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Hélène Périvier, Gregory Verdugo

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**WHERE ARE THE FATHERS? THE EFFECTS  
OF EARMARKING PARENTAL LEAVE ON  
FATHERS IN FRANCE**

**Hélène Périvier**

**Gregory Verdugo**



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OFCE  
10 place de Catalogne | 75014 Paris | France  
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### ABOUT THE AUTHORS

Hélène Périvier, OFCE-Sciences Po

Email Address: [helene.perivier@sciencespo.fr](mailto:helene.perivier@sciencespo.fr)

Gregory Verdugo, Université Paris-Saclay, Univ Evry, EPEE and OFCE-Sciences Po

Email Address: [gregory.verdugo@univ-evry.fr](mailto:gregory.verdugo@univ-evry.fr)

### ABSTRACT

Does providing nontransferable months of parental leave earmarked to fathers, as mandated by the European Union to its member countries since 2019, increase their participation? To answer that question, the authors investigate the consequences of a 2015 French reform that earmarked up to 12 months of paid leave for fathers while simultaneously reducing the maximum paid leave for mothers by the same number of months. While the benefits were low, parental leave could be taken part-time, which can be more attractive for fathers. Using administrative data and comparing parents of children born before and after the reform, the authors find that in response to a 25 p.p. decline in mothers' participation rate triggered by the reform, fathers' participation increased by less than 1 p.p., mostly through part-time leave. The reform increased mothers' labor earnings, but it had no significant impact on fathers' earnings.

### KEYWORDS

Parental leave, Labor supply, Gender inequality.

### JEL

J16, D13, J18.



# Where are the Fathers? The Effects of Earmarking Parental Leave on Fathers in France<sup>1</sup>

Hélène Périvier<sup>2</sup>

Gregory Verdugo<sup>3</sup>

June 2023

## Abstract

Does providing nontransferable months of parental leave earmarked to fathers, as mandated by the European Union to its member countries since 2019, increase their participation? To answer that question, the authors investigate the consequences of a 2015 French reform that earmarked up to 12 months of paid leave for fathers while simultaneously reducing the maximum paid leave for mothers by the same number of months. While the benefits were low, parental leave could be taken part-time, which can be more attractive for fathers. Using administrative data and comparing parents of children born before and after the reform, the authors find that in response to a 25 p.p. decline in mothers' participation rate triggered by the reform, fathers' participation increased by less than 1 p.p., mostly through part-time leave. The reform increased mothers' labor earnings, but it had no significant impact on fathers' earnings.

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<sup>2</sup> OFCE, Sciences Po Email: [helene.perivier@sciencespo.fr](mailto:helene.perivier@sciencespo.fr) Orcid ID: <https://orcid.org/0000-0003-4088-9839>

<sup>3</sup> Corresponding author. Université Paris-Saclay, Univ Evry, EPEE and OFCE. Address: 23 Boulevard François Mitterrand, 91025 Evry, France. Email: [gregory.verdugo@univ-evry.fr](mailto:gregory.verdugo@univ-evry.fr) Orcid ID: <https://orcid.org/0000-0003-0289-5146>

In most OECD countries, parents can take paid parental leave, known as paid family leave in the United States, if they stop working to care for a young child. As they are most often utilized by mothers, these policies increase the gender gap in participation and earnings following the birth of a child (Kleven et al. 2020). To promote a more equal division of childcare, in 2019, the European Union mandated that all member countries offer nontransferable periods of parental leave earmarked for fathers (European Union 2019).

Earmarking parental leave is introduced to increase the incentives for fathers to take parental leave. As any period taken by the father no longer reduces the length of the parental leave of the mother, fathers should participate more when part of parental leave is earmarked for them (Boyer 2017; Koslowski et al. 2020). Having a period reserved for fathers also could reduce the potential stigma associated with fathers' parental leave (Wayne and Cordeiro 2003). Understanding whether earmarking increases fathers' take-up rate is thus important to assess how much of fathers' low participation in parental leave in some countries reflects the fact that no period of parental leave is flagged for them.

As in the reform studied here, some countries introduced earmarking while simultaneously reducing mothers' maximum parental leave to provide additional incentives for fathers to substitute for them (Addati, Cassirer, and Gilchrist 2014). However, it is unclear whether such a reduction increases fathers' incentives to participate. When the mother has low labor earnings, fathers might instead work more to compensate for the loss of paid benefits and the increase in childcare costs. Knowing the conditions under which fathers could substitute for mothers on parental leave is thus essential for designing parental leave policies that aim to increase their participation.

To investigate these questions, we examine a 2015 reform in France that earmarked 6 to 12 nontransferable months of leave for fathers. Although the fathers' prereform participation rate was low (approximately 2.4%), the government expected that many fathers would

substitute for mothers and predicted a tenfold increase in the percentage of fathers taking parental leave, which would enable their participation rate to reach 25% (Collombet 2016).

One crucial advantage of the French reform is that it only modified the earmarking of months of leave between parents, leaving other parameters unchanged, which allows us to isolate the causal effect of earmarking on parents' participation. While earmarking was introduced for all parents, the reform reduced the length of mothers' leave only for parents of second or higher parity births, a group referred to as 'second-time parents' for brevity in the rest of this text.

In contrast to first-time parents, who had to share 6 months of paid leave after birth before the reform, second-time parents were eligible for up to 36 months of paid leave to increase incentives to have a second child. The reform shortened their maximal parental leave period by 12 months, decreasing the length of parental leave for approximately 25% of mothers in this group. Such differences in the effects of the reform between parents allow us to estimate separately the effects of earmarking on fathers in cases when the length of mothers' leave remained unchanged and in cases when it was reduced.

In addition to a relatively long duration, another important characteristic of the French parental leave is that the paid benefits are low, corresponding to approximately one-third of the minimum wage for full-time leave. However, parental leave could be taken part-time, which can be particularly attractive for fathers, as it reduces the financial cost of taking leave and allows them to stay connected with work. An interesting implication of the reform is that, in the absence of stigma or lack of information about the program, any eligible parent working part-time should ask for parental leave benefits, as she can receive at least 150 euros of benefits without changing her labor supply and, after the reform, without affecting the months of leave of the other parent. To assess the potential presence of specific barriers to fathers' participation,



we compare the take-up rates of part-time parental leave between mothers and fathers who are part-time workers after the birth of their child.

We estimate the causal impact of the reform by following Lalive and Zweimüller (2009) and Schönberg and Ludsteck (2014), among others, using a difference-in-differences design. Using data from social security records, we compare the parental leave take-up rate of parents whose children were born in January 2015, just after the reform, with that of parents whose children were born in December 2014 and thus remained in the old system. To account for the influence of calendar effects on outcomes, we use households with children born in December 2013 and January 2014, one year before the reform, as a control group.

The literature on the labor market consequences of parental leave for mothers is large, including for France (Piketty 1998; 2005; Joseph et al. 2013). However, there have been few evaluations of recent reforms earmarking parental leave for fathers. Investigating the impact of these reforms is useful for understanding the determinants of fathers' participation and the interactions between the parental leave take-up rates of mothers and fathers.

### **Related Literature on Earmarked Parental Leave**

In 2018, one year before the 2019 directive that mandated earmarking, one-third of countries in the European Union had already earmarked a share of parental leave for fathers (Janta and Stewart 2018). However, parental leave duration and compensation have varied widely among these countries, which could significantly impact fathers' response to earmarking.

Most recent research has focused on the 'daddy months' policies implemented in Scandinavia and Germany since the 1990s and 2000s. Unlike the program studied here, these 'daddy months' policies were short, consisting of one or at most two months. They also offered high replacement rates of previous earnings, ranging from 67% in Germany up to a ceiling, 80% in Sweden and even 100% in Norway. The daddy months were very popular, as they attracted 30% of fathers in Germany (Bünning 2015), 60% in Norway, and 70% in Sweden

(Rege and Solli 2013). In Spain in 2007, the government introduced two weeks of fully compensated paternity leave, and that was taken by more than 55% of fathers (Farré and González 2019).

In the US, over the last decade, six states and the District of Columbia adopted family leave policies similar to these parental leave policies (Rossin-Slater and Uniat 2019). In particular, California introduced in 2006 the possibility for each parent to take six weeks of paid leave with a 55 percent wage replacement rate up to a ceiling. Despite a duration and compensation level similar to those in Germany, only 2.9% of fathers in California took some family leave after the reform (Bartel et al. 2018).

In Europe, where fathers' participation was more important, these reforms produced a more equal division of household tasks, as fathers taking paid parental leave became more involved in childcare and housework, even after their parental leave (Tamm 2019). However, as it is usually not fully compensated, parental leave decreases household income; consequently, recent work reported increases in the risk of parental separation in low-income households in Sweden (Avdic and Karimi 2018) and Spain, where in addition the fertility rate decreased (González and Zoabi 2021).

In contrast to the short and well-compensated “daddy month” model, reforms implemented in the 2010s in the UK, Portugal, France, and Italy earmarked much longer periods of paid leave for each parent, up to 12 months in France. Typical of countries with long entitlements, the paid benefits offered have been much lower. In Italy, for example, since 2015, 11 months have been earmarked for each parent for 30% of their previous pay (Addati, Cassirer, and Gilchrist 2014).

Many countries, such as France, Spain, Belgium, the Netherlands, South Korea, and more than 14 others, allow parents to take part-time parental leave. Part-time leave mitigates the financial consequences of taking leave while allowing parents to take one or two days off per week to spend time with their child (Bueno and Grau-Grau 2021). The availability of part-time

parental leave might be crucial for fathers, especially in countries with low compensation levels (Boyer 2017).

Because of these important differences among countries, previous research that reported high participation rates of fathers in Scandinavian countries and Germany in response to the introduction of the short and well-compensated ‘daddy month’ might have little relevance in other contexts. One key contribution of our study is thus to document fathers’ response to earmarking months of parental leave in the context where a long period of parental leave is available, the benefits are low, but part-time parental leave is possible.

### **The French Parental Leave Reform**

Mothers can take paid parental leave after up to eighteen weeks of maternity leave, some of which are compulsory, and fathers after up to eleven days of well-compensated paternity leave. Parental leave can be taken without being employed, but as detailed in online Appendix A1, a minimum period of prior employment is required. These eligibility conditions are not restrictive for second-time mothers, as previous periods of parental leave are considered equivalent to work periods. In 2019, 63% and 95% of mothers of a first and second child, respectively, were eligible (HCFEA 2019).

Table 1 summarizes the characteristics of the French parental leave system and how the reform modified the number of months available for each parent. The reform applied only to parents whose child was born after the 1st of January 2015, and parents whose child was born before this date remained in the old system.

#### **First-time parents**

As parental leave is taken monthly, let  $p_f$  and  $p_m$  denote the months of parental leave taken by the mother and father, respectively. For first-time parents, no period was earmarked before the reform. Instead, the restriction was that the *sum* of the months taken by the parents must not be

more than 6 months, that is,  $p_m + p_f \leq 6$ . The reform earmarked six months of paid leave to each parent, such that the new constraints are given by  $p_m \leq 6$  and  $p_f \leq 6$ . Thus, for these parents, the reform allows us to estimate how much fathers increase their participation when additional months are earmarked for them without affecting the maximum length of parental leave that mothers can take. Another minor difference is that after the reform, the mother can take off any month before the child's first birthday. In contrast, before the reform, parental leave had to be taken consecutively during the first six months after birth.

### **Second-time parents**

For 'second-time parents,' the maximum combined length of parental leave before the reform was 36 months, such that  $p_m + p_f \leq 36$ . While the maximum combined length remained at 36 months, the reform added two constraints,  $p_m \leq 24$  and  $p_f \leq 24$ , thus reducing the maximum *per parent* by 12 months, from 36 to 24 months. This implies that, after the reform, 12 months of parental leave are earmarked for each parent, as these months can be taken without reducing the leave of the other parent.

### **Other parental leave characteristics**

All other characteristics of the program remained unchanged. As in the UK and Belgium, the benefits are not proportional to past earnings and are similar for mothers and fathers. In 2015, both before and after the reform, parental leave benefits amounted to approximately 400 euros per month for full-time leave, 250 euros for those working less than 50% of a full-time job, and 150 euros for those working more than 50% but less than 80% of a full-time job. Compared with the 1,150 euros of the net minimum wage for full-time work, these benefits are low. However, when leave is taken on a part-time basis, the benefits are two times greater per hour.<sup>4</sup>

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<sup>4</sup> Hourly paid benefits for part-time leave while employed up to 80% of standard working hours corresponds to 5.3 euros per hour not worked against 2.8 euros per hour for full-time leave.

The benefits are tax-free; they do not depend on the number of children and do not affect eligibility for other welfare programs except unemployment benefits, which are suspended during leave. Importantly, taking part-time leave does not require reducing labor supply even when the parent was already working part-time before the child's birth.

The application process is simple: to claim the parental leave benefits, the parent must ask her employer to sign a one-page form certifying that she has stopped working or is working part-time.<sup>5</sup> This form can be downloaded from the social security website, which also explains the program in detail.

For employees with at least one year of seniority in the firm, the employer cannot deny parental leave, and parents taking parental leave benefit from job protection and cannot be fired. After parental leave, the law guarantees a return to work in a similar position.

Despite the low compensation levels, 45% of second-time mothers took at least a month of parental leave after her child's birth, with more than a quarter taking the maximum length of 36 months before the reform. In contrast, only 2.4% of fathers took a month or more of parental leave.

### **Alternative to parental leave**

For parents, the formal alternative to parental leave is subsidized childcare through a daycare center or certified childminder. The cost and supply of these alternatives did not change over the study period. Parents with a child under three who are not on full-time parental leave can use either a daycare center (20% of children) or a registered childminder (30%) (Le Bouteillec, Kandil, and Solaz 2014; Givord and Marbot 2015). During the year of their third anniversary, all children can attend free and nonrationed preschool (*école maternelle*).<sup>6</sup>

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<sup>5</sup> The family benefit administration (CNAF) website stipulates that one requirement for eligibility is simply that 'You have stopped working or you are working part-time' (*'Vous avez cessé de travailler ou vous travaillez à temps partiel'*).

<sup>6</sup> Even if preschool at age three was not compulsory before 2019, approximately 98% of children attended preschool in September of their third birthday year (Direction de l'évaluation 2018).

Until the third anniversary, the cost for families does not change, and it amounts on average to approximately 10% of the family's net household income (OECD 2022). However, as we detail in Appendix A1, obtaining a slot in a daycare center is financially more attractive for low-income parents earning up to three times the minimum wage (approximately 82% of parents in our sample).<sup>7</sup> According to survey evidence, the scarcity of slots in daycare centers and higher childminder costs explain the large share of mothers on parental leave (Villaume and Legendre 2014).

### **Expected effects of the reform**

For first-time parents, the reform should increase the participation of those who would have taken all six months of leave before the reform. Before the reform, this constraint was binding for approximately 12% of households. Among 98% of these households, the mothers took all six months of parental leave. As a result, if anything, fathers from these households should take more parental leave in response to the reform.

In contrast, the effects might be ambiguous for second-time parents, as the reform simultaneously reduced parental leave by 12 months. For many households, the reform produced a significant shock to childcare arrangements, as just before the reform a parent took more than 24 months in more than 29% of households, and 97% of the time, it was the mother.

In online Appendix A2, we use a simple labor supply model to analyze the effects of earmarking parental leave while reducing the length of the mother's leave. The model suggests that fathers' responses depend on the difference between the mothers' earnings in the labor market and the parental leave benefits. When the labor earnings of mothers are superior to the parental leave benefits and fathers' earnings are low, fathers might replace the mother in

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<sup>7</sup> In a daycare center, the subsidy decreases linearly with household income, while for a childminder, the subsidy is constant below an income threshold. As a result, according to official calculations from French social security, for income levels close to 1,6 minimum wages, the cost of a childminder could be as high as 15% of income versus 5% for collective childcare (Sécurité Sociale 2018, 118).

parental leave, thus losing their wages but receiving the paid benefits. Instead, when the labor earnings of mothers are lower than the benefits, fathers might work more to compensate for the decline in household income.

In contrast, the predicted effects of the reform are straightforward for fathers working part-time: they should take up to 12 months of paid parental leave, as after the reform taking paid leave no longer decreases the length of the mother's leave. As discussed below, up to 7% of fathers work part-time in the population eligible for parental leave. Thus, if there is no additional cost to parental leave participation, such as a stigma associated with gender roles or a lack of information about the program, we expect these fathers to take the benefits they are entitled to.

This discussion leads us to the following hypotheses, which will be tested empirically. For first-time parents, the reform should increase fathers' take-up rate, if anything. For second-time parents, the effects depend on whether mothers earn more in the labor market, net of the associated childcare cost, than the paid benefits. Fathers might work more in response to the reform when the mother has low labor earnings. Finally, fathers working part-time should take parental leave after it has been earmarked, as it does not require them to change their labor supply, and after the reform it no longer affects the length of the mother's parental leave.

A limitation of the standard labor supply model is that it ignores the role of gender identity norms that might discourage fathers from taking parental leave (Ichino et al. 2019; Bertrand 2011; Cortes and Pan 2020). Peer effects (Dahl, Løken, and Mogstad 2014), career concerns (Yamaguchi 2019), or an unfriendly work environment (Haas and Hwang 2016) might increase the cost for fathers to take parental leave. By earmarking a part of parental leave for fathers, the reform could decrease the stigma associated with taking parental leave for men.

### **Implementation of the reform**

The reform is unlikely to have influenced the fertility decisions of mothers who gave birth in December 2014 and January 2015, which are compared to estimate the effects of the reform.

First, the legislative process was long and uncertain.<sup>8</sup> The law was first discussed in July 2013 and voted on by Parliament in July 2014. The government also had to specify after the vote, by decree, the new distribution of leave between parents. The decree was published only two days before the law's entry into force, on the 30th of December 2014. Thus, parents could not precisely anticipate the consequences of the reform.

### **Saliency of the reform**

When the law took effect in January 2015, the family benefit administration sent letters explaining the reform to parents as part of a large communication plan. The reform also was widely publicized in the press. The program's name also changed from '*benefits of free choice of activity*' to '*shared benefits of child rearing*' to emphasize that the new parental leave approach was designed to be 'shared' between parents since a specific period was now earmarked to the father.

### **The Data and Sample**

This empirical analysis relies on social security data from the French family benefits administration, complemented with administrative data on working time from employer records. Further details are provided in online Appendix A3.

### **Social security data**

We use administrative data from the French family benefits social security organization, the *Caisse Nationale des Allocation Familiales* (CNAF). More than 98% of households affected by the reform are covered by our sample; registration is automatic, and the health costs of pregnancies are covered only if they are registered.<sup>9</sup>

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<sup>8</sup> The reform was part of a law named 'the law for real equality between women and men' (in French, '*loi pour l'égalité réelle entre les femmes et les hommes*'), which was designed to reduce gender inequalities.

<sup>9</sup> The sample does not include agricultural workers, who have a separate administration and accounted for less than 1.7% of births in the year of the reform.



We exploit the monthly administrative files containing detailed information on family composition and the monthly benefits received. As the reform differs for single parents, we restrict our sample to families with two parents, whether married, in a civil union, or cohabiting.

For confidentiality, the exact date of birth is not reported. To identify whether a household is affected by the reform, we use the child's year and month of birth. We select households where a child is born in December 2014 or January 2015, i.e., either one month before or after the reform. As discussed below, we also include households that had a child in December 2013 or January 2014 in our sample to control for any calendar effects unrelated to the reform.

While social security benefits are reported monthly, earnings and unemployment benefits are extracted from annual tax returns. Tax returns are reported with a two-year lag because family allowances in year  $t$  depend on the taxable income in the antepenultimate year  $t-2$ . Because of this two-year lag, the tax data start two years before the child's birth and, thus, one year before the mother's pregnancy. On the other hand, while we have monthly information on family benefits until March 2022, annual tax returns are available only until fiscal year 2020, the sixth year after birth for the first cohort affected by the reform.

### **Administrative data on labor supply**

As discussed earlier, if there are no informational barriers or stigma, parents working part-time should take parental leave benefits they are entitled to after the reform. Estimating the take-up rate of parental leave benefits from part-time workers is thus interesting to assess the presence of potential barriers to the participation decision. A difficulty in estimating this take-up rate is that social security data do not report the number of hours or days worked corresponding to annual earnings. Instead, we rely on the French administrative panel '*Échantillon démographique permanent* (EDP),' which contains a 4% sample of the population.<sup>10</sup> The data

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<sup>10</sup> In an earlier version, we used the French Labor Force Survey (LFS) to estimate the share of part-time workers among parents. The sample size of the LFS is four times smaller (1% sample); consequently, the estimates were imprecise. We find a larger share of part-time workers in the EDP relative to the LFS. This reflects the fact that

include birth certificates that allow us to identify young parents affected by the reform and administrative information from employers on earnings and the number of hours worked. We exploit this information to estimate the share of part-time workers among eligible parents after the reform. A limitation is that we have information on labor force participation up to only 2018, which is only three years after the reform.

### **Empirical Approach**

We follow Lalive and Zweimüller (2009) and Schönberg and Ludsteck (2014) by using a difference-in-differences approach. Our approach compares the parents who had a child just before the reform with the parents who had a child just after the reform was put in place on January 1st, 2015, and it uses parents in the year before the reform as a control group to account for any systematic calendar effects unrelated to the reform. To ensure that the groups of parents are comparable, we exploit the smallest possible window by comparing parents who had a child one month before the reform, in December 2014, relative to one month after the reform, in January 2015.<sup>11</sup> As the discontinuity on the 1<sup>st</sup> of January also affects the year of entry to public preschool, we use parents who had a birth one year before the reform during the same months (i.e., December or January) to account for calendar effects.<sup>12</sup> Despite being restricted to a two-month window, our sample is large and includes approximately 108,000 households each year.

Our baseline estimates are obtained from the standard difference-in-differences model estimated with OLS as follows:

$$Y_i = \alpha + \gamma J_i + \lambda R_i + \delta(J_i \times R_i) + u_{it} \quad (1)$$

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the LFS reports the labor force status only during the reference week of the survey. In contrast, the EDP exploits annual information from employers.

<sup>11</sup> We also estimated difference-in-differences specifications using a larger window such as a quarter instead of a month. We obtained broadly similar results from these alternative estimates for the effects of the reform on participation.

<sup>12</sup>As children can enroll in preschool in September of the year when they turn three, a child born in December 2014 was admitted in September 2017. On the other hand, a child born only several days later but in January 2015 must wait until September 2018 to be admitted to preschool.

where  $Y_i$  is an outcome of household  $i$ , such as total earnings or participation in parental leave over the eligibility period,  $J_i$  is a dummy equal to one if the birth occurred in January relative to December, and  $R_i$  equals one if the birth occurred in December 2014 or January 2015, just before or just after the reform of the 1<sup>st</sup> of January 2015, and zero if it occurred one year before, in December 2013 or January 2014. Our key parameter of interest,  $\delta$ , is associated with the interaction term between birth in January in the year of the reform ( $J_i \times R_i$ ) and captures the effects of the reform on  $Y_i$ .<sup>13</sup>

To capture the dynamic effects of the reform on annual earnings or monthly participation rates, we also estimate with OLS an event-study version of the previous difference-in-differences model:

$$Y_{it} = \alpha_t + \sum_{\tau} \gamma_{\tau} J_i I[\tau = t] + \sum_{\tau} \lambda_{\tau} R_i I[\tau = t] + \sum_{\tau \neq -1} \delta_{\tau} (J_i \times R_i) I[\tau = t] + u_{it} \quad (2)$$

where  $Y_{it}$  denotes a measure of participation in month  $t$  relative to the birth month or annual earnings in year  $t$  relative to the birth year. The model includes a full set of event time dummies ( $\alpha_t$ ), event time dummies interacted with birth in January ( $\gamma_{\tau}$ ), and event time dummies interacted with birth in December 2014 or January 2015 ( $\lambda_{\tau}$ ). Our parameters of interest are the event time dummies associated with birth in January 2015, after the reform ( $\delta_{\tau}$ ). For annual earnings, the event time dummy is omitted for  $t = -1$ , and thus, the coefficients  $\delta_{\tau}$  are scaled to measure the impact of the reform relative to earnings one year before.<sup>14</sup>

Finally, notice that our data are not a random sample and include almost the entire population of interest, as emphasized earlier. To interpret the estimated standard errors, we rely

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<sup>13</sup> In our main estimates, we do not add any additional control variable for the predetermined observable characteristics of households. In practice, adding such variables has no effect on the results, consistent with evidence reported below that there are no statistically significant differences between the two groups.

<sup>14</sup> As the model is saturated with respect to groups and periods, this approach is equivalent to estimating separately for each outcome  $Y_{it}$  associated with period  $t$  the difference-in-differences model  $Y_{it} = \alpha_t + \gamma_t J_i + \lambda_t R_i + \delta_t (J_i \times R_i) + u_{it}$ .

on the notion of “superpopulation”, in which the population from the sample is conceptualized as a random sample from a larger superpopulation (Imbens and Wooldridge 2009).

### **Validity of the empirical approach**

Our ability to estimate a causal effect of the reform depends on the validity of the parallel-trend assumption. This assumption implies that absent the reform, the differences in outcomes between households with a child in December relative to January would have remained stable over time. The plausibility of this hypothesis can first be assessed in Figure 1, which compares the participation rate of parents on parental leave across different cohorts up to 2 years before and after the reform. Clearly, the data do not reject the parallel trend hypothesis: the participation rates for births in January and December are statistically indistinguishable in the years before and after the reform, in contrast to the reform year.

**Smoothness of the daily birth distribution** A concern of this study is that the reform might have influenced the birth timing of households, which could introduce nonrandom selection between households affected or not by the reform. These endogenous compositional changes might impair our ability to estimate a causal effect, as any difference in outcome associated with the reform might reflect its effects on the composition of households in addition to their behavior. Using daily birth aggregate data, we investigate in supplementary Appendix A4 whether there are discontinuities in the distribution of births around the threshold of the reform. We find little evidence of a jump around the threshold in the data and cannot reject the null hypothesis of no discontinuity. However, our ability to detect manipulations of the date of birth is limited, as the daily birth data do not distinguish first births from higher-order births, and as discussed in Appendix A4, the incentive for manipulation goes in the opposite direction for first- and second-time parents.

**Observable differences between births in December and January** In Table 2, we directly investigate whether the composition of households between births in December and January

changed differentially in the reform year relative to the previous year. If the reform did not influence the selection around the cutoff, the differences between households around the cutoff in the reform year should be similar to those observed in the previous year. In Column (3), we report simple difference estimates that capture how the two groups around the cutoff differ in the reform year, while Column (4) reports difference-in-differences estimates that test for any differential change in composition. Both the simple differences and difference-in-differences estimates reveal no significant differences in the composition of households across the indicated variables. We find no significant differences in the average age of the mother or the father, the average earnings before the child's birth, the share of mothers with zero earnings before the child's birth, or the number of children in the household.<sup>15</sup>

Another issue is that our analysis focuses on couples with children who are affected by the reform. A selection bias might arise from this restriction if the reform influences the couples' separation probability. Additionally, as having another child renews parents' eligibility for paid parental leave, our estimates might be affected if there is an endogenous fertility response to the reform. However, as reported in Appendix Table A1, we find no effects of the reform on the probability of separation or fertility at the 3- and 5-year horizons.

A possibility is that these nonsignificant results reflect the combination of offsetting effects if the risk of separation is simultaneously increased for some households and reduced for others. While we cannot rule out this possibility, such effects are likely to be small, as the estimates are rather precise. In practice, the 95 percent confidence intervals rule out an increase of more than 1 percent in the probability of separation.

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<sup>15</sup> We use earnings measured two years before the child's birth, as they are not affected by slight differences in the timing of pregnancy between these two groups, because mothers who gave birth in December 2014 became pregnant slightly earlier than those who gave birth in January 2015.

## Effects of the reform on parental leave take-up rate

### First-time parents

For first-time parents, if anything, we expect fathers' take-up rate to increase, as the reform earmarked 6 nontransferable months to each parent, while previously the parents could only share 6 months together. Consistent with this hypothesis, our estimates in Panel A in Table 3 indicate that more fathers participated after the reform. However, the estimated effects are small, as they suggest that the reform increased the probability of taking at least one month of leave by only 0.4 p.p. While small in absolute value, the effect is nevertheless large in relative terms. Compared to the low baseline rate of 1% before the reform, the reform increased the participation rate of fathers by 40%, to 1.4%, and the average number of months taken by fathers also increased, by 50%. Despite this, fathers' participation rate remains extremely low relative to mothers' 25% participation rate.

Figure 2 provides visual evidence of mothers' and fathers' monthly take-up rates of full- and part-time parental leave. For mothers, the reform increased the probability of taking parental leave in the 9<sup>th</sup> and 10<sup>th</sup> months after birth. This result reflects that after the reform, mothers do not have to take months of parental leave consecutively after maternity leave.<sup>16</sup> For fathers, a significant impact is observed only after the 6<sup>th</sup> month following birth, thus suggesting that the small effect of the reform is driven by fathers who took parental leave after the mother took six consecutive months of leave after birth.

Table 4 compares the percentage of eligible part-time workers in the population with the percentage who took parental leave. As discussed earlier, if there are no participation costs or stigma, any parent working part-time for at least one month during the eligibility period should take parental leave. In practice, however, the take-up rates are low: even if 6% of first-time

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<sup>16</sup> Maternal leave is extended in the case of premature birth, which explains why small rates of parental leave in the 9<sup>th</sup> month before the reform can be observed.

fathers worked part-time, only 1% of fathers took a month of part-time parental leave. Taken literally, these figures imply that 85% of fathers did not take the minimum 150 euros in monthly benefits of paid parental leave for which they were eligible. For mothers, these take-up rates are higher, close to 50%, consistent with earlier studies (Reinstadler 2000). While these results should be interpreted with caution, as they rely on aggregate statistics from two different sources, they point to a large nontake-up of parental leave from both parents that is remarkably larger for fathers.

### **Second-time parents**

The reform earmarked 12 months of parental leave for second-time parents while reducing the maximum length from 36 to 24 months after birth. As expected, event-study estimates in Figure 3 show that after the 24-month threshold, the reform is associated with a decline in participation for mothers of more than 25 p.p. after the 25<sup>th</sup> month.<sup>17</sup> This decline is progressive, reflecting that some mothers do not take the maximum of 24 consecutive months after birth, even if a large majority do.

In response to such a large decline, the share of fathers taking part-time parental leave increased by only 0.8 p.p. after the 25<sup>th</sup> month, an increase entirely accounted for by part-time leave. For first-time parents, while modest in absolute terms, the increase is substantial in relative terms. For part-time parental leave, the share of fathers taking at least one month of leave increased by 50%, from 1.5 to 2.3 p.p. Despite this increase, such rates remain low relative to the fact that 9.6% of second-time fathers work part-time in the population, as reported in Table 4. Taken literally, these estimates imply that 81% of the eligible fathers working part-

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<sup>17</sup> As highlighted earlier, the group of second-time parents includes parents of a third, fourth, or other additional children. As the length of compulsory maternity leave increases with the number of children, the first month of paid leave for the mother varies from the 3<sup>rd</sup> to the 6<sup>th</sup> month, which explains why we start the figure at the fifth month.

time are not taking the paid leave benefits to which they are entitled. For mothers, the nontake-up rates are dramatically lower, as they are close to 33%.

### **Effects of the reform on labor earnings and household income**

To understand the labor supply response to the reform of fathers and mothers, we examine how the reform affected the household's income and its components. As the income data are obtained from fiscal data, their frequency is annual and thus cannot be adjusted to the precise birth month. We define the 2015 calendar year as event year zero for births in both December 2014 and January 2015. By convention, the year zero is defined as the child's first year, from birth to her first anniversary.

We consider in Table 5 how the reform affected total household income over the eligibility period, that is, over the first year of the child for first-time parents and up to the third anniversary for second-time parents. For first-time parents, Panel A indicates that despite the 0.4 p.p. increase in fathers' participation, the reform had no significant negative effect on their average annual earnings. More generally, no statistically significant effects on any component of household income are observed for first-time parents.

For second-time parents, Panel B indicates that the reduction of 12 months in parental leave is associated, on average, with a 1,260-euro decline in paid benefits over the three-year eligibility period. In response, the average labor earnings of mothers increased by approximately the same amount, while the receipt of unemployment benefits also increased dramatically.

For fathers, despite the 1 p.p. increase in participation in parental leave, no negative effects on earnings are observed. In contrast, the coefficient is positive and large, and the point estimate is approximately half of the estimated effect, for mothers. However, the estimate is very imprecise, and the coefficient is statistically insignificant. Despite this, a large negative effect of the reform on fathers' earnings can be ruled out.



In response to the lack of parental leave take-up from fathers and the decline in the length of parental leave of mothers, Column 5 also reports an increase in the receipt of benefits associated with formal childcare for second-time parents.

### **Longer-run consequences**

Figure 4 reports the evolution of annual labor earnings for second-time parents, normalized relative to the year before the child's birth. The reform's positive effects on mothers' earnings are concentrated in the third year after birth (event year 2) and to a lesser extent in the first year after birth (event year 0). In contrast, the reform did not significantly affect earnings in any year after birth for fathers.

Figure 4 also reports the earnings up to three years after the eligibility period (event years 3 to 5), which allows us to capture any persistent effects of the reform on earnings. Interestingly, as early as the first year after the eligibility period (event year 3), we find no difference in average earnings between mothers affected or not affected by the reform, even though 25% of mothers took 12 additional months of paid leave before the reform. Such a surprising lack of difference in earnings is consistent with Kleven et al. (2020), who also find no persistent effect of parental leave on earnings. This implies that for mothers, the earning costs of taking parental leave are temporary and concentrated during the parental leave period.

### **Heterogeneity by prebirth earnings of mothers**

The imprecise effects of the reform on fathers' earnings can be explained by the fact that their responses vary with the potential earnings of the mother in the labor market. As discussed earlier, in households where mothers have low potential earnings, fathers might not take more parental leave but instead might increase their labor supply to compensate for the loss of benefits and the additional cost of childcare.

To investigate this hypothesis, we approximate mothers' potential earnings using their earnings two years before the child's birth. We divide the population into five groups by separating first mothers who had no earnings two years before the child's birth, i.e., approximately 27% of mothers (see Table 2), and then separating mothers with strictly positive earnings into four groups using the quartiles of their prebirth earnings distribution.

Table 6 documents important differences in the effects of the reform among these groups. For the average number of months of parental leave, Panel A indicates that the reform mostly affected mothers from the distribution's second and third quartiles. However, Panel B shows that the decline in the amount of paid leave benefits is quite similar across groups except for the fourth quartile. Panel C shows that the positive effects of the reforms on fathers' participation in parental leave are concentrated in households in which mothers have the highest potential earnings, i.e., from the third and fourth quartiles of the distribution.

Despite these large variations in the impact of the reforms on mothers, Panel D shows no statistically significant effect of the reforms on fathers' earnings in any of these groups. Across all groups, the estimated coefficients are all positive, but the estimates are imprecise. If anything, the coefficient is larger in households where the mother had no earnings before the child's birth but also, somewhat surprisingly, in the fourth quartile. The fact that the coefficients are large and positive rules out a strong negative effect of the reform on earnings in most groups.

For mothers, on the other hand, Panel E shows a large and statistically significant increase in their labor earnings, mostly in the second and third quartiles. For those in other quartiles, the point estimates are ten times lower and even negative in the first quartile. Consistent with this result, Panel F reports a large increase in the receipt of unemployment benefits for mothers in the first quartile for whom we find no positive effect of the reform on labor earnings. In addition, Panel G shows that the increase in childcare subsidies associated with formal childcare also is

concentrated on mothers in the second and third quartiles, which are those in which labor earnings increase substantially.

For total income in Panel H, even though the decline in paid benefits is substantial in some groups, we do not find any significant negative average effects of the reform, even in groups where mothers have the lowest prebirth earnings. Nevertheless, the sign of the coefficient varies among groups, and the estimates are imprecise.

### **What explains the low take-up rates of fathers?**

Several mechanisms could explain the low take-up rates of fathers. To gain insights into these mechanisms, we first examine whether an initially imperfect knowledge of the reform might explain the low take-up rates of fathers. Next, we examine the role of gender norms by estimating differences in fathers' take-up rates across subgroups of parents for whom the influence of gender norms might vary.

### **Did the take-up rate of fathers increase later?**

A possible explanation for the low take-up rates of fathers is that they initially lacked information about the reform. Despite the communication campaign, many fathers might not have been immediately aware of the reform.<sup>18</sup> In addition, even though the application process is simple, as discussed earlier, fathers might not be familiar with the programs of the family benefits administration. In practice, our administrative data indicate that the mother is the household member responsible for correspondence with the family benefit administration in 77% of households.

If the slow information diffusion explains the fathers' lack of participation, then the share of fathers taking paid leave should increase over time, as knowledge of the reform spreads in the population. To assess this explanation, we report in Figure 5 the share of fathers taking

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<sup>18</sup> See Chetty et al. (2013) for evidence that the knowledge of welfare reforms in the population spread over time.

parental leave for births occurring one and two years after the reform, in January 2016 and 2017.

Overall, we find little evidence of fathers' increased participation in more recent cohorts, particularly for second-time fathers. For first-time fathers, we find a 0.2 p.p. increase in participation between 2015 and 2016, but the differences are not statistically significant.

As the participation rates remained low, the percentage of eligible fathers working part-time remained dramatically larger than the percentage taking paid leave. We estimate in Table 4 that nontake-up rates tend to decline for first-time fathers, consistent with a diffusion of information about the reform over time. While these nontake-up rates must be interpreted cautiously, they remain greater than 80% over time.

### **Differences in the response across groups of fathers**

As we find no significant correlations between the mothers' past earnings and the fathers' response to the reform, a possibility is that gender norms might be more important in influencing the participation of fathers. While we do not observe gender attitudes in our data, we investigate whether there are substantial response differences among fathers related to differences in their working status, location, or prebirth earnings.

**Effects of the reform on independent workers** To receive paid parental leave, employees must ask their employer to fill out a one-page form for family social security that certifies that they work part-time or have stopped working entirely. Qualitative studies suggest that fathers asking for parental leave might be stigmatized by their employer (Coltrane et al. 2013; Kaufman 2018; Haas and Hwang 2019), and real or supposed employer resistance might be an important barrier to fathers taking leave. Alternatively, peer effects in the workplace might discourage fathers from taking leave (Dahl, Løken, and Mogstad 2014).

If employer stigma or peer effects are important factors in the decision, we should observe a higher response to the reform from self-employed workers who do not have an employer and

declare to the social security administration that they have reduced their working hours to take paid parental leave. To investigate this hypothesis, we report separate estimates for self-employed workers in Column 2 of Table 7. The estimates show that self-employed fathers are two to three times more likely to take leave after the reform, as the estimated coefficient is two to three times larger than the baseline rate in the population. However, these results must be interpreted with caution, as they might also reflect that it is easier for self-employed workers to falsely declare a reduction in work hours and receive undue benefits (Chetty, Friedman, and Saez 2013). In addition, these results could reflect that independent workers are more familiar with dealing with administrative procedures.

**Differences in fathers' earnings** The simple model of labor supply discussed earlier predicts that, *ceteris paribus*, fathers with lower earnings are more likely to respond to the reform and take more leave, as the opportunity cost of paid leave is lower for them. On the other hand, if gender attitudes are a more important factor in the decision, the relationship between fathers' earnings and participation in parental leave after the reform might not be straightforward. Men with higher economic status tend to have less conservative attitudes toward gender roles, which can compensate for the higher opportunity cost of taking parental leave for them (Papuchon 2017).

To test for differences in response to the reform relative to the prebirth earnings of fathers, we report in Columns 3 to 6 in Table 7 separate estimates depending on the quartile of the fathers' annual labor earnings two years before the child's birth. The results confirm that there are large differences in response across fathers that are associated with their labor earnings. Fathers with lower levels of prebirth earnings are much less likely to take paid leave in response to the reform. For first-time parents, the response from fathers in the third quartile is four times the response from those in the first quartile. For second-time parents, we find a significant effect of the reform only on fathers in the fourth quartile but no effect on those in the other quartiles.

However, we do not find any effects in the fourth quartile for first-time parents, in contrast to second-time parents.<sup>19</sup> Overall, the fact that fathers with higher earnings are more likely to take leave is consistent with an important role of gender values relative to earnings in the decision. However, as stated previously, these results must be interpreted with caution, as they might also reflect that higher-earning fathers are more able to deal with administrative procedures.

**Local differences in response** Gender attitudes vary widely across regions in Europe, even within countries (Lalive and Stutzer 2010; Powers et al. 2003), and these local differences have important consequences on the labor market of women (Janssen, Tuor Sartore, and Backes-Gellner 2016). If local attitudes influence take-up rates, fathers should be more likely to take parental leave after the reform in regions where the share of fathers taking leave was larger before the reform. To investigate this hypothesis, we perform separate estimates depending on whether the share of fathers taking leave in the county of residency (*département*) one year before the reform is below or above the median. In accordance with the previous evidence that fathers with higher earnings participate more, Table A2 shows that counties with higher father participation rates before the reform were characterized by higher prebirth earnings for both fathers and mothers, particularly for second-time parents, and a lower share of mothers without earnings before the child's birth.

Columns 7 and 8 in Table 7 report separate estimates relative to these groups. Consistent with the hypothesis that a higher local share of fathers taking leave is correlated with a more favorable local attitude toward fathers taking leave, we find that all the effects of the reform on fathers' participation are driven by counties with an above-median share of fathers taking leave before the reform. In these counties, the estimated effects of the reform are twice as large as the

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<sup>19</sup> The low take-up rate among fathers with low prebirth earnings could also reflect that many are on a temporary contract and do not have proper job protection if they take parental leave. Unfortunately, the type of contract is not reported in the administrative data, and a separate analysis cannot be performed. In any case, these effects are likely to be small; according to our estimates from the Labor Force Survey, only 6% of fathers are on a temporary contract in our group.

baseline estimate. In contrast, in counties with below-median leave, we find little effect of the reform on the take-up rate of fathers.

## **Discussion**

As fathers' participation in parental leave remains low, many countries are reforming their parental leave programs to increase their participation. Inspired by 'daddy months' policies in Scandinavian countries, recent reforms earmarked fathers' specific months of parental leave to increase their participation. As the levels of benefits offered tend to be much lower than those in Scandinavian countries, the impact of such reforms is uncertain in this context. To investigate this issue, this paper studied a 2015 reform of French parental leave that earmarked 6 to 12 nontransferable months of leave for fathers compensated with a fixed and low level of benefits while simultaneously reducing the length of parental leave of many mothers.

Our investigation suggests that earmarking alone does not substantially increase fathers' participation. The substitutability of parental leave between parents also appears to be low. In response to a 25 p.p. decline in participation by mothers triggered by the reform, fathers' participation increased by only 0.8 p.p, primarily through part-time parental leave. While this result is statistically insignificant and imprecise, we also find a positive effect of the reform on fathers' earnings, consistent with the theoretical prediction that reducing the mother's parental leave might encourage some fathers to work more instead of taking parental leave. Overall, we cannot reject the hypothesis that the reform led some fathers to increase their labor supply instead of taking parental leave.

A limitation of our work is that we cannot disentangle the role of the low level of compensation from the role of gender norms in explaining the low participation of fathers. Recent survey evidence suggests that low levels of benefits and career concerns are fathers' most important barriers to participation in France (Sponton 2022). That the level of benefits plays a substantial role in a father's decision is consistent with the large participation rate of

fathers in the short and well-compensated French *paternity* leave program, which offers eleven days of leave compensated at 80% of the wage. In contrast to the parental leave program studied here, which attracts less than 3% of fathers after the reform, more than 70% of fathers participate in the paternity leave program.

Other empirical evidence we uncover also is consistent with the role of stigma in explaining the low take-up rates of fathers, compatible with traditional models of gender identity (Bertrand, Kamenica, and Pan 2015). We find evidence that most fathers working part-time do not participate in parental leave even though taking paid part-time leave would have increased their monthly income by approximately 200€. Their share does not decline much over time, thus suggesting that a lack of information about the program cannot be the sole explanation for their nonparticipation. Additionally, independent workers who did not suffer stigma from their employers and fathers living in a region where other fathers were more likely to take leave before the reform were twice as likely to take more parental leave after the reform. Fathers with higher incomes also were more likely to take more leave time, which is consistent with evidence that they are less influenced by traditional gender norms.



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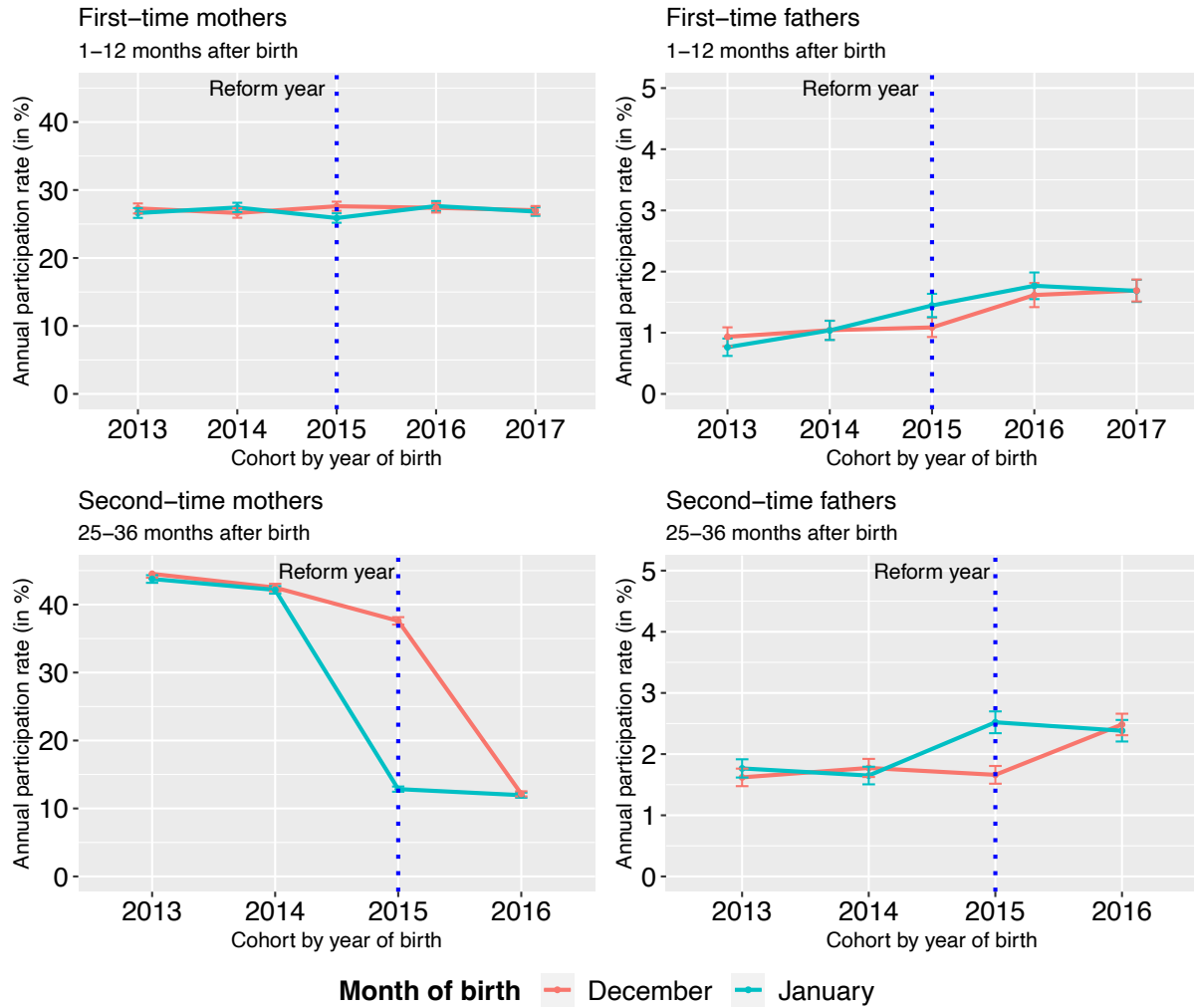
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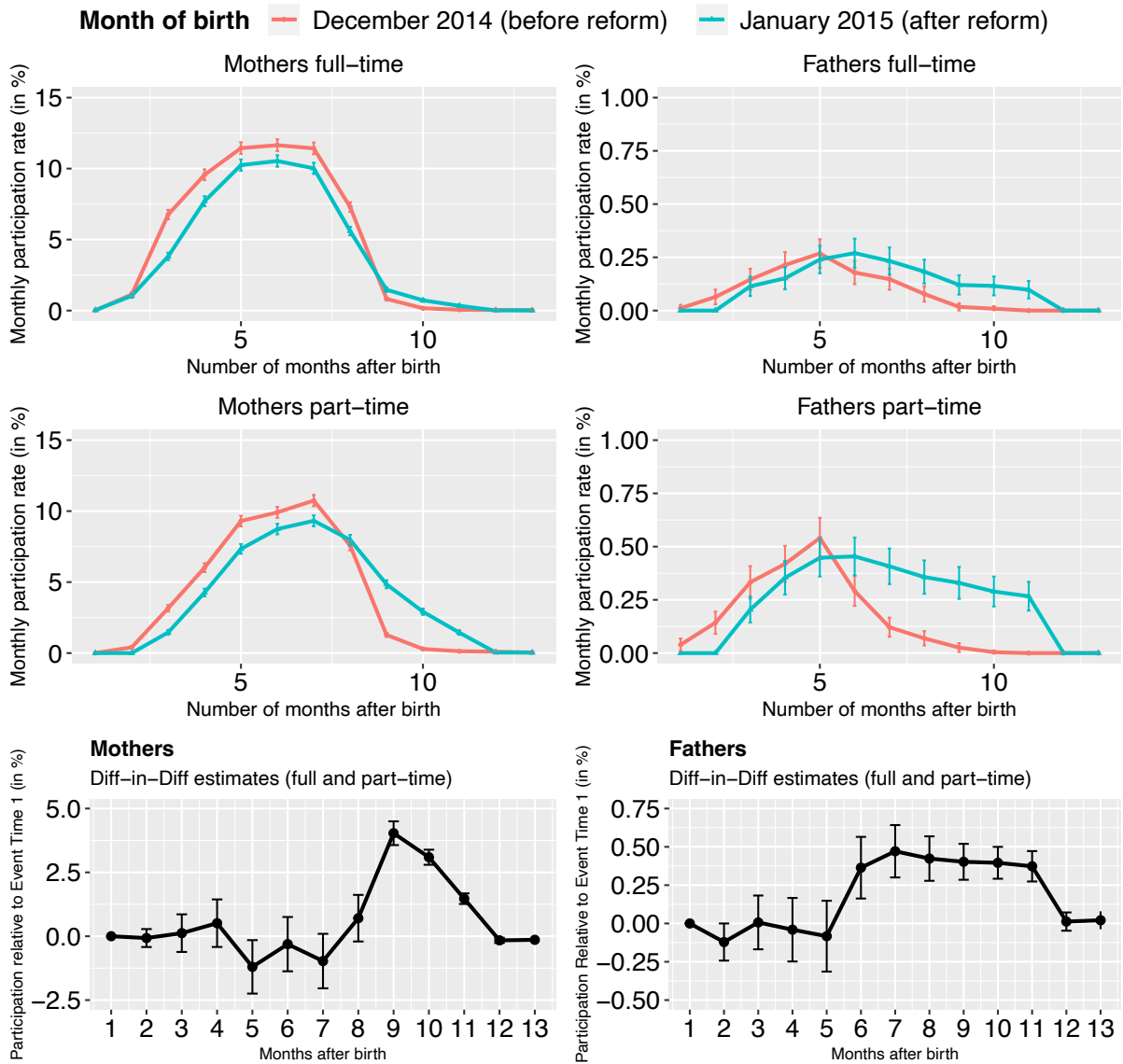
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Figure 1: Cross-cohort comparisons for second-time parents



Source: Monthly social security files (*Allstat* and *Basestat* from CNAF). Note: The figure compares the annual participation rates across cohorts of parents of children born in January of the indicated year relative to those born one month before in December of the previous year. The annual participation rate is defined as the probability to take at least one month of parental leave during the eligibility period. Participation rates of first-time parents are reported in the first row and in the second row for second-time parents. Participation rates of mothers are reported in the first column and for fathers in the second column.

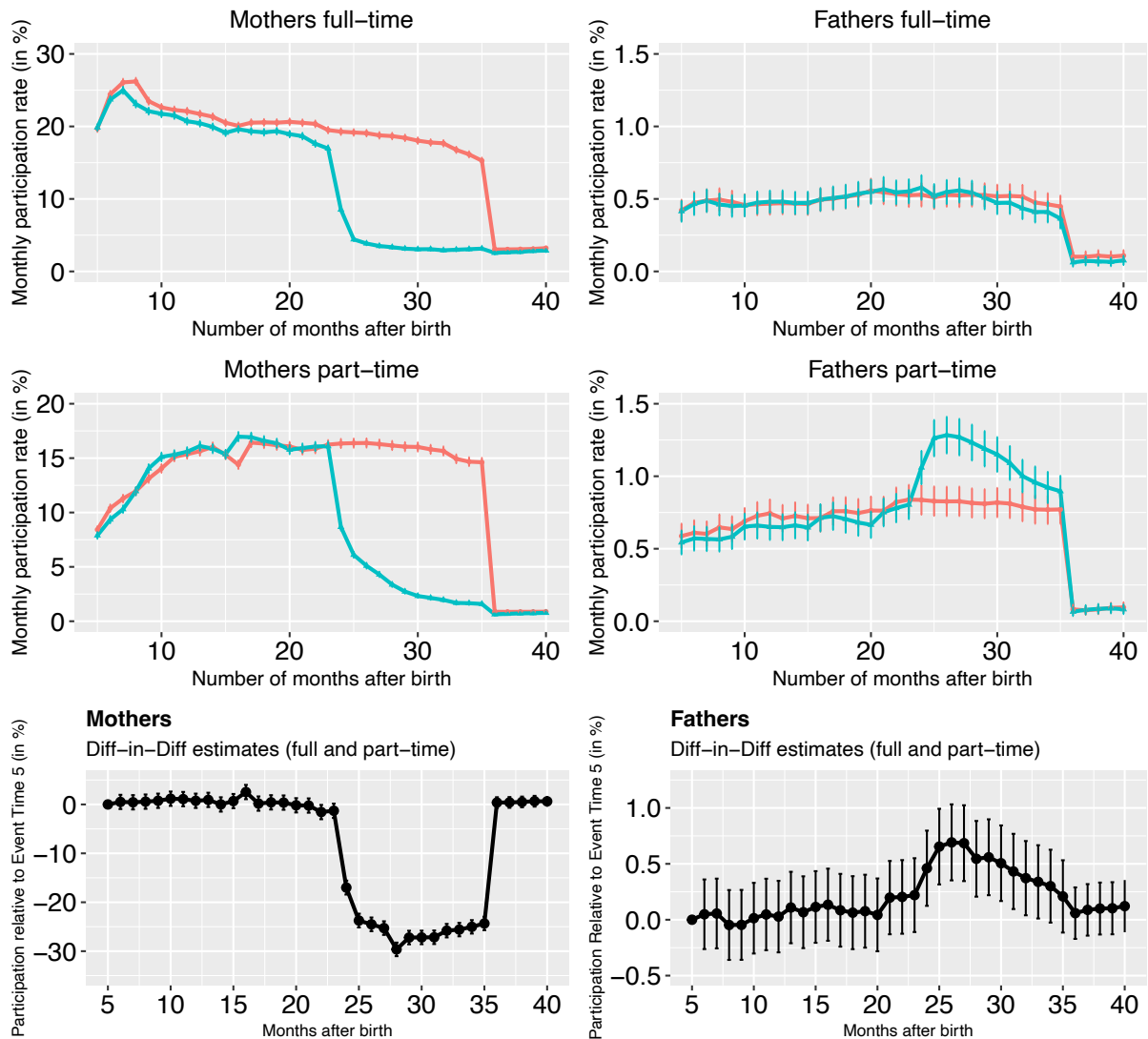
Figure 2: Monthly parental leave participation rates, first-time parents



Source: Monthly social security files (*Allstat* and *Basestat* from CNAF). Note: The graphs in the first two rows represent the monthly rates of participation in parental leave of first-time fathers and mothers of a child born in December 2014, in red, and in January 2015, in blue. The graphs show these rates for full-time leave in the first row and for part-time leave in the second row. The third-row reports event time coefficients estimated using the difference in differences model from Eq. (2), using births in December 2014/January 2015 and in December 2013/January 2014, including a full set of month dummies.

Figure 3: Monthly parental leave participation rates, second-time parents

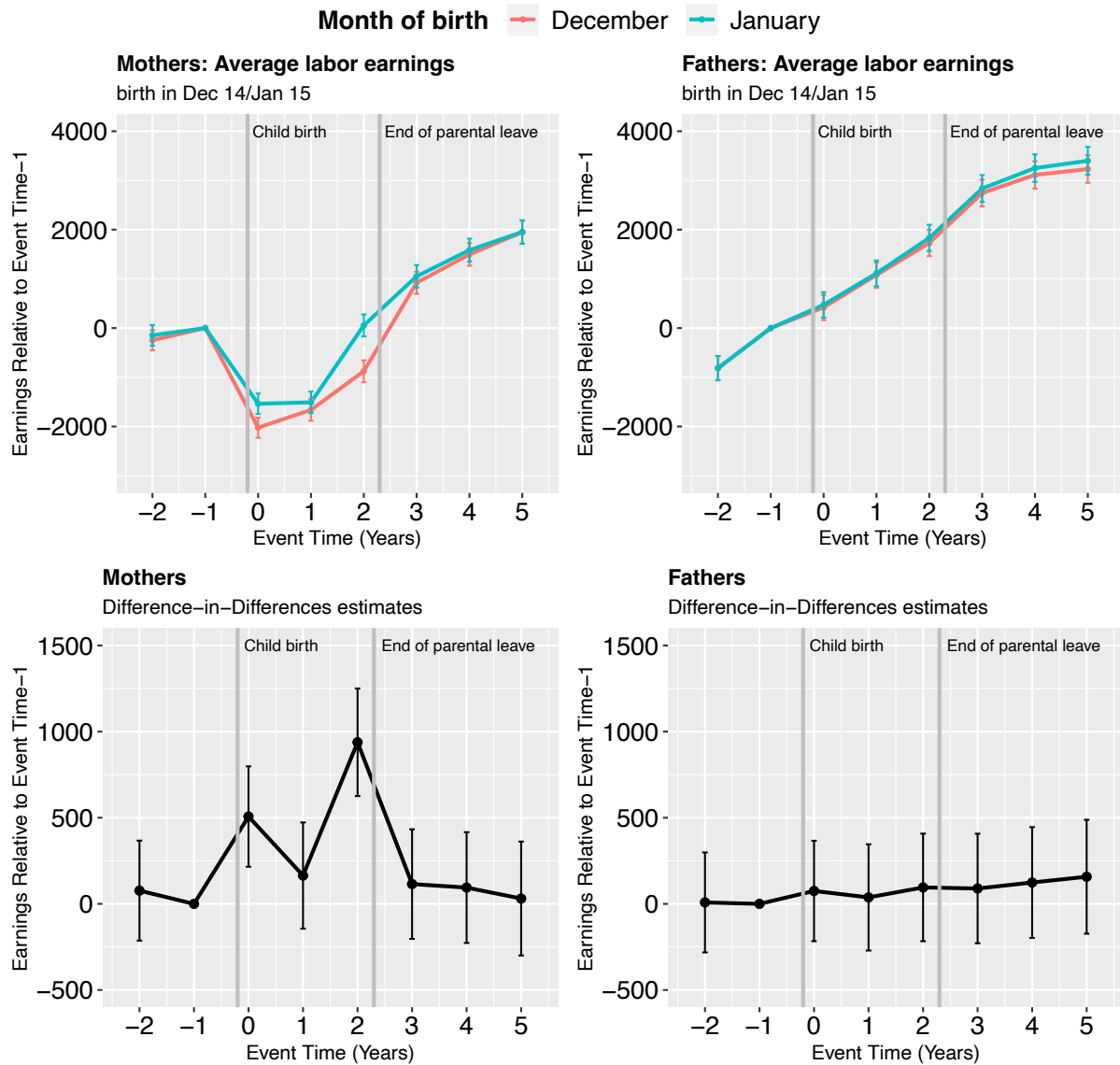
Month of birth — December 2014 (before reform) — January 2015 (after reform)



Source: Monthly social security files (*Allstat* and *Basestat* from CNAF). Note: The graphs in the first two rows represent the monthly rates of participation in parental leave of second-time fathers and mothers of a child born in December 2014, in red, and in January 2015, in blue. The graphs show these rates for full-time leave in the first row and for part-time leave in the second row. The third-row reports event time coefficients estimated using the difference in differences model from Eq. (2), using births in December 2014/January 2015 and in December 2013/January 2014, including a full set of month dummies.

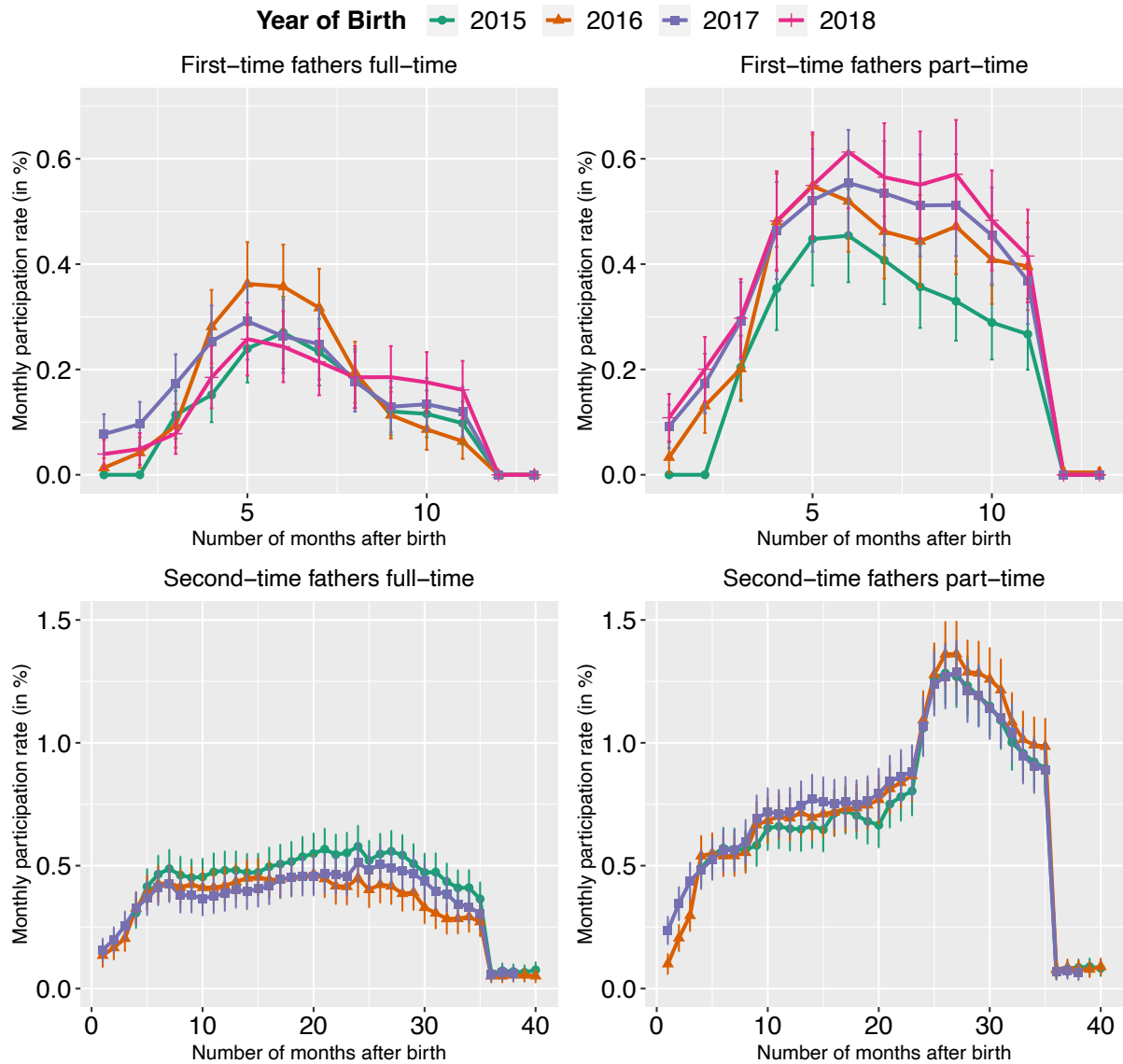


Figure 4: Annual labor earnings and unemployment benefits for second time-parents



Source: Monthly social security files (*Allstat* and *Basestat* from CNAF). Note: The graphs on the first row compare the average annual labor earnings of mothers and fathers for birth in December 2014, in red, and in January 2015, in blue. The years are defined with respect to the distance from the year of birth (event year zero). The second-row reports event time coefficients estimated using the difference-in-differences model from Eq. (2), using births in December 2014/January 2015 and in December 2013/January 2014, including a full set of year dummies.

Figure 5: Monthly parental leave participation rates for fathers for births in January in years after the reform



Source: Monthly social security files (*Allstat* and *Basestat* from CNAF). Note: The graphs represent the monthly share of first and second-time fathers taking a part- or full-time paid parental leave benefits for birth in January of the indicated year. The first and second rows show these figures for first- and second-time fathers, respectively. The left and right columns show the share of full- and part-time leave, respectively.

Table 1: The parental leave reform

Period	<b>Before the reform:</b> <i>Births before 1<sup>st</sup> January 2015</i>	<b>After the reform:</b> <i>Births after 1<sup>st</sup> January 2015</i>
<i>A. First child</i>		
Length	6 months max to be taken consecutively after the end of the maternity leave, each month can be taken by any parent: $p_m + p_f \leq 6$	6 months max for the mother & 6 months max for the father: $p_m \leq 6, p_f \leq 6$
Benefits	≈ 400€ full-time, 250€ up to 50% standard working hours, 150€ up to 80% standard working hours	Unchanged
Age of child	Maximum of 6 months of age plus the length of the maternity leave	Before 1st birthday
Eligibility of the parent	Minimum level of earnings corresponding to one year of work at the minimum wage in last 2 years.	Unchanged
<i>B. Second children</i>		
Length	36 months max, each month can be taken by any parent: $p_m + p_f \leq 36$	24 months max per parents, exceptional prolongation for a few months possible for low-income households, 36 months max in total for both parents: $p_m + p_f \leq 36,$ $p_m \leq 24, p_f \leq 24$
Benefits	Similar to those for a first child	Unchanged
Age of child	Before 3rd birthday	Unchanged
Eligibility of the parent	Minimum level of earnings corresponding to one year of work at the minimum wage in last 4 years, in last 5 years if more than 2 children. The previous periods of parental leave count as work	Unchanged

Note:  $p_f$  and  $p_m$  denote the months of parental leave taken by the mother and father, respectively

Table 2: Predetermined Characteristics of First- and Second-time parents in the reform year

	(1)	(2)	(3)	(4)
Birth in	<i>Before reform</i> December 2014	<i>After reform</i> January 2015	Simple Difference	Difference-in- differences
A. First-time parents				
Age mother	28.9	28.9	-0.008	-0.015
			(0.046)	(0.064)
Age father	31.7	31.7	-0.000	-0.051
			(0.058)	(0.079)
Earnings of father in 2013	19,839	20,104	-264.9	218.8
			(177.3)	(243.6)
Earnings of mother in 2013	15,657	15,602	54.7	825.3
			(190.8)	(553.1)
Share mothers with zero earnings in 2013	16.4%	15.8%	0.006	-0.004
			(0.003)	(0.005)
N			46,023	94,566
B. Second-time Parents				
Age mother	32.1	32.1	-0.020	0.023
			(0.038)	(0.052)
Age father	35.3	35.4	-0.061	0.069
			(0.050)	(0.067)
Number of children	2.6	2.6	-0.011	-0.0003
			(0.007)	(0.010)
Number children aged 3 and 5	0.6	0.6	-0.004	0.006
			(0.005)	(0.006)
Earnings of father in 2013	21,527	21,773	-245.7	11.9
			(168.8)	(231.9)
Earnings of mother in 2013	13,193	13,064	128.7	350.1
			(161.2)	(193.4)
Share mothers with zero earnings in 2013	26.6%	26.7%	-0.001	-0.004
			(0.004)	(0.005)
N			62,749	125,056

*Source:* Monthly social security files (*Allstat* and *Basestat* from CNAF). *Note:* The table compares the average pre-determined characteristics of French households living as a couple that had a child in December 2014 (second column) and January 2015 (third column). Panel A compares these characteristics for households that are first-time parents while panel B reports these characteristics for second-time parents. Column (1) and (2) reports the average of the indicated variables for parents that had a child in December 2014 and January 2015, respectively. Column (3) and (4) shows estimates of a regression of the indicated predetermined characteristics on a birth in January after the reform dummy variable. Column (3) reports simple difference estimates while column (4) uses difference-in-differences specification including households with births in December 2013 and January 2014. Robust standard errors are reported in parenthesis.

Table 3: Regression estimates of the effect of the reform on paid parental leave take-up

	(1)	(2)	(3)	(4)	(5)	(6)
	Probability to take at least one month of paid leave			Number of months of leave taken		
	All leave	Full-time	Part-time	All leave	Full-time	Part-time
A. First-time parents, 1 to 12 months of age						
A1. Mothers						
After reform	0.006	-0.002	0.008*	0.058**	-0.019	0.078***
	(0.006)	(0.005)	(0.004)	(0.025)	(0.020)	(0.018)
Pre-reform means	0.25	0.14	0.12	1.06	0.59	0.47
A2. Fathers						
After reform	0.004***	0.002***	0.002*	0.017***	0.007**	0.011***
	(0.001)	(0.001)	(0.001)	(0.005)	(0.003)	(0.004)
Pre-reform means	0.009	0.003	0.007	0.03	0.01	0.02
N	94,566	94,566	94,566	94,566	94,566	94,566
B. Second-time parents, 1 to 36 months of age						
B1. Mothers						
After reform	-0.011*	-0.016***	-0.006	-3.287***	-1.849***	-1.438***
	(0.006)	(0.005)	(0.005)	(0.141)	(0.118)	(0.104)
Pre-reform means	0.45	0.31	0.22	0.105	0.60	0.45
B2. Fathers						
After reform	0.010***	0.002	0.008***	0.045	0.007	0.038
	(0.002)	(0.001)	(0.001)	(0.032)	(0.020)	(0.024)
Pre-reform means	0.024	0.010	0.015	0.37	0.14	0.23
N	125,056	125,056	125,056	125,056	125,056	125,056

*Source:* Monthly social security files (*Allstat* and *Basestat* from CNAF). *Note:* The Table shows regression results in which the dependent variable is, in columns 1 to 3, the probability to take one month or more of paid parental leave during the eligibility period and, in columns 4 to 6, the number of months of leave taken during the eligibility period. Panel A and B report these estimates for first- and second-time parents, respectively. Within each panel, the estimates are reported separately for mothers and fathers and for full- and part-time leave. The estimates are obtained using the difference-in-differences model from Eq. (1), using births in December 2014/January 2015 and December 2013/January 2014, including a year dummy and a month of birth dummy. Robust standard errors are reported in parenthesis. (\*), (\*\*), and (\*\*\*) denote statistical significance at, respectively, 10%, 5%, and 1% level.

Table 4: Estimated non-take-up of paid leave for parents working part-time after the reform

	First-time parents			Second-time parents
	1 to 12 months of age			1 to 36 months of age
	A. Fathers			
<i>Birth in January of the year</i>	2015	2016	2017	2015
Share Part-time work	6.1	6.3	6.2	9.6
Share Part time paid parental leave	0.9	1.2	1.2	1.8
Estimated Non-take-up rate	85.2	81.0	80.6	81.3
	B. Mothers			
Share Part-time work	27.0	28.2	28.5	28.8
Share Part time paid parental leave	13.2	11.9	11.9	19.1
Estimated Non-take-up rate	51.1	57.8	58.2	33.7

*Source: Echantillon démographique permanent for part-time work and Monthly social security files for participation to paid-leave benefits (Allstat and Basestat from CNAF). Note: The table compares the share of fathers and mothers working part-time among the eligible population with their share taking at least one month of paid part-time parental leave according the family benefits files. Panel A considers fathers while panel B considers mothers.*

Table 5: Effects of the reform on household income

	(1)	(2)	(3)	(4)	(5)	(6)
Outcomes	Paid benefits of parental leave	Fathers' Labor Earnings	Mothers' Labor Earnings	Unemployment benefits	Childcare subsidies	Total Household Income
	A. First-time parents, 1 to 12 months of age					
After reform	-11.3	80.5	254.3	55.8	2.2	166.9
	(39.0)	(205.4)	(174.3)	(35.6)	(16.4)	(260.8)
N	70,406	70,406	70,406	70,406	70,406	70,406
Pre-reform means	4081	21,534	14,203	864	853	40,613
	B. Second-time parents, 1 to 36 months of age					
After reform	-1,259.8***	608.9	1,193.3***	324.4***	191.9***	-364.1
	(179.1)	(394.5)	(376.8)	(66.7)	(62.6)	(410.1)
N	99,521	99,521	99,521	99,521	99,521	99,521
Pre-reform means	7,274	74,859	36,749	1,731	2,865	120,613

*Source:* Monthly social security files (*Allstat* and *Basestat* from CNAF). *Note:* Each column in the table shows regression results in which the dependent variable is the amount of paid parental leave benefits (column 1), the labor earnings of fathers (2), of mothers (3), the unemployment benefits (4), the childcare subsidies (5), total household income (6) over the eligibility period. Panel A shows estimates for first-time parents, panel B for second-time parents obtained using earnings over the indicated eligibility period. The estimates are obtained using the difference-in-differences model from Eq. (1), including a year dummy and a month of birth dummy. Robust standard errors in parenthesis. (\*), (\*\*), and (\*\*\*) denote statistical significance at, respectively, 10%, 5%, and 1% level.

Table 6: Heterogeneous Effects of the Reform on Second-Time Parents

	(1)	(2)	(3)	(4)	(5)	(6)
	Quartile of mothers' earnings two years before birth					
Sample	All households	No Earnings	Q1	Q2	Q3	Q4
	A. Effect of the reform on Number of months of leave taken by the mother					
After reform	-3.605***	-1.441***	-3.174***	-5.241***	-5.792***	-2.775***
	(0.161)	(0.266)	(0.381)	(0.360)	(0.360)	(0.346)
	B. Paid leave benefits received by the household					
After reform	-1,259.8***	-1,443.1***	-1,180.3***	-1,609.7***	-1,301.2***	-598.1**
	(179.1)	(430.2)	(405.1)	(301.7)	(256.2)	(269.5)
	C. Probability to take at least one month of paid leave for fathers					
After reform	0.011***	0.002	-0.003	0.008*	0.024***	0.028***
	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)
	D. Fathers' Labor Earnings					
After reform	608.9	964.3	219.4	452.2	403.1	1,303.7
	(394.5)	(761.3)	(906.4)	(810.0)	(752.2)	(828.5)
	E. Mothers' Labor Earnings					
After reform	1,193.3***	315.4	-228.4	3,134.1***	3,170.5***	211.9
	(376.8)	(242.8)	(505.8)	(596.2)	(600.4)	(658.7)
	F. Unemployment benefits					
After reform	324.4***	117.3*	449.1***	548.4***	309.0**	332.1
	(66.7)	(63.3)	(141.3)	(166.6)	(142.8)	(222.4)
	G. Childcare subsidies					
After reform	191.9***	16.0	154.5	546.9***	364.2**	-28.4
	(62.6)	(48.5)	(114.5)	(155.6)	(164.4)	(149.4)
	H. Total Household Income					
After reform	-364.1	175.6	486.1	-937.7	68.2	-931.1
	(410.1)	(569.0)	(1,147.8)	(1,074.7)	(1,005.5)	(959.3)
N	99,521	25,014	18,629	18,625	18,627	18,626

*Source:* Monthly social security files (*Allstat* and *Basestat* from CNAF). *Note:* Each panel in the table shows regression results in which the dependent variable is the number of months of leave taken (panel A), the amount of paid parental leave benefits (B), the labour earnings of fathers (C), of mothers (D), the unemployment benefits (E), the childcare subsidies (F), total household income (G) over the eligibility period. Column 1 shows the estimates for all households while column 2 uses households in which the mother had no earnings two years before the birth. Columns Q1-Q4 report estimates performed separately on groups defined by the quartiles of the distribution of the mother earnings two years before the birth, conditional on being positive. The estimates are obtained using the difference-in-differences model from Eq. (1), including a year dummy and a month of birth dummy. Robust standard errors in parenthesis. (\*), (\*\*), and (\*\*\*) denote statistical significance at, respectively, 10%, 5%, and 1% level.



Table 7: Differences in response of fathers to the reform across households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Father independent worker	Pre-birth quartile in the earning distribution of the father				Local pre-reform participation rates of fathers in department	
			Q1	Q2	Q3	Q4	Above median	Below median
	Dependent variable: Number of months of leave taken by the father							
	A. First-time parents							
After reform	0.017***	0.025*	0.010*	0.018*	0.043***	-0.001	0.029***	0.005
	(0.005)	(0.013)	(0.005)	(0.011)	(0.013)	(0.010)	(0.008)	(0.006)
Pre-reform means	0.032	0.023	0.008	0.043	0.045	0.033	0.039	0.026
N	93,397	6,837	23,349	23,345	23,353	23,350	46,540	46,857
	B. Second-time parents							
After reform	0.043	0.149**	0.020	0.033	-0.001	0.130***	0.119**	-0.026
	(0.031)	(0.072)	(0.063)	(0.072)	(0.069)	(0.045)	(0.049)	(0.040)
Pre-reform means	0.37	0.14	0.34	0.46	0.52	0.18	0.42	0.33
N	121,987	10,664	30,075	30,737	30,789	30,386	60,276	61,711

*Source:* Monthly social security files (*Allstat* and *Basestat* from CNAF). *Note:* The Table shows regression results from difference-in-differences models in which the dependent variable is the probability to take at least one month of paid-parental leave during the first year of age for first-time parent and the third year of age for second-time parents. Panel A and B report estimates for first- and second-time parents, respectively. Column 1 reports the baseline estimate for the entire population. Column 2 report the estimates performed separately for fathers that are categorized as independent workers. Columns 3 and 4 reports separate estimates depending on whether the households is living in a department where fathers take above or below median leave one year before the reform. Columns 5 to 8 reports separate estimates for groups categorized with respect to the quartile of the initial earning distribution of the father two years before birth. Robust standard errors are reported in parenthesis. (\*), (\*\*), and (\*\*\*) denote statistical significance at, respectively, 10%, 5%, and 1% level.

## Online supplementary appendix not for publication

### A1. Further details on parental leave and childcare in France

***Duration of Maternity leave:*** For the first and second child, paid maternity leave is mandatory. Maternity leave starts six weeks before birth and finishes ten weeks after birth at most. Starting with the third child, maternity leave starts eight weeks before birth and finishes eighteen weeks after birth, at most. It is mandatory to take at least six weeks after birth, and the length of maternity leave can be extended for medical reasons. The benefits correspond to 100% of the previous earnings up to a maximum of 2,500 euros.

***Eligibility to parental leave:*** For a first (and, respectively, the second and third) child, the parents must have contributed to the pension system for at least 8 quarters within the two (and, respectively, the four and five) years preceding the birth or adoption. To contribute to the pension system, the total earnings of the quarter must correspond to at least six weeks of a full-time minimum wage. Periods of paid leave also contribute to the pension system. These conditions are evaluated by the families' benefits administration when applying for leave. Taking paid leave does not diminish one's right to retirement benefits (Bonnet and Rapoport, 2020). Paid parental leave is associated with job protection only for those with at least one year of tenure before birth.

***Childcare cost and subsidies:*** Up to three years of age, financial assistance does not depend on the child's age but on whether a daycare center or a childminder is used. For a childminder, the average net hourly wage per child is 3€ per hour, corresponding to about 540€ per month for 9 hours per day. In compensation, parents receive financial aid from the "*complement de mode de garde*" which varies with three income categories. For parents of a single child, the financial aid corresponds to 460€ if their total income is below 1.5 minimum wages, 290€ for income levels up to 3.5 minimum wages, and 174€ if above. These thresholds create strong discontinuities in the cost of a childminder for parents close to different sides of the thresholds.

The thresholds associated with each income category increase with the number of children, but their amount does not depend on the number of hours paid to the childminder. The only requirement is that at least 15% of the cost must be paid by parents, and thus the financial aid cannot be superior to more than 85% of the childminder cost.

Daycare centers are also subsidized as parents pay an hourly cost that depends on their income, and the remaining cost is directly paid by the administration to the daycare center. In 2015, the hourly rate was 0.4 € for total household income below half of the minimum wage. After this threshold, the rate increases linearly from 0.4 € to 3 € up to 4 minimum wages, and above, the hourly rate remains at 3 €.

For both childminders and daycare centers, parents receive an additional tax credit of 1150 € max per child, which corresponds to at most 50% of the remaining cost.

## A2. Theoretical Model

To analyze the consequence of the reform, consider a simple model where each parent has to choose, over  $T$  months of eligibility, between  $l$  month of work and  $p$  months of leave such that  $T = l_i + p_i$  for  $i \in \{f, m\}$ , where  $f$  and  $m$  denote the father and mother, respectively. To make things simple, we neglect part-time leave to concentrate on the interactions between parents. We denote by  $(PL0)$  and  $(PL1)$  the constraints governing the allocation of the months of parental leave between parents before and after the reform, respectively. We consider that the household maximizes a well-behaved utility function  $U(c, p_f, p_m)$  that depends on pooled consumption  $c$  and on the parental leave taken by each parent, that we assume enter the utility function symmetrically, and are imperfect substitutes. The budget constraint is given by  $c = w_m(T - p_m) + w_f(T - p_f) + (p_m + p_f)b$ , where  $w_i$  is the monthly wage of parent  $i$  and  $b$  is the monthly leave benefit. We assume that the wages of fathers are such that  $w_f > w_m$  and  $w_f > b$ , so the opportunity cost of leave is higher for fathers, and mothers should always take more leave. However, we allow  $b$  to be superior or inferior to the wages of mothers  $w_m$ .

The problem of the household is to choose the optimal length of parental leave  $p_f^*$  and  $p_m^*$  to maximize the utility function  $U(c, p_f, p_m)$  under the budget constraint  $c = w_m(T - p_m) + w_f(T - p_f) + (p_m + p_f)b$  and the parental leave constraints which varies with the number of children and depends on whether the reform has been implemented or not. We assume  $p_f$  and  $p_m$  enter symmetrically in the utility function and each element are imperfect substitutes. We denote  $c^*$ ,  $p_f^*$ , and  $p_m^*$  the consumption and optimal choice of parental leave before the reform and  $\tilde{c}^*$ ,  $\tilde{p}_f^*$  and  $\tilde{p}_m^*$  after the reform. We assume the wages of fathers are such that  $w_f > w_m$  and  $w_f > b$ .

**Interior solution** When no parental leave constraints is binding and  $w_m > b$ , an interior solution

is given by  $U_c(c^*, p_f^*, p_m^*) = \frac{U_{p_f}(c^*, p_f^*, p_m^*)}{(w_f - b)} = \frac{U_{p_m}(c^*, p_f^*, p_m^*)}{(w_m - b)}$ .

**First-time parents** The reform changes the parental leave constraints from (PL0) :  $p_m + p_f \leq 6$  before the reform to (PL1) :  $p_m \leq 6$  and  $p_f \leq 6$  after the reform. Parents affected by the reform are parents in which the mother took all the leave before the reform and  $p_m^* = 6$

or parents that took all six months of leave  $p_m^* + p_f^* = 6$ . In this case, as the constraint is binding,

we have  $\frac{U_{p_f}(c^*, p_f^*, 6 - p_f^*)}{w_f - b} > U_c(c^*, p_f^*, 6 - p_f^*)$  before the reform. After the reform, fathers will

increase  $p_f^*$  to  $\widetilde{p}_f^*$  until  $\frac{U_{p_f}(\widetilde{c}^*, \widetilde{p}_f^*, 6)}{w_f - b} = U_c(\widetilde{c}^*, \widetilde{p}_f^*, 6)$  for an interior solution or both parents will

take six months if  $\frac{U_{p_f}(\widetilde{c}^*, 6, 6)}{w_f - b} > U_c(\widetilde{c}^*, 6, 6)$ .

**Second-time parents** The parental leave constraints are given by (PL0) :  $p_m + p_f \leq 36$  before the reform. The reform adds the following two constraints (PL1) :  $p_m \leq 24$  and  $p_f \leq 24$  which implies the reform affects households that would have chosen  $p_m^* > 24$  before the reform. To simplify, assume parents take the maximum of leave and that the constraint is binding before the reform such that  $p_f^* + p_m^* = 36$ . In that case, a standard Lagrangian analysis indicates that the optimal

choice  $p_f^*$  solves  $\frac{U_{p_f}(c^*, p_f^*, 36 - p_f^*)}{U_c(c^*, p_f^*, 36 - p_f^*)} - (w_f - b) = \frac{U_{p_m}(c^*, p_f^*, 36 - p_f^*)}{U_c(c^*, p_f^*, 36 - p_f^*)} - (w_m - b)$ .

This implies that  $\frac{U_{p_f}(c^*, p_f^*, 36 - p_f^*)}{(w_f - b)} > U_c(c^*, p_f^*, 36 - p_f^*)$ .

After the reform, the mother will take the max  $\widetilde{p}_m^* = 24 < p_m^*$ . When  $w_m > b$ , the consumption of the household increase after the reform and marginal utility  $U_c$  should decrease. As a result,

fathers will increase their parental leave up to  $\widetilde{p}_f^*$  to equalize  $\frac{U_{p_f}(\widetilde{c}^*, \widetilde{p}_f^*, 24)}{(w_f - b)} = U_c(\widetilde{c}^*, \widetilde{p}_f^*, 24)$ . On

the other hand, when  $w_m < b$ , then the reform decrease consumption if fathers do not work more. When the decline in  $p_m$  does not increase sufficiently the marginal utility of consumption,

we might have  $\widetilde{p}_f^* < p_f^*$  as fathers decrease parental leave until the marginal utility of parental leave of the father and the marginal utility of consumption are equalized.

### **A3. Data Appendix**

Data come from the monthly social security files constructed and exploited by the CNAF. We use the monthly BASESTAT database until April 2016 and monthly ALLSTAT files thereafter. When available, we use the FR6 files that are updated until 6 months after their initial production. To ensure confidentiality, these files have been accessed, and the analysis was performed on the CNAF premises in Paris. Tax returns report separately earnings received by each parent. Data on unemployment benefits is also reported at the household level and separately for each parent after 2017.

#### **A4. Smoothness of the daily birth distribution**

To detect manipulations related to the day of birth by households, we test for the smoothness of daily births' density around the reform's threshold. Theoretically, the direction of such manipulations might differ between first- and second-time parents. First-time parents should prefer to have a child *after* the reform, as they receive six additional months of paid leave for the other parent. On the other hand, second-time parents should prefer to have a child before the reform to avoid sharing 36 months of leave. In any case, if there is manipulation, we should observe unusual spikes in the number of births just before or after the threshold in the reform year.

As the exact day of the birth is not reported in the social security data for confidentiality reasons, we report in Figure A1 the histogram of the daily number of births from vital record data. Unfortunately, vital records data do not distinguish between first and second children. We compare the daily distribution of daily births in panel A for December 2014 and January 2015, which is the reform year, with the distribution for December 2013 and January 2014 in panel B, which was one year before the reform.

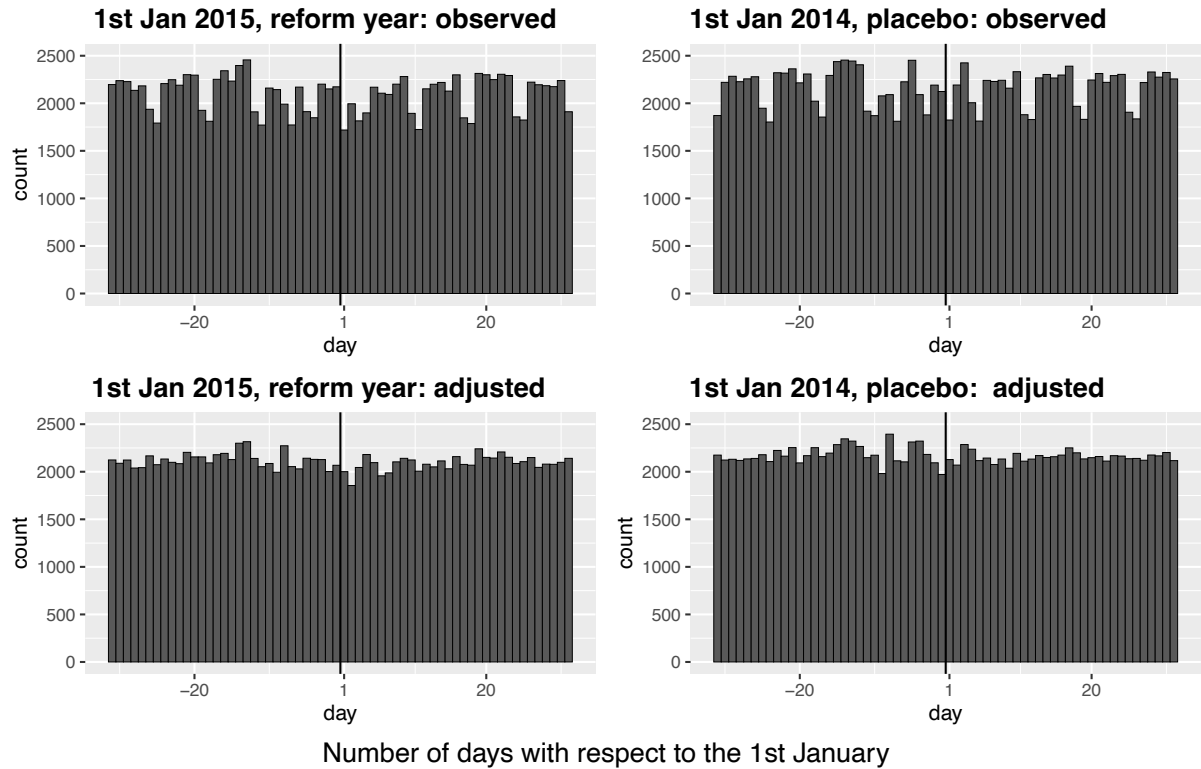
Clearly, the distribution of daily births is not uniform, as births are less frequent on the weekend and on days off, such as the 1st of January. To isolate any discontinuities from predictable calendar effects, panels C and D report histograms that have been adjusted using calendar day fixed effects estimated using data from daily births in November and in February in the same years. Overall, from both the observed and adjusted series, the visual evidence suggests that there is no exceptional spike in daily births before or after the 1st of January 2015. Using the test of Cattaneo et al. (2018), we cannot reject the null hypothesis of a no-density jump around the threshold in January 2015 either on the observed or adjusted daily birth series (p-value = 0.14 and 0.82, respectively).



In Figure A2, we consider an alternative approach to deal with the fact that the number of births is lower during the weekend. We aggregate instead birth by week to detect whether the reform year is characterized by an exceptional number of births the week before or the week after the reform. Once again, there is limited evidence that there was a strong difference relative to the previous year without reform.

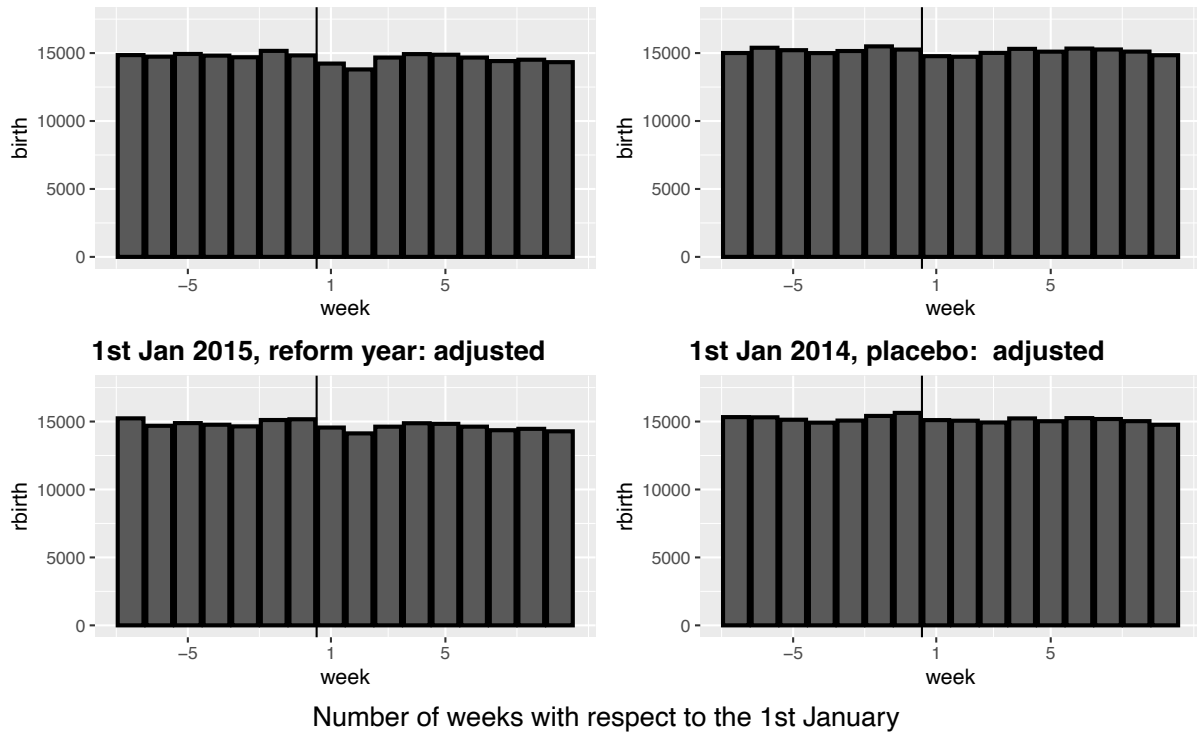
## Online Appendix Figures

Figure A1: Number of daily births around the 1st of January 2014 and 2015



*Source:* Daily distribution of live birth of metropolitan France from French Statistical Institute (*T79JNAIS*). *Note:* Each graph shows the observed or adjusted number of daily live birth in France for days relative to the 1st of January 2015 (left column) or 1st of January 2014 (right column). In the second row, the daily births are adjusted for weekend days and days off effects using a regression of daily births on days of the week and days off fixed effects using data from the closest months of November and February.

Figure A2: Number of weekly births around the 1st of January 2014 and 2015  
**1st Jan 2015, reform year: observed**      **1st Jan 2014, placebo: observed**



*Source:* Weekly distribution of live birth of metropolitan France from French Statistical Institute (*T79JNAIS*). *Note:* Each graph shows the observed or adjusted number of weekly live birth in France for days relative to the 1st of January 2015 (left column) or 1st of January 2014 (right column). In the second row, the weekly births are adjusted for weekend days and days off effects using a regression of daily births on days of the week and days off fixed effects using data from the closest months of November and February.

## Online Appendix Tables

Table A1: Effect of the reform on the probability of separation and fertility

	(1)	(2)	(3)	(4)
Outcome	Divorce or separation		Number of children	
	3 years	5 years	3 years	5 years
	A. First-time parents			
After reform	0.001	-0.003	0.006	-0.012
	(0.005)	(0.006)	(0.005)	(0.007)
N	94,566	94,566	94,566	94,566
	B. Second-time parents			
After reform	0.003	0.004	-0.001	-0.002
	(0.004)	(0.004)	(0.010)	(0.011)
N	125,056	125,056	125,056	125,056

*Source:* Monthly families benefits files (*Allstat* and *Basestat* from CNAF). *Note:* The Table shows difference-in-differences regressions using a sample of households with births in December 2014 and January 2015 and in December 2013 and January 2014. We consider the probability of divorce or separation of the household in columns 1 and 2 and the number of children in columns 3 and 4. Difference-in-differences estimates of the effects of the reform on these outcomes in the third year after birth in columns 1 and 3 and the fifth year after birth in columns 2 and 4. (\*), (\*\*), and (\*\*\*) denote statistical significance at, respectively, 10%, 5%, and 1% level.

Table A2: Characteristics of households in counties below and above the median participation rate to part-time leave

	(1)	(2)	(3)
A. First-time parents			
Fathers' participation rate to parental leave in county	<i>Below median</i>	<i>Above median</i>	<i>P-value</i>
Population density in county	2,340	1,073	0.00
Age mother	29.0	28.9	0.10
Age father	31.8	31.6	0.01
Earnings of father in 2013	19,805	20,116	0.27
Earnings of mother in 2013	15,753	15,715	0.91
Share mothers with zero earnings in 2013	17.3	15.4	0.00
<i>Share fathers taking parental leave after the reform</i>	1.32	1.61	0.00
N	11,036	10,399	
B. Second-time parents			
Population density in county	2,276	749	0.00
Age mother	32.1	32.2	0.13
Age father	35.4	35.3	0.03
Number of children	2.59	2.55	0.00
Number children aged 3 and 5	0.64	0.65	0.02
Earnings of father in 2013	21,247	21,954	0.00
Earnings of mother in 2013	12,938	13,565	0.00
Share mothers with zero earnings in 2013	28.9	24.4	0.00
<i>Share fathers taking parental leave after the reform</i>	2.97	3.77	0.000
N	13,898	15,762	

*Source:* Monthly social security files (*Allstat* and *Basestat* from CNAF). *Note:* The table compares the average pre-birth characteristics of French households that had a child in January 2015 between counties below and above the median participation rate in parental leave before the reform. Panel A compares these characteristics for households that are first-time parents while panel B reports these characteristics for second-time parents.

### References to the online Appendix

Bonnet, C. and Rapoport, B. (2020). Is there a child penalty in pensions? the role of caregiver credits in the French retirement system. European Journal of Population, 36(1):27–52.

Cattaneo, M. D., Jansson, M., and Ma, X. (2018). Manipulation testing based on density discontinuity. The Stata Journal, 18(1):234–261.



## ABOUT OFCE

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