# When are we having the second? Effects of paternity leave extensions on the decision to have a second child[*]

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June, 2024

## Abstract

In a context of low fertility rates in developed countries, some governments are adopting family policies in an attempt to encourage childbearing. I exploit a set of three paternity leave extensions in Spain from 2017 to 2019 to analyze whether this kind of policy affects second order fertility decisions. For that purpose, I use a rich administrative dataset of birth statistics provided by the Spanish National Statistical Institute and employ a regression discontinuity difference-in-differences approach. I compare the trends in birth spacing between the first two children of women who had their first infant near each reform, to those who had them during the same period in non-reform years. In line with some previous literature, the results do not show any generalized response of mothers in the time they take to have their second child, which suggests that Spain may have entered a new phase in which policies that break gender norms do not have, at least, a negative impact on fertility. Moreover, the analysis of heterogeneous effects contributes in understanding possible mechanisms through which family policies could alter fertility decisions.

**Keywords:** Child spacing, gender, fertility, paternity leave, regression discontinuity.
**JEL classification:** J13, J16, J18

# 1 Introduction

Demographic changes are one of the main challenges of contemporary society. Spain in particular has one of the lowest fertility rates among developed countries, which, combined with being one of the countries with the highest life expectancy, is resulting into a population ageing process. If the trends are not reverted on time, in the next decades Spain could be facing serious consequences that could put the quality and sustainability of its welfare state at risk. That is why the different governments that the country has had during recent times have introduced some policies that focused on improving the well-being of families with children. Specifically I focus on the extension of paternity leave here.

Since 2017, Spain has passed a set of reforms that extended its paternity leave from 2 to 16 non-transferable weeks, equalizing it to the length of maternity leave. That is currently the longest paid leave reserved to fathers among OECD countries, and the 9th if we consider permits shareable between both parents (OECD, 2024c), which states the country's strong bet on policy-making as a means to promote gender equality at home.

In this paper I analyze the subset of three paternity leave extensions and reforms that happened in Spain between 2017 and 2019, to evaluate whether they could have impacted second order fertility. To do so, I employ a regression discontinuity difference-in-differences (RD-DiD) design in which I assess how the reforms have impacted the time that parents just affected by each extension took to have their second child after the first one. The main assumption is that seasonality in child spacing would have remained equal to previous years in the absence of treatment. The choice of birth spacing as outcome variable is due to previous findings in the literature that pointed the postponement of childbearing to cause a decrease in completed fertility (Berrington et al., 2015; Tomkinson, 2019; Beaujouan et al., 2023), specially for women who have their first child at a higher age, which states the relevance of conception timing to predict total fertility.

My results show that the extensions of paternity leave did not have any significant generalized effect on the decision to have a second child, at least in the short and medium-run. Nonetheless, the analysis of heterogeneous effects provides some insights on the mechanisms through which paternity leave might impact subsequent fertility decisions. Those results contribute in the area of knowledge explaining the causal impact of father involvement in childcare and family-friendly polices on fertility.

The remainder of this thesis is structured as follows: Section 2 describes the situation of fertility in Spain in the global context. Section 3 summarizes the previous knowledge by the literature on the effects of family policies on fertility. Section 4 describes the institutional

setting of the policy I evaluate. Then, the data and empirical strategy used in the analysis are explained in sections 5 and 6 respectively. Results, robustness checks and heterogeneous effects are presented in section 7. Finally, section 8 concludes.

## 2 The context of current fertility in Spain

In developed economies, there has been a general decreasing trend in fertility rates during the last decades, with only a few exceptions. While in 1970 the OECD average fertility rate was of 2.84 children per woman, by 2021 it had decreased to 1.58 (OECD, 2024a). That is below the 2.1 replacement level, which is the birth rate that ensures a broadly stable population in developed countries, assuming no migration and unchanged mortality (Craig, 1994). Spain, the country we focus on here, currently ranks as the country with the second lowest fertility rate among the OECD-38. In particular, it went from 2.90 to 1.19 live births per woman during the 1970-2021 time-span, though it has been rather stable for the last 30 years, with low values ranging from 1.16 to 1.45 (OECD, 2024a), i.e. below the replacement rate, as Figure 1 illustrates.

Figure 1. Evolution of Total Fertility Rate (1970-2021)
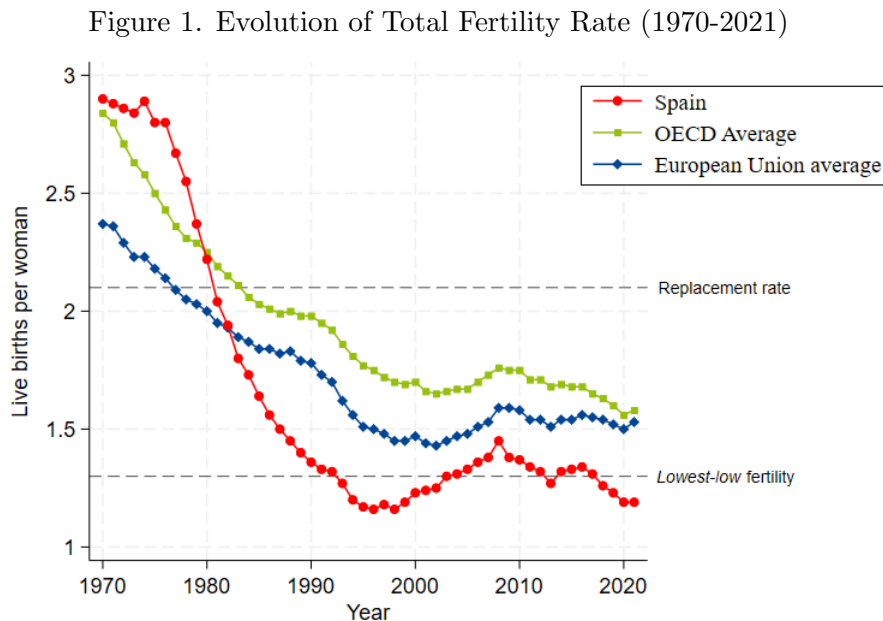


*Source:* Own elaboration with OECD data (OECD, 2024a)

In parallel, due to the improvement of medicine knowledge and health conditions, life expectancy at birth has been expanding through the last decades worldwide. In Spain it has gone from 72.0 years in 1970 to 83.2 in 2022 (OECD, 2024b), peaking at 84.0 years in 2019, before the COVID-19 pandemic. Currently, the country has the longest life expectancy from the European Union, and one of the highest among the OECD-38. Altogether with the Spanish

*lowest-low*[1] fertility, the situation is resulting into a population ageing process that is reverting the population pyramid (see Figure 2). Namely, there are progressively more elderly population than youth and middle-aged.

Figure 2. Spain population pyramid.

(a) January 2023                      (b) 2070 Projection
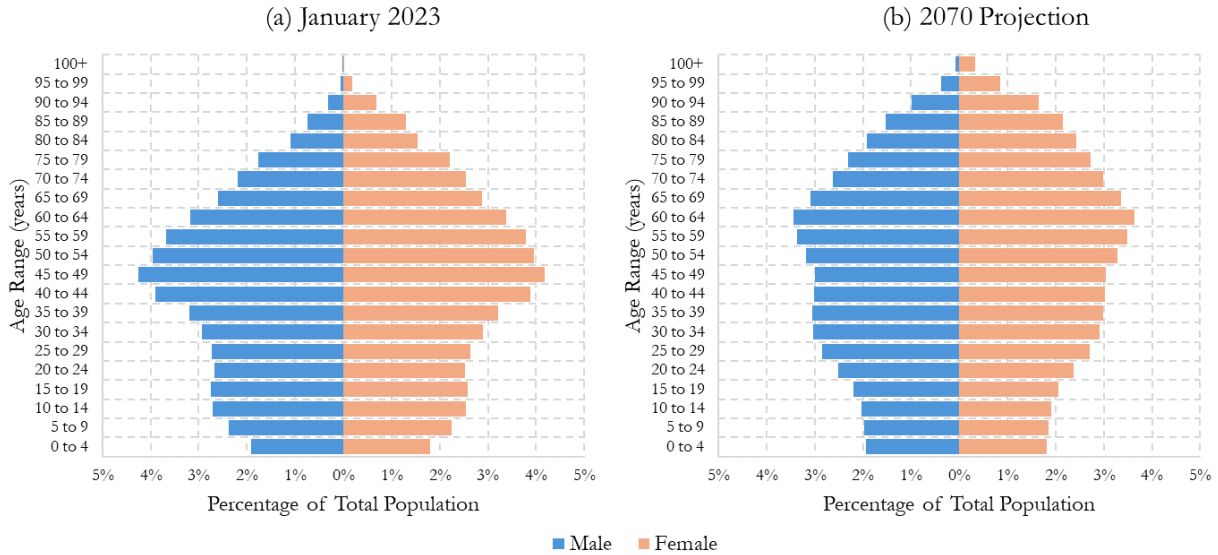


*Source:* Own elaboration with data and projections from the Spanish National Statistics Institute (INE).

These trends have raised concern on the sustainability of the welfare state. In particular, there is the risk of having to face issues such as a threatened public pensions system, that will have to go through several reforms in order to ensure its continuity (Bongaarts, 2004; Nickel et al., 2008), or a shortage of support and care workers that might not be able to meet the demand of an ageing population (Valls Martínez et al., 2021; Ismail et al., 2021), specially in rural regions, where the process is being accelerated due to the emigration of young adults to urban areas[2]. Moreover, the European Commission and Directorate-General for Employment, Social Affairs and Inclusion (2023) has pointed the decrease in working age population[3] to be a major trend affecting current labour shortages, already perceived across all skill levels. Hence, there is the need to understand what are the key drivers of the decrease in fertility and which policies can be implemented to revert the trend.

According to the literature exploring the reasons why couples are having less children, a decline in the willingness to have them does not seem to be the main driver, since women desire more children than they actually have (Kohler et al., 2002; Goldstein et al., 2003; Puig-Barrachina et al., 2020). Instead, some academic publications have focused on the postponement

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[1]Term defined in Kohler et al. (2002) as a Total Fertility Rate under 1.3.

[2]To deepen on rural exodus of young people and depopulation see, for example, Battino and Lampreu (2019) or Llorent-Bedmar et al. (2021).

[3]Defined as 20 to 64 year-old population.

of childbearing as a one of the major causes of the decrease in fertility. The idea is that an older age of parents at first and subsequent births would be associated with a lower complete fertility, as they cannot fully recover that of parents who have their first children younger (Berrington et al., 2015; Tomkinson, 2019; Beaujouan et al., 2023). At the same time, the delay in parenthood and fertility decrease would be explained principally by economic and social changes, including increasing returns to education (Kohler et al., 2006), higher opportunity costs of having a child due to the incorporation of women to the labor market (Feyrer et al., 2008), economic uncertainty and the housing affordability crisis (Japaridze and Sayour, 2024), and changes in social norms (Kohler et al., 2006; Ciganda and Villavicencio, 2017; Lebano and Jamieson, 2020).

## 3 The effects of family policies on fertility decisions

In countries like Spain, incoming fluxes of migration are being helpful to partially cope with the low-fertility problem in the short-term, but they might not be enough to avoid a demographic crisis in the future. In this environment, government policies are key to enhance childbearing and have a positive impact on population structure. To this extent, many countries have introduced family policies that focus on the well-being of families with children. Although they do not often have specific aims regarding population size or ageing, but instead, target other goals such as improving maternal labor market prospects or enabling the balance of work and family, many authors have used the quasi-experimental settings they supply to analyze their effects on fertility. Hereby I outline some relevant findings from the literature.

A first group of family polices comprehends all those related to the cost of childcare. Overall, policies that subsidize early childhood education and care have been found to have a positive impact on fertility (Bauernschuster et al., 2015; Olivetti and Petrongolo, 2017) or not have any effect at all (Bick, 2016; Nollenberger and Rodríguez-Planas, 2015). This suggests that, even though this kind of policies have the potential to foster birth rates, their effectiveness is likely to depend on the context.

Secondly, there is also research on how financial incentives based on the presence of a child, in the form of subsidies or tax credits, impact fertility. General findings show that universal child subsidies naturally increase fertility (González, 2013; González and Trommlerová, 2020; Sorvachev and Yakovlev, 2020), but at the cost of a negative impact on maternal labor supply, due to their income effect (González, 2013; Schirle, 2015; Iga et al., 2020; Asakawa and Sasaki, 2022), which consequently further increases the gender gap on labour market outcomes. On the other hand, tax credits conditional on employment increase both fertility and women labor supply simultaneously (Haan and Wrohlich, 2011; Bastian, 2020; Bastian and Lochner, 2022).

Finally, a third set of family policies consists of the introduction of parental leave licenses, which are defined as paid work permits that parents can take after childbirth while keeping the right to go back to their workplace after the licence ends. They are based on the need to compensate the temporal increase in housework that parents get after having a child, due to care responsibilities.

Parental leave vary on the direct beneficiary, their length and whether they are paid or not. Accounting for those differences is necessary to correctly interpret their impact on parents' labor market outcomes, gender equality at home, father involvement on childcare and fertility, which are usually interconnected. Depending on who is the direct beneficiary of the leave, we can identify three types of policies:

- Maternity leave, which is the permit granted exclusively to the mother and, thus, allocates the responsibility of childcare after birth on them.

- Family parental leave, given at the household-level in the form of transferable permits.

- Paternity leave, which I focus on in this study, is a non-transferable permit assigned to the father, promoting gender equality in childcare.

Taking this into consideration, several research papers have explored the causal impacts of those policies on the outcomes previously mentioned. Malkova (2018) finds that a partially paid maternity leave in soviet Russia increased childbearing during the ten-year duration of the program. Raute (2019) more specifically observes that an increase in the benefits of a maternity program in Germany affected positively the birth rate of higher-earning women, who bear the highest opportunity costs of having a child, potentially changing the socioeconomic composition of fertility. Nonetheless, maternity leave also seem to increase the motherhood penalty on earnings and employment, at least in the short term (Schönberg and Ludsteck, 2014; Bergemann and Riphahn, 2023). Regarding family leave at the household level, Lalive and Zweimüller (2009) explore an Austrian reform that expanded a transferable parental leave from one to two years. They find that roughly only mothers took up the leave (scarcely less than 1% of the fathers took it), so the effects of the policy were the same as those found for maternity leave, even though it is considered an equality-enabling policy. Evaluating California's Paid Family Leave Program, Baum II and Ruhm (2016) agree that, when given a transferable parental leave, virtually only women take the leave. In the Californian context, however, the permit had a positive short-term effect on mother employment due to the job continuity right provided by the program.

Hence, in order to enforce father involvement in childcare responsibilities, the governments of some developed nations have been establishing non-transferable take-it-or-leave-it paternity leave for fathers, the so-called "daddy quotas". The exclusive reservation of some paternity leave time has been documented by some papers to be have large effects on take-up in the US, Norway, Sweden and Quebec (Bartel et al., 2018; Cools et al., 2015; Ekberg et al., 2013; Patnaik, 2019). A number of rigorous empirical studies have evaluated the impact of such paternity quotas on a variety of outcomes. However, evidence on the field is quite mixed. Many authors associate them with a more equal distribution of unpaid housework time within the household, due to an increase of father involvement in childcare activities (Almqvist and Duvander, 2014; Bünning, 2015; Huerta et al., 2012; Tanaka and Waldfogel, 2007; Farré and González, 2019; Patnaik, 2019), while some others find no effects (Ekberg et al., 2013; Kluve and Tamm, 2013). Furthermore, paternity leave are generally found to enforce short-run reductions of the gender gap in parenthood labor penalty among parents (Bünning and Pollmann-Schult, 2016; Tamm, 2019; Byker, 2016), whereas evidence is mixed on their long-run causal effects (Cools et al., 2015; Lalive and Zweimüller, 2009; Ekberg et al., 2013; Kluve and Schmitz, 2018; Dunatchik and Özcan, 2021).

There is also some mixed evidence from recent empirical studies on how paternity leave may affect fertility choices. Cools et al. (2015) find no evidence of altered fertility due to paternity leave extensions in Norway. Carnicelli (2024) also shows that 2-week extensions of paternity leave in Finland had no effect on childbearing. However, he finds that a reform in 2001 that made the Finnish paternity leave more flexible improved fertility of women below the age of 30. He shows that the reform decreased birth spacing of subsequent fertility in that group and increased the probability of having another child in, at least, 2 to 5 years after. On the contrary, Farré and González (2019) identify that the introduction of a two week nontransferable paternity leave in Spain in 2007 postponed and decreased subsequent fertility. They show two possible mechanisms that could have caused such reaction. Firstly, they point that greater father involvement due to the father quota lead to a reduction in men's reported desired fertility, which could be due to increased awareness on the costs of having a child. Secondly, they find that the reform had positive effects on maternal employment, which would have increased the opportunity costs of having another child. Similarly, Lee (2022) document that South Korean fathers who take longer leave are less likely to report intentions for another child and Fontenay and Tojerow (2020) argue that a 7-day paternity leave extension in Belgium in 2002 increased birth spacing between the first two children of treated couples.

Those results diverge from some previous literature that examines the relation between

working mother support in childcare and subsequent fertility. Actually, investigating why women in Denmark are more likely to have a second child than Spanish mothers, Brodmann et al. (2007) conclude that the reason is that Danish women can reconcile motherhood and career more easily, because they get greater welfare state support and father involvement in child-rearing than the Spanish. Fanelli and Profeta (2021) also point on the importance of gender symmetry in child care on fertility decisions. While these results might seem altogether incoherent, De Laat and Sevilla-Sanz (2020) find that, within countries, families with more egalitarian attitudes enhance higher mother labor market participation at the cost of having less children. On the other hand, due to the presence of social externalities, countries with more egalitarian attitudes allow for higher fertility rates overall. Feyrer et al. (2008) propose an interesting hypothesis that explains the evolution of fertility in developed economies through three distinct phases. In a first phase, until the 20th century, women would specialize in housework and fertility would be high. Then in a second phase, they start accessing the labor market while still solely holding the burden of childcare, so fertility decreases as a result of the increased opportunity costs of childbearing. Finally, as a result of women's labour outcomes (almost) equalizing those of men, greater father involvement in household production would reduce mothers' disincentive of having children, thus improving total fertility with respect to the intermediate phase. The hypothesis is consistent with the idea that Spain and other high-income countries would be described by this intermediate low-fertility phase. Thus, in this context, an exogenous increase in father involvement on childcare within a family, such as the one magnified by paternity leave, would result into lower subsequent fertility, because of an improvement in maternal labor market prospects, as Farré and González (2019), Lee (2022) and Fontenay and Tojerow (2020) demonstrate. Nonetheless, the hypothesis and some evidence previously cited support that policies that encourage a more equal society, including daddy quotas, will lead these countries to a higher fertility context in the long-run, such as the one that Scandinavian countries experience, due to consequent changes in gender norms regarding the burden of childcare[4].

In short, as Hupkau and Ruiz-Valenzuela (2022) conclude, it appears that family policies that make it easier to combine work and family for women help in raising fertility at the same time as reducing motherhood penalty, specially if they are paired with changes towards more equal gender roles. In the particular case of the paternity quotas, their effects are contingent on whether their ability to change fathers' long-term childcare involvement is enough to overcome the opportunity cost that arises from mothers' better comeback to work and the shift in fathers'

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[4]In fact, Farré and González (2019) already account for the fact that the time spent in childcare differs to a great extent between mothers and fathers in Spain, which implies that their results might not extrapolate to other contexts where gender norms are less established.

fertility preferences. My research improves the existing literature by providing empirical evidence that supports that hypothesis, through the analysis of heterogeneous effects by mother's level of education. It also contributes by assessing in which fertility context Spain is at the present, and how further family policies like the paternity quota might impact fertility in the near future.

## 4 Institutional setting

### 4.1 Reforms of the paternity leave in Spain

Between 2017 and 2021, Spain passed a set of paternity leave reforms that equalized the permit exclusive to new fathers to those of mothers. Since 1989, Spain has granted mothers with a job-protected 16-week paid leave. At the beginning, 6 weeks were compulsory after the birth and the 4 last weeks could be transferred to the father, who only had a 2-day paid job absence after the birth reserved for them (BOE, 1989). In 1999, a new law reduced non-transferable maternity leave to the 6 compulsory weeks after birth, allowing the other 10 weeks to be taken by the mother, the father, or be shared between both (BOE, 1999). This new possibility gave more flexibility to couples' preferences, who could choose to distribute the parental leave between them as they wished and enjoy the 10 weeks simultaneously or subsequently. Nonetheless, in practice a very low number of parents decided to make use of that right (Farré and González, 2019), as most mothers kept consuming the entirety of the 16 weeks of parental leave.

In March 2007, the central government introduced the first 2-week exclusive paternity leave, which consisted on the 2-day post-birth paid absence that fathers already had, and a new take-it-or-leave-it 13-day fully compensated leave that they could enjoy at the same time as the maternity leave period or right after it. To be entitled for the permit, it was enough to be affiliated to the Social Security and to have worked at least 180 days during the previous 7 years. Farré and González (2019) found that the reservation of some exclusive leave time to the fathers had a positive effect on their involvement in housework, which decreased motherhood labour penalty. However, they show that the policy overall had a negative effect on subsequent fertility, through two identified mechanisms. Firstly, the improvement of maternal labor prospects associated to the policy raised the opportunity cost of having another child. Secondly, the salience of the effort needed in childcare for fathers decreased their desire to have more children.

Since then, the duration of the leave remained unchanged until ten years later, when the government started passing a set of reforms that extended paternity leave in Spain to the 16 weeks there are today. The first paternity leave extension came into force on the 1st of January 2017, doubling its duration from two to four weeks. It was still exclusive to the father and had to be taken uninterruptedly before the child's first birthday. On the 5th of July 2018, the permit

was extended an additional week. The main novelty was that the first four weeks had to be taken immediately after the birth, while, for the first time, the law allowed fathers to split the permit and enjoy the fifth week whenever they desired until the minor became 9 months old. Another innovation is that the fifth week could be taken in full or part-time (BOE, 2018).
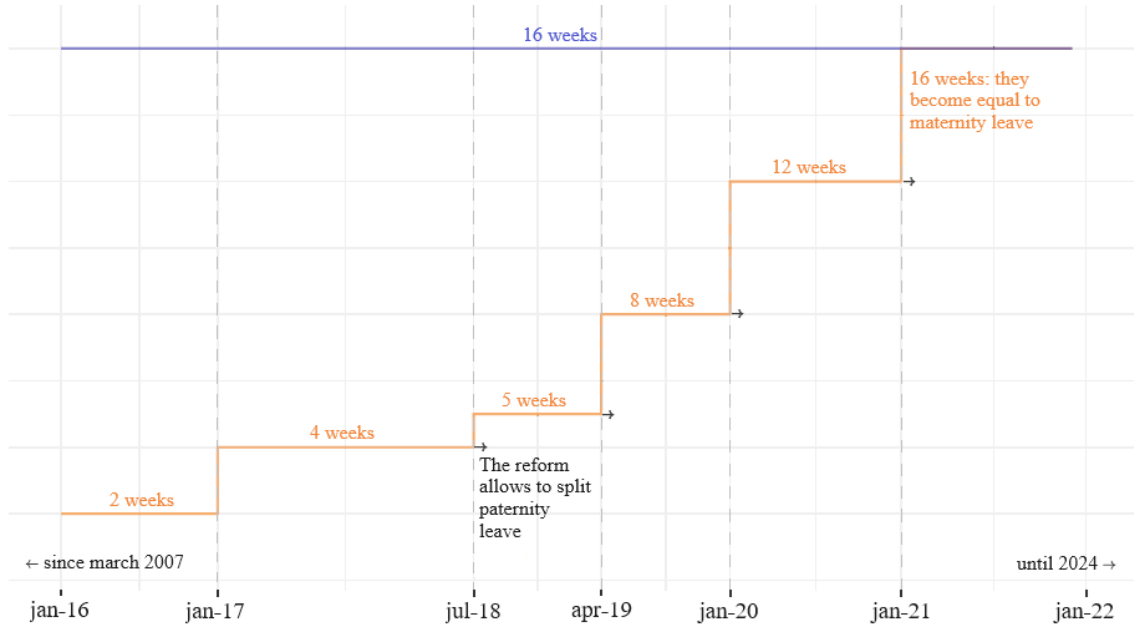
The 1st of March 2019, the Spanish government passed the Royal Decree-Law 6/2019 of "urgent measures to guarantee equal treatment and opportunities between women and men in employment and occupation" (*Ley de medidas urgentes para garantía de la igualdad de trato y de oportunidades entre mujeres y hombres en el empleo y la ocupación*, as officially in Spanish) which introduced a new birth and childcare subsidy in substitution of the previous maternity and paternity leave[5] (BOE, 2019). The law announced three new subsequent extensions of the permit reserved to fathers. On the 1st of April 2019, the leave would be extended to 8 paid weeks, from which 2 had to be taken just after the birth, and the other 6 could be enjoyed, full or part-time, whenever the father desired until the infant became 12 months old. Additionally, mothers could transfer up to 4 weeks of their maternity leave to their partner. On the 1st of January of 2020, the paternity leave would reach 12 weeks of length, from which the first 4 would be mandatory after the birth, and the other 8 could be taken separately, full or part-time until the child's first birthday. By 2020 mothers would still be allowed to transfer two weeks from the maternity leave to the father. Finally, from the start of 2021, both maternity and paternity leave would become completely equal, making Spain be the first country to accomplish that. Particularly, they would consist of non-transferable 16-week permits, from which each parent would have to take a mandatory 6-week uninterrupted leave after the child's birth, while the other take-it-or-leave-it 10 weeks would be allowed to be spent in blocks of at least one week until the infant became 12 months old, full or part-time. It has also been announced that the current government intends to extend maternity and paternity leave to 20 weeks each, without a specific date at the time of this study. Figure 3 illustrates the main changes that happened with each paternity leave reform since 2017.

## 4.2 Take-up

To analyze paternity leave effects, it is key to describe how fathers made use of it after each reform. One of the best features of Spanish maternity and paternity leave is that parents earn 100% of their gross salary while they are on permit, but since the subsidy is tax-free, they actually get a higher net pay if they use it (except very high earners who have a net salary over

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[5]For unification of the terminology used in this paper, I keep referring to the permit introduced by the birth and childcare subsidy to the mother and the father as *maternity* and *paternity leave*, respectively.

Figure 3. Evolution of paternity and maternity leave in Spain.

the maximum subsidy of 4,070.10€ per month). This characteristic makes the leave economically profitable, which is a good incentive for both parents to take it in most cases. Moreover, the first 6 weeks are mandatory immediately after the birth for both parents since the 2021 reform, which ensures a minimum universal take-up. Farré et al. (2024) analyze in detail how its use has evolved during the last decade. The most important points they show are the following.

Mothers take 16 weeks of maternity leave during all the period analyzed, suggesting that the reforms do not affect them, as they use all available leave and do not transfer any week to the father. About the proportion of fathers who take at least one week of paternity leave, around 63% did between 2016 and march 2019, while the introduction of some mandatory weeks in 2019 increased that share to an average 71% until January 2022. The mean duration of paternity leave follows almost perfectly the evolution of the length of the permit reserved for fathers, regardless of the distribution of mandatory and non-mandatory weeks, and less than 5% of them use it part-time, which suggest that fathers who take the leave spend all available weeks by law.

While the possibility of splitting paternity leave was firstly available with the 2018 reform, fathers started using it after the April 2019 reform, as the number of weeks that could be taken any time until the minor became 12 months old increased from one to six. The data also exhibit how the option popularized rapidly, as by 2020 around 50% of them split the leave. They key point is that, considering only fathers who make use of this option, they enjoy the non-mandatory

period after birth in non-simultaneous weeks with the mother's leave, which demonstrates that families learnt to use that possibility as a means to extend the total time that the minor was in care of one of the parents. It is also important to highlight that less than 5% of mothers split their leave, implying that they would go back to work before fathers did, which is something that may have had long lasting effects on father involvement in childcare.

## 5 Data

My analysis is based on a detailed administrative dataset that covers the full birth history of the universe of females who gave birth in Spain from 2013 to 2022 – in total 3.818.974 births, about 380,000 per year –. The information comes from the Statistical Birth Bulletin issued by Civil Registry and is provided by the Spanish National Statistical Institute[6].

For each birth there is information on the month and year of the birth in question, as well as for the previous child of the same mother, if any. The dataset also discloses the birth order of the child and the mother's total number of children born alive during her life, which allows to identify second-time mothers (who represent about 76% of non first-time mothers). Additionally, it also provides a rich set of characteristics of the born child, the previous child, the mother and the father at the time of the second birth. For the birth in question, there is information on the infant's gender, citizenship, province of birth and some health indicators (such as weight, gestation time in weeks, born at a hospital or not, natural birth or C-section and dummy taking value 1 if the baby lived more than 24 hours). From the previous child, apart from their month and year of birth, the dataset also specifies their place of birth, as well as their nationality. Finally, there is also information reported on mother and father's age, month, year and place of birth, citizenship, place of residence, civil status, years of stable relationship, level of studies and occupation area, all at the time of the second childbirth.

As I am interested in determining the impact of the Spanish paternity leave reforms on time spacing between the first and second child of a mother, I restrict my sample to second-time mothers whose first child was born in Spain or holds the Spanish citizenship, in order to avoid including child-rearing experiences of parents who were not subject to the policy (which account about 4% of the sample). Likewise, I exclude single mothers from the sample, since their paternity leave rights are not clear[7]. To do so, I exclude all observations for which no father

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[6]The full dataset is publicly available at https://www.ine.es/uc/lLE3tBxy.

[7]As in the case of single mothers there is no father, there have been some reported sentences that allowed single mothers to accumulate paternity leave rights to those they already get from their maternity leave, while some others have been denied that right. Moreover, since these additional leave weeks cannot affect the mechanisms that are usually considered (father childcare involvement and desired subsequent fertility), it is best not to include them in the final sample.

characteristics are reported for the second child ($<0.5\%$ of the sample). In spite of that not being a complete indicator on whether the first child was raised solely by the mother, it ensures that at least the second one had a recognized father, which is a reasonable proxy. Finally, with the aim of using the same exact sample across different specifications, I restrict the sample to those observations that provide all the characteristics I use as controls in my analysis (about 86% of the sample)[8].

After the previously mentioned filtering, my final database consists of 415,205 women residing in Spain who had their first two children between 2013 and 2022, whose first baby was also born in Spain or holds Spanish nationality and from whom, at least the second child, had a recognised father. In Table 1 I present some descriptive statistics of the variables I use in the analysis, during the reform years that I evaluate and its predecessor.

Some limitations that arise from the level of detail provided by the database must also be considered. Since the only information on the date of birth of each child and its predecessor made public are the month and year that they were born, I can only measure birth spacing as the difference between the month-of-birth of one another, but I cannot obtain the measure in days, which would provide more precise estimates. Besides, due to this same issue, I need to assume that all fathers whose children were born in July 2018 were given five weeks of leave, even though the reform came into force on the 5th, meaning that those who had their children during the first four days of the month had a four-week long paid paternity leave[9].

## 6 Empirical strategy

I exploit the sharp introduction of a series of reforms and extensions of paternity leave in Spain to evaluate its effects on fertility, focusing on the time spacing between a mother's first baby and the second, which is a recognized good predictor of overall completed fertility in the literature. By simply looking at Figure (4), it is noticeable how there is a strong seasonality in child spacing on the years previous to the reforms. In other words, without any type of intervention, the time that mothers take to have a second child depends on the time of the year that their first child is born. This could be explained by the fact that there is a strong heterogeneous demand for season of birth, as documented by Buckles and Hungerman (2013) and Clarke et al. (2019), that

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[8]As an additional robustness check, I checked that the estimates of my main table of results were similar if, instead of dropping observations with missing covariates, I replaced them with the mean value of the sample (or an extra value for categorical variables) and added a dummy variable indicating whether an observation had some missing characteristic replaced. Those estimates, reported in Appendix Table A1, do not diverge much from the ones reported in my main table of results (Table 3), which indicates that dropping those observations lead to a sample that was still representative of the whole population.

[9]Nonetheless, I report in my robustness checks a "donut" type regression where I exclude June and July 2018 observations, and the results stay equivalent

## Table 1: Descriptive statistics

|  | 2016 | 2017 | 2018 | 2019 |
|---|---|---|---|---|
| **Panel A: Outcome variables** | | | | |
| Child spacing during the 16 first months | 13.877 | 13.927 | 13.835 | 13.925 |
| | (1.845) | (1.791) | (1.899) | (1.873) |
| Child spacing during the 25 first months | 20.076 | 19.892 | 19.958 | 20.156 |
| | (3.878) | (3.881) | (3.930) | (3.819) |
| Child spacing during the 40 first months | 28.162 | 27.937 | 28.021 | 24.245[†] |
| | (7.450) | (7.563) | (7.373) | (5.557) |
| **Panel B: Mother characteristics** | | | | |
| Age at first birth | 30.597 | 30.749 | 30.918 | 30.831 |
| | (5.031) | (5.102) | (5.263) | (5.577) |
| Obtained higher education | 0.445 | 0.460 | 0.487 | 0.481 |
| | (0.497) | (0.498) | (0.500) | (0.500) |
| Born in Spain | 0.858 | 0.851 | 0.842 | 0.827 |
| | (0.349) | (0.356) | (0.365) | (0.378) |
| Married | 0.635 | 0.636 | 0.627 | 0.599 |
| | (0.481) | (0.481) | (0.484) | (0.490) |
| **Panel C: Father characteristics** | | | | |
| Age at first birth | 33.307 | 33.573 | 33.751 | 33.841 |
| | (5.418) | (5.540) | (5.691) | (6.089) |
| Obtained higher education | 0.294 | 0.308 | 0.331 | 0.338 |
| | (0.456) | (0.461) | (0.471) | (0.473) |
| Born in Spain | 0.861 | 0.852 | 0.848 | 0.832 |
| | (0.346) | (0.355) | (0.359) | (0.374) |
| **Panel D: Other second-birth characteristics** | | | | |
| C-section | 0.207 | 0.201 | 0.194 | 0.192 |
| | (0.405) | (0.401) | (0.395) | (0.394) |
| Gestation weeks | 39.064 | 39.067 | 39.067 | 39.004 |
| | (1.694) | (1.661) | (1.702) | (1.734) |
| Observations[‡] | 60,553 | 45,858 | 31,882 | 17,267 |

Note: The table presents the mean value of my outcome variables, mother and father controls and other second-birth characteristics of families who had their first child during the years of the reforms I evaluate and its predecessor. Standard deviations are also displayed between parentheses.

[†] This mean only includes data for mothers whose first birth took place between January and August 2019. That is because birth spacing within 40 months of mothers whose first child was born after that cannot be observed, as the dataset ends in December 2022.

[‡] Number of mothers who had her first two children within my database (until December 2022) and whose first child was born during the column year.

creates systematic differences in child spacing between mothers who give birth to their first child in distinct months. In particular, it looks from the raw data that mothers who have their first child by the end or beginning of the year take shorter to have the second child, whereas those who have the first child during spring and summer seasons postpone slightly the conception of their second child. Hence, simply comparing birth-spacing before and after the thresholds when each policy reform came into force would provide biased estimates of their causal impact on fertility decisions.

Figure 4. Child spacing between mothers' two first children by month of birth of the first
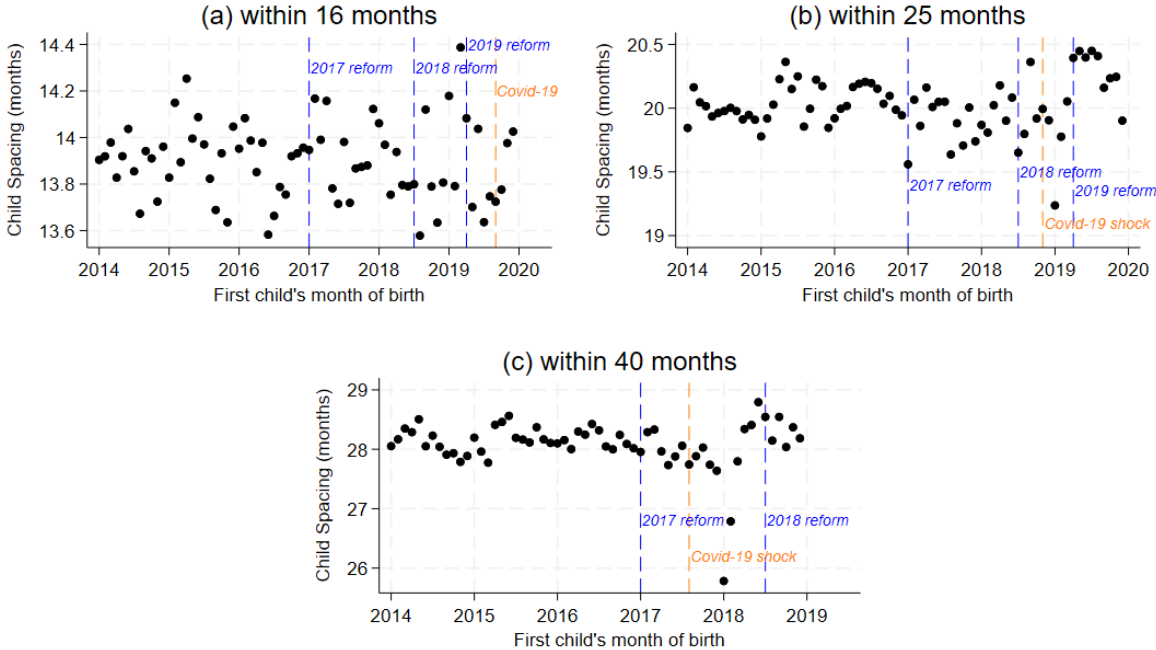


*Source:* Own elaboration with data from the Spanish National Statistics Institute (INE).
*Note:* The dots represent the mean time taken by mothers who had their first child in a given month to have the second one among those who had them both within 16, 25 and 40 months. Vertical blue lines represent the thresholds of the 2017, 2018 and 2019 reforms, while the orange one represents the cutoff from which fertility timing decisions are altered by the the Covid-19 shock. In all three cases it is possible to appreciate the seasonality in child spacing before the reforms, although for graph 4a there is some noise in the data due to the relative low number of observations.

Therefore, to identify the causal effect of paternity leave expansions, I employ a regression discontinuity difference-in-differences (RD-DiD) approach. I compare the time taken to have a second child by mothers just affected or unaffected by the reforms, with a control group consisting of mothers whose first child was born during the same months but in non-reform years. The main identification assumption here is that seasonality in birth spacing should be equal to previous cohorts in the absence of treatment. The same design is used, for example, by Fontenay and Tojerow (2020), to measure the impact of a two-week long paternity leave in mothers' work disability time, and Raute et al. (2022) to examine the effect of parental leave on

paternity acknowledgement.

Taken together, these elements lead to the estimation of the following baseline specification:

$$Y_{i,m,y} = \alpha_0 + \alpha_1 Treat_{i,y} + \alpha_2 C2_{i,y} + \alpha_3 Treat_{i,y} PostReform_{i,m} + \sum_m \gamma_m D_m + \delta X_{i,m,y} + \varepsilon_{i,m,y} \quad (1)$$

where $Y_{i,m,y}$ is the outcome of interest, i.e., child spacing between the first and second childbirth of mother $i$, whose first infant was born in month $m$ and cohort $y$[10], measured in months. $Treat_{i,y}$ represents a binary treatment variable which takes value 1 if the first child of mother $i$ was born during the treated cohort[11] and 0 otherwise. I use two control cohorts, consisting of mothers who had their first child during the same months as the treated cohort, but in the two previous years to each reform. To differentiate the two control cohorts used, I also include an indicator variable $C2_{i,y}$ taking value 1 if observation $i$ belongs to the second control cohort. $Treat_{i,y} PostReform_{i,m}$ is the interaction between the treatment cohort and the months after the reforms, which captures the impact of the reforms on treated individuals from the treatment cohort. $D_m$ are the set of month-fixed effects, that allow to control for time-constant seasonality effects in birth spacing. Finally, I include a vector of baseline exogenous characteristics, which include the second child's province of birth and mother and father's age at first birth, country of birth and level of studies[12]. Nonetheless, the aim of including these controls is merely to provide more precise estimates, and their inclusion in the regressions should not impact the estimates much.

Since the treatment is given at the month-level, the error term $\varepsilon_{i,m,y}$ is clustered at that level as well. Nonetheless, doing so results into managing a relative low number of clusters (six per cohort), which could result into a loss of statistical power to detect significant effects. Following Kolesár and Rothe (2018), I check that this is not the case here with two additional robustness checks. Firstly, I use conventional Eicker-Huber-White (EHW) robust standard errors for inference, as it is not clear that clustering might be needed in RDs with discrete running variables when the chosen window is sufficiently narrow and there is a big number of observations (I re-

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[10]Not to confuse with the year of birth, as the definition of cohort for some reforms includes first-time births from two separate years. For instance, as the 2017 paternity leave expansion to four weeks came into force on January 1st, I consider as the reform cohort all mothers whose first child was born between October 2016 and March 2017, inclusive. Similarly, the two control cohorts for that reform would comprehend women who delivered their first baby from October 2014 and 2015 until March 2015 and 2016, respectively.

[11]First births within a bandwidth of three months before and after each reform.

[12]While parents' age at first birth is possible to compute easily, as their month and year of birth are also disclosed in the dataset, and their country of birth is an unvarying predetermined characteristic, since the level of studies is measured by the time of the second birth, I need to assume that it remained constant within the time span from the first to the the second childbirth.

port results of that estimation in Appendix Table A3). Secondly, I use the Wild Bootstrapping method proposed in Cameron et al. (2008) to provide inference with a low number of clusters (results reported in Appendix Table A4). In both cases, the level of significance of my main estimates lead to the same conclusions, which suggests that the statistically insignificant effects I report in my tables are not driven by a low power to detect significant effects of specification (1), but by non-significant effects indeed.

The coefficient of interest is $\alpha_3$, which measures the causal impact of each paternity leave reform on child spacing, just after its introduction. All specifications estimate intention-to-treat (ITT) effects, as I do not observe the exact take-up rate of paternity leave each month. Nonetheless, as reported in section 4.2, Farré et al. (2024) find that the average take-up was between a 63 and 71% during the period I evaluate, which which suggests that actual average treatment effects (ATE) of paternity leave extensions should be between 1.4 and 1.6 times the magnitude of the estimates I report. Nonetheless in 2019 there might have been another dominant mechanism affecting child spacing due to the reform, as there was a sharp increase in the share of fathers who split the leave after the reform (around 30%), who took it in non-simultaneous periods with the mother's. The fact that fathers would exhaust their leave period after the mothers, might have improved much more their involvement in childcare activities than they would have with another simple extension of the leave. Such additional break in traditional gender roles might have improved mothers' long-term balance of work and family. If that was the case, the actual ATE of that mechanism would be about 3 times the magnitude of the estimates I report for that reform.

The inclusion of month-fixed effects to control for seasonality effects in specification (1) is due to the fact that, given that I do not have the exact date of birth of each child, that is the most precise way to account for seasonal trends. This strategy is also employed by Raute et al. (2022) who evaluate the impact of a parental leave reform in Germany on parent acknowledgement, managing an administrative dataset with the same level of detail on the date of childbirth as the one I use. Still, I also estimate the following equation as a robustness check:

$$
\begin{aligned}
Y_{i,m,y} = \alpha_0 &+ \alpha_1 Treat_{i,y} + \alpha_2 C2_{i,y} + \alpha_3 PostReform_{i,m} \\
&+ \alpha_4 Treat_{i,y} PostReform_{i,m} + \gamma_{ls}(c-m) \\
&+ \gamma_{rs}(m-c) PostReform_{i,m} + \delta X_{i,m,y} + \varepsilon_{i,m,y}
\end{aligned}
\tag{2}
$$

in which $\gamma_{ls}$ and $\gamma_{rs}$ capture seasonality through a linear function[13] in the running variable,

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[13]Since my bandwidth on the baseline specification consists of three months on each side of the threshold, introducing a polynomial of higher degree than one produces the exact same estimation of the coefficient of interest as equation (1).

which is the distance in months to the cutoff, allowing for a change in the slope on each side of the threshold. $Treat_{i,y}$ is a binary variable taking value 1 if woman $i$ belongs to the reform cohort, and 0 if it does to a control cohort, $C2_{i,y}$ is an indicator taking value 1 for observations that belong to the second control cohort, and $PostReform_{i,m}$ takes value 1 if mother $i$ had her first child after the cutoff month $c$ in any cohort-year. The variable of interest is again the interaction of the treatment cohort and post reform terms, $Treat_{i,y}PostReform_{i,m}$. Hence, in this alternative specification, $\alpha_4$ estimates the same effects as $\alpha_3$ from specification (1) and should report similar results.

My baseline regression includes mothers who gave birth to their first child up to three months before or after the policy reforms. Due to how close each reform happened after one another in different times of the year, using a wider bandwidth would make some previous paternity leave extensions interfere in the control groups that I use for the estimation of the causal impact of subsequent reforms. However, the results are robust to specifications restricting the sample to women who delivered in a 2-month window around each paternity leave reform. I also include a specification with one more cohort-year as an additional robustness check.

The RD-DiD identification strategy relies on the assumption that mothers did not arrange the exact date of birth to benefit from the reforms, which makes them comparable to their previous cohorts. The exact date in which every reform I evaluate came into force was given with a short time notice[14], which restricts the possibility that parents planned the date when their child was born in the basis of the reforms, providing quasi-experimental settings within the neighbourhood of the reforms. Nevertheless, it is not impossible that some women who gave birth through a non-urgent C-section were given the opportunity to time their conception in order to benefit from a more generous paternity leave. However, I do not observe a higher proportion of births through C-section after the reform (see balance checks for more detail). In addition, I also report a "donut" specification that excludes mothers whose first baby was born the month before or after the reforms, i.e. mothers who were more capable to alter their date of delivery. As I present in section 7.1, specifications excluding those observations provide similar estimates.

Finally, I limit my study to the impacts of the reforms on birth spacing up to October 2020,

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[14]The 2017 extension was initially going to come into force on January 2011, as the law that regularized it initially proposed (BOE, 2009). However, due to the economic crisis, the reform was continuously postponed until it finally became effective in 2017, which left no room to strategically plan fertility to benefit from it. The 2018 reform was also postponed, as it was initially due to January, but it had to wait until the 2018 State's General Budgets were approved in July 3rd and published in July 4th, taking effect just the day after its publication (BOE, 2018), which also occasioned a quasi-experimental setting. Likewise, the BOE (2019) announced in March 2019 that the remaining escalated increase of the paternity leave would take place through an extension to eight weeks in April that same year, twelve weeks in January 2020 and sixteen in 2021.

focusing on short and medium-term effects of the 2017, 2018 and 2019 reforms and excluding the 2020 and 2021 extensions from the analysis. The reason is that after November 2020, there was a drop in the number of births due to the exogenous shock produced by the Covid-19 pandemic. The effect lasted until March 2021, when the trend reverted due to postponed fertility, and the number of births became higher than expected for the season, as documented by Sobotka et al. (2022) and Alcaide et al. (2023). This obviously violates my main identification assumption that seasonality in child spacing should not vary in the absence of treatment (see how seasonality in birth spacing is affected by Covid-19 in Figure 3). Hence, including these months in my analysis would bring random estimates of the treatment effects that would have nothing to do with paternity leave reforms, which is why I had to exclude these observations from the sample.

## 6.1  Balance checks

To further validate my identification strategy, I analyze the composition of my different treatment groups. To do so, I estimate regressions of type (1) but using observed characteristics as outcome variables that should not be affected by the reforms, testing whether the structure of treatment groups is balanced with respect to the characteristics of control groups. The analysis reported in Table 2 only shows a small under-representation of women with higher education[15] in two of the 2017 treatment groups, as well as of fathers with higher education in the 2019 treatment group. On the contrary, mothers and fathers born in Spain seem to be slightly over-represented within the 2019 treatment group. However, small differences in some characteristics like these would even be expected in random samples and controlling for them barely impacts my results. It is important to highlight that the rest of attributes are balanced across groups and, specially, the proportion of births through C-section. This type of birthing is often scheduled to a chosen date when they are not caused by some health condition, which allows some potential flexibility on the date of birth. The fact that they are correctly balanced on treatment groups suggests that parents would not manipulate the childbirth with the aim of benefiting from a greater paternity leave, limiting concerns of sample selection.

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[15]Higher education is defined here as having at least obtained a university degree (Values 9 or higher in the Spanish National Institute of Statistics classification).

Table 2: Table of balance checks.

| | 2017 | | | 2018 | | 2019 |
|---|---|---|---|---|---|---|
| | 16 mths | 25 mths | 40 mths | 16 mths | 25 mths | 16 mths |
| **Panel A: Mother characteristics** | | | | | | |
| Age at first birth | -0.044 | 0.029 | -0.023 | 0.573 | 0.170 | 0.264 |
| | (0.335) | (0.165) | (0.057) | (0.581) | (0.136) | (0.493) |
| *Baseline mean* | [29.500] | [30.669] | [30.898] | [29.272] | [30.679] | [29.253] |
| Obtained higher education | -0.035 | -0.026** | -0.013*** | -0.059** | -0.001 | -0.020 |
| | (0.020) | (0.008) | (0.003) | (0.023) | (0.017) | (0.020) |
| *Baseline mean* | [0.293] | [0.422] | [0.456] | [0.256] | [0.415] | [0.298] |
| Born in Spain | 0.019 | 0.015 | 0.006 | -0.031 | -0.002 | 0.049** |
| | (0.026) | (0.018) | (0.005) | (0.019) | (0.012) | (0.016) |
| *Baseline mean* | [0.751] | [0.790] | [0.826] | [0.711] | [0.782] | [0.754] |
| **Panel B: Father characteristics** | | | | | | |
| Age at first birth | 0.019 | 0.025 | 0.089 | 0.839 | 0.277 | 0.385 |
| | (0.774) | (0.239) | (0.075) | (0.625) | (0.211) | (0.574) |
| *Baseline mean* | [33.112] | [33.755] | [33.756] | [33.505] | [34.008] | [33.157] |
| Obtained higher education | -0.024 | -0.022* | -0.016 | 0.012 | -0.007 | -0.046** |
| | (0.032) | (0.011) | (0.010) | (0.028) | (0.011) | (0.016) |
| *Baseline mean* | [0.224] | [0.305] | [0.312] | [0.228] | [0.296] | [0.179] |
| Born in Spain | -0.008 | 0.001 | -0.002 | -0.008 | 0.015* | 0.064** |
| | (0.021) | (0.013) | (0.004) | (0.020) | (0.006) | (0.022) |
| *Baseline mean* | [0.777] | [0.805] | [0.835] | [0.731] | [.800] | [0.754] |
| **Panel C: Bunching proxy** | | | | | | |
| C-section | 0.002 | 0.007 | -0.001 | -0.023 | 0.006 | -0.005 |
| | (0.010) | (0.008) | (0.009) | (0.020) | (0.015) | (0.024) |
| *Baseline mean* | [0.223] | [0.203] | [0.204] | [0.192] | [0.192] | [0.188] |
| Observations | 4417 | 22078 | 58634 | 3822 | 18597 | 3168 |

Note: The table reports regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on equation (1). Each coefficient comes from the estimation of a different regression in which the outcome variable is the characteristic displayed. No controls, except for month and cohort-fixed effects, are used in the estimation of this balance checks, although their introduction hardly change the estimates (see Appendix Table A2). Robust standard errors clustered at the calendar month level are reported in parentheses. The baseline mean refers to the mean of each variable for the three months before the threshold of the reform cohort only, of observations included in the sample of each regression. It allows to interpret the coefficients with a better perspective.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# 7 Results

In Figure (5), I plot the seasonality adjusted (residual) birth spacing between the first and second child of a mother who had them both within a 16, 25 and 40-month time spans between July 2014 and June 2018.[16] The running variable is the month of birth of the first child. I also include linear fits with a 95% confidence interval on each side of the 2017 reform threshold. The figure shows no changes in child spacing due to the reform for women who have their first two children within 16 months, while there seems to be a decrease, namely, a reduction in conception time of the second child, for those who have them within 25 and 40 months after the paternity leave 2017 reform, although the differences do not seem statistically significant.

Figure 5. Evaluation of monthly child spacing seasonality corrected.
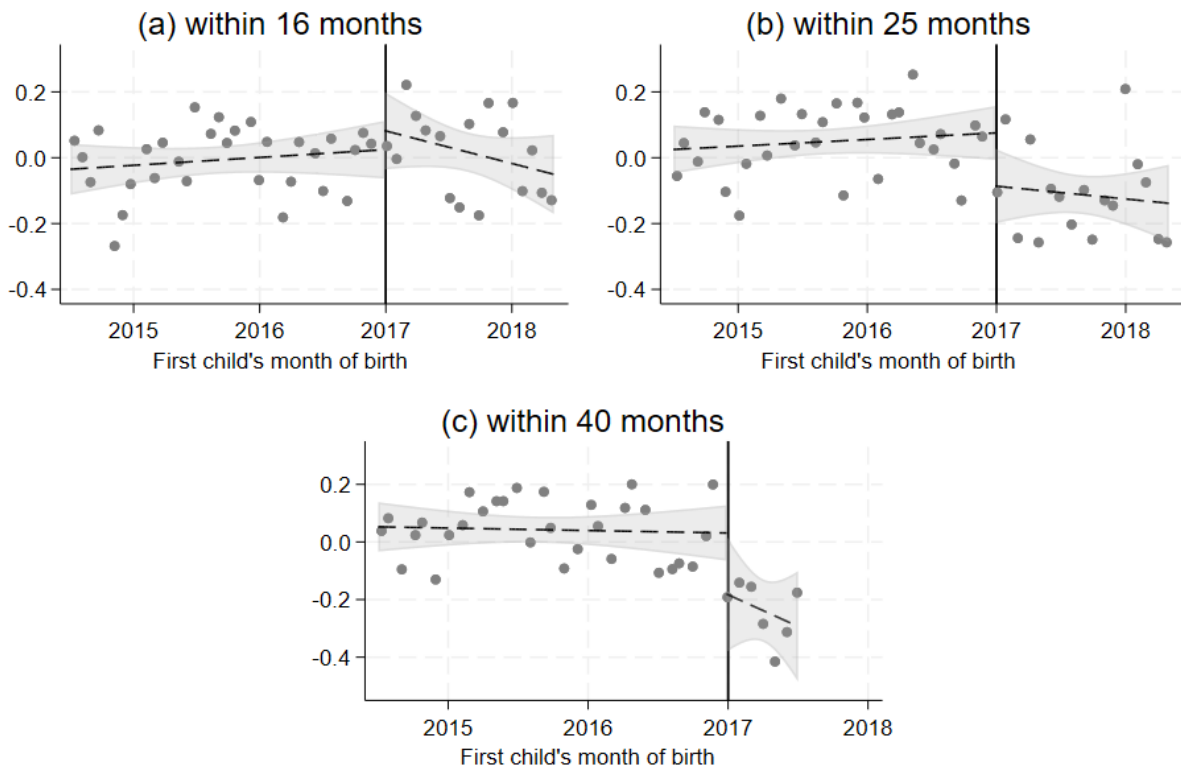


*Source:* Own elaboration with data from the Spanish National Statistics Institute (INE).
*Note:* The figure shows the seasonality-adjusted (residual) mean monthly child spacing over time, for women who have their first two children within 16 months (a), 25 months (b) and 40 months (c). The vertical line represents the January 2017 reform cutoff. The dashed lines are residual (first birth month adjusted) linear fits on each side of the threshold, while the shaded region represents 95% confidence intervals.
For figures 5a and 5b, the sample is limited at the month right before the second reform (June 2018), while the sample for figure 5c is limited at the month before child spacing is affected by the Covid-19 shock (July 2017).

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[16]I do not report the same graph for the 2018 and 2019 reforms, because seasonality in birth spacing of the years prior to those reforms is affected by previous paternity leave reforms, which alters the residuals through other channels than what should be observed only at the cut-off month.

I further explore this in Table 3, which reports the causal impact of the 2017, 2018 and 2019 reforms on child spacing, $\alpha_3$ when estimating equation (1). The results do not report any statistically significant overall effect of paternity leave expansions on the time that first parents take to have their second child. The only significant effects found refer to the 2017 reform, the one from two to four weeks. Specifically, it shows a negative impact of the 2017 reform on birth spacing within parents who had their second child within 40 months after the first, i.e., a reduction in that time interval, although the estimate loses its statistical significance when controlling for mother and father characteristics. On the contrary, the specification including mother and father fixed effects reports a weakly significant positive effect of the extension on child spacing within 16 months. It is important to note, however, that the sample size of women who have their first two children within 16 months is very limited and consists of less educated and younger women than those who have them within 25 or 40 months (see table 2) and are, thus, less likely to deliberately plan conception time (Buckles and Hungerman, 2013).

The empirical findings suggest that, generally speaking, the paternity leave expansions that took place in Spain between 2017 and 2019 did not have any clear effect on the time taken by first-time parents who were subject to more a generous leave to conceive their second child, at least in the short and medium-term, which implies that they would have not affected overall second order fertility. The results are in line with some previous literature which did not find any effects of this kind of policies on fertility timing (Cools et al., 2015; Carnicelli, 2024), whereas it contrasts with results exposed in Farré and González (2019), who concluded that the two-week paternity leave introduced in 2007, also in Spain, postponed and reduced subsequent fertility. Considering the mechanisms they argued for such effect, it could be the case that a first introduction of paternity leave indeed decreased father's desire for subsequent fertility, due to increased awareness on the sacrifices related to child-rearing, but that subsequent extensions of that right do not alter fertility timing through this mechanism, as fathers are already aware the costs of childcare. On the other hand, in line with the hypothesis proposed by Feyrer et al. (2008), it is plausible that, for the past few years, Spain has been entering a third phase in the development of its fertility rate. In this new phase, greater father involvement in childcare would dominate, or at least neutralize, the negative impact on fertility caused by better maternal labor prospects due to more generous family policies, which increase the opportunity cost of subsequent fertility. This would explain why, even after decreased motherhood penalties driven by the reforms that I evaluate (Gorjón and Lizarraga, 2024), subsequent fertility timing remains unaltered.

Table 3: The impact of the reforms on child spacing. Estimation Results.

| | 16 months | | | 25 months | | | 40 months | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **2017 Reform** | | | | | | | | | |
| $Treat*PostReform$ | 0.066 | 0.204 | 0.205* | -0.048 | 0.058 | 0.054 | -0.284** | -0.192 | -0.186 |
| | (0.112) | (0.103) | (0.097) | (0.170) | (0.174) | (0.163) | (0.104) | (0.112) | (0.115) |
| Baseline mean | | [13.787] | | | [19.920] | | | [27.998] | |
| Adjusted $R^2$ | 0.004 | 0.053 | 0.071 | 0.000 | 0.050 | 0.059 | 0.000 | 0.038 | 0.047 |
| Observations | 4417 | 4417 | 4417 | 22078 | 22078 | 22078 | 58634 | 58634 | 58634 |
| **2018 Reform** | | | | | | | | | |
| $Treat*PostReform$ | -0.110 | -0.090 | -0.099 | 0.155 | 0.178 | 0.195 | | | |
| | (0.093) | (0.081) | (0.085) | (0.106) | (0.119) | (0.108) | | | |
| Baseline mean | | [13.996] | | | [19.873] | | | | |
| Adjusted $R^2$ | 0.003 | 0.031 | 0.034 | 0.001 | 0.048 | 0.056 | | | |
| Observations | 3822 | 3822 | 3822 | 18597 | 18597 | 18597 | | | |
| **2019 Reform** | | | | | | | | | |
| $Treat*PostReform$ | 0.113 | 0.138 | 0.158 | | | | | | |
| | (0.100) | (0.127) | (0.133) | | | | | | |
| Baseline mean | | [13.854] | | | | | | | |
| Adjusted $R^2$ | 0.000 | 0.028 | 0.030 | | | | | | |
| Observations | 3168 | 3168 | 3168 | | | | | | |
| Month & Cohort FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Mother Controls | N | Y | Y | N | Y | Y | N | Y | Y |
| Father Controls | N | N | Y | N | N | Y | N | N | Y |
| Cluster | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. |

Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on equation (1). Each coefficient comes from a different regression that evaluates the effects of the paternity leave reforms on child spacing within three time-spans. Columns (1) to (3) report effects of the 2017, 2018 and 2019 reforms for mothers who have their second child within 16 months after the reform, columns (4) to (6) report the effects of the 2017 and 2018 reforms within 25 months and columns (7) to (9) report the effects of the 2017 reform within the first 40 months. For each reform and time-span, the table discloses the estimation of $\alpha_3$ in specifications that include month and cohort-fixed effects only, mother controls as well, and mother and father controls. Robust standard errors clustered at the calendar month level are reported in parentheses. The baseline mean reported between brackets refers to the mean of each outcome variable for the three months before the threshold of the reform cohort only, of observations included in the sample of each regression. It allows to interpret the coefficients with a better perspective. The exposure of the Adjusted $R^2$ related to each regression allows to select my preferred specification for the rest of this paper, which is the one including both mother and father controls, as their introduction improves the model more than it would be expected by chance.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

### 7.1 Robustness checks

Prior to moving on to the analysis of heterogeneous effects, Table 4 presents some robustness checks. Generally, I observe that the results are robust to different specifications, since the estimates of the causal impact of the reforms only vary slightly in magnitude and significance level, but maintain the same sign and economic significance. In particular, I check if the results vary with the sample window chosen around the cutoffs. I find that reducing the bandwidth hardly changes the estimates, although it misses some precision. Moreover, the cohort selection does not seem to alter my results either, as they remain equivalent after adding an extra one. Thirdly, I examine the estimates of a "donut" specification that excludes the months next to the cutoffs. The results limit concerns on sample selection, as they do not differ much from those of my baseline specification either.

Additionally, Appendix Table A5 reports the estimation of the causal effect of the reforms using the alternative specification given by equation (2). In this table the estimates barely diverge in value and significance from the my main results detailed in Table 3, which were based on equation (1). This provides a further validity check on how the simple introduction of month-fixed effects is appropriate for capturing seasonality on an RD-DiD setting with month-level data.

Finally, I present the results of two types of falsification tests which also contribute to test the robustness of my results. Firstly, Table 5 displays false treatment effects of the reforms on an outcome variable which ought not to be affected by them, as is the gestation time in weeks of the second birth. It can be seen how the values are generally smaller than those estimating effects of the reforms on child spacing, even considering that the baseline means of this substitute outcome variable are greater. Specially, it is noteworthy to observe how the estimates tend to zero as I add more observations to the sample, which is the actual expected value of the coefficient here. Hence, this test verifies the tendency of specification (1) to provide a consistent estimator for coefficient $\alpha_3$.

The second type of falsification tests comprehends analyzing the effects of two placebo reforms on birth spacing. I give treatment status to a cutoff month in a non-reform year (January 2016) and to a non-reform month during a reform year (October 2017). Results are presented in Table 6 and fail as well to provide any significant treatment effects of these placebo reforms on my outcome of interest. Likewise, the values of the estimates are smaller than those of my main results, which adds more validity to its interpretation as causal impacts rather than random effects of my baseline specification.

Table 4: The impact of the reform on birth spacing. Robustness checks.

| | 2017 | | | 2018 | | 2019 |
|---|---|---|---|---|---|---|
| | 16 mths | 25 mths | 40 mths | 16 mths | 25 mths | 16 mths |
| **Panel A: 2-month bandwidth** | | | | | | |
| Treat*PostReform | 0.114 | 0.072 | -0.302 | -0.112 | 0.089 | 0.061 |
| | (0.064) | (0.102) | (0.129) | (0.119) | (0.084) | (0.215) |
| Baseline mean | [13.74] | [19.93] | [27.98] | [13.98] | [19.88] | [13.84] |
| Adjusted $R^2$ | 0.085 | 0.059 | 0.048 | 0.030 | 0.061 | 0.028 |
| Observations | 2998 | 14696 | 38820 | 2599 | 8493 | 2054 |
| | | | | | | |
| **Panel B: One extra cohort** | | | | | | |
| Treat*PostReform | 0.125 | 0.131 | -0.154 | -0.165* | 0.151 | 0.107 |
| | (0.066) | (0.147) | (0.113) | (0.079) | (0.108) | (0.114) |
| Baseline mean | [13.79] | [19.92] | [28.00] | [14.00] | [19.87] | [13.85] |
| Adjusted $R^2$ | 0.042 | 0.043 | 0.066 | 0.033 | 0.054 | 0.034 |
| Observations | 6326 | 31702 | 83051 | 5352 | 26380 | 4421 |
| | | | | | | |
| **Panel C: "Donut" specification** | | | | | | |
| Treat*PostReform | 0.217 | 0.079 | -0.048 | -0.006 | 0.340** | 0.256 |
| | (0.149) | (0.250) | (0.053) | (0.096) | (0.076) | (0.170) |
| Baseline mean | [13.80] | [19.90] | [27.97] | [13.92] | [19.84] | [13.84] |
| Adjusted $R^2$ | 0.047 | 0.059 | 0.047 | 0.037 | 0.055 | 0.033 |
| Observations | 2971 | 14694 | 38967 | 2475 | 12215 | 2127 |

 Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on my preferred specification of equation (1), which includes mother and father characteristics as controls. Firstly, the table discloses the estimation of $\alpha_3$ in an specification with a two-month bandwidth around the threshold. Then, the results are reported for a specification that uses the three years prior to each reform as cohorts. Finally, the "donut" specification excludes the months before and after the reforms, to avoid potential manipulation of the date of birth. Robust standard errors are clustered at the calendar month level and reported in parentheses. The baseline mean reported between brackets refers to the sample mean of each outcome variable for the months before the threshold of the reform cohort only. It allows to interpret the coefficients with a better perspective.

 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: The "effect" of the reforms on gestation time. Falsification test.

| | 16 months | | | 25 months | | | 40 months | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **2017 Reform** | | | | | | | | | |
| *Treat\*PostReform* | -0.213 | -0.170 | -0.163 | -0.104 | -0.095 | -0.094 | -0.047 | -0.039 | -0.038 |
| | (0.109) | (0.099) | (0.099) | (0.071) | (0.072) | (0.070) | (0.040) | (0.042) | (0.041) |
| Baseline mean | | [38.646] | | | [38.946] | | | [39.060] | |
| Adjusted $R^2$ | -0.000 | 0.019 | 0.036 | 0.000 | 0.005 | 0.007 | -0.000 | 0.007 | 0.008 |
| Observations | 3676 | 3676 | 3676 | 18894 | 18894 | 18894 | 51097 | 51097 | 51097 |
| **2018 Reform** | | | | | | | | | |
| *Treat\*PostReform* | -0.104 | -0.138 | -0.120 | -0.011 | -0.017 | -0.013 | | | |
| | (0.140) | (0.176) | (0.175) | (0.050) | (0.050) | (0.051) | | | |
| Baseline mean | | [38.430] | | | [38.905] | | | | |
| Adjusted $R^2$ | 0.002 | -0.002 | -0.008 | -0.000 | 0.011 | 0.014 | | | |
| Observations | 3187 | 3187 | 3187 | 16085 | 16085 | 16085 | | | |
| **2019 Reform** | | | | | | | | | |
| *Treat\*PostReform* | 0.016 | 0.013 | 0.042 | | | | | | |
| | (0.143) | (0.120) | (0.114) | | | | | | |
| Baseline mean | | [38.884] | | | | | | | |
| Adjusted $R^2$ | 0.003 | 0.007 | 0.008 | | | | | | |
| Observations | 2643 | 2643 | 2643 | | | | | | |
| Month & Cohort FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Mother Controls | N | Y | Y | N | Y | Y | N | Y | Y |
| Father Controls | N | N | Y | N | N | Y | N | N | Y |
| Cluster | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. |

Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on equation (1). Each coefficient comes from a different regression that evaluates the fake effects of the paternity leave reforms on gestation time of the second child in weeks. Columns (1) to (3) report effects of the 2017, 2018 and 2019 reforms for mothers who have their first two children within 16 months, columns (4) to (6) report the effects of the 2017 and 2018 reforms within 25 months and columns (7) to (9) report the effects of the 2017 reform within the first 40 months. For each reform and time-span, the table discloses the estimation of $\alpha_3$ in specifications that include month and cohort-fixed effects only, mother controls as well, and mother and father control. Robust standard errors are clustered at the calendar month level and reported in parentheses. The baseline mean reported between brackets refers to the sample mean of the outcome variable for the three months before the threshold of the reform cohort only. It allows to interpret the coefficients with a better perspective.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: The impact of placebo-reforms on birth spacing. Non-reform months.

|  | 16 months | | | 25 months | | | 40 months | | |
|---|---|---|---|---|---|---|---|---|---|
|  | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **January 2016 Placebo** | | | | | | | | | |
| *Treat*PostReform* | -0.128 | -0.073 | -0.065 | 0.172 | 0.225 | 0.242 | -0.106 | 0.025 | 0.058 |
|  | (0.079) | (0.084) | (0.094) | (0.160) | (0.154) | (0.157) | (0.114) | (0.090) | (0.093) |
| Baseline mean |  | [13.810] |  |  | [19.981] |  |  | [27.938] |  |
| Adjusted $R^2$ | 0.006 | 0.041 | 0.044 | 0.001 | 0.039 | 0.045 | 0.001 | 0.059 | 0.079 |
| Observations | 5020 | 5020 | 5020 | 25253 | 25253 | 25253 | 66074 | 66074 | 66074 |
|  | | | | | | | | | |
| **October 2017 Placebo** | | | | | | | | | |
| *Treat*PostReform* | 0.052 | 0.121 | 0.101 | -0.014 | 0.009 | 0.004 | | | |
|  | (0.119) | (0.097) | (0.089) | (0.055) | (0.054) | (0.055) | | | |
| Baseline mean |  | [13.885] |  |  | [19.894] |  | | | |
| Adjusted $R^2$ | 0.001 | 0.030 | 0.034 | 0.001 | 0.041 | 0.046 | | | |
| Observations | 4484 | 4484 | 4484 | 21548 | 21548 | 21548 | | | |
| Month & Cohort FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Mother Controls | N | Y | Y | N | Y | Y | N | Y | Y |
| Father Controls | N | N | Y | N | N | Y | N | N | Y |
| Cluster | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. |

Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on equation (1). Each coefficient comes from a different regression that evaluates the whether giving placebo status to some non-reform periods finds some random effect on child spacing within three time-spans. For each reform and time-span evaluated, the table discloses the estimation of $\alpha_3$ in specifications that include month and cohort-fixed effects only, mother controls as well, and mother and father control. Robust standard errors clustered at the calendar month level are reported in parentheses. The baseline mean reported between brackets refers to the sample mean of each outcome variable for the three months before the threshold of the reform cohort only. It allows to interpret the coefficients with a better perspective.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

## 7.2 Heterogeneous effects

In this section I analyze whether the paternity leave reforms had any heterogeneous effects on child spacing across different demographic groups, focusing on three scopes: mother's age, place of birth and level of studies. Formal estimations are presented on Table 7.

Regarding age, it seems that the effects are less pronounced on women who have their first child at 30 years old or earlier, specially in the specifications with bigger samples. Considering that the mean age at first birth of mothers who have at least two children is around 30~31 years in Spain during the period I analyze, it might be the case that women who decide to become mothers under the age mean might be more determined to do so and, thus, are less affected by

Table 7: The impact of the reforms on child spacing. Heterogeneous effects.

|  | 2017 | | | 2018 | | 2019 |
|---|---|---|---|---|---|---|
|  | 16 mths | 25 mths | 40 mths | 16 mths | 25 mths | 16 mths |
| **Panel A: By mother's age** | | | | | | |
| 30 years or less | 0.162 | 0.102 | 0.025 | -0.174 | 0.092 | 0.105 |
|  | (0.136) | (0.336) | (0.137) | (0.110) | (0.168) | (0.170) |
| *Baseline mean* | [13.630] | [19.439] | [27.463] | [13.620] | [19.272] | [13.907] |
| *Observations* | 2122 | 8693 | 21902 | 1974 | 7643 | 1644 |
| Over 30 years | 0.264* | 0.054 | -0.292* | -0.027 | 0.255 | 0.119 |
|  | (0.108) | (0.083) | (0.135) | (0.157) | (0.164) | (0.151) |
| *Baseline mean* | [13.939] | [20.261] | [28.333] | [13.806] | [20.378] | [14.264] |
| *Observations* | 2295 | 13385 | 36732 | 1848 | 10954 | 1524 |
| **Panel B: By mother's place of birth** | | | | | | |
| Spain | 0.181 | 0.082 | -0.198 | 0.052 | 0.238 | 0.138 |
|  | (0.151) | (0.145) | (0.164) | (0.130) | (0.153) | (0.180) |
| *Baseline mean* | [13.869] | [20.040] | [28.273] | [13.731] | [20.169] | [14.117] |
| *Observations* | 3403 | 17933 | 49294 | 2808 | 14730 | 2353 |
| Outside of Spain | 0.373** | 0.001 | -0.086 | -0.603** | 0.062 | -0.025 |
|  | (0.124) | (0.250) | (0.369) | (0.161) | (0.108) | (0.370) |
| *Baseline mean* | [13.539] | [19.472] | [26.689] | [13.652] | [19.048] | [13.939] |
| *Observations* | 1014 | 4145 | 9340 | 1014 | 3867 | 815 |
| **Panel C: By mother's level of studies** | | | | | | |
| Higher education | 0.091 | -0.059 | -0.030 | -0.054 | 0.467** | 0.239** |
|  | (0.149) | (0.176) | (0.197) | (0.198) | (0.117) | (0.079) |
| *Baseline mean* | [13.789] | [19.987] | [27.883] | [13.683] | [20.079] | [14.049] |
| *Observations* | 1292 | 9182 | 26752 | 1086 | 7876 | 2835 |
| Less than higher education | -0.015 | -0.250** | -0.581** | 0.160 | 0.091 | 0.298 |
|  | (0.138) | (0.070) | (0.158) | (0.208) | (0.206) | (0.286) |
| *Baseline mean* | [13.785] | [19.825] | [28.157] | [13.736] | [19.709] | [14.105] |
| *Observations* | 2126 | 9361 | 24999 | 1717 | 5255 | 1446 |

Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on my preferred specification of equation (1), which includes mother and father characteristics and controls. Panel A discloses heterogeneous effects of the reforms according to mother's age. Then, panel B investigates divergence in treatment effects by mother's place of birth. Finally, panel C reports some heterogeneous effects across mother's level of studies. Robust standard errors are clustered at the calendar month level and reported in parentheses. The baseline mean reported between brackets refers to the sample mean of each outcome variable for the months before the threshold of the reform cohort only. It allows to interpret the coefficients with a better perspective.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

paternity leave extensions. Nevertheless, I do not find either any clear effect of the reforms on mothers who have their first child over the age mean. There is only a negative impact on birth spacing of the first reform within the first 40 months, but it is of small statistical significance.

Similarly, the country of birth does not seem to be a determinant of the effects that a longer paternity leave has on the timing decision to have the second baby. Overall, women born in Spain are unaffected by the reforms on the period I analyze, or at least, do not show any short or medium-term effects. There is some opposing statistically significant effects of the 2017 and 2018 reforms in the very short-term for women born outside of Spain. Nonetheless, the sample of such women who had their first two children in Spain within 16 months is very small. Considering as well that these effects vanish when we analyze a longer time-span including more observations, it is likely that they are driven by some outliers in the sample.

Lastly, the analysis of heterogeneity in treatment effects by level of studies reports some interesting insights. Focusing on mothers who obtained higher education, paternity leave reforms that benefit their partners after having the first child results into postponing the conception of the second one, if having any impact at all. On the contrary, the negative non-statistically significant effects of the 2017 reform on birth spacing (see Figure 5 or Table 3) would mainly be driven by women who did not go to university. For that group, the expansion from two to four weeks of the paternity leave that the father of their children enjoyed, resulted into a reduction of the time that they took to have their second child (of those who did within 25 and 40 months). These effects concord with previously explained potential mechanisms. In particular, if exclusive daddy quotas increase father involvement in childcare, having long-lasting effects on gender norms within the couple, there would be two forces affecting subsequent fertility decisions. On the one hand, this extra father involvement results into greater maternal labor prospects, such as higher earnings or working hours, as it is the case of the reforms I evaluate (Gorjón and Lizarraga, 2024), which imply a higher opportunity cost of pausing the mother's career once again to have a second child. On the other hand, a more equal distribution of the housework makes it easier for mothers to combine work and family, as their burden of childbearing is reduced. Hence, for mothers with higher education who probably work at better paid jobs, the first mechanism dominates the second, and the paternity leave expansions result into a postponement of subsequent fertility. Differently, for mothers with less education who go back to worse paid jobs, the second mechanism might be the dominant one. This could be the case if their profession limits the potential benefit of paternity leave extensions on their work comeback but, instead, the improvement of gender norms at home make it easier to combine their duties with more children, shortening the time that they take to have the second.

# 8 Conclusion

In this paper, I analyze the relationship between the allocation of more generous non-transferable paternity leave between 2017 and 2019 in Spain and the timing of second order births. I provide evidence that the extension of such leave rights would have not made a general impact on fertility decisions of second-time mothers. However, the analysis of heterogeneous effects suggests that women who did not attend university would have reduced the time interval between their first two children, as a consequence of the 2017 reform, potentially increasing their completed fertility, while mothers with university education postponed subsequent fertility if being affected at all.

The results provide some insight on how important the context of each household is in the potential impact of family policies on fertility. They also contribute in understanding possible mechanisms that might influence their decisions. I suggest that it is plausible that reduced motherhood penalty due to greater father involvement increases the opportunity costs of pausing or delaying the mother's career to have a child. Hence, mothers with higher education who have access to better jobs would be the principal subjects impacted through this channel, which is why they might postpone the decision. On the other hand, it seems that family policies that allow to break previously established gender roles have the potential to foster fertility, as they make more feasible the combination of work and family for women. This would fit with the previous mixed evidence found in the literature for the effects of these policies in different contexts. Even in the same country, the effects of Spanish paternity leave on subsequent fertility seem to have changed only within a decade, possibly due to the evolution of social norms.

This conclusion is based on the observed short and medium-term effects of three reforms, since the exogenous Covid-19 shock in fertility limited the comparable time-span. In this sense, further evidence on long-term effects of these reforms, and specially of those enabling the split of the permit and the 2021 equalization of maternity and paternity leave, will surely provide relevant contributions to the field. Moreover, I acknowledge that there might be other mechanisms in which family policies might affect fertility rather than the timing of second order births, such as the proportion of mothers having another child, or the incentives that greater family support provide on the decision of couples with no children to have one or not. Future research with more detailed datasets on desired fertility, gender norms within the household and individual social security records, should focus on these lines of research.

Some policy recommendations arise from these findings as well. Even when there is still a long way to full equality, as society is approaching that phase, the promotion of balanced gender norms and family policies that ease the combination of work and family is necessary to avoid further declines on birth rates and, consequently, the size and sustainability of the welfare state.

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# CONTENTS

# Appendix A: Additional Tables

Table A1: The impact of the reforms on child spacing. Includes observations with missing covariates.

|  | 16 months | | | 25 months | | | 40 months | | |
|---|---|---|---|---|---|---|---|---|---|
|  | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **2017 Reform** | | | | | | | | | |
| *Treat*PostReform* | 0.074 | 0.214* | 0.221** | -0.017 | 0.097 | 0.096 | -0.262** | -0.165 | -0.157 |
|  | (0.093) | (0.087) | (0.086) | (0.151) | (0.156) | (0.146) | (0.089) | (0.098) | (0.103) |
| Baseline mean | | [13.760] | | | [19.862] | | | [27.920] | |
| Adjusted $R^2$ | 0.004 | 0.051 | 0.069 | 0.004 | 0.055 | 0.063 | 0.003 | 0.041 | 0.051 |
| Observations | 4650 | 4650 | 4650 | 22784 | 22784 | 22784 | 60021 | 60021 | 60021 |
| **2018 Reform** | | | | | | | | | |
| *Treat*PostReform* | -0.119 | -0.098 | -0.106 | 0.143 | 0.167 | 0.182 | | | |
|  | (0.094) | (0.082) | (0.086) | (0.126) | (0.132) | (0.121) | | | |
| Baseline mean | | [13.979] | | | [19.854] | | | | |
| Adjusted $R^2$ | 0.005 | 0.032 | 0.034 | 0.002 | 0.050 | 0.057 | | | |
| Observations | 3993 | 3993 | 3993 | 19149 | 19149 | 19149 | | | |
| **2019 Reform** | | | | | | | | | |
| *Treat*PostReform* | 0.070 | 0.085 | 0.104 | | | | | | |
|  | (0.111) | (0.118) | (0.116) | | | | | | |
| Baseline mean | | [13.851] | | | | | | | |
| Adjusted $R^2$ | 0.005 | 0.033 | 0.035 | | | | | | |
| Observations | 3320 | 3320 | 3320 | | | | | | |
| Month & Cohort FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Mother Controls | N | Y | Y | N | Y | Y | N | Y | Y |
| Father Controls | N | N | Y | N | N | Y | N | N | Y |
| Cluster | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. |

Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on equation (1). The table is equivalent to table 3 but includes observations with missing covariates that were substituted with the sample mean or given an extra value for categorical controls. A dummy was also added to indicate those observations that had some missing characteristic. Each coefficient comes from a different regression that evaluates the effects of the paternity leave reforms on child spacing within three time-spans. Columns (1) to (3) report effects of the 2017, 2018 and 2019 reforms for mothers who have their second child within 16 months after the reform, columns (4) to (6) report the effects of the 2017 and 2018 reforms within 25 months and columns (7) to (9) report the effects of the 2017 reform within the first 40 months. For each reform and time-span, the table discloses the estimation of $\alpha_3$ in specifications that include month and cohort-fixed effects only, mother controls as well, and mother and father controls. Robust standard errors clustered at the calendar month level are reported in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Table of balance checks. Includes controls in the estimation.

| | 2017 | | | 2018 | | 2019 |
|---|---|---|---|---|---|---|
| | 16 mths | 25 mths | 40 mths | 16 mths | 25 mths | 16 mths |
| **Panel A: Mother characteristics** | | | | | | |
| Age at first birth | -0.555 | -0.011 | -0.041 | 0.461 | 0.041 | 0.329 |
| | (0.277) | (0.107) | (0.056) | (0.263) | (0.103) | (0.403) |
| *Baseline mean* | [29.500] | [30.669] | [30.898] | [29.272] | [30.679] | [29.253] |
| Obtained higher education | -0.030 | -0.021 | -0.004 | -0.060* | 0.002 | -0.008 |
| | (0.017) | (0.012) | (0.004) | (0.025) | (0.016) | (0.032) |
| *Baseline mean* | [0.293] | [0.422] | [0.456] | [0.256] | [0.415] | [0.298] |
| Born in Spain | 0.010 | 0.019* | 0.011** | -0.013 | -0.002 | 0.005 |
| | (0.019) | (0.008) | (0.004) | (0.018) | (0.009) | (0.017) |
| *Baseline mean* | [0.751] | [0.790] | [0.826] | [0.711] | [0.782] | [0.754] |
| **Panel B: Father characteristics** | | | | | | |
| Age at first birth | -0.007 | 0.063 | 0.104 | 0.222 | 0.119 | 0.172 |
| | (0.480) | (0.157) | (0.064) | (0.282) | (0.176) | (0.520) |
| *Baseline mean* | [33.112] | [33.755] | [33.756] | [33.505] | [34.008] | [33.157] |
| Obtained higher education | -0.004 | -0.008 | -0.010 | 0.023 | -0.007 | -0.036 |
| | (0.036) | (0.009) | (0.008) | (0.024) | (0.013) | (0.019) |
| *Baseline mean* | [0.224] | [0.305] | [0.312] | [0.228] | [0.296] | [0.179] |
| Born in Spain | -0.034* | -0.009* | -0.002 | 0.011 | 0.021** | 0.012 |
| | (0.015) | (0.004) | (0.002) | (0.018) | (0.006) | (0.016) |
| *Baseline mean* | [0.777] | [0.805] | [0.835] | [0.731] | [.800] | [0.754] |
| **Panel C: Bunching proxy** | | | | | | |
| C-section | -0.000 | 0.006 | -0.001 | -0.029 | 0.007 | -0.002 |
| | (0.017) | (0.009) | (0.009) | (0.029) | (0.014) | (0.018) |
| *Baseline mean* | [0.223] | [0.203] | [0.204] | [0.192] | [0.192] | [0.188] |
| Observations | 4417 | 22078 | 58634 | 3822 | 18597 | 3168 |

Note: The table reports regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on equation (1). Each coefficient comes from the estimation of a different regression in which the outcome variable is the characteristic displayed. Month and cohort-fixed effects, as well as mother and father characteristics were included in the estimation of this balance checks. Robust standard errors clustered at the calendar month level are reported in parentheses. The baseline mean refers to the mean of each variable for the three months before the threshold of the reform cohort only, of observations included in the sample of each regression. It allows to interpret the coefficients with a better perspective.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: The impact of the reforms on child spacing. Non-clustered standard errors.

| | 16 months | | | 25 months | | | 40 months | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **2017 Reform** | | | | | | | | | |
| $Treat*PostReform$ | 0.066 | 0.204* | 0.205* | -0.048 | 0.058 | 0.054 | -0.284** | -0.192 | -0.186 |
| | (0.122) | (0.123) | (0.124) | (0.115) | (0.113) | (0.113) | (0.137) | (0.135) | (0.135) |
| Baseline mean | | [13.787] | | | [19.920] | | | [27.998] | |
| Adjusted $R^2$ | 0.004 | 0.053 | 0.071 | 0.000 | 0.050 | 0.059 | 0.000 | 0.038 | 0.047 |
| Observations | 4417 | 4417 | 4417 | 22078 | 22078 | 22078 | 58634 | 58634 | 58634 |
| **2018 Reform** | | | | | | | | | |
| $Treat*PostReform$ | -0.110 | -0.090 | -0.099 | 0.155 | 0.178 | 0.195 | | | |
| | (0.133) | (0.133) | (0.134) | (0.127) | (0.124) | (0.124) | | | |
| Baseline mean | | [13.996] | | | [19.873] | | | | |
| Adjusted $R^2$ | 0.003 | 0.031 | 0.034 | 0.001 | 0.048 | 0.056 | | | |
| Observations | 3822 | 3822 | 3822 | 18597 | 18597 | 18597 | | | |
| **2019 Reform** | | | | | | | | | |
| $Treat*PostReform$ | 0.113 | 0.138 | 0.158 | | | | | | |
| | (0.144) | (0.143) | (0.145) | | | | | | |
| Baseline mean | | [13.854] | | | | | | | |
| Adjusted $R^2$ | 0.000 | 0.028 | 0.030 | | | | | | |
| Observations | 3168 | 3168 | 3168 | | | | | | |
| Month & Cohort FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Mother Controls | N | Y | Y | N | Y | Y | N | Y | Y |
| Father Controls | N | N | Y | N | N | Y | N | N | Y |
| Cluster | N | N | N | N | N | N | N | N | N |

Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on equation (1). The table is equivalent to 3, but standard errors are not clustered at the month level. Each coefficient comes from a different regression that evaluates the effects of the paternity leave reforms on child spacing within three time-spans. Columns (1) to (3) report effects of the 2017, 2018 and 2019 reforms for mothers who have their second child within 16 months after the reform, columns (4) to (6) report the effects of the 2017 and 2018 reforms within 25 months and columns (7) to (9) report the effects of the 2017 reform within the first 40 months. For each reform and time-span, the table discloses the estimation of $\alpha_3$ in specifications that include month and cohort-fixed effects only, mother controls as well, and mother and father controls. Heteroscedasticity-robust standard errors are reported in parentheses. The baseline mean reported between brackets refers to the mean of each outcome variable for the three months before the threshold of the reform cohort only, of observations included in the sample of each regression. It allows to interpret the coefficients with a better perspective.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: The impact of the reforms on child spacing. Wild Bootstrap inference.

| | 16 months | | | 25 months | | | 40 months | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **2017 Reform** | | | | | | | | | |
| *Treat\*PostReform* | 0.066 | 0.204* | 0.205** | -0.048 | 0.058 | 0.054 | -0.284** | -0.192 | -0.186 |
| | (0.618) | (0.080) | (0.036) | (0.760) | (0.834) | (0.760) | (0.042) | (0.138) | (0.142) |
| Baseline mean | | [13.787] | | | [19.920] | | | [27.998] | |
| Observations | 4417 | 4417 | 4417 | 22078 | 22078 | 22078 | 58634 | 58634 | 58634 |
| **2018 Reform** | | | | | | | | | |
| *Treat\*PostReform* | -0.110 | -0.090 | -0.099 | 0.155 | 0.178 | 0.195 | | | |
| | (0.282) | (0.388) | (0.352) | (0.152) | (0.218) | (0.142) | | | |
| Baseline mean | | [13.996] | | | [19.873] | | | | |
| Observations | 3822 | 3822 | 3822 | 18597 | 18597 | 18597 | | | |
| **2019 Reform** | | | | | | | | | |
| *Treat\*PostReform* | 0.113 | 0.138 | 0.158 | | | | | | |
| | (0.404) | (0.394) | (0.356) | | | | | | |
| Baseline mean | | [13.854] | | | | | | | |
| Observations | 3168 | 3168 | 3168 | | | | | | |
| Month & Cohort FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Mother Controls | N | Y | Y | N | Y | Y | N | Y | Y |
| Father Controls | N | N | Y | N | N | Y | N | N | Y |
| Cluster | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. |

Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_3$ on equation (1). It is equivalent to table 3, but the Wild Bootstrapping method was used to provide inference and p-values are reported in parentheses. Each coefficient comes from a different regression that evaluates the effects of the paternity leave reforms on child spacing within three time-spans. Columns (1) to (3) report effects of the 2017, 2018 and 2019 reforms for mothers who have their second child within 16 months after the reform, columns (4) to (6) report the effects of the 2017 and 2018 reforms within 25 months and columns (7) to (9) report the effects of the 2017 reform within the first 40 months. For each reform and time-span, the table discloses the estimation of $\alpha_3$ in specifications that include month and cohort-fixed effects only, mother controls as well, and mother and father controls. The baseline mean reported between brackets refers to the mean of each outcome variable for the three months before the threshold of the reform cohort only, of observations included in the sample of each regression. It allows to interpret the coefficients with a better perspective.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: The impact of the reforms on child spacing. Alternative specification.

| | 16 months | | | 25 months | | | 40 months | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **2017 Reform** | | | | | | | | | |
| Treat*PostReform | 0.060 | 0.194 | 0.195 | -0.047 | 0.058 | 0.055 | -0.284** | -0.193 | -0.187 |
| | (0.113) | (0.105) | (0.099) | (0.170) | (0.174) | (0.163) | (0.104) | (0.113) | (0.115) |
| Baseline mean | | [13.787] | | | [19.920] | | | [27.998] | |
| Adjusted $R^2$ | 0.003 | 0.052 | 0.069 | 0.000 | 0.050 | 0.058 | 0.000 | 0.038 | 0.047 |
| Observations | 4417 | 4417 | 4417 | 22078 | 22078 | 22078 | 58634 | 58634 | 58634 |
| **2018 Reform** | | | | | | | | | |
| Treat*PostReform | -0.107 | -0.089 | -0.097 | 0.156 | 0.178 | 0.195 | | | |
| | (0.093) | (0.083) | (0.087) | (0.106) | (0.119) | (0.108) | | | |
| Baseline mean | | [13.996] | | | [19.873] | | | | |
| Adjusted $R^2$ | 0.003 | 0.032 | 0.034 | 0.001 | 0.048 | 0.056 | | | |
| Observations | 3822 | 3822 | 3822 | 18597 | 18597 | 18597 | | | |
| **2019 Reform** | | | | | | | | | |
| Treat*PostReform | 0.115 | 0.138 | 0.157 | | | | | | |
| | (0.101) | (0.127) | (0.132) | | | | | | |
| Baseline mean | | [13.854] | | | | | | | |
| Adjusted $R^2$ | 0.000 | 0.028 | 0.030 | | | | | | |
| Observations | 3168 | 3168 | 3168 | | | | | | |
| Cohort FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Mother Controls | N | Y | Y | N | Y | Y | N | Y | Y |
| Father Controls | N | N | Y | N | N | Y | N | N | Y |
| Cluster | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. | Mth. |

Note: The table presents regression discontinuity difference-in-differences estimates corresponding to $\alpha_4$ on the alternative specification (2), which is equivalent to $\alpha_3$ on equation (1). Each coefficient comes from a different regression that evaluates the effects of the paternity leave reforms on child spacing within three time-spans. Columns (1) to (3) report effects of the 2017, 2018 and 2019 reforms for mothers who have their second child within 16 months after the reform, columns (4) to (6) report the effects of the 2017 and 2018 reforms within 25 months and columns (7) to (9) report the effects of the 2017 reform within the first 40 months. For each reform and time-span, the table discloses the estimation of $\alpha_3$ in specifications that include month and cohort-fixed effects only, mother controls as well, and mother and father controls. Robust standard errors clustered at the calendar month level are reported in parentheses. The baseline mean reported between brackets refers to the mean of each outcome variable for the three months before the threshold of the reform cohort only, of observations included in the sample of each regression. It allows to interpret the coefficients with a better perspective.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$