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Fertility response to family policy: Insights from Spain's paternity leave

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Abstract

Purpose – This study aims to examine the effect of paternity leave on fertility over time. It exploits the staggered expansion of paternity leave from 2 to 12 weeks to assess how the leave influenced birth rates overall and among specific maternal groups.

Design/methodology/approach – The authors use national birth records and apply a time series synthetic control framework within a Bayesian Structural Time Series model to construct counterfactual fertility series and estimate the effect of paternity leave over time. As leave entitlement is determined by the child's date of birth, the authors exploit policy cutoffs to identify variation in exposure. This allows them to track the effects over time and beyond couples who had children immediately around each reform. Additionally, the authors distinguish impacts across maternal groups.

Findings – While no aggregate effect on fertility is observed, persistent increases in birth rates are found among specific groups. The introduction of a two-week paternity leave led to an 8.4% increase in third births and a doubling of birth rates among employed mothers. These effects remained for at least two years post-reform. Later extensions of paternity leave did not produce further changes in fertility.

Originality/value – This study provides novel evidence on the limited aggregate impact of paternity leave on fertility and highlights its heterogeneous effects over time across maternal characteristics and birth order. The findings underscore the potential of paternity leave to support fertility through father involvement among employed and non–first-time mothers, suggesting that targeted policies toward these groups could be more effective in promoting fertility.

Keywords Paternity leave, Fertility, Policy evaluation, Synthetic control

Paper type Research paper

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JEL classification – D13, H31, I12, J08, J13

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1. Introduction

Over the past three decades, fertility rates in OECD countries have declined by 20%, reaching an average of 1.6 births per woman, half a birth below the replacement level (OECD, 2024). Spain lies even further below this threshold, with 1.3 births per woman. New models of fertility point to the incompatibility of women's career and family goals as a major determinant to explain the ultra-low fertility levels in high-income countries. The ability to reconcile work and family life is shaped by contextual factors such as labor market flexibility, family policies and prevailing social norms (Doepke *et al.*, 2022). Among these, paternity leave policies that reserve a non-transferable share of leave for fathers play a critical role, as they can influence the division of paid and unpaid labor within households, challenge traditional gender roles and reduce the perceived costs of childrearing (Farré, 2020; Farré and González, 2019; Gauthier and Philipov, 2008).

Uptake of paternity leave has been associated with a range of changes in family dynamics, including promotion of gender equality (Almqvist and Duvander, 2014; Dearing, 2016; Doss, 2013), shifts in household bargaining (Arnalds *et al.*, 2021), and evolving gender norms (Hart *et al.*, 2022), all of which may influence fertility decisions. While previous studies, particularly in Spain, have explored the relationship between paternity leave and fertility (Farré and González, 2019; González and Zoabi, 2021), they have largely focused on subsequent fertility and local effects, that is, how the policy influenced the timing of having an additional child among couples who gave birth around the time of the policy's reform. However, significant gaps remain in understanding how paternity leave affected fertility in the overall population of potential beneficiaries (i.e. any couple having a birth at any time after the reform, regardless of whether they already have a child) and how these effects evolve over time and respond to more extensive policy reforms.

The objective of this paper is to study the effect of paternity leave on fertility in the overall population over time. To this end, variation in exposure to leave benefits is used. Particularly the introduction of a two-week entitlement in Spain in 2007 and later extensions up to 12 weeks. Using national birth records within a time series synthetic control framework, the analysis tracks the policy effect at each point in time, allowing the identification of any consolidation or delayed effect. This approach diverges from previous studies on paternity leave in three key aspects: it captures impacts for all couples giving birth at any point after the reform; it examines fertility beyond subsequent fertility; and it evaluates multiple stages of the policy, including both its introduction and subsequent expansions.

First, we graphically illustrate how paternity leave uptake among new fathers evolves following each policy extension. Next, we examine the effect of paternity leave on fertility across the population and among specific subgroups. Our findings indicate that paternity leave is not associated with overall changes in fertility; however, certain groups do respond. Employed mothers and women having a third child exhibit a positive and sustained increase in fertility over time linked to the introduction of the two-weeks leave. Specifically, the number of third-order births increases on average by 0.03 monthly births per 1,000 women – an 8.4% rise – amounting to approximately half an additional birth per 1,000 women over the 20 months following the reform. Among employed mothers, the effect is even more pronounced, with an average monthly increase of 2.7 births per 1,000 working mothers, effectively doubling pre-policy rates and accumulating 57 additional births per 1,000 women over 20 months. By contrast, neither group shows any measurable change in fertility following subsequent extensions of the leave. We find no consistent evidence that effects vary by maternal education or age.

In some countries, parental leave benefits are gender neutral, granting both parents equal duration, pay and transferability. However, in others, differences may exist (Van Belle, 2016). Additionally, social norms (Miyajima and Yamaguchi, 2017) and institutional factors,

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such as pre-existing childcare arrangements (Albanesi *et al.*, 2022; Thomas *et al.*, 2022), can mediate how parental leave affects fertility. As a result, empirical findings on the relationship between parental leave policies and fertility are often mixed and challenging to reconcile.

The earliest parental leave policies predominantly focused on women through maternity leave, sometimes allowing shared periods between parents. Some studies link paid and job-protected maternity benefits to positive effects on fertility, as observed in Austria (Lalive and Zweimüller, 2009) and Germany (Raute, 2019). In contrast, paid maternity leave did little to encourage fertility in Norway (Dahl *et al.*, 2016), and evidence remains mixed in the USA (Averett and Whittington, 2001; Flores *et al.*, 2023; Golightly and Meyerhofer, 2022; Olivetti and Petrongolo, 2017; Pihl and Basso, 2019; Rossin-Slater *et al.*, 2012). For a comprehensive review of the literature, see Olivetti and Petrongolo (2017).

To encourage greater paternal involvement, many countries have introduced targeted parental leave benefits, including father-specific quotas – non-transferable leave reserved exclusively for fathers and forfeited if unused. Evidence suggests that fathers largely respond to these incentives, with notable increases in paternity leave uptake observed in Sweden [except among traditionally oriented fathers or those with weak labor market attachment (Aldén *et al.*, 2023)], Norway, Spain, the USA and Canada (Almqvist and Duvander, 2014; Bartel *et al.*, 2017; Cools *et al.*, 2015; Dahl *et al.*, 2016; Duvander and Johansson, 2012; Farré and González, 2019; Patnaik, 2019). However, some studies highlight a "labeling effect," where fathers take only the leave explicitly allocated to them (Patnaik, 2019).

Despite increased paternity leave uptake, evidence on its effects on fertility remains inconclusive. In the Nordic context, several studies find no significant effect in Norway following the introduction of a one-month father quota, neither on the likelihood of having another child (Duvander *et al.*, 2020; Hart *et al.*, 2022) nor on birth spacing (Cools *et al.*, 2015) or total number of children (Cools *et al.*, 2015; Hart *et al.*, 2022; Kotsadam and Finseraas, 2011). Conversely, research from Sweden links the introduction of a similar one-month reserved leave for fathers to a higher probability of having a third child, particularly among low-income couples (Duvander *et al.*, 2020). In Belgium, the introduction of a two-weeks paternity leave reform is found to increase the spacing between the first two children for first-time young mothers (Fontenay and Tojerow, 2020). In Southern Europe, evidence from Spain suggests that the implementation of a two-week paternity leave led couples to delay subsequent childbearing (Farré and González, 2019) and, among those with intermediate wage gaps, to reduce the likelihood of having additional children (González and Zoabi, 2021). In contrast, evidence from Canada shows a positive association between the introduction of a five-week non-transferable leave reserved for fathers and increases in total fertility rates, particularly among mothers with a college diploma (Laplante, 2024).

A broader understanding of fertility responses to paternity leave is needed, one that goes beyond local effects (those observed among couples giving birth around the time of a reform), subsequent fertility and that assess the impact across different policy stages. Equally important is identifying which groups are most responsive to these policies, as this knowledge is essential for designing effective interventions to support fertility. Our study aims to contribute to fill these gaps by providing a comprehensive, over-time evaluation of paternity leave reforms and their heterogeneous effects on fertility in the overall population.

We hypothesize that the introduction and subsequent reforms of paternity leave may affect fertility in the overall population in ways that differ from the local effects observed on subsequent fertility. *A priori*, we expect a positive aggregate population effect on fertility. The signaling of greater father involvement and the enhanced work-family compatibility that

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paternity leave could provide may reduce the perceived opportunity costs of childbearing and, in turn, foster an increase in fertility.

The remainder of the paper is organized as follows. First, Section 2 outlines the institutional context that motivates our study. Next, Sections 3 and 4 present the methodology and the data. Then, Section 5 reports our main findings and robustness checks. Finally, Section 6 concludes with a discussion.

2. Institutional setting

On March 22, 2007, Spain passed Organic Law 3/2007, which introduced a 13-day leave exclusively for fathers to promote work-family balance. Applicants had to be employed (or actively looking for a job) and bound to Social Security for a minimum of 180 days in the seven years immediately preceding, or 360 days throughout their working life, with slightly different requirements for fathers under 26 years of age [1]. The leave was fully wage-replaced up to a maximum of 2,996.10 euros and had to be taken all at once, immediately after the birth of the child (Boletín Oficial del Estado, 2007). There was no change in maternity leave benefits.

The father leave benefits were progressively extended to 4 weeks in 2017, 5 weeks in 2018, 8 weeks in 2019 (with the first two weeks being mandatory after birth), 12 weeks in 2020 (with the first four weeks being mandatory after birth), 16 weeks in 2021 and finally to 19 weeks in 2025 (with the first six weeks being mandatory after birth). The introduction of paternity leave was not announced in advance, whereas its extensions were disclosed with some notice (Supplementary Table S1).

Prior to the intervention, few fathers (2% of annual births) used the share of maternity leave that mothers could transfer to them in Spain. Meanwhile, more than 6 out of 10 births had a mother on leave. Once the two-week benefit was introduced in 2007, the share of mothers on maternal leave remained almost constant at the level prior to the introduction (range between 65% and 70% of births), but fathers did comply with the policy. Almost 40% of children born in 2007 had a father at home after birth. The percentage of births with a father on leave (both from the shared periods on maternity leave and the father-exclusive periods) closely mimics the policy reforms. The percentage of fathers jumps with the introduction, rises further with the four-week extension, and reaches approximately 80% with the eight-week extension. It then fluctuates between 80% and 70% of births during the 12- and 16-week periods (Figure 1). Interestingly, there were jumps not only in the proportion of fathers taking leave but also in the duration of leave taken. Using data covering all paternity leave permits since 2016, Farré *et al.* (2024) show that fathers almost fully used the weeks available in each extension, and the number of weeks taken non-concurrently with mothers has also increased. Because of their recent approval, data from the latest 2025 extension are not included in the analysis.

3. Methodology

To assess the effect of paternity leave on fertility rates over time, we exploit the variation in exposure to leave benefits resulting from the staggered implementation of the paternity leave policy in Spain. Our analysis adopts a potential outcomes framework (Rubin, 1974), defining the causal effect of each reform as the difference between the observed fertility rate after the policy change and the counterfactual fertility rate that would have prevailed in the absence of the reform. Since the counterfactual is never observed, we estimate it using a Bayesian Structural Time Series (BSTS) model (Scott and Varian, 2013; Brodersen *et al.*, 2015).

The BSTS method is a flexible modeling approach that generalizes the synthetic control method to time series data. The model constructs the counterfactual fertility based on the

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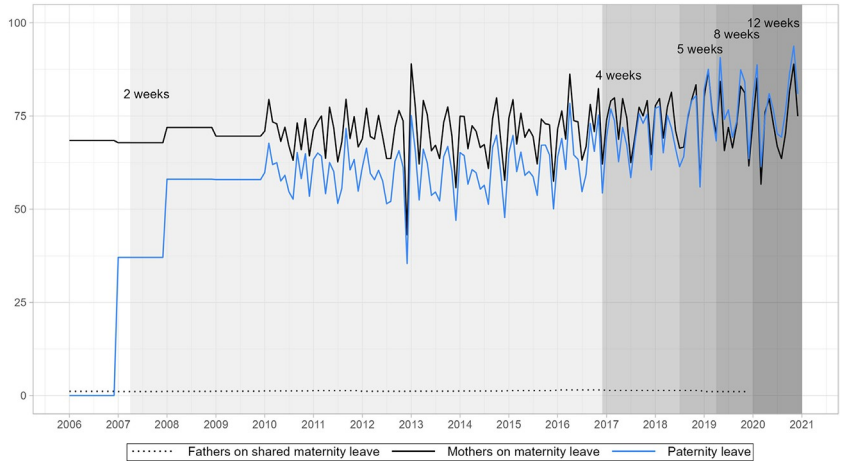

Figure 1. Applications for parental leave benefits as a share of the annual number of births

Note(s): The series *paternity leave* represents the number of applications for paternity leave benefits as a fraction of annual births. Similarly, *fathers on shared maternity* indicates the number of fathers applying for the shared periods of maternity leave, relative to the number of annual births. Lastly, *mothers on maternity leave* captures the number of maternity leave benefits claimed by mothers as a fraction of annual births. The shaded areas indicate policy reforms. Data is annual prior to 2010 and becomes monthly thereafter. In April 2019, maternity and paternity leave were unified into a single benefit known as *birth and childcare benefit* with periods reserved for each parent

Source: Spanish Statistical Office (annual number of births) and Social Security Statistics (annual number of parental leave takers)

own historical patterns of the series, adjusts for covariates (here, macroeconomic indicators), and accounts for uncertainty through the Bayesian posterior. This approach is particularly useful in settings where a suitable control group is unavailable. By comparing the model-based fertility predictions to actual post-reform fertility, we obtain the potential estimation of the policy's impact.

A critical identifying assumption is that the relationship between fertility and the control time series remains stable post-intervention. This assumption is plausible as long as the control series are not affected by the policy. Under this condition, any deviation between the observed and predicted fertility series following the intervention can be interpreted as the effect of the paternity leave policy. To provide evidence supporting this assumption, we regress the birth count on the set of predictors included in the model, interacting each with a dummy variable indicating the pre- and post-intervention periods. In addition, we compute the correlation between fertility and each predictor separately for the pre- and post-intervention periods. Comparisons of both slopes and correlations across these periods reveal no statistically significant differences, supporting the stability of these associations (Supplementary Figures S9 and S10).

Our choice of BSTS is motivated by its strengths in estimating counterfactual outcomes over time, particularly in our setting of a nationwide, staggered policy implementation, where the absence of a well-defined control group limits the use of traditional synthetic control methods (Abadie *et al.*, 2010). Unlike traditional approaches such as difference-in-differences or regression discontinuity design (RDD), which estimate local effects for units treated shortly around the intervention, BSTS enables us to trace the policy effect over time

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across a broader treated population, any couple giving birth post the intervention. In addition, the method provides uncertainty estimates for the estimated effects.

BSTS has been applied in a range of studies to address causal empirical questions, including the effects of sugar taxes on beverage sales (Kurz and Konig, 2021; Puig-Codina *et al.*, 2020); the influence of programs aimed at reducing avoidable hospital readmissions on elderly population mortality (Papadogeorgou *et al.*, 2023); and the effect of basic income from dividends on crime rates in Alaska (Dorsett, 2020).

3.1 The model

Structural time series models (STS) decompose a time series into underlying components such as trend, seasonality and cycles. BSTS models combine the STS framework with Bayesian methods, which allows for the incorporation of prior information and the explicit modeling of uncertainty in predictor selection. The time series structure of a BSTS model can be generalized by the following pair of equations:

$$y_t = Z_t^T \alpha_t + \epsilon_t \text{ where } \epsilon_t \sim N(0, H_t) \tag{1}$$

$$\alpha_{t+1} = T_t \alpha_t + R_t \eta_t \text{ where } \eta_t \sim N(0, Q_t) \tag{2}$$

Equation (1) is the observation equation, which links the observed fertility indicator y_t to the latent state vector α_t, which comprises a trend, seasonal and regression component. In our case, Z_t^T equals the vector [1 1 x]', and the observation variance H_t is set to constant equal to $\boldsymbol{\sigma}_\epsilon^2$. Equation (2) is the state or transition equation, which describes the evolution of the state vector α_t over time. The matrices T_t and R_t contain a mix of 0 and 1 and parameters that govern the dynamics of the state components. The error terms ϵ_t and η_t are assumed to be independent of all other unknowns. In our specification, we include a local linear trend, a seasonal component and a regression component that incorporates fertility predictors into the synthetic control. The full model specification is provided in the Supplementary Material (Section 3).

The regression component is estimated using Bayesian linear regression, specifically spike-and-slab regression, which imposes a prior distribution on the regression coefficients to retain a few regressors. This approach favors sparse models by retaining only a subset of predictors that significantly contribute to explaining the outcome in the pre-intervention period. The counterfactual fertility rate after the intervention is then a distribution of counterfactual data points under the assumption that no intervention occurred. Because the posterior distribution generally lacks a closed-from solution, we rely on Markov Chain Monte Carlo (MCMC) algorithms to simulate draws from the posterior and compute estimates of the intervention effect and its associated uncertainty (Brodersen *et al.*, 2015).

From the posterior distribution of counterfactual outcomes, we can calculate the posterior probability of a causal effect. Intuitively, this represents the probability that the observed fertility rate after the intervention differs from what would have been expected in the absence of the policy, according to the model and the data. Unlike a traditional *p*-value, which measures the probability of observing data under the null hypothesis, the posterior probability directly quantifies the likelihood that the intervention had an effect, incorporating uncertainty from both the model and the observed data.

3.2 The analysis

We treat each policy reform as a distinct intervention and re-estimate the counterfactual for each, focusing on the first five reforms. The extension to 16 weeks is excluded from the main

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analysis, as the COVID-19 pandemic may have compromised the identifying assumption of a stable relationship between covariates and fertility. The results for the 2020 extension should also be interpreted with caution, as previous evidence documents a large drop in the number of abortions in spring 2020 linked to the first COVID-19 lockdown in Spain (González and Trommlerová, 2024).

Each intervention is dated nine months after the corresponding policy reform under the assumption that any potential effect on fertility would emerge with a nine-month delay. Around each intervention point where the onset of the policy effects is expected, we define a window of 20 months before and after. This window length strikes a balance between capturing sufficient variation in the data to capture underlying structures, providing enough time to observe the effects of the policy and minimizing the risk of overfitting or violating identification assumptions. Additionally, the 20-month span corresponds to the maximum window for which data are available across all reforms, ensuring an equal window length for comparison. In the robustness section, we examine the sensitivity of our results to alternative window choices.

To estimate the impact of each reform, we divide the pre-intervention period in half, using the *first 10 months* as a training sample, and the *remaining 10 months* as a validation sample. The training sample is used to estimate the model parameters and capture the underlying time series structure. The validation sample, which immediately precedes the intervention, is then used to assess the model's predictive performance (Figure 2). This allows us to evaluate how well the model captures the pre-intervention behavior of the outcome variable. We selected the final model based on its performance in the validation period, using the minimization of the root mean squared error (RMSE) as the selection criterion (Supplementary Table S5).

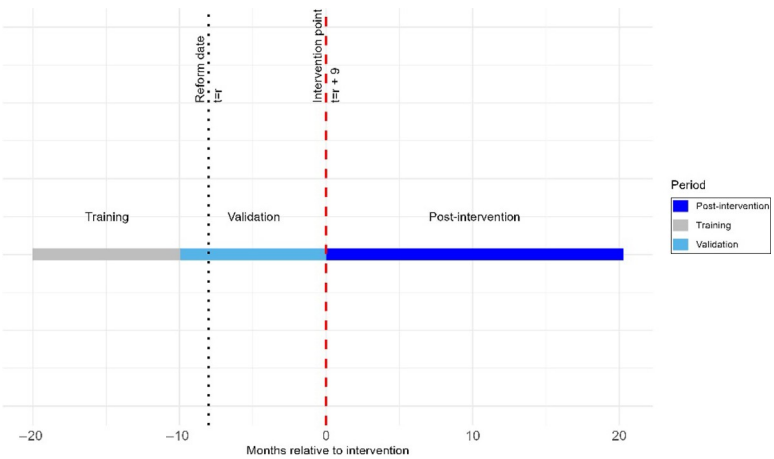

Figure 2. Identification strategy for the paternity leave policy for a representative intervention

Note(s): The reform date is denoted as $t = r$ and indicated by a dotted vertical line, while the intervention point, which is dated nine months after the reform at $t = r + 9$, is marked with a dashed line. The figure shows a 20-month window around the intervention point: the first 10 months are used as a training sample to fit the model, the following 10 months serve as a validation sample to assess model fit, and the post-intervention period is used to estimate the effect of the intervention

Source: Authors' own elaboration

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The BSTS model automatically handles uncertainty regarding the set of auxiliary series to include and how much influence they should exert. The auxiliary variables are used to enhance the identification of fertility behavior and to limit potential confounding. They include fertility predictors related to the cost of living and overall economic circumstances, such as indicators of housing market conditions (Atalay *et al.*, 2021; Dettling and Kearney, 2014; Liu *et al.*, 2020; Malmberg, 2012; Yi and Zhang, 2010), cost living indicators (He, 2018; Maruyama and Yamamoto, 2008), socioeconomic status (Simon, 1969; Weeden *et al.*, 2006), marriage markets (Gutiérrez-Domènech, 2006; Maitra, 2004) and labor market conditions (Adserà, 2010; Alderotti *et al.*, 2021; Sobotka *et al.*, 2011). Posterior inclusion probabilities for each predictor can be found in the Supplementary Material (Supplementary Figure S3).

The sequential implementation of the leave implies that, from the second reform onward, the pre-intervention period may already be partially influenced by earlier policies. In such cases, the estimated impact reflects changes relative to the preceding reform rather than to the complete absence of paternity leave.

All analyses are conducted in R using the *bsts* (Scott, 2022) and *CausalImpact* (Brodersen *et al.*, 2015) packages. Model estimation relies on 1,000 MCMC samples (Supplementary Figure S11), and standard diagnostic checks confirm normality and absence of autocorrelation in the residuals (Supplementary Figures S5–S8).

4. Data

We use administrative birth certificate data from January 2003 to December 2022, provided by the Spanish Statistical Office (INE). This data set covers all births registered in Spain annually and contains detailed information on the month and year of birth, municipality of residence, birth order, delivery characteristics and parents' sociodemographics.

To examine fertility behavior, we restrict the sample to women aged 16 and 44 years and compute the monthly fertility rate, defined as the number of births per 1,000 women in this age group (Supplementary Figure S4). This monthly time series captures fluctuations in fertility while accounting for changes in the size of the childbearing population. Population data comes from the Demographic Statistics issued by the INE.

To feed the identification of the counterfactual, we incorporate a set of socioeconomic, marriage-market and cost-of-living controls highlighted in the literature on fertility determinants. Housing market conditions are captured by the number of residential mortgages and the housing rental price index. Higher housing prices are associated with delayed fertility (Clark, 2012) and lower fertility intentions among renters (Atalay *et al.*, 2021; Liu *et al.*, 2020; Yi and Zhang, 2010), whereas homeownership increases both the likelihood and intention of having children when house prices rise (Atalay *et al.*, 2021; Dettling and Kearney, 2014). We also include the consumer price index as a cost-of-living indicator, reflecting the relative price of a composite of goods compared to child-rearing costs (Maruyama and Yamamoto, 2008). Disposable income serves as a proxy for socioeconomic status. We transformed it into a monthly series using cubic spline interpolation to preserve time trends. Its effect on fertility depends on educational attainment and the phase of economic growth (Simon, 1969; Weeden *et al.*, 2006). All series are obtained from INE.

In addition, we include the monthly number of marriages as a measure of marriage-market conditions, as delays in marriage tend to postpone fertility and both partner availability and societal norms around marriage can shape fertility decisions (Gutiérrez-Domènech, 2006; Maitra, 2004). Labor market conditions, which influence fertility through employment instability and economic recessions that delay childbearing and reduce fertility rates (Adserà, 2010; Alderotti *et al.*, 2021; Sobotka *et al.*, 2011), are captured by the number

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of social-security affiliates from the Spanish Social Security Statistics and by the active population disaggregated by gender from INE. The active population series, also reported quarterly, was converted to monthly frequency using cubic spline interpolation.

The final set of predictors was shaped by practical considerations of data availability (Supplementary Figure S1, Supplementary Table S2). We required indicators consistently reported at monthly (or at most quarterly) frequency and covering a sufficiently long period (from the early 2000s through 2022) to be incorporated into the model. We report descriptive statistics and the correlation matrix between predictors and the fertility outcome variable in Supplementary Material (Supplementary Table S4 and Supplementary Figure S2).

5. Results

We present the estimated effects in Figure 3, with each graph corresponding to a distinct reform. Each graph consists of three panels. The first panel displays the observed fertility rates over time, alongside the predicted counterfactuals fertility rates in the absence of intervention. As shown in the first panel for all interventions, the model provides a good fit to the pre-intervention fertility series, with no gap between the observed and the predicted fertility, indicating the model ability to accurately capture the pre-intervention structure. The second panel presents the point estimates of the policy effect over time. These estimates are obtained by subtracting the observed fertility from the predicted values. The third panel illustrates the cumulative effect of each intervention by summing the point estimates over time.

Following the implementation of the policy reforms, there is no noticeable divergence between the observed and the counterfactual fertility across interventions. However, there seems to be a temporary increase in fertility associated with the two-week intervention (Figure 3). The model predicts an average of 4.3 births per 1,000 women in the absence of father leave, compared to the observed 4.5 births in the post intervention period (Table 1), although the 95%Bayesian credible interval (BCI) does not rule out a null effect for the true policy effect (BCI [−0.18, 0.59]). The effect peaks four months after the intervention, when fertility rises by 0.48 births per 1,000 women (BCI [0.12; 0.81]); this effect, however, dissipates over time, with the gap between observed and predicted fertility narrowing to zero by the end of the study period. The third panel shows the cumulative effect of the two week-policy, which indicates no sustained increase in fertility.

No clear effects are observed neither for the subsequent extensions of paternity leave. For the four-week leave, the observed fertility is slightly lower than the counterfactual, but the estimates are highly uncertain, with Bayesian credible intervals consistently including zero at all points in time (Figure 3). Notably, the decline in fertility observed at the end of 2020 coincides with the impact of COVID-19 pandemic (Cozzani *et al.*, 2023; González and Trommlerová, 2024). Given this decline, the results for the extensions implemented during this period should be interpreted with caution.

As a next step, we examine whether policy effects vary by maternal age, birth order, employment status and educational attainment. These dimensions help to outline profiles of the groups most responsive to the interventions. For each group and for each policy reform, we re-estimate the counterfactual fertility using the same modelling framework.

Maternal age at birth may play a significant role in shaping fertility decisions. Delaying childbearing often aligns with higher educational attainment and career establishment (Nitsche and Brückner, 2020), both of which tend to increase the opportunity costs associated with having children (Cruces, 2024). As such, the availability of paternity leave – which can help redistribute caregiving responsibilities – may be particularly influential for older mothers. Conversely, younger mothers may also

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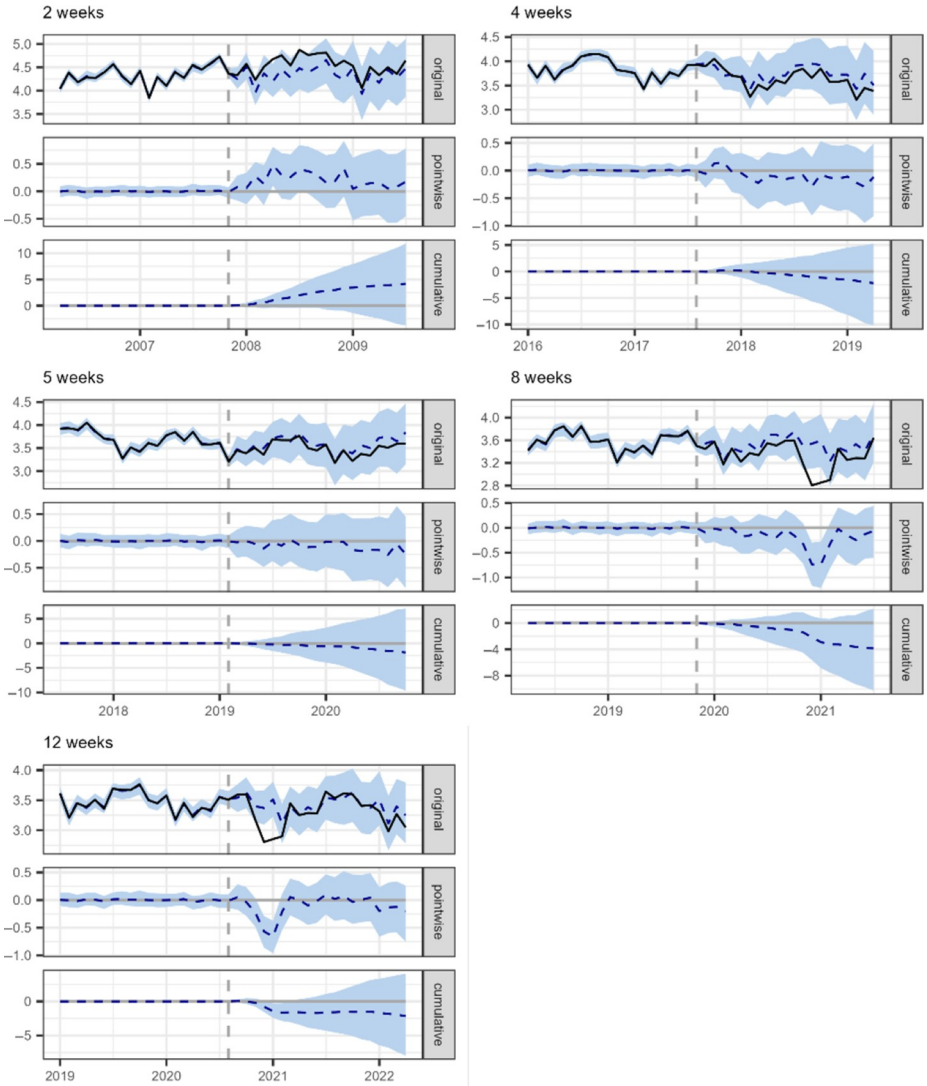

Figure 3. Pointwise and cumulative effect of the introduction of paternity leave on monthly births per 1,000 women aged 16–44

Note(s): In each graph, the top panel displays observed (solid line) and predicted (dashed line) fertility rates with 95% credible intervals (shaded areas). The middle panel illustrates pointwise effects, defined as the difference between observed rates and counterfactual predictions. The bottom panel presents cumulative effects over time. Model selection is based on model performance in the prior period, using root mean squared error (RMSE). Detailed model specification and posterior inclusion probabilities of predictors are provided in the Supplementary Material (Supplementary Figure S3, Supplementary Table S5)

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Table 1. Summary of causal impact analysis

Statistic	All births Average	Cumulative
Observed birth count	4.50	91
Predicted birth count (s.d.)	4.3 (0.19)	86.8 (3.78)
95% CI	[4, 4.7]	[79, 94.6]
Absolute effect (s.d.)	0.21 (0.19)	4.18 (3.78)
95% CI	[−0.18, 0.59]	[−3.66, 11.81]
Relative effect (s.d.)	5% (4.6%)	5% (4.6%)
95% CI	[−3.9%, 15%]	[−3.9%, 15%]
Posterior tail-area probability p	0.12	
Posterior probability	0.88	

Statistic	Third birth mothers Average	Cumulative	Employed mothers Average	Cumulative
Observed birth count	0.35	7.00	5.30	106.10
Predicted birth count (s.d.)	0.32 (0.01)	6.47 (0.25)	2.6 (0.8)	51.5 (15.9)
95% CI	[0.30, 0.35]	[6.0, 6.98]	[0.82, 4.3]	[16.40, 86.3]
Absolute effect (s.d.)	0.03 (0.01)	0.53 (0.25)	2.7 (0.8)	54.6 (15.9)
95% CI	[0.00, 0.05]	[0.02,0.99]	[0.99, 4.5]	[19.79, 89.7]
Relative effect (s.d.)	8.40% (4.20%)	8.40% (4.20%)	3.84% (63%)	3.84% (63%)
95% CI	[0.31%, 16%]	[0.31%, 16%]	[4.70%, 20.70%]	[4.70%, 20.70%]
Posterior tail-area probability p	0.03		0.01	
Posterior probability	0.98		0.99	

Note(s): Estimated effect of the introduction of Two weeks of paternity leave on monthly births per 1,000 women, by maternal group. Each column reports the average effect over time and the cumulative effect over the 20 months following the intervention. Posterior probability refers to evidence of an intervention impact given the observed data and the model

benefit from the policy, as they are more likely to be in egalitarian households with less traditional gender roles (Craig and Mullan, 2011), where partners are more inclined to share caregiving responsibilities from the outset. In our analysis, mothers across most age groups-defined in five-year intervals – follow the fertility pattern observed in the main results, showing a modest potential increase in fertility linked to the two-week paternity leave (Supplementary Figures S12–S16), but uncertainty estimates do not exclude a null effect, except when the policy effect peaks. The absence of effects from the longer extensions remains consistent across all age groups.

New parents may not yet have established gender roles of childcare (Craig and Mullan, 2011), and in this context, institutional factors – such as the availability of paternity leave entitlements – could exert a stronger influence on family dynamics than they do for couples who already have children (Hart et al, 2022). Despite this, first-time and second-time mothers do not exhibit any change in fertility following the two-week intervention. In contrast, third-order births report a pronounced divergence from counterfactual predictions following the first intervention, with the gap persisting through the 20-month post reform period (Figure 3). The observed fertility rate consistently exceeds predictions, with a posterior probability of 0.97 for a causal effect, indicating strong evidence of an intervention impact [2]. On average, third-order births increased by 0.03 per 1,000 women per month (BCI [0.002; 0.049]), representing an 8.4% rise. This translates into approximately half an additional birth per 1,000 women over the 20-month period. There is no observable effect for longer extensions among third- or higher-order births (Supplementary Figures S17–S20).

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Couples in which the mother is employed may reflect more egalitarian gender norms within the household (Bornatici and Zinn, 2025; Fanelli and Profeta, 2021), as well as a greater bargaining power to share domestic and caregiving responsibilities (Yeung *et al.*, 2001). Additionally, for employed women, the opportunity cost of having another child may be reduced if the policy facilitates a more balanced division of childcare between partners (Matysiak and Vignoli, 2007). Together these mechanisms suggest that the introduction of paternity leave could have a stronger effect on fertility among this subgroup. Indeed, a striking difference emerges when the analysis is stratified by maternal employment status. Employed mothers experience a sharp increase in their birth count following the two-week intervention (Figure 4). In the absence of the policy, the predicted number of monthly births per 1,000 women would have been 2.6 births, compared to the observed 5.3 births. This corresponds to an average increase of 2.7 births per 1,000 women (BCI [0.96; 4.5]), effectively doubling the monthly fertility rate. The posterior probability that this represents a causal effect is 0.99. The effect is sustained over time, translating into nearly 54 additional births per 1,000 working women over 20 months. In contrast, non-employed mothers exhibit

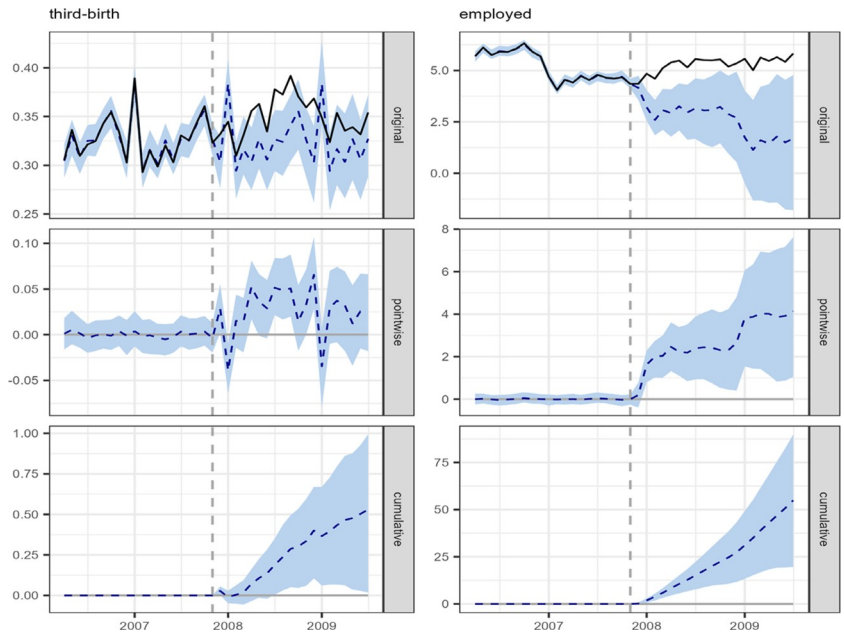

Figure 4. Pointwise and cumulative effect of the introduction of paternity leave on monthly births per 1,000 women aged 16–44, separately for third births (left column) and for employed women (right column)

Note(s): In each graph, the top panel displays observed (solid line) and predicted (dashed line) fertility rates with 95% credible intervals (shaded areas). The middle panel illustrates pointwise effects, defined as the difference between observed rates and counterfactual predictions. The bottom panel presents cumulative effects over time. Model selection is based on model performance in the prior period, using root mean squared error (RMSE). Detailed model specification and posterior inclusion probabilities of predictors are provided in the Supplementary Material (Supplementary Figure S3, Supplementary Table S5)

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no change in fertility (Supplementary Figures S21 and S22). Neither group shows any response to longer leave extensions.

Finally, maternal education can shape how couples respond to paternity leave reforms. Higher level of educational attainment is often associated with more egalitarian gender role attitudes within the household (Boehnke, 2011; Frodermann *et al.*, 2024). In addition, education can influence the perceived opportunity cost of childbearing, particularly among working women, by affecting career expectations and bargaining dynamics within couples (Becker, 1992; Blossfeld and Huinink, 1991; Doss, 2013; Goldin and Katz, 2002). However, the analysis reveals no significant changes in fertility in response to any of the paternity leave reforms for either educational group – those with college education and those without (Supplementary Figures S23 and S24). Note that the educational heterogeneity analysis is performed starting from the four-weeks extension, as data disaggregated by educational attainment are not available for the earlier two-weeks extension.

5.1 Robustness

We assess the robustness of our findings using a range of tests, including sensitivity to alternative window lengths, checks on the timing assumptions, placebo exercises and alternative model specifications.

The main specification uses a 20-month pre/post window, the longest period available consistently across all reforms. To assess whether this choice drives the results, we re-estimate the models using alternative window lengths ranging from eight months, the minimum span required to avoid overlap between consecutive reforms, to two years, which is the maximum period available for the more recent reforms (Supplementary Table S7). Shorter windows minimize overlap with preceding reforms, while longer windows maximize the available information. Across most of the alternative window lengths, the estimated effects remain qualitatively unchanged. This indicates that the findings are not meaningfully sensitive to the specific temporal span used to identify the intervention effect.

We also test the sensitivity of the results to alternative assumptions about the timing of the intervention. Since eligibility for paternity leave benefits depends on the child's birth occurring after a specific cutoff date, we test for potential strategic timing of births around the reform dates. While no such behavior is expected for the initial introduction, which was not announced in advance, later extensions were disclosed with some notice and could have led families to delay delivery slightly to gain eligibility. To assess the presence of such effects, we conduct a sensitivity analysis that re-dates the intervention to the time of the actual reform rather than nine months later. This alternative timing specification produces results that are largely unchanged with respect to the main findings (Supplementary Figure S25), reducing concerns that sorting of births around the reform dates may be influencing our estimates.

A related concern is that the main specification, which defines the intervention nine months after the reform, may fail to capture potential effects on preterm births. To evaluate this, we re-estimate the models using a shorter intervention lag of six months, corresponding to the minimum gestational length observed in our data. Results from this alternative timing specification (Supplementary Figure S30) closely mirror those from the baseline specification, suggesting that effects on preterm births are unlikely to be omitted from the main specification.

In addition, we evaluate whether the model generates artificial effects when no intervention has occurred. We conduct ten placebo exercises by shifting the intervention date to different months within the pre-intervention period, ensuring that each placebo point retains a sufficiently long pre-intervention window for estimating the counterfactual. For

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each placebo run, we compute the estimated pointwise and cumulative effects along with their 95% credibility intervals (Supplementary Figure S26). Across the ten placebo exercises, the estimated differences between observed and counterfactual fertility are negligible. Both pointwise and cumulative effects remain centred around zero, and their credibility intervals consistently include the null effect. Summary statistics across the ten placebo interventions confirm that the average pointwise effect is essentially zero (mean = −0.004, SD = 0.15). This absence of effects following the pseudo-interventions strengthens confidence that the impacts detected in the main analyses are not artefacts of model structure.

Finally, we assess the stability of the estimates with respect to covariate selection and model assumptions (Supplementary Figures S27–S29). Re-estimating the model while excluding individual covariates yields results that remain qualitatively unchanged, indicating that no single predictor drives the findings. We also re-estimate the models using alternative priors and starting values, including less informative priors that allow the model to rely more heavily on the data (Supplementary Table S6). The results again remain consistent with the main specification, reinforcing confidence in the robustness of our conclusions.

6. Discussion

This study uses birth certificates data to study the effect of paternity leave on fertility over time. A natural experiment arises from the staggered implementation of paternity leave in Spain. By March 2007, fathers were granted two weeks of leave to care for newborns, which progressively extended to 16 weeks by 2021 and most recently increased to 19 weeks in 2025. The analysis uses synthetic control methods for time series to construct fertility over time in the absence of the policy across the first five reforms.

Paternity leave policies may influence social norms, family structures (union formation and dissolution) and the division of labor and childcaring within households (Almqvist and Duvander, 2014; Dearing, 2016; Duvander and Johansson, 2012; Hart *et al.*, 2022; Lappegård *et al.*, 2019). These shifts may, in turn, affect fertility decisions, which are the outcome of a household optimization process. Prior research has examined the effect of paternity leave on fertility but has largely focused on local effects, the influence on couples who give birth shortly before or after the policy's implementation. In the Spanish context, earlier findings on subsequent fertility suggest that families having a child around the introduction of the two-week paternity leave tend to take longer to have an additional child, leading to reduce fertility among older couples (Farré and González, 2019. Moreover, only couples with an intermediate wage gap reported a reduced likelihood of having another child (González and Zoabi, 2021). However, the effect of paternity leave policies on fertility in the overall population may differ from findings on subsequent fertility among eligible families. To capture these dynamics, our analysis includes all couples giving birth after each policy change and examines whether policy effects fade out or consolidate over time, vary with characteristics of the mother, and strengthen with successive policy extensions.

Taken together, our findings reveal that paternity leave does not lead to consistent changes in fertility decisions. However, heterogeneous effects emerge for specific subgroups. Employed mothers and those having a third child report positive changes in fertility following the introduction of paternity leave. The birth count of women having a third child grew on average by 0.03 monthly births per 1,000 women, an 8.4% rise, resulting in approximately half an additional birth per 1,000 women over the 20-month following the reform. Employed mothers experience a more pronounced response, with an average monthly increase of 2.7 births per 1,000 working mothers, effectively doubling pre-policy rates and accumulating 57 additional births per 1,000 women after 20 months. We find no consistent evidence of differential effects based on maternal education or age. Importantly, it

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is the initial introduction of paternity leave, not subsequent extensions, that appears to influence fertility behavior.

Our findings align with previous studies that report no overall effect of paternity leave on fertility (Bartel *et al.*, 2017; Cools *et al.*, 2015; Kotsadam and Finseraas, 2011). Consistent with prior research, we also observe heterogeneity in the response to paternity leave by birth order, with third-order births showing positive effects (Duvander *et al.*, 2020; Thomas *et al.*, 2022). Crucially, our study extends previous evidence on delays in subsequent fertility and the lower likelihood of having an additional child among couples who had a child around the introduction of paternity leave in Spain (Farré and González, 2019; González and Zoabi, 2021). Our methodology captures the impact of the policy on the overall population, potentially reflecting a selection effect. Couples conceiving at least nine months after the reforms may not yet have directly experienced paternity leave themselves but could respond to the policy as a signal from the state that lowers the perceived cost of childbearing. By contrast, the delays on subsequent fertility documented among eligibles couples (Farré and González, 2019; González and Zoabi, 2021) concerns families who have already used paternity leave for a previous child, and whose decisions about additional births reflect the realized costs and benefits of leave, including potential preferences for child quality over quantity (Farré and González, 2019). These two effects – population-wide fertility responses to policy signals and within-family adjustments following direct exposure to leave – may capture distinct mechanisms and likely operate among different profiles of couples, which may ultimately lead to divergent findings on fertility.

Employed mothers, who appear to be the ones most affected by paternity leave with a sustained change in fertility, represent roughly half of all women aged 25–44 at the time of childbirth. Among them, nearly one in four identifies work-family conciliation features, such as telework, flexible schedules and holidays arrangements, as the most valued aspect of a job (Instituto Nacional de Estadística (INE), 2018). This suggests that employed mothers may be particularly responsive to policies like paternity leave that support better compatibility between work and family life. Indeed, previous studies characterizing households where fathers take leave highlight that couples in which the mother is employed are more likely to do so (Escot *et al.*, 2013; Moreno-Mínguez *et al.*, 2022). From a bargaining power perspective, employed mothers can use their economic resources to negotiate a more active role for fathers in childcare (Yeung *et al.*, 2001). Moreover, fathers' uptake of leave has been associated with a lower risk of relationship conflict when the mother is employed (Petts and Knoester, 2018), which could plausibly serve as a mechanism influencing fertility behavior.

While these results point toward stronger fertility responses among employed mothers, some caution is warranted when interpreting the heterogeneity analysis. A share of mothers cannot be assigned a working status due to missing or unclassifiable occupational codes – representing 5.51% of birth records. We therefore exclude these cases rather than attempting to impute their employment status. Our examination of these unclassified observations indicates that they more closely resemble non-working mothers across observable characteristics – being younger, less likely to be married, less educated and more likely to be non-Spanish nationals (Supplementary Tables S8–S9). Their exclusion therefore may reduce the size of the non-working group, potentially lowering the likelihood of detecting effects on this subgroup. Future research using data that more accurately capture occupational status would help validate and strengthen the subgroup-specific findings.

The fact that only the introduction of paternity leave, rather than its subsequent extensions, is associated with fertility changes resonates with previous evidence from studies on family leave policies. Prior research shows that policy introductions often alter family outcomes, while later extensions do not. For instance, in Norway, a four-week extension of fathers' leave had no measurable impact on fertility, earnings or family stability (Hart *et al.*, 2022), and in Sweden,

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adding a second "daddy month" did not affect family separation, whereas the initial introduction did (Avdic and Karimi, 2018). Likewise, extending paid maternity leave in Norway from 18 to 35 weeks led to limited changes in completed fertility, marriage, divorce, and child outcomes (Dahl *et al.*, 2016), contrasting with earlier findings showing effects from the initial introduction (Carneiro *et al.*, 2015). These studies suggest that each policy stage may influence a different marginal group of compliers. The initial introduction may have a stronger impact by reducing the first barrier to leave-taking and encouraging behavioral change among a broader set of households, whereas later extensions mainly benefit those who would already have taken leave. Further evidence on the characteristics of these compliers on extensions of the Spanish policy could provide valuable insights.

The absence of a fertility response among childless women is notable, as this group represents approximately 40% of women aged 25–44 in Spain (Instituto Nacional de Estadística (INE), 2021). Despite nearly half of these women report an intention to have a child within the next three years and around 10% identify work-family conciliation as a key barrier (Instituto Nacional de Estadística (INE), 2018), paternity leave appears to have a limited ability to trigger a shift toward first-time motherhood. In contrast, third-order births, which accounted for about 10% of all births (Instituto Nacional de Estadística (INE), 2021), show a positive and sustained response to the policy. However, while statistically significant, the magnitude of this response is small, suggesting that its practical significance is likely limited. Still, examining the effect separately by birth order helps identify household profiles for which fertility behavior may react differently to the policy. In our sample, households with higher-order births (third or more) are significantly less likely to include labor-active parents, dual-earner households, college-educated individuals, or Spanish parents, but they are more likely to be married and have older parents compared with families with lower birth orders (Supplementary Table S3).

It is important to recognize that estimated effects may be context-dependent, influenced by institutional settings such as childcare arrangements and availability, the extent to which gender norms are present in the society and the labor market flexibility in supporting work–family compatibility. Additionally, due to data constraints, we cannot observe actual leave take-up behavior. As a result, any estimated effect would be an intention to treat effect (ITT). Given that between 40% and 60% of fathers take the two-week leave, the actual effect among these who use the leave could be up to twice as large as our ITT estimates. Furthermore, we are unable to assess how the leave is used, whether it is taken simultaneously with the mother or intermittently. Reports indicate that 75% of fathers took leave while the mother was still at home, 20% took it discontinuously in 2019 and 50% in 2021 (Farré *et al.*, 2024). We cannot also disaggregate the specific activities for which the leave is used. Recent research suggests that it may not be exclusively devoted to newborn care and may include leisure activities (González *et al.*, 2024).

We are also unable to compute fertility rates by birth order based on actual parity distributions because data on women's parity-specific population denominators are unavailable. Instead, we rely on rates computed over the population of women of childbearing age, regardless of past births (Lanzieri, 2013). This approach likely produces conservative estimates, as the true parity-specific populations would be smaller, potentially leading to larger effect sizes. Finally, our analysis does not cover the 2021 and 2025 reforms that equalized paid leave for both parents at 16 and 19 weeks. Future research should examine whether this more generous and gender-symmetric design has renewed potential to influence fertility.

Finally, potential confounding from concurrent events, such as the economic downturn and other policies, deserves consideration. Economic hardship and labor market uncertainty can delay or alter fertility decisions (Adserà, 2010; Sobotka, Skirbekk, and Philipov, 2011;

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Hofmann and Hohmeyer 2012; Schmitt 2012), particularly during the Great Recession (Ayllón, 2019; Comolli, 2017; Goldstein *et al.*, 2013; Schneider, 2015). To account for these effects, we include macroeconomic indicators, and the stable pre- and post-intervention relationship between fertility and economic variables (Supplementary Figures S9 and S10) suggests that at least part of this confounding is likely captured by these regressors. Similarly, concurrent policies, such as Spain's baby bonus, which provided a cash incentive for births after 1 July 2007, positively influenced fertility (González, 2013). As the policy was not announced in advance, conceptions in response would have begun in early July, leading to births from April 2008 onward. Since our estimated paternity leave effect appears from October 2007, this earlier period can be considered largely unaffected by the baby bonus, although caution is warranted when interpreting cumulative effects.

In conclusion, this study finds no evidence of an overall effect of paternity leave on fertility rates. However, employed mothers and women having a third child show a positive and sustained response linked to the introduction of the leave. Notably, this effect appears to be concentrated around the initial implementation, with no measurable impact from subsequent extensions. These results from a quasi-experimental framework, contribute to a broader understanding of how paternity leave influences fertility decisions over time in the overall population. The absence of an aggregate effect suggests that paternity leave on its own is unlikely to be sufficient to raise fertility rates and should instead be complemented by other family-support policies, such as subsidized childcare (Haan and Wrohlich, 2011; Mörk *et al.*, 2011; Rindfuss *et al.*, 2010), financial incentives for additional children (Cohen *et al.*, 2013; Laroque and Salanié, 2013) or improved workplace flexibility (Ariza *et al.*, 2005). At the same time, the positive responses observed among specific subgroups indicate that fertility promotion efforts may be more effective if strategically targeted toward these groups. Finally, the lack of a response among childless women underscores the need for additional measures beyond paternity leave to address the barriers to first-time motherhood. These insights are particularly relevant in the Spanish context, where fertility rates remain well below both the EU average and the replacement threshold.

Notes

[1.] Fathers had to be employed (or actively seeking employment). No affiliation period was required for fathers younger than 21. Fathers aged 21–25 needed at least 90 days of contributions in the last 7 years before the leave or 180 days over their entire working history. Fathers aged 26 and above required at least 180 days in the last 7 years before the leave or 360 days over their entire working history (BOE, 2007).

[2.] The posterior probability represents the likelihood that the observed fertility rate after the intervention differs from what would have been expected in the absence of the policy, given the model and the data. It is derived from the posterior distribution of counterfactual outcomes and provides a direct measure of confidence in the intervention's effect, incorporating uncertainty from both the model and the data.

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Supplementary material

The supplementary material for this article can be found online.

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