Paternity Leave and Child Development

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Lídia Farré, Libertad González, Claudia Hupkau, Jenifer Ruiz-Valenzuela

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Lídia Farré
IAE-CSIC & BSE

Libertad González
Universitat Pompeu Fabra & BSE

Claudia Hupkau
CUNEF Universidad & CEP (LSE)

Jenifer Ruiz-Valenzuela
Universitat de Barcelona & IEB & CEP (LSE)

Abstract

We study the effect of paternity leave on early child development. We collect survey data on 5,000 children under age six in Spain, and exploit several extensions of paternity leave that took place between 2017 and 2021. We follow a differences-in-discontinuities research design, based on the date of birth of each child and using cohorts born in non-reform years as controls. We show that the extensions led to significant increases in the length of leave taken by fathers, without affecting that of mothers, thus increasing parental time at home in the first year after birth. Eligibility for four additional weeks of paternity leave led to a significant 12 percentage-point increase in the fraction of children with developmental delays. We provide evidence for two potential mechanisms. First, children exposed to longer paternity leave spend less time alone with their mother, and more time with their father, during their first year of life. Second, treated children use less formal childcare. Our results suggest that paternity leave replaces higher-quality modes of early care. We conclude that the effects of parental leave policies on children depend crucially on the quality of parental versus counterfactual modes of childcare.

*Contact: lidia.farre@gmail.com, libertad.gonzalez@upf.edu, claudia.hupkau@cunef.edu, jruizv@ub.edu. González and Farré acknowledge financial support from the Spanish Agencia Estatal de Investigación (AEI), through the Severo Ochoa Programme for Centres of Excellence in R&D (Barcelona School of Economics CEX2019-000915-S). Farré, Hupkau and Ruiz-Valenzuela acknowledge financial support from the Spanish Ministry of Science, Innovation and Universities through grant PID2022-140206OB-I0 of the Proyectos de Generacion de Conocimiento 2022 program.

1 Introduction

Human capital is a major determinant of economic growth and individual life trajectories (Becker, 2009). Recent research has shown that human capital formation starts in the early stages of childhood (Attanasio et al., 2022) and that shocks during this initial period can have long-lasting effects (Almond et al., 2018; García et al., 2020). During the initial developmental stages, children primarily interact with their parents, so that parenting behaviors and the quality of the home environment can play a crucial role in shaping child outcomes (Cunha and Heckman, 2008; Cunha et al., 2010).

Mothers continue to be the main providers of childcare, especially during the very early stages of a child's life. To foster fathers' participation in childcare and promote gender equality both at home and in the labor market, many countries have introduced parental leave permits earmarked for fathers (not transferable to mothers). These policies have been shown to substantially increase fathers' take-up of parental leave, as well as men's participation in child rearing activities (Canaan et al., 2022). Much less is known about the effects of increased paternity leave on children's human capital formation.

We study the effect of paternity leave on child development at early ages. For identification, we leverage five successive extensions that took place in Spain between 2017 and 2021. The reforms increased the duration of paternity leave entitlements from two to four weeks in 2017, five in 2018, eight in 2019, twelve in 2020, and sixteen weeks in 2021, when paternity reached the same duration as maternity leave.[1]

Since the date of birth of the child determines the length of the paternity leave entitlement, we follow a regression discontinuity (RD) approach, comparing outcomes of children born just before each of the eligibility cutoffs with those born just after, to capture intent-to-treat effects of the policy changes. Due to the well-documented seasonality in births as well as child outcomes (Buckles and Hungerman, 2013; Currie and Schwandt, 2013), we combine the RD approach with a differences-in-differences design (RD-DD), using cohorts not affected by reforms as controls. Our identification strategy relies on the assumption that seasonality in births and child outcomes does not change across reform

[1] As a result of the most recent extension, Spain became the first country in establishing a symmetric parental leave policy, granting the same duration and generosity of leave benefits to mothers and fathers.

and non-reform years. We perform several tests that provide evidence in support of this assumption. Our RD-DD approach should thus net out seasonal differences in births that are not related to paternity leave extensions (Persson and Rossin-Slater, 2023; Avdic et al., 2023; Raute et al., 2020).

We measure child development using the Ages & Stages Questionnaire (ASQ-3, Squires et al. (2009)), a well-established diagnostic test for children ages zero to six, that helps to detect developmental delays along several dimensions (communication, fine and gross motor skills, social skills, and problem solving). The ASQ-3 includes 21 age-specific questionnaires, allowing us to define age-appropriate, comparable measures of child development. Our data comes from our own online survey of 5,000 households with children under age six.

We focus on the two most recent and most generous reforms (in 2020 and 2021), which increased the duration of the paternity leave entitlement by four weeks each. We find that these extensions led to an average increase of two weeks in the duration of the leave taken by (potentially eligible) fathers in our sample. We do not find evidence that the reforms replaced other forms of leave or time off from work in the months after the birth of the child, so we conclude that the extensions increased significantly the amount of time that fathers stayed home during the 12 months following childbirth.

We then show that the 2020 and 2021 extensions led to a robust (12 percentage-point) increase in the fraction of children with developmental delays (in at least one of the five developmental areas measured by the ASQ-3). The negative effects are robust and sizable, and they are mainly driven by a deterioration in communication, gross motor, and personal-social skills.

We explore two sets of potential mechanisms. First, we focus on the initial months of life of the child, with retrospective questions about the use of paternity and maternity leave. Our analysis reveals that fathers take about 1.2 weeks of the additional two weeks of paternity leave resulting from the reforms while the mother is also on leave, while the remaining 0.8 weeks are taken after the mother is back at work. We find no effect of paternity leave extensions on the length of maternity leave. Thus, the additional weeks of paternity leave increase the time that the child spends alone with the father, while also replacing time alone with the mother with time spent with both the mother and the father.

The effects on child development may also stem from changes that extend beyond the parental leave period. We thus study the effects of parental leave extensions on childcare modes at the time of our survey (when the relevant children were 1-2 years old). We find that children in treated households are nearly 17 percentage points less likely to attend formal childcare (significant and robust to multiple hypothesis testing), and spend fewer hours in formal childcare.

We are able to rule out other potential mechanisms, including changes in parental separation, and effects on parents' labor supply or household income. Our results thus suggest that the negative effects of paternity leave on child outcomes may have resulted from a reduction in both solo maternal care (in the initial weeks of life) and time in formal childcare (later on). We also find that the negative effects on child development are concentrated on children with highly educated mothers. We conclude that longer paternity leave may have led to a replacement from higher to lower-quality childcare time.

We contribute to four related strands of literature, on: i) the effects of early shocks and interventions on child outcomes, ii) the importance of parental investments (including time) for child development, iii) the impact of parental leave policies, and iv) the effects of early formal childcare. We study an early intervention focused on parental time. Our contribution relies on the novel focus on the role of fathers in early child development, relative to counterfactual modes of care, including maternal and formal childcare.

To our knowledge, we are the first to provide causal evidence on the effects of paternity leave on early child development, and one of the very first to focus on early *paternal* investments. The literature on the effects of parental leave reforms on child development has focused on maternal time (Rossin-Slater, 2017). Existing evidence suggests that the introduction of a short period of maternity leave is likely beneficial for long-term child outcomes, while longer extensions do not show additional returns.[2]

There is a growing recent literature on the effects of paternity leave policies. Most of

[2]Some weeks of job protected leave after the birth of the child have been shown to reduce physical and mental job-related stress (Stearns, 2015), allow mothers to breastfeed longer (Baker and Milligan, 2008) and increase the ability to seek for earlier interventions (Rossin, 2011). Carneiro et al. (2015) also document positive long-term effects on educational attainment and earnings.

this literature has focused on the labor market outcomes of parents (Canaan et al., 2022). Two recent papers have addressed the impact of paternity leave extensions on children's school outcomes, using administrative data from Nordic countries. Both studies analyze the *introduction* of earmarked paternity leave, while we study the *extensions* of earmarked leave for fathers. They find negligible effects on average, with some evidence of heterogeneity by parental characteristics. Cools et al. (2015) find positive effects of the introduction of a paternal leave quota on exam scores at age 16 in families where the father has more education than the mother (in Norway). Avdic et al. (2023) find a negative impact of the introduction of earmarked paternity leave in Sweden in 1995 on school-leaving grade point averages, driven by sons of non-college educated fathers.

There is also an extensive literature on the broader determinants of early child development. Previous studies have documented the importance of high-quality investments by parents during early childhood (Cunha and Heckman, 2007; García et al., 2020), highlighting the importance of material resources and time (Currie and Almond, 2011; Cunha et al., 2006). Our study represents a pioneering effort in providing causal evidence of the effects of *paternal* time on child development during this critical early period. Our results indicate that time alone with the mother or in formal childcare may be of higher quality than time spent with the father during early childhood.

Regarding non-parental modes of childcare, a substantial body of research has documented that attending formal, center-based care may have positive effects for children from disadvantaged backgrounds, who have as an alternative lower-quality in-home care (Heckman et al., 2017; Felfe and Lalive, 2018; Drange and Havnes, 2019). In contrast, the effects are zero or even negative among children of high-income families, who in the absence of formal care would have been at home with their highly educated mothers (Baker et al., 2008; Fort et al., 2020). This is consistent with our result that a decrease in formal childcare is associated with negative developmental effects among vulnerable children (at the margin of severe developmental delays).

Our findings suggest that there may be high returns to investments in fathers' parenting skills. Recent research highlights the importance of good parenting practices early in life for children of disadvantaged families. Carneiro et al. (2024), for instance, evaluate the effects of a large-scale parenting program targeted at poor families in Chile on cognitive and non-cognitive skills. They show that parental counseling by trained professionals changed parental beliefs and expectations about early investments in children and improved positive parenting strategies with their children. García and Heckman (2023) also show that the activation and promotion of parenting skills of caregivers is a successful component of iconic childhood enrichment programs, such as the Perry Preschool and The Carolina Abecedarian Project. Together with our results, the evidence suggests that policies aimed at promoting fathers' participation in child rearing activities may be complemented with interventions addressed to strengthening parenting skills. These types of interventions have the potential to be highly beneficial for the new generations of fathers (and their offspring), who are expected to spend more time with their children than ever before.

The rest of the paper is organized as follows. We introduce the institutional context and describe the relevant reforms in Section 2. Section 3 presents the empirical strategy, while we describe our data in Section 4. Section 5 details our results on paternity leave take-up, child outcomes, and mechanisms, and Section 7 concludes.

2 Institutional context

In 2007, Spain introduced thirteen days of job-protected, paid leave for fathers for the first time. The length of paternity leave remained unchanged until 2017. Since then, five reforms have taken place. The permit was extended to four weeks in January 2017, five weeks in July 2018, eight weeks in April 2019, 12 weeks in January 2020, and 16 weeks in January 2021. The extension to 16 weeks in 2021 effectively equalized earmarked parental leave for mothers and fathers.

Until 2018, paternity leave had to be taken *simultaneously* with maternity leave, and were typically taken immediately after the birth of the child. One can thus think of paternity leave in Spain until 2018 as comparable to *baseline days* (Persson and Rossin-Slater, 2023) in Sweden, i.e., wage-replaced leave while mothers claim full-time leave. Since 2018, fathers can take some weeks of their leave entitlement at any time before the child turns nine (later 12) months. A set number of weeks, which has changed with each reform, has to be taken right after the birth of the child. Thus, the reforms (which we describe in more detail below) increased both the number of weeks available to fathers simultaneously with the mother, as well as the number of weeks that could potentially be taken while the mother had already returned to work. We summarize the main characteristics of maternity and paternity leave over the last two decades in Table 1.

Over the same period, the maternity leave permit has remained unchanged at 16 weeks, with six compulsory weeks right after birth and 10 additional weeks that could be shared with the father.[3] In practice, very few mothers ever shared their leave with fathers (Farré and González, 2019) and most mothers take their entire 16 weeks of leave entitlement all at once right after childbirth. In recent years, a small share of mothers have started to split their leave into initial and subsequent periods, going back to work in between (Farré et al., 2024).

Maternity and paternity leave in Spain is fully wage-replaced up to a maximum amount, which has varied over time and stood at 4,070.10€ per month in 2021.[4] The parental leave payment can be complemented by the employer to match regular net pay. This depends on firm policy and collective bargaining agreements and has remained unchanged over the whole period we study. Eligibility criteria for the paternity leave benefit did not change over time.[5] Since 2014, maternity and paternity leave benefits have been exempt from personal income tax, creating strong incentives for workers to take full advantage of

[3] Spain has granted 16 weeks of fully compensated maternity leave since 1989, and the main eligibility requirement was having worked in the formal sector for at least 180 days prior to birth. Since 2013, after the paid leave period, either parent can take unpaid parental leave for up to 3 years, with a right to return to the same job. One of the parents can also reduce working hours up to 50% (with a proportional reduction in pay) until the child turns 12. In practice, very few fathers make use of either the unpaid leave or the reduction in hours (Fernández-Kranz and Rodríguez-Planas, 2021).

[4] Only about 5% of fathers receive the maximum amount of the benefit payment each year (Farré et al., 2024).

[5] These are as follows: One has to be employed or self-employed and paying social security contributions. For fathers who are 20 or younger, no minimum contribution period is required. For fathers between 21 and 25 years old, the minimum contribution period required is 90 days of contributions within the seven years immediately prior to the start of the leave. This requirement shall be deemed to have been met if, alternatively, he proves 180 days of contributions during his working life, prior to the latter date. For fathers who are older than 25, the minimum contribution period required is 180 days of contributions within the seven years immediately prior to the start of the leave period. This requirement shall be deemed to have been met if, alternatively, he can prove that he has paid 360 days of contributions during his working life prior to the latter date.

the leave. Payments come directly from the Treasury of the Social Security and employers only have to cover their employees' social security contributions while these are on leave.

Extensions in 2017 and 2018 The first extension of paternity leave – from two to four weeks – is that of January 1, 2017. The increase was foreseen in Law 9/2009, of October 6 (Boletin Oficial de Estado, 2009), on the extension of the duration of paternity leave in cases of birth, adoption or fostering. This law was to come into force as from January 1, 2011, but by means of successive Budget Laws, its implementation was delayed. Eventually, on December 16, 2016, the council of ministers under the conservative government announced that the increase in paternity leave would come into force for all children born on and after January 1, 2017. The additional leave entitlement was entirely reserved for fathers, and could not be shared with or transferred to the mother. Also, the entire four weeks had to be taken at once, typically starting on the day of birth of the child.[6] Thus, the 2017 reform effectively increased *simultaneous* leave with the mother, who had to take six mandatory weeks of leave right after giving birth.

The 2018 extension – from four to five weeks – introduced for the first time the possibility to split paternity leave into different periods. The first four weeks had to be taken simultaneously with the mother, typically starting right after birth, while the fifth could be taken at any time before the child turned nine months, either on a full- or part-time basis. This reform was first presented in the Spanish Congress by the conservative government on April 3, 2018, and was approved within Law 6/2018, of July 3 (Boletin Oficial de Estado, 2018), coming into effect on July 5, 2018.

Extensions in 2019, 2020 and 2021 On March 1, 2019, the socialist government passed the "Law on Urgent Measures to Guarantee Equality between Men and Women" (Boletin Oficial de Estado, 2019). The law had not been discussed in parliament prior to its implementation because it was passed by Royal Decree. It was validated in Congress on March 7, 2019, but did not come into force until April 1, 2019. This law announced the staggered increase of paternity leave from five to eight weeks for all children born on and after April

[6] Even though fathers could take their four weeks at any time during the duration of maternity leave, most fathers started their leave with the birth date of the child.

1, 2019, to 12 weeks for those born on and after January 1, 2020, and to 16 weeks for those born on and after January 1, 2021.

In addition to the substantial increases in the leave permits taking place between 2019 and 2021, with the 2019 law, paternity leave was made mandatory.[7] The length of the mandatory portion of the leave was gradually increased from initially two weeks in 2019, four weeks in 2020, to six weeks in 2021. Another novelty of the 2019 law was that of the eight weeks granted in 2019, four could be transferred to the mother. In 2020 still two weeks could be transferred to the mother, and in 2021 no transfers were permitted. The non-mandatory portion of paternity leave could be taken either part- or full-time and at any time before the child turns 12 months.

To be able to use the cutoff dates for identification of the causal effect of paternity leave extensions, there should be no selection of births into either side of the cutoff date. For instance, if families who care particularly about paternal involvement in childcare strategically delay childbirth to fall on or after the cutoff date, our estimates might be biased. Indeed, for the reforms in 2017, 2018 and 2019, which were announced between two and four weeks before coming into force, mothers who had their due date close to the cutoff dates might have been able to delay births. We will discuss the implications of manipulation around the cutoff dates in our empirical strategy and implement robustness checks where we exclude observations for children born in the months before and after the cutoff dates.

The case of the 2020 and 2021 is different, because these extensions were announced nine months and 21 months before the respective cutoff dates, raising concerns about strategic timing of pregnancies and births. To address these concerns, we perform tests of manipulation around the cutoff dates in reform years, which we describe in more detail in Section 3. We also check balance of observed pre-determined characteristics for each of the reform cut-offs.

[7] According to the law (Boletin Oficial de Estado, 2019), "The birth shall suspend the employment contract of the parent other than the biological mother for 16 weeks, of which the six uninterrupted weeks immediately following childbirth shall be mandatory, to be taken on a full-time basis, for the fulfillment of the caregiving duties provided for in Article 68 of the Civil Code". Moreover, according to officials from Social Security, not taking up the paternity leave permit could be a cause for a firm to terminate an employees' contract.

3 Empirical strategy

Our identification strategy leverages the fact that eligibility for extended paternity leave after the reforms under study was based on sharp cut-offs for children's birth dates. While this setup suggests the application of a sharp regression discontinuity design (RD), potential seasonality in births threatens causal identification. Indeed, previous research has shown that a child's month of birth is related to birth weight and early childhood outcomes (Marcotte and Engel, 2023). Children born in winter have poorer health and socio-economic outcomes (Buckles and Hungerman, 2013), attributable to poorer nutrition and higher incidences of disease (Currie and Schwandt, 2013). Birth seasonality has also been linked to socio-economic characteristics, with mothers conceiving in the first half of the year being significantly more likely to be of lower socio-economic status than those conceiving in the second half of the year (Currie and Schwandt, 2013; Buckles and Hungerman, 2013). Of particular relevance for the Spanish case is the fact that January 1st, the cutoff date for the 2017, 2020 and 2021 reforms, coincides with the school starting age cutoff and parents may strategically time births on the January side of the cutoff to avoid their child to be the youngest in the class. Additionally, only children born before May can start publicly funded childcare (for 0-3 year-olds) at the beginning of the academic year following birth (i.e. in September of the same calendar year they are born).

To control for potential seasonality in births, we employ a regression discontinuity differences-in-difference (RD-DD) design, that compares children born just below the cutoff dates to children born just above the cutoff, and uses children born in the same months but not affected by reforms (i.e. born in previous years) as a control group.[8] For each reform, we separately estimate equations of the form:

$$Y_{i,m,r} = \alpha_0 + \alpha_1 Cohort_{i,m,r} + \delta PostReform_{i,m,r} + \lambda_{i,m} + X_i'\sigma + \epsilon_{it} \tag{1}$$

where Y is the outcome of child i (or an outcome of the parent of child i), born in month m,

[8] Avdic and Karimi (2018), Raute et al. (2020) and Persson and Rossin-Slater (2023) use a very similar strategy to estimate the causal effect of paternity leave on marital stability, parental leave reform on paternity acknowledgment, and the effect of increased workplace flexibility for fathers on women's postpartum health outcomes, respectively.

belonging to the reform sample r, where $r \in \{2018, 2019, 2020, 2021\}$. The reform samples are described in more detail in Figure 1, which shows which children belong to the treatment and control group around each reform cutoff.[9] $Cohort$ is a dummy variable equal to one if the child belongs to the treatment cohort in reform sample r, meaning the child was born in the 12-month window around the cutoff date for the specific reform under study (6-month window for the 2019 reform). For instance, for the 2018 reform sample, where the cutoff date was 5th July 2018, the treatment cohort is comprised of children born between January and December 2018, and the control cohort is comprised of children born between January 2017 and December 2017. $PostReform$ is an indicator variable for children born after the paternity leave extensions (e.g., on or after July 5, 2018, for the 2018 reform sample), and our coefficient of interest is δ. It measures the difference in outcomes of children (or their parents) born after and those born before the cutoff date in the reform years, relative to the homologous difference in outcomes among children (or their parents) in non-reform years (i.e. the non-reform cohorts in Figure 1).

We control flexibly for the children's month of birth by including month of birth fixed effects ($\lambda_{i,m}$) and a vector of pre-determined characteristics (X_i'), which include the child's gender, mother's and father's age, mother's and father's education, dummies for whether the mother and father are Spanish, region of residence (NUTS-2) fixed effects, a dummy for same-sex couples, a dummy for whether the father was the main respondent to the survey, a dummy for being young in questionnaire and a dummy for being born mid-questionnaire[10]. We include these two latter controls to take into account the possibility that there are discontinuities in child development scores for children who are among the youngest to answer their age-specific questionnaire versus those who are the oldest to

[9]Note that we are unable to analyze the impact of the 2017 reform using the preferred RD-DD design. This is because, as it will become clearer in Section 4, our rich child development measures can only measure child development for children under age six. For the 2017 reform, we are unable to measure child development in a comparable way for a potential control (or non-reform) cohort, which would include births taking place from July 2015 to June 2016. For the 2017 reform, we will only report plain results using a plain RD design.

[10]For both father's and mother's education, we include the following categories capturing the maximum level of education reached: graduate degree (masters or PhD), undergraduate degree, further vocational education, Spanish Baccalaureate, basic vocational education, compulsory schooling, primary schooling, no schooling, and a further dummy that identifies whether the father's/mother's education is missing or not observed.

answer the same age-specific questionnaire.

As we will show in the next section, the discontinuous effects on take-up of paternity leave are concentrated in the last two reforms, in 2020 and 2021, while we find a positive trend but no discontinuous jumps on take-up for the earlier reforms. Thus, in a second specification we will focus on the last two reforms and estimate pooled effects using equations of the form:

$$Y_{i,m} = \alpha_0 + \alpha_1 Cohort^{2020}_{i,m} + \alpha_2 Cohort^{2021}_{i,m} + \delta PostReform^{2020,2021}_{i,m} + \lambda_{i,m} + X'_i\sigma + \epsilon_{it} \quad (2)$$

Y again is the outcome of child i (or an outcome of the parent of child i), born in month m for reform samples 2020 and 2021. $Cohort^{2020}$ and $Cohort^{2021}$ are dummy variables equal to one if the child belongs to the treatment cohort affected by the 2020 reform (born between July 1, 2019 and June 30, 2020) or by the 2021 reform (born between July 1, 2020 and June 30, 2021), respectively, allowing us to control for differences across the different treatment cohorts. The control cohort consists of children who were not affected by a paternity leave extension throughout the period, and were born between July 1, 2017 and June 30, 2018.[11] $PostReform$ is an indicator variable for children born after the paternity leave extensions (i.e., after January 1, 2020 or after January 1, 2021), and our coefficient of interest is δ. It measures the difference in outcomes of children (or their parents) born January-June and those born July-December in the reform periods, relative to the homologous difference in outcomes among children (or their parents) in the non-reform period.

Identifying assumptions As discussed above, the RD-DD design helps us net out seasonality in births and child outcomes between treated (reform) and control (non-reform) cohorts. Our key identifying assumption is that seasonality in child outcomes is the same in reform and non-reform years, requiring, for instance, that parents were not timing births (differently) during reform periods versus non-reform periods. Figure 2 shows a histogram of birth months of the children in our sample.

We first test for the presence of manipulation around the reform cut-offs using the approach proposed by Frandsen (2017) in the context of regression discontinuity designs

[11] That is, in this cohort all fathers could enjoy 4 weeks of paternity leave, and those were not compulsory.

with a discrete running variable (month of birth). This test relies only on support points at and immediately adjacent to the RD threshold when the running variable is discrete. We cannot reject the null hypothesis of absence of manipulation for the five reform thresholds (p-values of 0.888, 0.874, 0.268, 0.714 and 0.449, respectively).

Next, we implement a RD-DD version of a manipulation test of the running variable (month of birth), where we estimate regression models similar to Equation 1, with the dependent variable being the total number of monthly births, excluding the vector of control variables. The data on the number of births comes from the Spanish National Statistical Institute (INE, 2024). Given that we have monthly birth data available for many more years than child outcomes data, we include four control cohorts (non-reform years) in these estimations. We also include month of birth fixed effects and control cohort fixed effects. The results are shown in Table A1. The coefficient on the interaction between treatment (reform cohort) and being born in the post-reform period is the RD-DD estimator of the effect of reforms on the number of births. We find no significant jumps in the number of births for treated cohorts at the cutoff dates for any of the reforms that we analyze (i.e. those happening from 2018 onwards). While this gives reassurance regarding the absence of manipulation around the cutoff dates, as an additional robustness check we will show results also when excluding children born in December and January (doughnut estimator).

Identification additionally requires that any potential seasonality in observable or unobservable characteristics around the cutoff dates should not vary across reform and non-reform years. In Table A2 we therefore provide results of estimating Equation 1 without the vector of control variables X', where the dependent variables are pre-determined family or child characteristics. Out of the fifty coefficients presented for the five reforms, only five are statistically significant. F-tests of the joint hypothesis that for each reform sample all coefficients are simultaneously equal to zero lead us to fail to reject the null hypotheses of joint insignificance.

As discussed earlier, the effects on take-up of paternity leave are concentrated in the last two reforms, in 2020 and 2021, while we find no discontinuous effects on take-up for the earlier reforms. As a result, and since our main specifications will consider the pooled effect of the reforms implemented in 2020 and 2021, we perform the same exercise for the pooled sample of the 2020 and 2021 reforms in Table A3. Again, we cannot reject the null hypothesis of joint insignificance of all coefficients. Additionally, we show that characteristics for this sample are balanced when we reduce the time window around the cutoff dates to four months, and when we drop children born in December and January from our estimation sample. The former restriction, i.e. restricting the sample to those within a four month window of the cutoffs, is potentially important because the month of birth is crucial to be able to enter formal childcare: Only children born until April can apply to enter public childcare for 0-3 year-olds at the start of the next academic year (September) in each calendar year. Restricting our sample of analysis to those born within four months on either side of the cutoff therefore excludes births after April for the pooled 2020 and 2021 reform sample. The latter restriction is important because it allows us to exclude births that were potentially postponed (to January) in order to take advantage of more generous leave policies.

We first investigate whether the passing of the law(s) had the intended effects of increasing the number of weeks of leave taken by fathers in our sample. While one could think that leave-taking may not suffer from the same seasonality problems as births, as argued by Avdic et al. (2023), the institutional context in Spain, where fathers are obliged to take part of their leave right after childbirth and have to take all their leave entitlement before the child turns 12 months, warrants the use of the RD-DD approach also for this outcome.

Next, we study the impact of the reforms on several measures of child development, derived from an (age-standardized) index of child development based on the ASQ questionnaire. We also look at sub-indices for different developmental areas, as we explain in Section 4. In addition, we estimate regressions for potential mechanisms. These outcomes include time spent by the parents on childcare-related activities (reading, playing, putting to bed), household division of labor (in the home and outside the home), parental well-being, and variables measuring the use of formal and informal childcare.

4 Data

Our main data source is an online survey of 5,000 households with children under the age of six, conducted in February 2022 in Spain. The data collection was implemented online by the survey company Ipsos. The target population was men and women that were residing in Spain in February 2022, whose ages were comprised between 18 and 64, and with biological or adopted children under the age of six. Table A4 compares our survey sample to the equivalent sample in the Spanish Labor Force Survey collected by the Spanish Statistical Office during the first quarter of 2022. From this comparison, we observe that in our sample there is an over-representation of female respondents (63% vs 52%), respondents with at least a college degree (55% vs. 43%), Spanish-born individuals (93% vs 73%) and employed respondents (82% vs. 76%). All differences, except the age difference, are statistically significant. Thus, it will be important to include these controls in our empirical analysis. We also construct weights that will make our sample reproduce the characteristics in the Spanish Labor Force Survey in several important dimensions (whether the respondent is female, has above college education, is Spanish-born, is a single parent, and is employed). We will run robustness checks using these weights.

We collected information on take-up and length of maternity and paternity leave, as well as background characteristics of the family, labor market variables, parent-child interactions, and time dedicated to childcare and household chores.[12]

Our measures of child development come from the third edition of the Ages and Stages Questionnaires (ASQ-3, Squires et al. 2009). These are 21 age-specific questionnaires, designed to measure child development according to the age of the child. They are filled by parents and are one of the most widely used developmental screeners for babies, toddlers and young children. [13]

Answers in five different areas of development are aggregated into two indicators per

[12] The full questionnaire, excluding the ASQ questions, which are available only under license, is available on request.

[13] The UK National Health Service (NHS), for instance, uses the ASQ for their regular health and development reviews (health visitor checks) for babies until they are around 2 years old (NHS, 30 November 2023). They have also been used as child development outcomes in the economics literature. An example is Araujo et al. 2021, who use ASQ-3 to measure child development in an evaluation of a large-scale home visiting program in Peru.

area. The first indicates whether the child has severe problems in that area, leading to a “referral” recommendation, while the second one identifies potential difficulties, indicating that the area should be “monitored”. The five areas are communication, gross motor, fine motor, problem solving, and personal-social skills. Squires et al. (2009) provide age-specific cutoff scores for the referral as well as monitoring zones. The cutoff scores for the referral zone are two standard deviations below the mean in the area for the specific age group, and the monitoring zone cutoff is equal to one standard deviation below the mean in the area for the age group.[14]

We follow the ASQ-3 instructions and construct a continuous score for each developmental area, and classify each child into whether they score above the referral cutoff and whether they score above the monitoring cutoff. We then create two aggregate indicators per child that combine all five areas. The first indicator takes value one if the child is above the monitoring threshold in all areas, which we label “good progress”, meaning that the child exhibits no developmental delays and does not require monitoring in any of the areas. The second one indicates whether the child scores above the referral cutoff in all five dimensions. This means that the child may be in the monitoring zone for one or more developmental areas, but does not appear to have any severe developmental problems. We label this outcome "normal development". In our sample, 16% of children score below the referral cutoff in one area, and 7% are below the cutoff in two areas (see Table A5), in line with the targets provided in Squires et al. (2009), indicating that the tool diagnoses the expected levels of developmental delays as in the data used for calibration. We also use the total score achieved, by adding the sub-scores for each developmental area. Because the distribution of the total ASQ score is skewed to the right (the ASQ is designed to detect developmental delays and show more variation on the left tail of the distribution), we use the log of the total ASQ score as an additional outcome.

We use different sub-samples to analyze each of the reforms. The maximum window

[14]These cutoff scores were chosen because they were the most balanced in terms of the true positive and false positive proportions (Squires et al., 2009, p. 169).The choice of cutoff scores also implied that about 12-16% of the population of children in the calibration sample used by Squires et al. (2009) fell below the referral cutoff in one developmental area, and 2-7% in two or more areas. This is in line with target percentages based on data on developmental disabilities in young children from the U.S. Census Bureau and Centers for Disease Control and Prevention (Squires et al., 2009; U.S. Census Bureau, 2004; Cornell University, 2003-2009).

(in terms of months of birth) around each threshold is determined by the nearest reforms on both sides and the availability of a control cohort around the same window in a non-reform year. Additionally, we use administrative data from the Spanish Ministry of Social Security to study the duration of parental leave permits. This data covers all individuals who have taken leave permits over the period from January 2016 to June 2022.[15]

5 Results

Take-up of paternity leave We first show leave-taking behavior since 2016 graphically. Figure 3 shows the average length of maternity and paternity leave permits taken for births between January 2016 and June 2021. Figure 3a uses administrative data from the Spanish Ministry of Social Security on all permits taken. Maternity leave has remained stable and very close to 16 weeks for the whole period. The average length of paternity leave has followed the different extensions closely, from an average of two weeks in 2016 to close to 16 weeks for children born in and after January 2021. This shows that the reforms had the desired effect of increasing the length of paternity leave taken.[16] Figure 3b shows the average length of maternity and paternity leave among those who took parental leave, based on our own survey of 5,000 households. As with administrative data, the evolution in leave reported by the parents follows the leave extensions closely, but less pronounced. Our survey data shows a clear positive trend, with the average number of weeks taken by fathers for births in the spring of 2021 being around 14 weeks (the maximum is 16 weeks). There are several potential explanations for why the changes in our survey data are less marked than in administrative data. First, our data is naturally noisier as it is based on a sample of 5,000 households (not the universe of over 1 million permits in administrative data for each parent). Second, our survey data relies on recall and can therefore contain some amount of measurement error. On the contrary, administrative data relies on

[15]The data covers employed and self-employed people with singleton births, representing around 1.3 million permits taken by mothers and 1.4 million permits taken by fathers over the period from January 2016 to June 2022. We exclude multiple births (2% of full sample) and individuals working in special regimes (mainly agriculture, about 5% of full sample) and drop observations with implausible lengths of maternity/paternity leave, likely due to errors in administrative data records (2% of full sample).

[16]Given that from 2019 onward at least some portion of paternity leave was mandatory, the averages shown in the figure represent practically 100% of all eligible fathers from 2019 onward.

the data directly coming from the registers of the Social Security, linked to payments of leave. As such, the amount of measurement error is minimal. Third, differences between reported leave taken and leave taken according to administrative data may reflect differences in *de facto* leave taken and leave taken *on paper*. As described in Section 2, employees have strong financial incentives to take the leave, as they do not pay personal income tax on the benefit and the payments equal 100% of most individuals' net pay.[17] While workers are not supposed to attend their workplace during parental leave, those who can work from home might be officially on leave but working to some extent. To probe into this possibility, we will explore different aspects of behavior during leave, including whether individuals report to have been working, further below. Also, self-reported leave taken by fathers seems to be showing some seasonality, providing further support for using our RD-DD approach.

We next explore empirically to what extent more generous paternity leave permits led fathers to take more weeks of paternity leave after having a child, using self-reported data from our own survey. The results are shown in Table 2. The table shows the estimates of the δ coefficient from Equation 1, separately for each reform sample. In columns 1 and 2, the dependent variable is the total leave taken in weeks. This variable takes value zero for individuals who were not on leave, either because they did not take it up or because they were not eligible (i.e. not having a long enough work history to be eligible for the permit). The difference between Column 1 and Column 2 is that the latter introduces the full set of control variables (X') specified in Equation 1.

We find no statistically significant discontinuity of the leave extensions on the total number of weeks of leave taken for the reforms in 2018 and 2019.[18] However, the trends seen in Figure 3b are apparent from the increases in the dependent variable mean (which correspond to the mean in the treatment cohort before each reform). For the 2020 and 2021 reforms, we find significant increases of 1.8 and 2.2 weeks, respectively,

[17] According to administrative data, only about 5% of individuals receive the maximum parental leave benefit payment, and in these cases, firms often complement payments such that they equal the individual's regular net pay.

[18] As noted previously, we do not have a cohort of children in a non-reform year to study the 2017 reform using an RD-DD design. However, as we will explain further below, we show plain RD results for the 2017 reform in Tables A6 and A7 and find no significant increase in paternity leave length for the 2017 reform either.

when pre-determined controls are added in Column 2. Overall, the inclusion of these pre-determined controls affects only slightly both the point estimates and the standard errors.[19] These are intention-to-treat effects, as the sample includes both eligible and non-eligible fathers.

We next look at the total time taken off in the first six months after the birth of the child, reported in columns 3 and 4 of Table 2. This information allows us to explore to what extent fathers are effectively using the additional weeks of the permit to be at home with the child for longer, or whether they are replacing other forms of job absences, such as unpaid or paid annual leave. We find that only after the last reform of 2021, total time off increased significantly, by between 1.5 and 1.3 weeks, depending on the specification. Notice that while the coefficient estimates suggest that the additional leave entitlement has partially substituted other forms of leave, such that total time off has not increased by the same amount as total leave taken, we cannot capture the full effect because we only measure total time off until six months after birth, while big part of the paternity leave entitlement can be taken up to 12 months after childbirth.

Given that the effects on leave taking and time off are concentrated in the last two reforms, in Table 3, we show estimates of the δ coefficient from Equation 2 for the pooled 2020 and 2021 reform sample. Panel A shows coefficients for the full sample, using observations in a window of +/- six months around the cutoff date. Across the two reforms, leave taking by fathers increased by approximately two weeks (column 1), and this estimate is robust to including pre-determined control variables (column 2). Total time off in the first six months after birth increased by an average of approximately one week (column 3), and this estimate remains practically unchanged when including pre-determined controls (column 4). In Panel B, we check robustness of our results to reducing the window to +/- four months (between September and April) around the reform cutoff dates to take into account the fact that children born after April have a different likelihood of entering formal childcare in their first year of life due to admission rules for publicly subsidized childcare for children ages 0-3. Estimates are practically unchanged and suggest

[19] Figure 4 plots the raw averages separately for the 2020 (left) and 2021 (right) reform. A clear discontinuity can be seen around the reform threshold for each of the reforms.

the reforms increased leave taking by fathers by around 2.1 weeks and total time taken off in the first six months after birth by 0.8 weeks, using the specifications including controls (columns 2 and 4, respectively). In Panel C, we keep the window of analysis at +/- six months, but exclude January and December births to account for potential manipulation of births around the cutoff on January 1st. Estimates are very similar for this sub-sample. In Panel D, we additionally provide robustness checks where we include a trend in the running variable (rather than month of birth fixed effects) and an interaction between the cutoff and the running variable. Coefficient estimates are consistent with our main specification (Panel A) albeit slightly higher in magnitude. Rather than imposing a linear trend for the evolution of take-up, we see the specification that has been widely used in this literature (with month of birth fixed effects), as our preferred specification. In Panel E we show estimates when we use weights computed to replicate several characteristics observed in the analogous Spanish LFS population with children under age six (see Section 4). Again, these results are very similar to our main results in Panel A.

Other than the seasonality in paternity leave-taking patterns already seen in Figure 3b, the use of the RD-DD design is further justified when we compare the RD-DD results with the plain RD results (shown in Table A6). Even if results are in the same direction, the RD results show much larger magnitudes and significant effects, especially for the 2021 reform, and for the total time off in the first six months after birth, than when seasonality patterns are taken into account in the RD-DD setting.

All in all, we conclude from our take-up analysis that the 2020 and 2021 reforms, which extended paternity leave by four weeks each, led to significant jumps in the duration of leave taken at the reform thresholds. We do not find statistically significant jumps at the threshold for the earlier reforms for fathers in our sample.

Effects on child development We now analyze the effects of increased paternity leave entitlement on child development. The first outcome we study, “Normal development”, is an indicator that takes value one if the child is above the referral zone in all the five areas of development tested in ASQ-3, and zero otherwise. Thus, a value of one indicates that the child presents no sign of severe developmental problems. The second measure, "Good progress" is an indicator for being above the monitoring zone in all areas. It takes value one if the child does not show signs of either moderate or severe developmental delays in any of the areas in the ASQ, and zero otherwise.

Our analysis of paternity leave uptake revealed significant discontinuities around the 2020 and 2021 reform thresholds only. Therefore, we focus our analysis of child development on the impacts of the last two reforms. In Table 4, we show estimation results when we pool the 2020 and 2021 reform samples (Equation 2). Panel A shows results for the full sample, using a window of +/- 6 months around the cutoff dates. The specifications without (column 1) and with pre-determined controls (column 2) show very similar effect sizes. Children whose fathers were potentially eligible for extended paternity leave are 12.4 percentage points less likely to exhibit normal development for their age. We find no impact on either the likelihood of exhibiting good progress or the log of the total ASQ score. This means that negative effects are concentrated on the left tail of the child ability distribution, without altering average child development measured in our sample of children.[20] When restricting the time window around the reform cutoff to +/- four months (Panel B), results remain qualitatively the same, with slightly larger coefficient estimates: We find a significant negative impact of the extensions on the fraction of children with normal development, and no effect on the fraction with good progress or the log ASQ score. Results also remain robust to the exclusion of January and December births (Panel C), although coefficient estimates are smaller, and to the alternative specification that includes a trend in the running variable and its interaction with the cutoff (Panel D). Results are also robust when we use weights in order to mimic several key dimensions of the Spanish LFS (Panel E). [21]

[20]For completeness, Table A8 shows results estimated separately for each of the four reforms. Across all four reform samples, between 68% and 76% of children show normal development as per our definition. For the reforms in 2018 and 2019, we find that exposure to longer paternity leave did not lead to a change in the fraction of children with normal development. For the first two reforms we analyze, the only statistically significant effect is for the 2018 reform, with a reduction in the share of children exhibiting good progress by about 10 percentage points. We find that none of the reforms had an impact on average log ASQ score. That is, out of the 6 regressions with pre-determined controls, only 1 shows significant effects. This is reassuring for our identification strategy given that we did not observe significant discontinuities in take-up for those three reforms.

[21]For completeness, and as for the take-up results, we also show RD results of paternity leave extensions on child development in Table A7.

The reform of January 2020 could have affected children born above or below the threshold in a different way due to the Covid-19 pandemic. For instance, parents of those born in the first months of 2020 are most likely on paternity/maternity leave when lockdown was imposed in Spain, while parents of those born in the last months of 2019 might have already returned to work at the time of the lockdown in March 2020. This is not the case for those children born around the 2021 reform cutoff. It is therefore reassuring to see that if we test the impacts of these reforms separately, results are not driven by the 2020 reform. The last two panels of Table A8 show that the 2020 paternity leave extension decreased the fraction of children exhibiting normal development by about 14 percentage points (about 20% of the sample mean). The results are very similar for the 2021 extension, with a decrease in the fraction that show normal development by 11.2 percentage points. Graphically, the raw data for the 2020 and 2021 reform are shown in Figure 6, which plots the average share of children classified as having normal development by month of birth relative to the cutoff dates.

Taken together, these results suggest that the last two reforms, which were also the ones with the largest increases in paternity leave (from 8 to 12 and 12 to 16 weeks, respectively), significantly increased the fraction of children with developmental delays.

Our two aggregate measures combine the results in five different developmental areas. Table 5 shows the results for each of those areas separately, for the specification where we pool the 2020 and 2021 reform samples. We find that the paternity leave extensions had negative impacts on all but one area (problem solving), with significant negative effects found for gross motor, personal social, and communication skills. These results are robust to changing the window of analysis to +/- four months around the cutoff (Panel B) and using computed weights to mimic several characteristics in the LFS Spanish population with children under six (Panel E). When we exclude December and January births (Panel C) or when we include the trend in the running variable (Panel D), coefficient estimates on communication and personal social skills remain negative but are smaller in magnitude and less precise.

Overall, our results suggest that the extensions of paternity leave permits in Spain had negative effects on child development at the lower end of the child ability distribution.

These negative effects seem to be driven by a deterioration in gross motor, communication, and personal social skills. We next analyze potential channels driving these findings.

6 Mechanisms

In this section, we explore potential mechanisms that may drive our estimated negative effects of paternity leave on early child development. We first investigate how the different extensions affected other aspects of parental leave taking behavior. Next, we examine longer-term outcomes measured at the time of the survey. These include parent-child interactions, the division of labor in the household and measures of family well-being. Finally, we study how the reforms affected the use of different childcare arrangements. Given that we only find a positive effect on take-up and negative effects on child outcomes for these reforms, we focus our analysis of mechanisms on the pooled 2020 and 2021 reform samples. Because we study many outcome variables, which raises the risk for false positives, we adjust for the fact that we are testing multiple hypotheses and may incorrectly reject null hypothesis of no effects. To do so, we calculate Romano-Wolf step-down adjusted p-values for five families of outcome variables: (1) take-up of parental leave; (2) parent-child interactions; (3) household division of labor; (4) parental well-being; and (5) use of childcare.

Our take-up results indicate that exposure to longer paternity leave led fathers to spend more time off from work during the first year of the child's life. We now explore in more detail how fathers used this time and whether maternal leave taking was also affected. This is important because the leave extension did not only increase the total time fathers could take off after childbirth, but it also altered the possibilities with respect to how exactly additional leave entitlements could be used: Some of the additional leave had to be taken simultaneously with the mother, but the reforms also significantly increased the number of weeks that could potentially be taken non-concurrently with the mother (i.e., after the mother had already returned to work). For instance, both the 2020 and the 2021 reform increased overall leave entitlements by four weeks, but out of these, only two had to be taken at the same time as the mother (right after birth), while the remaining two weeks could be taken at any time before the child turned 12 months.[22] Furthermore, in 2020, fathers could still transfer two weeks out of their total leave entitlement to the mother, raising the possibility of increased maternity leave after the extension. Finally, fathers could also take leave on a part-time basis, so that additional leave might not have translated in children being alone at home exclusively with the father.

Results are summarized in Panel A of Table 6. First, we find that while reforms had a significant impact on fathers' leave-taking, they did not significantly increase the number of weeks of leave taken by mothers.[23] Second, out of the two additional weeks fathers spent at home, about 0.8 weeks were solo time of the father with the child, while 1.2 weeks was time shared with the mother. So while about 60% of the additional leave entitlement was used at the same time as mothers were on leave, children in treated cohorts were exposed to about one additional week alone with their fathers after the extensions. Overall, we find that the total amount of leave taken by both parents increased by nearly three weeks, but when excluding overlaps between maternal and paternal time at home, this increase amounted to only 1.6 weeks. These results indicate that slightly more than half of the extra time at home of fathers in the initial months after birth was replacing *exclusive* maternal time, and slightly less than half of the extra time at home was substituting other forms of childcare the child would otherwise have received. All of these results are robust to multiple hypothesis testing.

Additionally, most of the additional weeks of leave were taken on a full-time basis (as opposed to part-time). Pre-reform, 26% of fathers report that they spent some time working during their official period of leave, and this fraction increases by about 5 percentage points after the extensions, although this coefficient is not significant at standard levels. For mothers, we find a zero effect of paternity leave extensions on the likelihood of working while on leave. This suggests that additional leave entitlements might not have translated fully into fathers spending more time with children, but engaging in other (pro-

[22] In January 2020, the number of initial compulsory weeks increased from two to four weeks, meaning that the additional leave entitlement of four weeks had to be partially taken right after birth, and the remaining two weeks could be taken at any time during the first 12 months of life of the child. In January 2021, the initial compulsory weeks increased from four to six, again meaning that fathers could use only two out of their additional four weeks of entitlement at any point before the child turned 12 months.

[23] These results mean that fathers were not using the possibility of transferring two weeks of their paternity leave to mothers. This possibility was only in place until December 2020.

ductive) activities while on leave.[24]

Previous research has documented that paternity leave may increase fathers' involvement in childcare beyond the leave period (Farré and González, 2019). Panel B of Table 6 investigates this possibility, by analyzing outcomes related to parent-child interactions when treated children were on average 20 months old. We construct indicator variables equal to one if the father (mother) reads, plays, or puts the child to bed once a week or more often, and zero if less than once a week. We find that fathers who were exposed to the reforms were 7.2 percentage points less likely to read often and 3.2 percentage points less likely to play often with their child. Interestingly, we also find a reduction by 8.6 percentage points in the likelihood of mothers reading often with their child. However, none but the results on the decrease in maternal reading are robust to multiple hypothesis testing.

We also test whether the overall division of labor in the household has changed in response to the reforms. This is reported in Panel C of Table 6. The first outcome measures the share of childcare activities performed by the father, while the second measures the share of household chores performed by the father.[25] We do not find that paternity leave extensions changed fathers' overall involvement in childcare, but it significantly decreased their involvement in household chores by 3.4 percentage points, compared to a pre-reform mean in the cohort born around the reform cutoff of 39 percent. However, this result is not robust to multiple hypothesis testing.

Overall, these results suggest that the reforms had no impact on fathers' involvement in childcare. If anything, there is some suggestive evidence that fathers perform a lower share of household chores and seem to be involved less frequently in activities such as reading or playing with their children (even though these results are not robust to correcting for multiple hypothesis testing). This is consistent with findings in Ekberg et al. (2013), who estimate the impact of the introduction of 30 days of earmarked paternity

[24] Antecol et al. (2018), for instance, show that gender neutral tenure clock stopping policies increase male publishing in top journals and male tenure rates.

[25] Derived from how childcare and household tasks are typically shared in the couple. Possible answers are translated into a numerical scale as follows: *Always me: 100%; I do much more: 85%; I do somewhat more: 75%, equally shared: 50%; my partner does somewhat more: 25%; my partner does much more: 15%; Always my partner: 0%*.

leave in Sweden in 1995. They show that the reform did not increase fathers' shares of leave taken to care for sick children, which they use as a measure for household work. However, it is in contrast to Farré and González (2019), who do find an increase in fathers' involvement in childcare after the introduction of two weeks of paid paternity leave in Spain in 2007. It may well be the case that the effects of successive extensions of leave are non-linear, and that leave *extensions* have different effects than the *introduction* of paternity leave for the first time. Overall, the suggestive evidence of a decrease in the involvement in activities with children by both fathers and mothers is consistent with the increase in developmental delays we identify in response to the leave extensions.

We also look at effects of paternity leave reforms on market work. If the leave extension increased maternal labor supply, such a reduction in time spent at home caring for children by mothers might be a channel driving the negative effects on child outcomes we identify. We find no effect of leave extensions on the likelihood of either parent working at the time of the survey, when treated children were on average 20 months old. We find neither an effect on working hours for mothers nor fathers. This is in line with findings in Avdic et al. (2023), who study the same Swedish reform in 1995 as Ekberg et al. (2013), and find no long-run increase in mothers' labor supply. We also look at the number of hours fathers and mothers work from home as an outcome. The hypothesis is that if fathers take on more responsibility in household or childcare tasks, they might do so by increasing working from home, enabling them to better reconcile family and work duties. This way they might be able to take over what would otherwise have been maternal time with kids or formal childcare time, beyond the initial period of parental leave. However, the reforms had no impact on the number of hours fathers work from home. They also did not change mothers' hours worked from home. Overall, we conclude that the reforms did not affect labor market results in the medium run of either fathers or mothers. However, evidence from Gorjón and Lizarraga (2024) using administrative data suggests that after the 2021 extension of paternity leave, mothers were less likely to request work hour reductions during the first year of the child's life, suggesting that the increased presence of fathers might have reduced maternal time with children after maternity leave had ended during this period. Although we do not measure working hours in the first year of the child's life (only at the time of the survey), this can be a potential explanation for the negative effects we find on child outcomes.

Research by Avdic et al. (2023) and Olafsson and Steingrimsdottir (2020) has shown that leave extensions can increase the likelihood of parental separation and, through reduced availability of fathers to their children, negatively impact child outcomes. We test whether this could potentially explain our results by looking at an indicator of whether the natural parents of the child are separated, which affected about 7 percent of our treatment cohort born before the reform cutoff. We find no impact of the 2020 and 2021 leave extensions on the likelihood of parental separation (Panel D of Table 6).[26] We do find, however, that fathers who had children after the reform cutoff dates exhibit lower levels of subjective well-being. This deterioration might explain the decrease in the frequency of father-child interactions, and could negatively affect the quality of these interactions. Interestingly, mothers who had children after the reform cutoff dates show higher subjective well-being as those who had children just before. While this is unlikely an explanation for the negative effects we find on child development, it is in itself an interesting finding and in line with improved maternal (physical) health reported in Persson and Rossin-Slater (2023) in response to the introduction of 30 days of paternity leave in Sweden that could be taken concurrently with mothers.

As noted by Avdic et al. (2023), the quality of care parents offer, compared to alternative care-giving arrangements, is crucial for understanding whether longer parental leave benefits children. In Panel E of Table 6 we study the extent to which the increase in paternal leave taking was accompanied by a change in the use of other types of childcare. We find a significant and large drop by 16.6 percentage points in the likelihood of attending formal childcare (at the time of survey), compared to a pre-reform mean of 74 percent in the treatment cohort. The average number of weekly hours in formal childcare decreased by nearly four (compared to a mean for children in treatment cohorts born before the cutoffs of 17.3 hours). This drop in the use of formal childcare was not accompanied by an increase in the usage of other types of informal arrangements, as measured

[26] It could also be that separations have not yet had time to materialize, but that the level of conflict in the household has increased. We did not collect data to measure conflict in the household, but the results on mother's subjective well-being do not seem to support the hypothesis that conflict might have increased.

by the number of different childcare modes used, and it did not increase the likelihood of using childminders, grandparents or other modes of informal care. Anecdotal evidence suggests that more and more families time their parental leave entitlement to postpone their offspring's entry into formal childcare. Analysis of administrative data for Spain in Farré et al. (2024) also supports the idea that an increasing number of fathers are splitting their leave entitlement (over 50% in January 2022, as shown in administrative Social Security data). They are taking the non-mandatory part of their leave separately from the mother, after she has already returned to work. This trend increases the total time the child spends at home with at least one of its parents. In Table A9, we report the results on childcare use separately for the last two reforms. The effects are very similar for the 2020 and 2021 reforms, and hence, do not seem to be driven by the 2020 reform cohort, which was potentially affected by nursery school closures due to Covid-19.

Overall, our results suggest that the leave extensions resulted in a replacement of solo maternal care and formal childcare with mixed or solo paternal care at home. In the case of mothers, it has been shown that short periods of job-protected leave can be beneficial in terms of infant health outcomes (Rossin, 2011), and paid maternity leave has been shown to positively affect long-run health outcomes (Le and Nguyen, 2022). Research studying the effect of early childcare (ages 0-2) has found that its impact varies depending on the quality of counterfactual parental care that is being replaced (see for instance Fort et al. (2020), which includes a review of the existing evidence). The take-away from these studies is that early childcare attendance tends to favor cognitive and non-cognitive outcomes for children of disadvantaged families, while it can negatively impact those of children from more affluent families. This suggests that our overall negative effects on child outcomes might hide heterogeneity with respect to parental education.

In Table 7 we probe into this possibility by estimating Equation 2 augmented with an interaction between the $PostReform$ indicator and a dummy indicating whether the father or the mother held a college degree or higher level of education. First, we find that the increase in paternity leave taken is slightly higher for families in which the mother has at least a college degree. Second, there is no difference in the increase in leave-taking between highly and less educated fathers, meaning that the reform did not increase leave-taking differently for fathers of different levels of education. When looking at child development outcomes, we observe that negative effects on child development are driven by children of highly educated mothers, while fathers' education does not seem to matter. In terms of the drop in formal childcare usage, this seems to be driven by children of less educated parents. Taken together, these results suggest that our results might be driven by a combination of replacement of high quality maternal care initially and high quality formal childcare in the medium run with lower quality paternal care. When looking at heterogeneous effects on working during leave by parental education level, we find that fathers with college education or above significantly increased the likelihood of working while on leave by nearly 10 percentage points after the last two reforms, thus supporting the hypothesis that fathers may not be fully focused on childcare activities while on leave, but engage in other (productive) activities, which could contribute to the negative effects.

We also test whether birth order might matter for the effects on child outcomes. If the child affected by leave extensions is a second or later-born, then increased paternity leave might allow mothers to spend more time with older kids while delegating care for the new-born to the father. The third Panel of Table 7 suggests that additional leave-taking did not depend on the child's birth order, but that the negative effects on child development are only about half the size among first-born children than among children of higher parity, supporting our hypothesis.

Finally, we check whether the negative effect on child development varies by the gender of the child. Some studies in the economics of education and child development have shown that parental investments vary according to the gender of the child. For instance, Baker and Milligan (2016) show that, at early ages, both fathers and mothers spend more time in learning activities (reading, singing, teaching of letters and numbers) with girls than with boys; and this is especially the case in *broken* families (i.e. families where the head of the household is a single mom) (Bertrand and Pan, 2013). We show whether our results are heterogeneous with respect to child's gender in the last panel of Table 7, but find no differences when it comes to leave-taking or child development outcomes.

7 Conclusions

We study the effects of paternity leave on early child development by exploiting several extensions that took place in Spain between 2017 and 2021. We conduct a survey of 5,000 families with children under six years old, and follow a RD-DD design based on birth dates (which determined eligibility for the different extensions), using children born in non-reform years as control cohorts.

We first show that the most recent (and most generous) paternity leave extensions in 2020 (from 8 to 12 weeks) and 2021 (from 12 to 16 weeks) resulted in fathers taking longer leave during the first year after childbirth. We then show that children whose fathers were potentially eligible for extended paternity leave are 12 percentage points more likely to exhibit developmental delays for their age. The negative effects are concentrated on the left tail of the child development distribution.

We examine potential mechanisms and find that the extended weeks of paternity leave reduced the time that children spent alone with their mother in the early months, and increased the time they spent with their father and mother together, as well as the time alone with the father in their first year of life. Moreover, children whose fathers were eligible for increased paternity leave were less likely to attend formal childcare during early childhood.

We conclude that the effects of parental leave policies on children depend crucially on the counterfactual modes of childcare. Our findings underscore the importance of interventions aimed at promoting parenting skills (especially among fathers), as well as the role that early formal childcare may play in detecting and/or mitigating early developmental delays.

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Tables

Table 1
Maternity and paternity leave regulations in Spain

Period	Maternity leave	Paternity leave	Pay
Before 24 March 2007	**16 weeks**, 6 first weeks mandatory right after birth, **10 remaining weeks can be shared with father**	no paternity leave	100% of salary (mothers) up to 2,996.10€
From 24 March 2007	**16 weeks**, 6 first weeks mandatory right after birth, **10 remaining weeks can be shared with father**	**2 weeks**, must be taken at once, any time before the end of 16 weeks of maternity leave, **cannot be shared with mother**	100% of salary up to 3,642.00€
From 1 Jan 2017	**16 weeks**, 6 first weeks mandatory right after birth, **10 remaining weeks can be shared with father**	**4 weeks**, must be taken at once, any time before the end of 16 weeks of maternity leave, **cannot be shared with mother**	100% of salary up to 3751.20€.
From 5 Jul 2018	**16 weeks**, 6 first weeks mandatory right after birth, **10 remaining weeks can be shared with father**	**5 weeks**, 4 of which must be taken at once any time before the end of 16 weeks of maternity leave, last week can be taken at different time and either full- or part-time*, any time before child turns 9 months, **cannot be shared with mother**	100% of salary up to 3,803.70€
From 1 Apr 2019	**16 weeks**, 6 first weeks mandatory right after birth, **4 weeks can be shared with father**	**8 weeks**, 2 weeks mandatory** and immediately after birth, the remainder at any time before child turns 12 months, can be taken full- or part-time; **4 weeks can be shared with mother**	100% of salary up to 4,070.10€
From 1 Jan 2020	**16 weeks**, 6 first weeks mandatory right after birth, **2 weeks can be shared with father**	**12 weeks**, 4 weeks mandatory and immediately after birth, the remainder at any time before child turns 12 months, can be taken full- or part-time; **2 weeks can be shared with mother**	100% of salary up to 4,070.10€
From 1 Jan 2021	**16 weeks**, 6 first weeks mandatory right after birth, **cannot be shared with father**	**16 weeks**, 6 weeks mandatory and immediately after birth, the remainder at any time before child turns 12 months, can be taken full- or part-time; **cannot be shared with mother**	100% of salary up to 4,070.10€

Source: Boletin Oficial de Estado (2007, 2009, 2018, 2019).
Notes: *Until 5 July 2018, paternity leave had to be taken at once and could not be split and taken at different time periods. **Parental leave for fathers became mandatory in 2019 with Royal Decree-Law 6/2019, of March 1, on urgent measures to guarantee equal treatment and opportunities between women and men in employment and occupation, which came into force on April 1, 2019.

Table 2
Paternity leave extensions and leave take-up

	Total leave (weeks)		Total time off (weeks)	
	(1)	(2)	(3)	(4)
PostReform$_{2018}$	0.726*	0.571	0.221	0.284
	(0.424)	(0.429)	(0.413)	(0.420)
Mean dep. var	4.40		3.71	
SD dep. var.	4.62		4.68	
Obs.	1,893		1,893	
PostReform$_{2019}$	0.275	0.307	0.536	0.608
	(0.639)	(0.668)	(0.643)	(0.650)
Mean dep. var	6.00		4.79	
SD dep. var.	5.40		5.42	
Obs.	992		992	
PostReform$_{2020}$	1.893***	1.809***	0.699	0.486
	(0.490)	(0.490)	(0.510)	(0.512)
Mean dep. var	7.25		5.57	
SD dep. var.	5.52		5.87	
Obs.	1,879		1,879	
PostReform$_{2021}$	2.367***	2.222***	1.541***	1.307**
	(0.547)	(0.543)	(0.596)	(0.592)
Mean dep. var	9.13		6.96	
SD dep. var.	6.05		6.76	
Obs.	1,767		1,767	
Controls	No	Yes	No	Yes

Notes: Significance levels are indicated by * $<$ .1, ** $<$ .05, *** $<$ .01. RD-DD estimates (Equation 1) of the effect of paternity leave extensions on take-up of paternity leave. Reform and non-reform cohorts as defined in Figure 1. Means and standard deviations calculated across reform cohorts in pre-reform period. Specifications with controls include the following set of variables: gender of the child, dummies for father's and mother's level of education, separate dummies for whether the father and mother is Spanish-born, mother's and father's age, a dummy for whether the couple is opposite sex, region fixed-effects, a dummy for whether the father was the main respondent, a dummy equal to 1 if the child was born in latest month of birth within the ASQ-3 questionnaire appropriate for their age at the time of survey and an indicator for being born in latest month of birth (minus 1 month) within a given questionnaire. All specifications include month of birth fixed effects, as shown in Equation 1. "Total weeks leave" is the total number of full-time equivalent weeks of paternity (maternity) leave taken by the father (mother), including zeros for those that did not take any leave and those that were not eligible for paternity (maternity) leave at the time of birth. "Total time off in first 6 months" is the total number of weeks taken off in the first six months after the birth of the child, including unpaid leave and annual leave and zeros for those that did not take any time off and those that were not working at the time of birth.

Table 3
Effect of 2020-21 reforms on paternity leave take-up

	Total leave (weeks)		Total time off (weeks)	
Panel A: RD-DD full sample				
$\text{PostReform}_{20,21}$	2.108***	2.058***	1.095**	0.945**
	(0.418)	(0.417)	(0.435)	(0.438)
Mean dep. var.	8.08		6.19	
SD dep. var.	5.84		6.31	
Obs.	2,649		2,649	
Panel B: RD-DD +/- 4 months				
$\text{PostReform}_{20,21}$	2.126***	2.142***	0.985**	0.837*
	(0.452)	(0.450)	(0.478)	(0.479)
Mean dep. var.	8.18		6.26	
SD dep. var.	5.73		6.36	
Obs.	2,241		2,241	
Panel C: RD-DD excl. Dec & Jan births				
$\text{PostReform}_{20,21}$	2.320***	2.407***	1.264**	1.344**
	(0.523)	(0.518)	(0.537)	(0.547)
Mean dep. var.	7.96		6.05	
SD dep. var.	5.88		6.15	
Obs.	1,715		1,715	
Panel D: RD-DD trend in running var				
$\text{PostReform}_{20,21}$	3.200***	2.862***	2.320***	2.054***
	(0.640)	(0.626)	(0.676)	(0.672)
Mean dep. var.	8.08		6.19	
SD dep. var.	5.84		6.31	
Obs.	2,649		2,649	
Panel E: Using LFS weights				
$\text{PostReform}_{20,21}$	2.464***	2.376***	1.273**	1.193**
	(0.554)	(0.521)	(0.574)	(0.554)
Mean dep. var.	7.32		5.53	
SD dep. var.	5.86		6.14	
Obs.	2,649		2,649	
Controls	No	Yes	No	Yes

Notes: Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$. RD-DD estimates (Equation 2) of the effect of paternity leave extensions on take-up of paternity leave for the 2020 and 2021 reform. Means and standard deviations calculated across reform cohorts in pre-reform period. *Panel A:* Full sample. Reform cohorts: born between July 2019 and June 2020 (Jan 1st 2020 reform) and between July 2020 and June 2021 (Jan 1st 2021 reform). Non-reform cohort: born between July 2017 and June 2018. *Panel B:* Reform cohorts: born between September 2019 and April 2020 (Jan 1st 2020 reform) and between September 2020 and April 2021 (Jan 1st 2021 reform). Non-reform cohort: born between September 2017 and April 2018. *Panel C:* As Panel A, but excluding births in January 2018, 2020 and 2021, and December 2017, 2019 and 2020. *Panel D*: Specification includes a linear trend in the running variable plus its interaction with the cutoff. *Panel E*: Same as in *A* but using weights derived from LFS sample. Dependent variables defined as: "Total leave": total number of full-time equivalent weeks of paternity leave taken, including zeros for those that did not take any leave and those that were not eligible for paternity leave at the time of birth. "Total time off": total number of weeks taken off in the first six months after the birth of the child, including unpaid leave and annual leave, including zeros for those that did not take any time off and those that were not working at the time of birth. Means and standard deviations calculated across reform cohorts in pre-reform period (i.e., average among children born between July 2019 and December 2019 and between July 2020 and December 2020). Specifications with controls include the same variables as specified in the notes to Table 2.

Table 4
Effects of 2020-21 paternity leave extensions on child development

	Normal development		Good progress		ASQ score (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: RD-DD full sample						
$PostReform_{20,21}$	-0.131^{***}	-0.124^{***}	-0.001	0.007	-0.007	-0.003
	(0.036)	(0.036)	(0.039)	(0.039)	(0.027)	(0.026)
Mean dep. var.	0.70		0.34		5.37	
SD dep. var.	0.46		0.47		0.27	
Obs.	2,649		2,649		2,649	
Panel B: RD-DD +/- 4 months						
$PostReform_{20,21}$	-0.172^{***}	-0.145^{***}	-0.034	-0.005	-0.035	-0.019
	(0.039)	(0.040)	(0.042)	(0.043)	(0.029)	(0.029)
Mean dep. var.	0.70		0.35		5.38	
SD dep. var.	0.46		0.48		0.25	
Obs.	2,241		2,241		2,241	
Panel C: RD-DD excl. Dec & Jan births						
$PostReform_{20,21}$	-0.100^{**}	-0.078^{*}	0.015	0.030	-0.004	0.009
	(0.044)	(0.045)	(0.049)	(0.049)	(0.032)	(0.032)
Mean dep. var.	0.72		0.35		5.38	
SD dep. var.	0.45		0.48		0.27	
Obs.	1,715		1,715		1,715	
Panel D: RD-DD trend in running var						
$PostReform_{20,21}$	-0.076	-0.102^{**}	-0.052	-0.079	0.015	-0.004
	(0.048)	(0.049)	(0.048)	(0.049)	(0.030)	(0.030)
Mean dep. var.	0.70		0.34		5.37	
SD dep. var.	0.46		0.47		0.27	
Obs.	2,649		2,649		2,649	
Panel E: Using LFS weights						
$PostReform_{20,21}$	-0.171^{***}	-0.167^{***}	-0.057	-0.055	-0.034	-0.033
	(0.049)	(0.047)	(0.053)	(0.050)	(0.033)	(0.031)
Mean dep. var.	0.69		0.36		5.38	
SD dep. var.	0.46		0.48		0.27	
Obs.	2,649		2,649		2,649	
Controls	No	Yes	No	Yes	No	Yes

Notes: Significance levels are indicated by * $<$ .1, ** $<$ .05, *** $<$ .01. RD-DD estimates (Equation 2) of the effect of paternity leave extensions on take-up of paternity leave for the 2020 and 2021 reform. Means and standard deviations calculated across reform cohorts in pre-reform period. Panels as defined in the notes to Table 3. Dependent variables defined as: "Normal development": dummy variable equal to one if the child scored above the referral cutoff in all areas. "Good progress": dummy variable equal to one if the child scored above the monitoring cutoff in all areas. "ASQ Score (log)": Logarithm of total score on ASQ-3 questionnaire (minimum 0, maximum 300). Means and standard deviations calculated across reform cohorts in pre-reform period (i.e., average among children born between July 2019 and December 2019 and between July 2020 and December 2020). Specifications with controls include the same variables as specified in the notes to Table 2.

Table 5
Effects of 2020-21 reforms on normal development by area

	(1) Communi-cation	(2) Gross motor	(3) Fine motor	(4) Problem solving	(5) Personal social
Panel A: RD-DD full sample					
PostReform$_{20,21}$	-0.061^{***}	-0.137^{***}	-0.025	0.022	-0.063^{***}
	(0.021)	(0.030)	(0.025)	(0.023)	(0.024)
Mean dep. var.	0.94	0.88	0.86	0.86	0.91
SD dep. var.	0.24	0.32	0.34	0.34	0.29
Obs.	2,649	2,649	2,649	2,649	2,649
Panel B: RD-DD +/- 4 months					
PostReform$_{20,21}$	-0.057^{**}	-0.170^{***}	-0.038	0.030	-0.073^{***}
	(0.023)	(0.033)	(0.028)	(0.026)	(0.027)
Mean dep. var.	0.94	0.88	0.87	0.86	0.91
SD dep. var.	0.24	0.32	0.34	0.35	0.28
Obs.	2,241	2,241	2,241	2,241	2,241
Panel C: RD-DD excl. Dec & Jan births					
PostReform$_{20,21}$	-0.030	-0.092^{***}	-0.001	0.006	-0.041
	(0.025)	(0.036)	(0.030)	(0.027)	(0.030)
Mean dep. var.	0.95	0.90	0.88	0.88	0.91
SD dep. var.	0.22	0.31	0.33	0.32	0.29
Obs.	1,715	1,715	1,715	1,715	1,715
Panel D: RD-DD trend in running var					
PostReform$_{20,21}$	-0.030	-0.079^{**}	-0.013	-0.049	-0.025
	(0.026)	(0.037)	(0.036)	(0.034)	(0.032)
Mean dep. var.	0.94	0.88	0.86	0.86	0.91
SD dep. var.	0.24	0.32	0.34	0.34	0.29
Obs.	2,649	2,649	2,649	2,649	2,649
Panel E: Using LFS weights					
PostReform$_{20,21}$	-0.085^{***}	-0.184^{***}	-0.041	0.058^{**}	-0.100^{***}
	(0.031)	(0.036)	(0.031)	(0.027)	(0.035)
Mean dep. var.	0.93	0.90	0.87	0.85	0.91
SD dep. var.	0.26	0.30	0.34	0.36	0.29
Obs.	2,649	2,649	2,649	2,649	2,649

Notes: Significance levels are indicated by $^* < .1$, $^{**} < .05$, $^{***} < .01$. RD-DD estimates (Equation 1) of the effect of paternity leave extensions on the probability of scoring above the referral cutoff (normal development) in the area indicated in the column header for the 2020 and 2021 reform. Means and standard deviations calculated across reform cohorts in pre-reform period. Treated cohorts are those born between July 2019 and June 2020 (Jan 1st 2020 reform) and those born between July 2020 and June 2021 (Jan 1st 2021 reform). The control cohort is composed of children born between July 2017 and June 2018. Means and standard deviations calculated across reform cohorts in pre-reform period (i.e., average among children born between July 2019 and December 2019 and between July 2020 and December 2020). All specifications include the full set of controls as defined in the notes to Table 2.

Table 6
Effects of 2020-21 reforms: Mechanisms

	$\text{PostReform}_{20,21}$	S.E.	Romano-Wolf p-value	Dep. var. mean	Obs.
Panel A: Take-up of parental leave					
Total leave (father)	2.058***	(0.417)	[0.000]	8.08	2,649
Total leave (mother)	0.827	(0.642)	[0.539]	11.86	2,649
Full-time leave alone (father)	0.803**	(0.357)	[0.118]	3.59	2,649
Full-time leave (father) with mother	1.210***	(0.385)	[0.010]	4.74	2,649
Total leave (mother & father) incl. overlaps	2.885***	(0.830)	[0.003]	19.94	2,649
Total leave (mother & father) excl. overlaps	1.630**	(0.655)	[0.067]	15.45	2,649
Full-time leave (father)	1.952***	(0.397)	[0.000]	7.52	2,649
Worked during leave (father)*	0.053	(0.039)	[0.979]	0.26	2,080
Worked during leave (mother)*	−0.001	(0.034)	[0.539]	0.17	1,996
Panel B: Parent-child interactions					
Reading often (father)	−0.072**	(0.036)	[0.205]	0.70	2,649
Playing often (father)	−0.032*	(0.019)	[0.288]	0.94	2,649
Put to bed often (father)	0.027	(0.032)	[0.550]	0.76	2,649
Reading often (mother)	−0.086***	(0.028)	[0.013]	0.84	2,649
Playing often (mother)	−0.015	(0.012)	[0.522]	0.98	2,649
Playing often (mother)	−0.014	(0.015)	[0.550]	0.96	2,649
Panel C: Household division of labor*					
Share childcare (father)	−0.010	(0.016)	[0.983]	0.38	2,351
Share hh chores (father)	−0.034**	(0.017)	[0.283]	0.39	2,346
Working (father)	−0.029	(0.023)	[0.786]	0.92	2,404
Working (mother)	−0.009	(0.034)	[0.997]	0.76	2,435
Hours worked (father)	−0.349	(1.244)	[0.997]	34.38	2,319
Hours worked (mother)	0.614	(1.376)	[0.992]	24.06	2,407
Hours WFH (father)	−0.178	(1.072)	[0.997]	7.44	2,283
Hours WFH (mother)	0.948	(0.907)	[0.885]	5.59	2,388
HH income	−2.574	(90.462)	[0.997]	2,584.10	2,331
Panel D: Parental wellbeing					
Parents separated	−0.001	(0.004)	[(0.823]	0.07	2,649
Subjective wellbeing (father)***	−0.601**	(0.268)	[(0.049]	7.62	913
Subjective wellbeing (mother)***	0.773***	(0.210)	[(0.009]	7.08	1,556
Panel E: Use of childcare					
Attends formal childcare	−0.166***	(0.025)	[0.000]	0.74	2,649
Hours formal childcare	−3.677***	(1.064)	[0.003]	17.25	2,649
Nb. of different childc. types used	0.021	(0.051)	[0.820]	0.50	2,649
Childminders and nannies	0.017	(0.016)	[0.551]	0.04	2,649
Grandparents	−0.018	(0.038)	[0.820]	0.36	2,649
Other informal childcare	0.013	(0.009)	[0.400]	0.01	2,649

Notes: Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$. Robust standard errors are reported in parentheses and Romano-Wolf step-down adjusted p-values based on 10,000 replications are reported in brackets. Coefficients from separate RD-DD regressions (Equation 2) showing the effect of paternity leave extensions on different outcomes indicated in the first column for the 2020 and 2021 reform. Dependent variable means calculated across reform cohorts in pre-reform period. *Panel A*: "Total leave" as defined in Table 2. "Full-time leave": As "Total leave", but restricted to those weeks taken full-time (as opposed to part-time). "Full-time leave with mother": As "Full-time leave", but restricted to leave taken at the same time as the mother. "Full-time leave alone": As "Full-time leave", but restricted to leave taken alone (not with mother). "Worked during leave": Dummy equal to one if working while on full-time leave. *Only defined for those who were eligible for parental leave and working at the time of birth. *Panel B*: Dummies equal to one if the parent does the activity once or twice a week or more often. *Panel C*: ** Household division of labor variables only defined when father (mother) was main respondent or when mother (father) was main respondent and still living with father (mother) of the child. 'Share childcare" and "Share hh chores" are the share in household childcare and chores done by the father. Only defined for households where natural mother and father still live together."Working": dummy equal to one if person was working at the time of the survey. "Hours worked": weekly working hours, including zeros for individuals not in work. "Hours WFH": Nb. of hours per week worked from home, on average, conditional on working. "HH Income": monthly household income. "Parents separated": dummy taking value one if natural parents of child do not live together. "Subjective well-being": Self-rated well-being on a scale from 0-10. *** Only available if father (mother) was the main respondent. All specifications include the full set of controls as defined in the notes to Table 2.

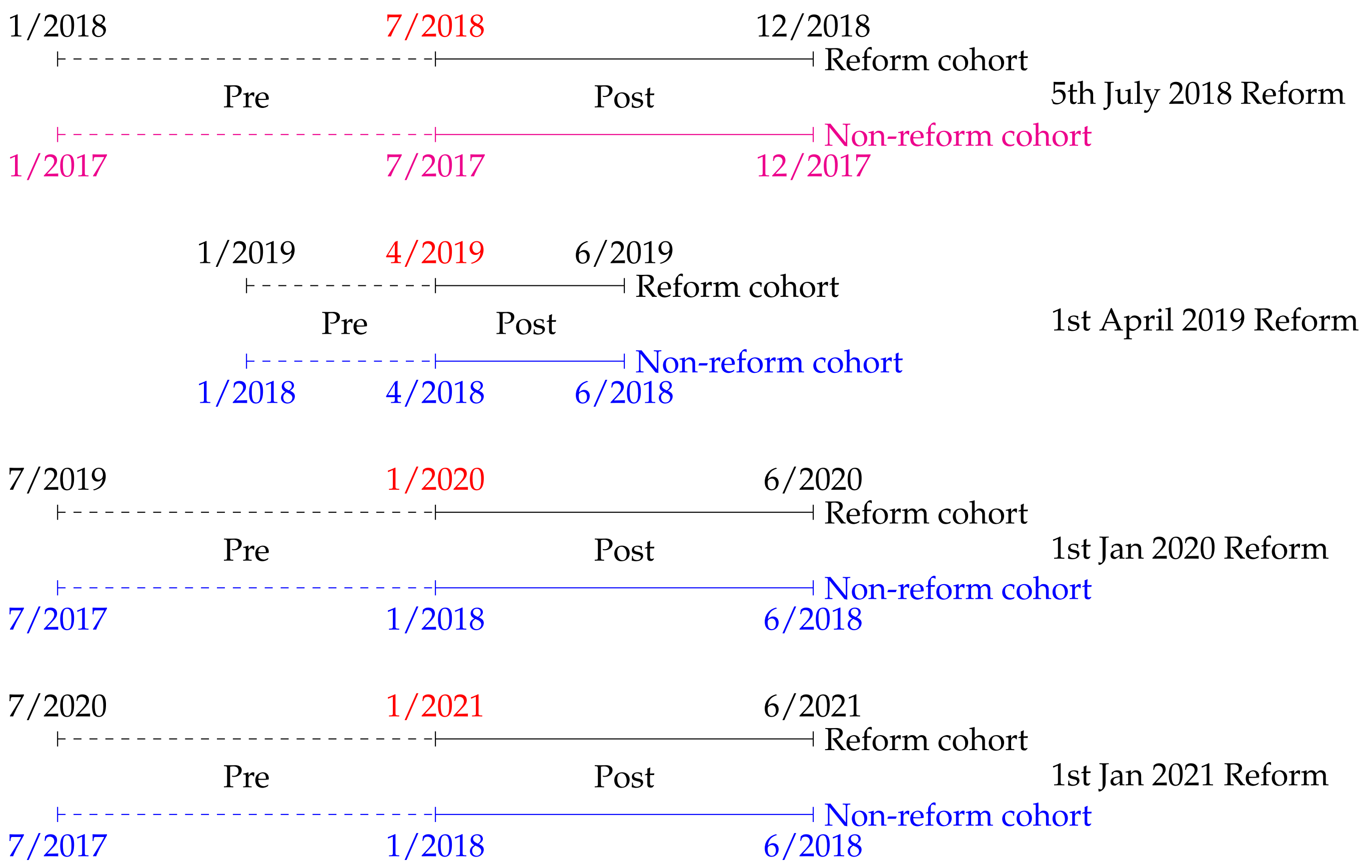
Table 7
Effects of 2020-21 reforms: Heterogeneity

	Total leave (weeks) (1)	Total time off (weeks) (2)	Normal developm. (3)	ASQ score (log) (4)	Worked dur. leave (father) (5)	Formal childcare (6)
$\text{PostReform}_{20,21}$	1.967***	0.715	−0.112***	0.017	0.006	−0.201***
	(0.498)	(0.512)	(0.041)	(0.028)	(0.042)	(0.030)
$\text{PostReform}_{20,21}$ x Father college	0.200	0.507	−0.026	−0.043*	0.096**	0.077**
	(0.512)	(0.569)	(0.041)	(0.025)	(0.042)	(0.037)
$\text{PostReform}_{20,21}$	1.501***	0.329	−0.060	0.026	0.013	−0.228***
	(0.550)	(0.561)	(0.043)	(0.031)	(0.045)	(0.033)
$\text{PostReform}_{20,21}$ x Mother college	0.948*	1.048*	−0.109***	−0.050*	0.065	0.106***
	(0.531)	(0.571)	(0.041)	(0.026)	(0.041)	(0.038)
$\text{PostReform}_{20,21}$	2.516***	1.791***	−0.182***	−0.014	0.072	−0.169***
	(0.497)	(0.550)	(0.044)	(0.031)	(0.047)	(0.034)
$\text{PostReform}_{20,21}$ x Firstborn	−0.700	−1.384**	0.094**	0.016	−0.040	0.005
	(0.511)	(0.569)	(0.041)	(0.027)	(0.042)	(0.038)
$\text{PostReform}_{20,21}$	2.279***	0.971*	−0.118***	−0.018	0.019	−0.179***
	(0.477)	(0.527)	(0.041)	(0.029)	(0.044)	(0.031)
$\text{PostReform}_{20,21}$ x Boy	−0.434	−0.052	−0.012	0.029	0.067	0.026
	(0.512)	(0.563)	(0.040)	(0.026)	(0.041)	(0.037)
Mean dep. var	8.08	6.19	0.70	5.37	0.26	0.74
SD dep. var.	5.84	6.31	0.46	0.27	0.44	0.44
Obs.	2,649	2,649	2,649	2,649	2,080	2,649

Notes: Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$. This table shows RD-DD estimates of heterogeneous effects of paternity leave extensions on child outcomes. Pooled across 2020 and 2021 reforms. All specifications include the following set of control variables: gender of the child, dummies for father's and mother's level of education, separate dummies for whether the father (mother) is Spanish-born, mother's and father's age, a dummy for whether the couple is opposite sex, region fixed-effects, a dummy for whether the father was the main respondent, a dummy equal to 1 if the child was born in latest month of birth within the ASQ-3 questionnaire appropriate for their age at the time of survey and an indicator for being born in latest month of birth (minus 1 month) within a given questionnaire. All specifications include month of birth fixed effects, as shown in Equation 2.

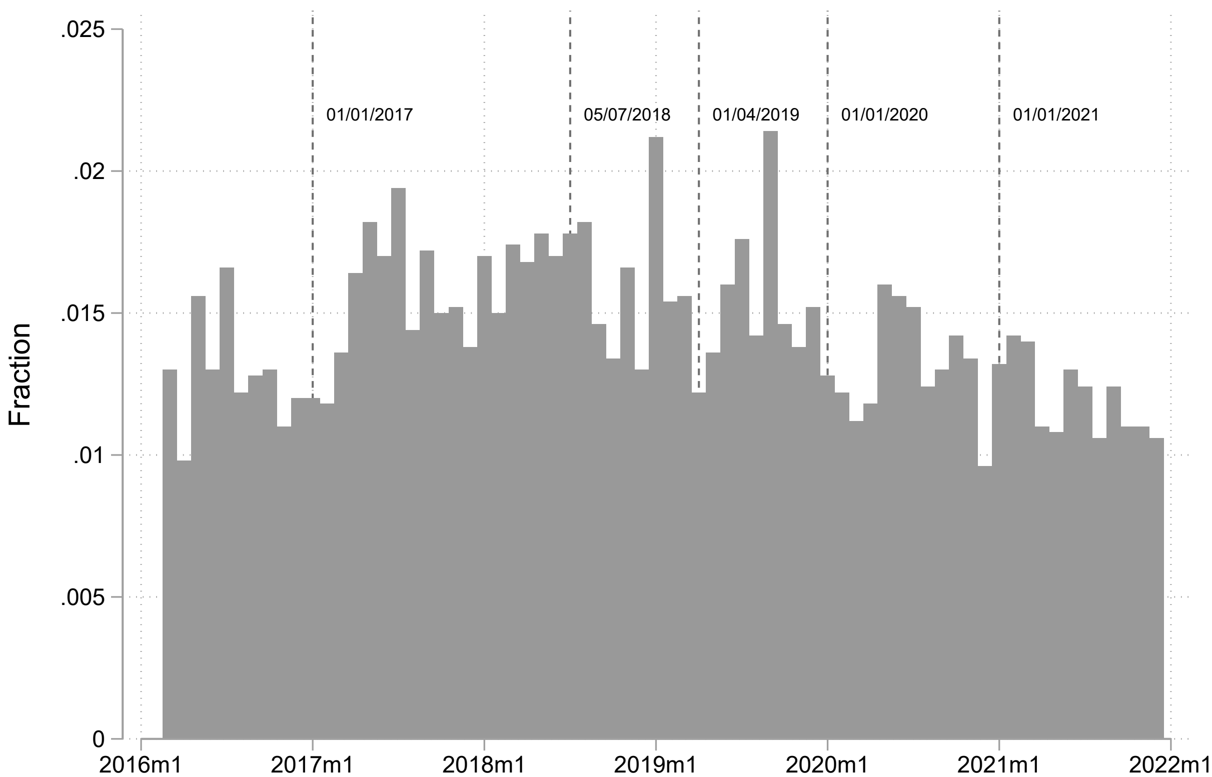
Figures

Figure 1
Reform and non-reform periods



Notes: This figure shows the difference-in-difference cohort setup for each of the reforms. For each reform, there is a cohort around the reform cutoff (Reform cohort) and a cohort around the same cutoff born in a period where no reform took place (Non-reform cohort), and there is a point in time which marks a pre- and post period. This implies that for the reform cohort, the post-period will identify the births really affected by the reform, whereas the pre-period will identify births within the reform cohort who are not treated (i.e. not affected by the reform). The control cohort is further divided into a pre- and post- period based on the month that the reform was enacted for reform cohorts.

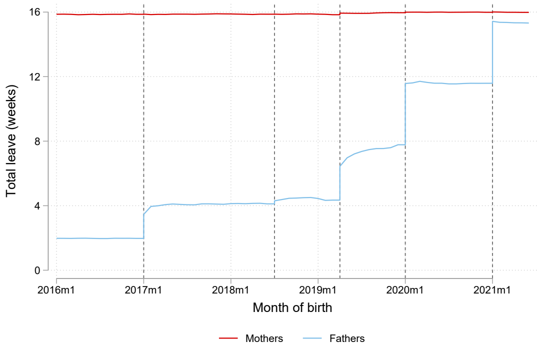
Figure 2
Distribution of monthly births



Notes: This figure shows a histogram of the running variable (month of birth of the child). We test for the presence of manipulation around the reform cut-offs using the test proposed by Frandsen (2017) in the context of regression discontinuity designs with a discrete running variable (month of birth). We implement the test using the Stata command *rddisttestk* (Frandsen, 2017). This test relies only on support points at and immediately adjacent to the RD threshold when the running variable is discrete. We choose the parameter k, determining the maximal degree of nonlinearity in the probability mass function still considered to be compatible with no manipulation, to be able to detect manipulation in the most stringent situation (that is, when k=0). A large k implies that the mass at the threshold can deviate substantially from linearity before the test will reject with high probability. A small k means even small deviations from linearity will lead the test to reject with high probability. We cannot reject the null hypothesis of absence of manipulation for the five reform thresholds (p-values of 0.888, 0.874, 0.268, 0.714 and 0.449, respectively).

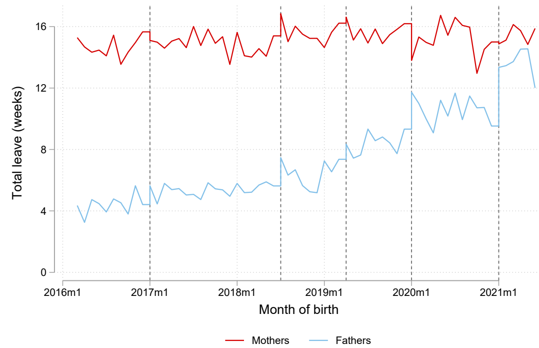
Figure 3
Maternity and paternity leave length (weeks) over time

(a) Administrative data



Notes: This figure shows the average length of maternity and paternity leave taken in full-time equivalent (FTE) weeks by month of birth of the child using the universe of individuals who have taken parental leave from Spanish Social Security Records for births between January 2016 and June 2021.

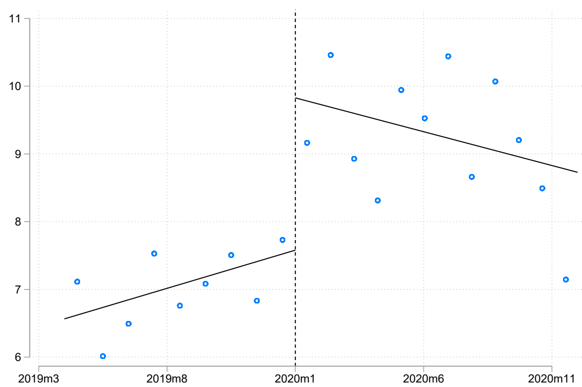
(b) Survey data



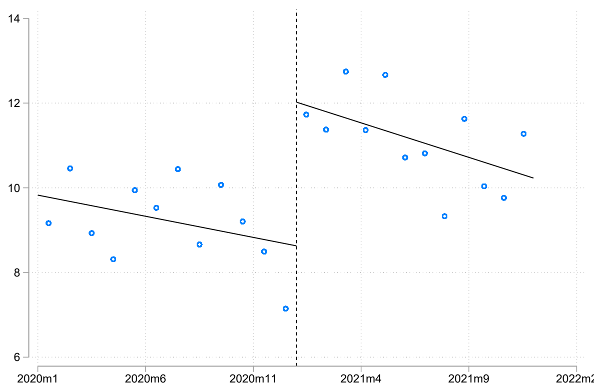
Notes: This figure shows the average length of maternity and paternity leave taken in full-time equivalent (FTE) weeks by month of birth of the child using the sample of individuals who have taken parental leave from a survey of 5,000 households for births between January 2016 and June 2021.

Figure 4
Weeks of paternity leave

Jan 2020 reform (8 to 12 weeks)



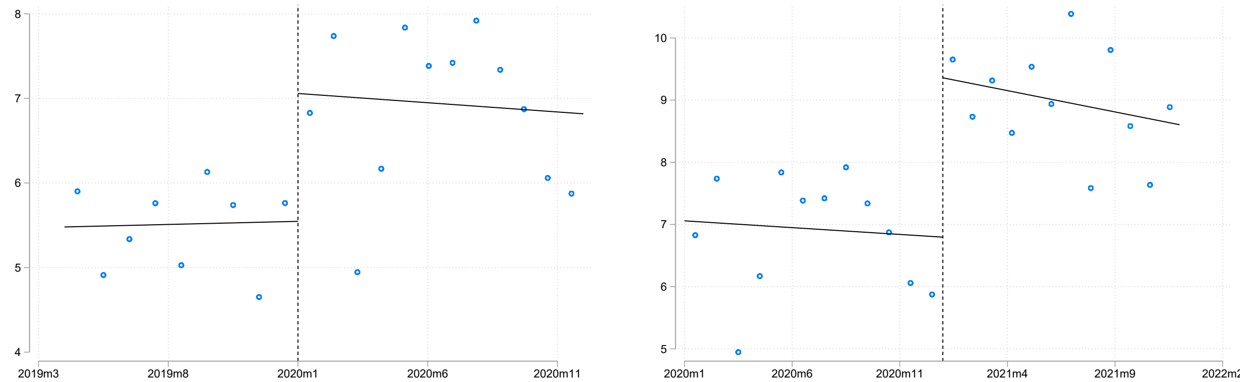
Jan 2021 reform (12 to 16 weeks)



Notes: This figure shows the number of full-time equivalent (FTE) weeks of leave taken by fathers, averaged over the month of birth of the child. The lines are linear fits, separately an each side of the threshold.

Figure 5
Fathers' total time taken off after birth

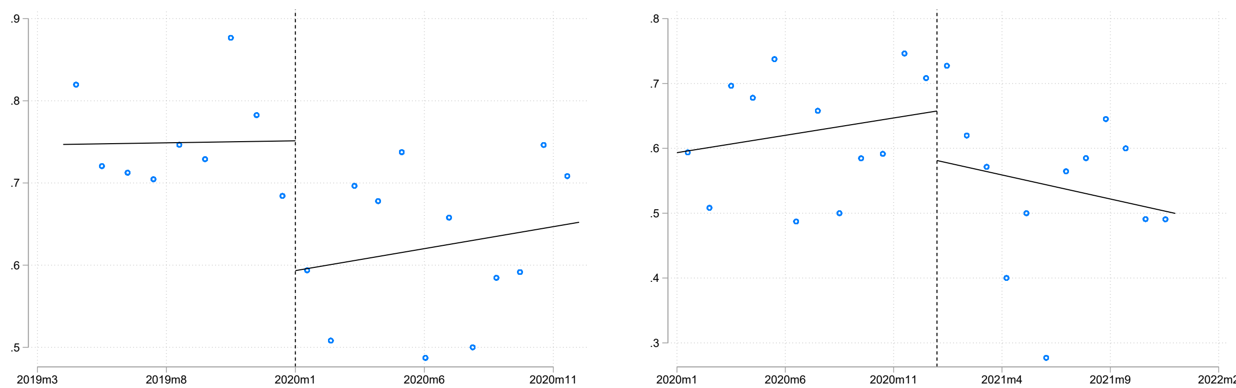
Jan 2020 reform (8 to 12 weeks) **Jan 2021 reform (12 to 16 weeks)**



Notes: This figure shows the total number of weeks taken off by fathers averaged over the month of birth of the child. The lines are linear fits, separately an each side of the threshold.

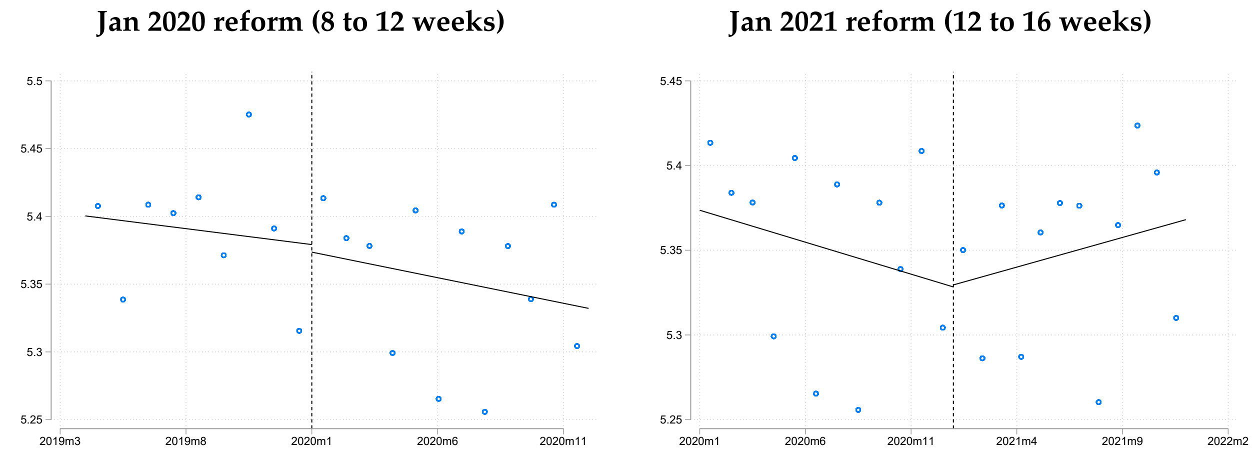
Figure 6
Normal development

Jan 2020 reform (8 to 12 weeks) **Jan 2021 reform (12 to 16 weeks)**



Notes: This figure shows the share of children with normal development, averaged over the month of birth of the child. The lines are linear fits, separately on each side of the threshold.

Figure 7
Log ASQ score



Notes: This figure shows the average total score on the ASQ, averaged over the month of birth of the child. The lines are linear fits, separately on each side of the threshold.

Online Appendix

Table A1
Test of manipulation of number of births around the reform thresholds

	(1) 2017	(2) 2018	(3) 2019	(4) 2020	(5) 2021
Treat x Post-reform	-774.750^{*}	-92.500	-209.000	385.944	1,000.111
	(397.728)	(365.389)	(543.609)	(324.560)	(1,066.772)
Post-reform	794.750^{**}	40.833	242.667	$1{,}135.611^{***}$	$1{,}135.611^{***}$
	(332.213)	(400.407)	(476.444)	(325.910)	(365.028)
Treat	$-1{,}332.667^{***}$	$-4{,}501.500^{***}$	$-5{,}386.500$	$-5{,}522.667^{***}$	$-7{,}958.333^{***}$
	(335.438)	(280.820)	(463.041)	(143.761)	(745.055)
Dep. var. mean	34,745	33,740	32,345	33,433	33,007
Obs.	48	60	30	60	60
Treatment births	7/2016-6/2017	1/2018-12/2018	1/2019-6/2019	7/2019-6/2020	7/2020-6/2021
Control births	7/2013-6/2016	1/2014-12/2017	1/2014-12/2016	7/2013-6/2016	7/2013-6/2016
			+1/2018-6/2018	+7/2017-6/2018	+7/2017-6/2018

Notes: Significance levels are indicated by * $<$ .1, ** $<$ .05, *** $<$ .01. This table shows RD-DD estimates of the effect of paternity leave extensions on number of monthly births using INE (2024) data. We report the δ coefficients from separate regressions of Equation 1 for each reform window, excluding the vector of control variables X_i'. Each sample contains four control cohorts (births in same months in years not affected by reforms). Specifications control for month of birth fixed effects and control cohort fixed effects.

Table A2
Balance in covariates around the reform thresholds

	(1) 2017	(2) 2018	(3) 2019	(4) 2020	(5) 2021
Child sex	−0.002	0.045	0.069	−0.018	0.009
	(0.047)	(0.046)	(0.064)	(0.046)	(0.048)
Dep. var. mean	0.485	0.490	0.481	0.485	0.488
Obs.	1,830	1,893	992	1,879	1,767
Age (father)	−0.091	−0.057	1.584**	−0.396	0.233
	(0.577)	(0.553)	(0.796)	(0.595)	(0.598)
Dep. var. mean	40.310	40.017	39.280	39.086	38.655
Obs.	1,626	1,693	890	1,696	1,613
Age (mother)	0.152	−0.572	1.278*	0.009	1.068*
	(0.540)	(0.527)	(0.762)	(0.534)	(0.578)
Dep. var. mean	38.240	37.922	37.089	36.972	36.643
Obs.	1,680	1,738	922	1,727	1,623
Above college (father)	−0.001	0.011	−0.164**	0.010	0.020
	(0.050)	(0.048)	(0.067)	(0.049)	(0.050)
Dep. var. mean	0.426	0.426	0.444	0.443	0.454
Obs.	1,626	1,693	890	1,696	1,613
Above college (mother)	0.019	0.010	−0.050	0.041	−0.060
	(0.049)	(0.048)	(0.065)	(0.048)	(0.049)
Dep. var. mean	0.524	0.527	0.579	0.558	0.577
Obs.	1,680	1,738	922	1,727	1,623
Father Spanish born	−0.040	−0.007	0.011	0.001	0.022
	(0.028)	(0.028)	(0.034)	(0.027)	(0.027)
Dep. var. mean	0.912	0.908	0.929	0.917	0.920
Obs.	1,626	1,693	890	1,696	1,613
Mother Spanish born	0.031	0.029	−0.016	0.013	0.037
	(0.027)	(0.025)	(0.036)	(0.028)	(0.029)
Dep. var. mean	0.921	0.925	0.919	0.907	0.913
Obs.	1,680	1,738	922	1,727	1,623
Eligible for mat. leave	0.053	0.047	−0.064	0.041	0.003
	(0.041)	(0.039)	(0.052)	(0.039)	(0.039)
Dep. var. mean	0.754	0.771	0.792	0.771	0.783
Obs.	1,830	1,893	992	1,879	1,767
Eligible for pat. leave	0.013	0.038	−0.011	0.061*	0.020
	(0.040)	(0.037)	(0.049)	(0.035)	(0.036)
Dep. var. mean	0.777	0.796	0.815	0.821	0.818
Obs.	1,830	1,893	992	1,879	1,767
Father main respondent	−0.010	0.018	0.003	0.062	−0.001
	(0.045)	(0.044)	(0.062)	(0.045)	(0.047)
Dep. var. mean	0.360	0.353	0.360	0.367	0.370
Obs.	1,830	1,893	992	1,879	1,767
F-Stat	0.680	1.215	1.455	0.602	1.293
p-value	0.744	0.275	0.150	0.814	0.228

Notes: Significance levels are indicated by * $<$ .1, ** $<$ .05, *** $<$ .01. This table shows RD-DD estimates of the effect of paternity leave extensions on predetermined controls for paternity leave extensions in 2017, 2018, 2019, 2020 and 2021. Treatment and control cohorts are defined as in Figure 1. We report the δ coefficients from separate regressions of Equation 1, excluding the vector of control variables X_i', where the dependent variable is specified in the row in bold. At the bottom of the table we report the F-statistic and p-value from a joint test of all the coefficients being equal to zero using a seemingly unrelated regression model.

Table A3
Balance in covariates - pooled 2020 and 2021 reforms

	Main sample	+/- 4 months	Excl. Jan & Dec births
Child sex	-0.004	-0.025	-0.028
	(0.040)	(0.050)	(0.044)
Dep. var. mean	0.490	0.492	0.494
Obs.	2,649	1,715	2,241
Age (father)	-0.058	-0.855	-0.450
	(0.505)	(0.635)	(0.542)
Dep. var. mean	38.437	38.390	38.477
Obs.	2,404	1,544	2,038
Age (mother)	0.527	-0.223	0.411
	(0.471)	(0.593)	(0.505)
Dep. var. mean	36.345	36.325	36.386
Obs.	2,435	1,583	2,054
Above college (father)	0.013	0.045	0.016
	(0.042)	(0.052)	(0.046)
Dep. var. mean	0.458	0.443	0.462
Obs.	2,404	1,544	2,038
Above college (mother)	-0.005	-0.027	-0.004
	(0.041)	(0.051)	(0.045)
Dep. var. mean	0.584	0.582	0.585
Obs.	2,435	1,583	2,054
Father Spanish born	0.009	0.033	0.010
	(0.023)	(0.029)	(0.025)
Dep. var. mean	0.921	0.917	0.922
Obs.	2,404	1,544	2,038
Mother Spanish born	0.023	0.012	0.024
	(0.024)	(0.030)	(0.026)
Dep. var. mean	0.907	0.902	0.906
Obs.	2,435	1,583	2,054
Eligible for mat. leave	0.025	-0.015	0.026
	(0.034)	(0.042)	(0.036)
Dep. var. mean	0.783	0.778	0.786
Obs.	2,649	1,715	2,241
Eligible for pat. leave	0.042	0.022	0.046
	(0.031)	(0.039)	(0.034)
Dep. var. mean	0.832	0.831	0.837
Obs.	2,649	1,715	2,241
Father main respondent	0.033	0.028	0.036
	(0.039)	(0.048)	(0.042)
Dep. var. mean	0.376	0.370	0.379
Obs.	2,649	1,715	2,241
F-Stat	0.543	0.816	0.835
p-value	0.861	0.614	0.594

Notes: Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$. This table shows RD-DD estimates of the effect of paternity leave extensions on predetermined controls for the 2020 and 2021 reforms (pooled). We report the δ coefficients from separate regressions of Equation 1, excluding the vector of control variables X_i'. The dependent variable is specified in each row in bold. *Main sample:* treated cohorts are children born between July 2019 and June 2020 (Jan 1st 2020 reform) and between July 2020 and June 2021 (Jan 1st 2021 reform). The control cohort is comprised of children born between July 2017 and June 2018. *+/- 4 months*: as before, but the window around the reform cutoffs are reduced to +/- 4 months. *Excl. Jan & Dec births*: As in column 1, but excluding births in January and December from treatment and control cohorts. At the bottom of the table we report the F-statistic and p-value from a joint test of all the coefficients being equal to zero using a seemingly unrelated regression model.

Table A4
Descriptive statistics in our survey sample vs. the Spanish Labor Force Survey

	Survey sample	LFS Q1 2022	Difference (S.E.)	p-value
Age	37.59	37.56	$-0.025(0.106)$	0.81
Female	0.63	0.52	$-0.105(0.008)$	0.00
Above college	0.55	0.43	$-0.119(0.009)$	0.00
Spanish-born	0.93	0.73	$-0.194(0.006)$	0.00
Single parent	0.08	0.06	$-0.019(0.004)$	0.00
Employed	0.82	0.76	$-0.057(0.007)$	0.00
Weekly hours worked	28.22	24.25	$-3.968(0.314)$	0.00
N	5,000	8,593		

Notes: This table shows summary statistics for our full survey sample (N=5,000) and for a representative sample of adults living with children under the age of six from the Spanish Labor Force Survey (EPA) from the first quarter of 2022 (N=8,593). The third column shows the coefficient of a regression of the variable in the first column on a dummy variable indicating the EPA sample, and its standard error in parenthesis. The last column shows the p-value of this coefficient.

Table A5
Summary statistics

	Reform samples						Full
	2017	2018	2019	2020	2021	2020-21	sample
Panel A: Child characteristics							
Child sex	0.49	0.49	0.48	0.49	0.49	0.49	0.49
Age (months)	54.76	49.35	40.82	38.28	33.79	31.12	37.49
Normal development	0.78	0.76	0.73	0.73	0.68	0.68	0.71
Good progress	0.53	0.50	0.45	0.44	0.41	0.39	0.42
One area in referral zone	0.13	0.15	0.14	0.14	0.18	0.17	0.16
Two areas in referral zone	0.05	0.05	0.06	0.06	0.07	0.07	0.07
ASQ score (log)	5.48	5.46	5.41	5.42	5.41	5.39	5.42
Panel B: Parental characteristics							
Age (father)	40.16	39.91	39.25	39.07	38.68	38.49	38.96
Age (mother)	38.13	37.83	37.07	36.96	36.66	36.39	36.85
Above college (father)	0.38	0.38	0.40	0.40	0.41	0.42	0.40
Above college (mother)	0.48	0.48	0.54	0.51	0.53	0.54	0.53
Father Spanish born	0.81	0.81	0.83	0.83	0.84	0.84	0.82
Mother Spanish born	0.85	0.85	0.85	0.83	0.84	0.83	0.84
Eligible for mat. leave	0.75	0.77	0.79	0.77	0.78	0.78	0.78
Took maternity leave	0.74	0.76	0.78	0.76	0.77	0.77	0.76
Eligible for pat. leave	0.78	0.80	0.81	0.82	0.82	0.83	0.82
Took paternity leave	0.73	0.75	0.78	0.78	0.78	0.80	0.78
Father main respondent	0.36	0.35	0.36	0.37	0.37	0.38	0.37
Obs.	1,830	1,893	992	1,879	1,767	2,649	5,000

Notes: This table shows summary statistics for the different reform samples as specified in Figure 1. For instance, the 2018 reform sample includes children born between 1 January, 2018 and 31 December, 2018 (reform year), and those born between 1 January, 2017, and 31 December, 2017 (non-reform year). The column "2020-21" comprises the pooled reform samples of 2020 and 2021, and the last column shows summary statistics for the entire survey sample obtained.

Table A6
Paternity leave extensions and leave take-up - RD specifications

	Total leave (weeks)		Total time off (weeks)	
	(1)	(2)	(3)	(4)
PostReform$_{2017}$	0.403	0.284	-0.143	-0.228
	(0.516)	(0.511)	(0.484)	(0.496)
Mean dep. var.	3.36		3.04	
SD dep. var.	4.72		4.49	
Obs.	2,087		2,087	
PostReform$_{2018}$	0.799*	0.801*	-0.100	0.092
	(0.455)	(0.460)	(0.423)	(0.424)
Mean dep. var.	4.21		3.36	
SD dep. var.	4.55		4.42	
Obs.	2,154		2,154	
PostReform$_{2019}$	0.893	0.892	0.715	0.850
	(0.593)	(0.597)	(0.587)	(0.585)
Mean dep. var.	5.44		4.14	
SD dep. var.	4.99		4.88	
Obs.	1,405		1,405	
PostReform$_{2020}$	2.247***	1.970***	1.511**	1.132*
	(0.655)	(0.650)	(0.673)	(0.677)
Mean dep. var.	7.03		5.51	
SD dep. var.	5.44		5.68	
Obs.	1,480		1,480	
PostReform$_{2021}$	3.393***	2.876***	2.562***	2.176***
	(0.660)	(0.646)	(0.733)	(0.718)
Mean dep. var.	9.28		6.94	
SD dep. var.	6.10		6.73	
Obs.	1,508		1,508	
Controls	No	Yes	No	Yes

Notes: Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$. RD estimates of the following equation on take-up of paternity leave: $Y_i = \alpha + \tau \times D_i + \beta_1 \times (X_i - c) + \beta_2 \times D_i \times (X_i - c) + \epsilon_i$, where Y_i is the outcome variable for individual i, $D_i = 1$ if $X_i \geq c$ (treated), and $D_i = 0$ otherwise (control), X_i is the running variable (month-year of birth) for individual i, c is the cutoff point, τ is the treatment effect we are interested in, and ϵ_i is the error term. All specifications use the maximum bandwidth available, i.e.: control group: all births in months since last reform up to one month before the relevant reform; treatment group: all births in month of reform and up to the last month before next reform. Means and standard deviations calculated in pre-reform period. Control variables added in columns 2 and 4, and dependent variables as defined in notes to Table 2.

Table A7
Effects of each paternity leave extension on child development - RD specification

	Normal development		Good progress		ASQ score (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
PostReform$_{2017}$	-0.041	-0.050	0.011	0.003	-0.004	0.000
	(0.039)	(0.040)	(0.051)	(0.030)	(0.031)	(0.050)
Mean dep. var.	0.84		0.59		5.54	
SD dep. var.	0.37		0.49		0.25	
Obs.	2,087		2,087		2,087	
PostReform$_{2018}$	-0.007	0.007	-0.076^{*}	-0.067	0.011	0.016
	(0.038)	(0.038)	(0.044)	(0.023)	(0.022)	(0.045)
Mean dep. var.	0.77		0.52		5.47	
SD dep. var.	0.42		0.50		0.35	
Obs.	2,154		2,154		2,154	
PostReform$_{2019}$	0.140^{***}	0.123^{**}	0.092^{*}	0.068	0.020	-0.001
	(0.049)	(0.050)	(0.054)	(0.034)	(0.035)	(0.055)
Mean dep. var.	0.69		0.39		5.42	
SD dep. var.	0.46		0.49		0.28	
Obs.	1,405		1,405		1,405	
PostReform$_{2020}$	-0.158^{***}	-0.171^{***}	-0.030	-0.035	-0.006	-0.009
	(0.049)	(0.051)	(0.052)	(0.031)	(0.032)	(0.054)
Mean dep. var.	0.75		0.39		5.39	
SD dep. var.	0.43		0.49		0.27	
Obs.	1,480		1,480		1,480	
PostReform$_{2021}$	-0.076	-0.104^{**}	-0.078	-0.105^{**}	0.001	-0.021
	(0.050)	(0.051)	(0.048)	(0.030)	(0.030)	(0.048)
Mean dep. var.	0.62		0.31		5.35	
SD dep. var.	0.49		0.46		0.30	
Obs.	1,508		1,508		1,508	
Controls	No	Yes	No	Yes	No	Yes

Notes: Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$. RD estimates of the following equation on child development outcomes: $Y_i = \alpha + \tau \times D_i + \beta_1 \times (X_i - c) + \beta_2 \times D_i \times (X_i - c) + \epsilon_i$, where Y_i is the outcome variable for individual i, $D_i = 1$ if $X_i \geq c$ (treated), and $D_i = 0$ otherwise (control), X_i is the running variable (month-year of birth) for individual i, c is the cutoff point, τ is the treatment effect we are interested in, and ϵ_i is the error term. All specifications use the maximum bandwidth available, i.e.: control group: all births in months since last reform up to one month before the relevant reform; treatment group: all births in month of reform and up to the last month before next reform. Means and standard deviations calculated in pre-reform period. Control variables added in columns 2, 4, and 6, and dependent variables as defined in notes to Table A8.

Table A8
Effects of each paternity leave extension on child development

	Normal development		Good progress		ASQ score (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
PostReform$_{2018}$	-0.019	-0.002	-0.115^{**}	-0.103^{**}	0.022	0.024
	(0.040)	(0.040)	(0.046)	(0.028)	(0.029)	(0.046)
Mean dep. var.	0.77		0.49		5.45	
SD dep. var.	0.42		0.50		0.27	
Obs.	1,893		1,893		1,893	
PostReform$_{2019}$	0.083	0.069	0.067	0.054	-0.003	-0.013
	(0.056)	(0.060)	(0.064)	(0.037)	(0.039)	(0.066)
Mean dep. var.	0.64		0.38		5.39	
SD dep. var.	0.48		0.49		0.35	
Obs.	992		992		992	
PostReform$_{2020}$	-0.142^{***}	-0.142^{***}	0.011	0.019	-0.022	-0.021
	(0.041)	(0.042)	(0.045)	(0.028)	(0.030)	(0.047)
Mean dep. var.	0.75		0.37		5.39	
SD dep. var.	0.43		0.48		0.25	
Obs.	1,879		1,879		1,879	
PostReform$_{2021}$	-0.125^{***}	-0.112^{**}	-0.019	-0.003	0.008	0.018
	(0.044)	(0.045)	(0.045)	(0.031)	(0.031)	(0.045)
Mean dep. var.	0.63		0.31		5.35	
SD dep. var.	0.48		0.46		0.30	
Obs.	1,767		1,767		1,767	
Controls	No	Yes	No	Yes	No	Yes

Notes: Significance levels are indicated by * $<$ .1, ** $<$ .05, *** $<$ .01. This table shows RD-DD estimates of the effect of paternity leave extensions on different child development outcomes. Treated and cohorts are as defined in Figure 1. The dependent variables are defined as follows: "Normal development" is a dummy variable equal to one if the child scored above the referral cutoff in all areas. "Good progress" is a dummy variable equal to one if the child scored above the monitoring cutoff in all areas. Specifications with controls include the following set of variables: Gender of the child, dummies for father's and mother's level of education, separate dummies for whether the father and mother is Spanish-born, mother's and father's age, a dummy for whether the couple is opposite sex, region fixed-effects, a dummy for whether the father was the main respondent, a dummy equal to 1 if the child was born in latest month of birth within the ASQ-3 questionnaire appropriate for their age at the time of survey and an indicator for being born in the latest month of birth (minus 1 month) within a given questionnaire. All specifications include month of birth fixed effects, as shown in Equation 1.

Table A9
Effects of the 2020 and 2021 reforms on childcare modes (at the time of the survey)

	Formal childcare (1)	Hours formal childcare (2)	Nannies (3)	Grand-parents (4)	Other informal childcare (5)	N (6)
Treat x Post 2020 Reform	-0.150^{***}	-3.186^{**}	0.016	0.026	0.008	1,879
	(0.031)	(1.263)	(0.019)	(0.045)	(0.010)	
Treat x Post 2021 Reform	-0.186^{***}	-4.429^{***}	0.018	-0.068	0.019^{**}	1,767
	(0.037)	(1.328)	(0.018)	(0.046)	(0.009)	

Notes: Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$. This table shows RD-DD estimates of the effect of paternity leave extensions on childcare usage. Treated and control cohorts are as defined in Figure 1. All specifications include the following set of control variables: gender of the child, dummies for father's and mother's level of education, separate dummies for whether the father and mother is Spanish-born, mother's and father's age, a dummy for whether the couple is opposite sex, region fixed-effects, a dummy for whether the father was the main respondent, a dummy equal to 1 if the child was born in latest month of birth within the ASQ-3 questionnaire appropriate for their age at the time of survey and an indicator for being born in the latest month of birth (minus 1 month) within a given questionnaire. All specifications include month of birth fixed effects, as shown in Equation 1.