

Does Earmarked Parental Leave attract Fathers?¹

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Abstract

Does earmarking part of parental leave 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 to fathers while simultaneously reducing the maximum paid leave for mothers by the same number of months. Using administrative data and comparing parents of children born before and after the reform, the authors find little increase in fathers' participation rates. In response to a 25 p.p. decline in mothers' participation rate triggered by the reform, fathers' participation increased by only 0.8 p.p, primarily through part-time parental leave. The losses of paid benefits associated with the parental leave reduction were compensated through a large increase in labor earnings and receipt of unemployment benefits by mothers. The reform had no significant effects on fathers' earnings.

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In most OECD countries, parents can take paid parental leave, known as paid family leave in the United States, if they stop working to take care of a young child. As it is most often taken by mothers, and following the early example of Scandinavian countries, in 2019, the European Union mandated that all member countries should offer separate periods of parental leave earmarked for fathers (European Union 2019). When it is earmarked, any period taken by the father does not reduce the length or paid benefits of the parental leave of mothers. Paid benefits are lost if the father does not take parental leave, which should encourage them to participate.

Despite the popularity of such reforms, the empirical evidence on the consequences of earmarking parental leave to fathers remains limited. Most of the research has focused on ‘Daddy months’ policies implemented by Scandinavian countries in the 1990s that offered short and well-compensated parental leave to fathers (Rege and Solli 2013). In contrast, recent reforms in the 2010s in France, Portugal, and Italy earmarked much longer periods of paid leave to fathers, up to 12 months in France, for much lower levels of benefits, but allowed the possibility to take parental leave part-time (Boyer 2017; Koslowski et al. 2020). Often, as in the reform studied here, the reforms simultaneously reduced mothers’ maximum length of parental leave in order to provide additional incentives to fathers to substitute for them (Addati, Cassirer, and Gilchrist 2014). Because of these important differences across countries, previous research that found high participation rates of fathers in response to the introduction of the short and well-compensated ‘Daddy month’ in Scandinavian countries might have little relevance for other countries.

To investigate these questions, we examine a 2015 reform in France that earmarked a large share of parental leave to fathers, and which is similar to recent reforms implemented in continental Europe. To increase incentives to have a second child and support large families, the length of parental leave depends on the birth order in France. As a result, the effects of the reforms differed accordingly. For first-time parents, before the reform, no period was

earmarked, and a maximum of six months of leave could be shared between parents. After the reform, six separate months are earmarked for each parent.

For parents of second or higher parity births, referred to as ‘second-time parents’ for brevity in the rest of the text, the reform simultaneously reduced the maximum duration of the leave to increase further the incentives to fathers to take parental leave. In practice, the reform decreased the maximum leave duration per parent by 12 months from 36 to 24 months while earmarking 12 months for each parent.

In contrast to Scandinavian countries, the monthly benefits received for parental leave are low, as they do not depend on past earnings and correspond to a third of the minimum wage when parental leave is taken full-time. These levels of benefits have been unchanged by the reform. However, part-time parental leave is possible and allows remaining employed for up to 80% of standard working hours while receiving paid benefits that are twice as high on an hourly basis.

Despite these low compensation levels, the French program attracted many mothers. Just before the reform, about 50% of second-time mothers took at least a month of parental leave after the birth of a second child and more than 25% of them that took the maximum of 36 months of leave before the reform. In contrast, less than 2% of fathers took at least a month of parental leave before the reform.

Despite the low rate of fathers’ pre-reform participation, the government predicted that many fathers would substitute for mothers after the reform, such that the share of fathers participating in parental leave would be multiplied by ten to reach 25% (Collombet 2016) while increasing mothers' labor force participation. In order to understand the consequence of this reform and, more generally, of similar reforms currently implemented in Europe and elsewhere, we investigate whether these objectives were achieved by examining whether fathers took more leave in response and how each parent's earnings responded to the reform.

Following Lalive and Zweimüller (2009) and Schönberg and Ludsteck (2014), among others, the causal impact of the 2015 reform is identified by comparing parents whose children were born shortly before and shortly after the implementation of the reform. Using data from social security records that contain rich information on household earnings, we compare the take-up of parental leave of parents whose children were born in January 2015, just after the reform, with parents whose children were born in December 2014 and remained in the old system. In our baseline specification, we use a difference-in-differences design that includes as a control group the households that had children born in December 2013 and January 2014, one year before the reform, to account for the influence of calendar effects on outcomes.

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, despite their popularity, there have been few evaluations of recent reforms earmarking parental leave to fathers, particularly when the length of the leave for mothers is simultaneously reduced. Such reforms are interesting as they indicate how much the periods of parental leave taken by mothers will be substituted by fathers, which is crucial for the design of parental leave policies.

Related literature on earmarked parental leave

In 2018, one year before the 2019 directive, one-third of countries in the European Union already earmarked a share of parental leave for fathers (Janta and Stewart 2018). While the principles remained similar, the context in which parental leave has been earmarked varies widely across countries.

Early reforms in Scandinavia and Germany from the 1990s and 2000s introduced short ‘daddy months’ for fathers, over one or at most two months, with high replacement rates of previous earnings varying from 67% in Germany, up to a ceiling, of 80% in Sweden and even 100% in Norway. The daddy months were largely subscribed 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 taken by more than 55% of fathers (Farré and González 2019).

These reforms produced a more equal division of household tasks between parents as fathers taking periods of paid parental leave are more involved in childcare and housework, even after the end of the parental leave (Tamm 2019). However, when it is not fully compensated, parental leave decreases household income, and, as a consequence, the risks of the separation of parents in low-income households increased in Sweden (Avdic and Karimi 2018) and Spain where fertility also decreased (González and Zoabi 2021).

In the US, several states implemented family leave policies similar to parental leave. The State of 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 (Bartel et al. 2018). After the reform, only 2.9 percent of Californian fathers took some family leave. Despite a broadly similar length and level of compensation, this take-up rate appears dramatically lower relative to the one observed in Germany.

In contrast to the short and well-compensated daddy months typical of Scandinavian countries, reforms implemented in the 2010s in the UK, Portugal, France and Italy, earmarked much longer periods of paid leave for each parent. The levels of paid benefits offered to fathers are also much lower. In Italy for example, since 2015, 11 months are earmarked for each parent for 30% of previous pay (Addati, Cassirer, and Gilchrist 2014). Our study is thus interesting to document the response of fathers to the earmarking of parental leave in such a context.

Many countries, such as France, Belgium, the Netherlands, and South Korea among more than 14 other countries, allow parents to take parental leave only part-time. Part-time parental leave allows the parent to remain employed while taking one or two days off per week to spend time with their child (Boyer 2017; Bueno and Grau-Grau 2021). Our study allows us to see the

role of part-time parental leave in the response of fathers to the earmarking of specific months for them, which has not been examined in the literature.

The French Parental Leave Reform

The French parental leave system is rather similar to the one in many countries of continental Europe, except that the length of leave for second-time parents tends to be substantially longer (Addati, Cassirer, and Gilchrist 2014). Paid parental leave can be taken after up to eighteen weeks of maternity leave for the mother, some of which are compulsory, and up to eleven days of well-compensated paternity leave for the father. Paid parental leave does not require being employed, but as detailed in the online Appendix A1, a minimum period of prior employment is required. These eligibility conditions are not restrictive for second-time mothers because previous periods of parental leave are equivalent to work periods. As a result, 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. A crucial advantage for our study is that only parents of children born after the 1st of January 2015 have been affected by the reform, and parents of children born before this date remained in the old system.

First-time parents

As parental leave is taken on a monthly basis, let p_f and p_m denote the months of parental leave taken by the mother and father, respectively. For first-time parents, before the reform, no period was earmarked, and the main restriction was that the *sum* of the months taken by the mother and the father should not be superior to 6 months, that is $p_m + p_f \leq 6$. After the reform, six months of paid leave are earmarked to each parent such that the constraints are given by $p_m \leq 6$ and $p_f \leq 6$. Another minor difference is that after the reform, the mother can take off any

months before the child's first birthday. In contrast, before the reform, parental leave had to be taken consecutively only during the first six months after the birth.

Second-time parents

For second-time parents, before the reform, a total of 36 months combined for both parents could be taken such that $p_m + p_f \leq 36$. While the total remained at 36 months, the reform added two constraints $p_m \leq 24$ and $p_f \leq 24$, thus reducing the maximum per parent from 36 to 24 months. As a result, 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.

An important consequence of the reform is that, after it has been implemented, both parents need to take parental leave to cover their child's 36 first months of life. This threshold is crucial as, after that age, their child can attend free and non-rationed preschool.⁴

Other characteristics of parental leave

The reform is ideal for isolating the consequences of earmarking parental leave, given all other program characteristics remained unchanged. As in the UK or Belgium, the benefits are not proportional to past earnings, and they are similar for mothers and fathers as the program is gender neutral. Both before and after the reform in 2015, parental leave benefits corresponded to about 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. As the full-time net minimum wage was 1150 euros that year, these benefits are low. However, on an hourly basis, they are twice as large when the leave is taken part-time.⁵ These monthly benefits are tax-free, are the same whatever the number of children, and do not affect

⁴ Even if preschool at age three was not compulsory before 2019, about 98% of children attended preschool in the September of their third birthday (Direction de l'évaluation 2018).

⁵ 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.

eligibility for other welfare programs except unemployment benefits, which are suspended during the leave.

Importantly, taking part-time leave does not require reducing labor supply even when the parent was already working part-time before birth. The parent must simply ask her employer to sign a form certifying that she is working part-time or has stopped working.⁶ In addition, an employee that wants to take leave (either full-time or part-time) cannot be fired. Neither can the employer deny the parental leave. After the parental leave, the law guarantees the return to work at a similar position.

Alternative to parental leave

For parents, the formal alternative to parental leave is subsidized childcare through a daycare center or certified childminder. Before their third anniversary, 52% of children in 2015 were enrolled at least part-time in early childhood education and care services (Givord and Marbot 2015). Despite this large share of enrolled children, there is not enough supply of daycare centers or certified childminders to meet the demand from parents. The costs for families remain also non-negligible despite the subsidy, with about 10% of the net household income (OECD 2022). The scarcity of slots in addition to the non-trivial cost explains the large participation rate of mothers to parental leave in France (Villaume and Legendre 2014).

Expected effects of the reform

For first-time parents, the reform affects households for whom the pre-reform constraint would have been binding. As discussed earlier, these households are those in which the parents would have taken all six months of leave possible. Just before the reform, these constraints affected about 12% of households. Among 98% of these households, all six months of parental leave

⁶ 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’*).

were taken by the mothers. This implies that, if anything, fathers from these households in which the constraint would have been binding should take more parental leave.

For second-time parents, the reform affects households where a parent would have taken more than 24 months of parental leave before the reform. Many households were directly affected 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. The reform thus implies a significant shock to childcare arrangements for these households.

In online Appendix A2, we present a simple labor supply model to analyze the effects of earmarking parental leave while reducing the length of the mother's leave. The predictions of the model are ambiguous as fathers' response depends on cost of childcare if both parents are working and the difference between mothers' earnings in the labor market and the parental leave benefits. When the labor earnings of mothers are superior to the parental leave benefits, the household's total income should increase after the reform. Thus, fathers might substitute the mother in parental leave, thus keeping the paid benefits but losing their wage. On the other hand, when the labor earnings of mothers are lower than the benefits, fathers might instead work more in order compensate for the decline in household income.

In contrast, the predicted effects of the reform are straightforward for fathers observed as 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 leave of the mother. As discussed below, up to 7% of fathers are working part-time in the eligible population. 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 that are going to be tested empirically:

H1: For first-time parents, the reform should increase fathers' uptake.

H2: For second-time parents, whether fathers take more leave depends on whether mothers will earn more in the labor market, net of the associated childcare cost, than the paid benefits. When the mother has low labor earnings, fathers might work more in response to the reform to compensate for the loss of benefits and the increase in childcare cost.

H3: Fathers observed as working part-time should take parental leave after it has been earmarked as they do not affect the length of parental leave of the mother anymore

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 also increase the cost for fathers to take parental leave. As a result, there might be important differences in response across fathers that are explained by differences in gender attitudes in addition to differences in potential earnings in the labor market.

Implementation of the reform

The reform is unlikely to have influenced the fertility decision 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.⁷ While the law was first discussed in July 2013, it was voted by the Parliament much later 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 entry into force of the law, on the 30th of December 2014. Thus, parents could not precisely anticipate the consequences of the reform.

⁷ 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.

Salience of the reform

When the law took effect in January 2015, the family benefit administration elaborated a communication plan and sent flyers explaining the reform to parents. 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 was designed to be 'shared' between parents since a specific period was now earmarked to the father. The reform was also widely publicized in the press.

The Data and Sample

The 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 on the data are provided in online Appendix A3.

Social security data

We use administrative data from the French family benefits social security, the *Caisse Nationale des Allocation Familiales* (CNAF). More than 98% of households affected by the reform are covered by our sample: registration is automatic, as pregnancies are reported such that their health costs are covered.⁸

We exploit the monthly administrative files that contain detailed information on family composition and the monthly levels of benefits received, including family allowances and parental leave benefits. As the reform differs for single parents, we restrict our sample to families with two parents, whether they are married, in a civil union, or cohabiting.

To identify whether a household is affected by the reform, we use the year and month of birth of the child, as for confidentiality the exact day is not reported. We select households

⁸ The sample does not include agricultural workers, who accounted for less than 1.7% of births in the year of the reform. While the benefits are similar, agricultural workers have a separate family benefit administration.

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 control for any calendar effects unrelated to the reform by also including households that had a child in December 2013 or January 2014 in our sample.

While the information on social security benefits is reported monthly, earnings and unemployment benefits are extracted from the annual tax returns. Tax returns are reported with a two-year lag because family allowances received in year t depend on the taxable income in the antepenultimate year $t-2$. Because for administrative reasons it is reported with a two-year lag, it has the advantage of starting two years before birth and thus one year before the mother's pregnancy. Therefore, in contrast to family benefits for which we have monthly information until March 2022, annual tax returns are available until the fiscal year 2020, that is, the sixth year after birth for the first cohort affected by the reform.

Administrative Panel Data on Labor Supply

An important question is the relationship between the probability to work part-time and the participation to parental leave. We investigate whether, as predicted by our hypothesis H3, parents working part-time are taking parental leave benefits they are entitled to or whether a large share of parents, in particular fathers, are working part-time and do not take the benefits, thus leaving money on the table. If the share of non-take-up is substantial, this would suggest substantial stigma or informational barriers associated with the parental leave program.

To answer this question, a limitation is that social security data does not report the number of hours or days worked corresponding to the annual earnings of each parent. To measure the share of fathers or mothers working part-time after the reform, we rely on the French administrative panel '*Échantillon démographique permanent (EDP)*'. One advantage of this data is that it contains a 4% sample of the population which is four times as large as the

existing alternative sources such as the Labor Force Survey.⁹ The data combines birth certificates that allow us to identify young parents affected by the reform with administrative information from employers on earnings and the number of hours worked. By combining these two sources of information, we can estimate the share of part-time workers among parents eligible for parental leave after the reform. A limitation is that we only have information up to 2018 on labor force participation, which is only three years after the reform.

Empirical Approach

We follow Lalive and Zweimüller (2009) by comparing parents who had a child just before relative to just after the implementation of the reform on the 1st of January 2015. Such an empirical approach relies on a local randomization hypothesis (Cattaneo, Idrobo, and Titiunik 2020), which is valid herein if the timing of the birth is random within the chosen window around the eligibility cut-off for the reform.

Given that our data only report the month of birth, 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 2015.¹⁰ Despite such restrictions, our sample is large and includes approximately 108,000 households.

A concern is that the discontinuity on the 1st of January also affects the year of entry to public preschool (*école maternelle*). As children can enroll in preschool in September of the year when they turn three, a child born in December 2014 will be admitted in September 2017.

⁹ In an earlier version (Périer and Verdugo 2021), we used the French Labor Force Survey (LFS) to estimate the share of part-time workers among young parents. The sample size of the LFS is four-time 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 the LFS reports the labor force status only during the reference week of the survey. In contrast, the EDP exploits annual information from employers.

¹⁰ We also experimented with a larger window such as a quarter before and after the reform and the results for participation were similar. However, while the differences were economically small, the null hypothesis of equality of average characteristics between groups of households affected or not affected by the reform was rejected. This result is consistent with evidence from the US suggesting that the characteristics of households having a child vary within years across quarters of birth (Buckles and Hungerman 2013).

On the other hand, a child born only several days later but in January 2015 need to wait until September 2018 to be admitted to preschool.

To account for calendar effects unrelated to the reform, we follow Lalive and Zweimüller (2009) and Schönberg and Ludsteck (2014) by using a difference-in-differences approach, with the group of households that had a birth one year before the reform during the same months (i.e., December or January) as a control group. We consider the standard difference-in-differences model estimated with OLS as follows:

$$Y_i = \beta_0 + \beta_1 G_i + \beta_2 T_i + \beta_3 (G_i \times T_i) + u_{it} \quad (1)$$

where Y_i is an outcome of household i , such as total earnings or participation in parental leave over the eligibility period, G_i is a dummy equal to one if the birth occurred in January relative to December, and T_i equals one if the birth occurred in the year of the reform, either in December 2014 or January 2015, and zero if it occurred the year before (December 2013 and January 2014). Our key parameter of interest β_3 is associated with the interaction term between the year of the reform and the group affected by the reform ($G_i \times T_i$) and captures the effects of the reform on Y_i .¹¹

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-difference model:

$$Y_{it} = \beta_0 + \sum_{j \neq -1} \beta_1^j G_i I[j = t] + \sum_{j \neq -1} \beta_2^j T_i I[j = t] + \sum_{j \neq -1} \beta_3^j (G_i \times T_i) I[j = t] + u_{it} \quad (2)$$

where Y_{it} denotes a measure of annual earnings in year t or participation rate in month t . The model includes a full set of event time dummies for each group (first term on the right-hand side), of event dummies for each year (second term), and event time dummies for birth in

¹¹ 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.

January after the reform (third term) which captures the effects of the reform. For annual earnings, the event time dummy is omitted for $t = -1$, and thus the coefficients β_3^j are scaled such that they measure the impact of the reform relative to earnings one year before.

Validity of the Empirical Approach

Our ability to estimate a causal effect of the reform depends on whether households having a child in December are on average identical to those having a child in January and on whether the parallel-trend assumption is valid. In accordance with earlier work using that method, we report evidence below that such hypothesis is not rejected by the data.

Smoothness of the daily birth distribution Our empirical strategy is not valid if households manipulated the timing of the birth in response to the reform. Using daily birth aggregate data, we investigate in the 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 that there is no discontinuity. However, our ability to detect manipulations of the day of birth is limited as the daily birth data does not distinguish first from higher-order births and, as discussed in the Appendix, the incentive for manipulation goes in the opposite direction for first- and second-time parents.

Observable differences between births in December and January Whether the local randomization hypothesis is valid can be assessed by comparing the predetermined characteristics of households where a child is born in December 2014, just before the reform, and in January 2015, just after. If the assignment of their child to one of these two months of birth is as good as random, we should not find any systematic difference between households that had a child born just before relative to just after the reform. We compare these characteristics separately for first- and second-time parents in panels A and B of Table 2,

respectively.¹² Overall, we find no significant differences in the average age of the mother or the father, the average earnings, the share of mothers with zero earnings before birth, or the number of children in the household.

Another possibility might be that the composition of households between births in December and January would change *differentially* over the consecutive years that are used in the difference-in-differences estimates. To assess this hypothesis, we report in appendix Table A1 estimates of the difference-in-differences model using as a dependent variable the same set of background characteristics as in Table 2 and including in the sample birth in December 2013 and January 2014, one year before the reform, in addition to birth in December 2014 and January 2015. For all outcomes, we find no statistically significant differences and thus little evidence that the composition of households did change differentially in the reform year.

Another issue is that our analysis focuses on couples with children that are the ones affected by the reform. This restriction might create a selection bias over time if the reform influences the probability of separation of the couple. Also, as having another child renews the eligibility for paid parental leave, our estimates might be affected by an endogenous response of fertility to the reform. However, as reported in Appendix Table A2, we find no effects of the reform on the probability of separation of the couple or the number of children at a 3- and 5-year horizon for both first- and second-time parents.

A possibility is that these non-significant results reflect the combination of offsetting effects of the reform if it simultaneously increases the risk of separation for some households while it reduces it for others. While we cannot rule out this possibility, such effects are likely

¹² We use earnings measured two years before 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.

to be small as the estimates are rather precise. In practice, the 95 percent confidence intervals rule out an increase in the probability of separation superior to 1 percent.

Plausibility of the parallel-trend assumption The validity of our differences-in-differences design requires the parallel-trend assumption to be valid. This assumption implies that absent the reform, the differences in outcomes between households that had a child in December relative to January would have remained stable over time. To assess the plausibility of this hypothesis Another possibility might be that the composition is, Figure 1 compares the participation rate of parents on parental leave across different cohorts, up to 2 years before and after the reform. Overall, the parallel trend hypothesis is not rejected by the data: participation rates for birth in January and December are statistically indistinguishable from each other in years without reform.

Effects of the reform on parental leave take-up

First-time parents

Panel A in Table 3 reports difference-in-differences estimates of the effects of the reform on the take-up of parental leave of first-time parents. For these parents, the reform earmarked 6 months of non-transferable leave to each parent, while prior to the reform they had only 6 months to share together. In column 1, we use as a dependent variable the probability to take at least one month of parental leave over the eligibility period while in columns 2 and 3 we consider separately full- and part-time parental leave, respectively.

Clearly, more fathers participate after the reform, consistent with our hypothesis H1, as the point estimate is measured precisely and is statistically significant. However, the effects are small as the reform increased by only 0.4 p.p. the probability of taking at least one month of leave for fathers. While such an effect might appear small in absolute value, it is, in relative terms, large. Compared to the low baseline rate of 1% before the reform, the reform increased

by 40% the participation rate of fathers. Despite this increase, these participation rates are low. Only 1% of fathers took part-time parental leave after the reform in this group.

To interpret the effects of the reform on the take-up of part-time parental leave, Table 4 compares the share of parents taking at least one month of parental leave, estimated from social security data, with the share who worked part-time during the eligibility period, estimated from employers' declarations. As discussed earlier, if there are no participation costs or stigma, parental leave should be taken by any parent observed working part-time for at least one month during the eligibility period.

We find that the take-up rates of parental leave are dramatically lower than the share of part-time workers among parents: even if 6% of first-time fathers are working part-time, only 1% of fathers took a month of parental leave. Taken literally, these figures imply that 85% of fathers do not take the minimum 150 euros monthly benefits of paid parental leave they are eligible for. For mothers, the non-takeup rates are lower, close to 50%, consistent with earlier studies for France (Reinstadler 2000). While these results should be interpreted with caution as they rely on aggregate statistics from two different data sources, they suggest a large non-take-up of parental leave from both parents that is substantially larger for fathers.

To capture changes in the intensive margin in parental leave participation, Column 4 of the Table uses the total number of months of parental leave taken by the household (including zero when no month has been taken) as a dependent variable. The results suggest that the reform increased by 50% the average number of months taken by fathers. Columns 5 and 6 show that most of this increase is driven by part-time leave, the availability of which thus appears crucial for fathers.

Figure 2 provides visual evidence on the *monthly* take-up rates of full- and part-time parental leave of mothers and fathers. Both the unadjusted series and the event study coefficients estimated using the difference-in-differences model in the last row indicate that the

reform had a significant effect on fathers only after the 6th month after birth. Overall, the small effects of the reform on fathers are driven by households in which fathers took parental leave after the mother took six months consecutively after birth.

For mothers, the reform increased the probability of taking parental leave in the 9th and 10th months after birth. This result reflects that, after the reform, mothers do not have to take months of parental leave consecutively after maternity leave.¹³

Second-time parents

For second-time parents, the reform reduced the maximum length of leave per parent by 12 months, from 36 to 24 months after birth. As expected, Figure 3 shows that after the 24 months threshold, the participation of mothers decreased abruptly.¹⁴ This decline is progressive, reflecting that some mothers do not take the maximum of 24 months in a row after birth, even if a large majority do.

Event-study estimates in Panel B of Figure 3 indicate that the reform is associated with a decline in participation of parental leave for mothers superior to 25 p.p. after the 25th month. Fathers responded to such a large decline by a limited 0.8 p.p. increase in the probability of taking part-time parental leave after the 25th month. It is clear from the figure that their response is entirely explained by an increase in part-time leave.

As for first-time parents, the low participation before the reform implies that, 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. Nevertheless, despite this relatively large increase, such rates remain low relative to the fact

¹³ Maternal leave is extended in the case of premature birth, which explains why small rates of parental leave the 9th month before the reform can be observed.

¹⁴ 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 3rd to the 6th month, which explains why we start the figure at the fifth month.

that 9.6% of second-time fathers work part-time in the population reported in Table 4. Taken literally, this gap suggests that 81% of the eligible fathers working part-time are not taking the paid leave benefits to which they are entitled. For mothers, the non-take-up rates are dramatically lower as they are close to 33%.

Effects of the reform on labor earnings and household income

Next, we examine how the reform affected the income of the household and its components, particularly the earnings of mothers and fathers. As the income data is obtained from fiscal data, its frequency is annual. By convention, the year zero is defined as the first year of the child, from birth to its first anniversary. As the income data is reported for a calendar year and thus cannot be adjusted to the precise month of birth, we define the 2015 calendar year as event year zero for both births in December 2014 and January 2015.

We first 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, we find no statistically significant effect of the reform on any component of household income. Even if the estimates are imprecise, these results suggest that the modest increase in fathers' participation in parental leave did not help mothers to participate and earn more in the labor market.

For second-time parents, the reform had, as expected, more significant effects on average household income as it restricted by 12 months for about 30% of mothers the receipt of parental leave paid benefits. Panel B indicates that this restriction is associated, on average, with a 1,200 euros decline in paid benefits in the population over the three-year eligibility period. In response, the average labor earnings of mothers increased by about the same amount. However, for many mothers, unemployment benefits also compensated the decline in paid leave.

For fathers, despite the 1 p.p. increase in participation in parental leave, we find no negative effects on earnings. On the contrary, the coefficient is positive and large, as the point estimate is about half of the estimated effect for mothers. However, even if a strong negative effect of the reform on fathers' earnings can be ruled out, the estimate is very imprecise, and the coefficient is statistically insignificant.

As expected, given parental leave is an alternative to formal childcare, Column 5 shows that the reduction in paid parental leave is associated with an increase in the receipt of benefits associated with formal childcare thus indicating that formal childcare compensated for part of the decline in parental leave availability.

Figure 4 reports the evolution of annual labor earnings for second-time parents, normalized relative to the year before birth. The positive effects of the reform on the earnings of mothers are concentrated in the third year after birth (event year 2) and to a lower extent in the first year after birth (event year 0). For fathers, in contrast, there is little evidence of any significant effect of the reform on earnings in any year after birth.

Longer-run consequences

To estimate whether the effects of the reform persist over time, Figure 4 also reports the earnings of these households up to three years after the eligibility period (event years 3 to 5). 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 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) that also find no persistent effect of parental leave on earnings after the eligibility period. Overall, this suggests that, for mothers, the earning costs of taking parental leave are temporary and concentrated during the parental leave period.

Heterogeneity by pre-birth earnings of mothers

That no statistically significant effects of the reform on the average labor earnings of second-time fathers are observed could be explained by the fact that the response of fathers depends on the potential earnings of the mother in the labor market. As suggested by our hypothesis H2, in households in which mothers have low potential earnings, fathers might not respond by taking parental leave but by increasing their labor supply to compensate for the losses of benefits after the reform and the additional cost of childcare in case the mother is also working.

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

Panel A and B of Table 6 first document differences in the effects of the reform across these groups on the number of months taken and total benefits of parental leave. Panel A indicates that the reform had relatively higher negative effects on the average number of months of parental leave taken for mothers from the second and third quartiles of the distribution. However, Panel B indicates that the decline in the amount of paid leave benefits is much more similar across groups except for the fourth decile.

Even if there are large variations in the impact of the reforms on mothers across these groups, Panel C shows there is no statistically significant effect of the reforms on the earnings of fathers in any of these groups. Across all groups, the estimated coefficients are all positive and, consistent with our hypothesis H2, the coefficient is relatively larger in households in which the mother had no earnings before birth. However, somewhat surprisingly, the point estimate is even larger 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. However, across all groups, the estimates are very imprecise.

For mothers, on the other hand, Panel D shows that the increase in their labor earnings is larger and statistically significant only in the second and third quartile of the pre-birth earning distribution. For other quartiles, the point estimates are ten times lower and even negative for those in the first quartile. Consistent with this result, Panel E 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 the use of formal childcare is also concentrated on mothers in the second and third quartile for whom the labor earnings tend to increase substantially.

Panel H documents the effects of the reform on total income across households. Even though we observe a substantial decline in paid benefits in some groups, we do not find any significant negative average effect of the reform on household income, even in groups where mothers have the lowest pre-birth earnings. Still, the sign of the coefficient varies across groups, and the estimates are very imprecise.

What explains the low take-up rates of fathers?

To gain some insights into what explains the low take-up rates of fathers, we first examine whether an initial imperfect knowledge of the reform might explain the low take-up of fathers, in particular for fathers observed as working part-time. Next, we examine differences in the take-up of fathers across subgroups of parents for whom the influence of gender norms might vary.

Did the take-up of fathers increase later?

A possible explanation for the low take-up rates of fathers is that they lacked initially information about the reform. Despite the official communication campaign, many fathers

might not have been immediately aware of the reform after its implementation.¹⁵ If the slow diffusion of information explains the lack of participation of fathers, then the share of fathers taking paid leave should increase over time as knowledge of the reform spreads in the population. To assess the plausibility of this hypothesis, we report in Figure 5 the share of fathers taking parental leave for births occurring one and two years after the reform in January 2016 and 2017.

The evidence does not support the hypothesis that the low take-up of fathers was temporary. In practice, differences in take-up rates in more recent cohorts are small, 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 share of eligible fathers working part-time remained dramatically larger than the share taking paid leave. By combining information on part-time work from administrative records with participation rates from social security data, we estimate in Table 4 that non-take-up rates tend to decline for first-time fathers, consistent with a diffusion of information about the reform over time. While these non-take-up rates must be interpreted with caution, they remain greater than 80%.

Differences in the response across groups of fathers

As we find no significant correlations between the past earnings of the mothers and the response of fathers, a possibility is that other factors than earnings such as gender norms might be more important in influencing the participation of fathers. To assess this possibility, we investigate whether we find differences in response across fathers related with differences in their working status, location, and pre-birth earnings.

¹⁵ See Chetty et al. (2013) for evidence that the knowledge of welfare reforms in the population spread over time.

Effects of the reform on independent workers To receive paid parental leave, employees need to ask their employer to fill out a one-page form for the 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 must declare to the social security office 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 fathers who are self-employed are twice to three times as likely to take leave after the reform, as the estimated coefficient is twice to three times as large relative to the baseline rate in the population. However, these results might be interpreted with caution as they might also reflect the fact that it might be easier for self-employed workers to falsely declare a reduction in hours of work and receive undue benefits (Chetty, Friedman, and Saez 2013).

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 relative labor market outcome for women (Janssen, Tuor Sartore, and Backes-Gellner 2016). If local attitudes influence the take-up rate of parental leave, 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, Columns 3 and 4 in Table 7 report separate estimates of the effects of the reform on fathers depending on the share of fathers taking leave in the county of residency (*département*) one year before the

reform. Consistent with the hypothesis that a higher local share of fathers taking leave is correlated with a more favorable local attitude towards fathers taking leave, the response of fathers to the reform varies widely with the pre-reform local participation rates of fathers. In practice, we find that all of the effects 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 relative to the baseline estimate. In contrast, in counties with below median leave, the reform had no effect on the take-up rate of fathers.

Differences in fathers' earnings The simple model of labor supply discussed before 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 the earnings of fathers 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 to take parental leave for them (Papuchon 2017).

To test for differences in response to the reform relative to the pre-birth earnings of fathers, we report in columns 5 to 8 in Table 7 separate estimations depending on the quartile of the annual labor earnings of fathers two years before 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 pre-birth earnings are much less likely to take paid leave in response to the reform. For first-time parents, in particular, the response from fathers in the third quartile is four times as large relative to those in the first quartile. For second-time parents, we only find a significant effect of the reform from fathers in the fourth quartile but no effect on the other quartiles. Overall, the fact that fathers with higher levels of earnings are more likely to take leave is consistent with an important role of gender values relative to earnings in the decision.

However, we do not find any effects on parents in the fourth quartile for first-time parents, in contrast to second-time parents.

Discussion

As fathers' participation in parental leave remains dramatically low, many countries are reforming their parental leave programs to increase their participation. Inspired by the successful 'daddy months' policies that offered one month of parental leave to fathers in Scandinavian countries, recent reforms also earmarked fathers' specific months of parental leave to increase their participation while keeping other characteristics of the program unchanged. Several countries have also simultaneously reduced the length of mothers' leave to provide fathers with additional incentives to substitute for mothers. As, in most countries, the levels of benefits offered are much lower than in Scandinavian countries, the impact of such reforms is uncertain in this context. Despite the importance of these recent reforms, there has been relatively little evidence of their consequences on fathers' participation.

To investigate these issues, this paper studies a 2015 reform of French parental leave that earmarked 6 to 12 non-transferable 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 the main effect of the French reform was to reduce the length of parental leave for mothers. In contrast, the effects on fathers remained very modest. 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. Overall, the substitutability of parental leave between spouses appears to be low in our context. In addition, while statistically insignificant and very imprecise, we find a positive effect of the reform on the earnings of fathers, consistent with the theoretical prediction that reducing the parental leave of the mother 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 respective importance of the low level of compensation from the role of gender norms in explaining the low participation of fathers. Recent survey evidence suggests that the most important barriers to participation for fathers are the low level of benefits combined with career concerns (Sponton 2022). That the level of benefits plays a substantial role in the father's decision is consistent with the large participation rate of fathers in the short and well-compensated French *paternity* leave program, which offers only eleven days of leave but is compensated at 80% of the wage. In contrast to the parental leave program studied here which attracts less than 3% of fathers, the paternity leave program attracts more than 70% of them.

Other empirical evidence we uncover is also consistent with a 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 that are part-time workers do not participate in parental leave even though taking paid part-time leave would have increased their monthly income by about 200€ without requiring them to change their labor supply. Their share only declines slightly over time thus suggesting that the lack of information about the program cannot be the sole explanation for their non-participation. Also, independent workers who did not suffer from 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 were also more likely to share more leave time, consistent with the evidence that they are less influenced by traditional gender norms.

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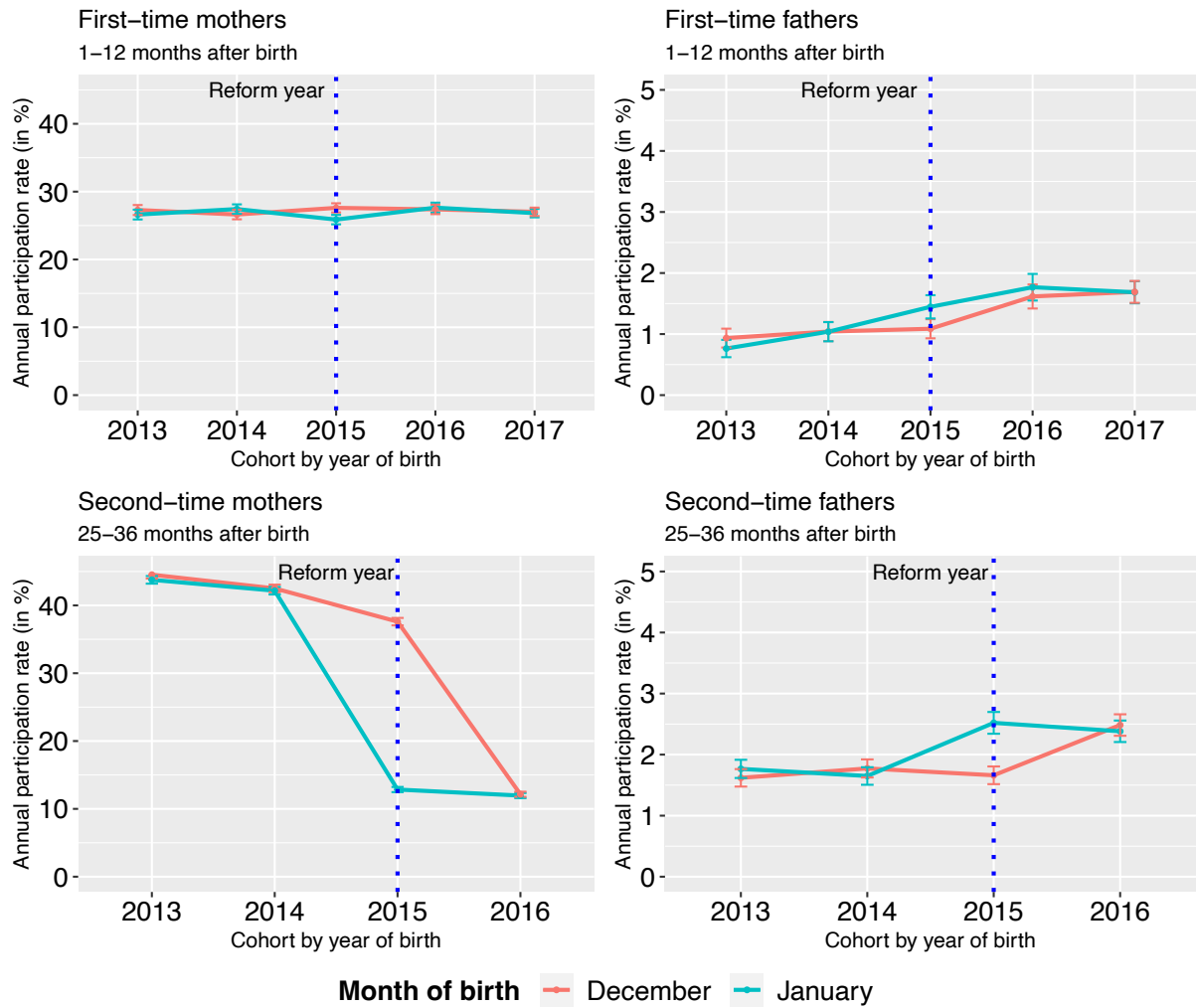
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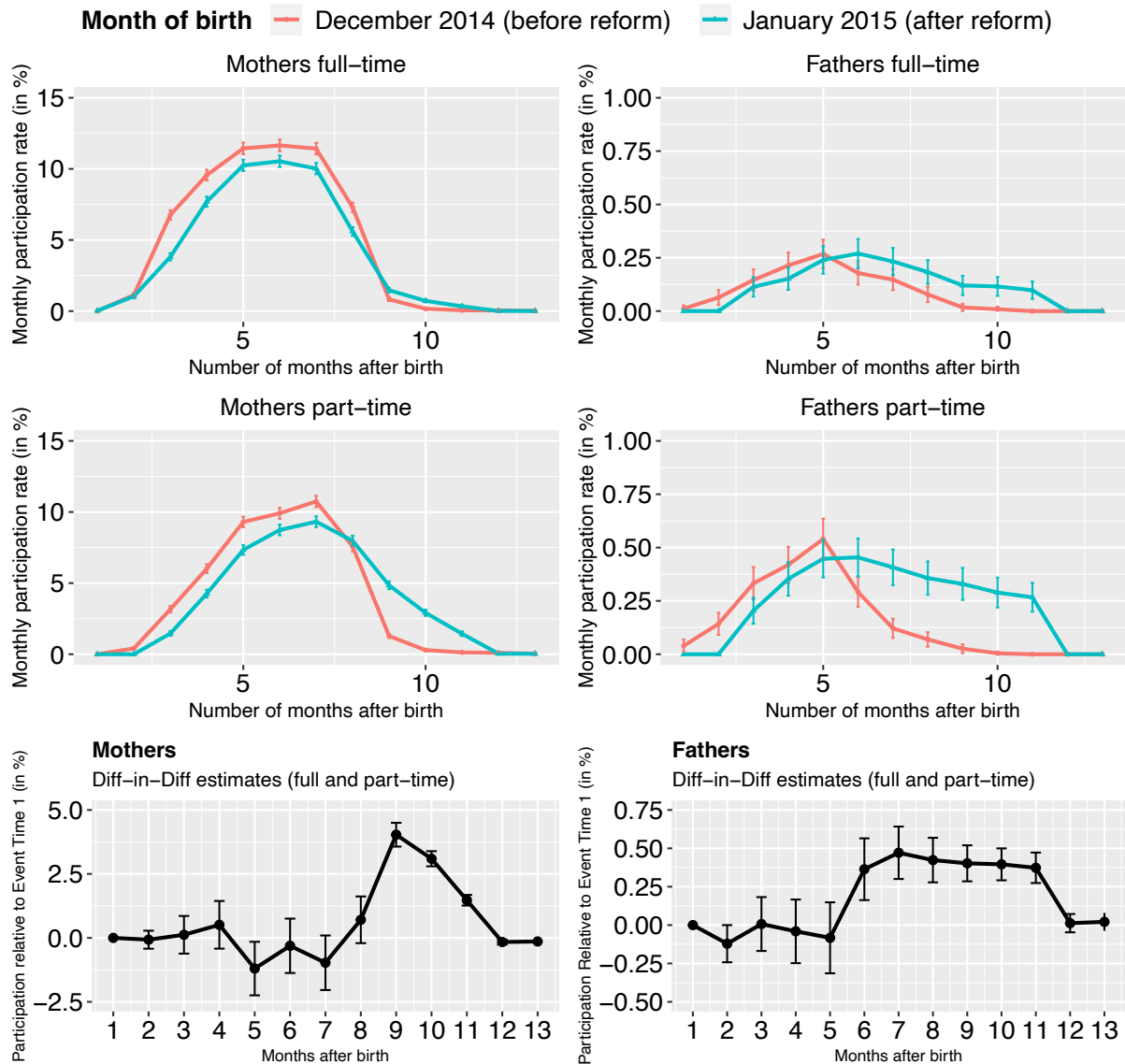
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Figure 1: Cross-cohort comparisons for second-time parents

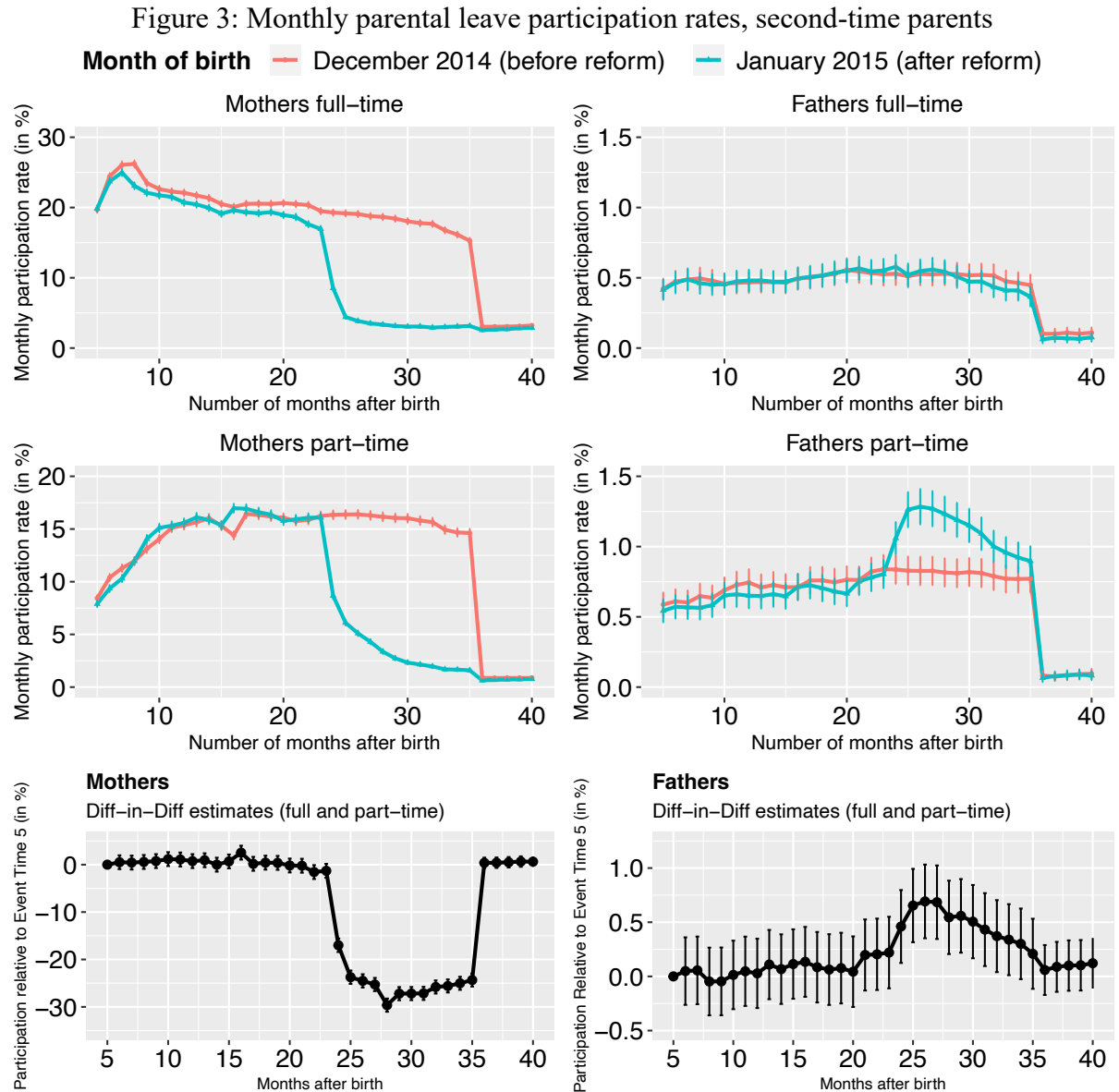


Source: Monthly families benefit files (*Allstat* and *Basestat* from CNAF). *Note:* The figure compares the annual participation rates across cohorts of parents of children born in January of the indicated year relative to those born one month before in December of the previous year. The annual participation 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

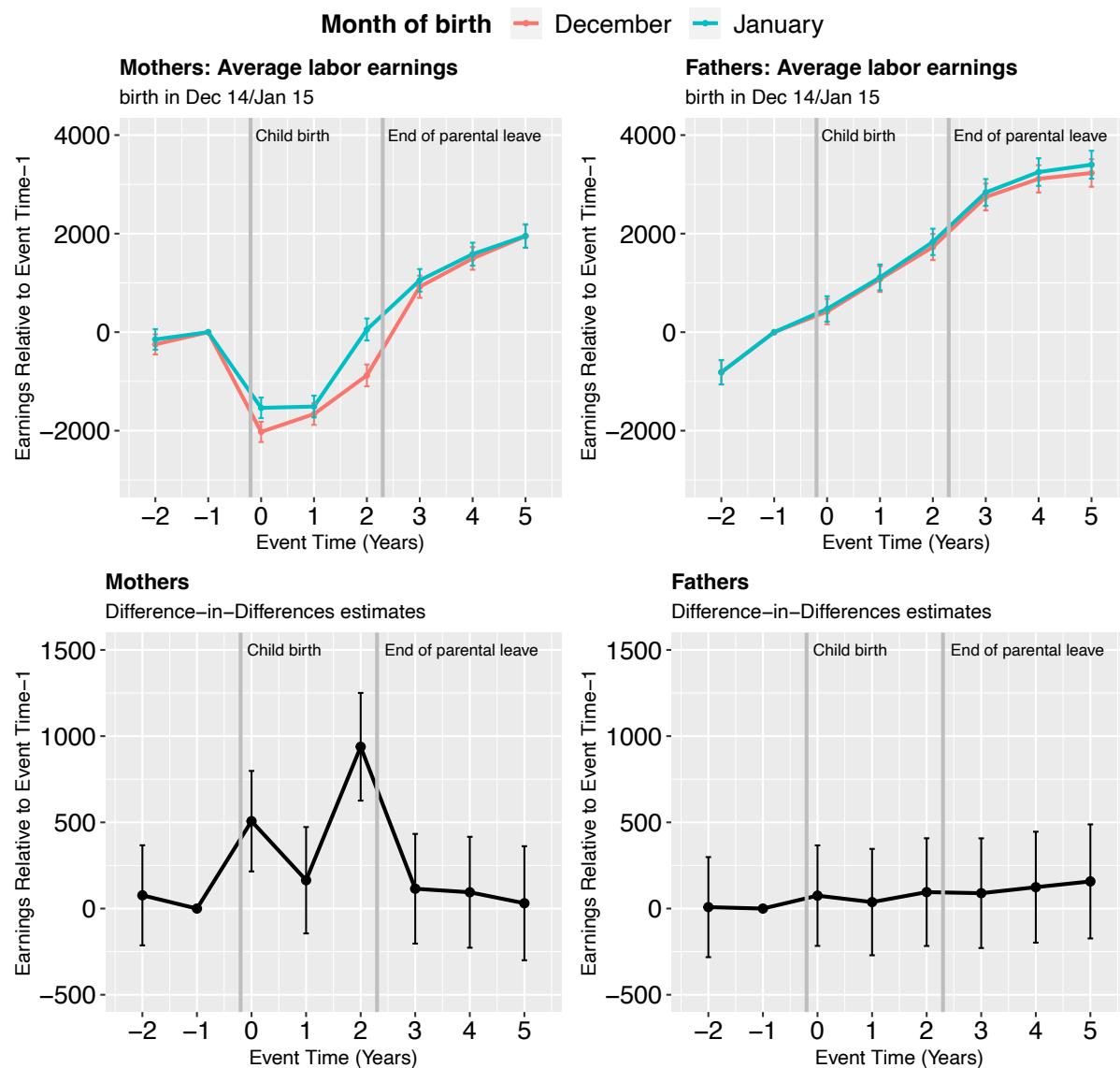


Source: Monthly social security files (*Allstat* and *Basestat* from CNAF). *Note:* The graphs in the first two rows represent the monthly rates of participation in parental leave of first-time fathers and mothers of a child born in December 2014, in red, and in January 2015, in blue. The graphs show 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.



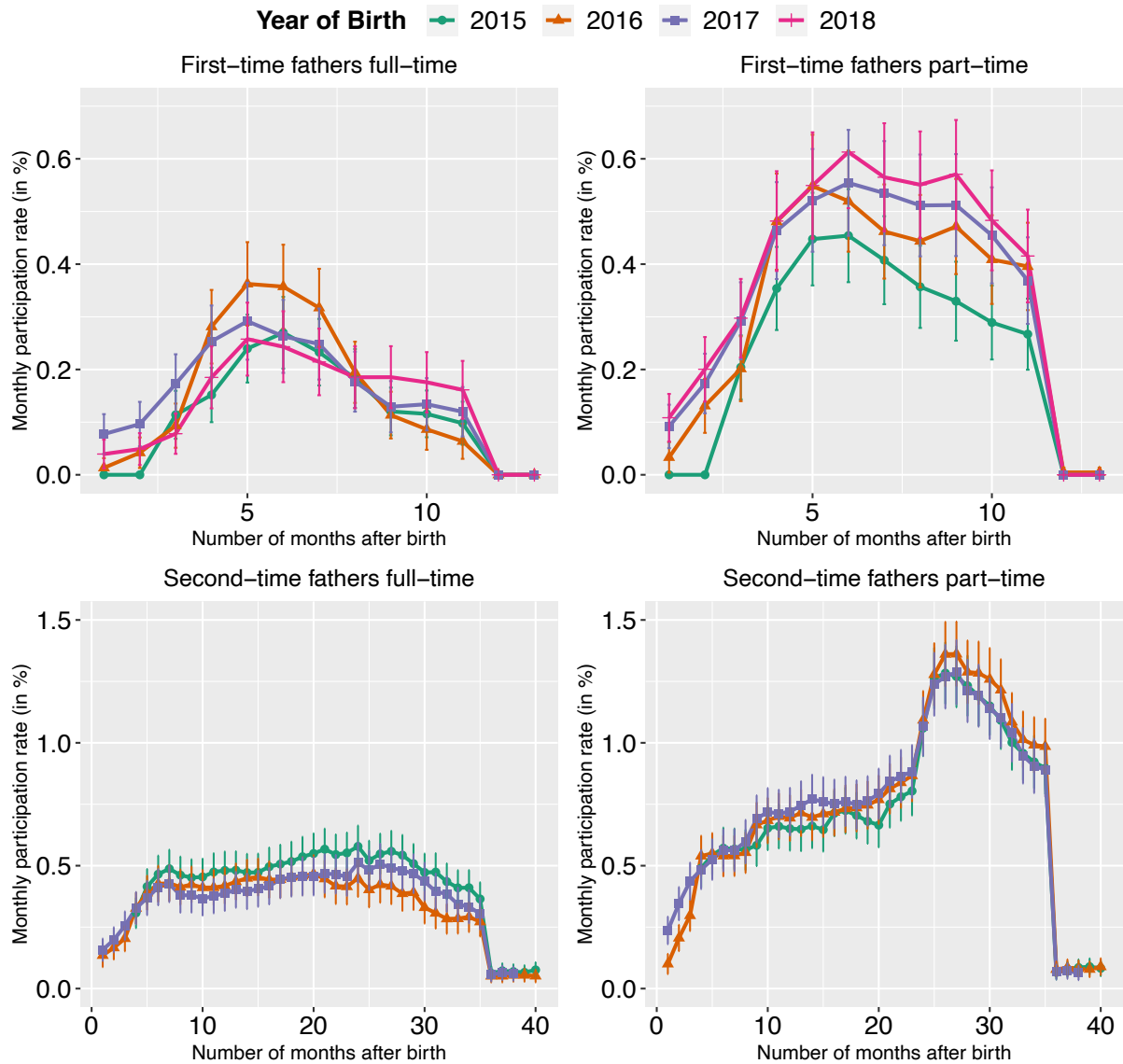
Source: Monthly social security files (*Allstat* and *Basestat* from CNAF). *Note:* The graphs in the first two rows represent the monthly rates of participation in parental leave of second-time fathers and mothers of a child born in December 2014, in red, and in January 2015, in blue. The graphs show 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 families benefits 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 families benefit 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	Before the reform: <i>Births before 1st January 2015</i>	After the reform: <i>Births after 1st 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: Balancing Tests: First- and Second-time parents in the reform year

Birth in	<i>Before reform</i> December 2014	<i>After reform</i> January 2015	Difference	T-stat	p-value	N
<i>A. First-time parents</i>						
Age mother	28.9	28.9	0.0	-0.17	0.86	46 028
Age father	31.7	31.7	0.0	0.00	1.00	46 028
Earnings of father in 2013	19 839	20 104	-265	-1.49	0.14	46 028
Earnings of mother in 2013	15 657	15 602	55	0.29	0.77	46 028
Share mothers with zero earnings in 2013	16.5%	15.8%	0.6%	1.81	0.07	46 028
<i>B. Second-time Parents</i>						
Age mother	32.1	32.1	0.0	-0.53	0.60	62 749
Age father	35.3	35.4	-0.1	-1.21	0.23	62 749
Number of children	2.6	2.6	0.0	-1.49	0.14	62 749
Number children aged 3 and 5	0.6	0.6	0.0	-0.77	0.44	62 749
Earnings of father in 2013	21 527	21 773	-246	-1.46	0.15	62 749
Earnings of mother in 2013	13 193	13 064	129	0.80	0.42	62 749
Share mothers with zero earnings in 2013	26.6%	26.7%	-0.1%	-0.14	0.89	62 749

Source: Monthly families benefits 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.

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
Method	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff

Source: Monthly families benefits 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 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 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 families benefit 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
	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
	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
Estimation method	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff

Source: Monthly families benefits 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
Estimation method	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff

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

Source: Monthly families benefits 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 the French parental leave program

Duration of Maternity leave: For the first and second child, paid maternity leave is mandatory and the leave starts from six weeks before birth and finishes at ten weeks after birth, at most. Starting with the third child, maternity leave starts eight weeks before birth and finishes at eighteen weeks after birth, at most. It is mandatory to take at least six weeks after birth. The length of maternity leave can also be extended for medical reasons. For both mothers and fathers, the benefits correspond to 100% of the previous earning, up to a certain ceiling.

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 also 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.

A2. Theoretical Appendix

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 administrative 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 and the respect of the statistical secret, these files have been accessed and the analysis 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

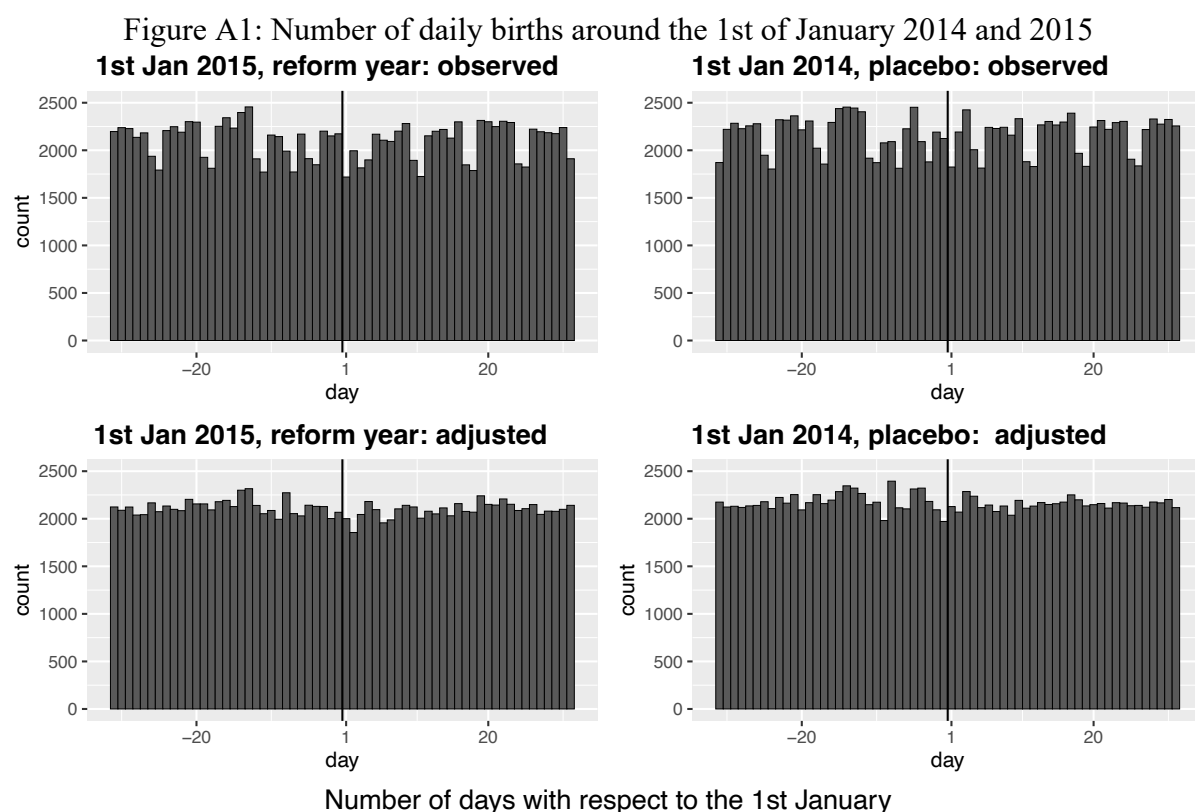
To detect manipulations related to the day of birth by households, we test for the smoothness of the density of daily births around the threshold of the reform. Theoretically, the direction of such manipulations might differ between first- and second-time parents. First-time parents should prefer to 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).

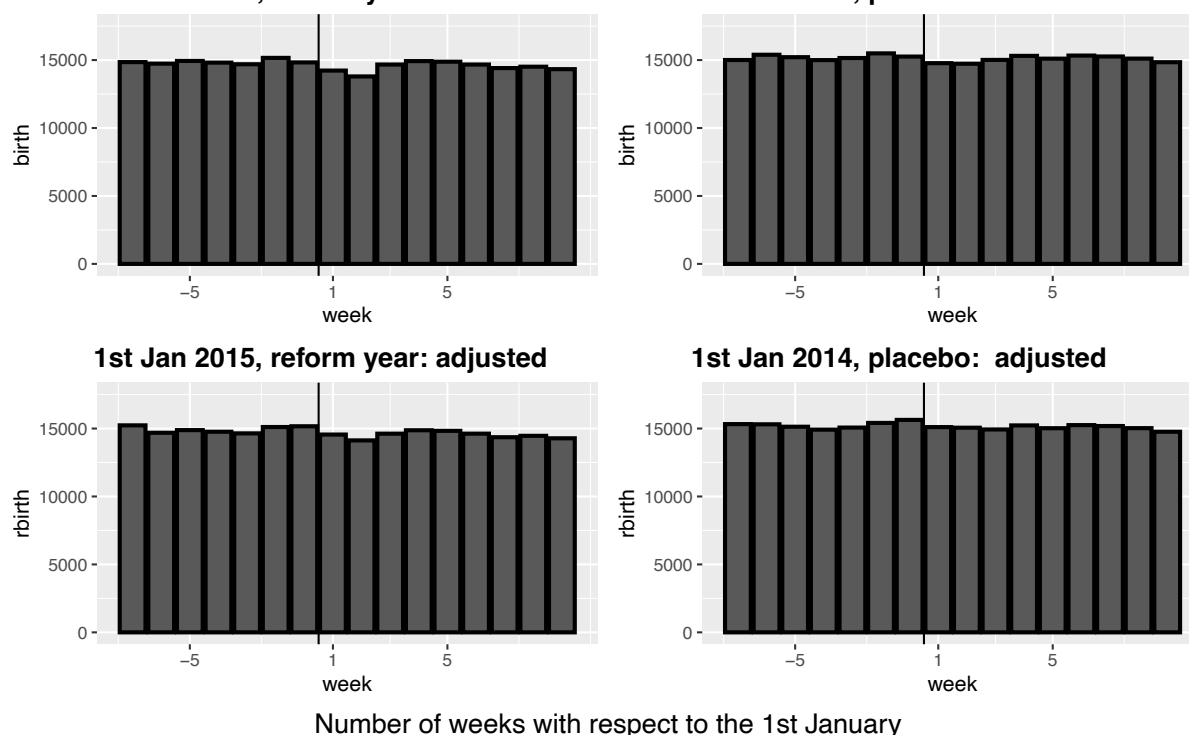
In Figure A2, we consider an alternative approach to deal with the fact that the number of births is lower during the weekend. We aggregate instead birth by week to detect whether the reform year is characterized by an exceptional number of births the week before or the week after the reform. Once again,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

Online Appendix Figures



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: Differences-in-differences estimates on predetermined characteristics of households

<i>Dependent variable</i>	First-time parents	Second-time parents
Age mother	-0.015 (0.064)	0.023 (0.052)
Age father	-0.051 (0.079)	0.069 (0.067)
Earnings of father two years before birth	218.8 (243.6)	11.9 (231.9)
Earnings of mother two years before birth	825.3 (553.1)	350.1 (193.4)
Number of children before birth	0.002 (0.003)	-0.0003 (0.010)
Number children aged 3 to 5 before birth	0.003 (0.003)	0.006 (0.006)
Share of mothers with zero earnings two years before birth	-0.004 (0.005)	-0.004 (0.005)
N	94,566	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 effect of the reform on the predetermined characteristics of households reported in each row. (*), (**), and (***) denote statistical significance at, respectively, 10%, 5%, and 1% level.

Table A2: 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
Method	Diff. in Diff	Diff. in Diff	Diff. in Diff	Diff. in Diff

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. We estimate the effects of the reform on these outcomes on the third year after birth in column 1 and 3 and the fifth year after birth in column 2 and 4. (*), (**), and (***) denote statistical significance at, respectively, 10%, 5%, and 1% level.

References to the online Appendix

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