mobility and wage growth. This allows us to obtain precise estimates of the differential returns to experience across fertility groups. The conditional estimates show different returns to labor market experience by fertility choice at entry. Average wage growth is 10% higher for nonparents than for parents. This difference shrinks gradually and disappears after the 10^{th} year in the labor market.[1] Since the arrival of the first child is expected to occur during parents' sixth year in the labor market, our results challenge researchers' standard approach of choosing nonparents as the reference group when measuring child penalties.

Related Literature We contribute to four strands of literature. First, we contribute to the literature on identification of the direct costs of having children and, more precisely, the direct cost of the first newborn. While common approaches to calculating the child cost are either to calculate the hypothetical cost of a "baby budget" or to compare average expenditures between families with and without children (Lindert, 1980), both methods suffer from drawbacks. The most common method, the equivalence scale, proxies the

[1] This is more than 10 calendar years since we account for work gaps in workers' careers.

child cost by the income required to maintain the household's prechild standard of living. In the absence of direct prebirth data, these methods frequently normalize spending patterns against those of households without children. The Engel approach, the most used despite its known limitations, involves comparing the demand for essential goods (food) across households of varying compositions, adjusting income levels until an equivalent welfare level is achieved. Another approach is the Rothbarth estimator, which proxies the cost by the income adjustment but, on the other hand, utilizes variations in spending on adult goods—e.g., tobacco, clothing, leisure, and alcohol—to account for substitution in consumption patterns (Deaton and Muellbauer, 1986; Phipps, 1998; Bradbury, 2008; Bargain and Donni, 2012; Browning *et al.*, 2013). Our article adds an additional layer to the literature, measuring the cost of having a child considering the expenditure recomposition and increase with respect to the prepregnancy period. Our use of administrative data allows us to control for unobserved household characteristics and provide novel evidence of consumption recomposition across time due to childbirth.

This paper is the first to estimate the child penalty in Portugal using administrative data. Research by Kleven *et al.* (2023) relies on survey data and focuses only on labor force participation. We provide estimates for several other labor market outcomes and examine effect heterogeneity along different dimensions. Two of our results extend our general understanding of the child penalty. The first is the evidence of rapid change in households' gender norms, manifesting in the different child penalties across different female cohorts. While the firm type has a role in the child penalty (Casarico and Lattanzio, 2023), a large part of the other factors in this penalty relates to cultural characteristics and gender norms (Andresen and Nix, 2022; Kleven *et al.*, 2019a, 2023). While the evidence on these drivers for the US spans a century (Goldin, 2021), rapid changes in the Portuguese labor market for women and the rapid modernization of its economy make the results for Portugal consistent with those for the US. The second result adds to the literature on the crucial role of childcare supply in mitigating the child penalty. One strand of the literature focused on the existing link between childcare access and female labor force participation (Givord and Marbot, 2015; Havnes and Mogstad, 2011; Bick, 2016). Relying on existing models (Cardia and Ng, 2003; Fernández and Wong, 2014; García-Morán and Kuehn, 2017), we contribute additional empirical evidence on how grandparents can support parents in providing childcare, affecting the incidence of the penalty (Arpino *et al.*, 2014; Aparicio Fenoll, 2020; Yu *et al.*, 2023). More importantly for this paper's purposes, the fact that our analysis shows a reduction in the child penalty for mothers who potentially have access to informal childcare is evidence of the relationship between the indirect and direct costs of children in the household.

This paper also contributes to the vast literature estimating the returns to job tenure. There is a recent interest in understanding career profiles and differential wage growth (Adda and Dustmann, 2023). While previous literature focuses on gender (Manning and Swaffield, 2008; Del Bono and Vuri, 2011) and starting points (Arellano-Bover, 2024), we document differential wage growth by workers' fertility choices. Finally, we contribute to the growing literature using administrative data to estimate consumption responses to shocks (Landais and Spinnewijn, 2020). While other papers in this stream study shocks to health (Kolsrud *et al.*, 2020), our paper is the first to show a change in consumption and its composition due to childbirth.

Outline. The remainder of the paper is organized as follows. Section (2) describes our main data sources and the estimation samples. Section (3) explores changes in consumption patterns when a couple has a child, including the differential recomposition in consumption along the earnings distribution. Section (4) presents a set of stylized facts on the gender pay gap after the arrival of children, highlighting its determinants and heterogeneous dimensions. Section (4.2) explores the wage growth patterns for workers with different fertility profiles; specifically, it shows how fertility decisions might determine a different wage profile even *prior* to the arrival of a child. Section (5) concludes.

2 Data

This section presents our data sources, key variables, and sample. It also details the construction of the control group for mothers and fathers. Data access was provided via Statistics Portugal's secure infrastructure, where all processing was conducted.

2.1 Data sources

e-Fatura. The e-Fatura system is an electronic invoicing software system adopted by the Portuguese government in January 2013 to fight VAT fraud. Decree-law 198/2012 requires electronic reporting of invoices to the tax authority and covers all individuals or legal entities with a headquarters, stable establishment, or tax domicile in Portugal. The data generated by e-Fatura cover all business-to-business transactions. For final consumers, the government provides a set of incentives for them to ask for an invoice with their taxpayer number at the time of purchase.[2] The data generated by e-Fatura cover between 70% and 75% of the net-of-VAT consumption in the national accounts. Almost

[2]These incentives are weekly public debt lotteries and deductions on income tax, up to a limit, for expenditures on health, education, nursing homes and general household spending. The government also rebates 15% of the VAT on expenditures on public transportation passes, hotels and restaurants, hairdressers, and car repair. In addition, invoices for expenditures on utilities (e.g., telecommunications, gas, electricity and

all goods and services are included in e-Fatura, with the notable exception of housing rent invoices issued by individual landlords. Durable goods such as cars, mobile phones, and computers are included, but it is not possible to separate purchases of durable and non-durable goods because I do not have access to itemized invoices that specify the nature of the goods. In our data, individual monthly expenditures are aggregated by seller taxpayer number, so we proxy the expenditure type by economic activity of the seller.

Quadros de Pessoal. Our main data source to study the evolution of workers' careers and compensation is the *Quadros de Pessoal* (QP), a comprehensive matched employer–employee dataset maintained by the Portuguese Ministry of Employment since 1982. Collected annually, it mandates that private-sector firms with at least one employee report detailed information about their personnel in October of each year. This includes worker compensation, work hours, collective agreements governing contracts, and various variables describing the firm and employment relationship. Since firms themselves provide the data, reported earnings are expected to be highly accurate. In particular, the availability of unique identifiers for workers and firms allow us to track individual workers' careers within the private sector over time. The QP worker data include demographic characteristics such as age, gender, and education level. Firms are characterized in the QP data by variables such as location and sector. The registries also provide detailed information on monthly wages, broken down into several components. Additionally, the data include number of hours worked, occupation, collective agreement details, and seniority within the firm. Our research focuses on examining how fertility decisions and childbirth impact labor market outcomes throughout individuals' careers. To assess earnings, we consider total wages, comprising both base wages and additional compensation such as extra hours. Our primary focus is on real log-hourly total wages, their growth over time, and inflation adjusted monthly labor earnings. Additionally, we examine labor market participation rates and changes in the number of hours worked per month. While the limitations of the data prevent precise identification of employment and nonemployment periods, their focus on the private sector allows us to track individual progression and career histories based on gender and by workers' fertility choice. Our sample consists of workers' histories in the private sector, starting from their initial entry in the workforce. The analysis uses data spanning 1994 to 2020 to ensure match quality between birth registries and QP. To mitigate potential selection bias related to parenthood, we follow young first-time earners (ages 18–35, the age range typically associated with fertility decision-making). We select all workers who enter the market with no more than 11 months of experience

water supply) and expenditures in retailers where the consumer possesses a loyalty card are automatically issued with the consumer taxpayer number.

after 1994. Aside from these criteria, our sample selection follows standard procedures. We exclude observations in the agricultural sector and eliminate uncategorized collective agreements. Moreover, we exclude observations from the islands of Madeira and Azores. For every worker, we keep the largest observed wage each year and remove observations that are outliers.[3] While it is common practice to exclude wages below a certain threshold, we keep zeroed wages. The primary advantage of this approach is that it enables us to identify periods during which the worker is part of the firm's personnel but does not receive a wage from the firm (e.g., during maternity leave). To ensure the robustness of our estimates, we restrict our sample to the largest connected component connected by worker mobility (Abowd *et al.*, 2002). The data limitations restrict our ability to track individuals beyond their private-sector employment, but this sample still enables us to reconstruct career trajectories: we are able to identify labor market entry points, employment gaps, and exits for a large portion of the active population.

2.2 Analysis datasets

We use deidentified data from annual information on employment and earnings (QP) between 1994 to 2020 and monthly consumption data from electronic invoices (e-Fatura) from between January 2016 to December 2019. We supplement these with data from annual personal income tax declarations (IRSs) and data on births from the population registry (BPR).

Panel of monthly expenditures linked to births. To analyze the evolution of household expenditures around childbirth, we construct a panel of monthly expenditures from electronic invoices around the month of birth of the first child. Our construction of the panel follows three steps. First, we use data from annual IRSs to identify households whose first child was born between January 2016 and December 2019. Second, we obtain information on labor market status of women from the population registry to identify households with employed vs. nonemployed women in the year of birth and in the subsequent year. Third, we match this information to monthly final consumption expenditures from electronic invoices (e-Fatura) to follow household consumption spending in the months around childbirth. Note that monthly expenditures are aggregated by the seller's taxpayer number, so we proxy expenditure by the economic activity of the seller. The final dataset is a panel that contains 149,006 households—and 6,260,607 household–month observations—from between January 2016 and December 2019. Appendix Table (5) describes the panel and provides descriptive statistics.

[3] We remove from our sample workers with more than 600 months of tenure and with missing or atypical working hours.

Panel of employment and earnings linked to births. The creation of the panel encompasses the following stages. First, we integrate the panel of linked employer–employee data (QP) with data on births from the population registry (BPR).[4] In this process, we consider individuals born in 1960–2002 and aged 18 years or older at the time of birth of their first child. Individuals born between 1960 and 1980 who were not yet 40 years old by 1994 but reached 40 by the final year of the sample (2020) and are observed without children are considered "never-parents" and are allocated to the control group (nontruncated group). On the other hand, individuals born between 1981 and 2002 are subject to right-censoring since they did not reach age 40 by the end of the observation period and therefore could potentially have had children after 2020 (truncated group). To solve this issue, we assign a parenthood probability to the cohort born between 1981 and 2002. Specifically, we estimate a linear probability model of zero lifetime fertility in the group of individuals born between 1960 and 1980.[5] Then, we sort men and women born between 1981 and 2002 based on the predicted probabilities from the zero lifetime fertility model and assign them to the control group up to the point at which the fraction of predicted never-parents equals the fraction of actual never-parents from the cohorts born between 1960 and 1980. Therefore, the control group consists of two groups of individuals (with men and women considered separately): actual never-parents from the cohorts born between 1960 and 1980 and predicted never-parents from the cohorts born between 1981 and 2002. The final sample consists of 731,937 individuals—corresponding to approximately 11 million person–year observations—comprising 371,408 women and 360,937 men, wherein 72% are actual mothers or fathers. For women, the treated group comprises 309,582 women who had children between 1960 and 2002 ("actual mothers"), while the control group comprises 44,256 never-mothers (in the nontruncated cohorts) and 17,570 predicted never-mothers (in the truncated cohorts). Appendix Tables (1) to (2) present summary statistics on women. Similarly, for men, the treated group comprises 291,608 men who had children between 1969 and 2002, while the control group comprises 48,625 never-fathers and 20,296 predicted never-fathers. Appendix Tables (3) to (4) present summary statistics on men.

[4] We use two versions of the resulting panel dataset: an unbalanced one, which we use to analyze wages and hours with gaps in employment, and another, extended panel balanced until the last worker observation, which we employ to study labor force participation and earnings.

[5] In practice, we estimate the regression

$$NeverParent_{it} = \alpha + \underline{x}_{it}\beta + \varepsilon_{it} \tag{1}$$

separately for men and women, where $NeverParent_{it}$ is a dummy that assumes value 1 for "never-mothers" or "never-fathers" born between 1960 and 1980 and $\underline{x}_{it}$ is a vector of observable characteristics that includes quartiles of the pretax income distribution of the women's (or men's) cohort, education length (4 categories), and region of the employer (which we take as a proxy for region of residency).

3 Direct cost-of-childbirth

In this section, our goal is to examine the direct impact of childbirth on household spending from the time that a pregnancy is confirmed. We leverage monthly household consumption data from electronic invoices to estimate changes in household spending relative to that in the period before pregnancy. We conduct the following event study:

$$C_{it}^{T(h)} = \sum_{k\in\{-14+,\ldots,-1,0,1,\ldots 20+\}} \beta_k \mathbf{1}(t = k) + \theta_i + \lambda_t + \varepsilon_{it}, \tag{2}$$

where $C_{it}^{T(h)}$ is the log of monthly consumption expenditure from electronic invoices for household h in expenditure category $T(h)$ (recall that expenditure categories are proxied by seller industry), β_k represents the coefficients of interest on the event-time indicators θ_i and λ_t are individual and time fixed effects, respectively. We estimate Eq. (2) using the two-way fixed effects model with reference to the prepregnancy period ($k = -9$ is the omitted month in the regression). As the comparison group in our event study, we use the "not-yet-pregnant" households such that we can avoid normalizing the estimates with respect to external comparison groups—e.g., households without children—which can introduce bias due to intrinsically divergent preferences in spending and earnings. The analysis dataset corresponds to the panel of monthly expenditures linked to births described in Section (2.2). The panel includes 6,260,607 household–month observations and 149,006 households from January 2016 to December 2019. Table (5) presents summary statistics. We estimate Eq. (2) for total household expenditure and by category of expenditure.

Baseline results. Figures (1) to (2) display the event-study coefficients for the effects on total expenditures and by expenditure category. We highlight three stages that are key in shaping the magnitude and composition of household expenditures: *pregnancy* ($k = -9$), *pregnancy confirmation* ($k = -6$), and *childbirth* ($k = 0$). Our results indicate that total household expenditures start to increase after confirmation of the pregnancy, peaking in the month of birth, where they increase 24% over their level in the prepregnancy period (Figure 1a). Defining the average effects on household expenditure, $\overline{\delta}$, as the average of δ_k for the post period $k \in \{1, ..., 12\}$, we observe that, on average, total household expenditure is 18.6% higher than its prepregnancy level. In addition, we find notable substitution effects: expenditures on recreational activities, hotels and restaurants, and transportation (Figure 2b) seem to be reallocated toward expenditures on healthcare and retail (Figure 1b). At the month of birth ($k = 0$), the peak in total expenditure is attributable to a substantial rise in healthcare expenses, which more than double from their level in the prepregnancy period. Following childbirth, utility bills experience a significant increase,

while insurance costs also rise, albeit to a smaller degree. Specifically, expenditures on electricity, gas, and air conditioning grow 15.4%, and costs associated with water distribution grow 7.9% (Figure 2a). Educational spending decreases following the confirmation of pregnancy but starts rising six months post-childbirth. This can be explained by the fact that educational expenses prior to pregnancy and up to the sixth month post-birth primarily cover the adults in the household while, from six months after the birth onward, these expenses are related to the childcare expenses of the newborn. Interestingly, Figure (2b) displays a significant rise in expenditures on hotels and restaurants twelve months after the birth, presumably related to the one-year birthday celebrations of the child.

Direct costs by mother's labor force participation. To what extent is the pattern of household spending influenced by the potential for certain expenditures to be substituted with home production? To answer this question, we estimate Eq. (2) separately for households in which mothers are nonemployed during the year of childbirth and the subsequent year and households in which mothers are employed during the year of childbirth. Figure (3) depicts the event-study coefficients, and Figure (4) illustrates the outcomes for overall spending and average effects by category of expenditure. There are no statistical differences in spending between nonemployed and employed mothers during the time from pregnancy confirmation to childbirth, except in retail trade spending, which is 7.7 p.p. greater for employed than for nonemployed mothers during the four months preceding childbirth, with no significant differences following that period. However, post-childbirth, significant differences emerge in spending patterns between households with nonemployed vs. employed mothers, in particular in expenditures related to childcare, but also in utilities. On average, six months after childbirth, households with employed mothers spend 24.6% on health and child daycare vs. 5.6% in households with nonemployed mothers. Similarly, spending on education is 25.6% greater in households with employed mothers than in those with nonemployed mothers, who devote some 7.4% to educational expenditures.[6] These findings suggest that nonemployed women are likely to substitute paid employment with domestic childcare activities, in contrast to their counterparts who remain active in the workforce, who instead rely on childcare services.

[6]Note that providers of childcare for children under three years old share the same industry code as healthcare providers (NACE code Q), such that our categorization of expenditures based on the seller's industry can lead to conflation of healthcare and childcare expenses for this age group. Conversely, providers of childcare to children older than three years are categorized under the "education" industry code (NACE code P). Nonetheless, providers providing childcare to children both younger and older than three years are typically also classified under "education" (primary industry code). Thus, the reason we see an increase in both health and childcare (NACE Q) and educational expenditures (NACE P) starting six months after birth is related to the increase in childcare expenditures six months after birth, mainly among mothers participating in the labor force.

4 Indirect cost-of-childbirth

4.1 Gender gaps in labor market outcomes after birth

Our goal is to assess how the first child's arrival impacts labor market outcomes and how this impact varies between men and women. We consider the following event-study specification:

$$y_{itk}^{G(i)} = \sum_{k \in \{-5+,\ldots,-1,0,1,\ldots 10\}} \delta_k^{G(i)} \mathbf{1}(t = k) + \alpha_i + \lambda_t + \varepsilon_{it}^{G(i)}, \tag{3}$$

where $y_{itk}^{G(i)}$ is the labor market outcome for individual i belonging to group $G(i)$ (women with and without children and men with and without children), t is the calendar year, k is the event time (i.e., years relative to the first childbirth), $\mathbf{1}(t = k)$ is an indicator that takes the value 1 in the years after the birth of the first child and 0 before, and α_i and λ_t are individual and time fixed effects, respectively. The analysis dataset corresponds to the panel of employment and earnings linked to births presented in Section (2.2). Descriptive statistics are provided in Appendix Tables (3)–(4). To account for heterogeneity in treatment timing, we estimate Eq. (3) with the estimator from Callaway and Sant'Anna (2020) with event-study parameters δ_k aggregated by relative time and with bootstrap standard errors.

Baseline results. Consistent with existing research, our findings show that women experience child penalties to their labor market outcomes following childbirth, mainly driven by changes on the extensive margin, whereas men's labor market outcomes remain largely unchanged. Figure (5) displays the impacts of the arrival of the first child on earnings, wages, hours worked, and participation separately for men and women. The earnings impacts are estimated unconditionally on employment status, while the effects on wages and hours are conditional on having a job.[7]

There is an abrupt decline in all labor market outcomes of women in both the year of childbirth ($k = 0$) and the preceding year ($k = -1$). These abrupt drops result from the fact that our employer–employee dataset (QP) records as zero the labor compensation and hours of workers on sick or maternity/paternity leave. Hence, the drops in the year of childbirth are essentially due to maternity and paternity leaves, while in the case of women, the drop in the preceding year is attributable to sick leaves related to pregnancy.[8]

[7] In practice, individuals with recorded earnings and hours in QP are regarded as employed; those exiting QP are attributed zero earnings. Note that exits from QP could imply nonparticipation but also a shift to the agriculture or public sector.

[8] In Portugal, mothers can take 120 days of maternity leave at full pay, and an optional 30 days at 80% pay, during the 30 days before and/or the period after childbirth.

Following childbirth, some mothers return to work and initially see a rebound in earnings from the sharp decline experienced in the childbirth year. However, over the first year, the earnings disparity between mothers and nonmothers expands, and by the sixth year, mothers' earnings are some 22% lower than nonmothers'. Following the sixth year, there is a gradual recovery in the earnings of mothers, and by the tenth year after childbirth, the earnings disparity shrinks to 16% (Figure 5a). The evolution of mother's earnings after childbirth is driven mostly by adjustments on the extensive margin, as total wages—conditional on employment—drop by approximately 3.5% one year after childbirth and remain flat until the sixth year after maternity, when they start gradually increasing. Ten years after childbirth, the wage gap of mothers vs. nonmothers narrows to 2.4%. Additionally, hours of work seem relatively unaffected, which suggests that the decreases in wages stem from decreases in overtime and bonuses instead of transitions to part-time work, confirming that the extensive margin is the main margin of adjustment behind the evolution of women's earnings (Figure 5c).

On the other hand, the evolution of men's labor market outcomes does not seem affected by the arrival of the first child. Apart from a statistically significant, yet temporary, increase in earnings in the first year after birth, the evolution of earnings, wages, and hours varies around zero for men. To measure the gender-specific penalty after childbirth, we set $\delta^{\text{Men}} - \delta^{\text{Women}}$, where δ^{g} represents the average of the event study coefficients δ_k for the post-periods $k \in \{1, \ldots, 10\}$ for $g = \{\text{Men, Women}\}$. Throughout the ten years after birth, women lag 17% behind men in earnings, while among women who never change jobs, the lag is some 3%. The penalties associated with the arrival of the first child are lower than those found for other countries but consistent with the work of Kleven, Landais and Leite-Mariante (2023), who document that Portugal is a one of the countries with the smallest penalties in southern Europe.

4.1.1 Heterogeneity

This section complements the previously presented findings by conducting a series of heterogeneity analyses that allow us to understand the drivers of the observed effects. Using the same framework introduced previously, we explore how the estimated child penalty varies across different samples, dividing our analysis into four parts. First, we investigate how firm characteristics, including industry (which captures the interaction of market conditions and labor regulations with firm-level dynamics) and firm size, influence the child penalty. Second, we evaluate the differential impact by the mother and child characteristics. We examine first whether the gender of the child plays a role and then assess whether the penalty varies by mother birth cohort. Third, we quantify the variation in the child penalty along the earnings distribution. Finally, we explore how the availability of

nonmarket childcare can impact the magnitude of the child penalty experienced by mothers. This extended analysis allows us to gain a detailed understanding of the child penalty and identify its potential drivers.

Role of industry and firm size. We expect some variation in the child penalty across industries because of differences in occupational composition and institutional arrangements. Industries with a high concentration of women-dominated occupations or those with more flexible working arrangements tend to exhibit lower child penalties for mothers. We define industry as the sector of activity of the worker's last employer before childbirth. Figure (6b) presents the results across industries. Across all industries, we observe a difference between genders. Men's earnings loss after childbirth is consistently smaller than women's, with the former being on average very close to zero. Furthermore, the figure suggests heterogeneity within genders. While men's penalty remains negligible across all industries, women's penalty varies considerably in size. In particular, it is higher in industries characterized by less flexible work arrangements, such as hotels and restaurants, manufacturing, and retail trade. Finally, the figure suggests that while the earnings loss is present for all women across all industries, the child penalty is generally stronger in the hotel and restaurants sector (approximately 30%), an industry with lower wages and fewer flexible work options.

Regarding firm size, we find that it differently shapes the child penalty. Figure (7a) presents the estimated coefficients from Eq. (3) for women working in small ($n \leq 50$), medium ($50 \leq n < 250$), and large firms ($n \geq 250$) in the last observed year before childbirth, where each line plots the estimated coefficient, indicating the earnings loss by type of firm. Our results reveal a nonlinear relationship between firm size and the child penalty. While the earnings loss for women is statistically similar across all firm sizes in the first five years after childbirth, right afterward it begins to diverge. Women working in small and large firms present higher earnings drops than those working in medium-sized ones. However, when we analyze the child penalty itself, a different picture emerges. Women in larger firms experience a smaller relative earnings loss than men after the initial five years (approximately 11.7% smaller). Standardized firm-level pay policies in large firms might explain the mitigation of the gender gap in earnings loss over the longer term. Considering the child penalty rather than solely focusing on the raw earnings drop for women is important since the two might not go hand in hand.

Role of mothers' cohort and child characteristics. In this section, we analyze heterogeneity in the child penalty based on two key dimensions: mother's cohort and child characteristics. First, given the shifts in the labor market over recent decades, including

variations in women's participation in the workforce and educational attainment, it is reasonable to anticipate that the child penalty might differ among cohorts. Such variation may arise from changes in societal norms regarding women's roles, changes in fertility patterns, or shifts in mothers' education levels. Figure (8a) presents the results, revealing that newer generations of mothers experience lower average earnings loss after childbirth than older generations. The differentiated smaller effects in new cohorts could be due to different conditions: the increase in female education along with a change in societal roles among the latest generations, or relative changes in the cost of childcare.

We also investigate whether child gender influences mothers' earning loss, under the assumption that children of different genders might require varying levels of care, potentially impacting the mother's labor market participation and earnings. Figure (7b) shows that, despite a larger earnings reduction observed for the mothers of male newborns, there is no statistically significant difference in the child penalty between mothers of sons and mothers of daughters.

Differences along the earnings distribution. A simple labor market model for mothers' labor supply decision considers a weighting of the costs and benefits of resuming work after childbirth. This decision-making process involves comparing the wage earned against the combined cost of childcare and the potential loss of income from not working (outside option of employment). In countries with lower wages such as Portugal, this cost–benefit analysis becomes particularly crucial. Therefore, it is important to examine how the child penalty varies across the labor earnings distribution. Figure (6a) presents the distribution of the child penalty by earnings before the birth of the child. Each woman is classified into a specific earnings ventile based on her last observed wage before childbirth. The results show the following: i) Women in the bottom and top quartiles of the distribution experience the largest child penalty, and the effect on these groups drives the overall results. ii) Women at the bottom of the distribution are the worst off since they have the largest earnings drop. In the top quartile, the earnings loss is attenuated, but the child penalty is equally large because men in the top quartile actually experience an increase in their earnings after the arrival of a child. iii) In the middle of the distribution, the situation is more balanced, with women's earnings loss comparable to men's. This could be because of the limited flexibility in earnings adjustments near the minimum wage, where both genders face similar constraints in negotiating higher wages to offset childcare costs. Overall, these results evidence different behaviors that lead to different outcomes along the earnings distribution, suggesting how labor market participation can play an important role in shaping the results.

Intergenerational safety net. Previous findings highlight a clear link between labor market participation and the child penalty in Portugal. When we tease apart the child penalty on labor market outcomes, our analysis suggests that it is not primarily changes in hourly wages or work intensity that drive the adjustments in earnings after childbirth. Instead, they appear to be largely influenced by work interruptions and decreased female labor force participation. Therefore, is important to understand what options are available to mothers facing the economic dilemma of reentering the workforce after childbirth. While the interplay of various factors is undeniable, simplifying the initial choice to a purely economic decision offers a starting point for further analysis. If mothers have access to nonmarket childcare arrangements that allow them to return to work, does the penalty disappear? To explore this potential mechanism, we compare the earnings outcomes of mothers who can recur to nonmarket childcare with those of mothers who cannot. Using our family relationships dataset from the population registry (BPR), along with information on social security status, we proxy women's access to nonmarket childcare by whether the child has a retired grandparent. This choice is informed by the prevalence of intergenerational transfers within families, which often extend beyond financial support to include in-kind contributions such as childcare.

Previous literature highlights that childcare access plays a key role in explaining both female labor market participation (Givord and Marbot, 2015; Havnes and Mogstad, 2011; Bick, 2016) and household fertility decisions (Bauernschuster and Schlotter, 2015; Eibich and Siedler, 2020; Müller and Wrohlich, 2020). Childcare provision can occur through formal arrangements (e.g., facilities, subsidies) or informal arrangements within the family. This section investigates the informal childcare provision by retired grandparents and its potential role in mitigating the estimated child penalty. The potential effectiveness of such an intergenerational support system is supported both theoretically and empirically (Cardia and Ng, 2003; Arpino *et al.*, 2014; Aparicio Fenoll, 2020). To understand how grandparents shape mothers' labor force participation and earnings, we should consider that a grandparent's being in retirement potentially diminishes the extended family's pooled income, thus increasing the likelihood of a mother's reentering the labor market. Moreover, being retired increases the amount of free time that could be used for grandchildren's care, which might further increase female labor force participation. While these channels seem plausible, they might not be the only mechanism driving the impact of the presence of retired grandparents on women's labor force participation. Indeed, depending on the age and autonomy of the grandparent, mothers might *decrease* their labor participation to devote time to elder care (Carney, 2023).

To investigate the potential influence of grandparents on the child penalty, we run two separate regressions: one examines the effect in a sample of mothers with a retired

parent or parent-in-law (the child's grandparent), while the other evaluates the relationship in the sample of nonretired parents or parents-in-law. While previous literature tests the proposed channels on the basis of the presence of *living* grandparents (Posadas and Vidal-Fernandez, 2013), we focus on retirement status, which we view as an indicator of potential availability for caregiving. Figure (8b) presents the results of the estimation. A striking finding emerges that contrasts with our initial expectations: the child penalty does not disappear. However, its magnitude decreases starting from the second year onward, with the gap gradually closing over a ten-year period. Although the availability of nonmarket care does not completely eliminate the earnings gap, our findings highlight a long-term reduction of approximately 30%. This substantial effect underscores the importance of nonmarket childcare arrangements (both publicly and privately provided) in influencing female labor market participation and earnings. In our setting, the presence of retired grandparents seems to mitigate the child penalty, serving as a form of insurance for women against the burdens associated with childbirth.

4.2 Fertility and career profiles

The child penalty has been recognized as a determinant of the gender pay gap. Its contribution ranges from 20% to 60% (Kleven *et al.*, 2019b; Casarico and Lattanzio, 2023). While the common approach to calculating this penalty is to examine pay differences across parents and nonparents around the time of childbirth, it is important to recognize that fertility choices and expectations may also influence the career trajectories of both of these groups, contributing to the gender differences. This section explores the economic repercussions of fertility in shaping career profiles and their contribution to the gender pay gap even prior to the birth of a child. Our analysis focuses on comparing worker histories and evaluates how gender and fertility impact the returns to labor market experience, determining different career paths.

While a common assumption implicit in the conceptualization of the child penalty is that new parents' behavior changes as a result of their assuming childcare responsibilities, our analysis suggest additional components of the penalty that may be affected even prior to childbirth, impacting human capital accumulation and the returns to labor market experience. We consider two main channels through which these effects can operate: First, we consider changes along the career path. After childbirth, mothers may experience more frequent labor market interruptions and reductions in work schedules, potentially leading to slower acquisition of work experience and job-related skills and thus impacting compensation and job growth. There could also be changes prior to labor market entry. Workers who anticipate a potential impact of childbirth on their work–life balance may adjust their career decisions even before entering the workforce, leading to

different paths in education and human capital investment. Recent literature shows that such expectations could manifest in a prioritization of flexible work arrangements and location preferences (Petrongolo and Ronchi, 2020; Le Barbanchon *et al.*, 2021), such that earnings are differentially affected along both gender and fertility dimensions.

To study career profiles, we follow Manning and Swaffield (2008) and Del Bono and Vuri (2011). In this approach, expected wage growth depends on experience, employment gaps, and worker mobility. We also incorporate elements of the dual wage ladder specification introduced by Di Addario *et al.* (2023), accounting for the effects of job-to-job mobility. Specifically, our wage growth model has the following form:

$$\begin{aligned}\Delta w_{it} = & \underbrace{g_{it}}_{\text{Gap in employment between observations}} + \underbrace{\beta\phi(e_{it}) + \gamma[\phi(e_{it})]^2}_{\text{Adjusted experience}} + \\ & \underbrace{\mu \times \mathbb{1}_{\text{External}}}_{\text{Firm change}} + \underbrace{\psi_{J(i,t)}}_{\text{Firm pay premium at origin}} + \underbrace{\lambda_{H(i,t+g)}}_{\text{Firm pay premium at destination}} \\ & + \underbrace{\eta_{K(i,t)}}_{\text{Job-title pay premium}} + \underbrace{\alpha_i + \tau_t}_{\text{Worker and year}} + \varepsilon_{it},\end{aligned} \tag{4}$$

where Δw_{ijt} is the wage growth of an individual i between time t and the next observation $t+1$, calculated forward to maximize the number of observations in the data. Employment growth is modeled as a linear function of the adjusted experience, composed of employment gaps g_{it} and the potential experience $\phi(e_{it})$ and its square.[9] To control for mobility, we include an external dummy indicator variable that takes value 1 when the individual changes employer in the next period, capturing the potential wage benefit associated with changing jobs. Furthermore, we include a set of origin and destination firm fixed effects, as in Di Addario *et al.* (2023), that capture the specific unobserved pay policy of the firm that receives the worker and the firm from which she departs. We further control for constant unobserved heterogeneity at the level of the worker, year and job title. Finally, the term ε_{it} represents the unexplained error term that accounts for any remaining variation. We first run the model using the entire sample of individuals. Then, we focus on the two groups of interest (gender and fertility choice). This approach allows us to characterize five distinct career profiles.

While the coefficients of the regression provide valuable insights, the conditional estimates of wage growth are more informative. The conditional wage growth accounts for job interruptions, the duration of such interruptions, and firm changes—elements that we

[9]Employment interruptions affect the accumulation of human capital and learning. If the incidence of the gaps varies by gender or fertility choice, the returns to experience might vary between groups. We follow Manning and Swaffield (2008) to construct a gap-adjusted measure of wage growth. Wage growth is a function of the adjusted experience $\phi(\tilde{e}_{ij}) = \sum_{j=0}^{g-1} \phi(e_{ij} + j)$. In Eq. (4), adjusted experience is nonlinear.

would expect to vary across genders, fertility and the arrival of the first child. Figure (9a) presents the conditional estimates for parents and nonparents. We first observe that the first ten years of the career trajectory exhibit the most substantial growth, after which wage growth stabilizes at a similar level for both fertility groups. We also plot the average time of arrival of the first child, which suggests that the wage growth loss associated with delaying childbirth decreases with each additional year of delay. While the child penalty is often interpreted as the magnitude of the jump between the two curves, this interpretation can be misleading, as it accounts only for the transition between states. Instead, the true impact is reflected in the cumulative difference between the curves from the time of labor market entry.

When we analyze fertility and gender together, we observe differences in workers' wage profiles along both dimensions. Figure (9b) presents the conditional estimates for both groups. We observe no difference between genders in wage growth in the first years of the career—the difference comes mainly from fertility. A closer look within genders and fertility choice shows that nonparents have larger returns to their labor market experience. Interestingly, for women, the wage loss is reversed after the 8th year. In contrast, the wage gap within men is minimal by the end of the career. Across all groups, the highest growth occurs at the beginning of the career, in line with the lower penalty observed for women who delay having their first child. These findings underscore the significant losses experienced before the arrival of the first child, emphasizing the substantial impact of fertility decisions on wage trajectories and overall career outcomes.

5 Conclusion

This paper combines administrative data on employment and earnings and monthly consumption expenditure data from electronic invoices with information on household structure and births to present evidence on the direct and indirect costs of having children. Our analysis reveals that total household expenditures increase by 24% in the month of birth and twenty months after birth rise on average by 18.6% with respect to their level in the prepregnancy period. Significant substitution effects are documented: from the confirmation of pregnancy until six months postpartum, expenditure shifts from the hotel, restaurant, recreational activity, and transportation categories toward healthcare and retail trade. Subsequently, childcare expenditure becomes the primary driver of increased total spending, especially among households in which mothers stay employed in the birth year.

Consistent with the literature, we find significant gender gaps. Following the birth of the first child, mothers' earnings drop on average 17.9% in comparison to those of nonmothers, while fathers experience almost no earnings gap vis-à-vis nonfathers. Hence,

women tend to bear the indirect cost of childbirth, experiencing a gap in earnings of approximately 17.2% relative to men's. Note that this effect is lessened for mothers who have access to potential informal childcare sources: there is a 30% decrease in the child penalty for women with retired parents (i.e., the child's grandparents).

Our findings suggest that the child penalties on women's labor market outcomes are likely proportional to the direct costs of childcare. Nevertheless, the question remains as to which sources of income bridge the gap between income trajectories and expenditures, with possible explanations including reliance on savings, social security income, or transfers between households.

Finally, we look at the conditional wage growth along the career path for groups of parents versus nonparents and, among these groups, between men and women. These findings indicate that nonparents enjoy significantly greater returns to their labor market experience than parents during the initial years of their careers and prior to the average year in the career path around which the first child is born. These results challenge the standard approach in the child penalty literature of assuming comparable career trajectories between parents vs. nonparents, pointing to possible overstatement of the child penalty estimates obtained through the standard approach.

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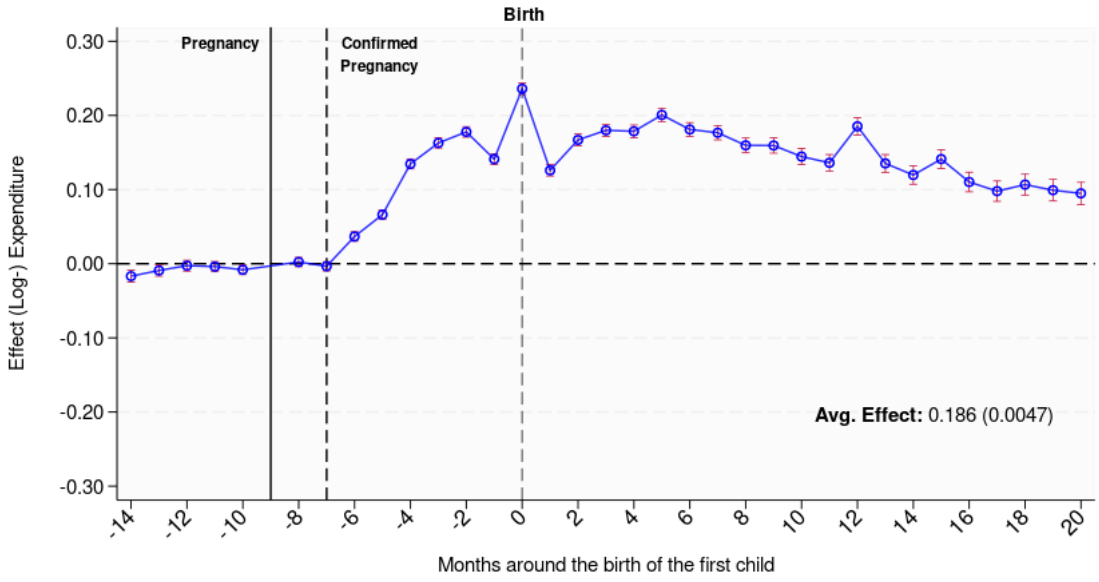
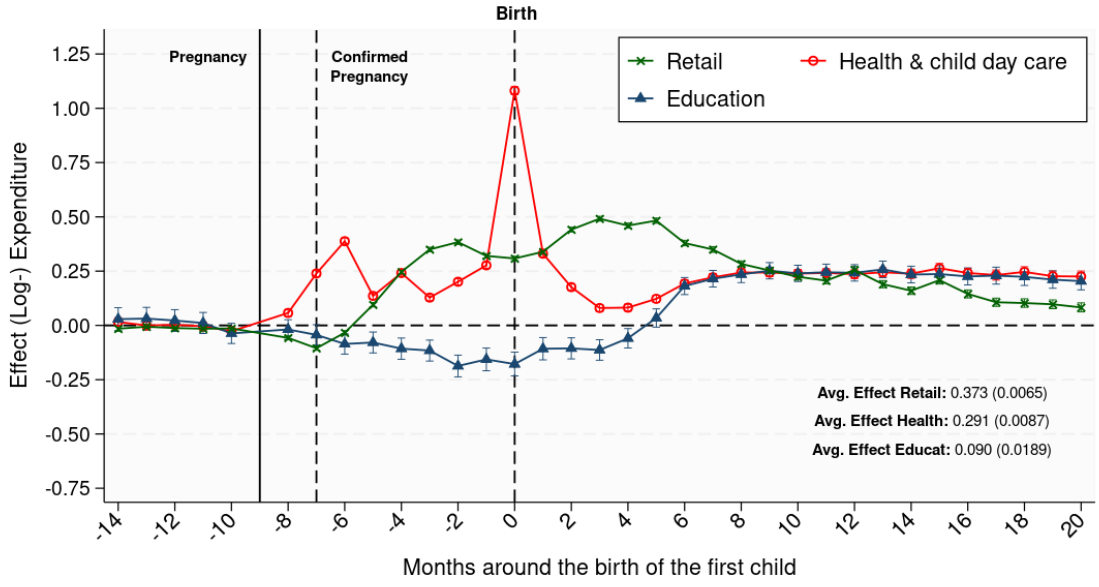
Figure 1: Effects of pregnancy and childbirth on household expenditures

(a) Total expenditure



(b) Expenditures in health and child day-care, education and retail

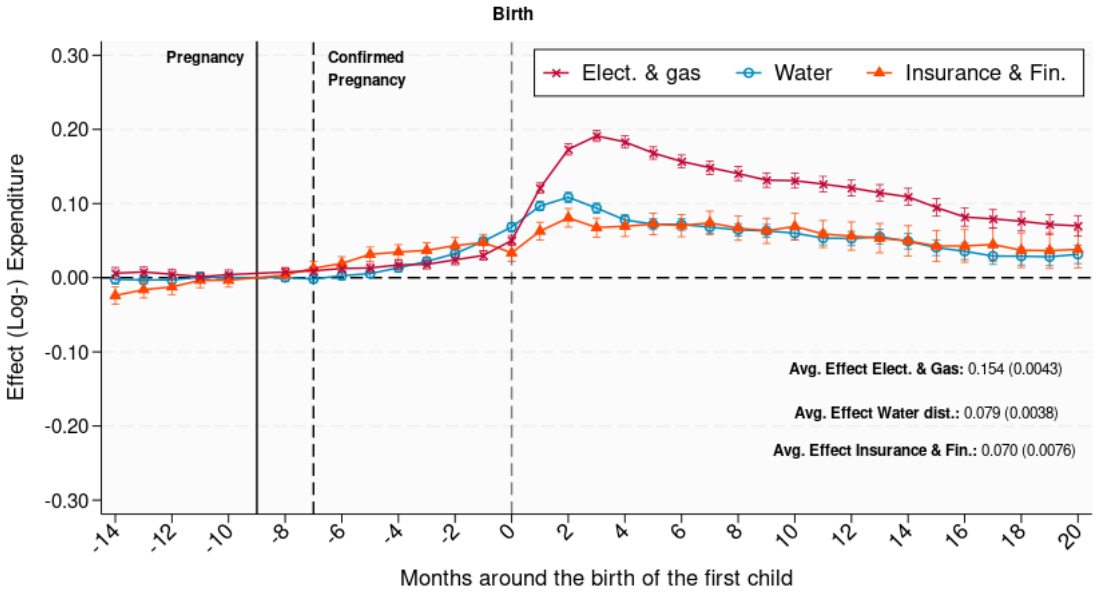


Notes: These figures present the effects on monthly household expenditures reported in electronic invoices since pregnancy. The sample is the panel of expenditures linked to births presented in Section (2.2). Table (5) provides summary statistics. The estimates were obtained through equation Eq. (2) using a two-way fixed-effects model. Panel (a) displays the effects on total household expenditure reported in electronic invoices. Panel (b) displays the effects by expenditure categories proxied by the industry NACE code of the seller. Expenditures in retail trade correspond to sellers with the NACE code G; expenditures in health and child day care to sellers with NACE code Q; expenditures in education to sellers with NACE code P.

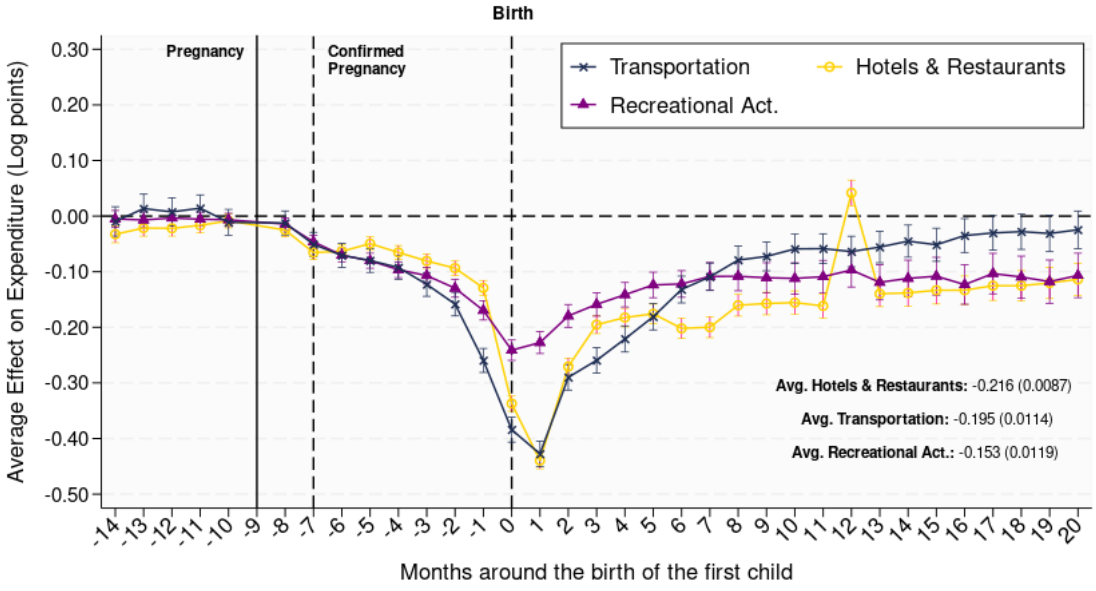
Sources: Calculations based on the administrative dataset of e-Fatura and IRS.

Figure 2: Effects of pregnancy and childbirth on household expenditures

(a) Expenditures in electricity, gas, air conditioning, water and insurance



(b) Expenditures in transportation, hotels and restaurants and recreational activities

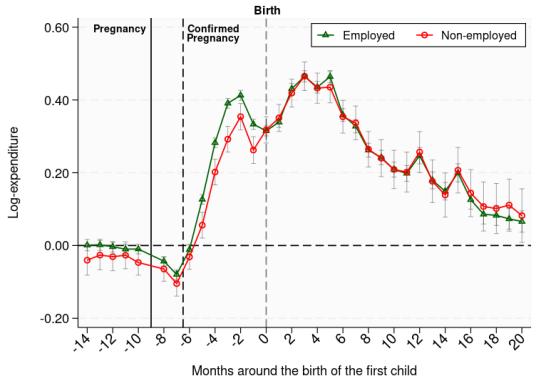


Notes: These figures present the effects on monthly household expenditures reported in electronic invoices since pregnancy. The sample is the panel of expenditures linked to births presented in Section (2.2). Table (5) provides summary statistics. The estimates were obtained through equation Eq. (2) using two-way fixed-effects. Panel (a) displays the effects of pregnancy on monthly expenditures in utilities, namely electricity, gas and air conditioning (sellers with NACE code D), water distribution (NACE code E); and insurance and finance (NACE code K). Panel (b) displays the effects of pregnancy on monthly expenditures in transportation (sellers with NACE code H), hotels and restaurants (NACE code I), and recreational activities (NACE code R).

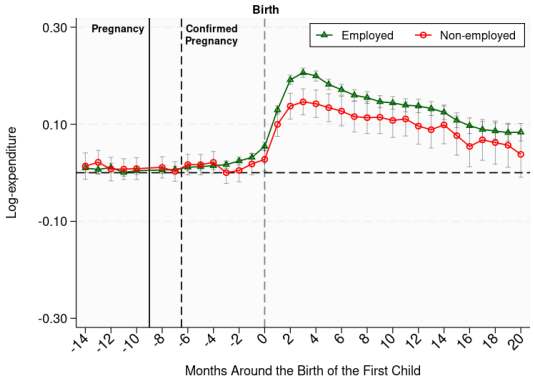
Sources: Calculations based on the administrative datasets of e-Fatura and IRS.

Figure 3: Effects of pregnancy and childbirth on household expenditures by women's labor force participation: employed women vs. non-employed women

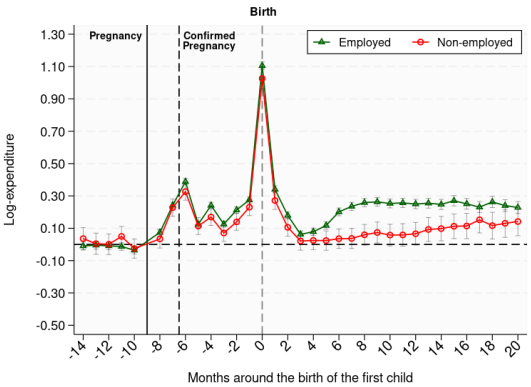
(a) Expenditures in wholesale & retail trade; car repair



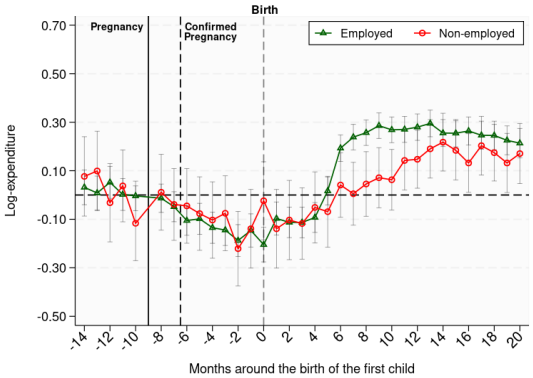
(b) Electricity, gas & air conditioning



(c) Expenditures in health & child day care



(d) Education

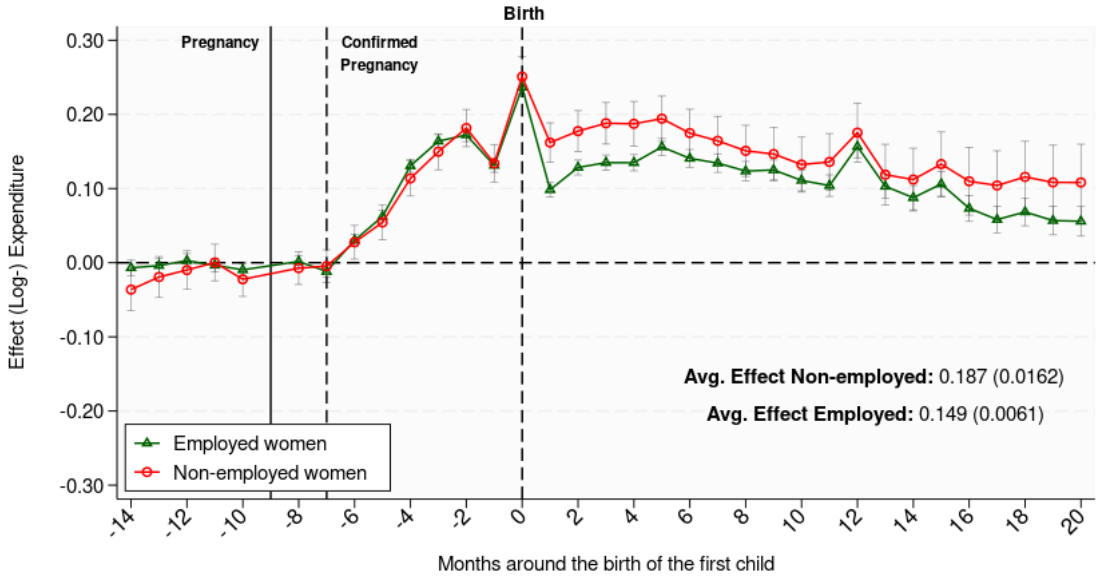


NOTES: These figures present the effects on monthly household expenditures of pregnancy and childbirth by women's labor market participation. Specifically, we estimate Equation (2) separately for households in which mothers are not employed in the year of childbirth and the year after (non-employed women) vs. households in which women are observed in employment in the year of childbirth and in the year after childbirth (employed women). Expenditures in wholesale, retail trade and car repair correspond to expenditures in sellers with NACE code G; expenditures in electricity, gas and air conditioning correspond to expenditures in sellers with NACE code D; expenditures with private health and child day care correspond to expenditures in sellers with NACE code Q; and expenditures with private education correspond to expenditures in sellers with NACE code P. It is worth noting that expenditures with child care of children who are less than three years old are registered in "health and child day care", however expenditures in establishments who also offer preschool might be registered in "education".

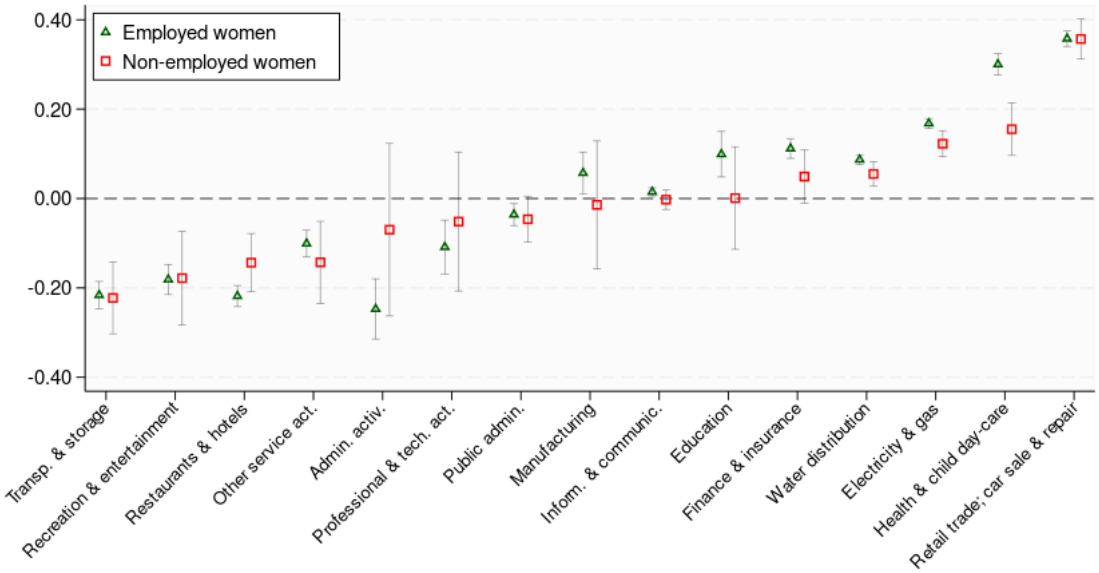
SOURCES: Calculations based on the administrative datasets of e-Fatura and IRS, 2017-2019.

Figure 4: Effects of pregnancy and childbirth on household expenditures by women's labor force participation: employed women vs. non-employed women

(a) Effects on total household expenditure



(b) Average effects by category of expenditure

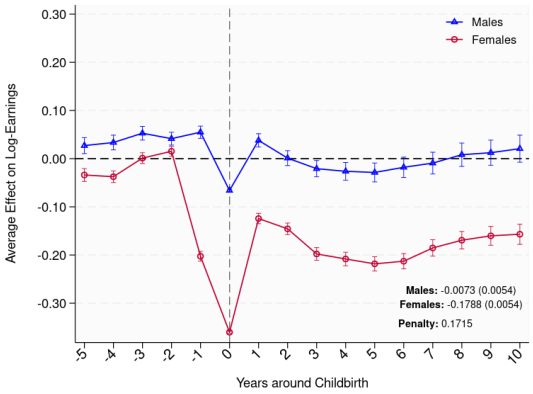


Notes: These figures present the effects on monthly household expenditures of pregnancy and childbirth by women's labor market participation. Specifically, we estimate Equation (2) separately for households in which mothers are not employed in the year of childbirth and the year after (non-employed women) vs. households in which women are observed in employment in the year of childbirth and in the year after childbirth (employed women). Panel (a) displays the effects on total household expenditure since pregnancy. Panel (b) displays the average effect on each type of expenditure by women's labor market status. The average effect on household expenditures, $\overline{\delta}$, is defined as the the average of δ_k for the post period $k \in \{1, ..., 12\}$.

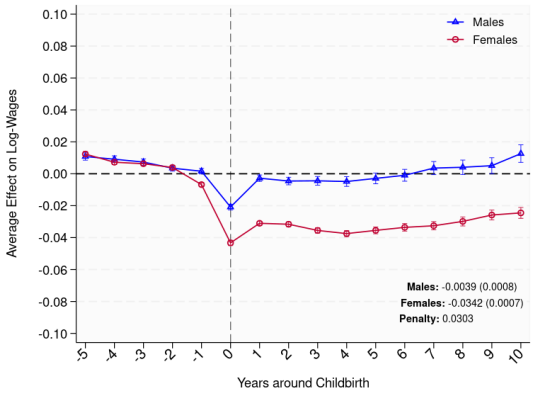
Sources: Calculations based on the administrative datasets of e-Fatura and IRS.

Figure 5: Effects of childbirth by gender in key labor market outcomes

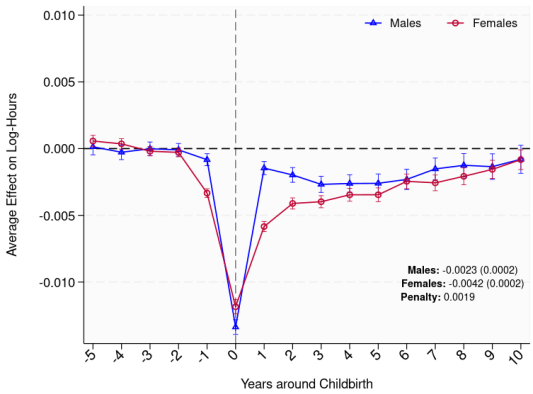
(a) Earnings (unconditional)



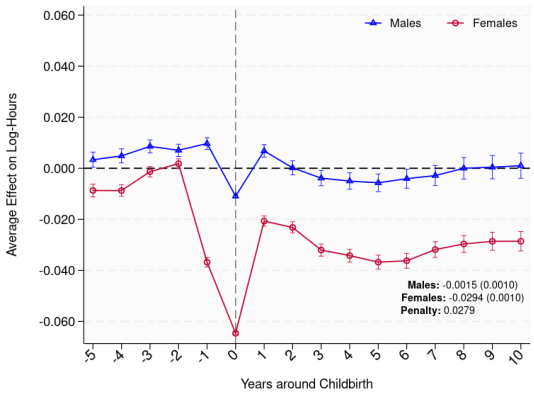
(b) Wages (conditional)



(c) Hours (conditional)



(d) Participation (unconditional)

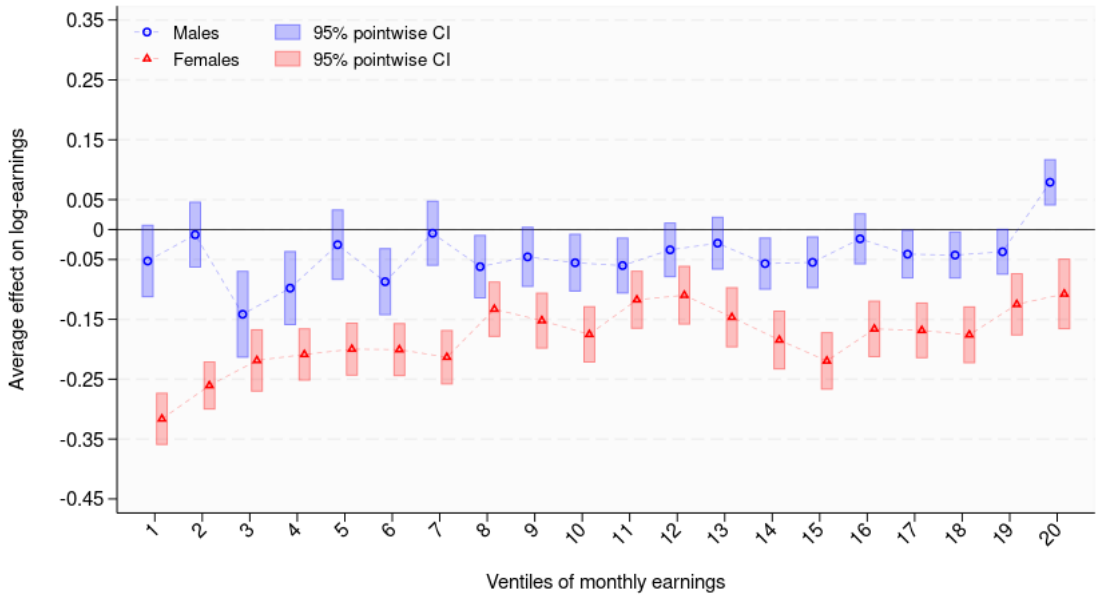


Notes: These figures present event study estimates on key labor market outcomes around the year of the first child's arrival, separately for women and men. The vertical lines depict 95% pointwise confidence intervals based on bootstrap standard errors. The post-event estimate in labor market outcomes depicted in each panel ("penalty") is defined as $\delta^{\text{Males}} - \delta^{\text{Females}}$—where δ^{g}, with $g \in \{\text{Males, Females}\}$—represents the average of the event-study coefficients δ_k for the post-periods $k \in \{1, \ldots, 10\}$, i.e., the difference in average event-study coefficients throughout the 10 years after the birth of the first child between males and females. The sample comprises the panel of employment and earnings linked to births between 1994 and 2020, as described in Section (2.2). Tables (1)-(4) provide descriptive statistics. The coefficients were obtained using the estimator by Callaway and Sant'Anna (2020), aggregated by relative time. Standard errors were computed using a multiplier bootstrap procedure, as described in Section (4). The effects on earnings (Panel 5a) and participation (Panel 5d) are estimated unconditional on employment status, while effects on wages (Panel 5b) and hours of work (Panel 5c) are conditional on having a job. As outlined in Section (2), QP registers labor market earnings as of October each year, with earnings of employees on sick or maternity/paternity leave recorded as zero. Consequently, we observe a decrease in earnings preceding childbirth among women (event time $k = -1$), attributable to those who give birth in the early months of the childbirth year ($k = 0$) but are already on leave from their job in October of the preceding year ($k = -1$).

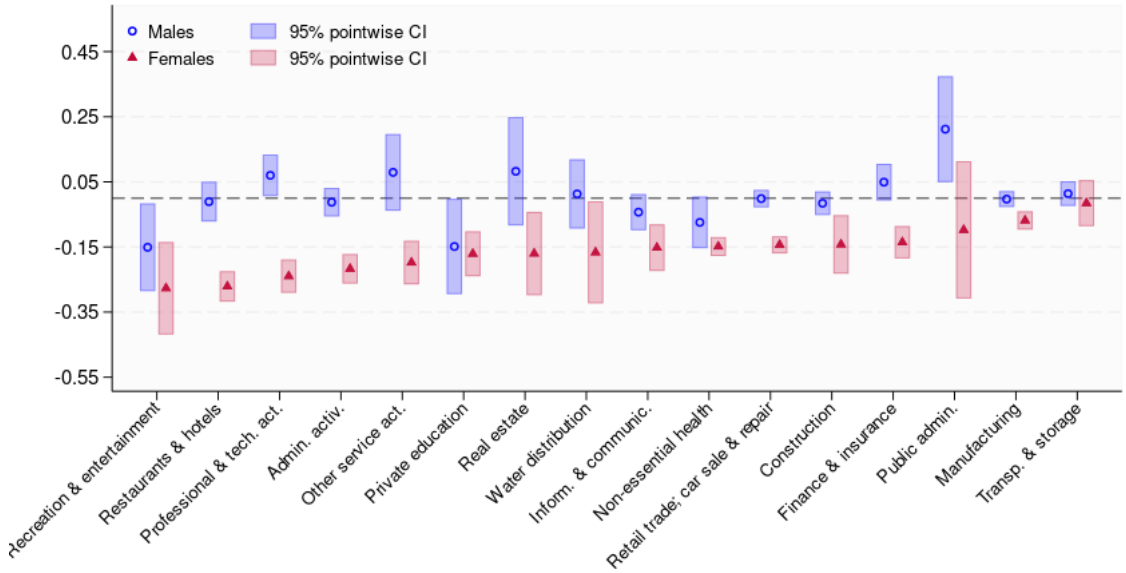
Sources: Calculations based on administrative dataset of QP, 1994-2020.

Figure 6: Heterogeneity dimensions of the arrival of the first child: average effects across the labor earnings distribution and employer's industry prior to childbirth

(a) Across the labor earnings distribution prior to childbirth



(b) Across the industry of the employer prior to childbirth

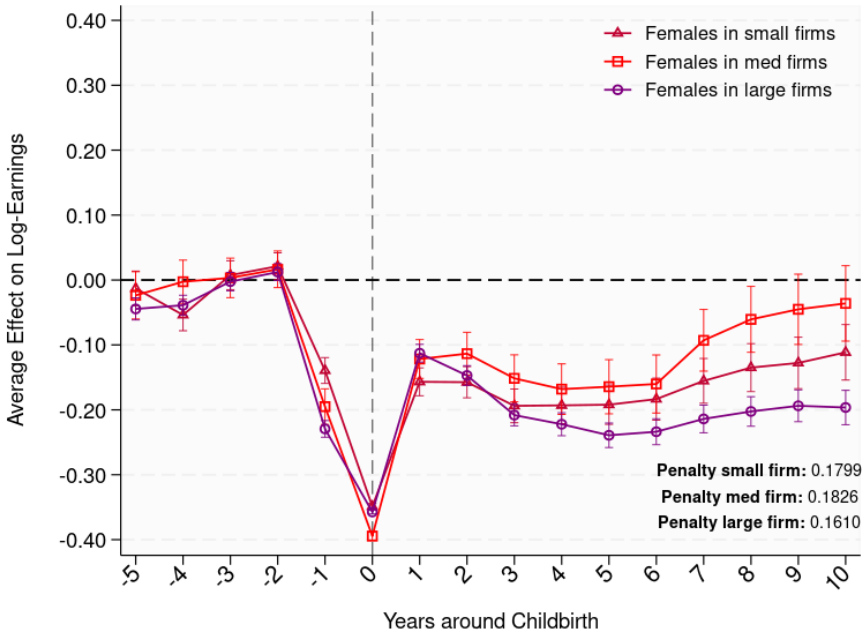


Notes: These figures present the average impacts on labor earnings of women and men after the arrival of the first child by employer's industry and across the earnings distribution. Panel (a) illustrates the average impacts on labor earnings by ventile of the wage distribution observed prior to childbirth. Panel (b) similarly displays the average impacts on labor earnings based on the industry of the employer observed prior to childbirth.

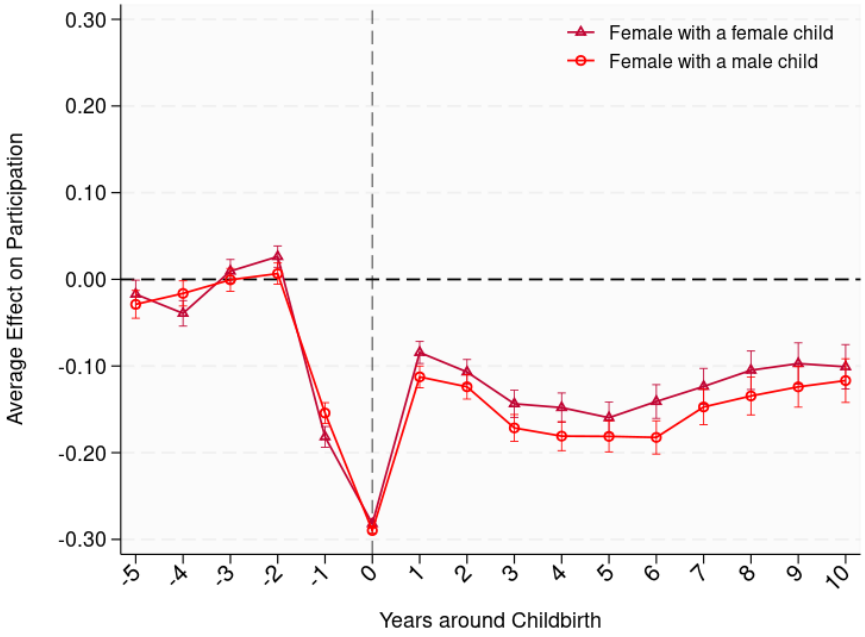
Sources: Calculations based on administrative dataset of QP, 1994-2020.

Figure 7: Heterogeneity dimensions of the arrival of the first child: effects on women's labor earnings by firm size and gender of the child

(a) Firm size of the employer prior to childbirth



(b) Gender of the child

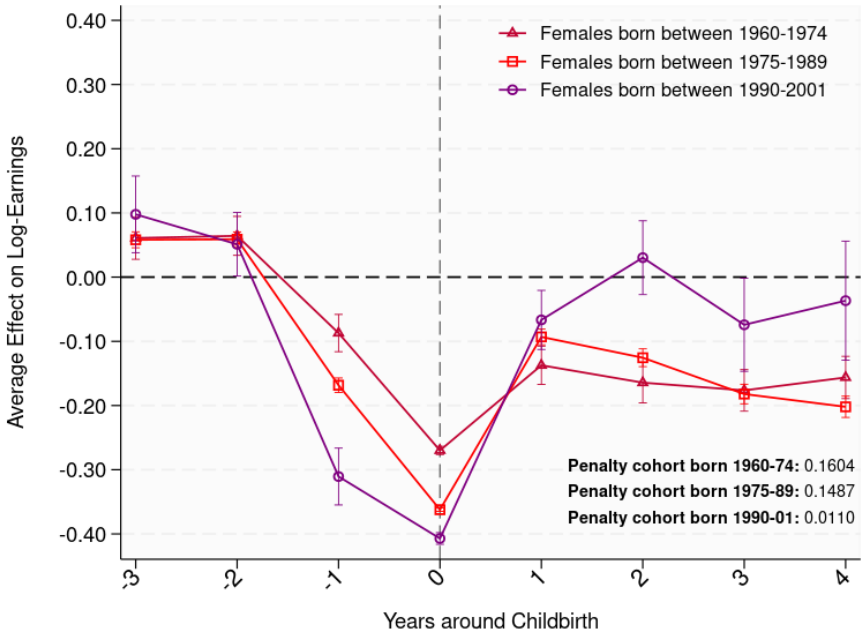


NOTES: These figures present the impacts on women's labor earnings of the arrival of the first child by firm size of the employer and gender of the child. Panel (a) displays the the earnings impacts by firm size of the employer prior to childbirth. We define small firms as those with less than 50 workers, med firms as those with 50 or more workers but less than 250 workers and large firms those with more than 250 workers. We present the gender-specific penalties at the bottom of the figure defined as the difference in average effects by males and females, $\delta^{\text{Males}} - \delta^{\text{Females}}$. Panel (b) displays the impacts on earnings by gender of the first child.

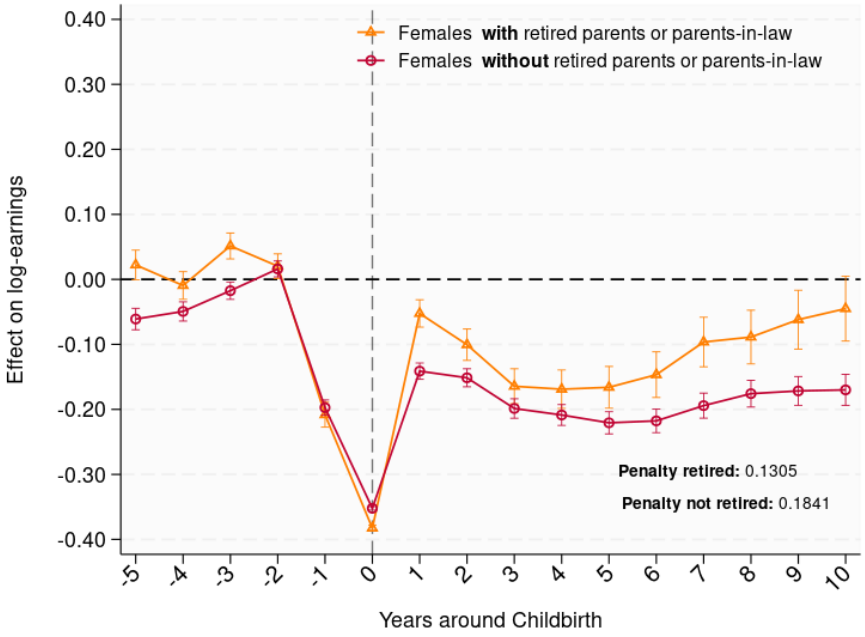
SOURCES: Calculations based on the administrative dataset of QP, 1994-2020.

Figure 8: Heterogeneity dimensions of the arrival of the first child: effects by birth cohort of the mother and existence of informal child-care institutions

(a) Birth cohort of the mother



(b) Informal child-care institutions: retired vs. non-retired grandparents

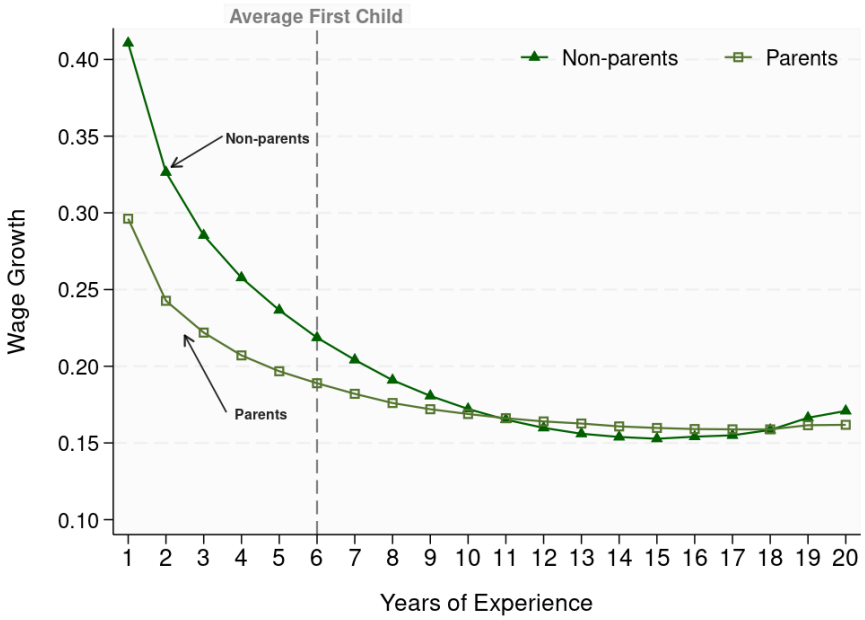


NOTES: These figures present the impacts on women's labor earnings of the arrival of the first child by women's birth cohort and by labor market status of the child's grandparents. Panel (b) displays the impacts by labor market status of the child's grandparents. To compute the impacts by retired vs. non-retired grandparent we consider: (i) women whose first child has at least one grandparent that is retired in year of childbirth or before; (ii) women whose first child has all grandparents working or seeking a job in the years prior to childbirth, and in the year of childbirth. As previously, the penalties are defined as $\delta^{\text{Males}} - \delta^{\text{Females}}$.

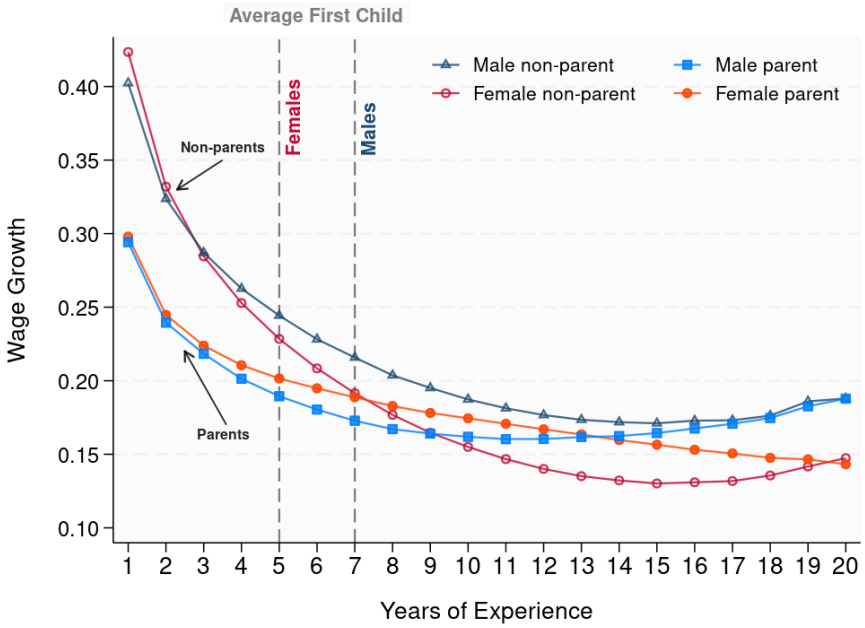
SOURCES: Calculations based on the administrative dataset of QP, 1994-2020.

Figure 9: Conditional wage growth profiles across gender and fertility choice

(a) Parents vs. non-parents: all individuals



(b) Parents vs. non-parents: gender-specific profiles



Notes: These figures present the conditional wage growth profiles computed using the coefficients estimated in Equation (4) presented in Section (4.2). Panel (a) displays the conditional wage growth profiles for parents vs. non-parents. Panel (b) displays the conditional wage growth profiles estimated for fourgroups: women with children, women without children, men with children, men without children. The vertical lines indicate the average years of labor market experience at the arrival of the first child.

Sources: Calculations based on the administrative dataset of QP, 1994-2020.

Appendix

Table 1: Summary statistics for the panel of employment and earnings linked to births: women with children, 1994-2020

	(1)		(2)		(3)	
	All - treated group		Before childbirth		After childbirth	
	Mean	(sd)	Mean	(sd)	Mean	(sd)
Birth Cohort						
1960-1974	0.25	0.43	0.14	0.35	0.25	0.44
1975-1989	0.72	0.45	0.81	0.39	0.71	0.45
1990-2002	0.04	0.18	0.05	0.21	0.03	0.18
Education						
Primary	0.24	0.43	0.18	0.39	0.28	0.45
Secondary	0.43	0.50	0.45	0.50	0.44	0.50
Tertiary	0.33	0.47	0.37	0.48	0.28	0.45
Monthly earnings	1,094	1,143	978	817	1,120	1,318
Hourly earnings	7	7	6	5	7	8
Weekly hours	113	80	129	73	110	81
Employer by industry prior to birth						
Agriculture	0.00	0.03	0.00	0.03	0.00	0.03
Mining & quarrying	0.00	0.03	0.00	0.03	0.00	0.03
Manufacturing	0.17	0.38	0.18	0.38	0.19	0.39
Electricity & gas	0.00	0.03	0.00	0.03	0.00	0.03
Water distribution	0.01	0.07	0.01	0.07	0.01	0.07
Construction	0.02	0.14	0.02	0.14	0.02	0.14
Retail trade; car sale & repair	0.21	0.41	0.23	0.42	0.21	0.41
Transp. & storage	0.03	0.17	0.03	0.18	0.03	0.17
Restaurants & hotels	0.08	0.27	0.06	0.24	0.08	0.28
Inform. & communic.	0.03	0.17	0.03	0.17	0.02	0.15
Finance & insurance	0.04	0.20	0.05	0.21	0.04	0.19
Real estate	0.01	0.10	0.01	0.10	0.01	0.10
Professional & tech. act.	0.06	0.23	0.06	0.24	0.05	0.22
Admin. activ.	0.08	0.28	0.08	0.26	0.08	0.27
Public admin.	0.00	0.06	0.00	0.06	0.00	0.06
Private education	0.04	0.18	0.03	0.18	0.03	0.18
Non-essential health	0.17	0.38	0.16	0.37	0.18	0.38
Recreation & entertainment	0.01	0.09	0.01	0.09	0.01	0.09
Other service act.	0.04	0.19	0.04	0.19	0.04	0.19
Observations						
Number of workers		371,408		309,582		
Person-year obs.		5,728,235		4,775,221		

NOTES: This table presents summary statistics on treatment group for women. In particular, it displays actual mothers from the panel of employment and earnings, and includes all women born between 1960-2002 that are observed with at least one child in period spanning from 1994 to 2020, as described in Section (2.2).

SOURCES: Calculations based on administrative datasets of QP and family relationships, 1994-2020.

Table 2: Summary statistics for the panel of employment and earnings linked to births: women without children, 1994-2020

	(1)		(2)		(3)	
	All - control group		Actual never mothers		Predic. never mothers	
	Mean	(sd)	Mean	(sd)	Mean	(sd)
Birth Cohort						
1960-1974	0.39	0.49	0.46	0.50	0.00	0.00
1975-1989	0.58	0.49	0.54	0.50	0.79	0.41
1990-2002	0.03	0.17	0.00	0.00	0.21	0.41
Education						
Primary	0.19	0.39	0.22	0.41	0.00	0.05
Secondary	0.39	0.49	0.39	0.49	0.38	0.48
Tertiary	0.42	0.49	0.39	0.49	0.62	0.48
Monthly earnings	1,206	991	1,197	1,033	1,253	731
Hourly earnings	7	6	7	6	7	4
Weekly hours	114	79	112	80	128	72
Employer by industry prior to birth						
Agriculture	0.00	0.03	0.00	0.03	0.00	0.01
Mining & quarrying	0.00	0.02	0.00	0.02	0.00	0.02
Manufacturing	0.12	0.32	0.13	0.33	0.07	0.25
Electricity & gas	0.00	0.03	0.00	0.03	0.00	0.05
Water distribution	0.01	0.08	0.01	0.08	0.01	0.08
Construction	0.02	0.14	0.02	0.14	0.01	0.12
Retail trade; car sale & repair	0.20	0.40	0.20	0.40	0.18	0.38
Transp. & storage	0.04	0.19	0.04	0.19	0.04	0.19
Restaurants & hotels	0.09	0.28	0.09	0.29	0.05	0.21
Inform. & communic.	0.05	0.21	0.04	0.19	0.09	0.29
Finance & insurance	0.05	0.21	0.04	0.21	0.07	0.25
Real estate	0.01	0.12	0.01	0.12	0.01	0.09
Professional & tech. act.	0.07	0.26	0.07	0.25	0.12	0.33
Admin. activ.	0.10	0.31	0.10	0.30	0.12	0.33
Public admin.	0.00	0.06	0.00	0.06	0.00	0.05
Private education	0.04	0.19	0.04	0.19	0.04	0.20
Non-essential health	0.15	0.36	0.15	0.36	0.15	0.35
Recreation & entertainment	0.01	0.11	0.01	0.11	0.02	0.13
Other service act.	0.04	0.19	0.04	0.20	0.03	0.17
Observations						
Number of workers		61,826		44,256		17,570
Person-year obs.		953,014		815,597		137,417

NOTES: This table presents summary statistics on the control group for women. In particular, column (1) displays all women in the control group; column (2) displays childless women born between 1960-1980, i.e., women that are not yet 40 years old in the first year of the sample (1994) but reach 40 until the last year of the sample (2020); column (3) displays childless women born between 1981-2002, i.e., women that do not reach 40 years old until the last year of the sample but that are assigned to the group control group of non-mothers according to the model of zero lifetime fertility described in Section (2.2).

SOURCES: Calculations based on administrative datasets of QP and family relationships, 1994-2020.

Table 3: Summary statistics for the panel of employment and earnings linked to births: men with children, 1994-2020

	(1)		(2)		(3)	
	All - treated group		Before childbirth		After childbirth	
	Mean	(sd)	Mean	(sd)	Mean	(sd)
Birth Cohort						
1960-1974	0.28	0.45	0.18	0.39	0.33	0.47
1975-1989	0.69	0.46	0.78	0.41	0.65	0.48
1990-2002	0.03	0.17	0.04	0.19	0.02	0.15
Education						
Primary	0.36	0.48	0.31	0.46	0.38	0.49
Secondary	0.41	0.49	0.44	0.50	0.39	0.49
Tertiary	0.23	0.42	0.25	0.43	0.22	0.42
Monthly earnings	1,397	1,697	1,140	972	1,573	2,045
Hourly earnings	8	11	7	6	9	13
Weekly hours	120	79	127	76	116	81
Employer by industry prior to birth						
Agriculture	0.00	0.04	0.00	0.04	0.00	0.05
Mining & quarrying	0.01	0.08	0.01	0.07	0.01	0.08
Manufacturing	0.23	0.42	0.24	0.43	0.22	0.41
Electricity & gas	0.00	0.06	0.00	0.06	0.00	0.06
Water distribution	0.01	0.11	0.01	0.11	0.01	0.12
Construction	0.13	0.34	0.11	0.32	0.15	0.36
Retail trade; car sale & repair	0.20	0.40	0.21	0.41	0.19	0.40
Transp. & storage	0.11	0.31	0.10	0.30	0.11	0.32
Restaurants & hotels	0.04	0.20	0.04	0.19	0.04	0.20
Inform. & communic.	0.04	0.20	0.05	0.22	0.04	0.19
Finance & insurance	0.03	0.18	0.03	0.18	0.03	0.18
Real estate	0.01	0.08	0.01	0.08	0.01	0.08
Professional & tech. act.	0.04	0.20	0.04	0.20	0.04	0.19
Admin. activ.	0.08	0.28	0.08	0.27	0.09	0.28
Public admin.	0.01	0.08	0.01	0.08	0.01	0.08
Private education	0.01	0.09	0.01	0.09	0.01	0.09
Non-essential health	0.02	0.15	0.02	0.15	0.02	0.14
Recreation & entertainment	0.01	0.10	0.01	0.10	0.01	0.10
Other service act.	0.01	0.11	0.01	0.11	0.01	0.11
Observations						
Number of workers		291,608		291,608		
Person-year obs.		4,806,941		4,806,941		

NOTES: This table presents summary statistics on treatment group for men. In particular, it displays actual fathers from the panel of employment and earnings, and includes all men born between 1960-2002 that are observed with at least one child in period spanning from 1994 to 2020, as described in Section (2.2).

SOURCES: Calculations based on administrative datasets of QP and family relationships, 1994-2020.

Table 4: Summary statistics for the panel of employment and earnings linked to births: men without children, 1994-2020

	(1)		(2)		(3)	
	All - control group		Actual never fathers		Predic. never fathers	
	Mean	(sd)	Mean	(sd)	Mean	(sd)
Birth Cohort						
1960-1974	0.32	0.47	0.39	0.49	0.00	0.00
1975-1989	0.65	0.48	0.61	0.49	0.83	0.38
1990-2002	0.03	0.17	0.00	0.00	0.17	0.38
Education						
Primary	0.29	0.45	0.34	0.48	0.06	0.23
Secondary	0.48	0.50	0.41	0.49	0.78	0.41
Tertiary	0.23	0.42	0.24	0.43	0.16	0.37
Monthly earnings	1,219	1,083	1,284	1,163	940	560
Hourly earnings	7	6	8	7	6	3
Weekly hours	110	82	109	83	115	80
Employer by industry prior to birth						
Agriculture	0.00	0.04	0.00	0.04	0.00	0.03
Mining & quarrying	0.00	0.06	0.00	0.06	0.00	0.05
Manufacturing	0.18	0.39	0.19	0.39	0.14	0.35
Electricity & gas	0.00	0.05	0.00	0.05	0.00	0.05
Water distribution	0.01	0.11	0.01	0.12	0.01	0.10
Construction	0.12	0.32	0.13	0.34	0.06	0.23
Retail trade; car sale & repair	0.18	0.39	0.17	0.38	0.23	0.42
Transp. & storage	0.09	0.29	0.10	0.30	0.06	0.25
Restaurants & hotels	0.07	0.25	0.06	0.24	0.11	0.31
Inform. & communic.	0.05	0.22	0.05	0.22	0.06	0.24
Finance & insurance	0.03	0.16	0.03	0.17	0.02	0.12
Real estate	0.01	0.08	0.01	0.08	0.01	0.08
Professional & tech. act.	0.05	0.21	0.05	0.21	0.05	0.22
Admin. activ.	0.13	0.34	0.12	0.33	0.18	0.38
Public admin.	0.00	0.07	0.01	0.07	0.00	0.06
Private education	0.01	0.10	0.01	0.10	0.01	0.09
Non-essential health	0.03	0.17	0.03	0.17	0.03	0.17
Recreation & entertainment	0.01	0.11	0.01	0.11	0.01	0.12
Other service act.	0.02	0.13	0.02	0.13	0.01	0.12
Observations						
Number of workers		68,921		48,625		20,296
Person-year obs.		1,094,755		895,754		199,001

NOTES: This table presents summary statistics on the control group for men. In particular, column (1) displays all men in the control group; column (2) displays childless men born between 1960-1980, i.e., men that are not yet 40 years old in the first year of the sample (1994) but reach 40 until the last year of the sample (2020); column (3) displays childless men born between 1981-2002, i.e., men that do not reach 40 years old until the last year of the sample but that are assigned to the group control group of non-fathers according to the model of zero lifetime fertility described in Section (2.2).

SOURCES: Calculations based on administrative datasets of QP and family relationships, 1994-2020.

Table 5: Summary statistics for the panel of expenditures linked to births, January 2017 to December 2019

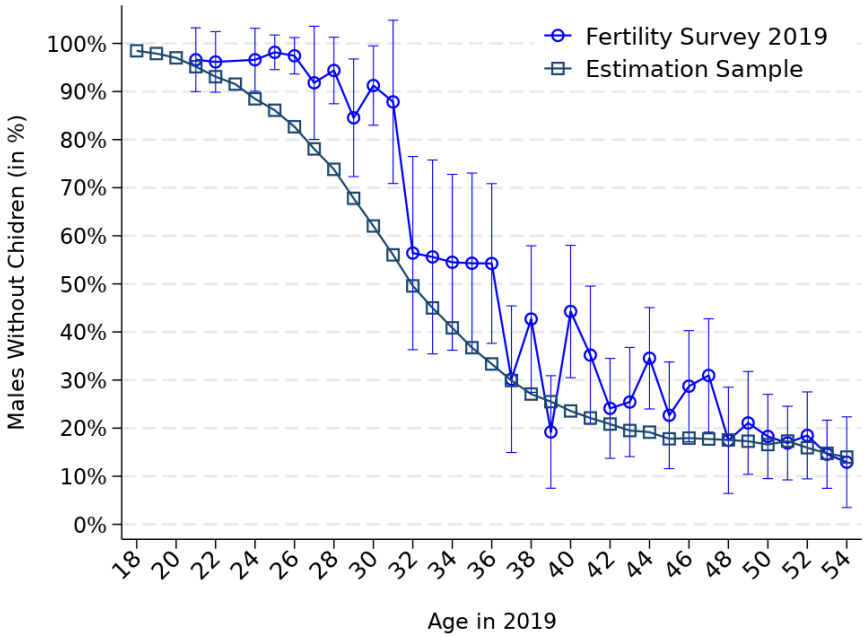
	(1) Full sample		(2) Before childbirth		(3) After childbirth	
	Mean	(sd)	Mean	(sd)	Mean	(sd)
Age at the first child						
Men	32.45	5.89	.	.	.	.
Women	31.24	4.80	.	.	.	.
Share of employed women	0.86	0.35	0.89	0.31	0.84	0.37
Monthly expenditure	1,195	8,220	1,083	11,697	1,283	3,298
Expenditure shares						
Agriculture	0.00	0.02	0.00	0.02	0.00	0.02
Mining & quarrying	0.00	0.00	0.00	0.01	0.00	0.00
Manufacturing	0.01	0.06	0.01	0.06	0.01	0.06
Electricity & gas	0.05	0.09	0.05	0.09	0.05	0.08
Water distribution	0.02	0.04	0.02	0.05	0.02	0.04
Construction	0.01	0.07	0.01	0.06	0.01	0.07
Retail trade; car sale & repair	0.38	0.27	0.37	0.28	0.39	0.26
Transp. & storage	0.01	0.05	0.01	0.06	0.01	0.05
Restaurants & hotels	0.04	0.09	0.05	0.10	0.03	0.08
Inform. & communic.	0.09	0.14	0.11	0.17	0.08	0.12
Finance & insurance	0.22	0.24	0.23	0.26	0.21	0.22
Real estate	0.01	0.05	0.01	0.06	0.01	0.05
Professional & tech. act.	0.01	0.06	0.02	0.07	0.01	0.05
Admin. activ.	0.01	0.06	0.01	0.07	0.01	0.06
Public admin.	0.01	0.04	0.01	0.04	0.01	0.04
Private education	0.03	0.10	0.02	0.09	0.04	0.11
Health & child day-care	0.08	0.15	0.06	0.13	0.10	0.15
Recreation & entertainment	0.01	0.05	0.02	0.06	0.01	0.03
Other service act.	0.01	0.06	0.02	0.06	0.01	0.05
Observations						
Number of households	149,006		131,494		148,882	
Household-month obs.	6,260,607		2,817,899		3,442,708	

NOTES: This table presents summary statistics on the panel of monthly expenditures linked to births from January 2016 to December 2019, as described in Section (2.2). Column (1) presents descriptives for the full sample; column (2) before childbirth on the normalized sample; column (3) after childbirth on the normalized sample.

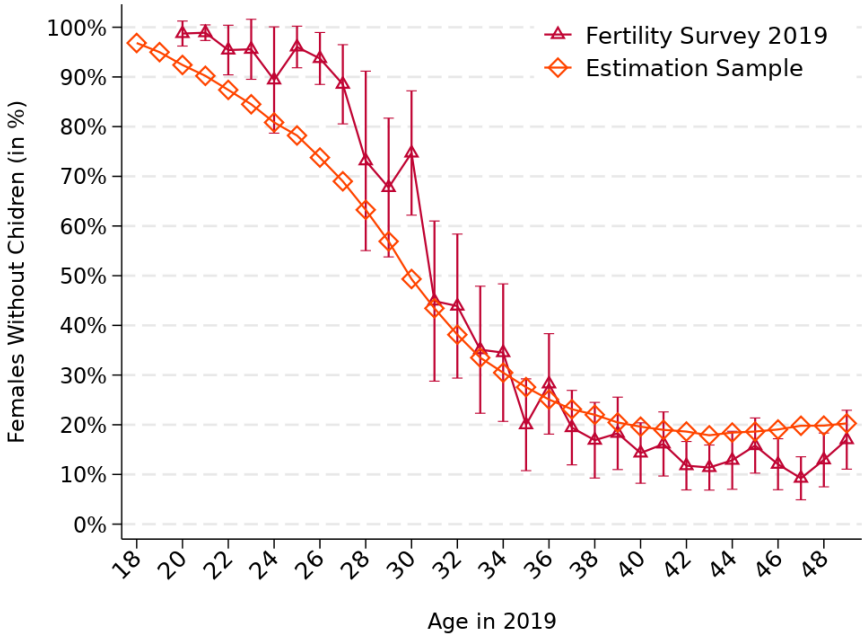
SOURCES: Calculations based on administrative datasets of e-Fatura and IRS, 2016-2019.

Figure 1: Fertility rates: administrative data vs. fertility survey

(a) Males



(b) Females



Notes: These figures display the comparison between the fertility rates computed from our sample based on administrative data and from survey data. Panel (a) displays the fertility rates for males. Panel (b) displays the fertility rates for females.

Sources: Calculations based on the administrative data of Quadros de Pessoal, and on survey data from *Inquerito à Fertilidade*, 2019.