Parental Leave, Household Specialization and Children's Well-Being*

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Abstract

Many countries offer new parents long periods of paid leave. Proponents argue that parental leave programs can reduce gender gaps in the labor market and promote children's well-being. In this paper, I show that lengthy leaves can instead work against these intended goals. Using a regression discontinuity design, I find that a 3-year expansion of paid leave in France increases household specialization by inducing mothers to exit the labor force and fathers to raise their work hours. The leave further harms children's verbal development.

Keywords: parental leave, household specialization, child development
JEL codes: J12, J13, J18, J22

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1 Introduction

Most countries currently provide new parents with some form of paid leave. Parental leave programs aim to decrease the gender gap in the labor market, promote couple stability and fertility, and support children's development. These claims formed the basis of expansions in the duration of benefits over the past years. Indeed, numerous countries now provide at least one year of paid leave.[1] By 2008, many governments, such as Austria, Norway and Sweden, also offered benefits for periods varying between 1.5 to 2 years, with others such as Finland, France, Germany and Spain, even extending the duration of leave to more than 3 years (Ruhm, 2011).

As further discussed below, an extensive body of literature documents that leaves that are shorter than one year typically have either positive or insignificant impacts on a range of family outcomes. However, critics argue that in contrast to their intended goals, longer periods of benefits can have undesirable effects. This claim is supported by mounting evidence that prolonged time off from work hurts women's careers by decreasing their labor supply and earnings (Rossin-Slater, 2018). Nonetheless, it is still not well understood how extended periods of leave affect other aspects of household behavior and child development. Answering this question is of critical importance for countries that are currently expanding the duration of benefits. It is also informative for governments that already provide lengthy periods of benefits and that are considering decreasing the duration of leave. For example, in 2008, the Czech Republic reduced the length of leave from 4 to 2 years.

The scarcity of evidence on this topic is mainly attributed to difficulties in identifying causal effects. Specifically, a major challenge is overcoming selection bias arising from the fact that taking long periods of leave is likely correlated with unobservable factors such as socioeconomic background, that may also affect outcomes of interest. In this paper, I exploit a unique extension of benefits in France—of approximately 3 years—to examine how *lengthy* periods of paid leave impact parents' labor market behavior and children's development.

My analysis focuses on a French parental leave program, which offered either one or both parents a flat-rate monthly cash allowance to take up to 3 years of time off from work after the birth of their child. During this time, a parent had to either work part-time or be out of the labor force, and the latter option provided a higher amount of benefits. To identify causal effects, I leverage a change in this program's eligibility conditions. Initially, only parents of three children and more qualified for the leave. On July 25, 1994, benefits were extended to parents whose second child was born on or after July 1, 1994. Second-borns' parents were also eligible for up to 3 years of job-protected unpaid leave both before and after the reform. Hence, the reform effectively increased parents' access to cash benefits for up to 3 years without changing the length of job-protection. Since the reform was

[1]Blau and Kahn (2013) report that the average length of parental leave was 57.3 weeks in 2010 for Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain and the United Kingdom.

announced after the cutoff date of July 1, 1994, parents had little opportunity to manipulate the date of birth of their second child in order to become eligible for the leave. I therefore overcome selection into leave-taking by using a regression discontinuity design that compares households on either side of the date of birth cutoff. The main assumption in this design is that households that are barely eligible due to the second child's date of birth, are similar to those that are barely ineligible.

I first document an increase in intra-household specialization in the years couples are eligible to receive the leave. Barely eligible women are around 23 percentage points more likely to be out of the labor force compared to those who are barely ineligible, suggesting that mothers are taking the maximum amount of benefits. Although the program is gender-neutral, men do not take up benefits, as they do not alter their labor force participation or part-time work decisions. The reform however induces fathers to work for an additional 2.8 hours per week. Since beneficiaries receive a fixed amount of cash benefits—and are thus unlikely to get full earnings replacement—this finding could imply that fathers are compensating for a loss of household income due to mothers taking the leave. The reform's effects on parents' do not persist after eligibility for leave benefits expires (i.e., in the fifth through seventh years after childbirth).

Increased household specialization has important consequences. Recent studies show that the divergence in men and women's labor supply and earnings after parenthood persists in the long run (Bertrand, Goldin and Katz, 2010; Angelov, Johansson and Lindahl, 2016), and that it explains most of the remaining gender gap in the labor market (Kleven, Landais and Søgaard, 2019). My results thus suggest that lengthy leaves can exacerbate gender differences in the labor market. While I can only test for leave-induced specialization for up to seven years after childbirth, the French reform was shown to have negative effects on women's wages for up to 10 years after childbirth (Lequien, 2012). Another potential consequence of specialization is a reinforcement of traditional social norms regarding couples' division of labor.

I also find that offering a long period of leave is detrimental to children's development. Results indicate that compared to children born just before the cutoff, those born just after are between 3.1 and 5.6 percentage points more likely to have below-normal scores on various tests that assess their verbal skills at ages 5 to 6. Given that mothers likely become the primary caregivers for 3 years as a result of the reform, a potential explanation for these adverse effects is that maternal care is replacing higher quality childcare arrangements. Spending more time in their mothers' care could also decrease the amount of time that children spend with other adults and children. The adverse effects can thus be driven by reduced social interactions, since interacting with other individuals is typically beneficial for children's development (Dustmann and Schönberg, 2012). As discussed in section 6.3, other channels that could explain the main results are reduced time spent with fathers, a potential loss of household income and the fact that the reform affected second-born children.

In summary, I show that providing a long period of paid leave reinforces a traditional division of labor within the household and has a detrimental impact on children's verbal skills. By doing so, this paper makes several contributions to the existing literature. First, to the best of my knowledge, this paper provides some of the first evidence that a single extension in leave benefits can work against several of these programs' intended goals. Parental leaves are typically designed to decrease the gender gap in the labor market and promote child well-being. Instead, I show that long periods of benefits can have the opposite effect on all these outcomes.

Prior evidence on how lengthy periods of leave affect household behavior and child development is relatively scarce. Most previous work looks at leaves that are shorter than one year (see Rossin-Slater, 2018). Studies on longer periods of leave mainly focus on their impacts on women's labor market opportunities. For example, several previous studies document that the French reform induces women to exit the labor market and that they incur a wage penalty after returning to work (Piketty, 2005; Pailhé and Solaz, 2006; Lequien, 2012).[2] However, these studies are different than mine as they do not examine fathers' response or children's outcomes. The few papers that provide more comprehensive evaluations of extended periods of leaves yield results that are significantly different than mine, as they report positive or no effects on fertility, marriage and child outcomes (Lalive and Zweimüller, 2009; Lalive et al., 2014; Danzer and Lavy, 2017; Danzer et al., 2017; Ginja et al., 2020). While these settings diverge from mine along several dimensions, one potential reason for why my findings are different is that the French reform provides access to up to three years of paid leave. Compared to the rest of the literature, this is the largest one-time expansion in the duration of parental leave benefits. This is important given that many countries have recently increased the length of paid leaves.

Second, this paper adds to the literature by showing that leave programs can increase household specialization, through inducing fathers to raise their work hours. While previous studies show that leaves can reduce women's labor supply, less is known about men's response to these programs. Most of the literature concerning fathers examines how their leave-taking affects subsequent labor market responses and division of housework. A key difference in my study is that fathers do not increase or alter their leave take-up. I therefore document that mothers' leave-taking affects men's labor supply, even if fathers do not take up leave.[3]

Finally, this study builds on a large body of literature which investigates the relation between

[2]In other settings, cross-country evidence suggests that lengthy leaves are detrimental to women's earnings (Ruhm, 1998; Olivetti and Petrongolo, 2017), and can increase their share in part-time and low-level occupations (Blau and Khan, 2013). These findings are largely consistent with studies that use expansions in the duration of leaves as natural experiments (Lalive and Zweimüller, 2009; Lequien, 2012; Schönberg and Ludsteck, 2014; Bičáková and Kalíšková, 2016; Stearns, 2018; Mullerova, 2017).

[3]Along similar lines, Johansson (2010) and Dahl et al. (2016) both look at the impact of maternal leave on men's labor outcomes, but find no significant effects on employment or earnings. Ginja et al. (2020) show that access to a higher amount of leave benefits in Sweden increases the earnings of spouses of women in the top third of the earnings distribution. However, Moberg (2017) shows that the same reform reduces fathers' take-up of parental leave. In my setting, fathers increase work hours without altering their leave take-up.

leave programs and children's outcomes. Studies looking at the introduction of paid and unpaid leaves find positive effects on children's health and long-term education and earnings (Rossin, 2011; Carneiro, Løken and Salvanes, 2015; Stearns, 2015). However, subsequent expansions in coverage—for up to one year—have no impact on short-term health, cognitive development or long-run education (Baker and Milligan, 2008, 2010 and 2015; Liu and Nordström Skans, 2010; Rasmussen, 2010; Dahl et al., 2016). My finding that children are adversely affected contrasts with most of the previous literature on parental leaves. One exception is the study by Dustmann and Schönberg (2012), which documents a small negative effect on children's educational achievement at age 14 following an increase in the length of unpaid leave from 18 to 36 months in Germany. I complement their results by showing that larger extensions in the duration of paid leave can also hinder child development.

The rest of this paper is organized as follows. Section 2 provides detailed information on the institutional setting. Sections 3 and 4 respectively present the data and identification strategy. Sections 5 and 6 show the results and robustness checks. Finally, I conclude in section 7.

2 Institutional Background

2.1 Parental Leave in France

All working mothers in France are entitled to job-protected maternity leave. Mothers of one or two children have access to 6 weeks of prenatal leave and 10 weeks of postnatal leave. A maximum of 3 weeks of prenatal leave can be transferred until after the child's birth. Mothers also receive 100% of their income, averaged over the three months prior to taking the leave.[4]

To examine how long periods of parental leave impact parents and children, I exploit the 1994 reform of the "Allocation Parentale d'Education" (or APE) program. The APE was created in 1985 to help parents balance their work and family life. It provides either one or both parents a fixed non taxable monthly cash allowance, to take time off from work after the birth of a child and until his/her third birthday. Mothers can take maternity leave first then start benefiting from the APE. Initially, the program was reserved for parents of three children and more. The law "Famille", passed on July 25, 1994, extended benefits to parents whose second child was born on or after July 1, 1994.[5] Hence, the reform extended the time period that parents are eligible to receive benefits by almost three years—making this the largest extension in the duration of benefits documented in the literature. The extension of the APE was retroactive and unannounced before the enactment of

[4]There is a ceiling on the amount of payments that can be disbursed.

[5]The law "Famille" changed several other family policies but the APE extension was the only one with a cutoff date of July 1994.

the law. This makes it impossible for parents to time the date of birth of their second child in order to gain eligibility for APE benefits.

Mothers and fathers are eligible for the APE if they worked or received unemployment benefits for 2 years—not necessarily consecutive—in the 5 years prior to a second birth. A parent has to be either out of the labor force or working part-time while receiving benefits. The monthly payment is approximately €452 if the parent exits the labor market. Parents who instead choose the part-time option, receive around €299 if they work less than 20 hours per week, or €226 if they work between 20 and 32 hours a week. Parents can simultaneously take the leave by both working part-time. In that case, their total monthly payment is €452. The maximum benefit is around 45% and 33% of the median wage for mothers and fathers of two children and more, respectively.[6] The APE is administered by the Caisses d'Allocations Familiales (CAF). The CAF are government agencies which operate at the départements level and are responsible for processing and approving applications for family benefits, as well as disbursing these benefits.

A parent can combine the APE with the "Congé Parental d'Education" (CPE) if he/she worked in the same company for at least a year prior to childbirth. The CPE allows new parents to take up to three years of job-protected unpaid leave. Unlike the APE, the CPE was already available to all parents in 1994, regardless of their children's birth order. Therefore, the reform increased the time period in which parents are eligible for cash benefits, without changing the length of job-protection. This distinction is important since as Stearns (2018) shows, increasing access to leave payments has different effects on mothers' outcomes compared to raising the amount of job-protection.

The APE's take-up rate was higher than expected and 98% of recipients were women. Piketty (1998) estimates that by the end of 1997, around 303,000 mothers of two children—with at least one child aged less than 3—benefited from the program. This constitutes almost 40% of all such mothers. Most beneficiaries withdrew completely from the labor force. In fact, 222,000 recipients—or around 30% of all mothers with two children aged less than 3—had taken the maximum amount of benefits by the end of 1997. The projected costs of the APE for mothers of two children who exited the labor market were approximately €1 billion, but by 1997 the actual costs were already €1.41 billion (Afsa, 1998).

2.2 Childcare in France

Since the 3-year leave allows parents to spend more time at home with their child, it is important to highlight the other available childcare options in France—that is what type of childcare

[6]Payments for part-time work are 30% and 23% of mothers' median wage. For fathers, these numbers are 22% and 17%, respectively. Numbers are based on author's calculations. Data are taken from the French Labor Force Survey (see section 3.1 for details). The sample includes parents aged 18-64 who are observed between the years 1990 and 2002.

arrangements would leave take-up be substituting for. Although not mandatory, nearly all children between ages 3 and 6 are enrolled in public preschools (or *écoles maternelles*). Around one third are admitted at age 2 depending on seat availability. Children are grouped into classes according to their age. As a result, those who enroll at ages 2 and 3 attend 4 and 3 years of preschool, respectively. Preschools are universal, free of charge, offer a government-mandated curriculum and employ teachers who have the same credentials as those who work in elementary schools. During the academic year, they are typically open 4 days a week for 6 hours a day, as well as on Saturday mornings.[7]

Parents of children aged less than 3 have access to several paid but subsidized childcare options. Children can be placed in publicly-funded nurseries (or *crèches*) or in the care of registered childminders (or *assistantes maternelles agréées*). Childminders care for children in their home and their work is regulated by the government. By law, they can care for a maximum of 3 children at the same time. Individuals wanting to work as childminders are required to obtain an authorization from the government that is renewable every 5 years. To receive authorization, applicants are required to pass medical and ethics exams, receive 60 hours of training, and have their homes inspected and approved for childcare by government officials. Childminders sign formal contracts with parents, and their remuneration and work hours are regulated by the government.

A little less than half of children aged less than 3 are placed in the care of registered childminders, preschools and nurseries. Specifically, among children aged less than 3 whose mothers were employed and had a partner in 1990, 11.4% and 9.2% were enrolled in nurseries and preschools, respectively.[8] Another 25.1% were placed in the care of out-of-home registered childminders and 19.4% were mainly in the care of their mothers (Math and Renaudat, 1997). The rest were cared for by other family members or individuals. On average, households spend around €300 on childcare arrangements (Goux and Maurin, 2010).

3 Data

Examining how extended leaves affect multiple dimensions of household behavior requires data that include a variety of outcomes for both parents and children observed in the same setting and time period. To that end, I collected data from multiple sources. The following provides a description of these datasets, as well as sample construction.

[7] Specifically, preschools are open from 8:30 to 11:30 a.m. and from 1:30 to 4:30 p.m., but parents also have the option of keeping their children in preschool during lunchtime and after 4:30 p.m.

[8] The numbers include children aged 2 who are eligible to enroll in preschools.

3.1 The French Labor Force Survey

Data on mothers' and fathers' labor market outcomes are taken from the French Labor Force Survey (LFS). The LFS is a household survey administered from 1990 to 2002 by the French National Institute of Statistics and Economic Studies (INSEE). It is a representative sample of the entire population, with a sampling rate of 1/300, covering around 150,000 households per year. Each household member aged 15 years and above is interviewed in March of every year for three consecutive years. The LFS provides demographic characteristics as well as detailed information on labor market outcomes such as labor force participation, employment, occupation and hours of work. The LFS also includes the month and year of birth of each child living in the household, but not birth order. I therefore consider a child to be a second-born if he/she is the second oldest among all children living in a household in a given year. A potential concern with this definition is that in some cases, I could be misassigning birth order if an older child already left the household. This issue is mitigated by focusing on parents' labor supply in the first few years after childbirth, coupled with the fact that average spacing between first and second births in France is less than 4 years (Toulemon and Mazuy, 2001).

The main analysis sample consists of mothers and fathers, aged 18-64, who are either married or cohabiting and have at least two children living in the household. Single parents did not benefit from the APE because they had access to a more generous program, the "Allocation pour Parent Isolé". I focus on parents' labor market response in the years they are eligible to take APE benefits (i.e., the first through third years after their second child's birth), and in the years after APE benefits expire (i.e., years 4 to 7 after their second child's birth) . Accordingly, I restrict my sample to individuals who are observed in at least one of the first 7 years after the birth of their second child.

Table 1 displays means for parents' main labor market outcomes. Benefit receipt was conditional on parents either being out of the labor force or working part-time. Since I do not have data on APE take-up, I focus on these outcomes to understand whether parents responded to the new benefits. The different columns show corresponding means for parents of children born within 4 months before and after the cutoff and in each year after the birth of the second child. Across all 4 years, mothers of children born before the cutoff are less likely to be out of the labor force compared to mothers of children born after the cutoff. Specifically, around 27%, 25% and 29.9% of mothers of children born before the cutoff (Panel A and columns (1), (3) and (5)) are out of the labor force, while around 50% of mothers of children born after the cutoff are out of the labor force in years 1 through 3 (columns (2), (4) and (6)). Around 45% of mothers of children born after the cutoff declare that their main occupation is a stay-at-home mother versus only around 25% of mothers of children born before the cutoff. At the same time, the share of women in middle-skilled occupations, which is between 50% and 55% for mothers of children born before the cutoff, drops to between 30% and 35% for mothers of children born after the cutoff. In contrast, the share of women in low and high-skilled occupations is more stable before and after the cutoff.[9] Taken together, these means suggest that women took up APE benefits through exiting the labor market and that the leave mainly affected women who are in middle-skilled occupations.

The means for most fathers' outcomes in Panel B of Table 1 do not show a clear pattern. In the first two years after childbirth, around 1.6% of fathers whose children were born before the cutoff are out of the labor force versus around 4% of fathers whose children were born after the cutoff. However, the drop in fathers' labor force participation at the cutoff does not persist in the following 2 years. Around 90% of fathers are employed and work full-time across all years. In my main analysis, I also examine how fathers change their work hours in response to mothers' leave take-up. I focus on fathers' actual hours of work during the reference week as well as usual hours. Usual hours are the number of hours worked during a typical week. Unlike actual hours, they do not include irregular overtime work or absences, as well as individuals who have irregular work schedules. Another difference between the two measures is that individuals arguably have more control over their actual hours. This is because an employee would have to renegotiate a new work contract to alter his/her usual hours. On the other hand, variation in actual but not usual hours reflects changes in take-up of vacations and sick leaves, absences, and overtime work (Goux, Maurin and Petrongolo, 2014). To reduce the influence of outliers, I drop men who report having more than 98 hours of work per week.[10] With the exception of the first year after the second child's birth, fathers of children born after the cutoff have higher actual hours than fathers of children born before. For usual hours, the pattern is less clear and fathers provide around 42 usual work hours per week.

Panel A of Table 2 shows means for demographic characteristics for individuals before (column (1)) and after the cutoff (column (2)). On average, mothers are 29 years-old at the birth of their second child while fathers are around 32. Approximately 90% of parents are born in France, and 40% of mothers and 35% of fathers have a high school degree or more.[11] I proxy parents' socioeconomic status by their fathers' occupations. Around 40% of parents have a father who is a manual

[9]The Labor Force Survey divides occupations into 6 different categories, with each category representing a specific skill level or socioeconomic status. The 6 categories are 1. Farmers, 2. Artisans, traders and businessmen, 3. Executives and other high-skilled occupations (such as engineers, college professors, medicals doctors etc.), 4. Intermediate occupations (such as school teachers, secretaries, nurses, massage therapists and dental assistants, various clerks and technicians…), 5. "Employees" (such as cleaning and maintenance workers, childcare and food preparation workers, hairdressers, cashiers, waitresses, etc.) and 6. Manual workers (such as laborers, machine operators, helpers, transportation and material moving occupations, etc.). I define high-skilled occupations to include: 2. Artisans, traders and businessmen and 3. Executives and other high-skilled occupations. Middle-skilled occupations include 4. Intermediate occupations and 5. "Employees". Finally, low-skilled occupations include 1. Farmers and 6. Manual workers.

[10]This excludes 0.32% of fathers in my main sample. In results available upon request, I find that my main estimates are robust to the inclusion of these individuals

[11]Specifically, these are individuals who have a *Baccalauréat* degree or more. French students can pursue one of various tracks in high school. The *Baccalauréat* is a degree awarded to individuals who graduate from an academic or technical track.

worker, while 10% have fathers who are in high-skilled or managerial occupations.

3.2 Data on Children's Outcomes

Data on children are taken from the Enquête Santé en Milieu Scolaire 1999-2000. The dataset includes information on children's health status, performance on tests that assess their verbal development, birth order as well as month and year of birth. The survey was administered by government-affiliated physicians to 30,000 children who were enrolled in their last year of preschool in the academic year 1999-2000. As mentioned earlier, children of the same age are grouped in the same classes in preschool. Hence, the data only include children born in 1994 and who are around 5-6 years-old at the time of the survey. Nonetheless, 99.4% of children aged 5 were enrolled in preschool in the year 1990-91 (Papon and Martin, 2008), which alleviates concerns over selection into the children's sample.

I restrict the sample to all second-born children. Panel B of Table 2 reports main outcomes' means for children born before and after the cutoff in columns (1) and (2), respectively. On average, children are around 2.9 years-old when they first enroll in preschool. I have information on children's performance in five tests of verbal development: phonological awareness, vocabulary development, oral comprehension, spontaneous and overall speech.[12] The survey does not report exact scores but rather whether the child has a normal score. For the phonological awareness, vocabulary development, oral comprehension tests, the survey also reports whether the child has a score that is 1 to 2, or 3 standard deviations below normal. For each test, I create a dummy variable that is equal to one if the child has a normal score, and zero if he/she scores below normal. The share of children in my sample who have normal scores on these tests varies between 80 and 95.6% depending on the test, with oral comprehension having the highest passing rate. Interestingly, across all outcomes, children born after the cutoff are less likely to have normal scores compared to children born before the cutoff.

[12]The five tests are conducted as follows. The phonological awareness test focuses on whether the child is aware of the sound structure of words. The child is asked to identify rhymes and syllables. In the vocabulary development test, the child is shown a series of images. The interviewer then gives him a word and asks him to point to the drawing that corresponds to the word. For oral comprehension, the child is presented with four images. The interviewer then gives him a sentence and asks him to point to the drawing that corresponds to that sentence. Physicians evaluate a child's spontaneous speech by identifying whether he/she (i) can form sentences with a minimum of 4 words, (ii) uses sentences that include the three subordinates: who, because, as, (iii) uses grammatically correct sentences. Finally, overall speech is considered as not normal if the child exhibits at least one of the following symptoms: speech impairment, speech disorder, elision of syllables, loss of word, stuttering, breathing problems while speaking, slowness of speech, very little talk.

4 Empirical Strategy

4.1 Regression Discontinuity Design

Eligibility for the leave was contingent on the second child's date of birth being after July 1, 1994. Importantly, the policy change was announced after that date. I therefore use a regression discontinuity design (RD) based on the second child's date of birth to estimate the causal effect of eligibility for the 3-year leave on parents' labor market outcomes and children's development (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). This allows me to overcome selection bias due to the fact that those who are eligible for parental leave might be different from those who are ineligible.

To conduct the RD analysis, I use both the continuity-based and local randomization approaches. Both approaches assume that individuals on either sides of the cutoff are comparable in every way and the only difference between them is the treatment status. The main difference between the two approaches is how this comparability is formalized. In the continuity-based approach, the average potential outcomes are assumed to be continuous functions of the running variable at the threshold. In this case, the average treatment effect at the threshold will be equal to the difference between the limits of average observed outcomes of the treated and control groups as the running variable converges to the threshold (Cattaneo, Idrobo and Titiunik, 2019). On the other hand, the local randomization approach explicitly formalizes the idea that around the threshold, the RD is similar to a randomized experiment. This implies that in a small window around the cutoff, the analysis can be conducted as if individuals were randomly assigned to treatment and control groups (Cattaneo, Idrobo and Titiunik, 2018).

For the continuity-based approach, I formally estimate the following reduced form equation:

$$Y_i = \alpha + \beta D_i + \tau g(R_i) + \delta g(R_i) \times D_i + \epsilon_i \tag{1}$$

where the dependent variable Y represents one of various outcomes for parent or child i. D is a dummy variable that is equal to 1 if the second child was born on or after the July 1, 1994 threshold. R is the running variable which represents the second child's month and year of birth and it is defined as months relative to the cutoff. $g(.)$ captures the relationship between R and Y. I specify $g(.)$ to be a linear function of R using data that is close to the cutoff. I also allow trends in month-year of birth to be different on either side of the cutoff by interacting $g(.)$ with D. ϵ is the error term. Further details on the choice of optimal bandwidth, polynomial order and kernel function are presented in Appendix Section C. The coefficient of interest, β, captures intent-to-treat (ITT) effects of parental leave eligibility on various outcomes. To get a local average treatment effect, I would need to rescale β by an estimate of leave take-up. Since data on actual receipt of benefits are not available, all results presented in this paper are ITT estimates and are interpreted as the effect of being eligible for an extended period of leave.

The running variable in this setting, month and year of birth, is discrete with a few mass points. As highlighted by Cattaneo, Idrobo and Titiunik (2018), the continuity-based approach (i.e., equation 1) may not be appropriate in this case and a preferable alternative is to use the local randomization approach. This involves choosing a bandwidth or a window that is close to the cutoff and within that window, comparing outcomes' means for individuals who are above and below the threshold. Throughout the paper, my main estimates will be taken from the local randomization approach. I also provide estimates from the continuity-based approach in the appendix.

In the local randomization, a crucial choice is the length of the window around the cutoff. To choose this window, Cattaneo et al. (2018) suggest implementing a data-driven window selection procedure that uses information provided by baseline covariates. This procedure is based on the assumption that the effect of the treatment on the covariate is zero for all individuals inside the window, but the treatment and control groups differ in their baseline covariates outside of this window. The procedure thus picks the largest window for which the covariates are balanced. Specifically, for each window, it tests the null hypothesis that the baseline covariate is unrelated to the treatment. The preferred window will be the largest window in which the null hypothesis fails to be rejected.

When implementing this procedure, I increase the length of the windows in which the test is conducted in fixed steps of 1 month. In other words, the procedure will first test the null hypothesis for a window of 1 month on either sides of the cutoff, then for a window of 2 months, 3 months and so on. The procedure also requires that the researcher chooses the baseline covariates to be used, the test statistic on which the test of the null hypothesis is based, the randomization mechanism that is assumed inside the window, and the significance level that determines the rejection of the null hypothesis. I use baseline covariates from the Labor Force Survey which include mother's and father's ages at the birth of their second child, their years of education as well as dummy variables for whether the second child is male, whether the mother and father were born in France, whether they have a high school degree or more, and whether parents' fathers are manual workers or in high-skilled occupations. Following Cattaneo et al. (2018), I use the difference-in-means test statistic, a complete randomization mechanism and a significance level of 0.15. The chosen window from this procedure is 4 months on either sides of the cutoff.[13]

In the appendix, I show that the main estimates are robust to the use of different bandwidths, the continuity-based approach and inclusion of controls and second child's month of birth fixed effects. I use robust standard errors since clustering by a discrete running variable leads to confidence

[13]Data on children's outcomes do not include baseline covariates. As a result, I conduct the window selection procedure using data from the Labor Force Survey. For children's outcomes, I use 4 months as my preferred window but I also show that results are robust to using different windows.

intervals with worse coverage properties and does not resolve specification bias issues (Kolesár and Rothe, 2018). In some specifications, parents' labor market outcomes are stacked across different years. In those cases, I cluster standard errors at the individual level to deal with the issue that some individuals are observed multiple times.

One potential issue is that throughout the paper, I use many outcomes to capture the effect of the reform on some broader outcome of interest. For example, since I am interested in estimating the impact of the reform on households' labor market decisions (i.e., the broader outcome), I report regression estimates for 14 parental labor supply outcomes across multiple years. These 14 outcomes across different years hence belong to the same "family". Having many outcomes that belong to the same "family" could result in overrejection of the null hypothesis of no treatment effect due to multiple inference (Anderson, 2008). To deal with this concern, I report q-values i.e., p-values adjusted for multiple inference using the False Discovery Rate method (Benjamini and Hochberg, 1995). When computing q-values, I consider two main families of outcomes, one intended to capture parents' labor market decisions and the other children's response to the reform. I provide details on which exact outcomes are included in each family in sections 5.2 and 6.1 for parents and children, respectively.

4.2 Tests of Identification

The main assumption in an RD design is that individuals cannot precisely manipulate the running variable to receive treatment. In this context, it would be problematic if parents can strategically time the conception or date of birth of their second child to become eligible for the 3-year leave. Given the timing of the policy change, it is impossible for parents to do so. The reform was passed on July 25, 1994—and was not announced in advance—but awards benefits to parents of children born before this date, on July 1, 1994.

I conduct two formal tests to alleviate concerns over manipulation of the running variable. First, I test whether the density of the running variable is discontinuous at the cutoff (McCrary, 2008). The result of the McCrary density test is shown in Figure 1. The density of the running variable is smooth around the cutoff and I cannot reject the null hypothesis of no discontinuity at conventional significance levels (corresponding test statistic=-0.830). This indicates that parents did not strategically time the date of birth of their second child in order to benefit from the leave.

Second, I show that the distribution of pre-determined characteristics is continuous around the threshold. Appendix Figures A1 and A2 plot various baseline covariates, taken from the French Labor Force Survey, as a function of the running variable. These covariates are mother's and father's ages at the birth of their second child, their years of education as well as dummy variables for whether the second child is male, whether the mother and father were born in France, whether they have a high school degree or more, and whether parents' fathers are manual workers or in high-skilled occupations. The figures are similar to subsequent ones in that each circle represents the outcome's local average over a one-month range. Since the running variable is defined as months relative to July 1, 1994, the cutoff is represented by a value of zero on the x-axis. All baseline covariates figures show no discontinuities around the cutoff. Furthermore, Appendix Table A1 presents RD estimates of the effect of the reform on these covariates using different bandwidths, as well as the local randomization (at bandwidths of 2, 4 and 6 months) and local linear (all other bandwidths) methods. Consistent with the lack of discontinuities at the cutoff, estimates are statistically insignificant at conventional levels.

In Appendix Figures A3 and A4 and Table A2, I conduct the same test using additional baseline covariates taken from the "Enquête Etude de L'Histoire Familiale". This survey was conducted among individuals who are aged 18 years and above and were part of the 1999 population census. I use data from this survey in section 6.3 to examine whether the reform affected mothers' marital outcomes. Detailed description of the survey and sample construction is provided in Appendix section B. This survey gives me access to baseline covariates that are also found in the Labor Force Survey.[14] However, it also allows me to examine additional covariates that capture mothers' labor market attachment before childbirth including a dummy variable for whether the mother had a period of work interruption or unemployment, mother's work interruption length and age at first job (Appendix Figures A4c to A4e).[15] Again, all covariates are smooth around the cutoff and corresponding regression estimates across different bandwidths in Appendix Table A2 are statistically insignificant.

5 Results for parents' outcomes

5.1 Effects of the reform on mothers' labor market outcomes

I start by showing the reform's effects on mothers' labor market outcomes in each year following the birth of their second child. The different panels in Figure 2 plot the reform's effects—estimated using the local randomization approach and a bandwidth of 4 months as described in section 4.1—on various outcomes along with their 95% confidence intervals, as a function of year since childbirth. I first provide evidence that mothers did take up the leave. Since data on leave take-up are not available, I leverage the fact that leave-taking was conditional on parents ei-

[14] These include mother's age at the birth of her second child and dummy variables for whether the second child is male, whether the mother was born in France, whether she has a high school degree or more, and whether her father is a manual worker or in a high-skilled occupation.

[15] For mothers who did not have a work interruption prior to childbirth, the variable "work interruption length" is set to 0.

ther being out of the labor market or working part-time. Figure 2a reveals that the reform induced mothers to leave the labor force for up to 4 years following the birth of their second child. In the first year, mothers who are barely eligible for the leave are 21.7 percentage points more likely to be out of the labor force compared to those who are barely ineligible. In the second and third years after childbirth, mothers are respectively 24.7 and 21.9 percentage points more likely to be out of the labor force due to the reform. The reform's effect on labor force participation drops to 11 percentage points in the fourth year, indicating that some mothers return to work once leave benefits expire. The reform's effects do not persist beyond the fourth year, as estimates in years 5 to 7 are centered around 0 and are not statistically significant at conventional levels. This indicates that mothers return to their jobs after leave benefits expire.

To further substantiate these results, I present RD graphs as well as estimates from different RD specifications for all mothers' labor market outcomes. For ease of exposition, I stack labor market outcomes for years in which parents were eligible to receive the leave (i.e., years 1 to 3 after the second child's birth), resulting in up to three observations for each individual.[16] I also show separately estimates for stacked labor market outcomes in the years after leave benefits expired (i.e., years 4 to 7 after the second child's birth). Stacking outcomes across years also allows me to increase sample size thereby improving statistical power. RD graphs, which plot the relationship between mothers' various labor market outcomes and distance of the second child's month and year of birth to the cutoff, are shown in Appendix Figure A5 for years 1 to 3 and in Appendix Figure A6 for years 4 to 7. Corresponding estimates using a local randomization approach with a bandwidth of 4 months are presented in Table 3.

Consistent with the year-by-year effects, Appendix Figure A5a reveals a large discontinuity at the threshold. This indicates that in the years they were eligible to receive the leave, mothers of second children born right after the cutoff are more likely to be out of the labor force than those with children born right before. In Panel A and column (1) of Table 3, I estimate that the magnitude of this discontinuity in on the order of 22.9 percentage points. In contrast, Appendix Figure A6a shows that mothers' labor force participation in years after leave eligibility expires, is smooth around the cutoff. The corresponding estimate in Panel B and column (1) of Table 3 is small (0.015 percentage points) and statistically insignificant at conventional levels.

Both employed and unemployed parents qualified for the leave if they either worked or received unemployment benefits for 2 years in the 5 years preceding the birth of their second child. Hence, a natural question is whether the reform induced women to actually leave employment. Figure 2b shows that the reform's effects on mothers' employment rate, across different years, mirror the ones for labor force participation. In year 1, mothers who are barely eligible for the leave are 15.8

[16]In other words, if a second child is born in 1994, his/her parents would appear in the sample for as many times as they are surveyed between March, 1995 and March, 1998.

percentage points less likely to be employed than those who are barely ineligible. In years 2 to 4, mothers are respectively 20.4, 16.2 and 10.7 percentage points less likely to be employed due to the reform, but effects do not persist in years 5 to 7. This implies that the documented decrease in labor force participation is largely driven by employed mothers leaving the workforce.

Year-by-year effects in Figures 2c and 2d further reveal that mothers are mainly leaving full-time (rather than part-time) jobs to take the leave. Indeed, in the years mothers were eligible for the leave, the drop in their likelihood of working full-time is 13.6 percentage points (Panel A, column (3) of Table 3), but no significant effects are detected in years after leave eligibility expires (Panel A, column (3) of Table 3). On the other hand, estimates for the likelihood of working part-time are statistically insignificant at conventional levels both during and after leave eligibility, but I cannot rule out sizable drops (column (4) of Table 3). The lack of significant effects on part-time employment are also consistent with previous work by Piketty (2005) showing that mothers took up the leave benefits mainly through exiting the labor market.

I next examine the types of occupations that were affected by the reform. Figure 2e reveals that in years 1 to 3 after childbirth, women are between 20.2 and 24 percentage points more likely to declare that they are stay-at-home mothers due to the reform, and the estimate drops to a statistically significant 8.6 percentage points in year 4—while no significant effects are observed in the following years. This provides further evidence that women are indeed leaving the workforce to take the leave. Figures 2f to 2h further reveal that mothers exited middle-skilled occupations. Indeed, the share of women in these jobs dropped by 20 percentage points in the first three years following second child's birth with no significant effects observed in the following years (column (7) of Table 3). On the other hand, no significant changes are detected in the share of women in low and high-skilled occupations (columns (5) and (8) of Table 3).

As a robustness check, Appendix Tables A3 and A4 further show for all mothers' labor outcomes, estimates of the reform's effects taken from RD regressions using (i) a local randomization approach across various bandwidths (specifically, bandwidths of 2, 4 and 6 months in columns (1) to (3)), (ii) a local linear approach across different bandwidths (specifically, bandwidths of 16, 19, 22, 25 and 28 months in columns (4) to (8)) as well as, (iii) with and without controls and second child's month of birth fixed effects. Importantly, estimates across various specifications are similar to the main effects for both the stacked years 1 to 3 (Appendix Table A3) and years 4 to 7 (Appendix Table A4).

Finally, while it would be interesting to document the incidence and magnitude of income loss due to mothers leaving the labor force, the relevant data are not available. The LFS only contains information on employed individuals' wages. I do a back-of-the-envelope calculation to estimate the loss of household income due to mothers taking leave. I document that mothers are exiting middle-skilled occupations to take the leave. Middle-skilled occupations include mothers who work in two types of occupations as defined by the Labor Force Survey: Intermediate occupations (such as school teachers, secretaries, nurses, massage therapists and dental assistants, various clerks and technicians…) and "Employees" (such as cleaning and maintenance workers, childcare and food preparation workers, hairdressers, cashiers, waitresses, etc.).[17] In Figure 2g, I show that mothers are between 14.1 and 22.5 percentage points less likely to hold middle-skilled occupations in the 4 years following childbirth, as they exited the labor force to take the leave. More specifically, in years 1 to 4 after the birth of their second child, mothers are respectively 6.8, 5.5, 8.6 and 8.8 percentage points less likely to be in intermediate occupations, and 12.1, 16.9, 9.8 and 5.3 percentage less likely to hold jobs as "Employees". The median monthly wages at the time of the reform were around €1,357 and €854 for mothers in the intermediate occupations and "employees" categories, respectively. Assuming that mothers gave up their entire wages while on leave, I calculate that the reform led to a decrease in mothers' monthly wages by around €196 (=(1,357 $\times$0.068) + (854$\times$ 0.121)), €219, €200 and €165 in years 1 to 4, respectively.[18] At the same time, mothers who left the labor force to take leave received €452 per month in cash benefits. Assuming that all mothers who left employment were receiving the full benefit, I estimate that the reform increased mothers' monthly benefit receipt in years 1 to 4 by €85 (=(452 $\times$0.068) + (452$\times$ 0.121)), €101, €83, €64, respectively.[19] Therefore, I calculate that the effect of the reform on mothers' monthly income in years 1 to 4 is a loss of €111 (=-196+85), €118, €117, and €101, respectively.

5.2 Effects of the reform on fathers' labor market outcomes

Since both parents are eligible to take the benefits, I examine whether fathers are also incentivized to take the leave in the years following a second child's birth. Previous studies suggest that parental leave take-up amongst men is generally low (Lalive and Zweimüller, 2009), although recent evidence from the U.S. indicates that parents can be incentivized to share leave when they are offered similar benefits (Bartel et al., 2018). The different panels in Figure 3 show the local randomization estimates of the impact of the reform on fathers' labor market outcomes, along with their 95% confidence intervals, in each year after the second child's birth. Figures 3a and 3d show that the reform does not have statistically significant effects on fathers' likelihood of being out of the labor force or working part-time. Figures 3b and 3c reveal some positive and statistically significant effects on the likelihood that fathers are employed and working full-time in the third year after the second child's birth. However, these effects do not pass falsification tests that use July 1

[17] See footnote 6 for more details on the Labor Force Survey occupational classification.

[18] The loss in mothers' wages is calculated by adding the wage losses for the "employees" and intermediate occupations categories. For years 2 to 4 is calculated similarly to year 1 and as follows: year 2=(1,357 $\times$0.055) + (854$\times$ 0.169); year 3 =(1,357 $\times$0.086) + (854$\times$ 0.098); year 4=(1,357 $\times$0.088) + (854$\times$ 0.053).

[19] The gain in mothers' benefits for years 2 to 4 is calculated similarly to year 1 and as follows: year 2=(452 $\times$0.055) + (452$\times$ 0.169); year 3 =(452 $\times$0.086) + (452$\times$ 0.098); year 4=(452 $\times$0.088) + (452$\times$ 0.053).

from other years as a fake cutoff (see section 5.4). Taken together, these results suggest that fathers are not taking the benefits, and are in line with previous reports documenting that 98% of recipients were women (Piketty, 2005).

Even if men do not take the leave, they might still adjust their labor supply at the intensive margin. Becker (1981) argues that household goods are more efficiently produced if spouses with differing comparative advantages specialize in market and non-market work. This typically means that women devote more time to home production while men specialize in the labor market. The APE reform makes home production more valuable since it provides parents with 3 years of benefits in order to take time off from work. This could increase gains to specialization, prompting mothers to spend more time at home and fathers to increase their labor market time. Specialization in this setting would thus induce mothers to either exit the labor market and fathers to increase their hours of work. I test this idea by focusing on weekly usual and actual hours of work in Figures 3e and 3f. I find no statistically significant treatment effects on usual hours. This result is unsurprising since a change in usual hours would indicate that fathers are either taking the benefits by switching to part-time work or that they negotiated a new labor contract with their employer. On the other hand, actual hours of work exhibit a clear pattern. In the first year after a second child's birth, there is a 2.16 decrease in actual hours but this estimate is not statistically significant at conventional levels. In year 2 however, actual work hours increase by 3.27 hours due to the reform (significant at the 10% level). The increase in actual hours persists in years 3 and 4 as corresponding estimates are on the order of 2.09 and 2.54 hours respectively—albeit they are not statistically significant at conventional levels.

Similar to mothers' outcomes, I present the reform's effects on fathers' labor market outcomes stacked for years in which parents were eligible to receive the leave (Panel A of Table 4), and for the years after leave benefits expired (Panel B of Table 4). However, in this case, results for the first year are reported separately from years 2 and 3 to accurately capture the evolution of the reform's effects on fathers' actual hours. Indeed, the year-by-year effects suggest that fathers reduced their work hours in year 1, but then compensated for this initial drop by increasing their labor supply the following years. Corresponding RD graphs, which plot the relationship between fathers' various labor market outcomes and distance of the second child's month and year of birth to the cutoff, are displayed in Appendix Figure A7 for year 1, Appendix Figure A8 for stacked years 2 and 3, and in Appendix Figure A9 for stacked years 4 to 7.

Consistent with the year-by-year effects, the RD graphs and Table 4 reveal that fathers are not changing their labor supply at the extensive margin as estimates for labor force participation, full-time and part-time work are mostly statistically insignificant both in years parents were eligible to receive the leave and in years after leave benefits expired. On the other hand, the RD graph for stacked actual hours in years 2 and 3 (Figure A8e) reveals a clear discontinuity at the cutoff. The corresponding estimate in Panel A and column (5) of Table 4 indicates an increase of 2.815 in actual hours of work—statistically significant at the 5% level. However, similar to mothers' labor supply effects, this increase in work hours does not persist after leave benefits expire (Panel B and column (5) of Table 4).

Appendix Tables A5, A6 and A7 respectively show for years 1, 2 to 3 and 4 to 7, results for fathers' main outcomes taken from RD regressions using (i) a local randomization approach across various bandwidths (specifically, bandwidths of 2, 4 and 6 months in columns (1) to (3)), (ii) a local linear approach across different bandwidths (specifically, bandwidths of 16, 19, 22, 25 and 28 months in columns (4) to (8)) as well as, (iii) with and without controls and second child's month of birth fixed effects. The main effects are robust to different specifications. Additionally, I detect statistically significant decreases in actual and usual hours across some but not all bandwidths in the first year following the second child's birth. This decline in work hours (especially usual hours) suggests that fathers may be taking some parental leave in the first year after childbirth, but then compensate for it by increasing their work hours in the following years—albeit the imprecision of the estimates does not allow me to draw definitive conclusions.

Finally, as discussed in section 4.1, I use many outcomes to examine how households' labor decisions are affected by the reform, which could cause multiple inference issues. To deal with this concern, Tables 3 and 4 report in brackets q-values for mothers' and fathers' outcomes, respectively. When computing q-values, I consider all parents' labor market outcomes across different years (i.e., the stacked years parents were eligible to receive the leave and stacked years after leave benefits expire) to belong to the same family. Specifically, the family includes the following outcomes across different years: the likelihood that mothers are out of the labor force, employed, working full-time, working part-time, stay-at-home mothers, in low-skilled, in middle-skilled and in high-skilled occupations, as well as the likelihood that fathers are out of the labor force, employed, working full-time, working part-time, fathers' actual and usual hours of work. The q-values in Table 3 reveal no noticeable changes in the statistical significance of mothers' main estimates. Effects on mothers' labor force participation, employment, full-time work, probability of being a stay-at-home mother and in middle-skilled occupations in years they were eligible to receive the leave all remain statistically significant at the 1% level after adjusting their p-values for multiple inference. For fathers, the only previously statistically significant effect was on their actual work hours in years 2 to 3 after childbirth. When adjusting for multiple inference, this estimate is no longer statistically significant at conventional levels but its q-value in Table 4 is nonetheless quite close to the 10% level (q-value= 0.116).

5.3 Heterogeneity by mothers' level of education

The overall effects documented so far indicate that mothers took up the APE benefits mainly through exiting the labor force. However, it is possible that women responded differently to the extension of benefits based on their educational level. For example, highly-educated women may find the option of switching to part-time work more attractive than exiting the labor force. To understand whether the reform affected women from various backgrounds differently, I conduct a heterogeneity analysis based on their educational level in Table 5. Panel A shows estimates of the reform's effects on main labor market outcomes during the three years following childbirth, separately for mothers with a high school degree or more and those with less than a high school degree.[20] I also report p-values from tests of equality of coefficients from these two samples.

Results indicate that both high and lower-educated women take the benefits through exiting the labor force. However, low-educated women do so at a higher rate as they are around 29 percentage points more likely to be out of the labor force due to the reform versus 11 percentage points for higher-educated mothers (p-value from test of equality= 0.004). The main difference however is that high-educated women leave exclusively full-time occupations in order to take benefits, while lower-educated women leave both full-time and part-time jobs. Specifically, the latter group experiences a significant 15.2 and 9.1 percentage points decrease in the probability of working full-time and part-time, respectively. This likely reflects that compared to high-educated women, lower-educated mothers were more likely to have part-time (versus full-time) jobs prior to the reform. Additionally, high-educated women are 12.8 percentage points less likely to be observed in middle-skilled occupations, while lower-educated mothers leave both middle and low-skilled occupations in order to benefit from the leave. Panel B shows estimates of the reform's effects on main labor market outcomes by mothers' educational level in the years after leave expires (i.e., in years 4 to 7 after childbirth). As in the overall sample, I detect no significant effects on any outcome for both high and low-educated women—indicating that the reform's effects do not persist after leave benefits expire.[21] Taken together, these results suggest that women who chose to take the leave did so mainly through exiting the labor force regardless of their educational level.

In the overall sample, I find that fathers respond to the reform by increasing their work hours in the years mothers are on leave. It is possible that fathers' response varies depending on mothers' level of education. For example, compared to low-educated mothers, high-educated women will lose a higher share of their income upon taking the leave, which may induce fathers to work more hours to compensate for this potential loss of household income. Estimates of the reform's effects on fathers' labor market outcomes split by mothers' education in Panel A of Table 6 are consistent with

[20] Specifically, I consider women to have a high school degree or more if their highest diploma is the *Baccalauréat* or higher.

[21] In Appendix Table A8, I show that all heterogeneity estimates are robust to the inclusion of controls.

this idea. Specifically, in years 2 and 3 after childbirth, fathers whose spouses are high-educated increase both their actual and usual work hours by 4.557 and 3.446, respectively. No significant effects on work hours are observed for fathers whose spouses are lower-educated (and the p-value for the test of equality of coefficients across both samples is 0.033 for usual hours). Another interesting observation is that fathers whose spouses are high-educated are 5.6 percentage points more likely to work part-time in the first year following childbirth. This suggests that spouses of high-educated women may be incentivized to take the leave, but compensate for it by increasing work hours in subsequent years. As in the overall sample, Panel B of Table 6 shows that regardless of mothers' education level, the reform's effects on fathers' outcomes do not persist after leave benefits end.[22]

5.4 Robustness tests

One concern with the identification strategy is that the observed discontinuities might not be driven by the reform. For example, they could simply reflect month-of-birth effects i.e. being born in July versus June. If this is the case, then we would expect to see similar discontinuities when using July 1 from other years as a fake cutoff. Appendix Figures A10a and A10b respectively plot mothers' likelihood of being out of the labor force and fathers' actual hours of work as a function of second child's month-year of birth. These are reported for the first (second) through third year after second child's birth for mothers (fathers), since these are the years in which I detect significant effects in the main sample. The running variable in this case is defined as months relative to the fake cutoff of July 1, 1992, which is represented by a value of zero on the x-axis. As expected, both figures are smooth around the threshold, alleviating concerns over month-of-birth effects. Regression estimates from this placebo test are reported for all mothers' and fathers' labor market outcomes across all years since childbirth in Panel A of Appendix Tables A10 and A11, respectively. No significant threshold-crossing effects are detected for outcomes that were previously found to be impacted by the reform.[23]

As another placebo test, I focus on parents of first children born on either side of July 1, 1994. The idea is that since parents of first children were not eligible for the APE program, we should not expect any discontinuities in their labor market outcomes unless another policy affected all children born in July 1, 1994. Appendix Figures A10c and A10d show mothers' labor force participation and fathers' actual work hours as a function of first child's month-year of birth—using data from for the first (second) through third year after second child's birth for mothers (fathers). The figures reveal no discontinuities and regression estimates for all labor market outcomes are statistically insignificant (Panel B of Appendix Tables A10 and A11).

[22] All estimates on fathers' labor outcomes by mothers' education are robust to the inclusion of controls in Appendix Table A9.

[23] A few statistically significant effects are detected but they concern outcomes that were not affected by the reform.

As mentioned in section 3.1, the LFS does not include children's birth order. It is thus possible that I am incorrectly identifying second-borns for families in which the first child left the household. To address this issue, I drop all observations for which mother's age was greater than 35 when the second child was born. This sample of younger mothers should be less prone to bias given that they are less likely to have adult children who already moved out. The figures for mothers' labor force participation and fathers' actual work hours reveal clear discontinuities at the cutoff (Appendix Figures A10e and A10f). Corresponding regression estimates as well as estimates for all other labor market outcomes reported in Appendix Table A12 and A13 are in line with the main results. This suggests that bias from birth order misassignment is not likely to be a major issue in my main specifications.

6 Results for children's outcomes

6.1 Effect of the reform on children's verbal development

While a large body of literature studies the impacts of parental leave on a multitude of child outcomes, relatively few papers examine how children are affected when mothers take extended periods of leave. The APE reform gives me a unique opportunity to answer this question since mothers were induced to exit the labor market for at least three years due to eligibility to receive the benefits. The different panels in Figure 4 plot the various measures of children's verbal development—discussed in section 3.2—as a function of the running variable and using data within 4 months on either side of the cutoff. Most figures show clear discontinuous drops at the cutoff. The only exception is the oral comprehension test which does not exhibit a clear pattern at the threshold. Corresponding regression discontinuity estimates taken from a local randomization regression, reported in Panel A and columns (1) through (5) of Table 7, are consistent with the visual evidence. Specifically, children of eligible parents are between 3.1 and 5.6 percentage points less likely to have normal scores on various verbal development tests. This corresponds to a decrease in the probability of having a normal score on the (i) phonological awareness test by 4.8%, (ii) vocabulary development test by 3.3%, (iii) spontaneous speech test by 6.2% and, (iv) overall speech test by 6.5%.[24] Following Kling, Liebman and Katz (2007), I next group the different verbal assessment tests by creating a "verbal development index". This involves taking an equally-weighted average of the standardized values of these outcomes. I standardize each test by taking the difference between the outcome and the control group's mean, then dividing by the control group's standard deviation. As in Kling et al. (2007), if a child has at least one reported verbal development test, I impute the other tests' missing values at the treatment group's mean. Consistent with the findings for the individual outcomes,

[24]The percent decrease is calculated off of the control group mean.

children's verbal development index is negatively affected by the reform, as shown in Figure 4f and Panel A column (6) of Table 7. As an alternative measure, I also create another verbal development index—which also computes equally-weighted average of the standardized values of individual outcomes—but excludes children that have a missing observation in any of the individual components of the index. In line with all measures of children's development, Figure 4g and Panel A column (7) of Table 7 also reveal a significant drop at the cutoff.

To deal with multiple inference issues, Table 7 reports q-values for all children's outcomes in curly brackets. When computing q-values, I consider all children's outcomes in Table 7 to belong to the same family. Specifically, these include the dummy variables for phonological awareness, vocabulary development, oral comprehension, spontaneous speech, overall speech, the two verbal development indices, children's preschool-starting age and their time spent in preschool.[25] Even after adjusting for multiple inference, all estimates remain statistically significant at the 1% level.

As a robustness check, I show that all RD regression estimates do not change when I (i) include fixed effects for the date in which the tests were administered and a dummy variable for whether the child is male in Panel A and columns (1) to (5) of Appendix Table A14 and, (ii) use a local randomization with a bandwidth of 2 months and a local linear regression with a bandwidth of 6 months (Appendix Table A15).

While the children's survey does not contain their exact score on verbal development tests, it does report for some of these tests whether the child has a score that is 1 to 2, or 3 standard deviations below normal. Since the reform decreases the probability that children have normal scores on these tests, I examine whether it therefore increases their likelihood of having scores that are 1-2 or 3 standard deviations below normal. Columns (1) to (3) of Appendix Table A16 show RD estimates of the effects of the reform on the probability of having a score that is 1 to 2 standard deviations below normal on the phonological awareness, vocabulary development and oral comprehension tests. Columns (4) to (6) present similar estimates but for the likelihood of having a score that is 3 standard deviations below normal. Results indicate that the documented decrease in performance on the phonological awareness and vocabulary development tests is mostly driven by children being between 2.6 and 3.2 percentage points more likely to have scores that are 1-2 standard deviations below normal. Some statistically significant effects are also detected for scores that are 3 standard deviations below normal when I use the local randomization approach with a bandwidth of 4 months. However, these effects are not statistically significant across all specifications and are smaller in magnitude (0.4 and 1.2 percentage points) than estimates for the 1-2 standard deviations below normal score.

[25] I discuss the effect of the reform on children's preschool-starting age and time spent in preschool in Section 6.3

6.2 Difference-in-discontinuities estimates

One potential concern with the children's results is that the documented effects could be simply capturing the fact that children around the cutoff are born in different months, regardless of leave eligibility. The children's survey only reports outcomes for those born in 1994. Thus, I cannot include month-of-birth fixed effects in my main specifications. To deal with this issue, I show that no similar effects can be detected for first children born around the same cutoff. Any documented discontinuities for first-borns would suggest that my main results are not driven by the reform since their parents are not eligible for the APE program. The different panels in Appendix Figure A11 plot first-borns' various verbal tests and the verbal development index, as a function of their month and year of birth. As expected, no clear jumps are visible at the cutoff, suggesting that the effects on second-borns are the result of the reform.

As an additional robustness test, I show that the regression discontinuity results for second-borns are similar to the ones from a difference-in-discontinuities design (RD-DID). The latter design allows me to combine the regression discontinuity with a difference-in-differences by using first children born around the same cutoff as a control group. The RD-DID estimator essentially takes the difference between the discontinuities in second-borns' outcomes (i.e. the effect of the policy and month-of-birth effects) and any potential threshold-crossing effects for first-borns (i.e. month-of-birth effects). Assuming that month-of-birth effects are similar for first and second-borns, the RD-DID isolates the impact of leave eligibility. Formally, I estimate the following reduced form equation:

$$Y_i = \beta_0 + \beta_1 R_i + \beta_2 A_i + \beta_3 T_i + \beta_4 R_i * T_i + \beta_5 A_i * R_i + \beta_6 A_i * T_i + \beta_7 A_i * T_i * R_i + \gamma_i \quad (2)$$

where the dependent variable Y represents one of various outcomes for child i. R is the child's age in months. A is a dummy variable that is equal to 1 if the child is born on or after July 1, 1994. T is a dummy variable that takes the values of 1 for second children (treated group) and 0 for first children (control group). I allow for interactions between T, R and A. β_6 is the coefficient of interest and γ_i is the error term. The results from the RD-DID design are reported in Panel B and columns (1) to (7) of Table 7. For various outcomes, results are consistent with the ones from the main regression discontinuity design, as they are close in magnitude—between 3.1 and 4.2 percentage points decrease—and remain mostly statistically significant at conventional levels—even after adjusting p-values for multiple inference (in curly brackets).[26] This indicates that the RD design is indeed capturing the effect of leave eligibility on children's outcomes.

[26]The RD-DID estimates are robust to the inclusion of fixed effects for the date in which the tests were administered and a dummy variable for whether the child is male in Panel B of Appendix Table A14.

6.3 Mechanisms

So far, I document that children of women who take the leave are adversely affected, as they are more likely to have below normal scores on tests that assess their verbal development. There are several channels that could explain these negative effects.

First, the reform increased the likelihood that women are stay-at-home mothers. This likely increased the time that children spent in their mothers' care and as a result, decreased the use of other childcare arrangements. Hence, the impact of leave take-up on children's outcomes potentially depends on whether increased time with the mother is substituting for lower or higher quality childcare arrangements. In France, around 43.3% and 31.1% of children under the age of 3—who are not primarily cared for by their mother—are placed in informal care and with registered childminders, respectively. The rest are enrolled in nurseries (14.2%) or preschools (11.4%). Unfortunately, I cannot definitively determine which types of care maternal time is substituting for since I not have data on most childcare arrangements.

Recent studies could however help shed light on how maternal care is expected to impact child outcomes relative to other childcare arrangements. A large portion of French children (43.3%) who are not primarily cared for by their mothers are placed in informal care. Previous evidence suggests that care provided by mothers and nurseries is of higher quality than informal care (i.e., care provided mainly by grandparents and relatives). Specifically, Danzer et al. (2017) show that children residing in areas where no formal childcare arrangements are available, benefit from a one year extension of maternity leave in Austria. On the other hand, children are unaffected if they reside in areas where nurseries are available. However, maternal care can, in some instances, be of lower quality than informal care. For example, Danzer and Lavy (2018) find that the Austrian reform—which led to maternal care primarily displacing informal care—harmed boys of lower-educated women but benefited those with higher-educated mothers.

To provide evidence on this channel, I examine heterogeneity in children's outcomes by socioeconomic background. Indeed, if children's overall negative effects in my setting are entirely driven by those who are from a low socioeconomic background, this could imply that the main channel driving these effects is that maternal care is of lower quality than other forms of childcare. The children's dataset does not include information on parents' socioeconomic background or education. However, it does report whether children live in a "Zone d'Education Prioritaire" (or ZEP) or in a "Zone Urbaine Sensible" (ZUS). The ZEP and ZUS are disadvantaged areas that were designated by the French government as high-priority areas for receiving aid and funds in an effort to reduce socioeconomic inequalities.[27] While children living in these areas are typically from low

[27]In Figure A12a and the first row of Appendix Table A17, I show that the reform has no impact on the likelihood that second-born children reside in a ZEP/ZUS area, ruling out concerns over selection into ZEP-ZUS/non ZEP-ZUS samples. As an additional validity test of the RD design, I also find no threshold-crossing impacts on the likelihood that

socioeconomic backgrounds, it should be noted that a large share of disadvantaged children may not reside in these areas. The RD-DID estimates of the effects of the reform in Panel C of Table 7 show that children living in ZEP-ZUS areas are significantly and negatively affected by the reform as in the overall sample. I do not find statistically significant impacts on children residing outside these areas (Panel D). However, I cannot rule out negative effects that are comparable in magnitude to estimates from the overall sample, and p-values from the test of equality of coefficients do not indicate that estimates are significantly different between the ZEP and non-ZEP samples. This precludes me from making definitive conclusions regarding whether the reform harmed exclusively disadvantaged children.

Over half of French children aged less than 3 who are not primarily cared for by their mothers have formal childcare arrangements (i.e., nurseries, preschool or registered childminders). While I do not have data on most of these arrangements, I can nonetheless test whether leave take-up delays children from entering preschool. This is possible since mothers can take the leave until the child's third birthday and children can be enrolled in preschool as early as age 2. In Panel B and columns (8) and (9) of Table 7, I report RD-DID estimates of the effects of the reform on children's preschool-starting age (in months) and the number of months that children have been enrolled in preschool by the survey date. No statistically significant effects are detected for both outcomes in the overall sample. However, for children living in ZEP-ZUS areas (Panel C), the reform significantly delayed their preschool-starting age and decreased their time in preschool by 1.9 months. Taken together, these results indicate that crowding out of preschool can explain at least part of the documented negative impacts on children's outcomes. However, the absence of preschool effects for the overall sample and for children residing outside of ZEP-ZUS areas suggests that there are other channels driving these effects.

One factor that could contribute to the documented adverse effects is that the reform affected second-born children. A large body of work shows that higher birth order has negative impacts on a range of individual outcomes, and this is partly due to parents investing more time and resources in first-born children (Black et al., 2005; 2018). Using data from the American Time Use Survey, Price (2008) further documents that parents spend less "quality time" with second-born children relative to first-borns. In my setting, children are spending more time with their mothers and less time in other forms of childcare. It is possible that children were negatively affected not because maternal care in general is of lower quality than other types of childcare, but because mothers do not spend enough "quality time" with their second-borns. This may also explain why my results contrast with the rest of the literature which typically focuses on first-born children (see for example Danzer et al., 2017).

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the second child is male (Figure A12b and second row of Appendix Table A17) and the date of the verbal development test was administered (Figure A12c and third row of Appendix Table A17).

Second, the negative impact on children's verbal development can be driven by a reduction in their social interactions. Specifically, increased time with the mother likely crowds out other forms of childcare and as a result, potentially decreases the time that children spend with other adults and children. Although psychologists believe that it is important for children to bond with their mothers in the first year of life, older children could benefit more from interacting with other adults and children (Dustmann and Schönberg, 2012). Since mothers took up three years of leave, having more limited social interactions between ages 1 and 3 could be a main channel driving the adverse effects on children.

Third, given that men increase their work hours for three years as a result of their spouses' leave take-up, children could be spending less time with their fathers. While the evidence regarding paternal involvement is scarce, some studies show that increased time spent with fathers can have positive effects on children's development (El Nokali, Bachman and Votruba-Drzal, 2010), and raises the correlation between fathers and children's level of education (Kalil et al., 2016).

Fourth, given that the APE program provides partial income replacement, a potential loss of household income is expected to have negative effects on child achievement (Dahl and Lochner, 2012). Indeed, back-of-the-envelope calculations in section 5.1 indicate that on average, mothers who are barely eligible for the reform lost about €111, €118, €117, and €101 per month in years 1 to 4 respectively, compared to mothers who are barely ineligible.

Finally, since the APE increased intra-household specialization and potentially led to a loss of household income, it could have also affected marital stability.[28] This could in turn impact children's development as previous research suggests that they benefit from being in intact families (Burstein, 2007). To understand whether and how the reform affects couple stability, I use data on parents' marital outcomes from the "Enquête Etude de L'Histoire Familiale", a survey administered to individuals aged 18 years and above who were also part of the 1999 population census and which contains detailed information on family life. Further details on the survey, sample construction and summary statistics are discussed in Appendix B. The data allow me to look at marital outcomes for three samples: mothers who were married, cohabiting or single at the date of birth of their second child. I start by looking at the sample of mothers who were cohabiting but not married at the date of birth of their second child in Figures A13a and A13b and columns (1) and (2) of

[28]The direction of this effect is however ambiguous. In a standard Becker model, specialization is expected to reduce couple dissolution, as it increases the value of marriage relative to being single for both parents. This is because women are investing in marriage-specific human capital by reducing their labor supply. Furthermore, men gain in the labor market and increase their work involvement since mothers take on a higher share of household responsibilities (Becker, Landes and Michael, 1977). However, Becker et al. (1977) argue that large deviations between couples' expectations—at the time of couple formation—and realized outcomes can increase the risk of dissolution. In this context, the APE could threaten marital stability because it induced couples to drastically deviate from their initial division of labor, as previously employed mothers completely exited the labor force and fathers increased their work hours. Furthermore, a potential loss of household income has ambiguous effects on couples' well-being (Burstein, 2007).

Table A19. Results indicate that the probability of being in the same pre-childbirth relationship is unaffected. Figure A13b reveals a positive shift at the cutoff in the probability that cohabiting mothers are unmarried almost five year after the second child's birth. The corresponding estimate in column (2) of Table A19 is not statistically significant at conventional levels but the sample size is very small and I cannot rule out a large decrease in mothers' marriage rate.[29] In Figure A13c, I plot the likelihood of being in the same marriage for mothers who were married when their second child was born. The graph is smooth around the cutoff and no significant threshold-crossing effect is detected in column (3) of Table A19, indicating that the reform does not affect divorce rates.[30] Taken together, these results suggest that the leave did not significantly affect couple stability and that the latter is unlikely to explain the negative effects on children's development.[31]

In conclusion, there could be many factors contributing to the negative impacts on children's verbal development and it is beyond the scope of this paper to determine the exact channel driving these effects.

7 Conclusion

Currently, the United States is the only high-income country that does not have nationwide paid parental leave. This is in stark contrast to European countries which provide new parents with generous periods of benefits. In fact, between 2013 and 2015, the median duration of leave amongst developed countries was 60 weeks (Olivetti and Petrongolo, 2017). While a large body of literature documents significant gains from relatively short leaves, it is less clear how extended periods of benefits affect household behavior and child well-being. In this paper, I provide some of the first evidence that offering lengthy leaves can have detrimental effects on a range of family outcomes.

My focus is on a French gender-neutral leave program, which offered parents a fixed monthly cash benefit to take up to three years of time off from work after the birth of a child. Leave take-up was conditional on the parent either working part-time or exiting the labor force, with the latter op-

[29]Estimates across different bandwidths in Appendix Table A20—using local randomization and local linear regressions—indicate that there is a statistically significant drop in the probability of marriage among cohabiting mothers.

[30]As a robustness check, Figure A13d and column (4) of Table A19 show that mothers who are single at their second child's birth are not more likely to be married or cohabiting 5 years later. The lack of threshold-crossing effects is consistent with the fact that single mothers did not benefit from the APE program. Results for all marital outcomes are also robust to different bandwidths as shown in Appendix Table A20. Finally, the different panels in Appendix Figure A14 and Appendix Table A21 present placebo tests for the main marital outcomes using July 1, 1992 as a fake cutoff and the month-year of birth of the first child as a running variable. As expected, the figures are smooth around the cutoff and the corresponding estimates are statistically insignificant.

[31]The extension of parental leave can further impact children's development if it affects fertility or birth spacing. However, Piketty (2005) finds that the reform has no effect on fertility. In results available upon request, I also show that the reform has no significant effects on the number of children in the household, as well as birth spacing measured by the age difference between the first and second child, and the age difference between the second and third child, up to 7 years after the second child's birth.

tion yielding a greater amount of benefits. Upon its introduction, the leave was reserved for parents of three or more children. Benefits were then extended to parents whose second child was born or after July 1, 1994. To identify the causal effects of leave extension, I therefore use a regression discontinuity design based on this date of birth cutoff. My findings indicate that leave eligibility induces mothers to take up benefits by exiting the labor force. Fathers do not alter their leave-taking behavior but they are incentivized to provide more weekly hours of work. I also document that leave eligibility harms children's verbal development at ages 5 to 6.

Some of the main arguments for parental leave programs are that they can help narrow the gender gap in the labor market as well as foster child well-being. Thus, my results suggest that parental leave programs can work against their intended goals. Indeed, leave-induced specialization can play a key role in exacerbating gender inequalities in the labor market. Furthermore, the documented negative effect on child development is important in light of evidence that childhood circumstances can shape future outcomes and that early interventions can be critical for reducing initial inequalities (Cunha and Heckman, 2007; Almond and Currie, 2011). The extent to which these results can be generalized to other settings largely depends on the design of other parental leave programs. Nonetheless, my findings imply that extensive expansions in the duration of parental leaves can have significant negative consequences.

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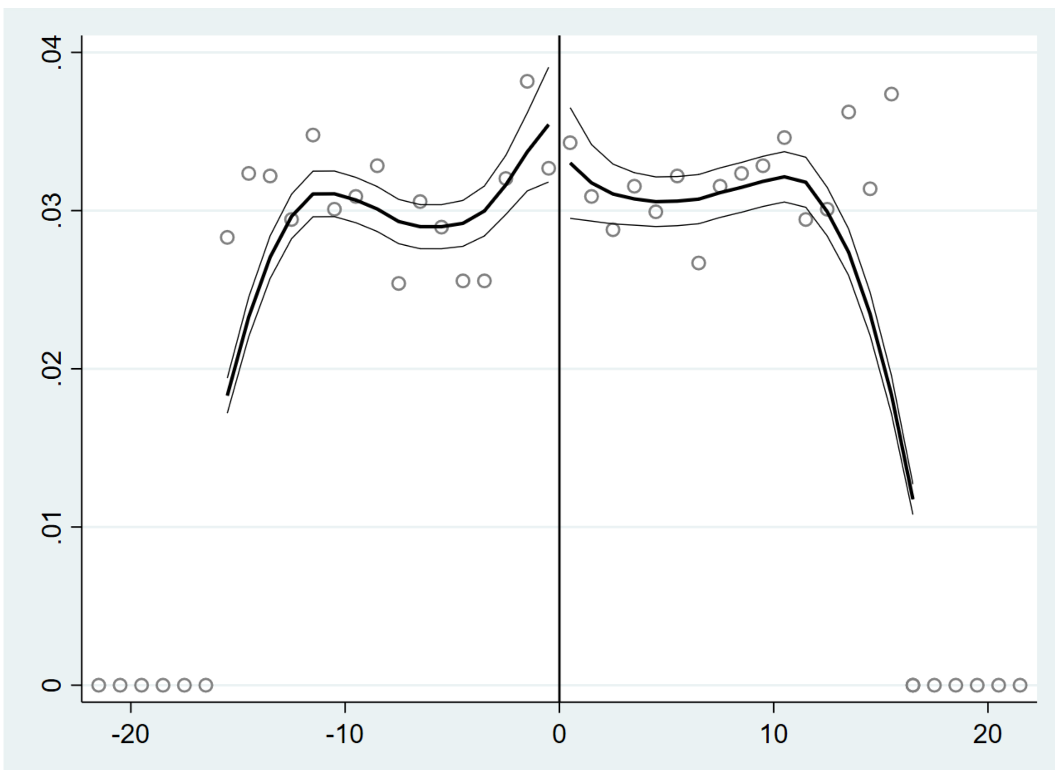
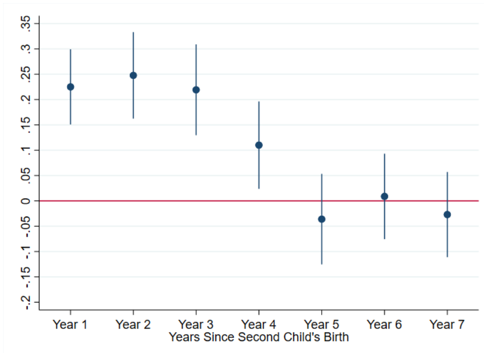
A Figures and Tables

Figure 1: McCrary density test

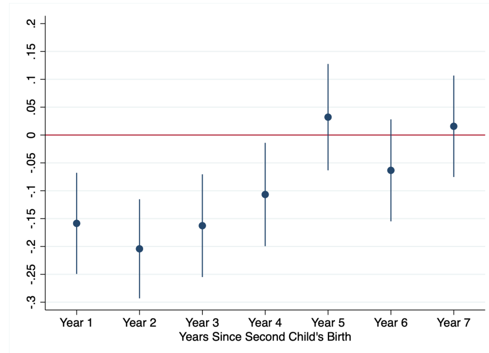


Notes: Each circle shows the average number of children born in each month-year. Lines represent the estimated density of the running variable and the corresponding 95% confidence intervals. Data are taken from the Labor Force Survey.

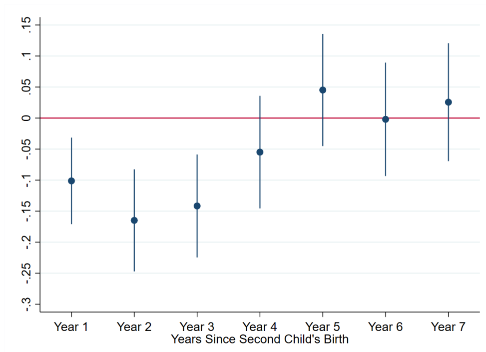
Figure 2: Effects of the reform on mothers' labor market outcomes by years since childbirth



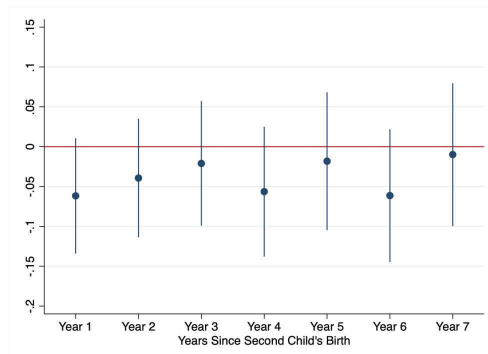
(a) Out of the labor force



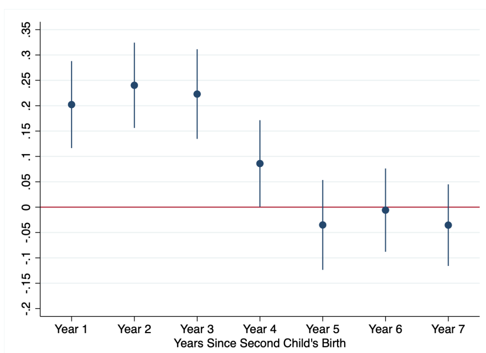
(b) Is employed



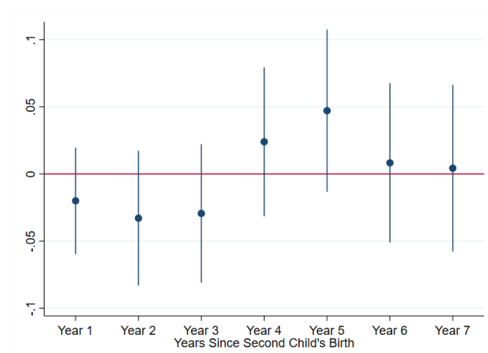
(c) Works full-time



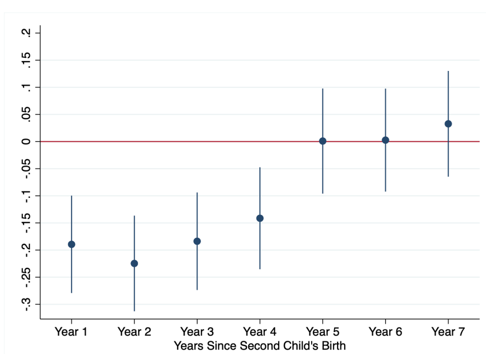
(d) Works part-time



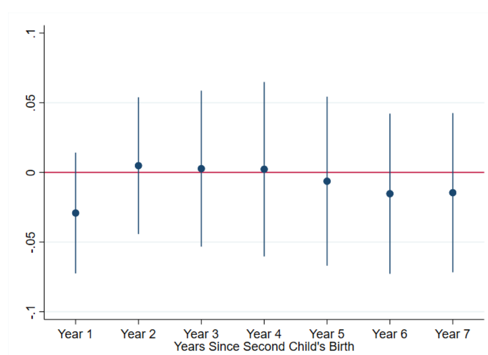
(e) Is stay-at-home mother



(f) Is in low-skilled occupation



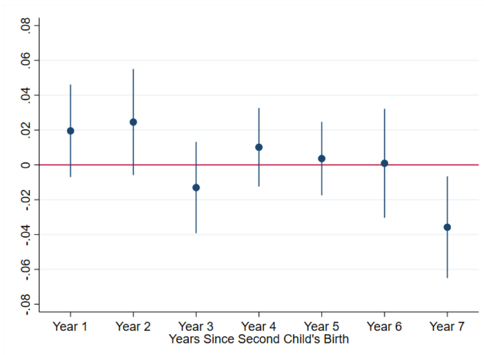
(g) Is in middle-skilled occupation



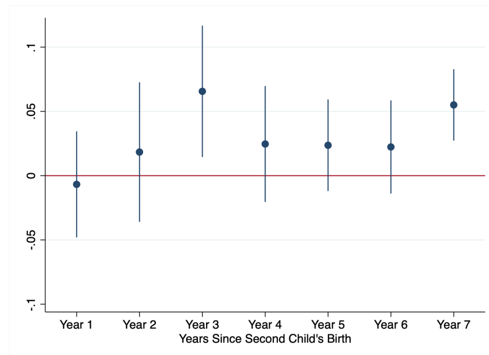
(h) Is in high-skilled occupation

Notes: The different panels plot RD estimates of the effect of the reform on mothers' labor market outcomes, along with their 95% confidence intervals, in each year since second child's birth. Estimates are taken from regressions using the local randomization approach and a bandwidth of 4 months on either side of the cutoff. Data are taken from the Labor Force Survey.

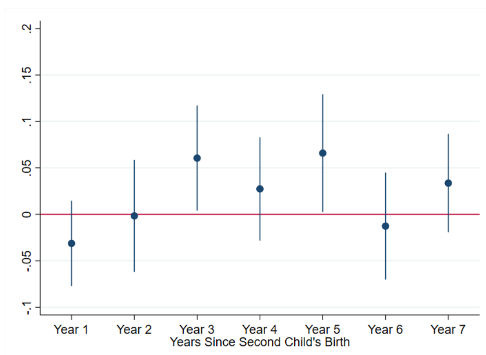
Figure 3: Effects of the reform on fathers' labor market outcomes by years since childbirth



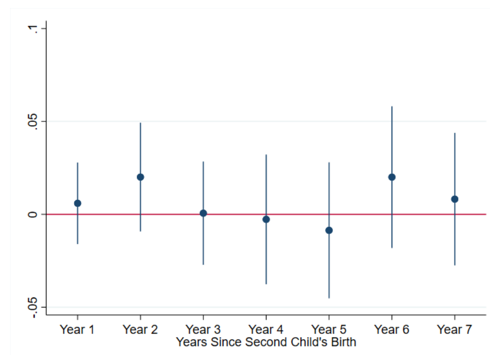
(a) Out of the labor force



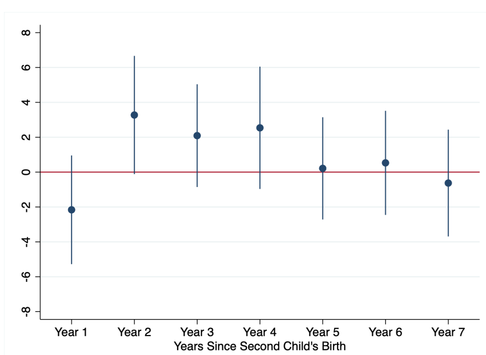
(b) Is employed



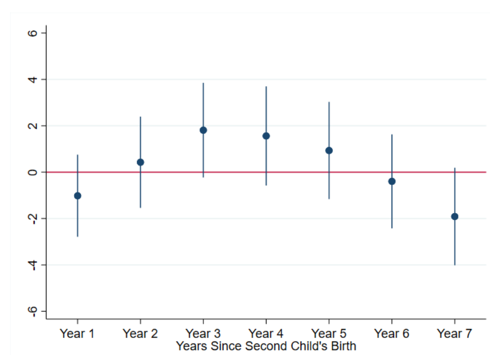
(c) Works full-time



(d) Works part-time



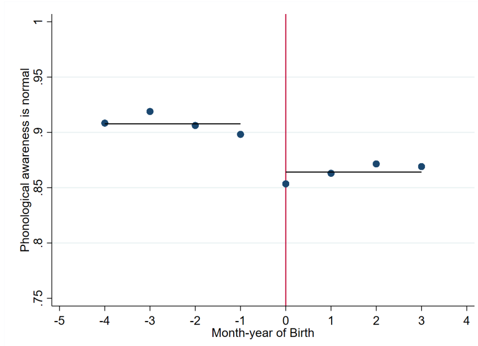
(e) Actual hours of work



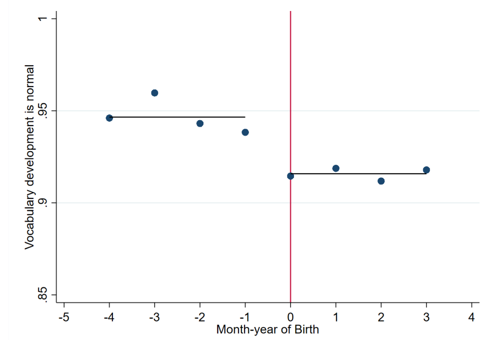
(f) Usual hours of work

Notes: The different panels plot RD estimates of the effect of the reform on fathers' labor market outcomes, along with their 95% confidence intervals, in each year since second child's birth. Estimates are taken from regressions using the local randomization approach and a bandwidth of 4 months on either side of the cutoff. Data are taken from the Labor Force Survey.

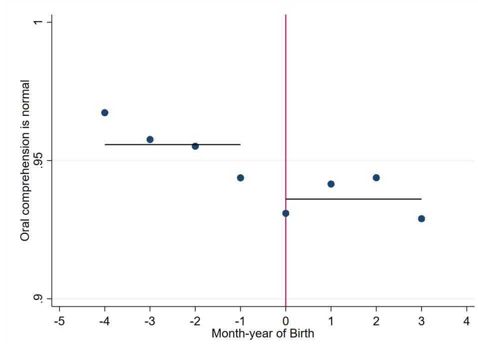
Figure 4: Effects of the reform on children's verbal development



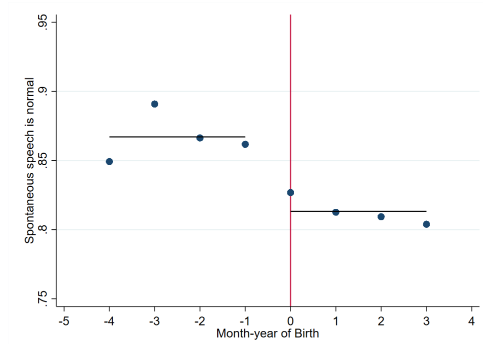
(a) Score on phonological awareness is normal



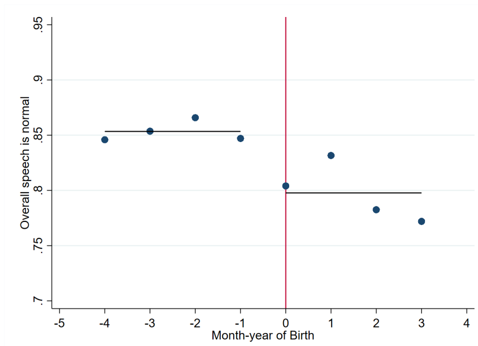
(b) Score on vocabulary development is normal



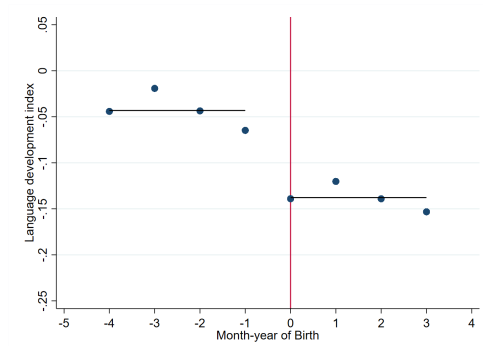
(c) Score on oral comprehension is normal



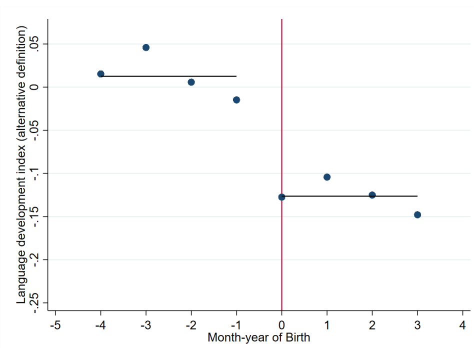
(d) Spontaneous speech is normal



(e) Overall speech is normal



(f) Verbal development index



(g) Alternative verbal development index

Notes: The different panels show second-born children's outcomes measured at ages 5-6, as a function of the distance of their month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Data are taken from the Enquête Santé en Milieu Scolaire.

Table 1: Sample means for parents' main labor market outcomes

	1st year after childbirth		2nd year after childbirth		3rd year after childbirth		4th year after childbirth	
	Before cutoff (1)	After cutoff (2)	Before cutoff (3)	After cutoff (4)	Before cutoff (5)	After cutoff (6)	Before cutoff (7)	After cutoff (8)
A) Mothers' outcomes								
Out of labor force	0.271	0.489	0.250	0.498	0.299	0.518	0.241	0.351
Employed	0.597	0.439	0.625	0.421	0.589	0.427	0.650	0.543
Works full-time	0.377	0.276	0.391	0.226	0.353	0.211	0.381	0.323
Works part-time	0.224	0.163	0.234	0.195	0.237	0.216	0.273	0.216
Is stay-at-home mother	0.246	0.448	0.226	0.466	0.268	0.491	0.241	0.327
In low-skilled occupation	0.085	0.068	0.101	0.068	0.098	0.069	0.082	0.106
In middle-skilled occupation	0.542	0.353	0.568	0.344	0.491	0.307	0.550	0.409
In high-skilled occupation	0.097	0.081	0.076	0.081	0.098	0.101	0.123	0.125
N	236	221	248	221	224	218	220	208
B) Fathers' outcomes								
Out of labor force	0.017	0.036	0.016	0 041	0.027	0.014	0.009	0.019
Employed	0.924	0.914	0.891	0.909	0.884	0.949	0.927	0.952
Works full-time	0.915	0.891	0.875	0.873	0.866	0.926	0.891	0.918
Works part-time	0.013	0.023	0.016	0.036	0.022	0.023	0.036	0.034
N	236	221	248	221	224	218	220	208
Actual hours of work	39.77	37.62	36.15	39.43	40.21	42.30	38.89	41.43
N	219	200	220	200	198	207	202	197
Usual hours of work	42.37	42.20	41.77	42.20	41.64	43.45	41.77	43.34
N	192	164	191	168	162	170	172	161

Note: This table reports means for parents' main labor market outcomes. The different columns show outcomes' means for parents whose second child is born within 4 months before and after the cutoff, and in the first through fourth years after the second child's birth. Data are taken from the Labor Force Survey.

Table 2: Sample means for demographic characteristics and children's outcomes

	Before cutoff (1)	After cutoff (2)
A) Socioeconomic characteristics in 1994		
Second child is male	0.546	0.520
Mother age at childbirth	29.46	29.69
Father age at childbirth	32.05	32.16
Mother born in France	0.891	0.886
Father born in France	0.900	0.878
Mother high school degree or more	0.434	0.404
Father high school degree or more	0.347	0.321
Mother's years of education	9.678	9.906
Father's years of education	9.524	9.682
Mother's father is manual worker	0.396	0.399
Father's father is manual worker	0.361	0.376
Mother's father is high-skilled	0.110	0.089
Father's father is high-skilled	0.131	0.115
N	790	778
B) Children's Outcomes in 1999		
Child has		
Normal score on phonological awareness	0.908	0.864
N	2,168	2,127
Normal score on vocabulary development	0.947	0.916
N	2,171	2,126
Normal score on oral comprehension	0.956	0.936
N	2,170	2,127
Normal score on spontaneous speech	0.867	0.813
N	2,257	2,196
Normal score on overall speech	0.853	0.798
N	2,374	2,304

Note: This table reports means for key variables for individuals who are within 4 months before and after the cutoff. Data on demographic characteristics are taken from the Labor Force Survey. In the main analysis sample, individuals are repeated for as many time as they are observed in the data. Means for demographic characteristics are instead based on a sample in which each individual is observed once—the last time he/she appears in the data. Data on children's outcomes are taken from the Enquête Santé en Milieu Scolaire.

Table 3: Effects of the reform on mothers' labor market outcomes, RD estimates

	Out of the labor force (1)	Is employed (2)	Works full-time (3)	Works part-time (4)	Is stay-at-home mother (5)	In low-skilled occupation (6)	In middle-skilled occupation (7)	In high-skilled occupation (8)
A) During Leave								
RD estimate	0.229***	-0.176***	-0.136***	-0.041	0.222***	-0.026	-0.200***	-0.003
	(0.033)	(0.034)	(0.032)	(0.028)	(0.032)	(0.019)	(0.034)	(0.020)
	[0.000]***	[0.000]***	[0.000]***	[0.437]	[0.000]***	[0.437]	[0.000]***	[0.947]
N	1,368	1,368	1,368	1,368	1,368	1,368	1,368	1,368
B) After Leave Expires								
RD estimate	0.015	-0.032	0.002	-0.036	0.003	0.021	-0.028	-0.009
	(0.028)	(0.031)	(0.031)	(0.028)	(0.028)	(0.020)	(0.032)	(0.021)
	[0.742]	[0.544]	[0.947]	[0.444]	[0.947]	[0.544]	[0.610]	[0.805]
N	1,672	1,672	1,672	1,672	1,672	1,672	1,672	1,672

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. All estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Estimates in Panel A are from the first through third years after the second child's birth. Estimates in Panel B are from the fourth through seventh years after the second child's birth. Standard errors are clustered by mothers' ID and are reported in parentheses. Q-values or p-values adjusted for multiple inference using the False Discovery Rate method (Benjamini and Hochberg, 1995) are reported in brackets (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table 4: Effects of the reform on fathers' labor market outcomes, RD estimates

	Out of the labor force (1)	Is Employed (2)	Works full-time (3)	Works part-time (4)	Actual hours of work (5)	Usual hours of work (6)
A) During Leave						
Year 1						
RD estimate	0.019	-0.010	-0.024	0.010	-2.161	-1.009
	(0.015)	(0.026)	(0.028)	(0.012)	(1.589)	(0.905)
	[0.444]	[0.805]	[0.610]	[0.629]	[0.437]	[0.518]
N	457	457	457	457	419	356
Years 2-3						
RD estimate	0.006	0.042*	0.029	0.011	2.815**	1.117
	(0.011)	(0.022)	(0.024)	(0.011)	(1.222)	(0.827)
	[0.742]	[0.226]	[0.469]	[0.582]	[0.116]	[0.437]
N	911	911	911	911	825	691
B) After Leave Expires						
RD estimate	-0.005	0.032**	0.028	0.004	0.707	-0.062
	(0.008)	(0.014)	(0.018)	(0.012)	(0.947)	(0.672)
	[0.737]	[0.116]	[0.437]	[0.805]	[0.649]	[0.947]
N	1,672	1,672	1,672	1,672	1,560	1,306

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. All estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Estimates in Panel A are from the first through third years after the second child's birth. Estimates in Panel B are from the fourth through seventh years after the second child's birth. Data are taken from the Labor Force Survey. For year 1, robust standard errors are reported in parentheses. For stacked years 2-3 and 4-7, standard errors are clustered by fathers' ID and are reported in parentheses. *Q*-values or *p*-values adjusted for multiple inference using the False Discovery Rate method (Benjamini and Hochberg, 1995) are reported in brackets. (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table 5: Effects of the reform on mothers' labor market effect by level of education, RD estimates

	Out of the labor force (1)	Is employed (2)	Works full-time (3)	Works part-time (4)	Is stay-at-home mother (5)	In low-skilled occupation (6)	In middle-skilled occupation (7)	In high-skilled occupation (8)
A) During leave								
Mother more than high school	0.111**	-0.056	-0.097*	0.040	0.099**	0.001	-0.128**	0.010
	(0.046)	(0.050)	(0.052)	(0.046)	(0.045)	(0.017)	(0.053)	(0.044)
N	572	572	572	572	572	572	572	572
Mother less than high school	0.294***	-0.240***	-0.152***	-0.091***	0.295***	-0.055*	-0.239***	0.002
	(0.042)	(0.043)	(0.038)	(0.034)	(0.042)	(0.031)	(0.042)	(0.012)
N	796	796	796	796	796	796	796	796
p-value	[0.004]	[0.006]	[0.392]	[0.022]	[0.002]	[0.109]	[0.101]	[0.858]
B) After leave expires								
Mother more than high school	0.026	-0.035	0.008	-0.043	-0.013	0.003	-0.017	-0.015
	(0.039)	(0.044)	(0.049)	(0.047)	(0.038)	(0.017)	(0.051)	(0.045)
N	682	682	682	682	682	682	682	682
Mother less than high school	0.005	-0.026	-0.000	-0.030	0.012	0.031	-0.033	-0.002
	(0.039)	(0.042)	(0.039)	(0.034)	(0.038)	(0.032)	(0.042)	(0.012)
N	990	990	990	990	990	990	990	990
p-value	[0.004]	[0.006]	[0.392]	[0.022]	[0.002]	[0.109]	[0.101]	[0.858]

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. All estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Estimates in Panel A are from the first through third years after the second child's birth. Estimates in Panel B are from the fourth through seventh years after the second child's birth. Estimates are reported separately for the sample of mothers who have a high school degree or more and the sample of mothers with less than a high school degree. P-values from tests of equality of coefficients from these two samples are reported in brackets. Data are taken from the Labor Force Survey. Standard errors are clustered by mothers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table 6: Effects of the reform on fathers' labor market outcomes by mothers' level of education, RD estimates

	Out of the labor force (1)	Is Employed (2)	Works full-time (3)	Works part-time (4)	Actual hours of work (5)	Usual hours of work (6)
A) During Leave						
Year 1						
Mother more than high school	0.004	0.011	-0.045	0.056**	-0.809	-2.501
	(0.021)	(0.031)	(0.039)	(0.025)	(2.561)	(1.592)
N	196	196	196	196	186	149
Mother less than high school	0.030	-0.021	-0.005	-0.023*	-3.257	0.001
	(0.021)	(0.038)	(0.039)	(0.013)	(2.003)	(1.087)
N	261	261	261	261	233	207
p-value	[0.376]	[0.519]	[0.470]	[0.005]	[0.452]	[0.194]
Years 2-3						
Mother more than high school	-0.011	0.026	0.011	0.015	4.557**	3.446**
	(0.018)	(0.027)	(0.032)	(0.018)	(1.975)	(1.376)
N	372	372	372	372	352	285
Mother less than high school	0.018	0.057*	0.046	0.007	1.641	-0.242
	(0.014)	(0.032)	(0.034)	(0.015)	(1.565)	(1.043)
N	535	535	535	535	473	406
p-value	[0.186]	[0.450]	[0.449]	[0.738]	[0.247]	[0.033]
B) After Leave Expires						
Mother more than high school	-0.009	0.020	0.020	0.000	1.779	0.360
	(0.012)	(0.018)	(0.027)	(0.019)	(1.505)	(1.178)
N	682	682	682	682	646	510
Mother less than high school	-0.003	0.041**	0.034	0.007	0.012	-0.321
	(0.011)	(0.020)	(0.025)	(0.015)	(1.208)	(0.801)
N	990	990	990	990	914	796
p-value	[0.717]	[0.442]	[0.696]	[0.791]	[0.359]	[0.632]

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. All estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Estimates in Panel A are from the first through third years after the second child's birth. Estimates in Panel B are from the fourth through seventh years after the second child's birth. Estimates are reported separately for the sample of fathers whose spouses a high school degree or more and the sample of fathers whose spouse have less than a high school degree. P-values from tests of equality of coefficients from these two samples are reported in brackets. Data are taken from the Labor Force Survey. Standard errors are clustered by fathers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table 7: Effects of the reform on children's outcomes

	Phonological Awareness (1)	Vocabulary Development (2)	Oral Comprehension (3)	Spontaneous Speech (4)	Overall Speech (5)	Verbal Dev. Index (6)	Alternative Verb. Dev. Index (7)	Age at beginning of preschool (8)	Time in preschool (9)
A) Overall sample									
RD estimate	-0.044***	-0.031***	-0.020***	-0.054***	-0.056***	-0.095***	-0.140***	-1.933***	-2.007***
	(0.010)	(0.008)	(0.007)	(0.011)	(0.011)	(0.014)	(0.021)	(0.154)	(0.161)
	{0.000}***	{0.000}***	{0.008}***	{0.000}***	{0.000}***	{0.000}***	{0.000}***	{0.000}***	{0.000}***
N	4,295	4,297	4,297	4,453	4,678	6,413	4,151	6,054	6,054
B) Overall sample									
RD-DID estimate	-0.031	-0.031*	0.003	-0.042*	-0.053**	-0.065**	-0.091**	0.063	-0.072
	(0.022)	(0.017)	(0.015)	(0.024)	(0.024)	(0.029)	(0.044)	(0.350)	(0.364)
	{0.200}	{0.088}*	{0.856}	{0.098}*	{0.041}**	{0.040}**	{0.056}*	{0.856}	{0.856}
N	13,830	13,848	13,861	14,497	15,263	20,878	13,411	19,822	19,822
C) In ZEP									
RD-DID estimate	-0.103	-0.116**	0.031	-0.104	-0.120**	-0.187**	-0.211*	1.902*	-2.185**
	(0.064)	(0.052)	(0.049)	(0.070)	(0.060)	(0.086)	(0.128)	(0.983)	(1.014)
N	2,161	2,157	2,153	2,232	2,408	3,198	2,058	3,047	3,047
D) Not in ZEP									
RD-DID estimate	-0.018	-0.015	-0.002	-0.030	-0.040	-0.046	-0.069	-0.272	0.314
	(0.023)	(0.017)	(0.015)	(0.025)	(0.026)	(0.030)	(0.046)	(0.372)	(0.388)
N	11,669	11,691	11,708	12,265	12,855	17,680	11,353	16,775	16,775
p-value	[0.208]	[0.064]	[0.511]	[0.322]	[0.226]	[0.122]	[0.294]	[0.039]	[0.021]

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates in Panel A are from a local randomization RD specification with a bandwidth of 4 months. Estimates in Panels B, C and D are from a difference-in-discontinuity regression using data within 6 months on either side of the cutoff. Panels A and B are for the overall sample. Panels C and D respectively restrict the overall sample to children residing in ZEP or ZUS and to children not residing in these areas. The varying number of observations is due to missing data. Data are taken from the Enquête Santé en Milieu Scolaire. Robust standard errors are reported in parentheses. *Q*-values or *p*-values adjusted for multiple inference using the False Discovery Rate method (Benjamini and Hochberg, 1995) are reported in curly brackets. P-values from tests of equality of coefficients from the samples of children living in and outside ZEP areas are reported in brackets (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

B Appendix: Data on Marital Outcomes

Data on parents' marital outcomes are taken from the "Enquête Etude de L'Histoire Familiale", which contains detailed information on family life. The survey is administered to individuals aged 18 years and above, who are also part of the 1999 population census. Within each household, either all men or all women are surveyed. The initial dataset includes 145,000 men and 235,000 women, with sampling rates of 1/170 and 1/110 respectively. Individuals are asked about their children's birth order and month and year of birth. I limit my sample to all women aged 18-64 who report having at least two children.

An advantage of this dataset is that it includes the date of beginning and end of the first and last cohabitation (marriage).[32] This allows me to look at marital responses for three different samples: mothers who were cohabiting but unmarried, mothers who were married and mothers who were neither cohabiting nor married at the date of birth of their second child. One caveat of the data is that the date of couple formation/dissolution is missing for some individuals. In that case, I cannot distinguish between those who did not start/end a relationship and those who did but did not report that information. For the cohabiting (married) sample, I focus on mothers whose cohabitation (marriage) started prior to the birth of their second child, and who either separated (divorced) after the child's birth or did not report the date of couple dissolution. I further restrict the sample to women who are in their first union and drop those who report being in a second cohabitation (marriage) prior to their second child's birth. This excludes around 6% of mothers in my main sample.

Table A18 presents marital outcomes' means for mothers of children born before and after the cutoff in columns (1) and (2), respectively. Marital outcomes are reported in 1999, around 4-5 years after the birth of the second child. For cohabiting mothers of children born before the cutoff, 87% are still in the same relationship, while 52.9% report not having been married. When looking at mothers of children born after the cutoff, 86.5% of cohabiting mothers are still in the same relationship and 60% are unmarried. 93% (94%) of mothers who were married and whose children were born before (after) the cutoff are still in the same relationship, while 64% (73%) of single mothers are now either married or cohabiting.

[32] A union is considered a cohabitation if parents co-reside for at least 6 months.

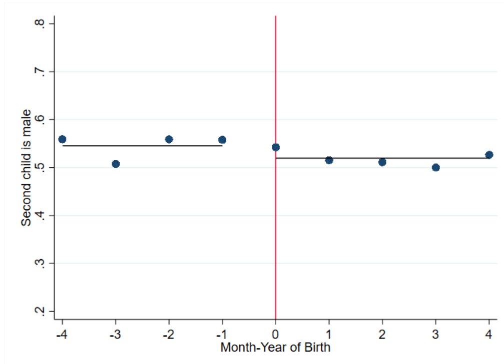
C Appendix: Alternative RD specifications

As discussed in section 4.1, an alternative way of conducting the RD analysis is to use the continuity-based approach. I present RD estimates using this approach in Appendix Tables (i) A1 and A2 for baseline covariates, (ii) A3 to A7 for parents' labor market outcomes, (iii) A15 for children's verbal development and, (iv) A20 for marital outcomes. For most outcomes, the preferred bandwidth for the continuity-based approach is 16 months, but I also provide estimates using bandwidths of 19, 22, 25 and 28 months. For children's outcomes, I only have data within 6 months on either sides of the cutoff. As a result, I use the bandwidth of 6 months to provide continuity-based estimates. As additional robustness checks, these tables also provide estimates from a local randomization approach using windows that are different than the preferred window of 4 months (specifically 2 and 6 months). They also show estimates from regressions which include baseline covariates for both the local randomization and the continuity-based approaches (and across all different bandwidths). As expected, all the paper's main results (i.e., taken from a local randomization approach with a window of 4 months) are robust to these alternative specifications.

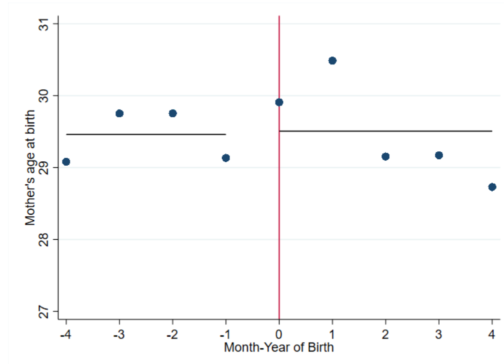
In the continuity-based approach, the preferred bandwidth of 16 months was chosen using the robust data-driven procedure introduced by Calonico, Cattaneo and Titiunik (2014). For a chosen polynomial order and kernel function, this procedure essentially minimizes the mean squared error (MSE) of the local polynomial RD point estimator, and as a result, picks the optimal bandwidth that balances the bias-variance tradeoff. As recommended by Cattaneo et al. (2019) and to obtain an MSE-optimal bandwidth, I use a triangular kernel function which assigns more weight to observations near the cutoff. In results available upon request, I find that my estimates are robust to using a uniform kernel function which assigns equal weights to all observations. Finally, I use a local linear RD estimator, as higher-order polynomials tend to overfit the data, while a polynomial of degree 0 has undesirable theoretical properties at boundary points (Cattaneo et al., 2019; Gelman and Imbens, 2019).

D Appendix Figures and Tables

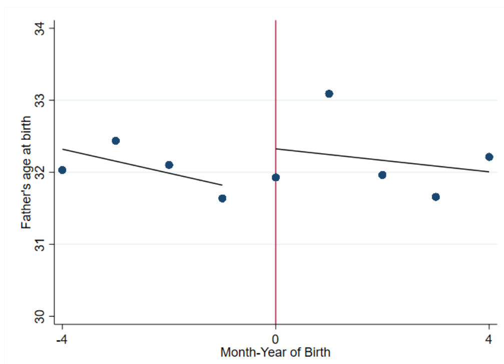
Figure A1: Smoothness of baseline covariates, Labor Force Survey



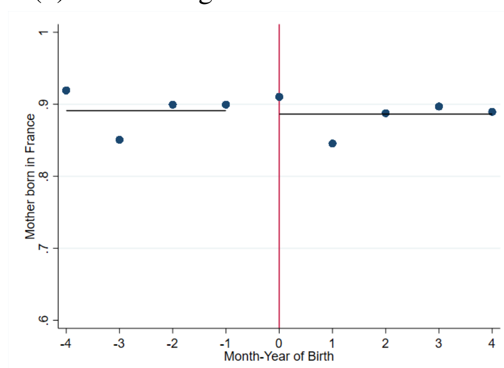
(a) Second child is male



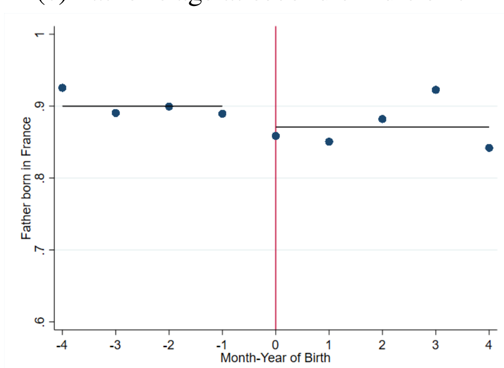
(b) Mother's age at second child's birth



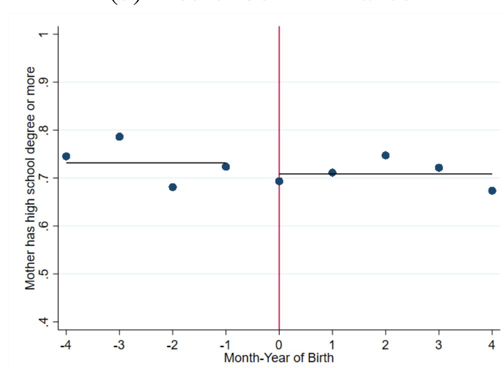
(c) Father's age at second child's birth



(d) Mother born in France



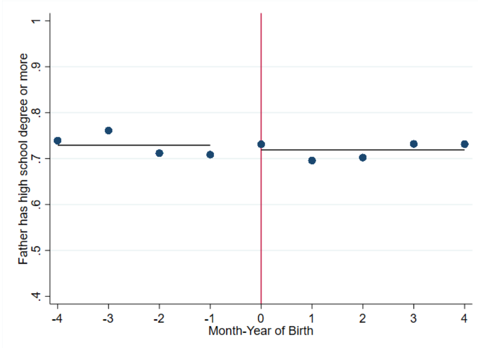
(e) Father born in France



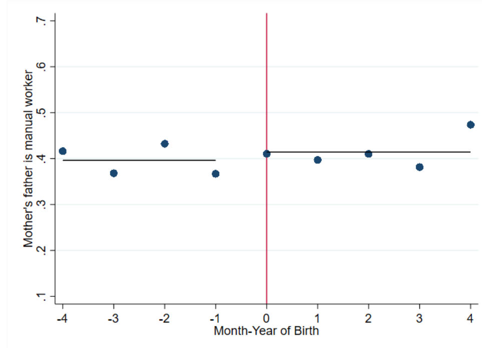
(f) Mother has high school or more

Notes: The different panels show various baseline covariates, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Data are taken from the Labor Force Survey.

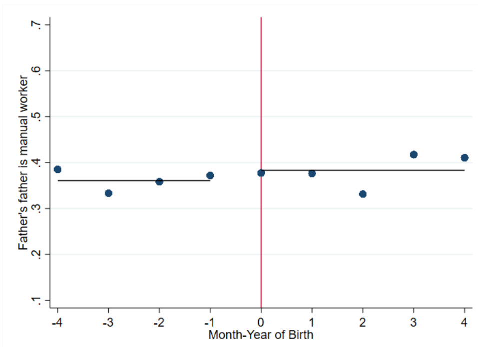
Figure A2: Smoothness of baseline covariates, Labor Force Survey (continued)



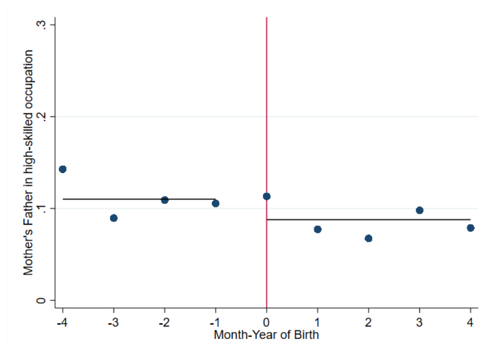
(a) Father has high school or more



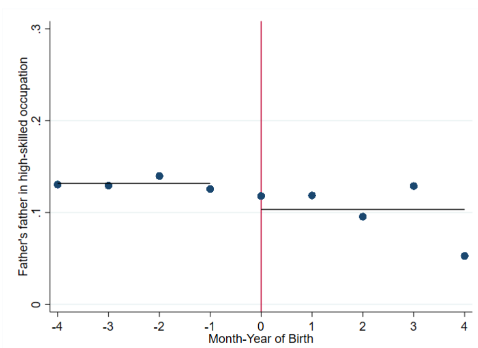
(b) Mother's father is manual worker



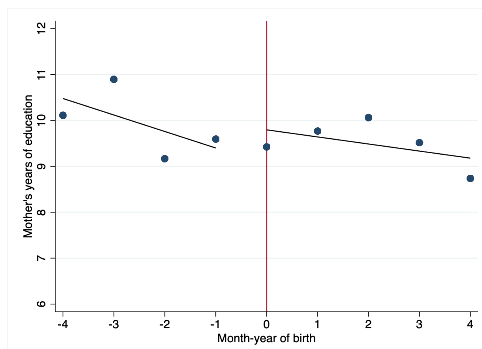
(c) Father's father is manual worker



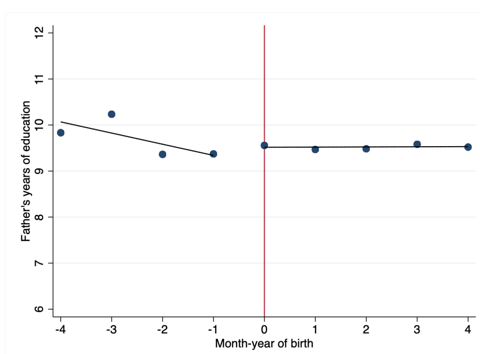
(d) Mother's father is high-skilled



(e) Father's father is high-skilled



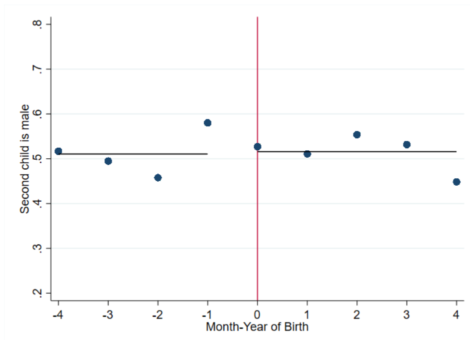
(f) Mother's years of education



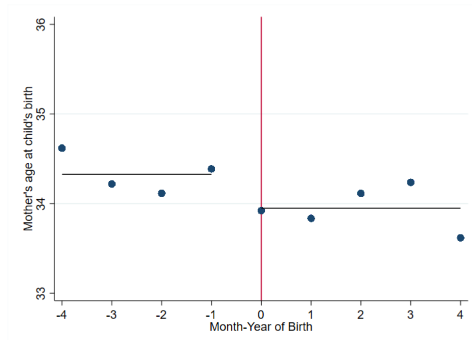
(g) Father's years of education

Notes: The different panels show various baseline covariates, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Data are taken from the Labor Force Survey.

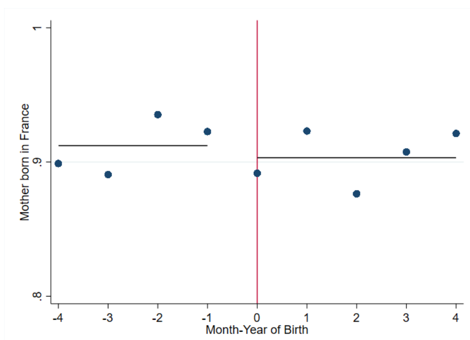
Figure A3: Smoothness of baseline covariates, Enquête Etude de L'Histoire Familiale



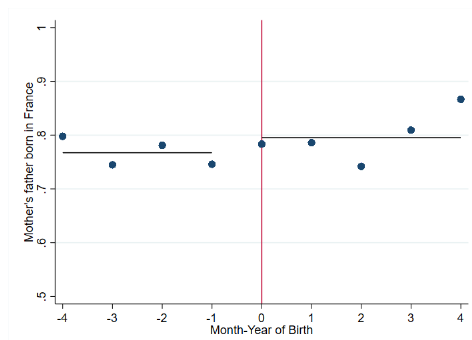
(a) Second child is male



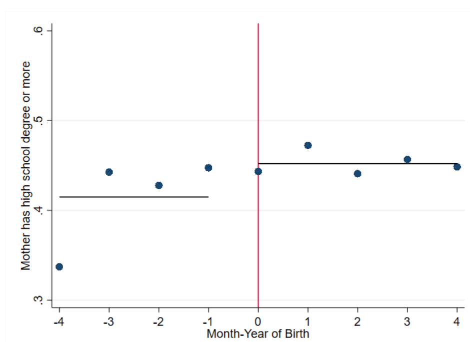
(b) Mother's age at second child's birth



(c) Mother is born in France



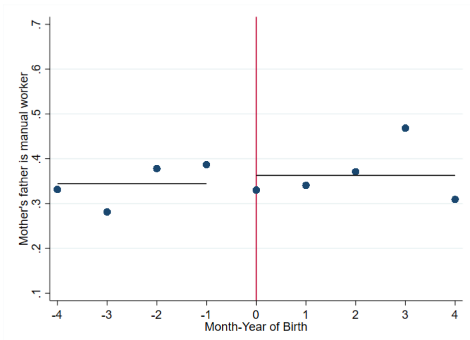
(d) Mother's father is born in France



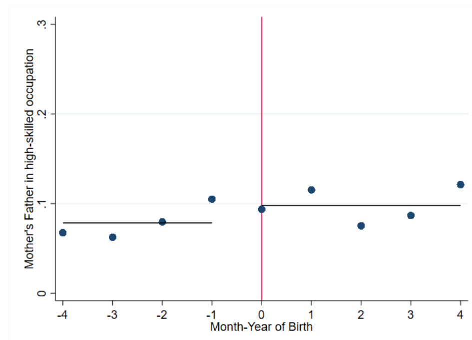
(e) Mother has high school or more

Notes: The different panels show various baseline covariates, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Data are taken from the Enquête Etude de L'Histoire Familiale.

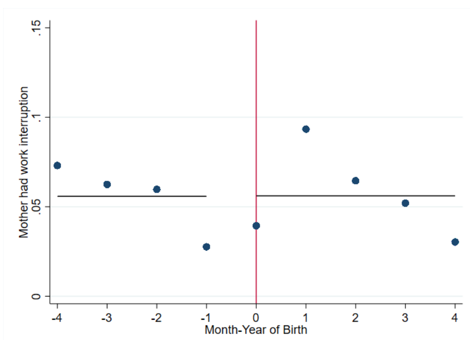
Figure A4: Smoothness of baseline covariates (continued), Enquête Etude de L'Histoire Familiale



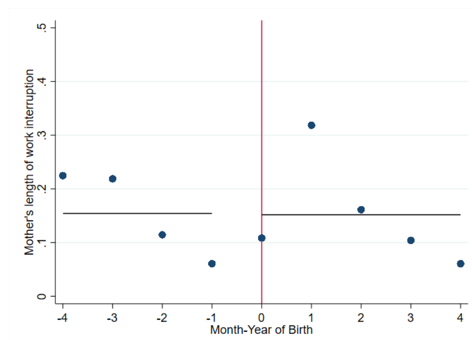
(a) Mother's father is manual worker



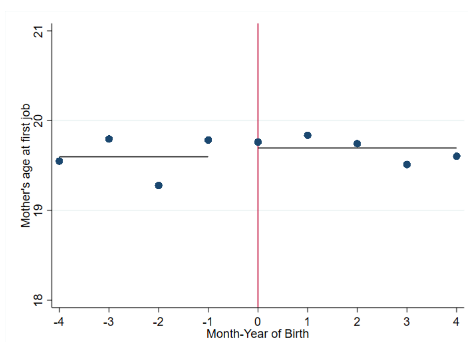
(b) Mother's father is high-skilled



(c) Mother had work interruption



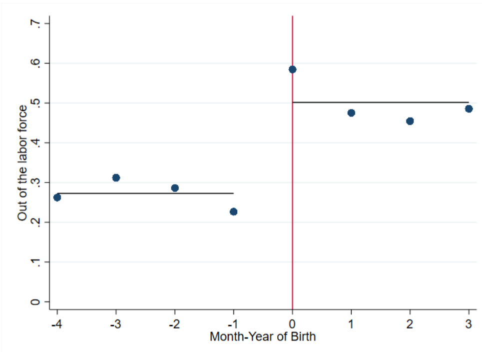
(d) Mother's length of work interruption



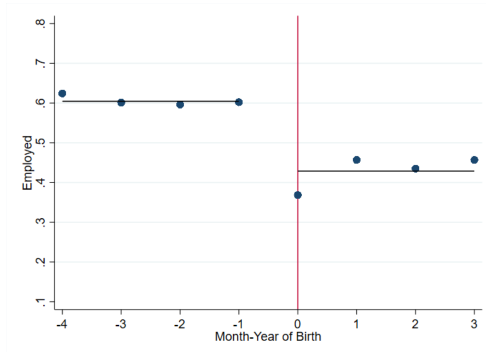
(e) Mother's age at first job

Notes: The different panels show various baseline covariates, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Data are taken from the Enquête Etude de L'Histoire Familiale.

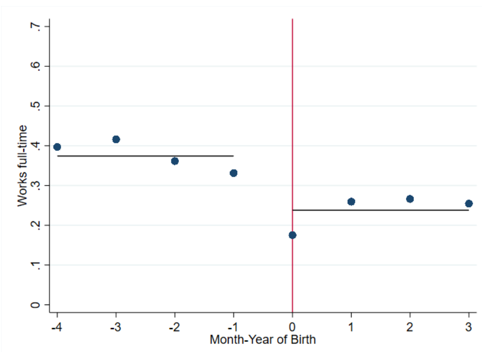
Figure A5: Effects of the reform on mothers' labor market outcomes during leave eligibility



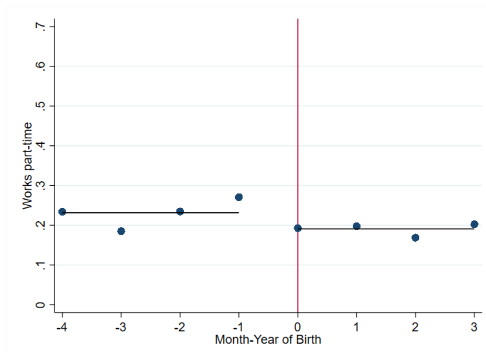
(a) Out of the labor force



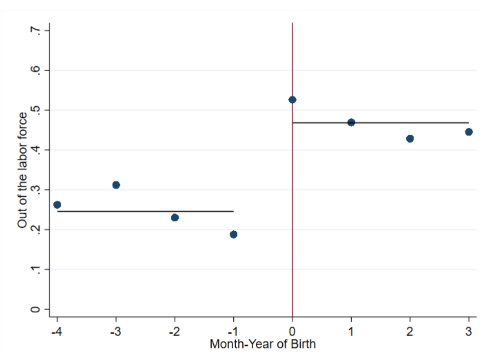
(b) Is employed



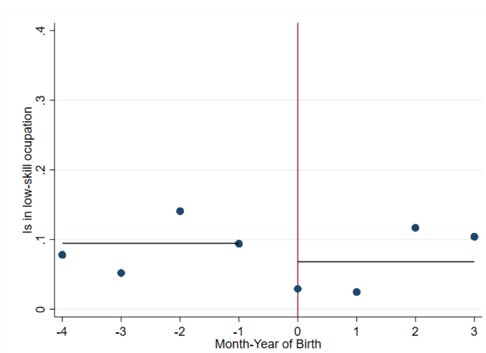
(c) Works full-time



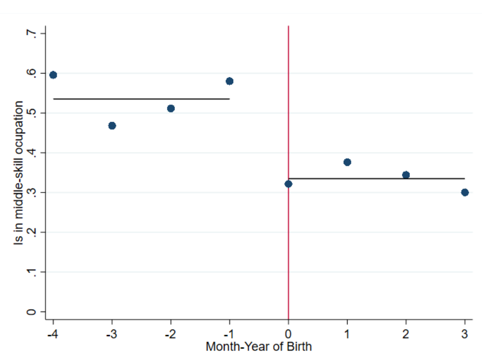
(d) Works part-time



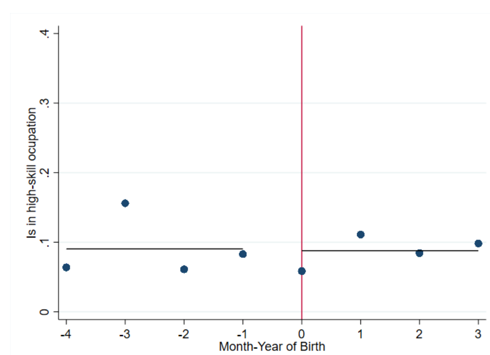
(e) Is stay-at-home mother



(f) Is in low-skilled occupation



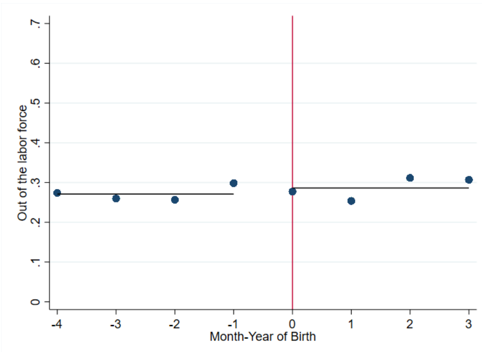
(g) Is in middle-skilled occupation



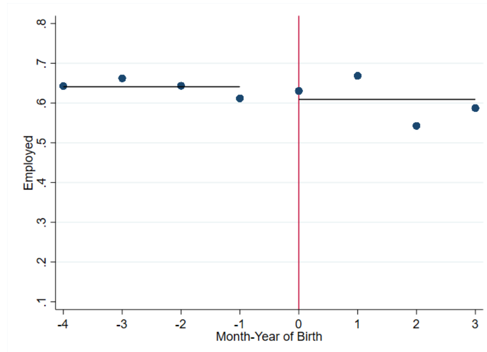
(h) Is in high-skilled occupation

Notes: The different panels show mothers' labor market outcomes in the first through third years after a second child's birth, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization regressions with a bandwidth of 4 months. Data are taken from the Labor Force Survey.

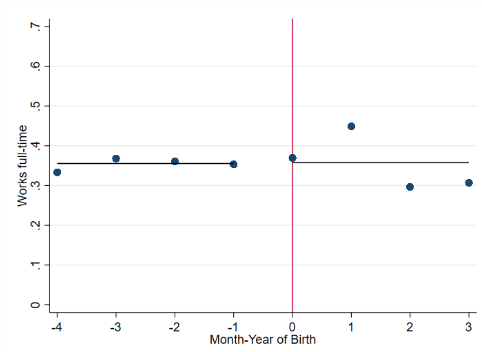
Figure A6: Effects of the reform on mothers' labor market outcomes after leave eligibility expires



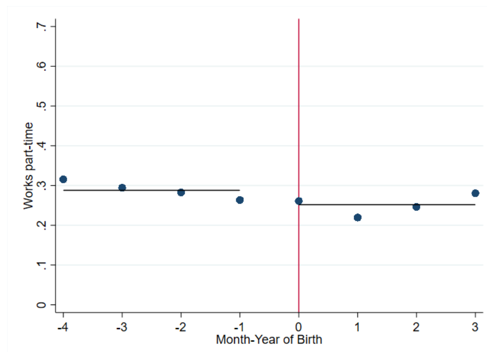
(a) Out of the labor force



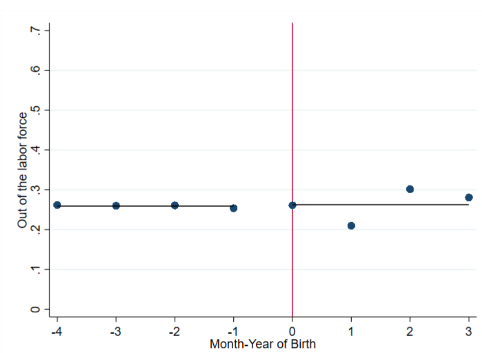
(b) Is employed



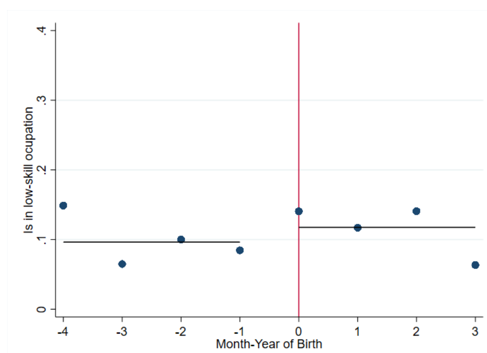
(c) Works full-time



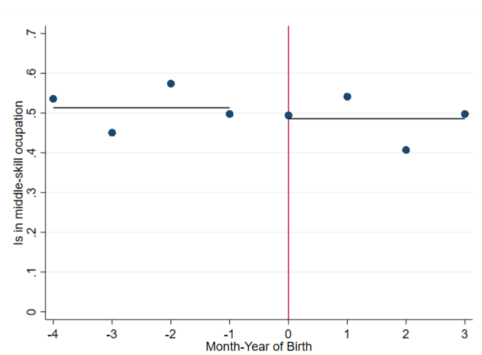
(d) Works part-time



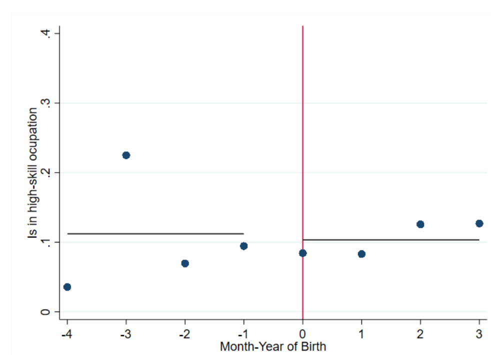
(e) Is stay-at-home mother



(f) Is in low-skilled occupation



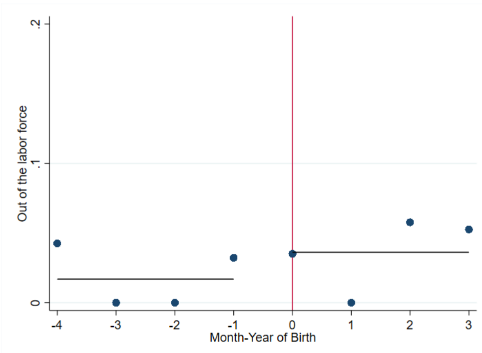
(g) Is in middle-skilled occupation



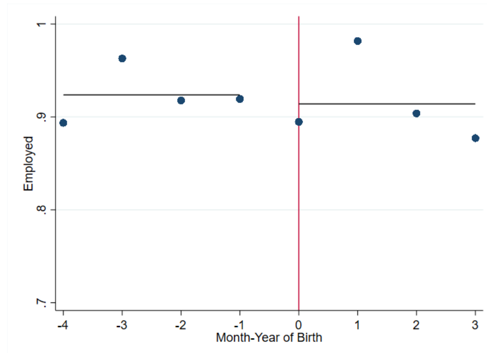
(h) Is in high-skilled occupation

Notes: The different panels show mothers' labor market outcomes in the fourth through seventh years after a second child's birth, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization regressions with a bandwidth of 4 months. Data are taken from the Labor Force Survey.

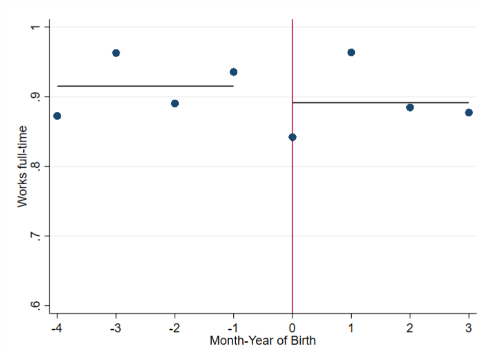
Figure A7: Effects of the reform on fathers' labor market outcomes during leave eligibility (Year 1)



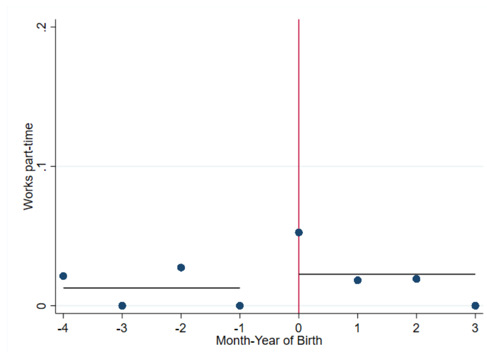
(a) Out of the labor force



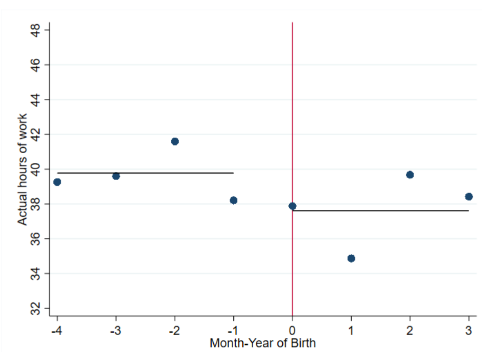
(b) Is employed



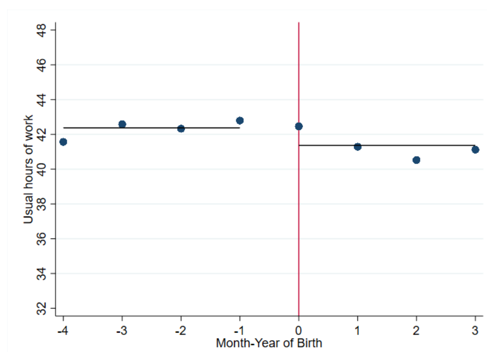
(c) Works full-time



(d) Works part-time



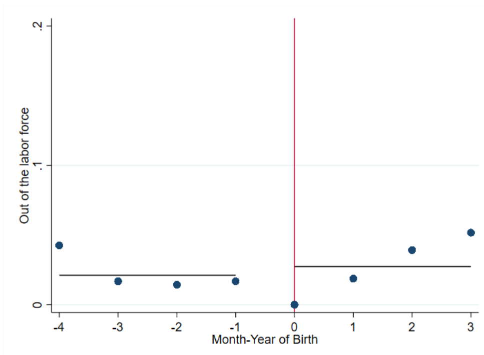
(e) Actual hours of work



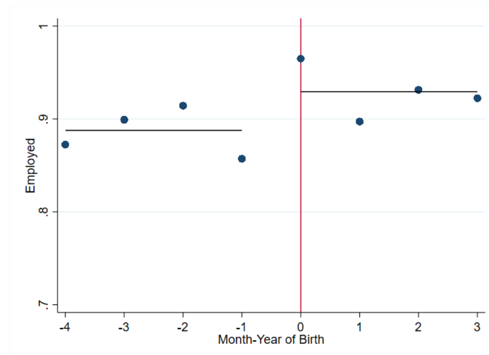
(f) Usual hours of work

Notes: The different panels show fathers' labor market outcomes in the first year after a second child's birth, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization regressions with a bandwidth of 4 months. Data are taken from the Labor Force Survey.

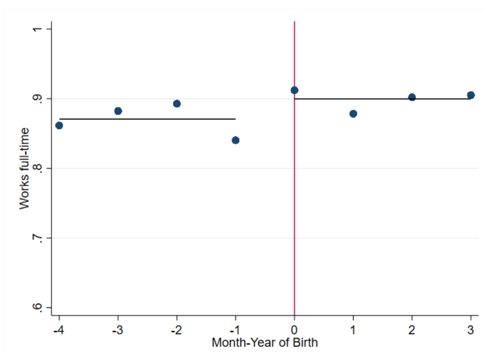
Figure A8: Effects of the reform on fathers' labor market outcomes during leave eligibility (Years 2 and 3)



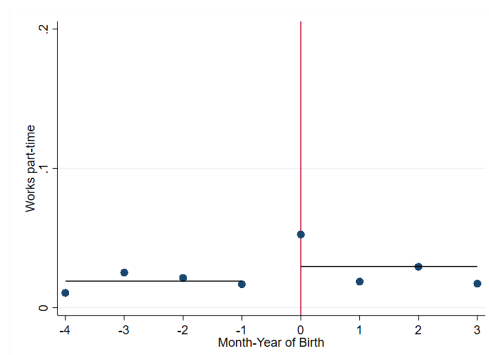
(a) Out of the labor force



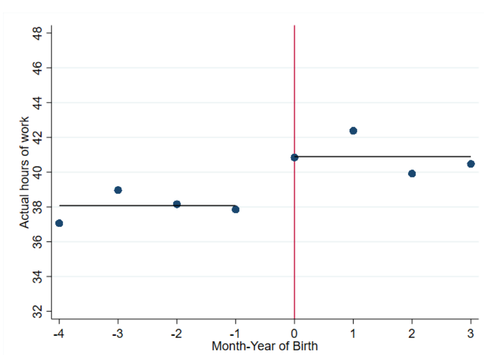
(b) Is employed



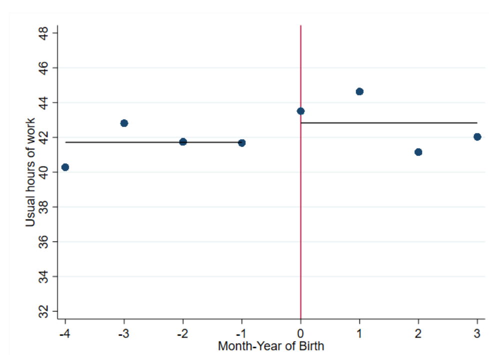
(c) Works full-time



(d) Works part-time



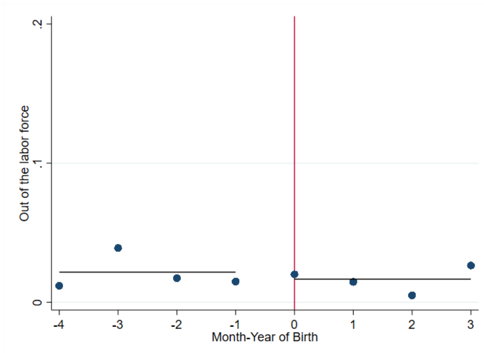
(e) Actual hours of work



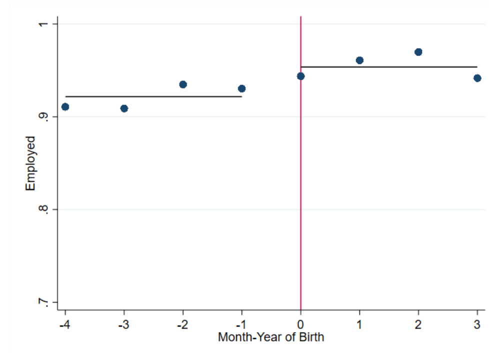
(f) Usual hours of work

Notes: The different panels show fathers' labor market outcomes in the second and third years after a second child's birth, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization regressions with a bandwidth of 4 months. Data are taken from the Labor Force Survey.

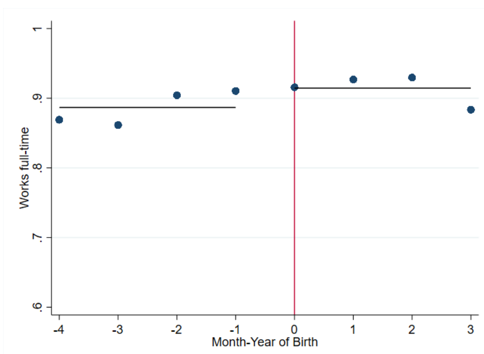
Figure A9: Effects of the reform on fathers' labor market outcomes after leave eligibility expires



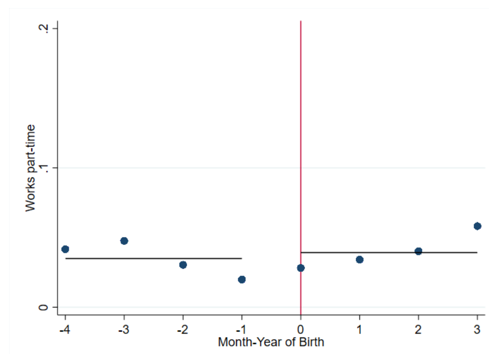
(a) Out of the labor force



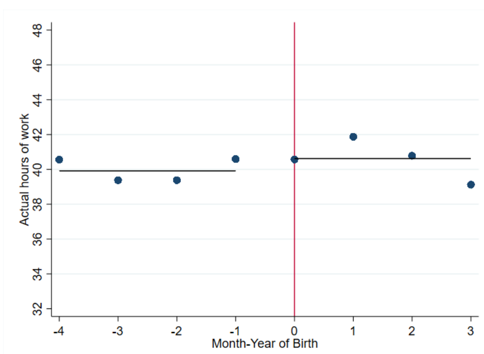
(b) Is employed



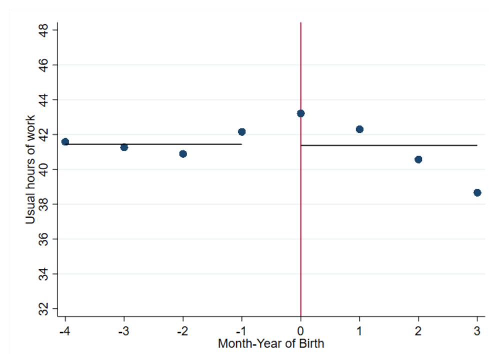
(c) Works full-time



(d) Works part-time



(e) Actual hours of work

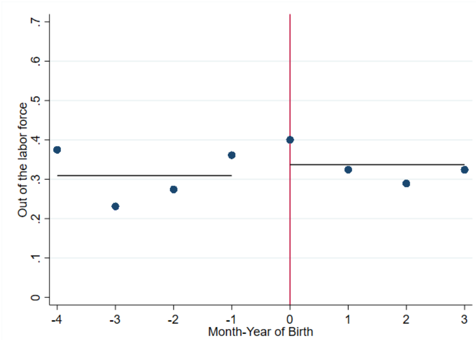


(f) Usual hours of work

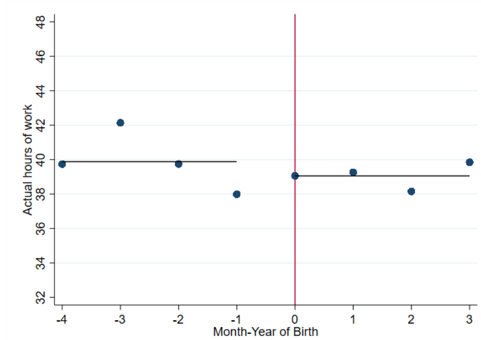
Notes: The different panels show fathers' labor market outcomes in the fourth through seventh years after a second child's birth, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization regressions with a bandwidth of 4 months. Data are taken from the Labor Force Survey.

Figure A10: Placebo and robustness tests for parents' labor outcomes

Cutoff is July 1992

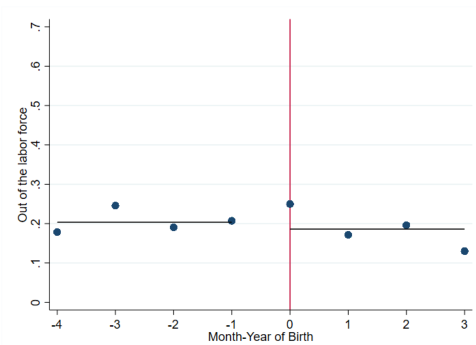


(a) Mother out of the labor force

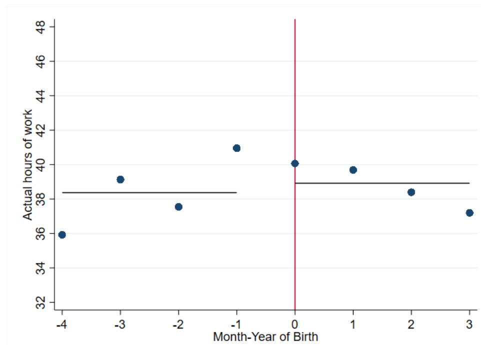


(b) Father's actual hours of work

Running variable is month-year of birth of first child

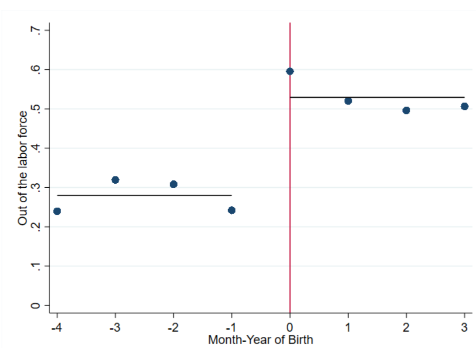


(c) Mother out of the labor force

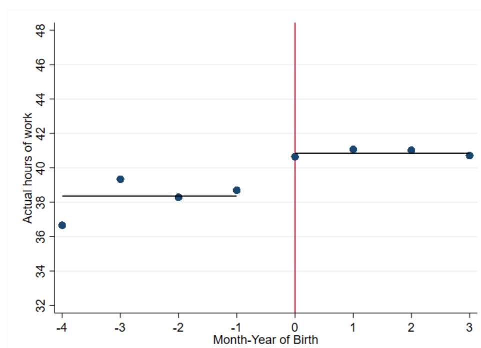


(d) Father's actual hours of work

Mothers aged 35 or less at birth



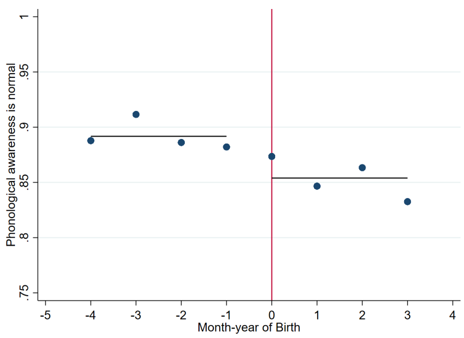
(e) Mother out of the labor force



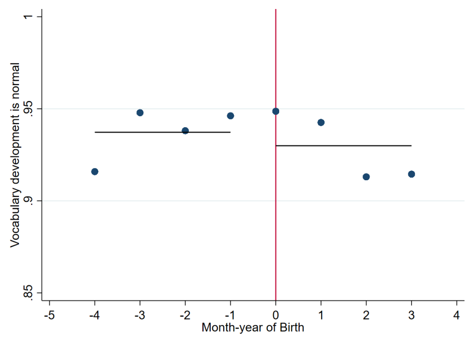
(f) Father's actual hours of work

Notes: Panels (a) and (b) show parents' labor outcomes as a function of distance of the second child's month-year of birth from July 1, 1992. Panels (c) and (d) show parents' labor outcomes as a function of distance of the first child's month-year of birth from the eligibility threshold. Panels (e) and (f) show parents' labor outcomes as a function of distance of the second child's month-year of birth from the eligibility threshold using the sample of mothers aged less than 35. Estimates for mothers' (fathers') outcomes are for the first (second) through third years after a second child's birth. Circles represent each outcome's average over a one month range. Fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Data are taken from the Labor Force Survey.

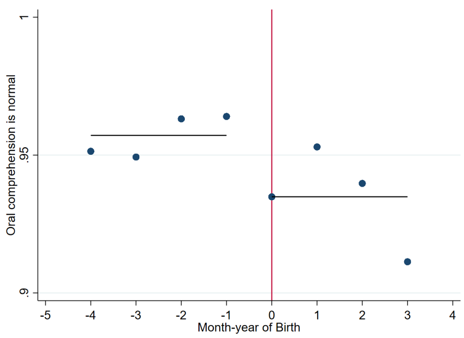
Figure A11: Effect of being born on or after the threshold on first-born children's outcomes



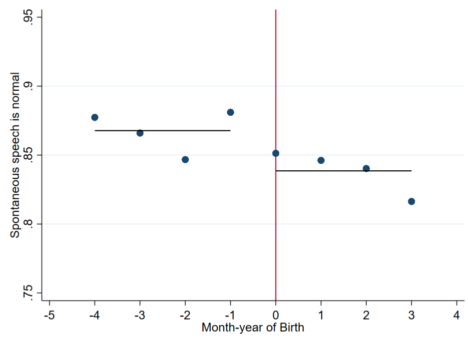
(a) Score on phonological awareness is normal



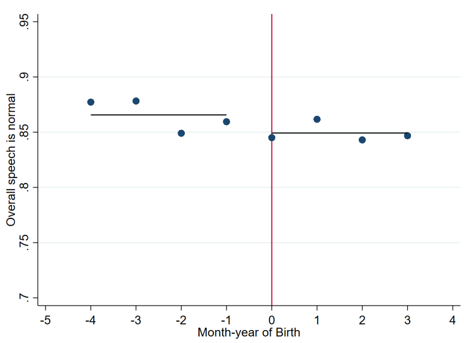
(b) Score on vocabulary development is normal



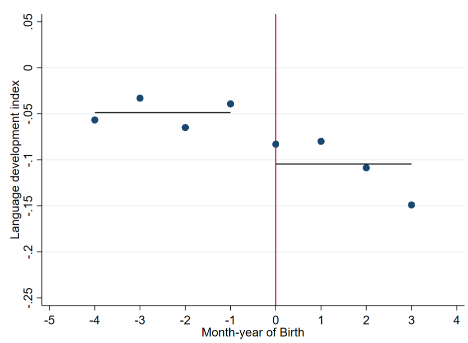
(c) Score on oral comprehension is normal



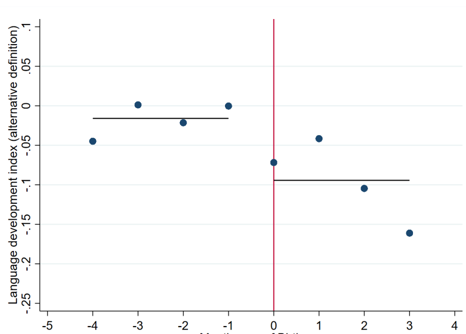
(d) Spontaneous speech is normal



(e) Overall speech is normal



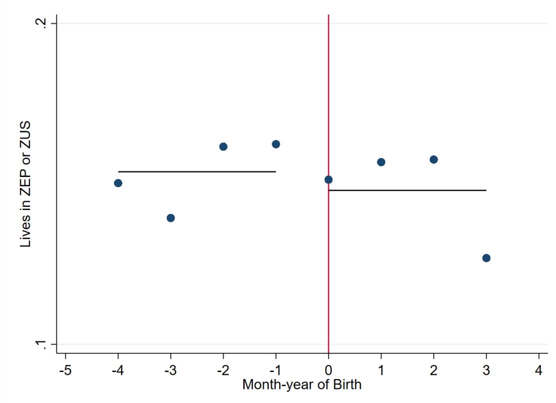
(f) Verbal development index



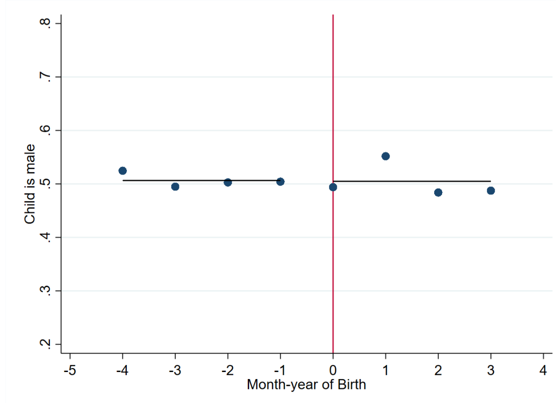
(g) Alternative verbal development index

Notes: The different panels show first-born children's outcomes measured at ages 5-6, as a function of the distance of first-born children's month-year of birth from July 1, 1994. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Data are taken from the Enquête Santé en Milieu Scolaire.

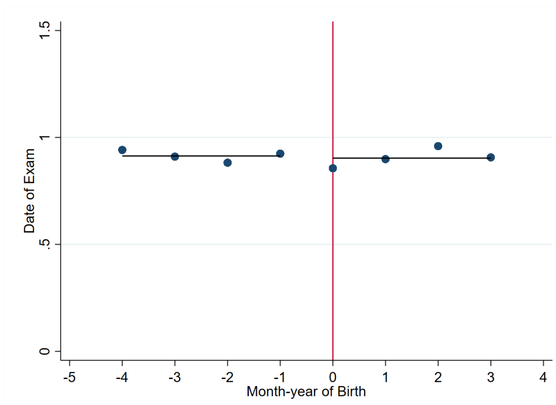
Figure A12: Effect of the reform on second-born's baseline covariates



(a) Child lives in ZEP or ZUS



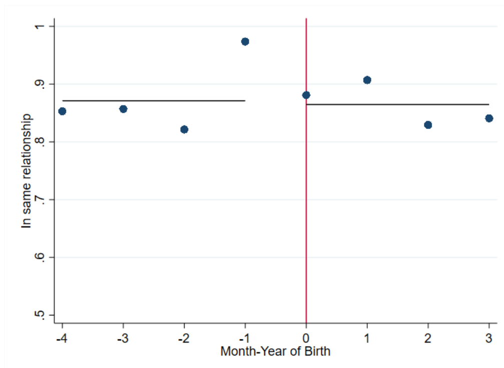
(b) Child is male



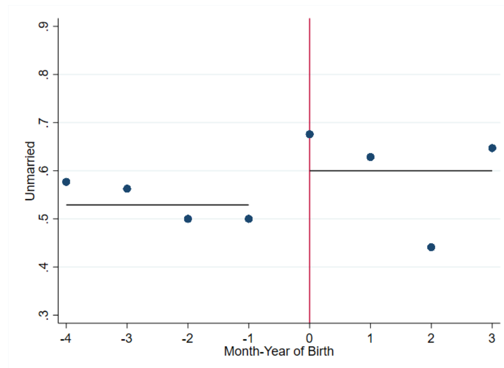
(c) Date of verbal development tests

Notes: The different panels show second-born children's baseline covariates, as a function of the distance their month-year of birth from July 1, 1994. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Data are taken from the Enquête Santé en Milieu Scolaire.

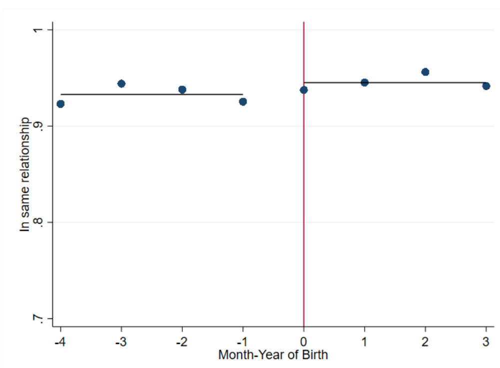
Figure A13: Effects of the reform on marital outcomes



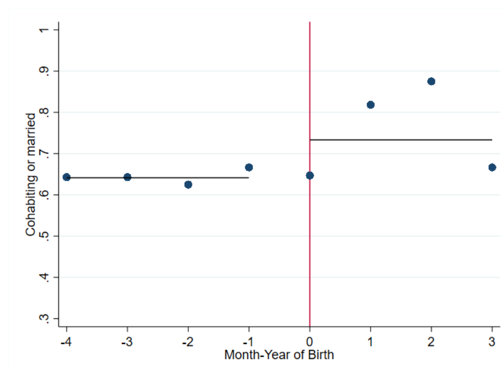
(a) Cohabiting mothers in same relationship



(b) Cohabiting mothers not married



(c) Married mothers in same relationship

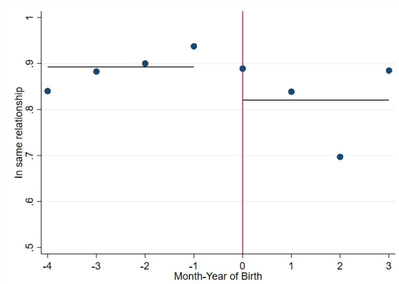


(d) Single mothers married or cohabiting

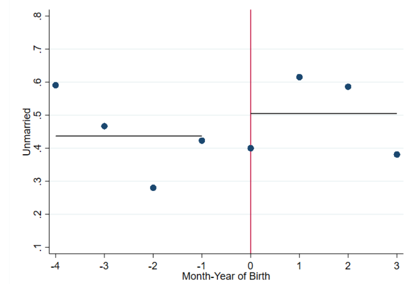
Notes: The different panels show marital outcomes as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months. Panels (a) and (b) include mothers who were cohabiting but unmarried at the birth of their second child. Panel (c) includes mothers who were married at the birth of their second child. Panel (d) includes mothers who were single at the birth of their second child. Data are taken from the Enquête Etude de L'Histoire Familiale.

Figure A14: Placebo tests for marital outcomes

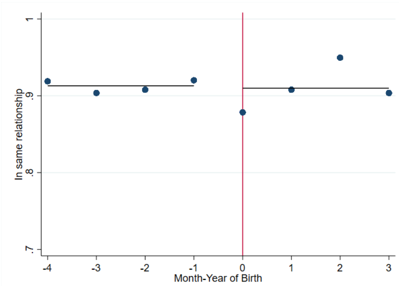
Cutoff is July 1992



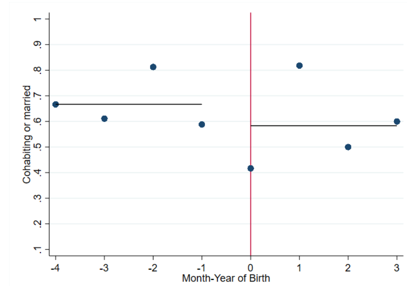
(a) Cohabiting mothers in same relationship



(b) Cohabiting mothers not married

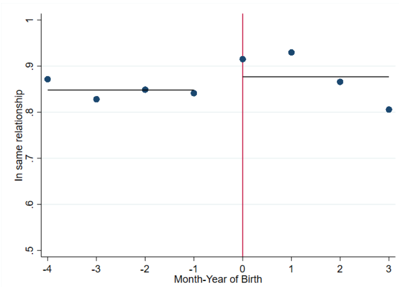


(c) Married mothers in same relationship

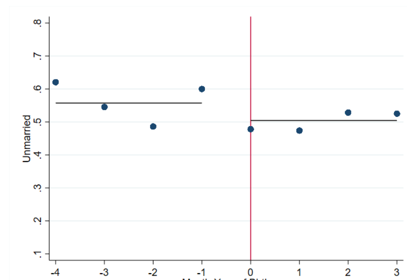


(d) Single mothers married or cohabiting

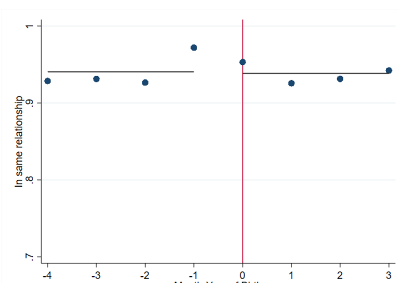
Running variable is month-year of birth of first child



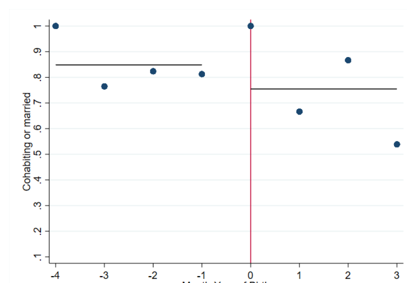
(e) Cohabiting mothers in same relationship



(f) Cohabiting mothers not married



(g) Married mothers in same relationship



(h) Single mothers married or cohabiting

Notes: Panels (a) to (d) show marital outcomes, as a function of distance of the second child's month-year of birth from July 1, 1992. Panels (e) to (h) show marital outcomes as a function of distance of the first child's month-year of birth from the eligibility threshold. Panels (a), (b), (e) and (f) use the sample of mothers who were cohabiting at the date of birth of their child. Panels (c) and (g) use the sample of mothers who were married at the date of birth of their child. Panels (d) and (h) use the sample of mothers who were single at the date of birth of their child. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from local randomization specifications with a bandwidth of 4 months.

Table A1: Regression estimates for baseline covariates using different bandwidths and labor force survey

	BW=2 (1)	BW=4 (2)	BW=6 (3)	BW=16 (4)	BW=19 (5)	BW=22 (6)	BW=25 (7)	BW=28 (8)
Second child is male	-0.029	-0.028	-0.017	-0.034	-0.032	-0.028	-0.026	-0.023
	(0.035)	(0.025)	(0.021)	(0.028)	(0.026)	(0.024)	(0.023)	(0.021)
Mother's age at childbirth	0.719**	0.236	0.102	0.170	0.127	0.113	0.126	0.140
	(0.303)	(0.215)	(0.177)	(0.238)	(0.219)	(0.204)	(0.192)	(0.181)
Father's age at childbirth	0.597*	0.103	0.103	0.162	0.107	0.098	0.102	0.088
	(0.353)	(0.258)	(0.212)	(0.286)	(0.263)	(0.245)	(0.231)	(0.218)
Mother born in France	-0.020	-0.006	0.002	-0.000	0.003	0.003	0.004	0.002
	(0.022)	(0.016)	(0.013)	(0.018)	(0.016)	(0.015)	(0.014)	(0.013)
Father born in France	-0.040*	-0.022	-0.018	-0.034*	-0.031*	-0.029*	-0.025*	-0.021
	(0.023)	(0.016)	(0.013)	(0.018)	(0.017)	(0.016)	(0.015)	(0.014)
Mother high school and more	0.001	-0.014	-0.004	-0.024	-0.015	-0.009	-0.004	0.002
	(0.032)	(0.023)	(0.019)	(0.025)	(0.023)	(0.022)	(0.020)	(0.019)
Father high school and more	0.004	-0.013	-0.001	-0.008	-0.004	-0.005	-0.008	-0.008
	(0.031)	(0.023)	(0.019)	(0.025)	(0.023)	(0.022)	(0.020)	(0.019)
Mother's years of education	0.224	-0.228	-0.179	-0.337	-0.241	-0.175	-0.122	-0.057
	(0.369)	(0.269)	(0.227)	(0.300)	(0.276)	(0.258)	(0.242)	(0.229)
Father 's years of education	0.148	-0.158	-0.044	-0.116	-0.059	-0.072	-0.099	-0.096
	(0.381)	(0.276)	(0.226)	(0.307)	(0.282)	(0.263)	(0.247)	(0.233)
Mother's father is manual worker	0.002	0.004	0.019	0.030	0.031	0.029	0.029	0.026
	(0.034)	(0.025)	(0.021)	(0.028)	(0.025)	(0.024)	(0.022)	(0.021)
Father's father is manual worker	0.012	0.016	0.033	0.033	0.029	0.028	0.026	0.026
	(0.034)	(0.024)	(0.020)	(0.027)	(0.025)	(0.023)	(0.022)	(0.021)
Mother's father is high-skilled	-0.011	-0.020	-0.009	-0.016	-0.012	-0.012	-0.013	-0.014
	(0.021)	(0.015)	(0.012)	(0.017)	(0.016)	(0.014)	(0.014)	(0.013)
Father's father is high-skilled	-0.015	-0.016	-0.030**	-0.034*	-0.029*	-0.023	-0.018	-0.014
	(0.023)	(0.017)	(0.013)	(0.018)	(0.017)	(0.016)	(0.015)	(0.014)
N	834	1,568	2,297	6,167	7,329	8,432	9,556	10,682

Notes: Each cell reports the RD estimate of the effect of reform on the corresponding baseline covariate. Each column uses the listed bandwidth (BW). Columns (1) to (3) use the local randomization approach, while the rest of the columns use local linear regressions and a triangular kernel. Data are taken from the Labor Force Survey. Robust standard errors are reported in parentheses (*** p $<$ 0.01 ** p $<$0.05 * p $<$0.1).

Table A2: Regression estimates for baseline covariates using different bandwidths and Enquête Etude de L'Histoire Familiale

	BW=2 (1)	BW=4 (2)	BW=6 (3)	BW=16 (4)	BW=19 (5)	BW=22 (6)	BW=25 (7)	BW=28 (8)
Second child is male	0.004	0.020	-0.014	-0.002	-0.008	-0.012	-0.014	-0.015
	(0.036)	(0.026)	(0.021)	(0.029)	(0.027)	(0.025)	(0.023)	(0.022)
Mother's age at childbirth	-0.363	-0.304	-0.296*	-0.009	0.041	0.093	0.126	0.146
	(0.301)	(0.215)	(0.178)	(0.242)	(0.223)	(0.207)	(0.193)	(0.182)
Mother born in France	-0.023	-0.013	-0.008	-0.020	-0.017	-0.012	-0.007	-0.005
	(0.020)	(0.015)	(0.012)	(0.017)	(0.015)	(0.014)	(0.014)	(0.013)
Mother's father born in France	0.020	0.012	0.024	0.021	0.021	0.022	0.024	0.023
	(0.030)	(0.022)	(0.018)	(0.024)	(0.022)	(0.021)	(0.019)	(0.018)
Mother high school and more	0.020	0.038	0.035*	0.028	0.036	0.040	0.037	0.033
	(0.036)	(0.026)	(0.021)	(0.029)	(0.026)	(0.025)	(0.023)	(0.022)
Mother's father is manual worker	-0.047	0.031	0.002	0.011	0.006	-0.000	-0.004	-0.008
	(0.035)	(0.025)	(0.020)	(0.028)	(0.025)	(0.024)	(0.022)	(0.021)
Mother's father is high-skilled	0.012	0.014	0.020	0.017	0.015	0.015	0.016	0.015
	(0.021)	(0.014)	(0.012)	(0.017)	(0.015)	(0.014)	(0.013)	(0.012)
Mother had work interruption	0.020	0.006	-0.000	0.012	0.005	0.003	0.001	-0.000
	(0.016)	(0.012)	(0.010)	(0.013)	(0.012)	(0.011)	(0.011)	(0.010)
Length of work interruption	0.119**	0.018	0.010	0.059	0.036	0.027	0.021	0.015
	(0.055)	(0.041)	(0.033)	(0.042)	(0.039)	(0.037)	(0.035)	(0.034)
Mother's age at first job	0.280	0.119	0.075	0.119	0.109	0.099	0.060	0.028
	(0.207)	(0.146)	(0.119)	(0.164)	(0.150)	(0.139)	(0.130)	(0.122)
N	767	1,496	2,197	5,982	7,047	8,156	9,348	10,481

Note: Each cell reports the RD estimate of the effect of reform on the corresponding baseline covariate. Each column uses the listed bandwidth (BW). Columns (1) to (3) use the local randomization approach, while the rest of the columns use local linear regressions and a triangular kernel. Data are taken from the Enquête Etude de L'Histoire Familiale. Robust standard errors are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A3: Effects of the reform on mothers' labor market outcomes during leave eligibility using different bandwidths

	BW=2 (1)	BW=4 (2)	BW=6 (3)	BW=16 (4)	BW=19 (5)	BW=22 (6)	BW=25 (7)	BW=28 (8)
Out of the labor force	0.273***	0.229***	0.214***	0.246***	0.226***	0.214***	0.208***	0.206***
	(0.044)	(0.033)	(0.027)	(0.036)	(0.033)	(0.031)	(0.029)	(0.027)
With controls	0.288***	0.224***	0.208***	0.194***	0.177***	0.171***	0.169***	0.173***
	(0.041)	(0.031)	(0.026)	(0.033)	(0.031)	(0.029)	(0.027)	(0.026)
Employed	–0.188***	–0.176***	–0.163***	-0.184***	-0.165***	-0.152***	-0.149***	-0.148***
	(0.047)	(0.034)	(0.028)	(0.038)	(0.035)	(0.032)	(0.031)	(0.029)
With controls	–0.202***	–0.168***	–0.154***	-0.115***	-0.105***	-0.101***	-0.100***	-0.108***
	(0.044)	(0.032)	(0.026)	(0.035)	(0.032)	(0.030)	(0.028)	(0.027)
Works full-time	–0.131***	–0.136***	–0.118***	-0.130***	-0.126***	-0.125***	-0.125***	-0.125***
	(0.042)	(0.032)	(0.026)	(0.035)	(0.032)	(0.030)	(0.028)	(0.027)
With controls	–0.143***	–0.137***	–0.115***	-0.118***	-0.114***	-0.114***	-0.114***	-0.113***
	(0.041)	(0.031)	(0.025)	(0.033)	(0.031)	(0.029)	(0.027)	(0.026)
Works part-time	–0.056	–0.041	–0.046**	-0.053*	-0.039	-0.028	-0.024	-0.024
	(0.038)	(0.028)	(0.023)	(0.031)	(0.029)	(0.027)	(0.025)	(0.024)
With controls	–0.060	–0.032	–0.041*	0.002	0.008	0.011	0.013	0.004
	(0.037)	(0.027)	(0.023)	(0.030)	(0.028)	(0.026)	(0.024)	(0.023)
Stay-at-home mother	0.288***	0.222***	0.211***	0.243***	0.222***	0.209***	0.200***	0.196***
	(0.043)	(0.032)	(0.026)	(0.035)	(0.032)	(0.030)	(0.028)	(0.027)
With controls	0.300***	0.218***	0.205***	0.164***	0.157***	0.151***	0.147***	0.152***
	(0.040)	(0.030)	(0.025)	(0.032)	(0.030)	(0.028)	(0.026)	(0.025)
Low-skilled occupation	–0.092***	–0.026	–0.027*	-0.043**	-0.034*	-0.031*	-0.027	-0.025
	(0.024)	(0.019)	(0.015)	(0.021)	(0.019)	(0.018)	(0.017)	(0.016)
With controls	–0.092***	–0.031	–0.030*	-0.032	-0.019	-0.017	-0.014	-0.016
	(0.023)	(0.019)	(0.016)	(0.020)	(0.019)	(0.017)	(0.016)	(0.015)
Middle-skilled occupation	–0.195***	–0.200***	–0.189***	-0.201***	-0.194***	-0.188***	-0.189***	-0.187***
	(0.046)	(0.034)	(0.028)	(0.038)	(0.034)	(0.032)	(0.030)	(0.028)
With controls	–0.206***	–0.200***	–0.188***	-0.191***	-0.186***	-0.182***	-0.182***	-0.181***
	(0.045)	(0.033)	(0.027)	(0.036)	(0.033)	(0.031)	(0.029)	(0.027)
High-skilled occupation	0.013	–0.003	0.003	-0.002	0.003	0.007	0.007	0.006
	(0.027)	(0.020)	(0.017)	(0.022)	(0.020)	(0.019)	(0.018)	(0.017)
With controls	0.011	0.006	0.011	0.027	0.028	0.028	0.027	0.023
	(0.025)	(0.019)	(0.016)	(0.021)	(0.019)	(0.018)	(0.017)	(0.016)
N	727	1,368	2,033	5,398	6,422	7,388	8,413	9,380

Note: Each cell reports the RD estimate of the effect of reform on the corresponding outcome. Estimates are for the first through third year after second child's birth. Each column uses the listed bandwidth (BW). Columns (1) to (3) use the local randomization approach, while the rest of the columns use local linear regressions and a triangular kernel. Results are shown both with and without controls. Controls include second child's month of birth fixed effects, year of survey fixed effects, parents' age at the birth of the second child (and their square), as well as dummy variables for whether the second child is male, whether parents are born in France, have a high school degree or more, and have fathers who are manual workers or in managerial positions. Data are taken from the Labor Force Survey. Standard errors are clustered by mothers' ID and are reported in parentheses (*** p < 0.01 ** p <0.05 * p <0.1).

Table A4: Effects of the reform on mothers' labor market outcomes after leave expires using different bandwidths

	BW=2 (1)	BW=4 (2)	BW=6 (3)	BW=16 (4)	BW=19 (5)	BW=22 (6)	BW=25 (7)	BW=28 (8)
Out of the labor force	-0.010	0.015	0.019	0.021	0.016	0.011	0.008	0.005
	(0.037)	(0.028)	(0.024)	(0.031)	(0.029)	(0.027)	(0.025)	(0.024)
With controls	0.002	0.026	0.027	-0.015	-0.006	-0.006	-0.008	-0.006
	(0.035)	(0.026)	(0.022)	(0.029)	(0.027)	(0.025)	(0.024)	(0.022)
Employed	0.019	-0.032	-0.035	-0.035	-0.029	-0.026	-0.025	-0.023
	(0.042)	(0.031)	(0.026)	(0.035)	(0.032)	(0.030)	(0.028)	(0.026)
With controls	0.006	-0.042	-0.044*	0.004	-0.007	-0.007	-0.005	-0.006
	(0.039)	(0.029)	(0.024)	(0.032)	(0.029)	(0.027)	(0.026)	(0.024)
Works full-time	0.048	0.002	-0.031	-0.003	-0.006	-0.005	-0.006	-0.008
	(0.042)	(0.031)	(0.026)	(0.035)	(0.032)	(0.030)	(0.028)	(0.026)
With controls	0.040	-0.006	-0.037	0.010	-0.016	-0.014	-0.013	-0.016
	(0.042)	(0.030)	(0.025)	(0.033)	(0.031)	(0.029)	(0.027)	(0.026)
Works part-time	-0.031	-0.036	-0.009	-0.035	-0.027	-0.025	-0.023	-0.018
	(0.038)	(0.028)	(0.024)	(0.031)	(0.029)	(0.027)	(0.025)	(0.024)
With controls	-0.036	-0.038	-0.011	-0.011	0.004	0.003	0.004	0.006
	(0.037)	(0.027)	(0.023)	(0.030)	(0.028)	(0.026)	(0.025)	(0.023)
Stay-at-home mother	-0.020	0.003	0.018	0.018	0.013	0.009	0.006	0.004
	(0.037)	(0.028)	(0.024)	(0.031)	(0.028)	(0.027)	(0.025)	(0.024)
With controls	-0.007	0.012	0.026	-0.013	-0.007	-0.008	-0.010	-0.008
	(0.034)	(0.026)	(0.022)	(0.029)	(0.026)	(0.025)	(0.023)	(0.022)
Low-skilled occupation	0.037	0.021	0.022	0.035	0.038*	0.038*	0.036**	0.032*
	(0.029)	(0.020)	(0.017)	(0.023)	(0.021)	(0.020)	(0.018)	(0.017)
With controls	0.035	0.018	0.017	0.044**	0.039*	0.038**	0.037**	0.031*
	(0.028)	(0.020)	(0.017)	(0.022)	(0.020)	(0.019)	(0.018)	(0.017)
Middle-skilled occupation	-0.023	-0.028	-0.026	-0.034	-0.034	-0.033	-0.029	-0.023
	(0.044)	(0.032)	(0.027)	(0.036)	(0.033)	(0.031)	(0.029)	(0.028)
With controls	-0.037	-0.039	-0.035	-0.032	-0.034	-0.033	-0.031	-0.026
	(0.043)	(0.031)	(0.026)	(0.034)	(0.032)	(0.030)	(0.028)	(0.027)
High-skilled occupation	0.002	-0.009	-0.015	-0.020	-0.018	-0.015	-0.014	-0.014
	(0.024)	(0.021)	(0.017)	(0.023)	(0.021)	(0.020)	(0.018)	(0.017)
With controls	0.007	-0.005	-0.009	0.001	-0.002	-0.001	0.000	-0.001
	(0.023)	(0.020)	(0.016)	(0.021)	(0.019)	(0.018)	(0.017)	(0.016)
N	885	1,672	2,411	6,566	7,799	8,940	10,073	11,297

Note: Each cell reports the RD estimate of the effect of reform on the corresponding outcome. Estimates are for the fourth through seventh year after second child's birth. Each column uses the listed bandwidth (BW). Columns (1) to (3) use the local randomization approach, while the rest of the columns use local linear regressions and a triangular kernel. Results are shown both with and without controls. Controls include second child's month of birth fixed effects, year of survey fixed effects, parents' age at the birth of the second child (and their square), as well as dummy variables for whether the second child is male, whether parents are born in France, have a high school degree or more, and have fathers who are manual workers or in managerial positions. Data are taken from the Labor Force Survey. Standard errors are clustered by mothers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A5: Effects of the reform on fathers' labor market outcomes in first year after second child's birth using different bandwidths

	BW=2 (1)	BW=4 (2)	BW=6 (3)	BW=16 (4)	BW=19 (5)	BW=22 (6)	BW=25 (7)	BW=28 (8)
Out of the labor force	0.003	0.019	0.008	0.014	0.013	0.010	0.009	0.007
	(0.016)	(0.015)	(0.011)	(0.015)	(0.014)	(0.013)	(0.012)	(0.012)
With controls	0.002	0.026*	0.008	0.011	0.007	0.006	0.006	0.006
	(0.015)	(0.015)	(0.011)	(0.015)	(0.014)	(0.013)	(0.012)	(0.011)
Employed	0.019	-0.010	-0.009	-0.015	-0.011	-0.006	-0.002	0.003
	(0.033)	(0.026)	(0.020)	(0.028)	(0.026)	(0.024)	(0.023)	(0.021)
With controls	0.031	-0.007	-0.001	0.042	0.032	0.031	0.030	0.028
	(0.031)	(0.025)	(0.020)	(0.027)	(0.025)	(0.023)	(0.022)	(0.021)
Works full-time	-0.009	–0.024	–0.025	-0.044	-0.036	-0.030	-0.024	-0.017
	(0.037)	(0.028)	(0.022)	(0.030)	(0.028)	(0.026)	(0.025)	(0.023)
With controls	-0.008	-0.026	-0.018	0.020	0.014	0.011	0.009	0.008
	(0.035)	(0.027)	(0.022)	(0.030)	(0.027)	(0.026)	(0.024)	(0.023)
Works part-time	0.021	0.010	0.014	0.022	0.020	0.020	0.019	0.017
	(0.020)	(0.012)	(0.010)	(0.015)	(0.014)	(0.013)	(0.012)	(0.011)
With controls	0.031	0.014	0.014	0.017	0.016	0.018	0.019	0.018*
	(0.020)	(0.013)	(0.011)	(0.015)	(0.013)	(0.012)	(0.012)	(0.011)
N	247	457	683	1,820	2,159	2,464	2,808	3,119
Actual hours	-3.707*	-2.161	-2.012	-2.459	-2.665	-2.690*	-2.626*	-2.560*
	(2.220)	(1.589)	(1.327)	(1.786)	(1.646)	(1.536)	(1.446)	(1.367)
With controls	–3.793*	–2.721*	–2.124	-1.762	-2.372	-2.568*	-2.710*	-2.867**
	(2.260)	(1.588)	(1.328)	(1.749)	(1.615)	(1.509)	(1.423)	(1.347)
N	229	419	628	1,659	1,968	2,241	2,567	2,854
Usual hours	-0.711	-1.009	-1.558**	-1.554	-1.726*	-1.720**	-1.653**	-1.488*
	(1.230)	(0.905)	(0.757)	(1.014)	(0.936)	(0.874)	(0.823)	(0.779)
With controls	-1.134	-1.200	-1.492*	-0.531	-1.260	-1.421*	-1.496*	-1.448*
	(1.188)	(0.911)	(0.768)	(0.989)	(0.916)	(0.856)	(0.807)	(0.765)
N	195	356	544	1,429	1,696	1,927	2,208	2,433

Note: Each cell reports the RD estimate of the effect of reform on the corresponding outcome. Each column uses the listed bandwidth (BW). Columns (1) to (3) use the local randomization approach, while the rest of the columns use local linear regressions and a triangular kernel. Results are shown both with and without controls. Controls include second child's month of birth fixed effects, year of survey fixed effects, parents' age at the birth of the second child (and their square), as well as dummy variables for whether the second child is male, whether parents are born in France, have a high school degree or more, and have fathers who are manual workers or in managerial positions. Data are taken from the Labor Force Survey. Robust standard errors are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A6: Effects of the reform on fathers' labor market outcomes in years 2 and 3 after second child's birth using different bandwidths

	BW=2 (1)	BW=4 (2)	BW=6 (3)	BW=16 (4)	BW=19 (5)	BW=22 (6)	BW=25 (7)	BW=28 (8)
Out of the labor force	-0.006	0.006	0.003	0.004	0.001	-0.000	-0.000	-0.001
	(0.012)	(0.011)	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)	(0.008)
With controls	-0.007	0.007	0.004	-0.004	-0.003	-0.004	-0.005	-0.005
	(0.011)	(0.011)	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)	(0.008)
Employed	0.044	0.042*	0.023	0.043*	0.039*	0.037*	0.032*	0.030*
	(0.029)	(0.022)	(0.017)	(0.024)	(0.022)	(0.020)	(0.019)	(0.018)
With controls	0.045	0.040*	0.023	0.045*	0.036*	0.036*	0.035*	0.036**
	(0.029)	(0.021)	(0.017)	(0.023)	(0.021)	(0.019)	(0.018)	(0.017)
Works full-time	0.027	0.029	0.011	0.027	0.022	0.018	0.012	0.010
	(0.033)	(0.024)	(0.019)	(0.027)	(0.025)	(0.023)	(0.021)	(0.020)
With controls	0.027	0.027	0.011	0.019	0.013	0.014	0.013	0.016
	(0.033)	(0.023)	(0.019)	(0.026)	(0.024)	(0.022)	(0.021)	(0.019)
Works part-time	0.017	0.011	0.011	0.015	0.016	0.017	0.018*	0.019*
	(0.018)	(0.011)	(0.010)	(0.013)	(0.012)	(0.011)	(0.010)	(0.010)
With controls	0.019	0.010	0.010	0.026*	0.021*	0.021*	0.021**	0.020**
	(0.018)	(0.011)	(0.010)	(0.013)	(0.012)	(0.011)	(0.010)	(0.010)
N	480	911	1,350	3,578	4,263	4,924	5,605	6,261
Actual hours	3.536**	2.815**	1.745*	2.218	2.204*	2.303*	2.414**	2.469**
	(1.737)	(1.222)	(0.986)	(1.400)	(1.271)	(1.178)	(1.101)	(1.037)
With controls	3.306*	2.735**	1.782*	2.167	1.968	2.010*	2.036*	2.010**
	(1.708)	(1.223)	(0.980)	(1.380)	(1.254)	(1.162)	(1.087)	(1.024)
N	434	825	1,238	3,288	3,904	4,515	5,148	5,753
Usual hours	2.292*	1.117	0.326	0.745	0.563	0.546	0.662	0.779
	(1.201)	(0.827)	(0.678)	(0.941)	(0.858)	(0.793)	(0.739)	(0.695)
With controls	2.296*	1.376*	0.639	0.708	0.416	0.497	0.610	0.732
	(1.186)	(0.825)	(0.660)	(0.911)	(0.831)	(0.769)	(0.717)	(0.675)
N	364	691	1,040	2,752	3,253	3,772	4,326	4,819

Note: Each cell reports the RD estimate of the effect of reform on the corresponding outcome. Each column uses the listed bandwidth (BW). Columns (1) and (2) use the local randomization approach, while the rest of the columns use local linear regressions and a triangular kernel. Results are shown both with and without controls. Controls include second child's month of birth fixed effects, year of survey fixed effects, parents' age at the birth of the second child (and their square), as well as dummy variables for whether the second child is male, whether parents are born in France, have a high school degree or more, and have fathers who are manual workers or in managerial positions. Data are taken from the Labor Force Survey. Standard errors are clustered by fathers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A7: Effects of the reform on fathers' labor market outcomes in years 4 through 7 after second child's birth using different bandwidths

	BW=2 (1)	BW=4 (2)	BW=6 (3)	BW=16 (4)	BW=19 (5)	BW=22 (6)	BW=25 (7)	BW=28 (8)
Out of the labor force	0.001	-0.005	0.001	-0.004	-0.003	-0.003	-0.002	-0.002
	(0.010)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)
With controls	0.003	–0.002	0.003	-0.006	-0.002	-0.002	-0.001	-0.000
	(0.009)	(0.007)	(0.007)	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)
Employed	0.019	0.032**	0.029**	0.030*	0.030**	0.029**	0.027**	0.025**
	(0.018)	(0.014)	(0.013)	(0.016)	(0.015)	(0.014)	(0.013)	(0.012)
With controls	0.022	0.030**	0.026**	0.033**	0.032**	0.031**	0.029**	0.026**
	(0.017)	(0.013)	(0.012)	(0.015)	(0.014)	(0.013)	(0.012)	(0.012)
Works full-time	0.014	0.028	0.026*	0.028	0.028	0.026	0.022	0.019
	(0.023)	(0.018)	(0.016)	(0.020)	(0.019)	(0.017)	(0.016)	(0.016)
With controls	0.017	0.025	0.023	0.036*	0.034*	0.031*	0.028*	0.023
	(0.022)	(0.018)	(0.015)	(0.019)	(0.018)	(0.017)	(0.016)	(0.015)
Works part-time	0.005	0.004	0.001	0.002	0.001	0.002	0.003	0.003
	(0.014)	(0.012)	(0.009)	(0.012)	(0.011)	(0.010)	(0.010)	(0.009)
With controls	0.005	0.005	0.001	-0.003	-0.004	-0.001	-0.001	0.000
	(0.014)	(0.012)	(0.009)	(0.012)	(0.011)	(0.010)	(0.010)	(0.009)
N	885	1,672	2,411	6,566	7,799	8,940	10,073	11,297
Actual hours	1.227	0.707	0.825	1.280	1.444	1.276	1.139	1.013
	(1.286)	(0.947)	(0.790)	(1.055)	(0.971)	(0.906)	(0.852)	(0.806)
With controls	1.358	0.708	0.881	1.851*	1.762*	1.506*	1.374*	1.195
	(1.271)	(0.937)	(0.776)	(1.031)	(0.949)	(0.885)	(0.833)	(0.788)
N	830	1,560	2,244	6,099	7,238	8,295	9,351	10,478
Usual hours	1.316	-0.062	0.273	0.573	0.552	0.490	0.469	0.427
	(0.928)	(0.672)	(0.545)	(0.750)	(0.686)	(0.639)	(0.602)	(0.570)
With controls	1.637*	-0.040	0.403	0.780	0.527	0.441	0.432	0.413
	(0.880)	(0.652)	(0.526)	(0.718)	(0.657)	(0.614)	(0.580)	(0.549)
N	709	1,306	1,856	5,007	5,954	6,807	7,662	8,636

Note: Each cell reports the RD estimate of the effect of reform on the corresponding outcome. Each column uses the listed bandwidth (BW). Columns (1) to (3) use the local randomization approach, while the rest of the columns use local linear regressions and a triangular kernel. Results are shown both with and without controls. Controls include second child's month of birth fixed effects, year of survey fixed effects, parents' age at the birth of the second child (and their square), as well as dummy variables for whether the second child is male, whether parents are born in France, have a high school degree or more, and have fathers who are manual workers or in managerial positions. Data are taken from the Labor Force Survey. Standard errors are clustered by fathers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A8: Effects of the reform on mothers' labor market outcomes by level of education (with controls), RD estimates

	Out of the labor force (1)	Is employed (2)	Works full-time (3)	Works part-time (4)	Is stay-at-home mother (5)	In low-skilled occupation (6)	In middle-skilled occupation (7)	In high-skilled occupation (8)
A) During leave								
Mother more than high school	0.120***	-0.067	-0.110**	0.044	0.104**	0.003	-0.138***	0.010
	(0.045)	(0.050)	(0.053)	(0.045)	(0.044)	(0.015)	(0.052)	(0.042)
N	572	572	572	572	572	572	572	572
Mother less than high school	0.296***	-0.239***	-0.155***	-0.086**	0.300***	-0.056*	-0.239***	0.002
	(0.043)	(0.043)	(0.038)	(0.033)	(0.041)	(0.031)	(0.042)	(0.011)
N	796	796	796	796	796	796	796	796
B) After leave expires								
Mother more than high school	0.014	-0.029	0.012	-0.041	-0.021	0.003	-0.026	0.006
	(0.037)	(0.041)	(0.049)	(0.045)	(0.036)	(0.016)	(0.050)	(0.043)
N	682	682	682	682	682	682	682	682
Mother less than high school	0.027	-0.046	-0.012	-0.037	0.027	0.029	-0.052	-0.004
	(0.036)	(0.039)	(0.038)	(0.034)	(0.036)	(0.031)	(0.040)	(0.014)
N	990	990	990	990	990	990	990	990

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. All estimates are taken from regressions including controls and using the local randomization approach with a bandwidth of 4 months. Estimates in Panel A are from the first through third years after the second child's birth. Estimates in Panel B are from the fourth through seventh years after the second child's birth. Controls include year of survey fixed effects, parents' age at the birth of the second child (and their square), as well as dummy variables for whether the second child is male, whether parents are born in France, and have fathers who are manual workers or are high-skilled. Data are taken from the Labor Force Survey. Standard errors are clustered by mothers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A9: Effects of the reform on fathers' labor market outcomes by mothers' level of education (with controls), RD estimates

	Out of the labor force (1)	Is Employed (2)	Works full-time (3)	Works part-time (4)	Actual hours of work (5)	Usual hours of work (6)
A) During Leave						
Year 1						
Mother more than high school	0.002	0.019	-0.040	0.059**	-2.068	-2.585
	(0.016)	(0.032)	(0.040)	(0.026)	(2.579)	(1.584)
N	196	196	196	196	186	149
Mother less than high school	0.040*	-0.023	-0.006	-0.025*	-3.296*	-0.084
	(0.022)	(0.035)	(0.037)	(0.014)	(2.068)	(1.107)
N	261	261	261	261	233	207
Years 2-3						
Mother more than high school	-0.014	0.020	0.009	0.012	4.129**	3.139**
	(0.020)	(0.029)	(0.035)	(0.020)	(1.887)	(1.276)
N	376	376	376	376	352	285
Mother less than high school	0.019	0.060**	0.047	0.009	1.667	-0.031
	(0.014)	(0.030)	(0.032)	(0.014)	(1.590)	(1.056)
N	535	535	535	535	473	406
B) After Leave Expires						
Mother more than high school	-0.004	0.017	0.018	-0.001	1.841	0.682
	(0.011)	(0.018)	(0.027)	(0.020)	(1.513)	(1.106)
N	682	682	682	682	646	510
Mother less than high school	-0.000	0.037*	0.029	0.008	-0.163	-0.404
	(0.010)	(0.019)	(0.024)	(0.015)	(1.208)	(0.786)
N	990	990	990	990	914	796

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. All estimates are taken from regressions including controls and using the local randomization approach with a bandwidth of 4 months. Estimates in Panel A are from the first through third years after the second child's birth. Estimates in Panel B are from the fourth through seventh years after the second child's birth. Controls include year of survey fixed effects, parents' age at the birth of the second child (and their square), as well as dummy variables for whether the second child is male, whether parents are born in France, and have fathers who are manual workers or are high-skilled. Data are taken from the Labor Force Survey. Standard errors are clustered by fathers' ID and are reported in parentheses (*** p < 0.01 ** p <0.05 * p <0.1).

Table A10: Placebo tests for mothers' labor market outcomes

	Out of the labor force (1)	Is employed (2)	Works full-time (3)	Works part-time (4)	Is stay-at-home mother (5)	In low-skilled occupation (6)	In middle-skilled occupation (7)	In high-skilled occupation (8)
A) Cutoff is July 1992								
Years 1-3								
RD estimate	0.028	-0.008	-0.027	0.019	0.002	0.013	-0.034	-0.012
	(0.032)	(0.035)	(0.034)	(0.029)	(0.031)	(0.022)	(0.035)	(0.020)
N	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285
Years 4-7								
RD estimate	0.020	-0.041	0.015	-0.056**	-0.001	0.020	-0.021	-0.020
	(0.028)	(0.031)	(0.031)	(0.028)	(0.027)	(0.017)	(0.032)	(0.020)
N	1,803	1,803	1,803	1,803	1,803	1,803	1,803	1,803
B) First child								
Years 1-3								
RD estimate	-0.017	-0.034	-0.039	0.002	-0.001	-0.001	0.040	-0.025
	(0.023)	(0.030)	(0.031)	(0.023)	(0.022)	(0.019)	(0.031)	(0.019)
N	1,618	1,618	1,618	1,618	1,618	1,618	1,618	1,618
Years 4-7								
RD estimate	-0.037	0.025	-0.019	0.044*	-0.017	0.000	0.059**	-0.023
	(0.026)	(0.028)	(0.029)	(0.024)	(0.025)	(0.017)	(0.029)	(0.019)
N	1,981	1,981	1,981	1,981	1,981	1,981	1,981	1,981

Note: Panel A reports the RD estimate of the effect of having a second child born after the fake cutoff July 1, 1992 on the corresponding outcome. Panel B reports the RD estimate of the effect of having a first child born after the July 1, 1994 cutoff on the corresponding outcome. In both panels, estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Data are taken from the Labor Force Survey. Standard errors are clustered by mothers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A11: Placebo tests for fathers' labor market outcomes

	Out of the labor force (1)	Is Employed (2)	Works full-time (3)	Works part-time (4)	Actual hours of work (5)	Usual hours of work (6)
A) Cutoff is July 1992						
Year 1						
RD estimate	0.011	-0.024	-0.051*	0.022	-1.110	-1.599
	(0.010)	(0.025)	(0.029)	(0.017)	(1.728)	(1.088)
N	406	406	406	406	378	312
Years 2-3						
RD estimate	0.016*	-0.025	0.048**	0.021*	-0.829	0.265
	(0.009)	(0.020)	(0.023)	(0.012)	(1.305)	(0.934)
N	879	879	879	879	812	692
Years 4-7						
RD estimate	-0.012	-0.009	-0.010	0.001	0.184	-0.246
	(0.008)	(0.016)	(0.018)	(0.008)	(0.963)	(0.651)
N	1,803	1,803	1,803	1,803	1,640	1,339
B) First child						
Year 1						
RD estimate	-0.001	0.005	-0.000	0.002	-0.825	-0.356
	(0.013)	(0.025)	(0.028)	(0.016)	(1.422)	(0.815)
N	569	569	569	569	512	446
Years 2-3						
RD estimate	-0.014	-0.002	-0.007	0.005	0.546	-0.156
	(0.010)	(0.021)	(0.024)	(0.012)	(1.093)	(0.643)
N	1,049	1,049	1,049	1,049	935	801
Years 4-7						
RD estimate	-0.019***	0.027*	0.026	0.000	0.083	0.213
	(0.007)	(0.014)	(0.016)	(0.009)	(0.831)	(0.586)
N	1,981	1,981	1,981	1,981	1,837	1,543

Note: Panel A reports the RD estimate of the effect of having a second child born after the fake cutoff July 1, 1992 on the corresponding outcome. Panel B reports the RD estimate of the effect of having a first child born after the July 1, 1994 cutoff on the corresponding outcome. In both panels, estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Data are taken from the Labor Force Survey. For year 1, robust standard errors are reported in parentheses. For stacked years 2-3 and 4-7, standard errors are clustered by fathers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A12: Effect of the reform on mothers' labor market outcomes, mothers aged 35 and less

	Out of the labor force (1)	Is employed (2)	Works full-time (3)	Works part-time (4)	Is stay-at-home mother (5)	In low-skilled occupation (6)	In middle-skilled occupation (7)	In high-skilled occupation (8)
Years 1-3								
RD estimate	0.250***	-0.196***	-0.141***	-0.057**	0.238***	-0.030	-0.212***	-0.009
	(0.036)	(0.037)	(0.034)	(0.029)	(0.035)	(0.021)	(0.037)	(0.019)
N	1,142	1,142	1,142	1,142	1,142	1,142	1,142	1,142
Years 4-7								
RD estimate	0.023	-0.047	-0.040	-0.012	0.025	0.018	-0.028	-0.013
	(0.039)	(0.041)	(0.037)	(0.035)	(0.038)	(0.028)	(0.041)	(0.021)
N	957	957	957	957	957	957	957	957

Note: Each cell reports the RD estimate of the reform on the corresponding outcome. The sample includes mothers who were aged 35 and less at the date of birth of their second child. Estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Data are taken from the Labor Force Survey. Standard errors are clustered by mothers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A13: Effect of the reform on fathers' labor market outcomes, mothers aged 35 and less

	Out of the labor force (1)	Is Employed (2)	Works full-time (3)	Works part-time (4)	Actual hours of work (5)	Usual hours of work (6)
Year 1						
RD estimate	0.023	-0.020	-0.031	0.007	-2.570	-1.480*
	(0.017)	(0.028)	(0.030)	(0.013)	(1.675)	(0.892)
N	406	406	406	406	371	323
Years 2-3						
RD estimate	0.007	0.041*	0.015	0.023*	2.676**	1.511*
	(0.013)	(0.024)	(0.027)	(0.013)	(1.323)	(0.887)
N	736	736	736	736	665	557
Years 4-7						
RD estimate	-0.004	0.030	0.036	-0.007	0.258	-0.527
	(0.010)	(0.020)	(0.026)	(0.016)	(1.180)	(0.858)
N	957	957	957	957	881	737

Note: Each cell reports the RD estimate of the reform on the corresponding outcome. The sample includes fathers whose spouses were aged 35 and less at the date of birth of their second child. Estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Data are taken from the Labor Force Survey. For year 1, robust standard errors are reported in parentheses. For stacked years 2-3 and 4-7, standard errors are clustered by fathers' ID and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A14: Effects of the reform on children's outcomes with controls

	Phonological Awareness (1)	Vocabulary Development (2)	Oral Comprehension (3)	Spontaneous Speech (4)	Overall Speech (5)	Verbal Dev. Index (6)	Alternative Verb. Dev. Index (7)	Age at beginning of preschool (8)	Time in preschool (9)
A) Overall sample									
RD estimate	-0.043***	-0.031***	-0.020***	-0.055***	-0.057***	-0.096***	-0.140***	-1.913***	-1.994***
	(0.010)	(0.008)	(0.007)	(0.011)	(0.011)	(0.014)	(0.021)	(0.153)	(0.154)
N	4,295	4,297	4,297	4,453	4,678	6,413	4,151	6,054	6,054
B) Overall sample									
RD-DID estimate	-0.030	-0.032*	0.002	-0.043*	-0.053**	-0.067**	-0.092**	0.038	-0.052
	(0.022)	(0.017)	(0.015)	(0.024)	(0.024)	(0.029)	(0.044)	(0.350)	(0.350)
N	13,830	13,848	13,861	14,497	15,263	20,878	13,411	19,822	19,822
C) In ZEP									
RD-DID estimate	-0.096	-0.114**	0.035	-0.089	-0.108*	-0.176**	-0.186	1.826*	-1.826*
	(0.064)	(0.052)	(0.049)	(0.069)	(0.061)	(0.086)	(0.127)	(0.978)	(0.978)
N	2,161	2,157	2,153	2,232	2,408	3,198	2,058	3,047	3,047
D) Not in ZEP									
RD-DID estimate	-0.018	-0.016	-0.003	-0.034	-0.041	-0.048	-0.073	-0.297	0.280
	(0.023)	(0.017)	(0.015)	(0.025)	(0.026)	(0.030)	(0.046)	(0.372)	(0.372)
N	11,669	11,691	11,708	12,265	12,855	17,680	11,353	16,775	16,775

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates in Panel A are from a local randomization RD specification with a bandwidth of 4 months. Estimates in Panels B, C and D are from a difference-in-discontinuity regression using data within 6 months on either side of the cutoff. Panels A and B are for the overall sample. Panels C and D respectively restrict the overall sample to children residing in ZEP or ZUS and to children not residing in these areas. Results are shown with controls. Controls include fixed effects for the months the exam was administered in and a dummy variable equal to 1 if the second child is male. The varying number of observations is due to missing data. Data are taken from the Enquête Santé en Milieu Scolaire. Robust standard errors are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A15: Effects of the reform on children's verbal development, RD estimates across different bandwidths

	Phonological Awareness (1)	Vocabulary Development (2)	Oral Comprehension (3)	Spontaneous Speech (4)	Overall Speech (5)	Verbal Development Index (6)	Alternative Verb. Dev. Index (7)	Age at beginning of preschool (8)	Time in preschool (9)
A) Local randomization									
BW= 2 months									
No controls	-0.044***	-0.024**	-0.013	-0.044***	-0.039***	-0.076***	-0.112***	-1.023***	-1.013***
	(0.014)	(0.011)	(0.010)	(0.015)	(0.015)	(0.020)	(0.030)	(0.214)	(0.224)
With controls	-0.043***	-0.024**	-0.014	-0.045***	-0.038**	-0.076****	-0.110***	-1.014***	-1.001***
	(0.014)	(0.010)	(0.010)	(0.015)	(0.015)	(0.020)	(0.030)	(0.214)	(0.215)
N	2,205	2,210	2,211	2,284	2,410	3,346	2,135	3,168	3,168
B) Local linear									
BW= 6 months									
No controls	-0.038**	-0.023	-0.004	-0.047**	-0.044**	-0.069**	-0.106**	-0.105	0.061
	(0.019)	(0.015)	(0.014)	(0.021)	(0.021)	(0.028)	(0.042)	(0.306)	(0.319)
With controls	-0.038**	-0.022	-0.005	-0.046**	-0.043**	-0.069**	-0.104**	-0.104	0.103
	(0.019)	(0.015)	(0.014)	(0.021)	(0.021)	(0.027)	(0.041)	(0.305)	(0.304)
N	6,215	6,210	6,210	6,459	6,780	9,316	6,017	8,781	8,781

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. Estimates in Panel A are taken from regressions using the local randomization approach with a bandwidth of 2 months. Estimates in Panel B are taken from local linear regressions using a bandwidth of 6 months and a triangular kernel. Controls include fixed effects for the date the exam was administered in and a dummy variable equal to 1 if the second child is male. The varying number observations is due to missing data. Data are taken from the Enquête Santé en Milieu Scolaire. Robust standard errors are reported in parentheses (*** p < 0.01 ** p <0.05 * p <0.1).

Table A16: Effects of the reform on children's verbal development tests (alternative definition)

	1 to 2 sd. below normal			3 sd. below normal		
	Phonological Awareness (1)	Vocabulary Development (2)	Oral Comprehension (3)	Phonological Awareness (4)	Vocabulary Development (5)	Oral Comprehension (6)
A) Local randomization						
BW= 2 months						
No controls	0.038***	0.022**	0.014	0.006	0.002	-0.001
	(0.013)	(0.011)	(0.009)	(0.006)	(0.004)	(0.003)
With controls	0.037***	0.022**	0.014	0.006	0.002	-0.000
	(0.013)	(0.010)	(0.009)	(0.006)	(0.004)	(0.004)
N	2,205	2,210	2,211	2,205	2,210	2,211
BW= 4 months						
No controls	0.032***	0.026***	0.019***	0.012***	0.004*	0.001
	(0.009)	(0.007)	(0.006)	(0.011)	(0.003)	(0.003)
With controls	0.031***	0.026***	0.019***	0.012***	0.004*	0.001
	(0.009)	(0.007)	(0.006)	(0.004)	(0.003)	(0.003)
N	4,295	4,297	4,297	4,295	4,297	4,297
B) Local linear						
No controls	0.033*	0.018	0.006	0.005	0.005	-0.001
	(0.017)	(0.014)	(0.013)	(0.008)	(0.005)	(0.005)
With controls	0.033*	0.018	0.006	0.005	0.005	-0.001
	(0.017)	(0.014)	(0.013)	(0.008)	(0.005)	(0.005)
N	6,215	6,210	6,210	6,215	6,210	6,210

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. Columns (1) to (3) show effects on the probability of having a score that is 1 to 2 standard deviations below normal, while columns (4) to (6) focus on the probability of having a score that is 3 standard deviations below normal. Estimates in Panel A are taken from regressions using the local randomization approach with bandwidths of 2 and 4 months. Estimates in Panel B are taken from local linear regressions using a bandwidth of 6 months and a triangular kernel. Controls include fixed effects for the date the exam was administered in and a dummy variable equal to 1 if the second child is male. The varying number observations is due to missing data. Data are taken from the Enquête Santé en Milieu Scolaire. Robust standard errors are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A17: Effects of the reform on children's baseline covariates

	BW=2 (1)	BW=4 (2)	BW=6 (3)
Child lives in ZEP or ZUS	-0.008	-0.006	-0.009
	(0.013)	(0.009)	(0.018)
Child is male	0.019	-0.001	0.014
	(0.017)	(0.012)	(0.024)
Date of verbal tests	-0.025	-0.010	-0.052
	(0.058)	(0.042)	(0.081)
N	3,346	6,413	9,316

Notes: Each cell reports the RD estimate of the effect of the reform on the baseline covariates. Each column uses the listed bandwidth (BW). Estimates in columns (1) and (2) are taken from regressions using the local randomization approach. Estimates in column (3) are taken from local linear regressions using a bandwidth of 6 months and a triangular kernel. The variable "Date of verbal tests" is the date the verbal tests were administered and it is measured in months relative to March 1, 2000. Data are taken from the Enquête Santé en Milieu Scolaire. Robust standard errors are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A18: Sample means for marital outcomes

	Before cutoff (1)	After cutoff (2)
Marital outcomes in 1999		
Mothers cohabiting at childbirth		
In same relationship	0.871	0.865
N	163	170
Unmarried	0.529	0.600
N	138	140
Mothers married at childbirth		
In same relationship	0.933	0.945
N	536	529
Unmarried	0.043	0.036
N	536	529
Mothers single at childbirth		
Cohabiting or married	0.642	0.733
N	63	45

Note: This table reports marital outcomes' means for mothers whose second child was born within 4 months before and after the cutoff. Data are taken from the "Enquête Etude de L'Histoire Familiale".

Table A19: Effects of the reform on marital outcomes

	Cohabiting before childbirth		Married before childbirth	Single before childbirth
	In same relationship (1)	Unmarried (2)	In same relationship (3)	Cohabiting or married (4)
RD estimate	-0.006	0.071	0.012	0.092
	(0.037)	(0.060)	(0.015)	(0.094)
	[0.862]	[0.533]	[0.533]	[0.533]
N	333	278	1,065	98

Notes: Each cell reports the RD estimate of the effect of the reform on the corresponding outcome. Estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months. Columns (1) and (2) use the sample of mothers who were cohabiting at the date of birth of their second child. Column (3) uses the sample of mothers who were married at the date of birth of their second child. Column (4) uses the sample of mothers who were single at the date of birth of their second child. Data are taken from the Enquête Etude de L'Histoire Familiale. Robust standard errors are reported in parentheses. Q-values or p-values adjusted for multiple inference using the False Discovery Rate method (Benjamini and Hochberg, 1995) are reported in brackets. When computing q-values, all outcomes listed in columns (1) through (4) are considered part of the same "family". (*** p < 0.01 ** p <0.05 * p <0.1).

Table A20: Effects of the reform on marital outcomes using different bandwidths

	BW=2 (1)	BW=4 (2)	BW=6 (3)	BW=16 (4)	BW=19 (5)	BW=22 (6)	BW=25 (7)	BW=28 (8)
A) Cohabiting before childbirth								
In same relationship	0.011	-0.006	0.006	-0.026	-0.018	-0.039	-0.033	-0.032
	(0.047)	(0.037)	(0.030)	(0.039)	(0.037)	(0.048)	(0.046)	(0.043)
With controls	0.010	0.001	0.004	-0.050	-0.031	-0.087*	-0.062	-0.058
	(0.050)	(0.038)	(0.030)	(0.039)	(0.036)	(0.047)	(0.045)	(0.042)
N	179	333	485	1,217	1,450	1,678	1,890	2,111
Unmarried	0.153*	0.071	0.089*	0.116*	0.109*	0.128	0.133*	0.139*
	(0.080)	(0.060)	(0.049)	(0.066)	(0.061)	(0.084)	(0.078)	(0.073)
With controls	0.168**	0.078	0.107**	0.120*	0.091	0.170**	0.153*	0.156**
	(0.082)	(0.060)	(0.049)	(0.066)	(0.061)	(0.084)	(0.078)	(0.073)
N	152	278	413	1,025	1,214	1,408	1,593	1,782
B) Married before childbirth								
In same marriage	0.010	0.012	0.005	-0.005	-0.005	-0.004	-0.007	-0.008
	(0.021)	(0.015)	(0.013)	(0.017)	(0.016)	(0.021)	(0.020)	(0.019)
With controls	0.009	0.011	0.003	-0.020	-0.012	-0.014	-0.016	-0.014
	(0.022)	(0.015)	(0.013)	(0.017)	(0.016)	(0.021)	(0.020)	(0.019)
N	535	1,065	1,553	4,264	4,993	5,781	6,676	7,486
C) Single before childbirth								
Cohabiting or married	0.074	0.092	0.015	0.074	0.054	0.113	0.100	0.069
	(0.131)	(0.094)	(0.074)	(0.103)	(0.093)	(0.131)	(0.123)	(0.114)
With controls	0.264*	0.191*	0.035	0.012	0.010	0.053	0.059	0.034
	(0.157)	(0.099)	(0.073)	(0.096)	(0.088)	(0.125)	(0.117)	(0.109)
N	53	98	159	501	604	697	782	884

Note: Each cell reports the RD estimate of the effect of reform on the corresponding outcome. Each column uses the listed bandwidth (BW). Columns (1) to (3) use the local randomization approach, while the rest of the columns use local linear regressions and a triangular kernel. Results are shown both with and without controls. Controls include second child's month of birth fixed effects, mother's age at the birth of the second child (and its square), as well as dummy variables for whether the second child is male, whether the mother is born in France, has a high school degree or more, and has a father who is a manual worker and is in a high skill occupation. Data are taken from the Enquête Etude de L'Histoire Familiale. Robust standard errors are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A21: Placebo tests for marital outcomes

	Cohabiting before childbirth		Married before childbirth	Single before childbirth
	In same relationship (1)	Unmarried (2)	In same relationship (3)	Cohabiting or married (4)
A) Cutoff is July 1992				
Local randomization	-0.072	0.079	-0.001	-0.083
	(0.047)	(0.071)	(0.017)	(0.093)
N	238	204	1,164	114
Local linear	-0.043	0.053	0.023	-0.161
	(0.047)	(0.075)	(0.019)	(0.098)
N	998	847	4,415	553
B) First child				
Local randomization	0.031	-0.048	-0.003	-0.094
	(0.029)	(0.047)	(0.015)	(0.074)
N	567	467	993	119
Local linear	0.035	-0.037	0.024	-0.029
	(0.031)	(0.051)	(0.016)	(0.077)
N	2,403	2,038	3,843	469

Note: Panel A reports the RD estimate of the effect of having a second child born after the fake cutoff July 1, 1992 on the corresponding outcome. Panel B reports the RD estimate of the effect of having a first child born after the July 1, 1994 cutoff on the corresponding outcome. In both panels, estimates are taken from regressions using the local randomization approach with a bandwidth of 4 months, as well as from local linear regressions using a bandwidth of 16 months and a triangular kernel. Columns (1) and (2) use the sample of mothers who were cohabiting at the date of birth of their child, column (3) uses mothers who were married at the date of birth of their child, and column (4) uses mothers who were neither married nor cohabiting at the date of birth of their child. Data are taken from the Enquête Etude de L'Histoire Familiale. Robust standard errors are reported in parentheses. (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).