

# The Child Penalty: Evidence from Spain\*

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## Abstract

Using data from social security records and an event study approach, we estimate the child penalty in Spain, looking at disparities for women and men across different labor outcomes following the birth of the first child. Our findings show that, the year after the first child is born, mothers' annual earnings drop by 11 percent while men's remain unaffected. The gender gap is even larger ten years after the birth. Our estimate of the long-run child penalty in earnings equals 28 percent, similar in magnitude to that found for Sweden and Denmark, and smaller than in the UK, the US, Germany, and Austria. In addition, we identify channels that may drive this phenomenon, including reductions in working days and shifts to part-time or fixed-term contracts. Finally, we provide evidence of heterogeneous responses in earnings and labor market participation by educational level: college-educated women react to motherhood more on the intensive margin (working part-time), while non-college-educated women are relatively more likely to do so in the extensive margin (working fewer days).

*JEL Codes:* I24, J13, J16, J21, J22, J31.

*Keywords:* gender, labor supply, employment, wages, fertility differentials, parenting, education.

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# 1 Introduction

The significant wage gap between male and female workers remains, to date, an undeniable reality in all countries. For instance, women’s median gross salary in Spain represented 78.4 percent of that of men’s in 2017.<sup>1</sup> The literature has discussed several explanations for this gap, including the role of human capital and career choices (Bertrand, 2011; Blau and Kahn, 2017; Buser et al., 2014; Olivetti and Petrongolo, 2016), gender-based discrimination (Goldin and Rouse, 2000), as well as the role of child-rearing responsibilities.

Related to the last explanation, a recent study by Kleven, Landais and Søgaaard (2019) concludes that most of the gender wage gap is linked to the effects of maternity. In their analysis, they use Danish administrative data and perform an event study around the birth of the first child. Intuitively, this empirical approach compares how earnings evolve in the years before and after giving birth, while flexibly accounting for age and year and month effects. They find evidence of a large and persistent impact of having children on various labor market outcomes.<sup>2</sup> Specifically, they find a female child penalty in earnings of 30 percent in the first year after the first childbirth, which converges to about 20 percent in the long run.<sup>3</sup> By contrast, men’s earnings are not affected by having children. Moreover, Kleven, Landais and Søgaaard (2019) provide evidence that women’s labor force participation, hours of work, and wage rate fall after the first childbirth, while this is not the case for men. Lastly, they conduct a decomposition analysis and find a striking increase in the fraction of child-related gender inequality from 40 percent in 1980 to about 80 percent in 2013.

A subsequent investigation by Kleven, Landais, Posch, Steinhauer, and Zweimüller (2019) extends the analysis to five additional countries: the United Kingdom, the United States, Germany, Austria, and Sweden. On the one hand, the study confirms the prevalence of child penalties for all the previously mentioned countries; on the other, it finds important differences in the magni-

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<sup>1</sup>Instituto Nacional de Estadística (INE), Encuesta Anual de Estructura Salarial (2017).

<sup>2</sup>Before them, several studies aimed at quantifying the importance of motherhood for explaining the gender wage gap using different methodologies and obtaining similar results. Adda, Dustmann, and Stevens (2017) develop a dynamic life-cycle model of the interaction between fertility, career and labor supply choices. Bertrand (2013) compares well-being measures among college-educated women with career and/or family- and, more specifically, on the impact of motherhood on earnings. Fernández-Kranz, Lacuesta, and Rodríguez Planas (2013) use Spanish Social Security records to investigate the underlying mechanisms that drive mothers’ lower earnings track, such as part-time work, accumulation of lower experience, or transitioning to lower-paying jobs. Angelov, Johansson, and Lindahl (2016) use an alternative approach focusing on the within-couple earnings gap. They find that 15 years after the first child has been born, the male-female gender gaps in income and wages have increased by 32 and 10 percentage points, respectively. Lundborg, Plug, and Rasmussen (2017) introduce a strategy based on in-vitro fertilization and find that fertility has negative, large, and long-term effects on earnings. See also recent work by Andresen and Nix (2020), Kleven, Landais, and Søgaaard (2019), and Pora and Wilner (2019) looking into the drivers of the penalty and potential reforms to address it.

<sup>3</sup>Following previous literature, throughout the document we refer to the short-run effect as the impact in the first year after the first childbirth, while the long-run effect refers to the impact 10 years after the first childbirth. Note that Kleven et al. (2019) and Sieppi and Pehkonen (2019) estimate the long-run effect as the average penalty five to ten years after the first childbirth.

tude of motherhood effects on earnings. Even though Sweden features similar long-run penalties than Denmark (26 percent), in the short-run the child penalty in Sweden exceeds 60 percent, twice the rate for Denmark. Both the US and the UK perform similarly, featuring penalties of almost 40 percent in the first year after childbirth that evolve to 31 and 44 percent after ten years. The most extreme cases are Austria and Germany, which feature short-run penalties of almost 80 percent and long-run penalties of 51 and 61 percent, respectively. Besides, Sieppi and Pehkonen (2019) complement the previous evidence by calculating the child penalty in Finland with similar results to those found for the other two Nordic countries: 61 percent in the short run and 25 percent in the long run.

Nonetheless, the literature on child penalties has focused on the European and American cases but remains sparse for developing countries with different institutional settings and cultural norms. Berniell et al. (2018) show that, after the first childbirth, women's gross earnings in Chile drop by 20 to 30 percent, and that this effect remains relatively stable in the long run. In a similar fashion, Querejeta (2020) studies the case of Uruguay and finds that formal employment falls by 30 percent in the short run; moreover, this penalty does not revert in the medium and long term, reaching 60 percent after ten years.

In this paper, we provide evidence from Spain using longitudinal data from Social Security records. Our contribution is twofold. First, we fill an existing gap in this literature by estimating the child penalty in Spain. Second, our dataset allows us to provide novel evidence regarding effects by contract characteristics. Specifically, we can test how parenthood affects men's and women's probability of working part-time (versus full-time) and in temporary (versus permanent) jobs.

To estimate the effects of parenthood on women's and men's labor earnings we follow closely the approach proposed by Kleven, Landais and Sogaard (2019). We find that female and male employees follow a similar trend until the birth of their first child. However, one year prior to the birth event, their earnings progression abruptly diverges. Our findings suggest that the short-run child penalty, defined as the amount by which women fall behind men due to motherhood in the first year, equals 11.4 percent. This phenomenon is amplified in the long run, with the child penalty widening to 28 percent after ten years. Our results point to a gender gap that is in line with that found for Denmark, although with higher persistence in the case of Spain. Our estimate of the long-run earnings child penalty is similar in magnitude to that found for Sweden and the US, and lower than that found in the UK, Austria, and Germany.

To shed some light on the drivers of this gap, we provide some additional results. First, there are similar child penalties (10 percent in the short-run and 23 percent after 10 years) on the number days worked - women reduce considerably their working days after their first childbirth,

while men’s working days are not affected. Second, women become more likely to work part-time right after having the first child, while that probability barely changes for men. Third, women’s likelihood of working on a fixed-term contract increases steadily over time while, again, men’s is not affected. Fourth, we provide results by the educational level of parents, showing that (i) the drops in earnings and days of work are substantially larger for non-college-educated than for college-educated women, (ii) the increase in women’s part-time work, by contrast, is larger for college than for non-college women, and (iii) the increase in fixed-term works is larger for non-college-educated women. Overall, our analysis of the mechanisms highlights the presence of substantial heterogeneity by educational level, suggesting that college-educated women react to motherhood more on the intensive margin (working part-time), while non-college-educated women are relatively more likely to do so in the extensive margin (working fewer days).

The remainder of this paper is structured as follows. Section 2 provides an overview of the data and the econometric specification of our analysis. Section 3 presents the results. Section 4 concludes. Supplementary information is available in the Appendix.

## 2 Data and empirical design

### 2.1 Data

The evidence presented is based on administrative records from the Continuous Sample of Working Histories (in Spanish, *Muestra Continua de Vidas Laborales*, MCVL). Each year, the MCVL selects a random sample of four percent of all Social Security affiliates and pensioners in that year. This is merged with richer data from the municipal census and the Tax Administration. MCVL annual issues, from 2005 to 2018, are representative of the population for the year of reference. Such representativeness is possible because, in addition to those individuals who were present in a past wave and remain registered within the social security system, the sample is refreshed with new members. Moreover, the MCVL includes historical labor market information dating as far back as 1967, with some earnings data being available since 1980, allowing us to construct the individuals’ employment history on a monthly basis: whenever a member stops working for a number of months but re-enters later, we identify the gap period as a career break and assign the value zero to earnings. In contrast, individuals ending their working life and never coming back as pensioners are no longer listed as Social Security affiliates in further MCVL waves.

The MCVL, therefore, represents a micro-level dataset that provides valuable information to evaluate the motherhood penalty. Following Kleven, Landais and Sogaard (2019), we track

the same workers up to ten years after the birth of their first child. In our setup, a balanced panel like this implies that individuals remain listed in the Social Security system for the whole period. In contrast, “unbalanced” encompasses all individuals affiliated at any point in time with no specific duration. Although the balanced sample ensures comparability across individuals over time, given that we will track each individual’s employment history for at least 15 years, this restriction also imposes some selection in terms of using information only on workers that managed to stabilize in the labor market for a long period.<sup>4</sup>

The sample we use only covers workers registered in the Social Security system under the general regime because, for the remaining schemes (mainly self-employment), the information on contribution bases has lower quality.<sup>5</sup> Besides, the exact family relationship of the employees to the individuals with whom they live is not made explicit in the dataset, thus implying that some assumptions are required to identify their children. In this respect, we infer that a worker’s first child has been born when we observe an individual of age 0 to 1 living together with an adult worker, as long as he/she is between 18 and 45 years old at the birth moment<sup>6</sup>, and no other child is present in the same household at that time.

Additionally to demographic characteristics of the individuals and their life partners, the labor market side is well-represented with income records from two different sources –monthly contribution bases and taxable income information–, job-specific variables, and the number of days worked per year.<sup>7</sup> Regarding the measurement of labor earnings, our preferred option is to use monthly contribution bases, for which comprehensive historical data is available since the late 1990s. Earnings from contribution bases are however top-coded due to regulatory constraints.<sup>8</sup> An alternative approach would be to extract annual, unbounded labor income from tax records, yet the data starts in 2005.

Our estimation sample covers 543,828 employees (264,391 mothers and 279,437 fathers) and almost 95 million monthly observations from 1990 to 2018. Table 1 shows the summary statistics. First, average monthly wages are higher for men than for women (1,893 vs. 1,281 euros, respectively), and for college-educated in relation to non-college-educated individuals. In terms of days worked in a month, men spend roughly 4 more days at work than women; within the latter, college-educated women work more days. Also, part-time work occurs more frequently

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<sup>4</sup>We repeated our estimation in an unbalanced sample obtaining robust estimates for the long run child penalty (see Figure A3 in the Appendix).

<sup>5</sup>In Spain, more than 80 percent of workers are enrolled in the general scheme of the social security system. There are alternative schemes for self-employed, workers in fishing, mining and agricultural activities, and domestic staff. For instance, as self-employed individuals choose how much to contribute themselves, García-Miralles et al. (2019) argue that income for self-employed can be poorly approximated using administrative data on tax returns.

<sup>6</sup>We impose this age range to capture parent-child relationship.

<sup>7</sup>The MCVL does not include information on hours worked.

<sup>8</sup>There is a legal upper bound on monthly contributions which, for the case of high-earners, makes a fraction of income unobservable.

for women (4 percent for men vs. 23 percent for women), a feature also observed for fixed-term contracts though to a lesser extent. Finally, women are more likely than men to hold college education (63 percent vs. 53 percent, respectively).

Table 1: Summary statistics

	Total		Men						Women					
	All		All		College		Non-college		All		College		Non-college	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	34.2	6.54	34.9	6.61	35.6	6.39	34.2	6.78	33.5	6.40	34.1	6.14	32.6	6.72
Monthly wages	1576	1197	1893	1142	2260	1210	1486	901	1281	1171	1593	1251	750	768
Monthly days worked	24.3	11.4	26.6	9.12	27.1	8.48	25.9	9.75	22.3	12.8	24.2	11.5	19.0	14.1
Part-time (1=yes)	0.13	0.34	0.04	0.20	0.05	0.21	0.04	0.20	0.23	0.42	0.21	0.41	0.26	0.44
Fixed-term (1=yes)	0.30	0.46	0.28	0.45	0.22	0.41	0.35	0.48	0.32	0.47	0.30	0.46	0.36	0.48
College (1=yes)	0.58	0.49	0.53	0.50					0.63	0.48				

Notes: Statistics computed over the full period (1990-2018). SD: Standard Deviation. Monthly wages in Euros of 2018.

Figure A1 in the Appendix shows average earnings profiles over time, comparing the different possible samples and income sources. The main takeaways from this figure are: (i) for women, we observe a decrease in earnings after the birth of the first child, a feature not seen in men (ii) differences in earnings profiles between balanced and unbalanced samples are more pronounced starting in 2005 than in 1990 because of the huge impact that the Great Recession had on employment, and (iii) the incidence of censorship for women is very small (note that the gray and brown lines overlap).

## 2.2 Empirical design

We follow the event-study specification proposed by Kleven, Landais and Sogaard (2019). This setup is based on comparing mothers' labor market outcomes relative to fathers' around the event of the first childbirth. The baseline specification stems from a balanced panel in which we observe each parent from five years before to ten years after their first child is born. Therefore, the event time  $t$  is indexed in relation to the year of the first childbirth, in a way such that  $t = 0$  for the first year after the birthdate, with  $t$  ranging from -5 to +10. Similarly, we extend our analysis to further capture very long-run effects by using observations up to 15 years after the first birth event.

In particular, we run the following regression separately for men and women:

$$Y_{i,y,m,t}^g = \sum_j \alpha_j^g \cdot I\{j = age_{iym}\} + \sum_k \beta_k^g \cdot I\{k = y \times m\} + \sum_{s \neq -1} \delta_s^g \cdot I\{s = t\} + \varepsilon_{i,y,m,t}^g \quad (1)$$

where  $Y_{i,y,m,t}^g$  represents the outcome of interest for individual  $i$  of gender  $g$  in year  $y$  and month  $m$  at time event  $t$ . Each individual  $i$  contributes with one observation per year  $t$ , referenced to the month  $m$  of birth. The right-hand side of the specification includes age, year  $\times$  month interactions, and event time binary variables. We exclude the event time dummy corresponding to  $t = -1$ , so that the event time coefficients capture the impact of parenthood relative to the year preceding the first childbirth. Moreover, the inclusion of age dummies controls non-parametrically for latent life-cycle trends and, similarly, the year and month dummies control for business cycle effects.<sup>9</sup>

Our main outcome  $Y_{i,y,m,t}^g$  is gross annual earnings. We construct this variable using monthly observations in the following way: for each individual, we take as a reference the year and month of birth of the first child; for each year until/since that point, we take the contribution base for the reference month and add the preceding 11 monthly bases. Besides, we have also considered two other measures yielding similar estimation results; however, the chosen baseline minimizes the observed differences in earnings between men and women before childbirth.<sup>10</sup>

The effects on earnings might arise from changes in the extensive margin (number of days worked), the intensive margin (number of hours worked per day), or from changes in the wage rate. Given that our data do not contain information on hours worked, we cannot estimate effects on the wage rate. However, we can, to some extent, estimate the relative changes in the extensive and intensive margins, by considering as alternative outcomes “number of days worked per year”, and “part-time job”. As an alternative outcome, we consider “fixed-term job”. Finally, for each of these outcomes, we perform a heterogeneity analysis by running the estimating equation separately for college-educated and non-college-educated workers.

In a second step, the estimated level effects are converted into percentage figures, calculated as

$$P_t^g = (\delta_t^g) / E[\tilde{Y}_{i,y,m,t}^g | t] \quad (2)$$

where  $\tilde{Y}_{i,y,m,t}^g$  is the predicted labor income net of the event time dummies, that is, the counterfactual in the hypothetical case of not having children:

$$\tilde{Y}_{i,y,m,t}^g = \sum_j \tilde{\alpha}_j^g I\{j = age_{iy m}\} + \sum_k \tilde{\beta}_k^g I\{k = y \times m\}$$

<sup>9</sup>Unlike Kleven, Landais and Sogaard (2019), we are able to pin down the month of birth of the first child.

<sup>10</sup>Firstly, for each individual and year from / to the birth of the child we add the 12 monthly observations in that year (event year measure). Second, for each individual and natural year we add the 12 corresponding monthly observations and then compute the average over the time event variable (natural year measure).

Once the children effect has been estimated separately for men and women, we measure child penalty as the percentage by which women fall behind men due to children at event time  $t$ :

$$P_t = \frac{\hat{\delta}_t^{men} - \hat{\delta}_t^{women}}{E[\tilde{Y}_{i,y,m,t}^{women} | t]} \quad (3)$$

As in Kleven, Landais and Sogaard (2019), there are two steps in the estimation of the child penalty: estimating the effect of the first child separately for men and women and then estimating the child penalty. The former requires a stronger identification assumption: that mothers (fathers) having first child in year  $t$  would have followed the same trends as mothers (fathers) having first child in another year had neither of them had children. Hence, the separate earnings dynamics for mothers and fathers should be interpreted with caution. The second step (the child penalty), by contrast, relies on the milder smoothness assumption of event studies. This assumption is validated with tests of parallel trends before childbirth, i.e., the wage (or other outcome) trajectories of men and women being parallel for  $t < 0$ . Even under parallel trends, it is important to keep in mind that this approach requires no anticipation (i.e., women who anticipate that next-year's earnings will decline might decide (to try) to become pregnant) and that the smoothness around the date of birth may be less informative as we consider periods further away from the event.

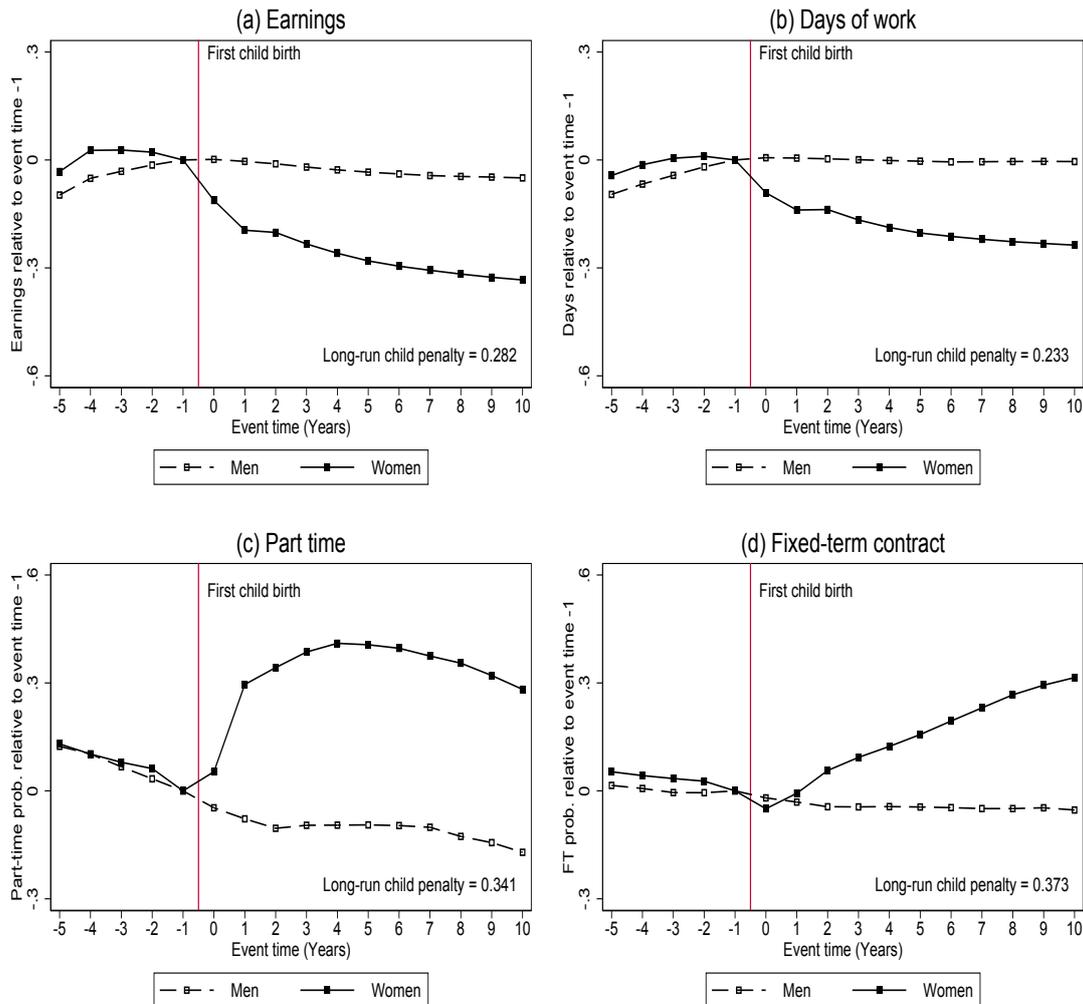
### 3 Results

#### 3.1 Impacts of first child

Graph (a) in Figure 1 presents the gross annual earnings trajectory for men and women throughout a 15-year window around the birth of their first child. For that purpose, we plot the gender-specific estimates of total earnings before taxes and transfers, previously defined as  $P_t^g$ , across event time. As mentioned above, outcome estimates at event time  $t$  are expressed relative to the year before the first childbirth, i.e., event time  $t - 1$ . As can be seen in the graph, in the year following the birth of the first child, mothers face a loss in gross earnings of 11.2 percent with respect to their pre-birth rate, while fathers' earnings increase by 0.15 percent. During the following year, women's earnings continue declining to 19.5 percent. This diverging trend in earnings for male and female workers continues even ten years after the first childbirth, so that throughout the years, women's earnings never return to levels prior to maternity. In fact, ten years after the birth of a first child, female earnings stabilize at around 33 percent lower, whereas male earnings drop by 5 percent. Hence, our estimate of the long-run child penalty is 28

percentage points. These estimates resulting from the baseline specification confirm a significant and persistent negative effect for mothers in a ten-year bracket; moreover, the divergence remains when extending the analysis to 15 years ahead (see Figure A2 in the Appendix).<sup>11</sup> Given that, like Kleven, Landais and Sjøgaard (2019), we focus on the first child, these estimates also include the penalties to subsequent births.

Figure 1: Impacts of first child



Notes: The figure shows event time coefficients estimated from equation (2), absent children for men and women separately and for different outcomes. Each graph also reports a “long-run child penalty”—the percentage by which women are falling behind men due to children, calculated from equation (3) at event time  $t=10$ . All of these statistics are estimated on a balanced sample of parents who have their first child between 1994 and 2009 and who are observed in the data during the entire period between five years before and ten years after child birth. The effects on earnings and days of work are estimated unconditional on employment status, while the effects on part time and fixed-term contract are estimated conditional on working.

<sup>11</sup>Estimates of the child penalty in the year following the child birth using the alternative definition of annual earnings are 15.2 and 12.5 for the event year and the natural year measures, respectively, relative to the 11.4 in the baseline specification; whereas the estimates in the long run are 26.7 and 26.1, respectively, relative to the 28.2 in the baseline.

Next, we study some possible causes underlying this gender gap. Graph (b) in Figure 1 shows that, while men and women are on similar trends in terms of their number of days worked in a year prior to becoming parents, women’s days worked significantly fall after childbirth, while men’s are not affected. In particular, our estimates reveal that, 10 years after childbirth, women have reduced the number of days worked by 23 percent while the change for men is negligible. The results in graph (c) also show large gender differences in the probability of working part-time. While part-time probability for men decreases after parenthood, that probability for women increases considerably (indeed, the relative gap equals 34 percent after 10 years). Finally, graph (d) shows that women become more likely (32 percent) to work under a fixed-term contract after childbirth, while men’s probability is 5 percent lower.

### 3.2 Impacts of first child by education

As noted by Kleven, Landais, and Sogaard (2018), if there is heterogeneity in child penalties by education, this might be suggestive that a comparative advantage channel is driving (part of) the child penalty. To check whether women at the top of the education distribution incur about the same penalty as women at the bottom, in Figure 2 we perform the same analysis as in Figure 1, but we condition on the educational level. We define education as the the maximum level reached pre-birth so that education cannot be a bad control (i.e., an outcome).

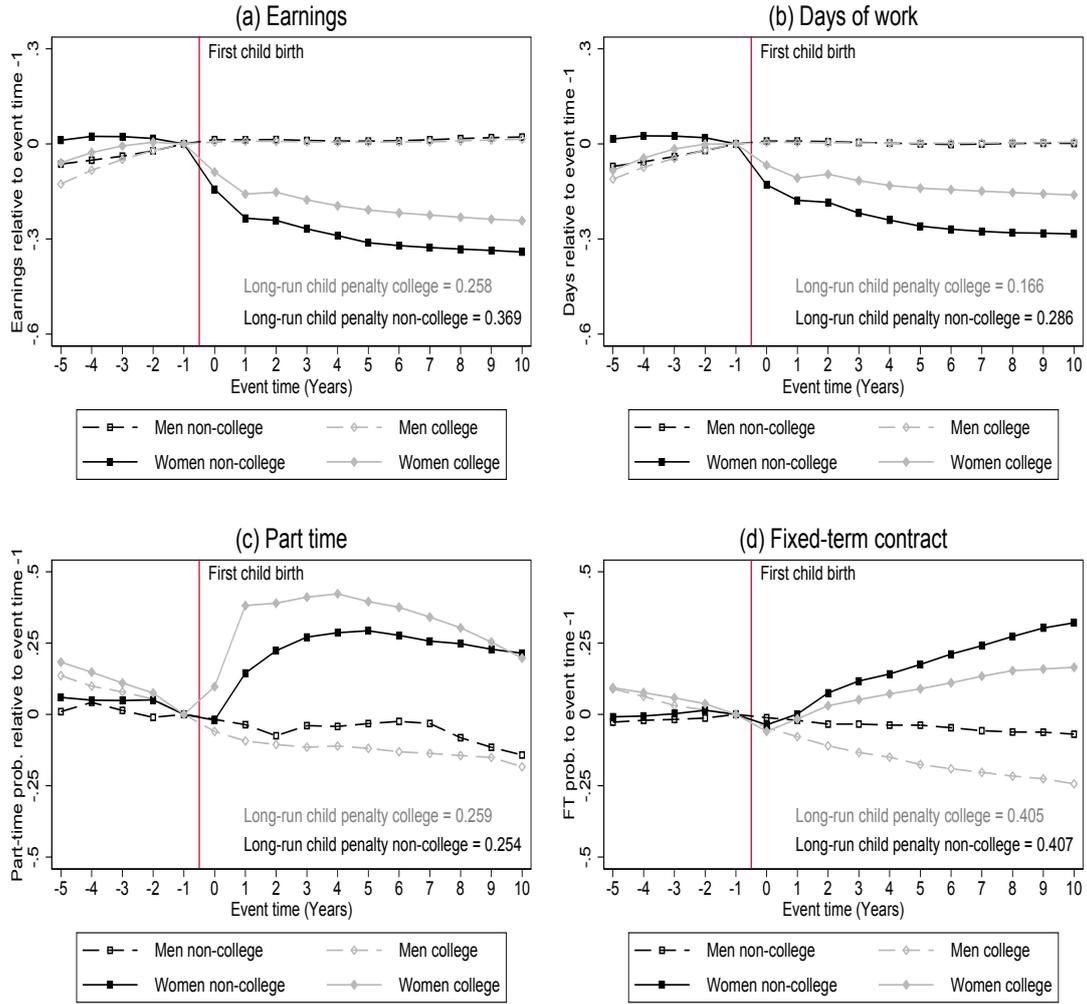
Graphs (a) and (b) show that the drops in earnings and days of work are significantly larger for non-college-educated than for college-educated women.<sup>12</sup> Graph (c) reveals that right after the birth the increase in women’s part-time work, by contrast, is larger for college than for non-college women (although the impacts tend to converge over time). Finally, the increase in fixed-term contracts is larger for non-college-educated women than for college-educated women. In this case, however, we also see some differences for men, with college-educated men also becoming significantly less likely to work on a fixed-term job over time once they become parents, while for non-college men that probability barely changes.

Our results in this respect are not completely aligned with those in Kleven, Landais, and Sogaard (2018), who show that there is essentially no heterogeneity in child penalties with respect to the relative education levels of the parents. The heterogeneity patterns we uncover for the Spanish case could be due to the comparative advantage channel, as proposed by the literature.

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<sup>12</sup>Repeating the exercise in the unbalanced sample for college graduates starting in 2005 and using the uncapped taxable income as earnings measure, yield very similar results (a long run child penalty of 24 percent instead of 25 percent), implying that the lower impact for this group is not due to the censoring of contribution bases (see Figure A4 in the Appendix).

Figure 2: Impacts of first child, by education



Notes: Same data and estimating equation as in Figure 1, conditioning on the educational level.

### 3.3 Comparison to other countries

Table 2 compares the long-run penalties in Spain with those found in other countries. The magnitude of the Spanish child penalty is similar to that found for the Nordic countries (Denmark, Finland, and Sweden), which present the lowest rates. On the contrary, the German-speaking countries (Germany, Austria) feature the highest motherhood penalties, with the UK and the US in between. We also observe that workers considered across studies are not very different in terms of year and parent's age at the first childbirth event.

Table 2: Comparison of country statistics and estimates

	Year of first child		Age at first child		No. children (at t=10)	Long-run child penalty
	Range	Mean	Men	Women	Mean	
Austria	1985-2007	1995	30.0	26.5	1.7	51
Denmark	1985-2003	1994	28.5	26.2	2.2	21
Finland	1992-2007		29.8	27.6		25
Germany	1989-2005	1997	30.4	27.7	1.9	61
Spain	1994-2009	2002	32.4	31.0	1.4	25
Sweden	1997-2011	2004	30.8	28.7	2.2	26
United Kingdom	1991-2008	1998	31.3	30.0	2.0	44
United States	1967-2006	1985	25.8	24.9	2.1	31

Notes: The long-run child penalty is defined as the average penalty from event time five to ten. The estimate for Spain has been adjusted for comparability purposes. Statistics for countries other than Spain are from Kleven, Landais, and Sogaard (2019); Kleven, Landais, Posch, Steinhauer, and Zweimüller (2019); Sieppi and Pehkonen (2019).

## 4 Concluding remarks

Motherhood explains a significant proportion of the gender gap in earnings. Using longitudinal data from the Social Security records, we follow the event-study methodology proposed by Kleven, Landais, and Sogaard (2019) to estimate the child penalty in Spain. We explore the profiles over time of different labor market outcomes for men and women. In general, there are no remarkable differences until the first childbirth but women diverge considerably from that moment on. Our findings suggest that the child penalty in earnings is 11.4 percent in the year after the first child is born and continues widening to 28 percent in the long run. Overall, the magnitude of the Spanish child penalty is similar to that found for the Nordic countries, and smaller than that in the UK, the US, Germany, and Austria.

Moreover, we document different channels through which mothers present a lower earnings profile. For instance, women reduce considerably their working time after their first childbirth, with a 9.8 percent child penalty in the number of days worked in the first year and 23 percent 10 years after. In terms of alternative work arrangements, the probability of women working part-time rises by 30 percent one year after their first child, while for men that probability decreases by 8 percent. Besides, women become increasingly more likely to work under a fixed-term contract while men remain unaffected. We also find that reductions in earnings and days worked after the first childbirth are substantially larger for women with no college degree. In contrast, college-graduated mothers appear more prone to remain employed but working part-time.

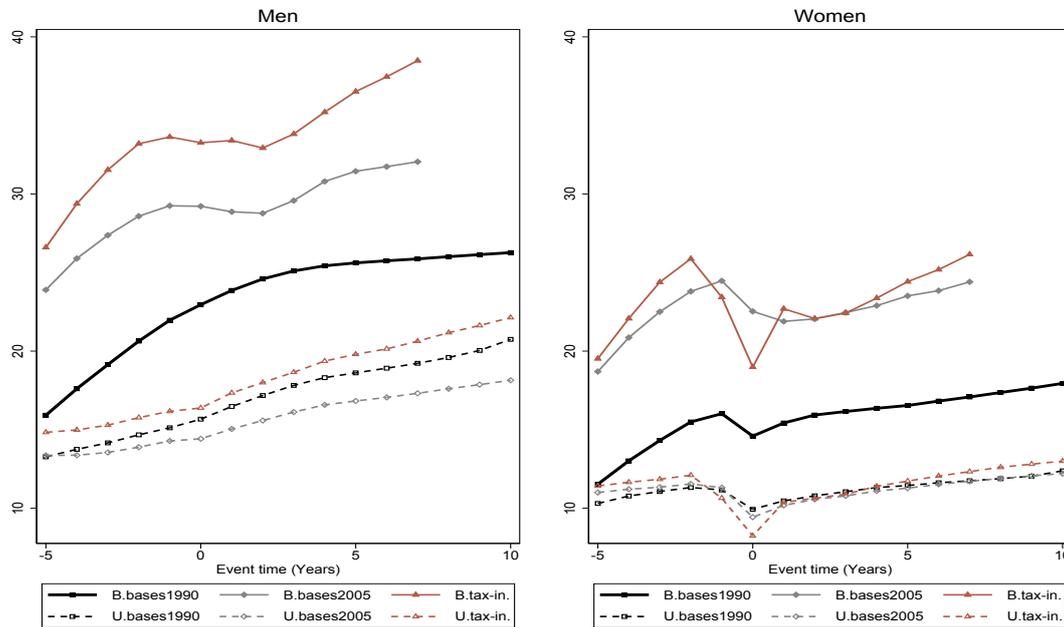
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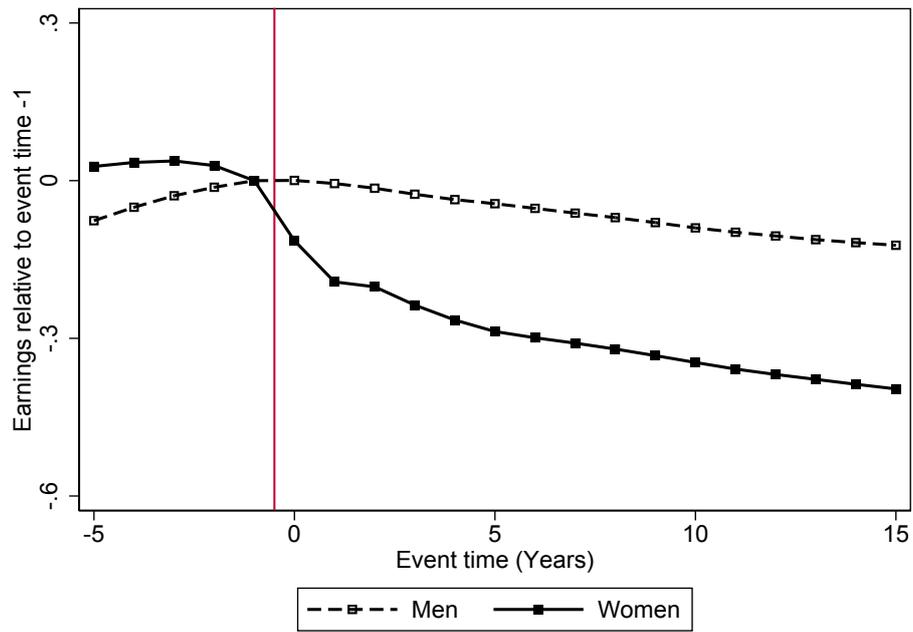
# Appendix

Figure A1: Earnings profiles



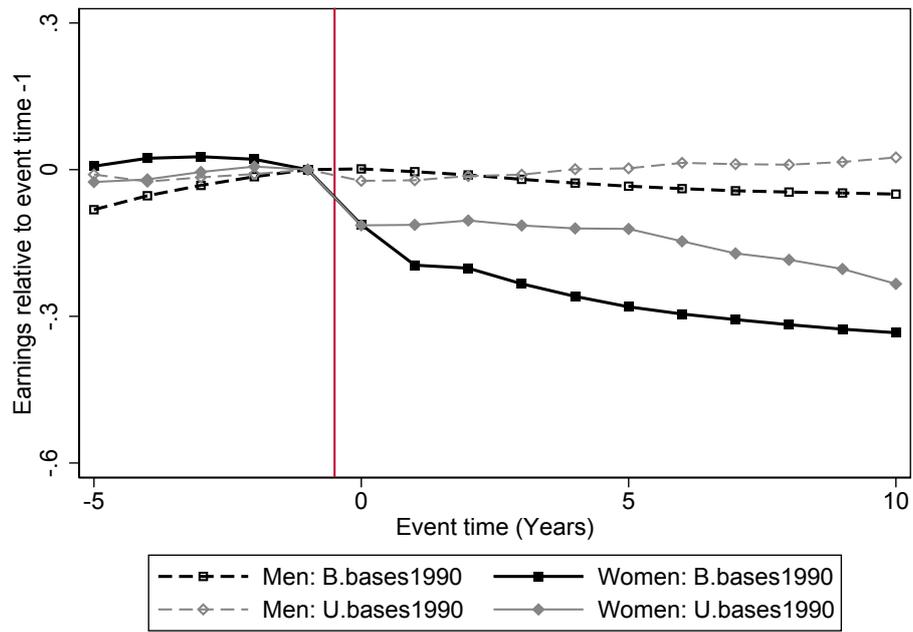
Notes: the figure represents averages of annual earnings (in 2018 thousand euros), calculated separately for men (on the left) and women (on the right). Each sample consists of individuals having their first child at event time  $t = 0$ . Solid lines represent averages obtained from balanced panels (B) starting in 1990 (black) or in 2005 (gray for contribution bases and brown for uncapped taxable income). Dashed lines represent averages obtained from unbalanced panels (U) starting in 1990 (black) or in 2005 (gray for contribution bases and brown for uncapped taxable income).

Figure A2: Impact of the first child on earnings, 15 years ahead



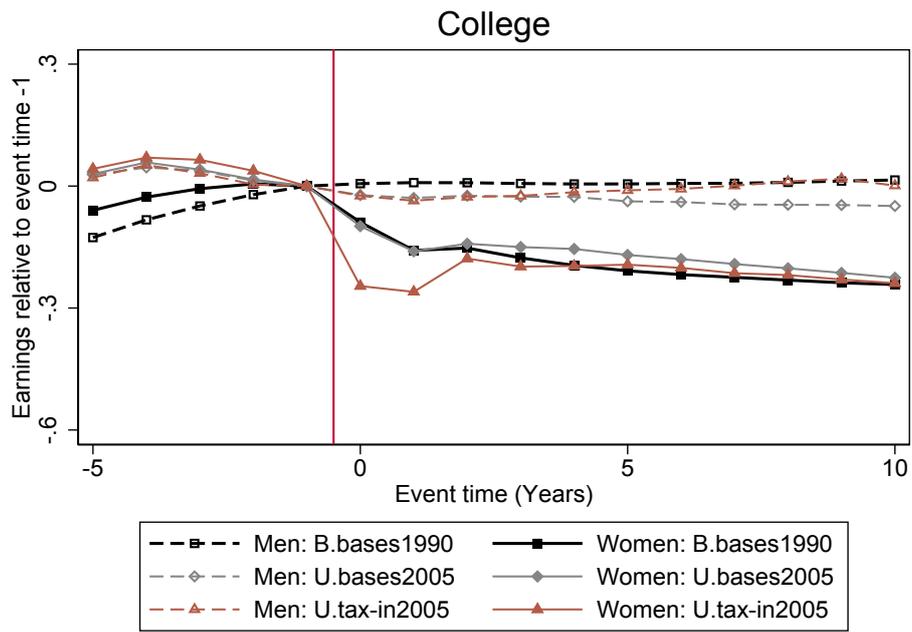
Notes: Same estimating equation as in graph (a) of Figure 1 but in a balanced panel of 20 years. The child penalty calculated from equation (3) at event time  $t=15$  is 0.275.

Figure A3: Impact of the first child on earnings, balanced vs unbalanced samples



Notes: Same estimating equation as in graph (a) of Figure 1 but in both a balanced and an unbalanced panel of 15 years.

Figure A4: Impact of the first child on earnings, College



Notes: Same estimating equation as in graph (a) of Figure 1 but in the unbalanced sample for college graduates starting in 2005 and using the uncapped taxable income as earnings measure.