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ABSTRACT

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

Many countries offer new parents long periods of paid leave. Proponents argue that parental leave programs can reduce gender gaps in the labor market, support marital stability and promote children's well-being. In this paper, I show that lengthy leaves can instead work against several of 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 discourages marriages among cohabiting couples and harms children's verbal development.

JEL Classification: J12, J13, J18, J22

Keywords: parental leave, household specialization, marriage, child development

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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. In 2017, Canada increased the length of parental leave from 12 to 18 months. 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, marital stability 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 three 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. Hence, parents gained access to almost three extra years of paid leave. Since the reform was announced after the cutoff date of July 1, 1994, parents had little

[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.

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 16 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 2.5 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. In section 5.2, I provide suggestive evidence that this is not the most likely channel by showing that men's earnings are unaffected by the reform and that the impact on work hours is driven by a decrease in absences, paid vacations and sick leaves. As a result, fathers could be instead spending more time at work in order to boost their chances of promotion and raise future earnings.

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, 2018). My results thus suggest that lengthy leaves can exacerbate gender differences in the labor market. While I can only test for leave-induced specialization in the short run, 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.

My second set of results indicate that eligibility for a long period of leave decreases couple stability. Although I find no significant effects on couple formation or dissolution, mothers who were cohabiting at the birth of their second child are around 10 percentage points less likely to marry within the next 4 years. The reduction in marital surplus could be driven by couples spending less time together due to specialization. Other channels that could lead to couple instability include a loss of household income and the drastic deviation from the pre-leave division of labor.

Finally, I 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 2.5 and 4.6 percentage points more likely to have below-average 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. In France, the vast majority of households rely on informal care—that is care provided by family members or childminders. In a recent study, Danzer et al. (2017) show that Austrian children benefit when maternal care replaces informal care. Hence, this channel is unlikely to explain my main results. Nonetheless, the reform likely substitutes maternal care for other childcare arrangements, and therefore reduces the amount of time that children spend with other adults and children. The adverse effects can 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 5.5, other channels that could explain the main results are the documented increase in couple instability and a potential loss of household income.

In summary, I show that providing a long period of paid leave reinforces a traditional division of labor within the household, discourages entry into marriage 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 and family stability. 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, marital stability 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, marital stability and child outcomes (Lalive and Zweimüller, 2009; Lalive et al., 2014; Danzer and Lavy, 2017; Danzer et al., 2017; Ginja et al., 2018). 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

[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, 2016; Mullerova, 2017).

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]

Third, I add to a growing literature that looks at marital responses to parental leave programs. Previous studies find that mothers' leave-taking does not affect the likelihood of divorce, but encourages entry into marriage and improves children's living arrangements (Dahl et al., 2016; Cygan-Rehm, Kühnle, and Riphahn, 2018; Danzer et al., 2017). In contrast, I show that parental leave can *discourage* cohabiting couples from entering into marriage. This contrasting result could be due to an increase in the time parents spend apart due to specialization. It could also be driven by the length of time that mothers are out of the labor market—3 years as opposed to 1 year at most in other studies.

Finally, my study builds on a large body of literature which investigates the relation between leave programs and children's outcomes. Studies looking at the *introduction* of paid and even 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. Section 5 describes the results and robustness checks. Finally, I conclude in section 6.

[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. (2018) 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.

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 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]

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

[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.

[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.

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*.

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 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*). Due to high demand, access to these services is usually limited. 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).

[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 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.

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 4 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 were eligible for APE benefits.[9] Accordingly, I restrict my sample to individuals who are observed in at least one of the first 4 years after the birth of their second child. In my main specifications, labor market outcomes are stacked for years two through four after birth, which results in up to three observations for each individual.[10]

[9] In results available upon request, I do not detect any significant effects on parents' labor market outcomes when APE eligibility expires.

[10] 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. Recall that the LFS is conducted in March of every year. Including data until the fourth year after birth —as opposed to the third year —ensures that parents can be observed for the full APE duration, regardless of their child's date of birth. Specifically, if a child is born in July 1994, limiting the data to March, 1997 would allow parents to appear in the sample for a maximum of 2 years and 8 months after birth. Expanding the sample until March, 1998 ensures that the entire 3 years of APE are covered.

Results for the first year are reported separately.

Appendix Table A1 displays means for parents' main labor market outcomes. Benefit receipt was conditional on parents either being out of the labor force, working part-time for less than 20 hours a week or working part-time between 20 and 32 hours a week. Since I do not have data on APE take-up, I focus on these outcomes to understand whether parents responded to the new benefits. Corresponding means are shown separately for each year after the birth of the second child in columns 1 through 4. Compared to fathers, the share of mothers who are out of the labor force is significantly higher at almost 40% (as opposed to only 2% for fathers). Conditional on being employed, around 6% of mothers work part-time for less than 20 hours a week and 30% work part-time for 20-32 hours per week (as opposed to 2% of fathers).

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.[11] Although not shown in this draft, my results are robust to the inclusion of these individuals. In my main sample, fathers work an average of 42 usual and 40 actual hours per week.

Panel A of Appendix Table A2 shows means for demographic characteristics. 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 41% of mothers and 32.8% of fathers have a high school degree or higher.[12] I proxy parents' socioeconomic status by their fathers' occupations. 40% of parents have a father who is a manual worker, while 10% have fathers who are in high-skilled or managerial occupations.

3.2 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

[11] This excludes 0.32% of fathers in my main sample.

[12] Specifically, these are individuals who have a *Baccalauréat* degree or higher. 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.

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 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).[13] 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.

Panel B of Appendix Table A2 presents means for marital outcomes reported in 1999, around 4-5 years after the birth of the second child. 86.9% of cohabiting mothers are still in the same relationship, while 53.6% report not having been married. For mothers who were married prior to the second child's birth, 93% are still in the same relationship, while 68.9% of single mothers are now either married or cohabiting.

3.3 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.

I restrict the sample to all second-born children. Panel C of Appendix Table A2 reports means for their main outcomes. 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.[14]

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

[14] The five tests are conducted as follows. The phonological awareness test focuses on whether the child is aware of

The survey does not report exact scores but rather whether the child has an average score or a score that is 1 to 2, or 3 standard deviations below average. For each test, I create a dummy variable that is equal to one if the child has an average score, and zero if he/she scores below average. The share of children in my sample who have average scores on these tests varies between 82 and 95%, with oral comprehension having the highest passing rate.

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, marital stability 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. Formally, I estimate the following reduced form equation:

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

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. In most specifications, 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. The coefficient of interest, β, captures intent-to-treat (ITT) effects of parental leave eligibility on various outcomes. To get the 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 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 below average 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.

In most specifications, I use uniform kernel weights and a bandwidth of 16 months, which is chosen by the robust data driven procedure introduced by Calonico, Cattaneo and Titiunik (2014).[15] I show that results are robust to the use of different bandwidths and inclusion of controls. Finally, standard errors are clustered by the second child's month-year of birth to deal with concerns over random specification error resulting from a discrete running variable (Lee and Card, 2008).

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. In what follows, I present empirical tests to alleviate concerns over manipulation of the assignment variable.

The first informative test is to look for bunching in the distribution of the running variable around the eligibility threshold (McCrary, 2008). Panels (a) and (b) of Figure I respectively plot the number and fraction of second-born children in each month using data within 30 months around the cutoff. Although some seasonal patterns emerge, the figures do not reveal a drop followed by a sharp increase in second births at the threshold. This is consistent with the ex-ante belief that parents have little opportunity to manipulate the date of birth of their second child.

Another test of validity of the RD design is to show that the distribution of pre-determined characteristics is continuous around the threshold. Appendix Figures A1 and A2 plot various baseline covariates as a function of the running variable. These covariates are mother's and father's ages at the birth of their second child, 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. Further, I use data that is within 16 months on either side of the threshold. 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 are smooth around the cutoff and regression estimates, presented over a range of bandwidths in Appendix Table A3, are statistically insignificant.

[15] Although the bandwidths chosen by the CCT procedure can be slightly different depending on the outcome, I fix the main bandwidth at 16 months to get a consistent sample.

5 Results

5.1 Mothers' Leave-Taking

I start by showing that mothers did take up 3 years of leave following the reform. Since data on leave take-up are not available, I leverage the fact that it was conditional on parents either being out of the labor market or working part-time. Figure II graphically depicts the relationship between mothers' labor supply in the second through fourth years after second child's birth and distance to the cutoff. Panel (a) reveals a large discontinuity, which indicates that 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. Using a linear regression, I estimate that the magnitude of this jump, shown in column 1 of Table I, is around 16.7 percentage points. This estimate slightly drops to 14.2 percentage points with the inclusion of controls and second child's month of birth fixed effects. In contrast, the figures for part-time work in panels (b) and (c) are smooth around the cutoff, and the corresponding estimates in columns 2 and 3 of Table I are small and not statistically significant. Consistent with previous findings by Piketty (2005), these results suggest that mothers took up the cash benefits mainly through exiting the labor market.

I next examine the characteristics of women who take up the benefits. 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. Panel (d) of Figure II and column 4 of Table I reveal a 10.6 to 13.7 percentage points decrease in the share of mothers who are employed, based on specifications with and without controls. This implies that the documented decrease in labor force participation is largely driven by employed mothers leaving the workforce. Panel (e) of Figure II and column 5 of Table I further indicate that women are 15.6 to 17.2 percentage points more likely to declare that they are stay-at-home mothers. Panel (f) reveals a concurrent 17.4 percentage points drop at the cutoff in the share of women who report having a low or middle skill job as their main occupation.[16] These results suggest that the availability of APE benefits mostly incentivizes women in low and middle skill occupations to leave employment in order to care for their children.

While it would be interesting to document the incidence and magnitude of income loss due

[16] I follow the LFS occupational classification. Low-skilled occupations comprise cleaning and maintenance workers such as janitors and housekeepers, as well as personal care and service jobs such as childcare and food preparation workers, hairdressers, cashiers, waitresses, etc. These are mostly female-dominated jobs and are different than the manual workers category which involves laborers, machine operators, helpers, transportation and material moving occupations,... Middle-skilled occupations include school teachers, secretaries, various healthcare workers such as nurses, massage therapists and dental assistants, as well as various clerks and technicians, etc. In results available upon request, I find that the reform has no impact on the share of women in other occupations.

to mothers leaving the labor force, the relevant data are not available. The LFS only contains information on employed individuals' wages. Panel (g) of figure II plots mothers' natural log of monthly wages as a function of the running variable. The figure along with the estimate in column 7 of Table I reveal no significant threshold-crossing effects. This is consistent with women not changing their labor supply at the intensive margin. Appendix Table A4 displays for all outcomes, the regression estimates using different bandwidths and functional forms and shows that the main results are not sensitive to varying specifications.[17]

5.2 Fathers' Labor Supply

Since both parents are eligible to take the benefits, I examine whether fathers are also incentivized to take the leave in the second through fourth 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). Panels (a) through (c) of Figure III respectively plot fathers' likelihoods of being out of the labor force, working part-time for less than 20 hours a week, as well as working part-time between 20 and 32 hours a week. None of the figures reveal a discontinuity at the cutoff and the estimates in columns 1 to 3 of Table II are small and statistically insignificant. These results imply 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 panels (d) and (e) of Figure III and columns 4 and 5 of Table II. I find no 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, I observe a positive shift in actual work hours at the cutoff. Specifically, men with a second child born just after the cutoff work an extra 2.5 hours per week, compared to fathers of children born right before.

[17] In Appendix Figure A3 and Table A5, I further show that mothers' labor market results in the first year after childbirth are similar to the ones from the second through fourth years.

The fact that fathers are altering their actual but not usual hours of work implies that they are either increasing their overtime work or reducing their absences or take-up of vacations. Panel (f) of Figure III reveals that men are 4.3 percentage points less likely to be absent from work for personal reasons during the reference week following the reform.[18] On the other hand, panel (g) of Figure III shows that the reform has no impact on absences that are less likely to be influenced by employees such as work reductions due to bad weather or business slowdown, partial unemployment, labor disputes (i.e. strikes, lockouts), phased retirement, etc.

I next examine the impact of the reform on men's natural log of wages. Changes in wages can indicate that men are working overtime in order to compensate for a potential loss of household income due to mothers' take-up of the leave. This is possible since the APE does not provide high income replacement but rather a fixed monthly cash allowance, and mothers completely exit the labor force for three years thereby forgoing their wages. Fathers' wages in panel (h) of Figure III are smooth around the cutoff and I detect no significant treatment effects in column 8 of Table II. This implies that the documented impact on actual work hours is mostly driven a reduction in men's absences and not an increase in overtime work. It also suggests that men are not necessarily working more in order to compensate for a loss of household income, but rather to increase their chances of promotion or raise future earnings.[19] Taken together, the results suggest that offering long periods of parental leave increases household specialization for an extended time period.[20]

Intra-household specialization is likely to occur when one spouse has a comparative advantage in the labor market and the other in home production (Becker, 1981). I therefore explore how the reform affects specialization when spouses have differing comparative advantages. Panels (a) and (b) of Figure IV plot the main labor outcomes for couples in which mothers are equally or less educated than fathers. Since education increases earning potential, I consider these to be couples in which the husband has an advantage in market work and the wife in non market work.[21] As expected, mothers with a comparative advantage in home production are around 16 percentage points more likely to exit the labor force, and fathers with an advantage in the labor market work approximately 4 extra hours per week (columns (1) and (2) of Table III) due to the reform.

[18]This variable includes absences that the employee can directly control such as annual leaves, leaves of absence for personal reasons, vacations and sick leaves.

[19]Appendix Table A6 shows that regression discontinuity estimates for fathers' labor outcomes are robust to using different bandwidths, functional forms and the inclusion of controls.

[20]In Appendix Figure A4, I show results for fathers' main outcomes in the first year after the second child's birth. The figures for labor force participation, part-time work, actual and usual work hours do not exhibit any clear patterns at the cutoff. The corresponding regression estimates in Appendix Table A7 do not reveal any significant and persistent threshold-crossing effects for all four outcomes. However, results are imprecisely estimated given the reduced sample size. This precludes me from drawing definitive conclusions regarding fathers' behavior in the first year after the birth of the second child.

[21]Spouses that are equally educated are considered to be couples in which the husband has an advantage in the labor market. This is because women in France earn on average 15% less than men who work in the same occupations and have the same level of education.

Panels (c) and (d) of Figure IV present figures for the same outcomes using the sample of couples in which women are more educated than men—that is women have a comparative advantage in the labor market. I find no significant threshold-crossing effects for men's actual work hours (column (4) of Table III), which is consistent with their lack of comparative advantage in the labor market. Although women in this sample have potentially higher earning power than men, they are still around 17 percentage points more likely to be out of the labor force (column (3) of Table III). This is in line with recent findings by Bertrand, Kamenica and Pan (2015) who argue that for women who earn more than their husbands, gender norms may distort labor market behavior. Specifically, females decrease their labor force attachment and take on a higher share of the housework, in order to appear less "threatening" to the man's position as the main breadwinner and mitigate the reversal of traditional gender roles.

5.3 Marital Stability

Since eligibility for the leave increases intra-household specialization for an extended time period, it may also affect couples' stability albeit the direction of this effect is ambiguous. In a standard Becker model, specialization is expected to reduce couple dissolution and increase marriage rates. A traditional division of labor 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). Under this hypothesis, access to a long period of parental leave is expected to have positive effects on couples' well-being.

However, technological advancements and changes in women's position in the labor market decreased the gains from specialized investments over time. Gains from consumption complementarities—such as the utility from sharing leisure activities and spending time together—have become a more important source of marital surplus (Stevenson and Wolfers, 2007). Some studies further document a negative association between couples' time together and the level of household specialization (Mansour and McKinnish, 2014). In that case, having access to a long period of parental leave could decrease the gains to marriage if it causes parents to specialize and spend less time together. Furthermore, 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, long leaves could threaten marital stability if it causes couples to drastically deviate from their initial division of labor. This is particularly relevant given that the reform induces previously employed mothers to completely exit the labor force, and fathers to increase their work involvement for an extended period of time. Finally, a potential loss of household income has ambiguous effects on couples' well-being (Burstein, 2007).

To understand whether and how the reform affects couple stability, I start by looking at the sample of mothers who were cohabiting but not married at the date of birth of their second child. Panels (a) and (b) of Figure V along with the corresponding estimates in columns (1) and (2) of Table IV, show that the probability of being in the same pre-birth relationship is unaffected, but mothers are 9 to 11 percentage points less likely to be married almost five years later. In other words, although the reform does not affect couple dissolution, it does deter entry into marriage. This result may seem surprising at first since household specialization is typically expected to increase gains to marriage. However, it is consistent with alternative explanations such as couples valuing time spent together, a large deviation between couples' expected and realized labor outcomes and a possible loss of household income.

In Panel (c) of Figure V, 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 IV, indicating that the reform does not affect divorce rates. The lack of threshold-crossing effects on couple/marriage dissolution does not necessarily conflict with the negative impact on entry into marriage. This could reflect the fact that couple/marriage dissolution is more costly than not upgrading to marriage.[22]

5.4 Children's Development

5.4.1 Impact of Extended Leave 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 as documented in section 5.1, mothers were induced to exit the labor market for at least three years due to eligibility to receive the benefits. Panels (a) through (e) of Figure VI plot the various measures of children's verbal development—discussed in section 3.3—as a function of the running variable and using data within 6 months on either side of the cutoff.[23] 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 reported in columns (1) through (5) of Panel A of Table V are consistent with the visual evidence. Specifically, children

[22] Results for all marital outcomes are robust to different bandwidths as shown in Panels A and B of Appendix Table A8. In Panel C, I focus on mothers who were neither married nor cohabiting at their second child's birth. For this sample, I cannot detect significant effects on the likelihood of entering marriage or cohabitation, but precision is reduced given the smaller sample size. Nonetheless, the lack of threshold-crossing effects is reassuring given that single mothers did not benefit from the APE program.

[23] The chosen bandwidth for these results is 6 months since I only have data on children born in 1994, which prohibits me from using larger bandwidths.

of eligible parents are between 2.5 and 4.6 percentage points less likely to have average scores on various verbal development tests. The regression estimates do not change when I include fixed effects for the date in which the tests were administered and a dummy variable for whether the child is male. 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.[24] Consistent with the findings for the individual outcomes, the children's verbal development index is negatively affected by the reform, as shown in Panel (f) of Figure VI and column (6), Panel A of Table V.

5.4.2 Mechanisms

Taken together, these results suggest that children of mothers who take a long period of leave are adversely affected, as they are more likely to have below average scores on tests that assess their verbal development. There are several channels that could explain these negative effects. First, as shown in section 5.1, the reform increases the likelihood that women are stay-at-home mothers and as a result, likely decreases 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 three—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%). I test whether leave take-up delays children from entering preschool. This is possible since mothers can take the benefits until the child's third birthday and children can be enrolled in preschool as early as age 2. In panels (g) and (h) of Figure VI, I respectively plot the age (in months) at which the child started preschool and the number of months that the child has been enrolled in preschool by the survey date, as a function of the running variable. The figures show no discontinuities at the cutoff and the estimates in columns (7) and (8) of Panel A of Table V are small and not statistically significant, indicating that crowding out of preschool is not a likely mechanism for the documented negative effects.

Maternal care is likely primarily replacing informal care and care provided by childminders. Therefore, a key question is whether maternal care is of lower quality than these two methods of childcare. Recent studies can help shed light on this question. 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. Their results suggest that care provided by mothers

[24] 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.

and nurseries is of higher quality than informal care (i.e. care provided mainly by grandparents but also relatives and childminders). However, this does not rule out that maternal care can, in some instances, be of lower quality than informal care. For example, Danzer and Lavy (2017) show that children of lower-educated women are harmed while those with higher-educated mothers benefit from the Austrian leave—where maternal care primarily displaced informal care.

To provide evidence on this channel, I look at heterogeneity in children's outcomes by socioeconomic background. Specifically, if the overall negative effects on children are entirely driven by those who are from a low socioeconomic background, this could imply that the main channel driving these effects is maternal care being of lower quality than informal care or care provided by childminders. 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). The ZEP is a program that is intended to provide additional funds for primary schools and high schools (but not to preschools or nurseries) that are located in disadvantaged areas. Therefore, I consider a child to be from a low socioeconomic background if he/she resides in a ZEP area. Panels B and C of Table V report the impact of the reform on children who reside in ZEP and non-ZEP areas, respectively.[25] Across all outcomes, the estimates for children from low socioeconomic backgrounds are larger than for those from higher socioeconomic backgrounds. This suggests that part of the main results could be driven by increased exposure to mothers from low socioeconomic backgrounds, who might provide lower quality care than other forms of childcare.[26] Nonetheless, the reform also has significant adverse impacts on the verbal development tests of children from higher socioeconomic backgrounds. This implies that my results cannot be fully explained by maternal care crowding out higher quality childcare arrangements.

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'

[25] In results available upon request, I find that the reform has no impact on the likelihood of residing in a ZEP area, ruling out concerns over selection into ZEP/non-ZEP samples. As a robustness check, I also find no threshold-crossing impacts on the likelihood that the child is male and the date of the survey exam.

[26] Note that for the sample of children residing in a ZEP area, there seems to be an increase in the age at which children enter preschool by around 1.65 months. This implies that part of the negative effects on ZEP children is potentially explained by crowding out of preschool. No similar effects can be detected for non-ZEP children.

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). If decreased time with fathers is the main channel driving children's effects, we should see an increase in work hours for men from both high and low socioeconomic backgrounds since both low and high SES children are harmed by the leave extension. I look for heterogeneous effects in couples' labor supply by fathers' level of education in columns (1) to (4) of Appendix Table A9. Most of the increase in work hours is driven by fathers with a high school degree or more, who work 5.4 extra hours per week due to the leave extension. While less time spent with fathers could explain part of the effects on high SES children, it is not the only mechanism behind children's effects.

Fourth, as shown in section 5.3, eligibility for a long period of leave affects couple stability. Previous studies suggest that increased marital instability might have adverse effects on a wide range of child outcomes (Gruber, 2004). To understand whether increased marital instability is a channel that could be driving the effects on both low and high SES children, I look at heterogeneous effects by mothers' level of education. Column (5) of Appendix Table A9 indicates that mothers with less than a high school degree who are cohabiting at the birth of their second child are 10 percentage points less likely to be married. For mothers with a high school degree or more, the estimate in column (7) is not statistically significant due to reduced sample size but I cannot rule out large effects. These results suggest that increased couple instability could be a main channel driving the adverse impacts on children from both low and high socioeconomic backgrounds. Finally, 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).[27]

It is difficult to pinpoint the main channel driving the adverse effects on children. Exposure to lower quality care could drive some of the effects for low SES children, and less time with fathers can negatively impact high SES children. However, these channels are not common between high and low SES children who are both adversely impacted by the reform. Therefore, they do not provide the most compelling explanations for the main results. Instead, the effects on children are potentially driven by decreased interactions with adults and children, increased couple instability and a potential loss of household income.

[27]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 4 years after the second child's birth.

5.5 Robustness Checks

5.5.1 Parents' Outcomes

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. Panels (a) and (b) of Appendix Figure A5 respectively plot mothers' likelihood of being out of the labor force and fathers' actual hours of work—in the second through fourth years after birth—as a function of second child's month-year of birth. 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 both parents' labor force participation, part-time work, mothers' probability of being employed and fathers' usual and actual work hours in Panel A of Appendix Table A10. No significant threshold-crossing effects can be detected for any of these outcomes, which is consistent with the main results not being sensitive to the inclusion of second child's month-of-birth fixed effects.

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 or shock affected all children born in July 1, 1994. Panels (c) and (d) of Appendix Figure A5 show mothers' labor force participation and fathers' actual work hours as a function of first child's month-year of birth—using data from the second through fourth years after first child birth. The figures reveal no discontinuities and regression estimates for all main labor market outcomes are statistically insignificant (Panel B of Appendix Table A10).

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 (Panels (e) and (f) of Appendix Figure A5). The corresponding regression estimates reported in Appendix Table A11 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.

Similarly, Appendix Figure A6 and Appendix Table A12 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.

5.5.2 Children's Outcomes

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 A7 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:

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

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 the different columns of Appendix Table A13. For various outcomes, results are consistent with the ones from the main regression discontinuity design, as they are close in magnitude and remain mostly statistically significant at conventional levels. This indicates that the RD design is indeed capturing the effect of leave eligibility on children's outcomes.

6 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 option 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. Additional results suggest that parents are substituting their time in home production, and men do not seem to be increasing their labor supply to compensate for a loss of income due to mothers' leave take-up. I then look at responses in the marriage market. While I find no evidence of leave expansion affecting divorce or couple separation, cohabiting mothers are less likely to enter marriage. This potentially implies that household specialization can reduce marital surplus if it decreases the time couples spend together. Finally, I document that leave eligibility harms children's verbal development at ages 5 to 6. Several mechanisms can explain this finding. Children could be adversely affected through a reduction in their social interactions if maternal care is substituting for other chidlcare arrangments. The documented couple instability could have also hurt child development. Additionally, the program I look at does not offer full income replacement, thus I am unable to rule out loss of household income as a potential channel that could also be driving the documented results.

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 promote family stability and 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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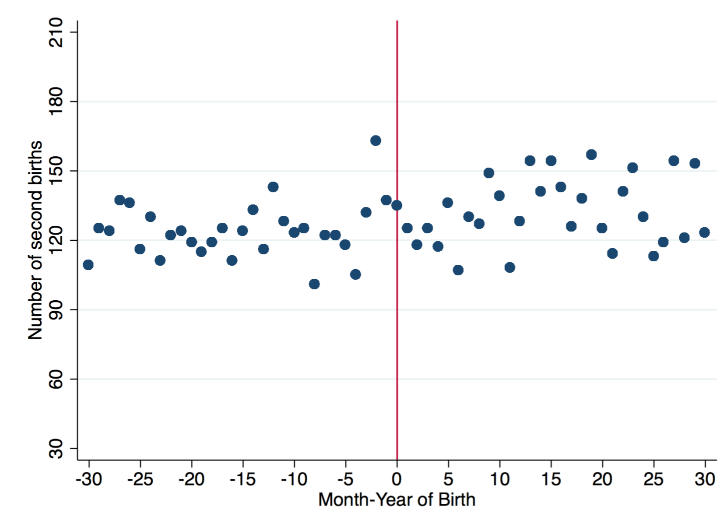
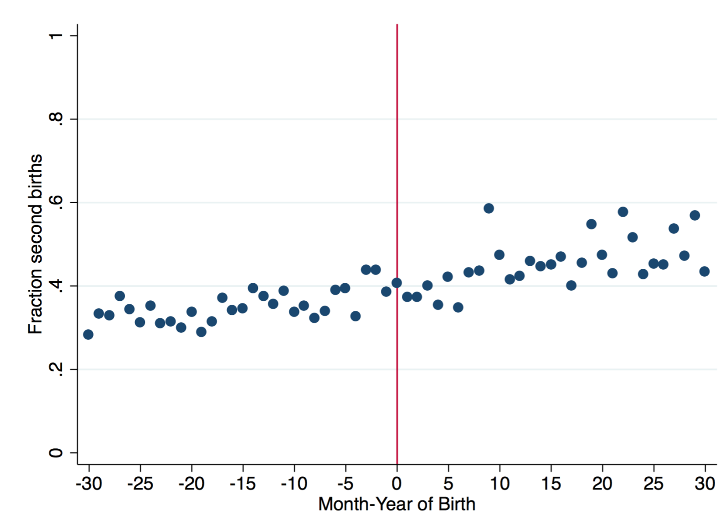
A Figures and Tables

Figure I: Number and fraction of second births



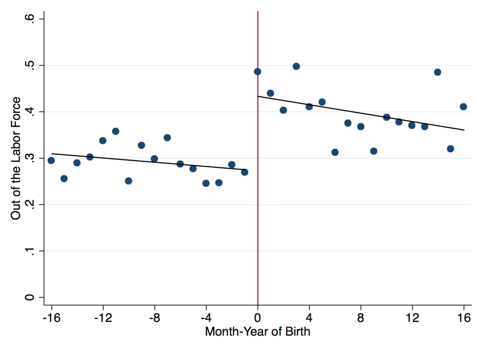
(a) Number of second births



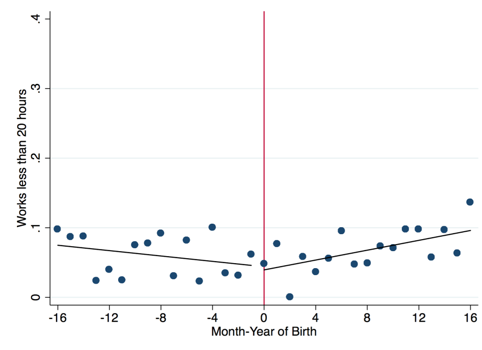
(b) Fraction of second-born children

Notes: Data are taken from the French Labor Force Survey. The "number of second births" is the number of second children born in each month-year whose parents are observed at least once between the first and fourth year after their birth. The "fraction of second-born children" divides the number of second-borns in a given month-year by the number of all births in that same month-year.

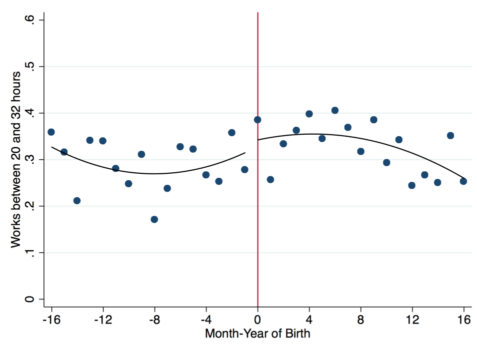
Figure II: Results for Mothers' Leave-Taking



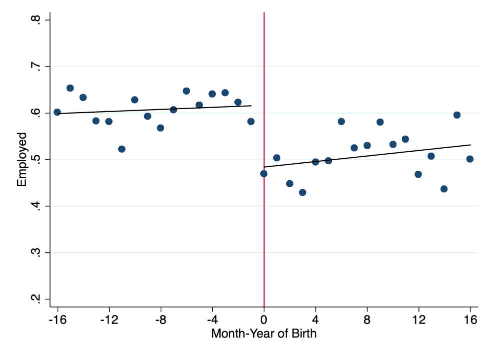
(a) Out of the labor force



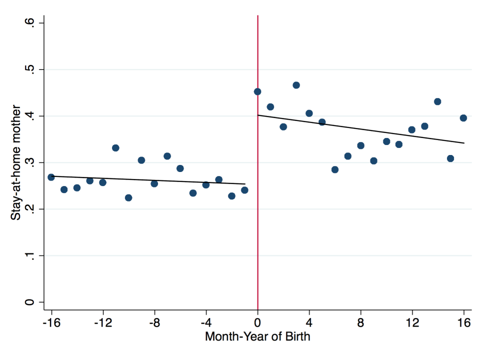
(b) Works part-time less than 20 hours



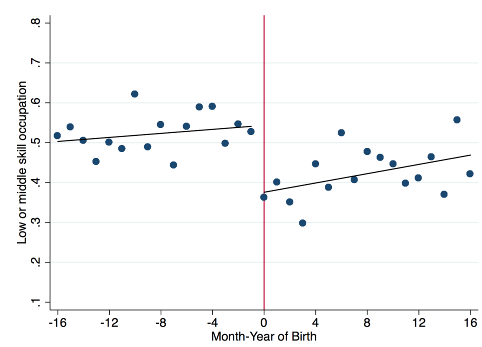
(c) Works part-time between 20 and 32 hours



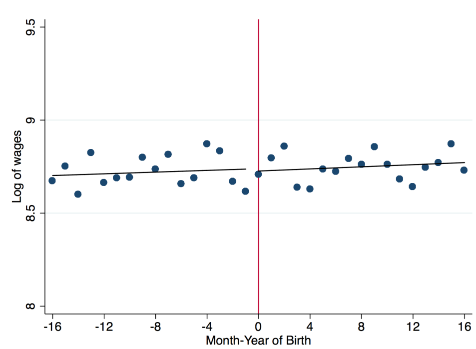
(d) Is Employed



(e) Is stay-at-home mother



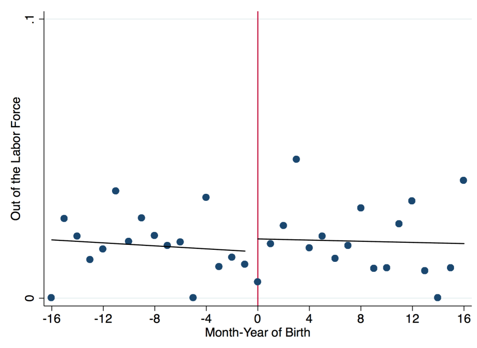
(f) Is in low to middle skill occupation



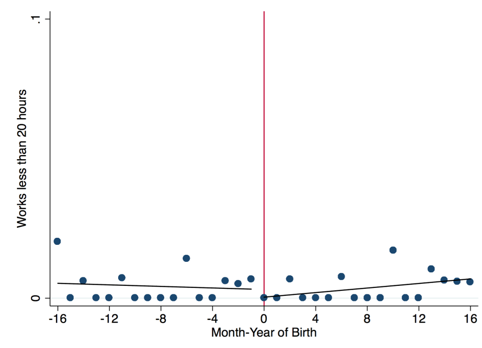
(g) Natural log of wages

Notes: The different panels show mothers' labor market outcomes in the second through fourth 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 specifications with a bandwidth of 16 months.

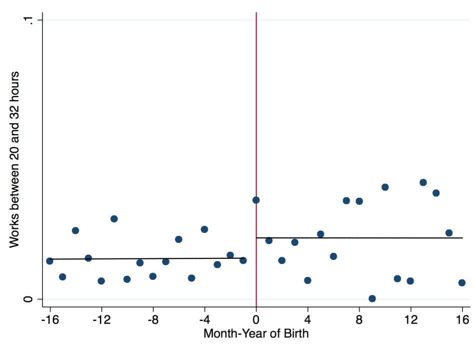
Figure III: Results for Fathers' Labor Supply



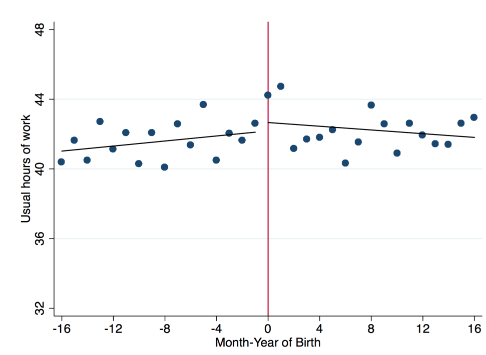
(a) Out of the labor force



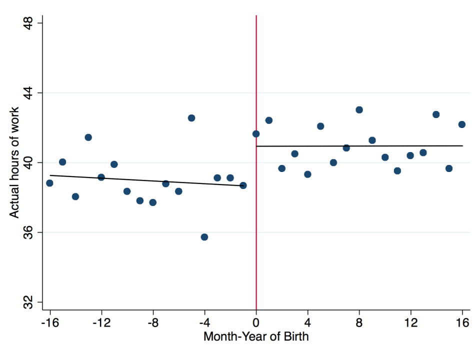
(b) Works part-time less than 20 hours



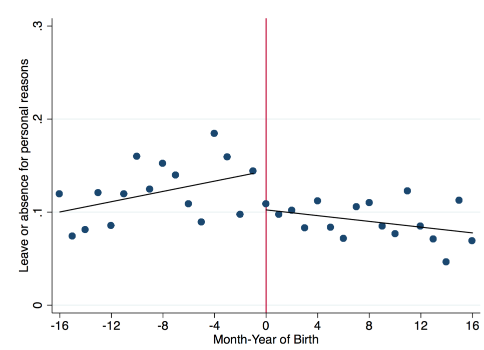
(c) Works part-time, between 20 and 32 hours



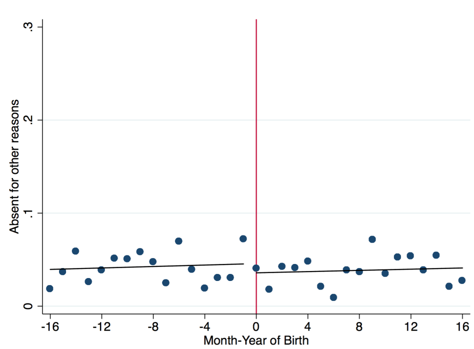
(d) Usual hours of work



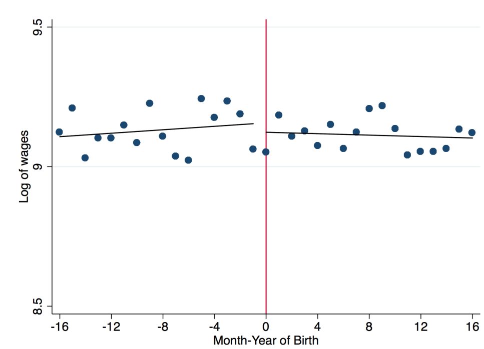
(e) Actual hours of work



(f) Is absent for personal reasons



(g) Is absent for other reasons

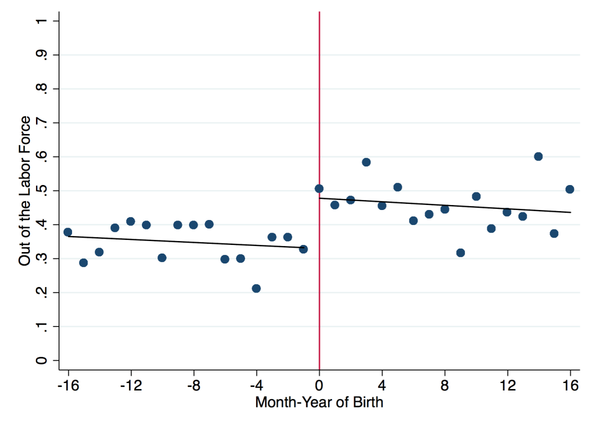


(h) Natural log of wages

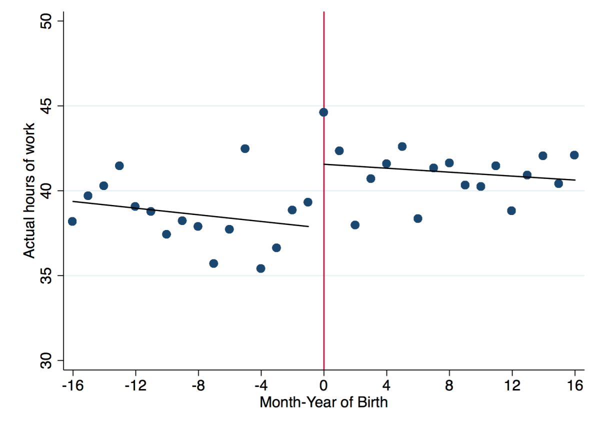
Notes: The different panels show fathers' labor market outcomes in the second through fourth years after a second child's birth, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 16 months.

Figure IV: Heterogeneous effects by parents' relative education

Mother less or equally educated as father

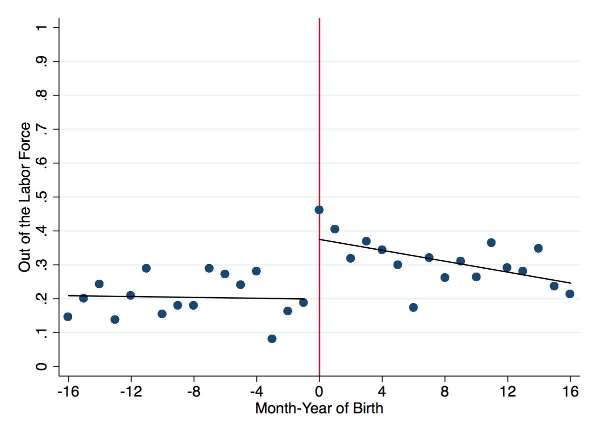


(a) Mothers out of the labor force

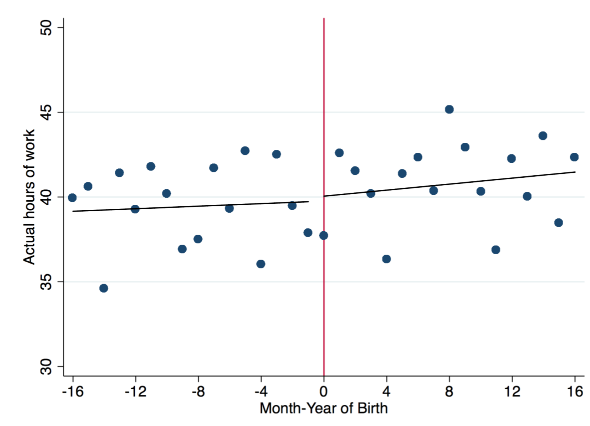


(b) Fathers' actual hours of work

Mother more educated than father



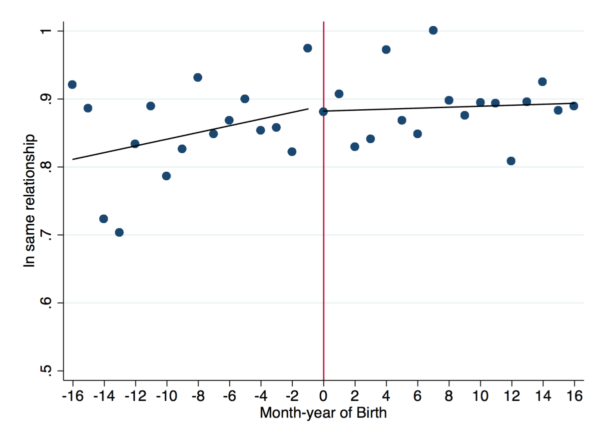
(c) Mothers out of the labor force



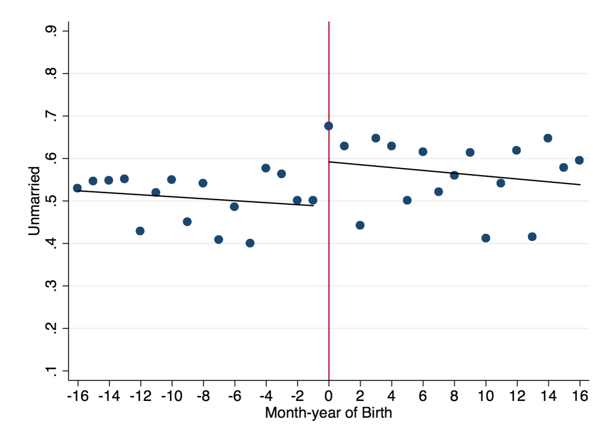
(d) Fathers' actual hours of work

Notes: The different panels show parents' labor market outcomes in the second through fourth 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 specifications with a bandwidth of 16 months. Panels (a) and (b) are from a sample of households in which mothers are less than or equally educated as fathers, while panels (c) and (d) are from a sample in which mothers are more educated than men.

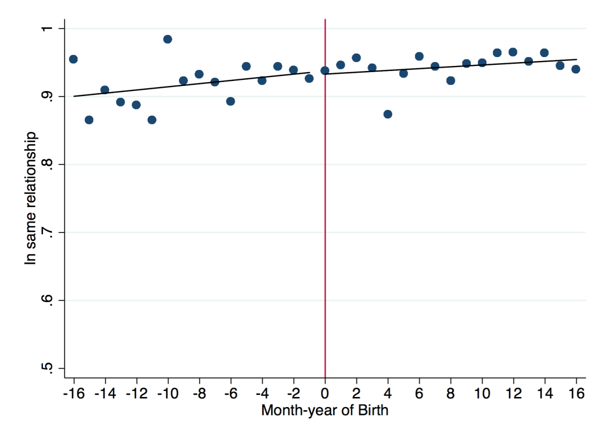
Figure V: Results for marital outcomes



(a) Cohabiting mothers in same relationship



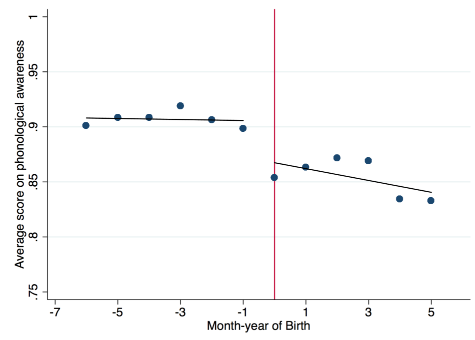
(b) Cohabiting mothers not married



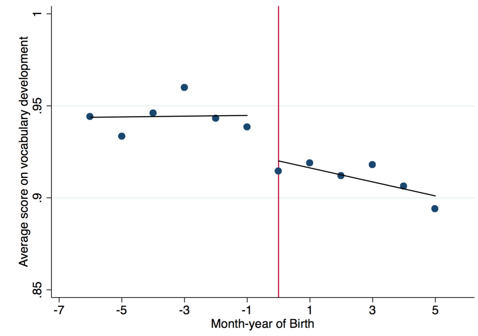
(c) Married mothers in same relationship

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 specifications with a bandwidth of 16 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 birth of their second child.

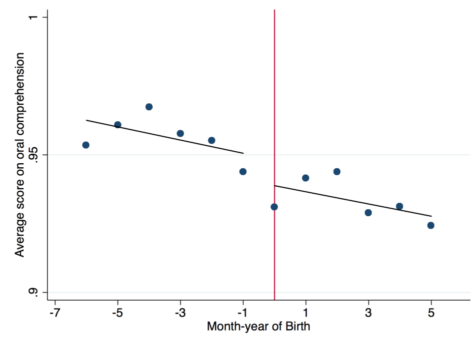
Figure VI: Results for children’s outcomes



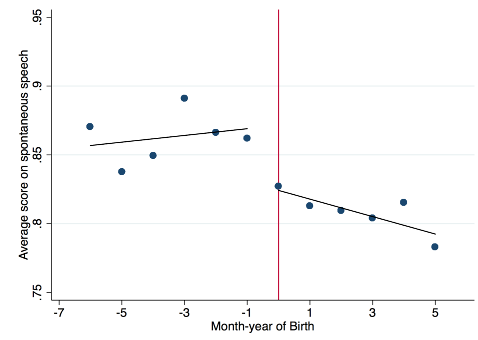
(a) Score on phonological awareness is average



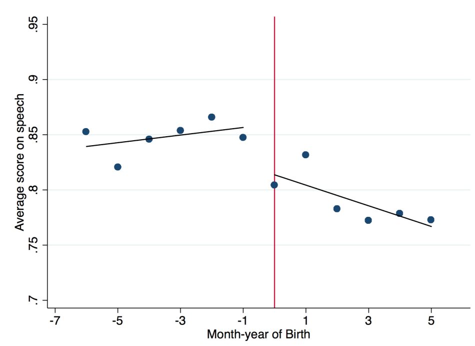
(b) Score on vocabulary development is average



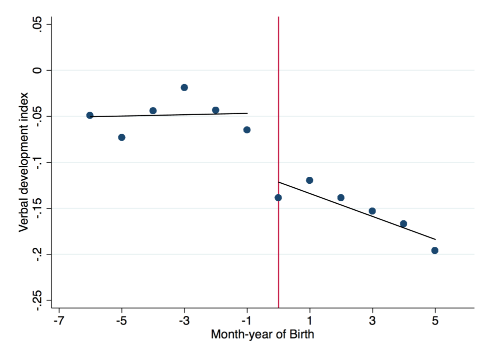
(c) Score on oral comprehension is average



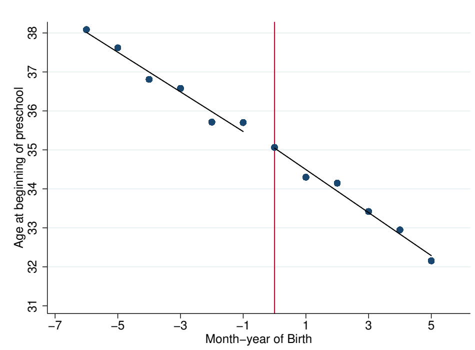
(d) Spontaneous speech is average



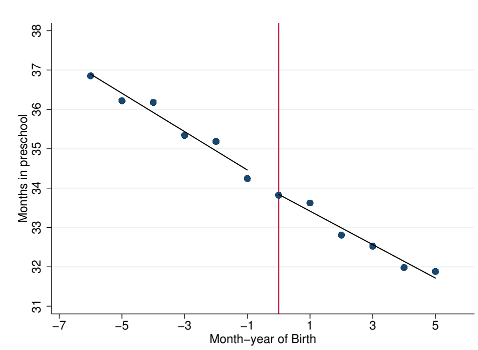
(e) Overall speech is average



(f) Verbal development index



(g) Age at beginning of preschool (in months)



(h) Number of months in preschool (by survey date)

Notes: The different panels show second children’s outcomes measured at ages 5-6 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 specifications with a bandwidth of 6 months.

Table I: Regression estimates for mothers' labor market outcomes

	Out of the labor force (1)	Works part-time less than 20 hours (2)	Works part-time, between 20 and 32 hours (3)	Employed (4)	Stay-at-home mother (5)	In low or middle skill occupation (6)	Natural log of wages (7)
No Controls	0.167***	0.001	0.015	– 0.137***	0.156***	– 0.174***	– 0.019
	(0.025)	(0.017)	(0.055)	(0.023)	(0.024)	(0.030)	(0.062)
With Controls	0.142***	0.003	0.007	– 0.106***	0.172***	– 0.184***	– 0.027
	(0.023)	(0.016)	(0.082)	(0.018)	(0.021)	(0.030)	(0.037)
Polynomial	One	One	Two	One	One	One	One
Observations	5,255	2,932	2,932	5,255	5,255	5,255	2,638

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are respectively taken from regressions without and with controls and using a bandwidth of 16 months. 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, are higher-educated, and have fathers who are manual workers or are high-skilled. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table II: Linear regression estimates for fathers' labor market outcomes

	Out of the labor force (1)	Works part-time, less than 20 hours (2)	Works part-time, between 20 and 32 hours (3)	Usual hours of work (4)	Actual hours of work (5)	Absent for personal reasons (6)	Absent for other reasons (7)	Natural log of wages (8)
No Controls	0.008	– 0.003	0.005	0.636	2.469***	– 0.043**	– 0.012	– 0.031
	(0.008)	(0.003)	(0.006)	(0.761)	(0.888)	(0.018)	(0.012)	(0.047)
With Controls	0.007	– 0.001	0.006	0.649	2.558***	– 0.046**	– 0.012	0.011
	(0.008)	(0.003)	(0.007)	(0.551)	(0.826)	(0.013)	(0.010)	(0.020)
Observations	5,255	4,856	4,856	4,015	4,840	4,015	4,015	4,114

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are respectively taken from linear regressions without and with controls and using a bandwidth of 16 months. 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, are higher-educated, and have fathers who are manual workers or are high-skilled. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table III: Heterogeneity for parents' labor market outcomes

	Mother Less Educated than Father		Father Less Educated than Mother	
	Mother out of the labor force (1)	Father's actual hours (2)	Mother out of the labor force (3)	Father's actual hours (4)
No controls	0.159***	3.988***	0.173***	0.383
	(0.034)	(1.353)	(.046)	(1.618)
With controls	0.156***	4.412***	0.165***	0.077
	(0.039)	(0.978)	(0.038)	(1.322)
Observations	3,124	2,863	2,131	1,977

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions using a bandwidth of 16 months and respectively without and with 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, are higher-educated, and have fathers who are manual workers or are high-skilled. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table IV: Local linear regression estimates for marital stability

	Cohabiting before birth		Married before birth
	In same relationship (1)	Unmarried (2)	In same relationship (3)
No controls	– 0.009	0.112**	– 0.006
	(0.039)	(0.047)	(0.014)
With controls	– 0.028	0.091**	– 0.006
	(0.030)	(0.043)	(0.014)
Observations	1,217	1,025	4,264

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are respectively taken from linear regressions without and with controls and using a bandwidth of 16 months. 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, is higher-educated, and has a father who is a manual worker or is high-skilled. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

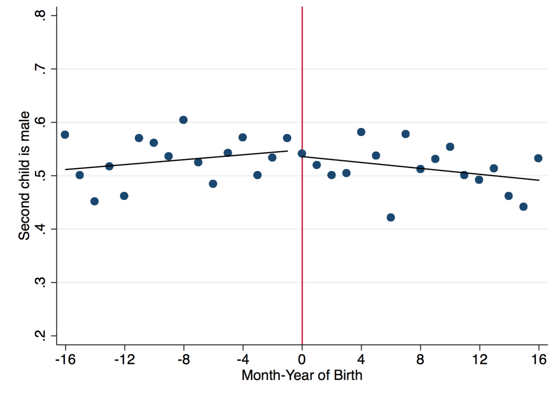
Table V: Regression estimates for children's outcomes

	Phonological Awareness (1)	Vocabulary Development (2)	Oral Comprehension (3)	Spontaneous Speech (4)	Overall Speech (5)	Verbal Development Index (6)	Age at beginning of preschool (7)	Time in preschool (8)
A. Overall Sample								
No controls	– 0.038*	– 0.025**	– 0.009	– 0.047**	– 0.046*	– 0.075***	0.084	– 0.132
	[0.068]	[0.038]	[0.466]	[0.024]	[0.092]	[0.010]	[0.718]	[0.598]
With controls	– 0.038*	– 0.025**	– 0.010	– 0.047**	– 0.046	– 0.075***	0.073	– 0.073
	[0.064]	[0.032]	[0.496]	[0.020]	[0.106]	[0.006]	[0.772]	[0.774]
Observations	6,215	6,210	6,210	6,459	6,780	9,316	8,781	8,781
B. In ZEP								
No controls	– 0.043	– 0.072**	0.011	– 0.135*	– 0.084	– 0.153*	1.600*	– 1.687**
	[0.412]	[0.015]	[0.806]	[0.059]	[0.102]	[0.084]	[0.064]	[0.050]
With controls	– 0.039	– 0.070**	0.011	– 0.128*	– 0.085*	– 0.151*	1.647*	– 1.648*
	[0.430]	[0.015]	[0.796]	[0.055]	[0.054]	[0.080]	[0.074]	[0.076]
Observations	869	861	859	874	948	1,161	1,096	1,096
C. Not In ZEP								
No controls	– 0.037**	– 0.017*	– 0.013	– 0.033**	– 0.039*	– 0.065***	– 0.144	0.103
	[0.044]	[0.088]	[0.139]	[0.016]	[0.086]	[0.008]	[0.472]	[0.558]
With controls	– 0.037**	– 0.018*	– 0.014	– 0.034**	– 0.039	– 0.064***	– 0.145	0.145
	[0.042]	[0.082]	[0.221]	[0.012]	[0.134]	[0.008]	[0.447]	[0.444]
Observations	5,346	5,349	5,351	5,585	5,832	8,155	7,685	7,685

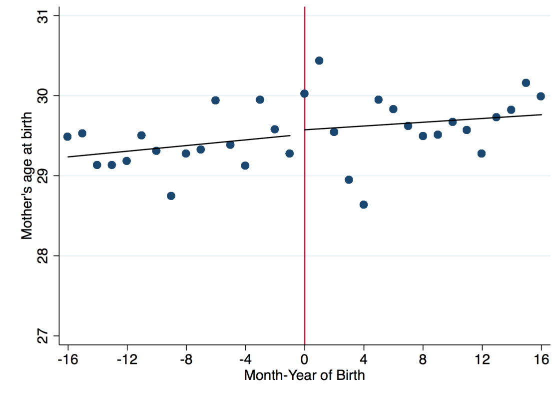
Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome taken from a local linear regression and using a bandwidth of 6 months. Panel A shows results using the entire sample, while panels B and C show results for children in and not in education priority zones respectively. Results are shown both with and without controls. Controls include fixed effects for the month 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. Age at beginning of preschool and time spent in preschool are measured in months. Due to the small number of clusters, standard errors are computed using a clustered wild bootstrap-t procedure and the corresponding p-values are reported in brackets (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

B Appendix Figures and Tables

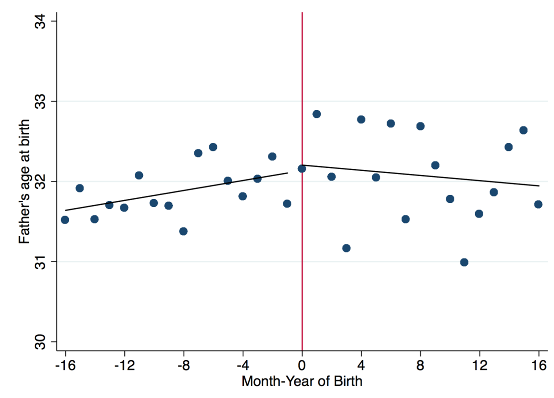
Figure A1: Smoothness of baseline covariates



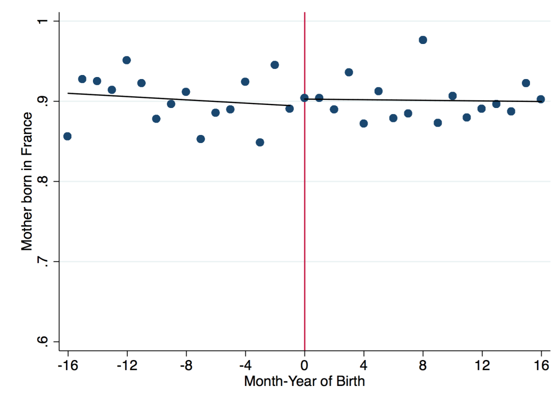
(a) Second child is male



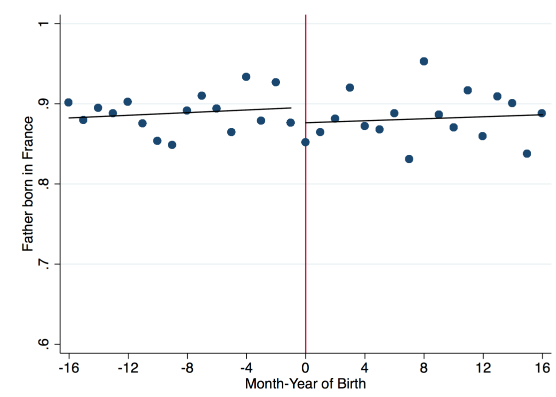
(b) Mother's age at second child's birth



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



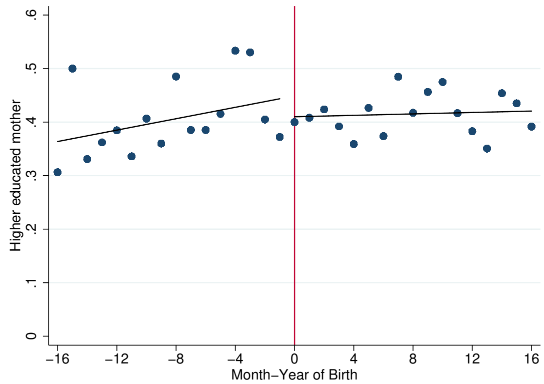
(d) Mother born in France



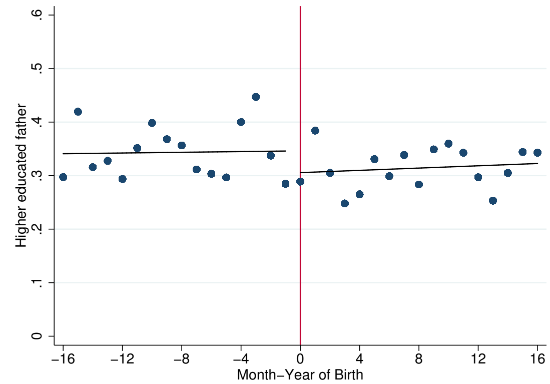
(e) Father born in France

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 specifications with a bandwidth of 16 months.

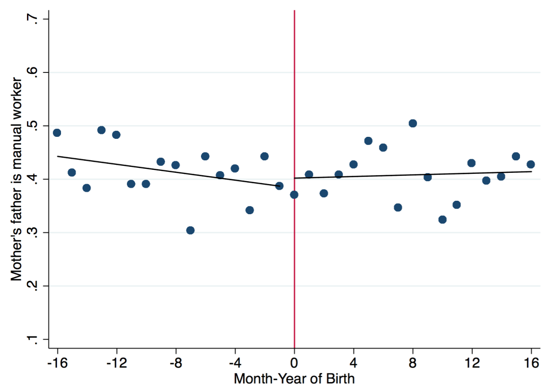
Figure A2: Smoothness of baseline covariates (continued)



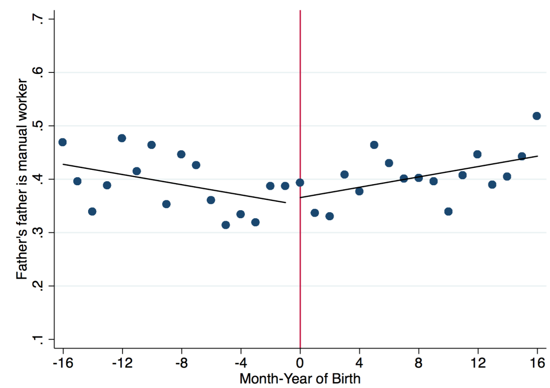
(a) Mother is higher educated



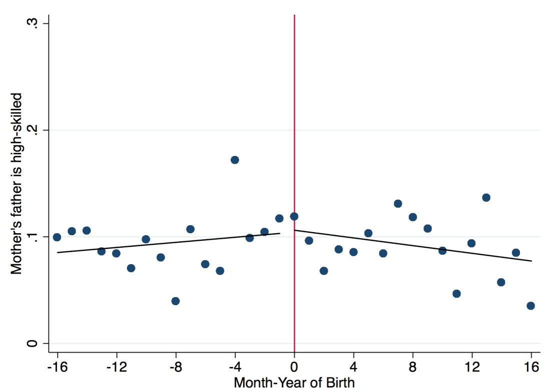
(b) Father is higher educated



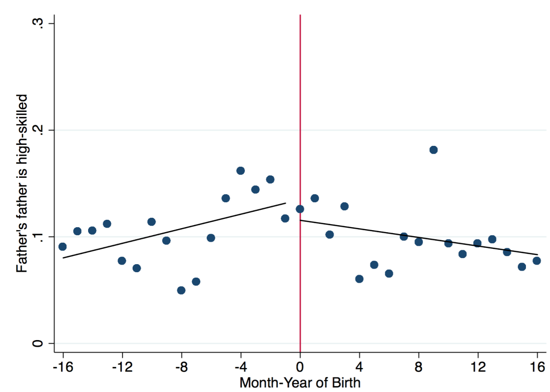
(c) Mother’s father is manual worker



(d) Father’s father is manual worker



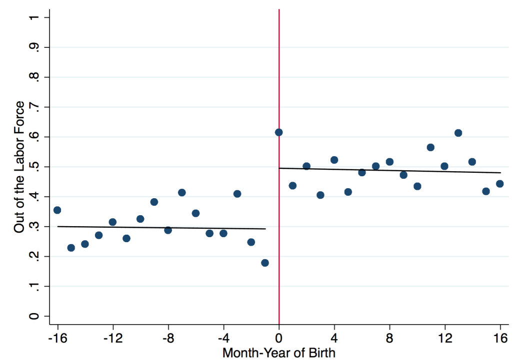
(e) Mother’s father is high-skilled



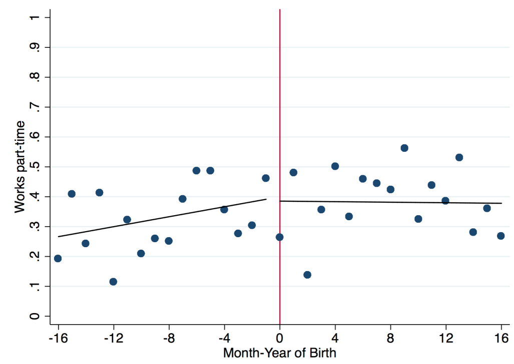
(f) Father’s father is high-skilled

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 specifications with a bandwidth of 16 months.

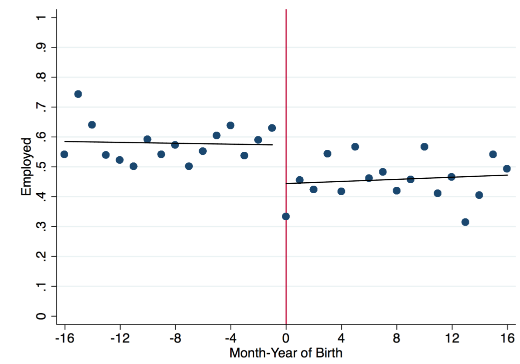
Figure A3: Effect of the reform on mothers' labor market outcomes in first year after second child's birth



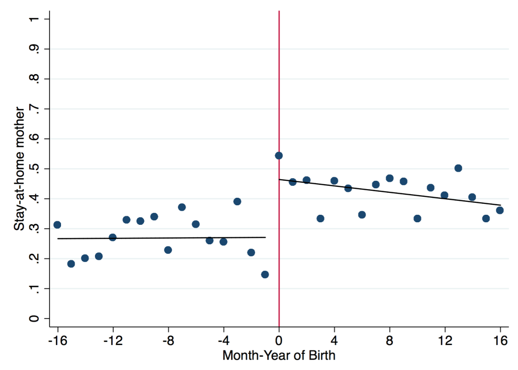
(a) Out of the labor force



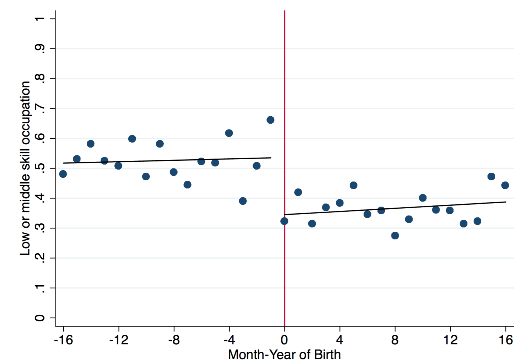
(b) Works part-time



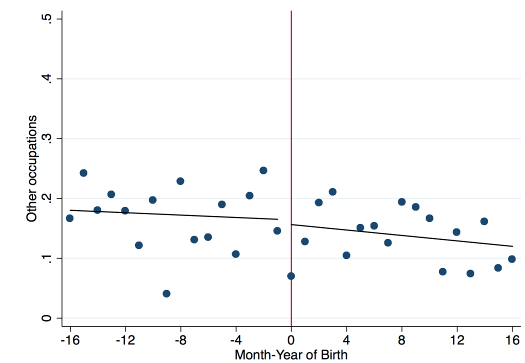
(c) Employed



(d) Stay-at-home mother



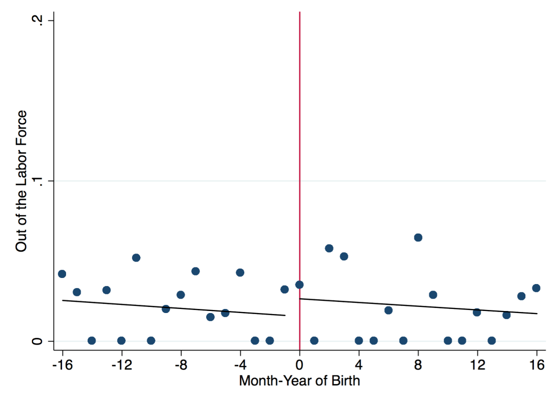
(e) Works in low or middle skill occupation



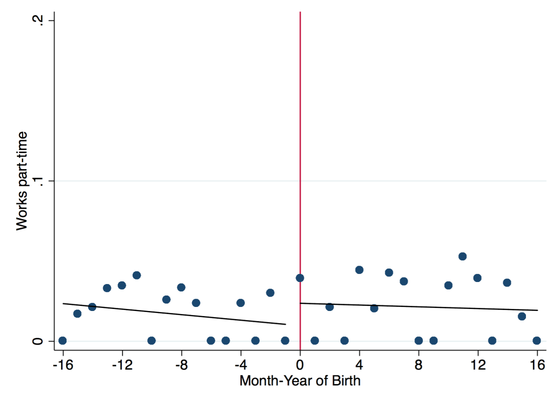
(f) Works in other occupation

Notes: The different panels show mothers' labor 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 specifications with a bandwidth of 16 months. Due to the small sample size, I look at the probability of working part-time regardless of the number of weekly work hours. The variable "other occupation" is the likelihood of working in jobs other than low or middle skill occupations i.e. liberal professions, entrepreneurs, managerial, high-skilled or manual workers.

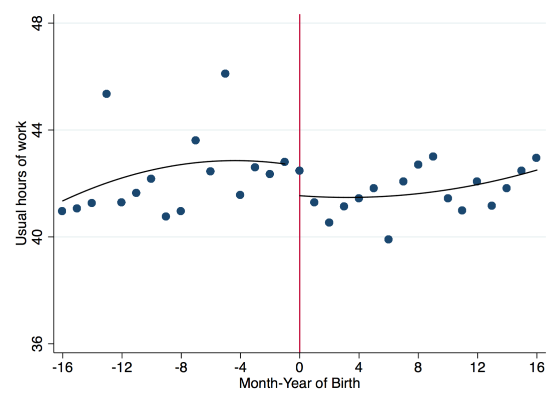
Figure A4: Effect of the reform on fathers' labor market outcomes in first year after second child's birth



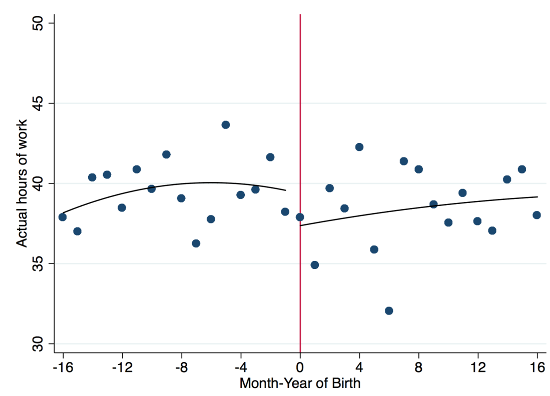
(a) Probability of being out of the labor force



(b) Probability of working part-time



(c) Usual hours of work

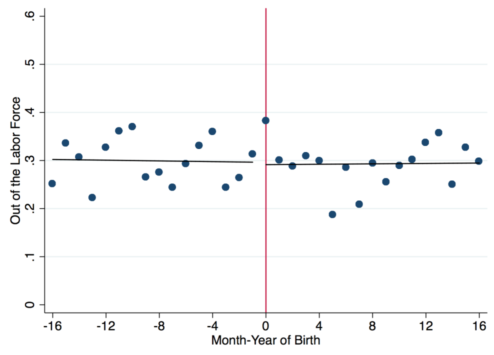


(d) Actual hours of work

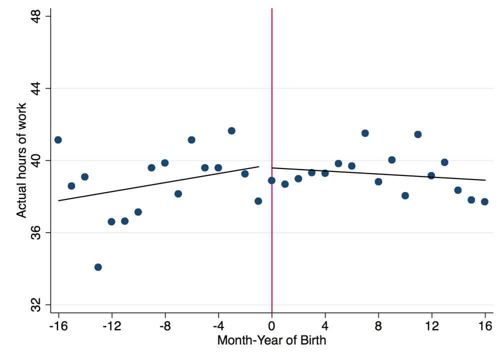
Notes: The different panels show fathers' labor outcomes in the first year after a second child's birth, as a function of the distance of second child's month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 16 months.

Figure A5: Placebo and robustness tests for parents' labor outcomes

Cutoff is July 1992

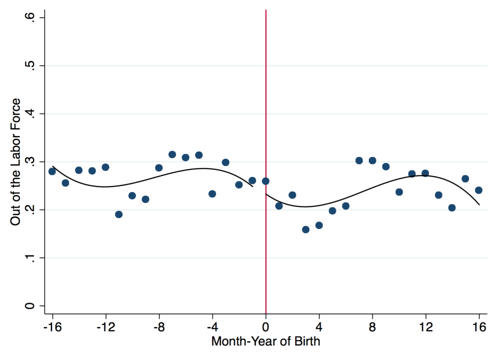


(a) Mothers out of the labor force

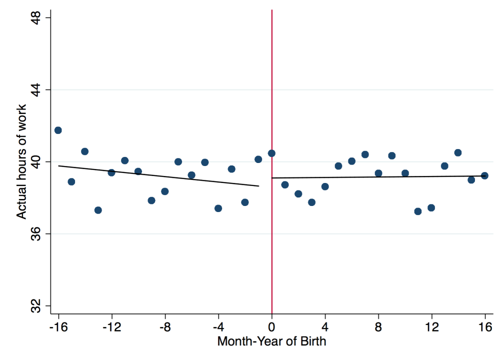


(b) Fathers' actual hours of work

Running variable is month-year of birth of first child

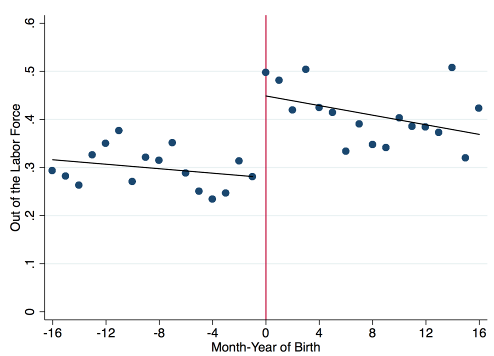


(c) Mothers out of the labor force

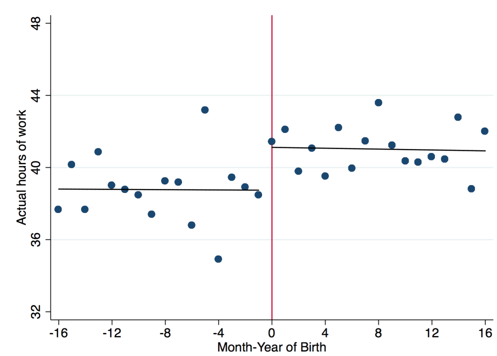


(d) Fathers' actual hours of work

Mothers aged 35 or less at birth



(e) Mothers out of the labor force

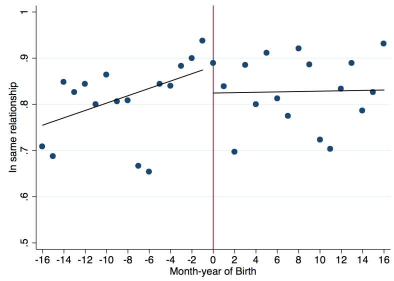


(f) Fathers' actual hours of work

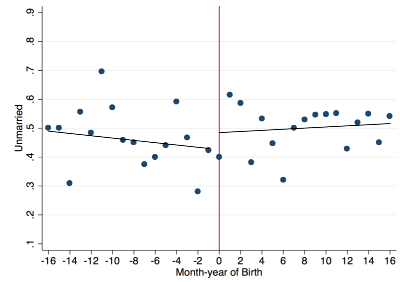
Notes: Panels (a) and (b) show parents' labor outcomes in second through fourth years after a second child's birth, as a function of distance of the second child's month-year of birth from July 1, 1992. Panels (c) and (d) show parents' labor outcomes in second through fourth years after a first child's birth, 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 in second through fourth years after a second child's birth using the sample of mothers aged less than 35.

Figure A6: Placebo tests for marital outcomes

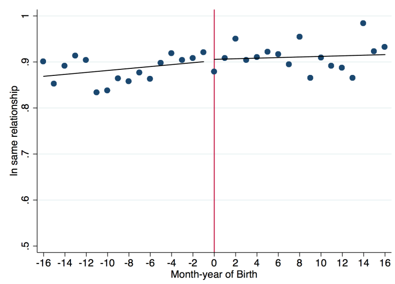
Cutoff is July 1992



(a) Cohabiting mothers in same relationship

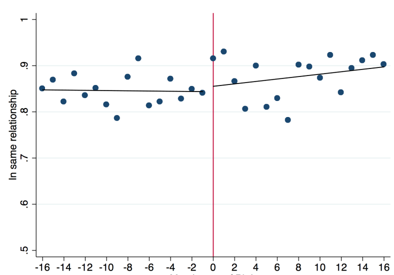


(b) Cohabiting mothers not married

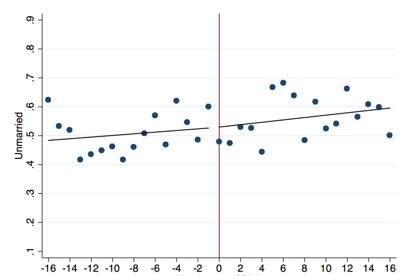


(c) Married mothers in same relationship

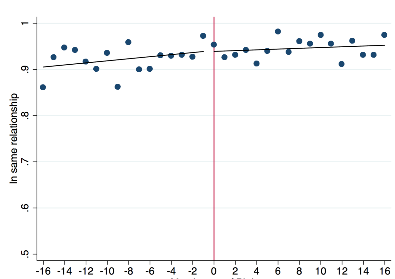
Running variable is month-year of birth of first child



(d) Cohabiting mothers in same relationship



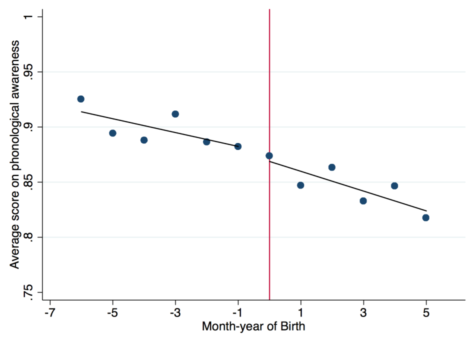
(e) Cohabiting mothers not married



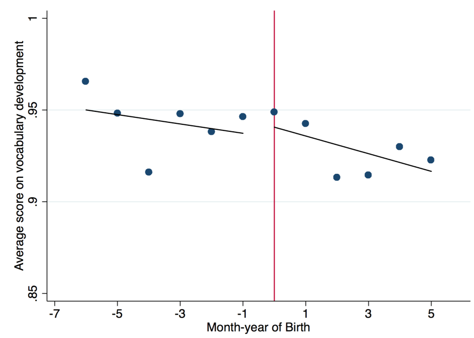
(f) Married mothers in same relationship

Notes: Panels (a) to (c) show marital outcomes, as a function of distance of the second child's month-year of birth from July 1, 1992. Panels (d) to (f) show marital outcomes as a function of distance of the first child's month-year of birth from the eligibility threshold.

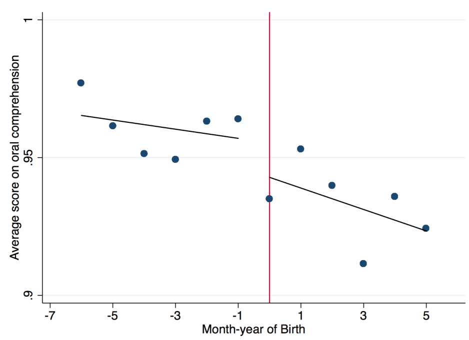
Figure A7: Effect of being born on or after the threshold on first-born children's outcomes



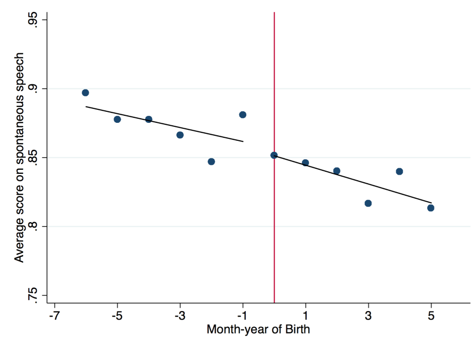
(a) Score on phonological awareness is average



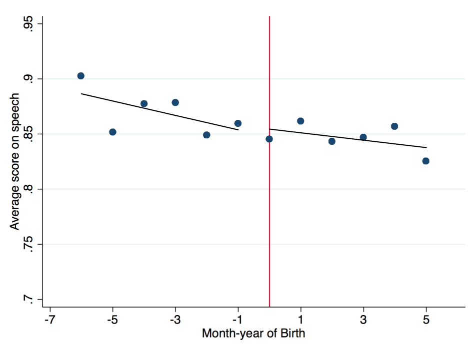
(b) Score on vocabulary development is average



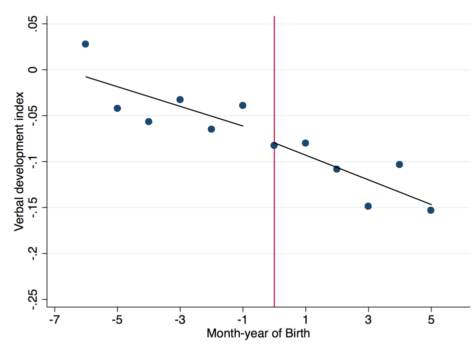
(c) Score on oral comprehension is average



(d) Spontaneous speech is average



(e) Overall speech is average



(f) Verbal development index

Notes: The different panels show first children's outcomes measured at ages 5-6, as a function of the distance of first children's month-year of birth from July 1, 1994. The fitted regression lines are taken from specifications with a bandwidth of 6 months.

Table A1: Sample description for parents' main labor market outcomes

	1st year after childbirth (1)	2nd year after childbirth (2)	3rd year after childbirth (3)	4th year after childbirth (4)
A. Mothers' Outcomes				
Out of labor force	0.393	0.384	0.365	0.281
N	1,820	1,814	1,714	1,677
Works part-time, <20 hours	0.053	0.067	0.058	0.061
N	948	960	963	1,009
Works part-time, 20-32 hours	0.283	0.300	0.319	0.310
N	948	960	963	1,009
Employed	0.499	0.499	0.498	0.489
N	1,820	1,814	1,714	1,677
B. Fathers' Outcomes				
Out of labor force	0.021	0.020	0.018	0.019
N	1,820	1,814	1,714	1,677
Works part-time, <20 hours	0.003	0.006	0.003	0.002
N	1,659	1,664	1,633	1,559
Works part-time, 20-32 hours	0.014	0.017	0.019	0.020
N	1,659	1,664	1,633	1,559
Usual hours	42.08	41.82	42.02	41.80
N	1,427	1,404	1,348	1,263
Actual hours	38.99	39.21	39.99	40.68
N	1,652	1, 659	1,629	1,552

Note: This table reports means for parents' main labor market outcomes. Columns 1 through 4 respectively show means for the first through fourth years after the second child's birth. The variables for part-time work and hours of work are conditional on the individual being employed.

Table A2: Sample description for demographic characteristics and main outcomes

	Mean	Observations
A. Demographic Characteristics		
Second child is male	0.520	4,096
Mother age at childbirth	29.51	4,096
Father age at childbirth	31.98	4,096
Mother born in France	0.902	4,096
Father born in France	0.884	4,096
Mother high school degree or more	0.411	4,096
Father high school degree or more	0.328	4,096
Mother's father is manual worker	0.411	4,096
Father's father is manual worker	0.395	4,096
Mother's father is high-skilled	0.095	4,096
Father's father is high-skilled	0.103	4,096
B. Marital Outcomes		
Mothers Cohabiting at Birth		
In same relationship	0.869	1,217
Unmarried	0.536	1,025
Mothers Married at Birth		
In same relationship	0.931	4,264
Mothers Single at Birth		
Cohabiting or married	0.689	501
C. Children's Outcomes		
Age at beginning of preschool	2.928	8,781
Child has		
Average score on phonological awareness	0.879	6,215
Average score on vocabulary development	0.927	6,210
Average score on oral comprehension	0.944	6,210
Average score on spontaneous speech	0.835	6,459
Average score on overall speech	0.819	6,780

Note: This table reports means for key variables. Data on demographic characteristics are taken from the French Labor Force Survey as defined in Section 3.1. 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 marital and children's outcomes are described in sections 3.2 and 3.3, respectively.

Table A3: Regression estimates for baseline covariates using different bandwidths

	BW=6 (1)	BW=13 (2)	BW=16 (3)	BW=19 (4)	BW=22 (5)	BW=28 (6)
Second child is male	– 0.025	– 0.019	– 0.007	– 0.016	– 0.022	– 0.023
	(0.017)	(0.022)	(0.023)	(0.020)	(0.020)	(0.023)
Mother's age at birth	0.240	0.159	0.085	0.031	0.073	0.070
	(0.334)	(0.316)	(0.309)	(0.282)	(0.259)	(0.329)
Father's age at birth	0.060	0.258	0.037	0.072	0.089	0.141
	(0.344)	(0.311)	(0.294)	(0.264)	(0.253)	(0.316)
Mother born in France	0.005	0.017	0.009	0.006	0.008	0.021
	(0.022)	(0.023)	(0.021)	(0.019)	(0.018)	(0.024)
Father born in France	– 0.024	– 0.029	– 0.019	– 0.022	– 0.017	– 0.035*
	(0.020)	(0.019)	(0.018)	(0.017)	(0.017)	(0.020)
Higher educated mother	–0.047	–0.055	–0.043	–0.022	–0.033	–0.061
	(0.038)	(0.041)	(0.038)	(0.032)	(0.030)	(0.044)
Higher educated father	–0.057	–0.047	–0.038	–0.060*	–0.060*	–0.058
	(0.042)	(0.041)	(0.037)	(0.033)	(0.031)	(0.044)
Mother's father is manual worker	–0.009	0.031	0.020	0.018	0.014	0.034
	(0.023)	(0.031)	(0.028)	(0.026)	(0.025)	(0.031)
Father's father is manual worker	0.008	0.035	0.024	0.017	0.015	0.023
	(0.025)	(0.031)	(0.027)	(0.026)	(0.023)	(0.031)
Mother's father is high-skilled	– 0.026	– 0.014	– 0.004	0.000	– 0.006	– 0.009
	(0.016)	(0.016)	(0.016)	(0.015)	(0.014)	(0.016)
Father's father is high-skilled	– 0.020	– 0.034*	– 0.020	– 0.012	– 0.011	– 0.025
	(0.011)	(0.018)	(0.017)	(0.018)	(0.018)	(0.019)
Polynomial	Zero	One	One	One	One	Two
Observations	1,040	3,279	4,096	4,862	5,623	7,185

Note: Each cell reports the reduced form estimate of the effect of reform on the corresponding baseline covariate. Each column uses the listed bandwidth (BW). Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A4: Regression estimates for mothers' labor market outcomes in second through fourth years after second child's birth using different bandwidths

	BW=6 (1)	BW=13 (2)	BW=16 (3)	BW=19 (4)	BW=22 (5)	BW=28 (6)
Out of the labor force	0.202***	0.200***	0.167***	0.140***	0.139***	0.176***
	(0.022)	(0.022)	(0.025)	(0.026)	(0.025)	(0.028)
With controls	0.211***	0.183***	0.142***	0.122***	0.115***	0.138***
	(0.028)	(0.019)	(0.023)	(0.021)	(0.022)	(0.032)
Is Employed	– 0.129***	– 0.162***	– 0.137***	– 0.118***	– 0.128***	– 0.139***
	(0.023)	(0.025)	(0.023)	(0.022)	(0.021)	(0.027)
With controls	– 0.139***	– 0.115***	– 0.106***	– 0.091***	– 0.095***	– 0.095***
	(0.016)	(0.016)	(0.018)	(0.018)	(0.019)	(0.026)
Is Stay-at-home mother	0.214***	0.185***	0.156***	0.137***	0.135***	0.168***
	(0.019)	(0.022)	(0.024)	(0.025)	(0.024)	(0.028)
With controls	0.224***	0.147***	0.126***	0.117***	0.110***	0.136***
	(0.021)	(0.021)	(0.021)	(0.020)	(0.020)	(0.033)
Is in Low or middle skill occupation	– 0.161***	– 0.182***	– 0.174***	– 0.149***	– 0.152***	– 0.174***
	(0.023)	(0.030)	(0.030)	(0.028)	(0.027)	(0.032)
With controls	– 0.168***	– 0.184***	– 0.172***	– 0.153***	– 0.144***	– 0.156***
	(0.028)	(0.025)	(0.030)	(0.028)	(0.029)	(0.037)
Observations	1,979	4,224	5,255	6,254	7,239	9,264
Works part-time, less than 20 hours	0.011	– 0.018	0.001	0.003	0.004	– 0.004
	(0.024)	(0.020)	(0.017)	(0.017)	(0.022)	(0.021)
With controls	0.018	– 0.040**	0.003	0.007	0.014	–0.002
	(0.022)	(0.017)	(0.016)	(0.017)	(0.023)	(0.023)
Works part-time, between 20 and 32 hours	0.072	0.026	– 0.019	– 0.022	0.020	– 0.021
	(0.087)	(0.067)	(0.062)	(0.055)	(0.076)	(0.070)
With controls	0.055	0.061	0.007	0.096***	0.059	0.031
	(0.064)	(0.073)	(0.082)	(0.033)	(0.047)	(0.055)
Observations	1,088	2,346	2,932	3,535	4,085	5,240
Log of wages	0.072	0.026	– 0.019	– 0.022	– 0.024	– 0.021
	(0.087)	(0.067)	(0.062)	(0.055)	(0.050)	(0.070)
With controls	0.009	0.046	– 0.027	– 0.015	– 0.009	– 0.014
	(0.071)	(0.039)	(0.037)	(0.037)	(0.036)	(0.046)
Observations	979	2,112	2,638	3,177	3,677	4,717
Polynomial	One	One	One	One	One	Two

Note: Each cell reports the reduced form estimate of the effect of reform on the corresponding outcome. Each column uses the listed bandwidth (BW). 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, are higher-educated, and have fathers who are manual workers or in managerial positions. For the variable "works part-time, between 20 and 32 hours", estimates are taken from a quadratic regression. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A5: Regression estimates for mothers' labor market outcomes in first year after second child's birth

	BW=6 (1)	BW=13 (2)	BW=16 (3)	BW=19 (4)	BW=22 (5)	BW=28 (6)
Out of the labor force	0.196**	0.216***	0.199***	0.204***	0.224***	0.228***
	(0.045)	(0.063)	(0.057)	(0.050)	(0.074)	(0.063)
With controls	0.184***	0.211***	0.153***	0.172***	0.170***	0.195***
	(0.048)	(0.055)	(0.044)	(0.040)	(0.063)	(0.069)
Observations	683	1,468	1,820	2,159	2,464	3,119
Works part-time	– 0.050	– 0.081	– 0.031	– 0.004	– 0.079	– 0.063
	(0.060)	(0.085)	(0.074)	(0.069)	(0.094)	(0.084)
With controls	– 0.056	– 0.034	– 0.002	0.031	– 0.054	– 0.004
	(0.067)	(0.053)	(0.046)	(0.050)	(0.079)	(0.072)
Observations	361	759	948	1,134	1,286	1,607
Employed	– 0.134**	– 0.177***	– 0.126**	– 0.118***	– 0.158***	– 0.138***
	(0.038)	(0.042)	(0.047)	(0.040)	(0.054)	(0.049)
With controls	– 0.124**	– 0.175***	– 0.074*	– 0.081**	– 0.095*	– 0.086
	(0.042)	(0.038)	(0.040)	(0.035)	(0.055)	(0.058)
Observations	683	1,468	1,820	2,159	2,464	3,119
Stay-at-home-mother	0.187**	0.213***	0.191***	0.183***	0.239***	0.219***
	(0.042)	(0.059)	(0.055)	(0.051)	(0.066)	(0.061)
With controls	0.177***	0.177***	0.132***	0.138***	0.172***	0.191***
	(0.049)	(0.050)	(0.040)	(0.039)	(0.053)	(0.065)
Observations	683	1,468	1,820	2,159	2,464	3,119
Low or middle-skill occupations	– 0.165**	– 0.182***	– 0.189***	– 0.190***	– 0.171***	– 0.209***
	(0.040)	(0.058)	(0.053)	(0.047)	(0.073)	(0.064)
With controls	– 0.158***	– 0.230***	– 0.167***	– 0.174***	– 0.152***	– 0.205***
	(0.037)	(0.034)	(0.037)	(0.033)	(0.050)	(0.063)
Observations	683	1,468	1,820	2,159	2,464	3,119
Other occupation	– 0.032	– 0.038	– 0.011	– 0.016	– 0.050	– 0.019
	(0.030)	(0.041)	(0.039)	(0.035)	(0.047)	(0.045)
With controls	– 0.027	0.019	0.013	0.002	– 0.010	0.011
	(0.030)	(0.036)	(0.031)	(0.029)	(0.037)	(0.038)
Observations	683	1,468	1,820	2,159	2,464	3,119
Polynomial	Zero	One	One	One	Two	Two

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Each column uses the listed bandwidth (BW). 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, are higher-educated, and have fathers who are manual workers or in managerial positions. Due to the small sample size, I look at the probability of working part-time regardless of the number of weekly work hours. The variable "other occupation" is the likelihood of working in jobs other than low or middle skill occupations i.e. liberal professions, entrepreneurs, managerial, high-skilled or manual workers. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A6: Regression estimates for fathers' labor market outcomes in second through fourth years after second child's birth using different bandwidths

	BW=6 (1)	BW=13 (2)	BW=16 (3)	BW=19 (4)	BW=22 (5)	BW=28 (6)
Out of the labor force	0.003	0.007	0.008	0.004	0.004	0.009
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.009)
With controls	0.004	– 0.010*	0.007	0.003	0.001	0.002
	(0.007)	(0.005)	(0.008)	(0.007)	(0.007)	(0.010)
Observations	1,979	4,224	5,255	6,254	7,239	9,264
Works part-time,	– 0.002	– 0.005**	– 0.003	0.001	– 0.001	– 0.002
less than 20 hours	(0.004)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)
With controls	– 0.003	– 0.002	– 0.001	0.002	0.001	0.000
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Works part-time,	0.014**	0.009	0.005	0.008	0.008	0.009
between 20 and 32 hours	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
With controls	0.017**	0.025***	0.006	0.005	0.006	0.005
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.008)
Observations	1,831	3,899	4,856	5,774	6,682	8,569
Actual hours of work	2.849***	2.797***	2.469***	2.629***	2.971***	2.902***
	(0.898)	(0.942)	(0.888)	(0.846)	(0.788)	(0.968)
With controls	2.947***	3.930***	2.558***	2.477***	2.806***	2.828***
	(0.842)	(0.904)	(0.826)	(0.813)	(0.763)	(1.039)
Observations	1,824	3,886	4,840	5,751	6,653	8,531
Usual hours of work	1.783*	0.972	0.636	0.934	1.402**	1.218
	(0.818)	(0.825)	(0.761)	(0.740)	(0.673)	(0.808)
With controls	2.134*	1.077**	0.649	0.950*	1.409**	0.876
	(1.059)	(0.480)	(0.551)	(0.558)	(0.603)	(0.764)
Observations	1,523	3,235	4,015	4,755	5,506	7,087
Absent for personal reasons	– 0.036	– 0.039*	– 0.043**	– 0.038**	– 0.039**	– 0.047**
	(0.024)	(0.019)	(0.018)	(0.016)	(0.015)	(0.021)
	– 0.032	– 0.048***	– 0.046***	– 0.035**	– 0.035**	– 0.042**
With controls	(0.027)	(0.015)	(0.013)	(0.014)	(0.013)	(0.019)
Absent for other reasons	– 0.004	– 0.015	– 0.012	– 0.007	– 0.008	– 0.005
	(0.024)	(0.013)	(0.012)	(0.011)	(0.010)	(0.014)
With controls	– 0.005	– 0.033**	– 0.012	– 0.008	– 0.009	– 0.011
	(0.025)	(0.013)	(0.010)	(0.010)	(0.010)	(0.014)
Observations	1,523	3,235	4,015	4,755	5,506	7,087
Log of wages	– 0.076	– 0.049	– 0.033	– 0.044	– 0.049	– 0.059
	(0.094)	(0.052)	(0.048)	(0.043)	(0.040)	(0.055)
With controls	– 0.022	0.028	0.011	0.005	0.011	0.004
	(0.043)	(0.021)	(0.020)	(0.022)	(0.021)	(0.029)
Observations	1,544	3,305	4,114	4,869	5,668	7,276
Polynomial	One	One	One	One	One	Two

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Each column uses the listed bandwidth (BW). 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, are higher-educated, and have fathers who are manual workers or in managerial positions. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** p < 0.01 ** p <0.05 * p <0.1).

Table A7: Regression estimates for fathers' labor market outcomes in first year after second child's birth

	BW=6 (1)	BW=13 (2)	BW=16 (3)	BW=19 (4)	BW=22 (5)	BW=28 (6)
Out of the labor force	0.008	0.014	0.013	0.009	0.016	0.016
	(0.012)	(0.015)	(0.014)	(0.013)	(0.017)	(0.016)
With controls	0.008	0.008	0.008	0.005	0.016	0.016
	(0.013)	(0.014)	(0.012)	(0.011)	(0.015)	(0.014)
Polynomial	Zero	One	One	One	Two	Two
Observations	683	1,468	1,820	2,159	2,464	3,119
Works part-time	0.011	0.013	0.012	0.022*	0.019	0.023
	(0.009)	(0.013)	(0.011)	(0.012)	(0.016)	(0.014)
With controls	0.012	0.020**	0.014	0.028**	0.029**	0.022
	(0.008)	(0.009)	(0.009)	(0.011)	(0.012)	(0.014)
Polynomial	Zero	One	One	One	Two	Two
Observations	632	1,341	1,659	1,968	2,241	2,854
Usual hours of work	– 1.558**	– 1.396	– 1.185	– 1.377	– 0.577	–1.443
	(0.653)	(0.851)	(0.712)	(0.865)	(0.861)	(0.905)
With controls	–1.492*	0.502	2.352	– 0.051	2.203	– 0.745
	(0.694)	(0.965)	(1.396)	(1.122)	(1.602)	(1.214)
Polynomial	Zero	One	Two	Two	Three	Three
Observation	544	1,162	1,427	1,694	1,924	2,429
Actual hours of work	– 2.012	– 2.248	– 1.786	– 1.807	– 1.528	– 2.286
	(1.354)	(1.578)	(1.769)	(1.649)	(2.063)	(1.817)
With controls	–2.124	–1.544	– 0.081	– 0.738	–1.080	–2.883*
	(1.392)	(.985)	(1.890)	(1.366)	(2.192)	(1.634)
Polynomial	Zero	One	Two	Two	Three	Three
Observations	628	1,334	1,652	1,961	2,233	2,841

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Each column uses the listed bandwidth (BW). 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, are higher-educated, and have fathers who are manual workers or in managerial positions. Due to the small sample size, I look at the probability of working part-time regardless of the number of weekly work hours. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A8: Regression estimates for marital outcomes using different bandwidths

	BW=6 (1)	BW=13 (2)	BW=16 (3)	BW=19 (4)	BW=22 (5)	BW=28 (6)
A. Cohabiting before birth						
In same relationship	– 0.022	– 0.026	– 0.009	0.011	– 0.022	– 0.027
	(0.069)	(0.044)	(0.039)	(0.035)	(0.054)	(0.044)
With controls	–0.018	–0.066	–0.028	–0.008	–0.056	–0.042
	(0.064)	(0.064)	(0.030)	(0.027)	(0.047)	(0.038)
Observations	485	958	1,217	1,450	1,678	2,111
Unmarried	0.100*	0.112**	0.112**	0.090**	0.141***	0.139***
	(0.052)	(0.050)	(0.047)	(0.044)	(0.051)	(0.050)
With controls	0.112*	0.094*	0.091**	0.072*	0.144**	.155***
	(0.061)	(0.053)	(0.043)	(0.042)	(0.067)	(0.054)
Observations	413	796	1,025	1,214	1,408	1,782
B. Married before birth						
In same relationship	0.003	– 0.013	– 0.006	0.002	– 0.016	– 0.005
	(0.017)	(0.014)	(0.014)	(0.012)	(0.016)	(0.013)
With controls	0.002	–0.027*	–0.006	0.000	–0.017	–0.007
	(0.018)	(0.013)	(0.014)	(0.014)	(0.015)	(0.016)
Observations	1,553	3,451	4,264	4,993	5,781	7,486
C. Single before birth						
Cohabiting or married	0.199*	0.047	0.028	0.023	0.103	0.024
	(0.108)	(0.067)	(0.063)	(0.062)	(0.065)	(0.069)
With controls	0.369**	–0.102	0.022	0.018	0.091	0.022
	(0.134)	(0.071)	(0.053)	(0.054)	(0.079)	(0.078)
Observations	159	401	501	604	697	884
Polynomial	One	One	One	One	Two	Two

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Each column uses the listed bandwidth (BW). 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, is higher-educated, and has a father who is a manual worker and is in a high skill occupation. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A9: Heterogeneous effects for couples' labor supply and marital stability

	Fathers less than high school		Fathers more than high school		Mothers less than high school		Mothers more than high school	
	Woman is stay-at-home mother	Father's actual hours	Woman is stay-at-home mother	Father's actual hours	Unmarried	In same relationship	Unmarried	In same relationship
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No controls	0.182***	1.054	0.106**	5.368***	0.101***	–0.031	0.132	0.026
	(0.029)	(1.081)	(0.040)	(1.643)	(0.033)	(0.046)	(0.129)	(0.057)
With controls	0.154***	1.127	0.083**	5.619**	0.095*	– 0.071*	0.099	0.042
	(0.028)	(0.924)	(0.033)	(1.439)	(0.052)	(0.037)	(0.115)	(0.038)
Observations	3,538	3,202	1,717	1,638	678	810	347	407

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Columns (1)-(2) and (3)-(4) report estimates for parents' main labor market outcomes using couples in which the father has less than high school degree and a high school degree and more, respectively. Columns (5)-(6) and (7)-(8) report estimates for the main marital outcomes using mothers who were cohabiting at the birth of their second child and who have less than high school degree and a high school degree and more, respectively. Estimates are taken from local linear regressions using a bandwidth of 16 months. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A10: Placebo tests for parents' labor market outcomes

	Mothers				Fathers				
	Out of labor force	Part-time, less than 20 hours	Part-time, between 20 and 32 hours	Employed	Out of labor force	Part-time, less than 20 hours	Part-time, between 20 and 32 hours	Usual hours	Actual hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A:									
Cutoff is July 1992	– 0.004	– 0.010	– 0.006	0.009	0.011	0.004	0.013	0.352	– 0.317
	(0.034)	(0.017)	(0.048)	(0.030)	(0.007)	(0.005)	(0.010)	(0.544)	(0.930)
With controls	– 0.041	– 0.012	0.076	0.050**	0.006	0.005	0.007	0.344	– 0.098
	(0.026)	(0.011)	(0.047)	(0.024)	(0.005)	(0.004)	(0.007)	(0.609)	(0.735)
Observations	5,291	3,166	3,166	5,291	5,291	4,886	4,886	4,088	4,851
Polynomial	One	One	One	One	One	One	One	One	One
Panel B:									
Month-year of birth of first child	0.019	0.019	0.002	0.032	– 0.013	– 0.003	– 0.003	0.281	0.522
	(0.030)	(0.012)	(0.044)	(0.020)	(0.015)	(.003)	(0.007)	(0.317)	(0.803)
With controls	0.041	0.014	0.058	0.004	– 0.003	– 0.003	0.004	0.096	0.332
	(0.037)	(0.010)	(0.061)	(0.016)	(0.012)	(0.003)	(0.008)	(0.437)	(0.901)
Observations	5,992	3,783	3,783	5,992	5,992	5,471	5,471	4,628	5,440
Polynomial	Three	One	One	One	Two	One	One	One	One

Note: Results are shown both with and without controls and estimates are taken from regressions using a bandwidth of 16 months. 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, are higher-educated, and have fathers who are manual workers or in managerial positions. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A11: Regression estimates for labor outcomes using mothers aged 35 and less

	Mothers				Fathers				
	Out of labor force	Part-time, less than 20 hours	Part-time, between 20 and 32 hours	Employed	Out of labor force	Part-time, less than 20 hours	Part-time, between 20 and 32 hours	Usual hours	Actual hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
No controls	0.177***	– 0.012	0.035	– 0.151***	0.009	– 0.004	0.008	0.746	2.518**
	(0.028)	(0.018)	(0.051)	(0.024)	(0.009)	(0.003)	(0.006)	(0.860)	(1.035)
With controls	0.165***	– 0.006	0.035	– 0.137***	0.010	– 0.002	0.007	0.704	2.450***
	(0.026)	(0.019)	(0.084)	(0.023)	(0.008)	(0.003)	(0.007)	(0.593)	(0.877)
Observations	4,688	2,558	2,558	4,688	4,688	4,343	4,343	3,604	4,328
Polynomial	One	One	One	One	One	One	One	One	One

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Results are shown both with and without controls and estimates are taken from regressions using a bandwidth of 16 months. 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, are higher-educated, and have fathers who are manual workers or in managerial positions. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A12: Placebo tests for marital outcomes

	Cohabiting before birth		Married before birth
	In same relationship	Unmarried	In same relationship
	(1)	(2)	(3)
Panel A:			
Cutoff is July 1992	– 0.045	0.062	0.005
	(0.048)	(0.066)	(0.016)
With controls	– 0.073*	0.044	0.015
	(0.042)	(0.041)	(0.014)
Observations	998	847	4,415
Panel B:			
Month-year of birth of first child	0.013	– 0.013	0.001
	(0.028)	(0.046)	(0.015)
With controls	0.008	0.007	0.016
	(0.025)	(0.036)	(0.014)
Observations	2,436	2,038	3,843

Note: 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, is higher-educated, and has a father who is a manual worker and is in a high skill occupation. Estimates are taken from local linear regressions using a bandwidth of 16 months. Standard errors are clustered by month-year of birth of the second child and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A13: RD-DID regression estimates for children's outcomes

	Phonological Awareness (1)	Vocabulary Development (2)	Oral Comprehension (3)	Spontaneous Speech (4)	Overall Speech (5)	Verbal Development Index (6)	Age at beginning of preschool (7)	Time in preschool (8)
No controls	– 0.029	– 0.030	0.002	– 0.038*	– 0.045	– 0.059*	0.024	– 0.033
	[0.172]	[0.168]	[0.954]	[0.059]	[0.274]	[0.057]	[0.938]	[0.918]
With controls	– 0.029**	– 0.031**	– 0.001	– 0.039***	– 0.044*	– 0.061***	0.004	– 0.017
	[0.018]	[0.022]	[0.102]	[0.009]	[0.062]	[0.009]	[0.917]	[0.852]
Observations	13,830	13,848	13,861	14,497	15,263	20,878	19,822	19,822

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome from a difference-in-discontinuity regression using data within 6 months on either side of the cutoff. Results are shown both with and without 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. Age at beginning of preschool is measured in years. Due to the small number of clusters, standard errors are computed using a clustered wild bootstrap-*t* procedure and the corresponding *p*-values are reported in brackets. (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).