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

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February 18, 2016

Abstract

This paper examines the impacts of a long duration of paid parental leave on parents' labor market decisions and children's outcomes. I leverage a French program that provided recipients with three years of partially paid leave conditional on being out of the labor market or working part-time. Initially, the program was reserved for parents of three children and more. On July 25, 1994, benefits were extended to parents whose second child was born on or after July 1, 1994. For identification, I use a regression discontinuity design based on the second child's date of birth cutoff. I find that mothers decrease their labor force participation in the three years following the birth of a second child. Fathers' response is heterogeneous. Well educated men increase their weekly hours of work, while some less educated fathers are more likely to work part-time. The policy has no effect on children's health but harms their verbal skills at age 6.

JEL Classification: I10, J12, J13, J22

Keywords: parental leave, hours of work, marriage, child development

*I am grateful to Olivier Deschênes, Peter Kuhn and Heather Royer for helpful comments and suggestions. I also thank Kelly Bedard, Shelly Lundberg, Jonathan Meer, Maya Rossin-Slater, Dick Startz, seminar participants at the UC Santa Barbara Labor Lunch and members of the UC Santa Barbara Human Capital Research Group. All errors are my own.

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1 Introduction

Many governments provide paid leave for parents who wish to take time off from work after the birth of a child. While the provision of leave is widespread, entitlements vary substantially across countries. For example, in the United States, only California, New Jersey and Rhode Island currently grant up to six weeks of job-protected leave with partial income replacement. This is in stark contrast to European countries such as Norway and Sweden which offer up to 13 months of coverage at high pay. Over the past years, governments have been expanding these programs along two dimensions. First, there has been an increase in the length of job-protected leave, with some countries like Austria and Germany extending coverage to 24 and 36 months, respectively.[1] Second, although programs that target women are prevalent, those that cover men are less common. Recently, more countries have been extending benefits to both mothers and fathers, with some even providing additional incentives for fathers.[2]

These expansions are motivated by the idea that mothers' and fathers' leave-taking can help narrow the gender gap in labor force participation and wages, promote family formation and have positive effects on children's health and development. Although the literature on parental leave is extensive, two issues warrant further consideration. Little work has been done regarding the impact of these programs on fathers' labor market outcomes. Furthermore, few papers look at how children are affected by the extension of leave beyond their first year of life.

In this paper, I ask how the provision of a long period of parental leave with partial income replacement affects parents' labor market decisions, as well as children's health and development. I focus on a French program, the "Allocation Parentale d'Education" (or APE), which offered one or both parents a fixed monthly cash allowance to take time off from work until the child's third birthday. Parents who held a job in the same company for at least a year prior to birth were guaranteed to return to their old position once the leave expired. Benefit receipt was conditional on the parent being out of the labor force or working part-time. For identification, I exploit a change in the program's eligibility conditions. Specifically, upon its instigation, only parents of three children and more qualified for the APE. On July 25, 1994, benefits were extended to parents whose second child was born on or after July 1, 1994. This new eligibility threshold and the retroactive nature of the extension allow me to

[1] It should be noted that income replacement is offered for 24 months in Austria and 17.5 months in Germany (Ruhm, 2011).

[2] For example, since 2004, several states in the U.S., started implementing the Paid Family Leave program which offers benefits to both mothers and fathers (Bartel et al., 2015). Countries that provide leave that is exclusive to fathers include Norway, Sweden and the United Kingdom (Ruhm, 2011).

use a regression discontinuity design based on the second child's date of birth cutoff.

I find that, consistent with leave take-up, mothers decrease their labor force participation in the three years following the birth of a second child. I further show that fathers' labor market response is heterogeneous. Some low educated men are more likely to work part-time (versus full-time) suggesting that they may be incentivized to take up the leave. In contrast, better educated fathers increase their weekly hours of work. There are two possible explanations for this finding. On one hand, the APE does not offer full income replacement. Therefore, mothers' leave-taking could generate a loss of household income. On the other hand, if couples substitute their time in home production, then men's opportunity cost from working might decrease. Both of these effects would induce fathers to increase their work hours. For children, I detect no significant effects on indicators of health. However, the APE has a negative impact on their verbal skills measured at age 6. This is captured by a decline in their performance on tests that assess their phonological awareness and vocabulary development. As further discussed in section 6, this effect can be driven by a decrease in the time children spend with their fathers or other caregivers. It can also be induced by a negative income shock due to the mother withdrawing from the labor market.

This paper is related to a large body of literature which documents the impacts of parental leave on a wide range of family outcomes. An extensive set of papers investigates whether mothers take up leave and how this alters their labor market outcomes and fertility. Piketty (2005) and Lequien (2012) respectively show that the APE has no impact on fertility and negatively affects mothers' earnings in the long run.[3] The evidence, however, regarding fathers' response to parental leave is relatively scarce. Previous studies focus on whether programs increase fathers' leave-taking (Han, Ruhm and Waldfogel, 2009; Ekberg, Eriksson and Friebel, 2013; Dahl, Løken and Mogstad, 2014; Cools, Fiva and Kirkbøen, 2015; Bartel et al., 2015) and how this affects the intra-household division of childcare (Tanaka and Waldfogel, 2007; Ekberg, Eriksson and Friebel, 2013).

To the best of my knowledge, only two other papers in economics look at fathers' labor market response to parental leave. Cools, Fiva and Kirkbøen (2015) show that offering four weeks of paternity leave has no impact on men's earnings or hours of work. Dahl, Løken, Mogstad and Salvanes (Forthcoming) focus on a series of maternity leave reforms in Norway- which resulted in an increase in paid leave from 18 to 35 weeks- and find no effect on fathers' earnings. However, these programs are different from the APE as they provide a shorter period of leave. I show that providing three years of partially paid parental leave

[3]For further evidence on the topic in other countries, see papers by Ruhm (1998), Waldfogel (1999), Baum (2003), Baker and Milligan (2008), Han, Ruhm and Waldfogel (2009), Lalive and Zweimüller (2009), Lalive, Schlosser, Steinhauer and Zweimüller (2014), Ludsteck and Schönberg (2014), Dahl, Løken, Mogstad and Salvanes (forthcoming).

can significantly alter fathers' labor supply and increase intra-household specialization.

My paper also builds on a series of studies that investigate the connection between parental leave and children's development. Carneiro, Løken and Salvanes (2015) find that providing mothers with 4 months of paid leave has positive effects on children's education and earnings. However, other studies generally report no significant effects on measures of cognitive skills and education from subsequent expansions in coverage in the child's first year of life (Baker and Milligan, 2010; Rasmussen, 2010; Dustmann and Schönberg, 2012; Baker and Milligan, 2015; Dahl, Løken, Mogstad and Salvanes, forthcoming). My paper is closest to previous work which focuses on programs that extend leave beyond the child's first birthday. Liu and Skans (2010) find that children's test scores are unaffected by an expansion in leave from 12 to 15 months in Sweden. Dustmann and Schönberg (2012) show that increasing the duration of unpaid leave from 18 to 36 months in Germany has small negative effects on educational attainment at age 14. Danzer and Lavy (2014) document heterogeneous impacts on boys in Austria from providing an additional 12 months of paid leave after the child's first year.

I find that extending paid leave until the child's third birthday has detrimental effects on measures of verbal skills at age 6. My results differ from previous studies in several ways. First, aside from the leave used by Dustmann and Schönberg (2012), the APE is the only studied program that provides benefits until the child's third birthday. Furthermore, although the German extension was from 18 to 36 months, mothers only took up the benefits for an additional 1.4 months. In the case of the APE, I find that mothers decrease their labor force participation in the second and third years after the birth of the child. These differences could be driven by the fact that the APE offered partial income replacement as opposed to the unpaid leave in Germany. Second, I document an increase in fathers' labor market hours, which could potentially cause a decrease in paternal time spent with the child. This suggests that fathers' labor response could play an important role in determining how parental leave affects children.

Section 2 presents detailed information on the institutional setting. Section 3 describes the data I use. Section 4 reviews my identification strategy. Section 5 presents the main empirical results as well as robustness checks. Finally, in section 6, I discuss the results and conclude in section 7.

2 Institutional Background

2.1 The "Allocation Parentale d'Education"

Mothers in France can benefit from several policies that allow them to take time off from work after the birth of a child. All working mothers are offered job-protected maternity leave. Mothers of one or two children have access to 6 weeks of prenatal leave and 10 weeks of postnatal leave. Mothers of three children and more are given 8 weeks of prenatal leave and 18 weeks of postnatal leave.[4] A maximum of 3 weeks of prenatal leave can be used after the birth of the child. Beneficiaries receive 100 percent of their pre-leave wage.[5]

Before July 1994, parents of three children and more could also benefit from the "Allocation Parentale d' Education" (APE). The program was created in 1985 with a goal of allowing parents to balance their work and family life. Under the APE, a parent received a fixed nontaxable monthly cash allowance if he/she exited the labor force following the birth of the child. The APE could not be combined with maternity leave. Mothers had the option to take up maternity leave first then start benefiting from the APE. Both mothers and fathers could receive benefits until the child's third birthday. In order to be eligible for the APE, a parent should have worked for 2 years (consecutive or not) in the 10 years prior to the birth of the child. The parent had to be out of the labor force when receiving the benefits but could start working part-time after the child's second birthday.

Parents who worked in the same company for at least a year prior to the birth of the child, could combine the APE with the "Congé Parental d'Education" (CPE). The CPE allowed parents to take a job-protected unpaid leave until the child's third birthday. Specifically, CPE recipients were guaranteed to return to the same job they held with their previous employers before taking the leave.

The law "Famille", passed on July 25, 1994, extended the APE benefits to parents whose second child was born on or after July 1, 1994.[6] The extension of the APE was retroactive and was not announced before the law was passed (Lequien, 2012). This alleviates concerns over the fact that parents could manipulate or strategically time their conception or delivery date in order to benefit from the APE extension. The eligibility conditions remained unchanged for parents of three children and more. Parents of two children were eligible to receive the benefits if they worked or received unemployment benefits for 2 years in the 5 years prior to the birth of the second child. Parents of two children and more could now benefit from the

[4] Mothers can take 34 to 46 weeks of leave for multiple births.

[5] This is based on the mother's average wage in the 3 months prior to leave taking. There is also a ceiling on the amount of money that can be received.

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

APE if they exited the labor force or worked part-time. The monthly payment was around 460 euros for a parent who was out of the labor force, 300 euros for those who worked less than 20 hours a week and 225 euros for those who worked between 20 and 32 hours a week.[7] Both parents could receive APE benefits at the same time if they were both working part-time. The combined monthly payment in that case was 460 euros.

Following the extension of the APE, the number of beneficiaries went up from 156,000 at the end of 1993 to 447,000 by the end of 1996.[8] Take-up was higher than expected and 98% of recipients were women (Piketty, 2005). The projected costs of the APE for mothers of two children who exited the labor market were around 1 billion euros but by 1997, the actual costs were already around 1.38 billion euros. Most beneficiaries withdrew completely from the labor force, with around 20% of recipients working part-time (Afsa, 1999). Take-up was restricted to women who were married or had a partner. Single mothers had access to another policy, the "Allocation pour Parent Isolé", which offered significantly higher benefits (Piketty, 2005).

2.2 Childcare in France

Given that the policy increases mothers' time at home, it is important to understand the other childcare options available in France. Parents in France have access to several paid but subsidized childcare services. In general, children can be placed in publicly-funded nurseries or in the care of registered child-minders. However, due to the high demand, access to these services is usually limited. In 1995, around 4% and 17% of households with a child aged less than 11 paid for the services of an in-home and out-of-home registered child-minders, respectively. 14% had access to publicly-funded nurseries (see Flipo, 1996).

Compulsory schooling starts at the age of six, and children who are between the ages of 3 and 6 are usually enrolled in preschools (or *Ecole Maternelle*). Around one third of children are admitted at the age of 2. Public preschools are universal, free of charge, offer a government-mandated curriculum and have teachers who have the same credentials as those who work in elementary schools. Although not mandatory, nearly all children in France are enrolled in preschools by age 3 (Goux and Maurin, 2010). Children are grouped into classes according to their age. Therefore, those who are enrolled at age 2 attend 4 years of preschool, as opposed to 3 years for those who are enrolled at age 3.

[7] These amounts remained unchanged before and after the policy extension.

[8] 270,000 were parents of two children.

3 Data

3.1 The 1990-2012 French Labor Force Survey

Data on mothers' and fathers' labor supply is taken from the French Labor Force Survey (LFS). The LFS is a household survey that is administered by the French statistical office (INSEE) and provides individual-level information on education, labor market outcomes such as labor force participation, employment and hours worked, as well as the month and year of birth of each child living in a household.

From 1990 to 2002, the LFS is conducted on a yearly basis and covers around 100,000 households per year. Each household member aged 15 years and above is interviewed for three consecutive years. To analyze parents' labor supply responses, I restrict my sample to mothers and fathers who are observed in the second (year 2) and/or third years (year 3) after the birth of their second child. The labor supply outcomes are stacked for both years and each individual is allowed to repeat. Panel A of Table 1 reports descriptive statistics for the main variables from this sample for years 2 and 3 separately. 37.2% and 35.6% of mothers are out of the labor force in years 2 and 3 respectively. 96% of fathers are in the labor force and amongst those who are employed, around 97% work full-time, with an average of 39 hours of work per week.

3.2 Enquête Histoire Familiale

Data on marital outcomes and infant mortality are taken from the Enquête Histoire Familiale. I also use this data to show that the baseline covariates are smooth around the cutoff. The survey reports detailed information on family life for 380,000 individuals aged 18 years and above, who are also part of the 1999 population census. For each household, either all men or all women are surveyed. The data contains information on the respondent's education, socio-economic background, marital status as well as the month and year of the beginning and the end of the first and last relationship and/or marriage. The survey also provides information on the respondent's children including their month and year of birth, birth order, whether the child is deceased and the age at death.

I keep in my sample mothers and fathers who have a second child. Panel B of Table 1 reports summary statistics for parents' and children's baseline characteristics and the infant mortality rate. Mothers' and Fathers' average ages at the birth of their second child are 29.3 and 31.8 years, respectively. Around 90% of parents are born in France, 37.2% of fathers have a high school degree and more and 51.1% of second children are male.

In some specifications, I analyze the marital responses of parents who are unmarried prior

to the birth of the second child. I restrict the sample to parents for whom the month-year of their first and last marriages do not precede the month-year of birth of their second child. Thus, this sample includes parents who, prior to the birth of their second child, are (i) single, (ii) in a relationship but neither married nor cohabiting, and (iii) cohabiting but not married.

3.3 Enquête Santé en Milieu Scolaire 1999-2000

Data for children's short-run outcomes is taken from the Enquête Santé en Milieu Scolaire 1999-2000. This survey provides information on children's month and year of birth, birth order as well as health and cognitive outcomes such as weight, vaccinations, dental health and scores on verbal skills tests. The information is reported by government-affiliated physicians, for 30,000 children who are enrolled in their last year of preschool. Given that children of the same age are grouped in the same classes in preschools, the sample only covers children aged 6 who are born in 1994.

Since preschool enrollment is not mandatory, one might worry about selection into the sample. Specifically, since parents are able to spend more time at home, the policy can induce them to not enroll the child in preschool. However, in the French context, this scenario is extremely unlikely. First, although not mandatory, it is estimated that 99% of children are enrolled in preschools by age 4. Second, APE benefits can only be received until the child's third birthday. While it is possible that parents may delay children's preschool enrollment if they are induced to spend more time at home, it is unlikely that they would do so until the child is aged 6.

I use children's performance of on phonological awareness and vocabulary development tests. The phonological awareness test focuses on whether the child is aware of the sound structure of words. The child is asked to identify rhymes and syllables. The vocabulary development test assesses the child's vocabulary development and comprehension. The child is given a series of pictures and asked to identify what he sees. The survey does not report the score on each test but instead, whether the child has a normal score, is between 1 and 2 standard deviations of the normal score or within more than 2 standard deviations of the normal score. The outcomes I look at are dummy variables that are equal to 1 if the child has a normal score on either tests or on both tests. Panel C of Table 1 reports summary statistics for this sample. On average, children enter preschool at age 2.93 and weigh 20.7 kg. 87.9% and 92.7% of children have a normal score on the phonological awareness and vocabulary development tests, respectively.

4 Empirical Strategy

To identify the effects of the APE extension, I exploit the facts that (i) parents of two children are only eligible to receive benefits if their second child is born on or after July 1, 1994, and (ii) the policy is not pre-announced. These two features allow me to use a regression discontinuity design based on the month and year of birth of the second child. For children's short-run outcomes, I further complement the analysis with a difference-in-discontinuity approach (RD-DID), due to data limitations that I discuss in section 4.2. The following describes both identification strategies and presents tests of the validity of the design.

4.1 Regression Discontinuity Design

I use a regression discontinuity design (Imbens and Lemieux, 2008; Lee and Lemieux, 2010) which leverages the cutoff date of July 1, 1994. Specifically, I document parents' response to the APE by comparing the outcomes of parents whose second child was born before July 1, 1994 to parents whose second child was born on or after that date. I also focus on how the APE affects children's well-being by comparing the outcomes of second children born before July 1, 1994 to second children born on or after that date. The only difference between these two groups of parents (children) should be that the latter are exposed to APE benefits while the former are not. The main identifying assumption of the RD design is that they are otherwise similar.

I estimate the following reduced form equation:

$$Y_i = \alpha + \beta D_i + \tau g(R_i) + \delta g(R_i) * D_i + \epsilon_i$$

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 July 1, 1994. R is the running variable and represents the second child's month and year of birth. It is defined as months relative to the cutoff. In most specifications, $g(.)$ is a linear function of R and the equation is estimated using a local linear regression. I allow for differential trends in month-year of birth on either sides of the cutoff by interacting $g(.)$ with D. ϵ is the error term. The coefficient of interest, β, captures the intent-to-treat (ITT) effects of the APE extension on various outcomes. To get the average treatment effect, I would need to rescale β by an estimate of the take-up of the APE. Unfortunately, data on actual take-up of APE benefits is not available. Therefore, all the results in this paper are intent-to-treat effects.

I employ local linear regressions using a narrow range of data around the cutoff. For each outcome, I use uniform kernel weights and the preferred bandwidth is chosen using a

robust data driven procedure introduced by Calonico, Cattaneo and Titiunik (2014). I also show that my results are robust to (i) the use of different bandwidths and functional forms, and (ii) the inclusion of second child's month of birth fixed effects and a set of controls. These controls include the parent's age at the birth of the second child, a dummy variable that is equal to 1 if the parent is born in France and the sex of the second child. In all regressions, standard errors are clustered at the second child's month-year of birth level to deal with concerns over random misspecification error resulting from a discrete running variable (Lee and Card, 2008).

4.2 Difference-in-Discontinuity

As previously mentioned, children's short-run outcomes are drawn from the Enquête Santé en Milieu Scolaire 1999-2000, which provides information on children born in 1994. Therefore, the outcomes are only available for children who are born within 6 months on either sides of the cutoff. One concern is that seasonal effects could be confounding the estimates. In other words, my estimates could be reflecting both month of birth effects and the impact of the policy. To deal with this issue, I show that the estimates for children's short-run outcomes are similar when using both an RD-DID and a regression discontinuity design.

I combine the regression discontinuity and difference-in-differences (RD-DID) approaches by using first children born in the same year, i.e. 1994, as a control group. This is motivated by the fact that parents of first children are not eligible for the APE. Therefore, the policy should not induce any differences between first children born before or after July 1, 1994. The intuition behind the RD-DID estimator is that it takes the difference between the discontinuity at the cutoff for second children (i.e. the effect of the policy and any seasonal effects) and the discontinuity at the cutoff for first children (i.e. the seasonal effects). Assuming that the seasonal effects are the same for first and second children, the RD-DID isolates the effects of the policy on second children's outcomes.

I estimate the following reduced form equation:

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

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 T to interact with R and A. β_5 is the coefficient of interest and γ_i is the error term.

To deal with random misspecification error, standard errors should be clustered at the month-year of birth level (Lee and Card, 2008). However, when looking at children's short-run outcomes, the number of clusters is small and cluster-robust standard errors can be downward biased. Therefore, in all specifications concerning children's short-run outcomes, I show both cluster-robust standard errors and p-values computed using a clustered wild bootstrap-t procedure (Cameron, Gelbach and Miller, 2008).

4.3 Validity Tests

One concern with the identification strategy is that if individuals are able to manipulate the running variable to receive treatment, then the estimated treatment effects would be biased. In this context, it would be problematic if parents are able to strategically time the conception or the date of birth of the second child to become eligible for APE benefits. The extension of the APE was retroactive and was not pre-announced. The law was passed on July 25, 1994 but awards benefits to parents of children born before this date, on July 1, 1994. Therefore, it is unlikely if not impossible that parents are able to precisely time the conception or the date of birth of the child. I present two formal tests that allow me to address concerns over manipulation of the assignment variable.

First, I show that the distribution of the running variable is smooth around the cutoff. Panel A of figure 1 plots the frequency of the running variable. Each circle represents the number of second children born in each month-year.[9] Panel B of figure 2 plots the second births rate as a function of the running variable. Each circle represents the fraction of all births that are second births in each month-year. Both graphs show no clear discontinuity 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.

Second, I show that the distribution of baseline covariates does not change around the threshold. Panels A through E in Figure 2 graph the baseline covariates as a function of the running variable. Unless stated otherwise, these figures take the same form as those after them in that (i) they depict local linear regressions within X months on either sides of the threshold, where X is the preferred bandwidth, in this case 18 months and, (ii) circles represent local averages over a one month range. Panels A through D show insignificant treatment effects on parents' predetermined characteristics such as mothers' and fathers' age at the birth of the second child (0.195 and -0.045 years respectively) and the probability of being born in France (-0.9 and 0.7 percentage points respectively). Panel E further shows that there is no significant effect on the probability that the second child is male (-1.9

[9]The number of second births in each month-year is divided by the number of days in each month. This removes variation in the number of births coming from different number of days within each month.

percentage points). Table 2 reports the corresponding regression discontinuity estimates. Unless stated otherwise, this table takes the same form as those after it in that (i) each row or panel reports the regression discontinuity estimates for the outcome of interest, and (ii) for each outcome, columns show the estimates using the preferred bandwidth as well as bandwidths that are within 3, 6, 9 and 12 months of the chosen bandwidth. The table shows that the estimates are insignificant across different specifications.

5 Results

5.1 Parents' Labor Market Outcomes

Eligible parents can benefit from the APE for up to three years after the birth of their second child. Further, benefit receipt is conditional on the parent either exiting the labor force or working part-time. Therefore, I analyze how the extension of the APE affects the labor supply of mothers and fathers in the first three years after the birth of the second child. This is especially important given that I do not have data on actual take-up of APE benefits.

Mothers' Labor Supply. Although parents can receive benefits by either working part-time or withdrawing completely from the labor force, reports suggest that most recipients choose the latter option (Afsa, 1999). Panels A and B of Figure 3 respectively graph mothers' likelihood of being out of the the labor force (versus being in the labor force i.e. employed or unemployed) and the probability of being out of the labor force or working part-time (versus being unemployed or working full-time) as a function of the second child's month-year of birth. The graphs show that, for the second and third years (years 2 and 3) after the birth of the second child, the policy has a significant impact on both outcomes.

Panels A and B in Table 3 report corresponding regression discontinuity estimates with and without controls and second child's month of birth fixed effects. For both outcomes, the estimates are robust to the choice of bandwidth and to the inclusion of controls. In the specifications that include second child's month of birth fixed effects, mothers are 10.6 percentage points more likely to be out of the labor force and 10.2 percentage points more likely to be out of the labor force or work part-time in years 2 and 3 after the birth of the second child. Both outcomes are similar in magnitudes suggesting that mothers mainly take up the benefits through decreasing their labor force participation as opposed to switching to part-time work.

Appendix Figure A1 and Table A1 show that mothers' labor force participation also declines in the first year (year 1) after the birth of the second child. However, the magnitudes

of the estimates are smaller and the results are more sensitive to the inclusion of second child's month of birth fixed effects. This is likely due to the fact that prior to taking APE benefits, mothers are on maternity leave in the first 10 weeks after the birth of the second child. Further, the LFS is conducted in March so, in my sample, mothers of children born in December are still on maternity leave in year 1.

Fathers' Labor Supply. *All Fathers.* Both mothers and fathers are eligible for the APE. However, mothers constitute 98% of beneficiaries (Piketty, 2005). Panels A through C in Figure 4 are consistent with this idea. The graphs show that the policy has no impact on fathers' likelihood of being in the labor force, being employed (conditional on being in the labor force) or working full-time (conditional on being employed). I next look at the effect of the policy on actual hours of work during the reference week and usual hours of work in a typical week. Usual hours reflects the number of weekly hours of work over a long period of time and, contrary to actual hours of work, it does not include (i) individuals who have irregular work schedules and (ii) irregular overtime work or absences. In that sense, for individuals with regular work schedules, actual hours of work can be interpreted as the sum of usual hours of work and any unusual overtime or absences.

Panel D of Figure 4 plots actual hours worked per week as a function of the running variable. Although the estimate is not statistically significant at the preferred bandwidth, the graph seems to show a positive effect. Panel E of Figure 4 plots a dummy variable that is equal to 1 if the father occasionally works at night (versus never works at night or usually works at night) as a function of the running variable. The graph reveals a significant positive treatment effect to the order of 4.1 percentage points. Panel F of Figure 4 shows that the policy has no effect on fathers' weekly usual hours of work. Taken together, these results suggest that in response to mothers' leave take-up, fathers increase their non usual work time (through the rise in occasional night work) but not their usual hours of work. This might explain the positive but not statistically significant effect on actual hours of work.

Table 4 reports regression discontinuity estimates for all different outcomes. The results are robust when using different bandwidths and including controls and second child's month of birth fixed effects. For actual hours of work (Panel D of Table 4), some bandwidths reveal statistically significant estimates ranging from 1.4 to 2.1 hours a week. The estimates for occasional night work in Panel E of Table 4 are between 3.4 and 5.1 percentage points.

Fathers with a high school degree and more. The above results indicate that fathers' labor supply is not necessarily affected by the policy. However, this does not rule out that their response to the APE can be heterogeneous. In fact, the amount of time that fathers spend at work versus home is usually expected to increase with their level of education (Lundberg

and Rose, 1999). Thus, I look at the effects on fathers with a high school degree and more in years 2 and 3 after the birth of the second child. Unfortunately, I do not have access to information on fathers' level of education prior to the birth of their second child. As a result, I check whether the policy affects fathers' probability of having a high school degree and more. Panel A of Appendix Figure A2 reveals no significant treatment effect.

Panels A through C of Figure 5 reveal that threshold crossing does not affect fathers' labor force participation, employment or the probability working full-time. Panels D through F of figure 5 show that (i) fathers with a high school degree and more significantly increase both their actual and usual hours of work and (ii) this effect is driven by a rise in the probability of usually working more than 40 hours a week i.e. working overtime.[10]

Table 5 reports regression discontinuity estimates for the outcomes of interest across different bandwidths as well as with and without controls. Panels A through C confirm that the policy has no effect on labor force participation, employment or full-time work as the results are all insignificant and robust across different specifications. At the preferred bandwidth, Panels D and E show an increase of 5.5 and 4.5 in actual and usual hours of work, respectively.[11] Across different bandwidths, the estimates range from 4.3 to 6.3 for actual hours of work and from 3 to 5 for usual hours of work. The probability of usually working overtime increases by 19.5 percentage points and the estimates are robust across various specifications.

Fathers with less than a high school degree. I now turn to the sample of fathers with less than a high school degree. Panels A through C of Figure 6 reveal no significant treatment effects on labor force participation, employment or the likelihood of working full-time. These results are confirmed in Panels A through C of Table 6 which report the corresponding regression discontinuity estimates.

Although most APE recipients are women, some fathers do take up the benefits. Reports suggest that these fathers (i) are usually less educated than their spouses and (ii) have professions that are classified as lower middle class (see Boyer, 2004).[12] Thus, I restrict my sample to fathers who have less than a high school degree and work in jobs that are considered lower middle class. Given that I do not have information on fathers' jobs prior to the birth of the second child, I show that the policy has no impact on the probability of being in a lower middle class profession in Panel C of Appendix Figure A2.

[10] Before 2000, full-time work in France was less than 39 hours a week. 40 hours and more were considered overtime.

[11] Usual hours of work do not include individuals with irregular schedules. Panel B of Appendix Figure 2 shows that the policy has no effect on the likelihood of having an irregular schedule.

[12] Specifically, most of them have jobs that are classified as *Employés* (employees) or *Professions Intermédiaires*.

The results for this subsample are reported in Panels D and E of Figure 6 and Panels D and E of Table 6. The graphs show that the policy has no effect on the probability of being employed but does significantly decrease the probability of working full-time versus part-time (conditional on being employed).[13] The results indicate that the likelihood of working full-time decreases by 6.4 percentage points, with the estimates ranging from -5.9 to -8.8 percentage points.

5.2 Marriage Outcomes

I now look at the effects of the policy on short-run marital outcomes. The results in this section are for the year 1999 i.e. within 4 to 6 years after the birth of the second child. This is when parents are no longer eligible to receive APE benefits.

I first show the results for all mothers and fathers. Panel A of Figure 7 and Table 7 report no significant treatment effect on the probability of being married in 1999. Next, I restrict my sample to individuals who are unmarried prior to the birth of the second child. Thus, this sample includes individuals who are either single or cohabiting but unmarried prior to the birth of the second child. I further show the results for mothers and fathers separately.

Panel B of Figure 7 and Table 7 indicate that the policy significantly increases unmarried fathers' likelihood of being married. Specifically, at the preferred bandwidth and with the inclusion of month of birth fixed effects, fathers are 11 percentage points more likely to be married in 1999. The results are robust across different specifications. Panel C of Figure 7 and Table 7 show the results for mothers who are unmarried prior to the birth of the second child. The graph along with the regression discontinuity estimates indicate that threshold crossing has no effect on mothers' likelihood of being married.

It should be noted that the sample does not include mothers and fathers of the same child. In fact, the Enquête Histoire Familiale surveys either all men or all women living in a household. One explanation for the divergent effects between mothers and fathers is that the sample of unmarried mothers is very different than the sample of unmarried fathers. Specifically, I allow for individuals who are single or living as a couple but unmarried (before the birth of the second child) to be included in the sample. It is likely that the sample of mothers has a higher number of individuals who are single (rather than unmarried but in a couple) than the sample of fathers. In fact, in 1990, 89.6% of single parent families, with children who are less than 16 years-old, are headed by women.[14] Including single mothers in the sample could be driving the insignificant treatment effects because they do not benefit

[13] I do not report the results for labor force participation as all individuals around the cutoff are in the labor force.

[14] `http://www.insee.fr/fr/ffc/docs_ffc/ref/CCFAMONOc_Demographi.pdf`

from the APE as they have access to a more generous policy.

5.3 Children's outcomes

I now analyze the effects of the extension of the parental leave on children's outcomes. I start by discussing why parental leave is expected to affect children's well-being. I then show results for children's short-run outcomes, as well as fertility and infant mortality.

Parental leave and children's outcomes. The main channel through which parental leave can affect a child's health and development is through increasing the time that parents spend at home. Mothers' time away from work is associated with an increase in the incidence and length of breast-feeding, as well as more frequent medical check-ups and closer monitoring of children (Berger, Hill and Waldfogel, 2005; Baker and Milligan, 2008). Breast-feeding in particular can decrease infant mortality, the occurrence of certain diseases and may have positive effects on children's cognitive outcomes (Ruhm, 2000; Tanaka, 2005). While the evidence regarding paternal involvement is scarce, it is often believed that increased time spent with the father can have positive effects on the child's development (El Nokali, Bachman and Votruba-Drzal, 2010).

An increase in parents' time at home usually reduces the time that the child spends with other caregivers. Although it is important for the child to bond with his mother in his first year, he could benefit more from interacting with other individuals at a later age (Dustmann and Schönberg, 2012).

A child's well-being can also be affected by a loss of household income. Specifically, negative income shocks can reduce access to health care, pediatric services and investments in child-related goods and services. This might deteriorate the child's health and impede his development. The APE offers partial compensation to parents who wish to exit the labor force or switch to part-time work. In that sense, it could lead to a loss of income for some households. However, it is unlikely that this would reduce access to medical services because France has a universal health care system.

Finally, parental leave policies can increase fertility and decrease birth spacing. This can hinder a child's development if he receives reduced care and attention due to the higher number of children at home (Dustmann and Schönberg, 2012).

Short-run outcomes. I start by looking at the impact of the APE on children's short-run outcomes. Panel A of Figure 8 plots the age of the child at the start of preschool as a function of the running variable. This graph takes the same form as all other panels of Figure 8 in that the lines are local linear regression using 6 months on either sides of

the cutoff and circles represent local averages over a one month range. This outcome can help determine whether parents substitute their time with the child for the time that the child spends in preschool. In fact, parents can benefit from the APE until the child's third birthday and children can be enrolled in preschool at the age of 2. The graph shows no noticeable discontinuity at the cutoff. Column 1 of Table 8 reports the estimates from the regression discontinuity design in Panel A and from the RD-DID in Panel B. The estimates (0.005 years or 2 days) are small, insignificant and comparable when using bandwidths of 4 and 6 months as well as both identification strategies.

Next, I look at the effect of the policy on the child's weight at age 6. This can indicate whether the policy allows parents to monitor their children's health more closely. Panel B of Figure 8 shows that the variable is smooth at the cutoff. The estimates are reported in column 2 of Table 8 and indicate no significant treatment effects.

Finally, I focus on the verbal skills tests administered at age 6. Panel C through E of Figure 8 graph a dummy variable that is equal to 1 if the child has a normal score on the phonological awareness, vocabulary development and both phonological awareness and vocabulary development tests, respectively. All three graphs show that the APE negatively affects the child's verbal skills. The estimates are reported in columns 3 to 5 of Table 8. The probability that the child receives a normal score decreases by 3.8, 2.4 and 4.2 percentage points on the phonological awareness, vocabulary development and both phonological awareness and vocabulary development tests, respectively. Precision is reduced when using the RD-DID in Panel B but the estimates are comparable to the ones from the RD in Panel A.

Fertility and infant mortality. I now turn to the effects of the parental leave on fertility and birth spacing. Although Piketty (2005) shows that the APE has no effect on both outcomes using differences-in-differences, I still present the results for several reasons. First, I am using a different identification strategy. Therefore, it is important to show that the results are similar when using a regression discontinuity design. Second, I document negative effects on children's cognitive abilities. By showing that the policy does not impact fertility and birth spacing, I rule this out as one of the channels through which the child is negatively affected.

Panels A through C of Figure 9 respectively plot the number of children, the age difference between the first and second child as well as between the second and third child as a function of the second child's month-year of birth. The results correspond to the year 1999, i.e. 5 years after the birth of the second child. The figures do not show any discontinuities across the threshold. This is confirmed by the regression discontinuity estimates that are reported in Panels A through C of Table 9. The estimates for number of children (0.006), age difference

between first and second child (0.038 years or 14 days) and between second and third child (-0.053 years or -19 days) are small and statistically insignificant. Further, they are robust to the inclusion of controls and across different bandwidths.

Next, I investigate the effects of the policy on infant mortality. Panel D of Figure 9 plots a dummy variable that is equal to 1 if the child passes away at birth or after birth until the age of 3 as a function of the running variable. The graph shows no discontinuity at the cutoff. Table 10 reports the regression discontinuity estimates for this outcome. The estimate is small (-0.9 percentage points), statistically insignificant and robust across different specifications.[15]

5.4 Robustness Checks

In this section, I run additional robustness checks. First, I check for discontinuities in the main outcomes when using July 1 in years other than 1994 as a fake cutoff. The idea is that if we see discontinuities at the fake cutoff, then I cannot interpret the observed effects as being the result of the policy. Panels A through D of Figure 10 plot parent's main outcomes as a function of the running variable, with July 1, 1992 as the fake cutoff. The figures show no significant treatment effects for mothers' labor force participation (-0.5 percentage points), the likelihood that fathers occasionally work at night (-1 percentage points), the usual weekly hours of work for fathers with a high school degree and more (0.33 hours), and the probability that fathers are married by 1999 given that no marriage was declared prior to the birth of the second child (-1.8 percentage points).[16]

Second, I check for discontinuities in children's short-run outcomes when using the month and year of birth of the first child as the running variable. Panels A through E of Appendix Figure A3 plot these outcomes as a function of the fake running variable. All graphs show no clear discontinuities at the cutoff. This highlights the fact that the observed discontinuities in second children's performance on the image and sounds test are driven by the policy and not by seasonal effects.

6 Interpretation

This paper shows that the APE affects various family outcomes. In this section, I discuss the potential mechanisms that could be driving these effects.

[15] I tested whether the policy affects infant mortality at birth and after birth separately. The estimates are small and statistically insignificant. The results are available upon request.

[16] I also find no significant effects or discontinuities when using July 1 1993, 1995 and 1996 as fake cutoffs. These results are available upon request.

One of the main findings of the paper is that fathers respond to the policy in a heterogeneous way. Fathers who are more educated increase their weekly hours of work. There are two possible explanations for this result. First, the APE does not offer full income replacement. Therefore, when mothers take up the policy, they could incur a loss of income. Fathers could then be increasing their hours of work to offset a negative household income shock. Second, if couples substitute their time in home production, then the increase in mothers' time at home could decrease fathers' opportunity cost of working. This would also drive the increase in work time.

Fathers who have less than a high school degree and hold jobs in "lower middle class" professions are more likely to switch to part-time work, which is consistent with APE take-up. This suggests that since the policy offers only partial income replacement, the lower earning spouse could be the one taking up the benefits. The fact that most APE recipients are women could be the result of mothers having a comparative advantage in childcare (for example, breastfeeding) but also a gender wage gap in the labor market.[17]

Another finding is that the APE increases the marriage rate of fathers who are unmarried prior to the birth of their second child. This can be driven by the increase in household specialization. In fact, the APE induces one parent, generally the mother, to invest in marriage-specific human capital through specializing in home production. This increases the costs of separation since the induced withdrawal from the labor market could result in losses in terms of job experience and future income. This could then translate into a rise in marriage rates among unmarried couples. Therefore, the marriage contract could serve as an "insurance" against these potential losses.

The policy has no effect on children's health but negatively impacts their verbal skills at age 6. There are several reasons why the program can result in a negative effect. First, if the policy increases the number of children in the household, then each child may receive less attention. However, as shown in section 5.3, the policy had no significant effect on fertility or birth spacing within 5 years of take-up.

Second, the APE provides only partial income replacement. This could decrease investments in child-related goods and services if the policy reduces household income. Third, since the policy increases one parent's – generally the mother– time at home for up to three years, it is important to understand with whom the child would have spent more time otherwise. In fact, parents could leave their children in the care of registered child minders or publicly-funded nurseries or resort to informal care. Unfortunately, I do not have access to data that would allow me to test for whether mother's increased time at home crowds out

[17]In 1993, women, in France, working full-time earned on average 15% less than men.
Source: `http://travail-emploi.gouv.fr/IMG/pdf/2012-015_v2_modif.pdf`

any of these childcare arrangements. The policy could also induce parents to postpone the child's entry into preschool. However, as shown in section 5.3, there are no significant effects on child's age at the beginning of preschool.

I do, however, find that more educated fathers work longer hours but some less educated fathers switch from full-time to part-time work. This suggests that a fraction of children are less exposed to their fathers. Further, children may be spending more time with low educated fathers and less time with high educated fathers. This could be an important mechanism since previous studies found strong associations between children and their fathers' level of education. Additionally, one potential channel for this association is that high educated parents provide their children with "more cognitively stimulating home learning environments and more verbal and supportive teaching styles" (Kalil, Mogstad, Rege and Votruba, Forthcoming).

7 Conclusion

This paper analyzes the effects of a long period of paid leave on parents' labor market decisions and children's well-being. I exploit the extension of a three-year French leave program to parents of second children born on or after July 1, 1994. Using a regression discontinuity design based on second child's date of birth cutoff, I find that this program increases intra-household specialization. Mothers take the leave by exiting the labor force for three years after the birth of a second child and well educated fathers increase their hours of work. I further show that offering a long period of paid leave has no significant effects on children's health but negatively affects their verbal skills in the short run.

My findings suggest that parental leave programs can work against their intended goals and could potentially have significant implications. In fact, the leave promotes a traditional division of labor within the household for at least three years. This could lead to possible losses for women in the labor market and a reduction of their bargaining power within the marriage.

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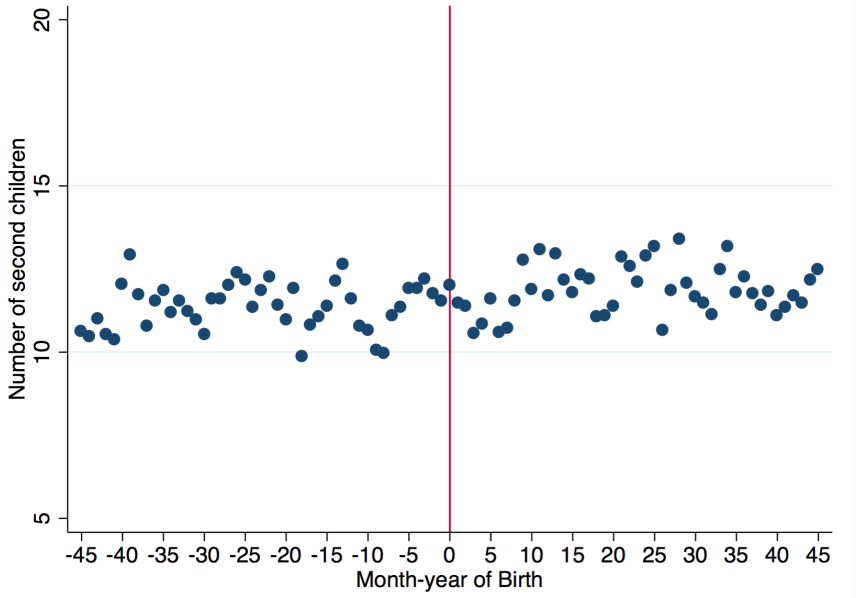
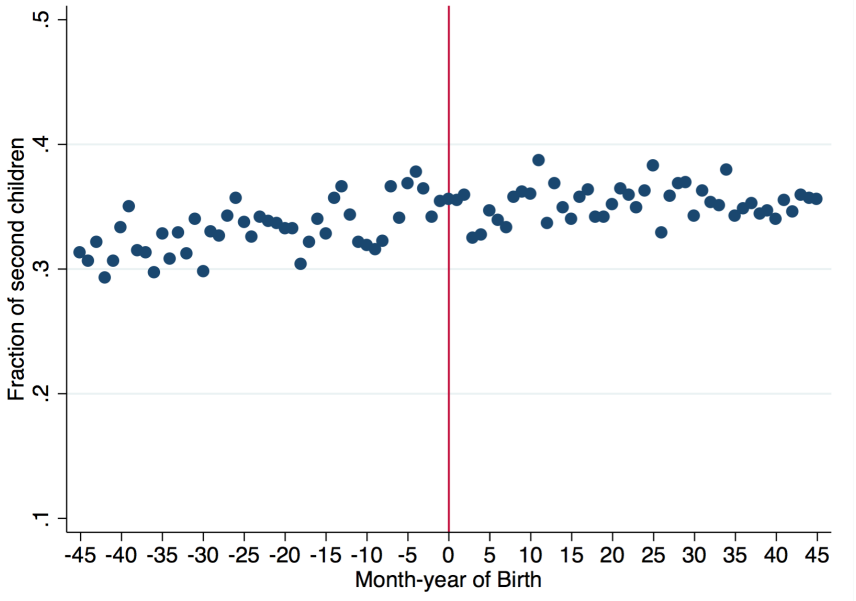
A Figures

Figure 1: Timing of births

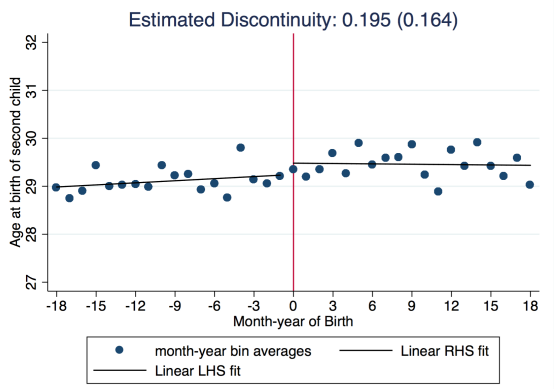


(a) Frequency of the running variable

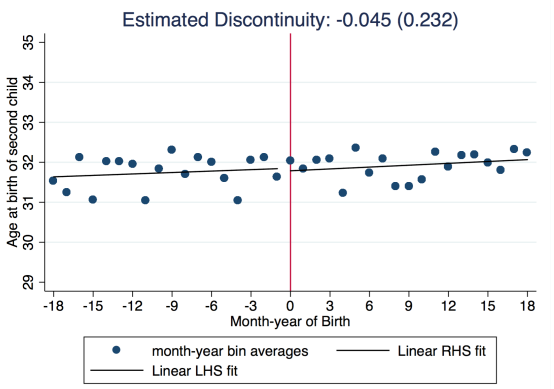


(b) Fraction of second children

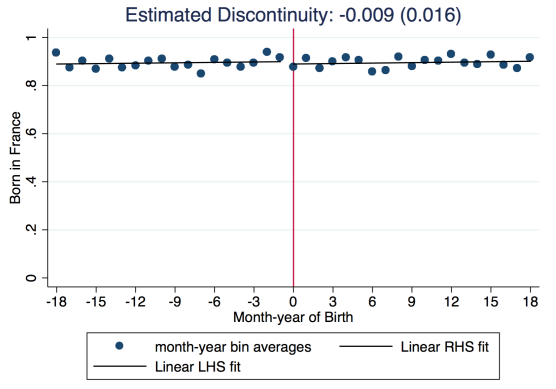
Figure 2: Smoothness of baseline covariates



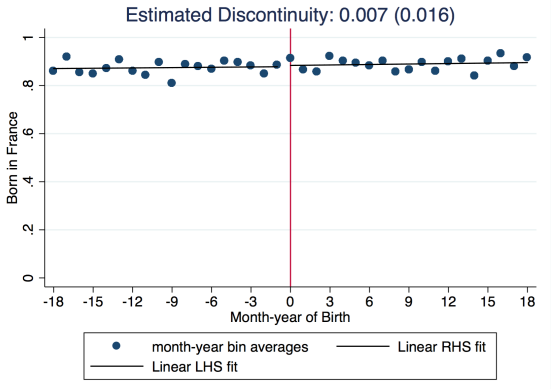
(a) Mother's age at birth of second child



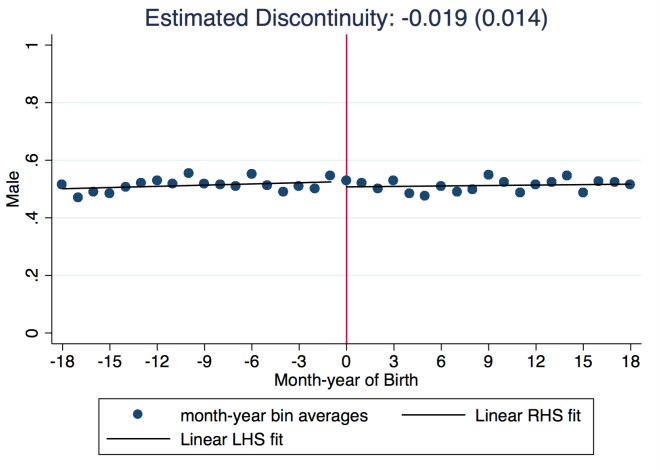
(b) Father's age at birth of second child



(c) Mother is born in France

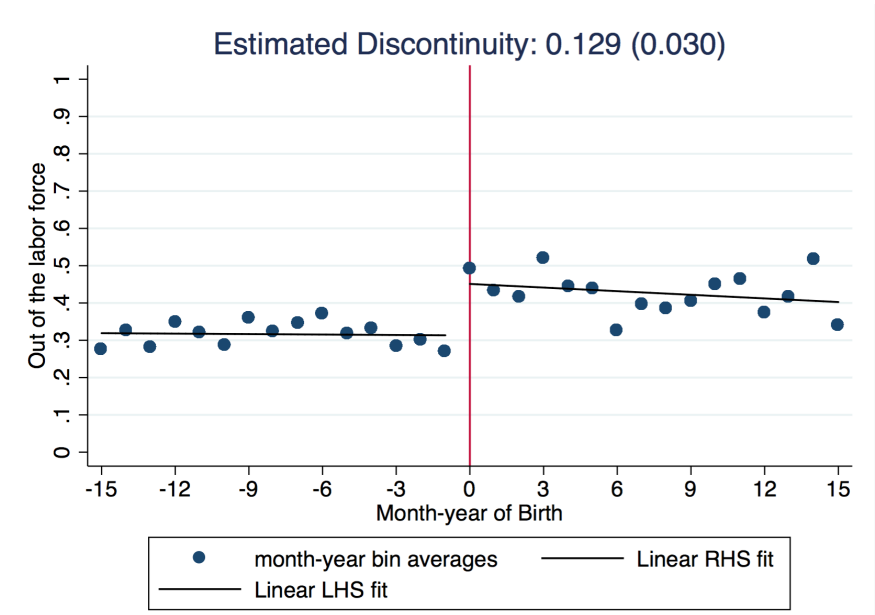


(d) Father is born in France

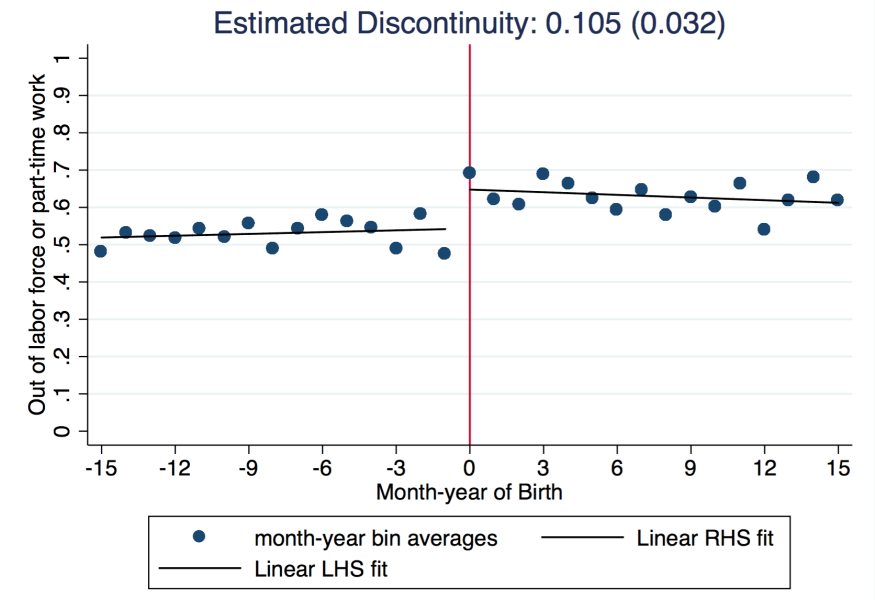


(e) Second child is male

Figure 3: Mothers' labor supply (Years 2 + 3)

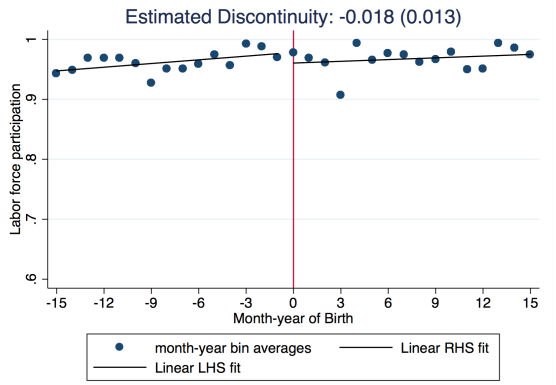


(a) Probability of being out of the labor force

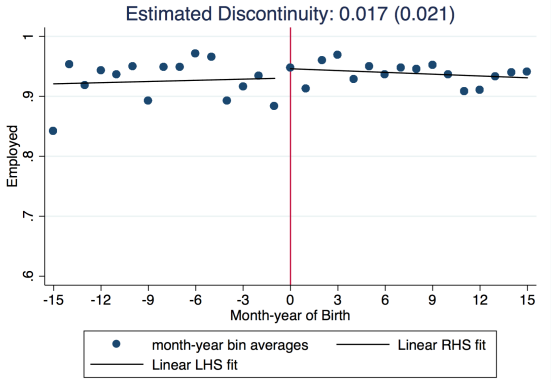


(b) Probability of being out of the labor force or working part-time

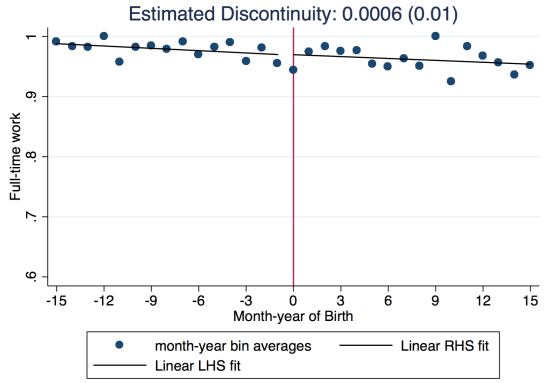
Figure 4: All Fathers' labor supply (Years 2 + 3)



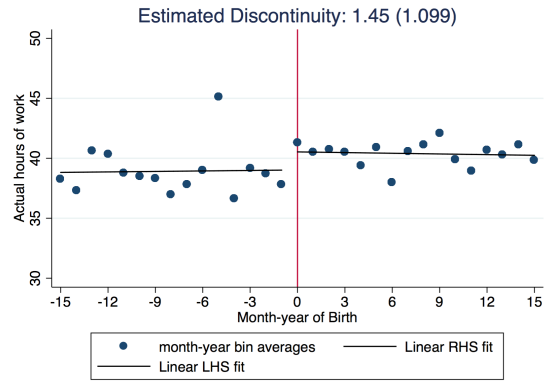
(a) Probability of being in the labor force



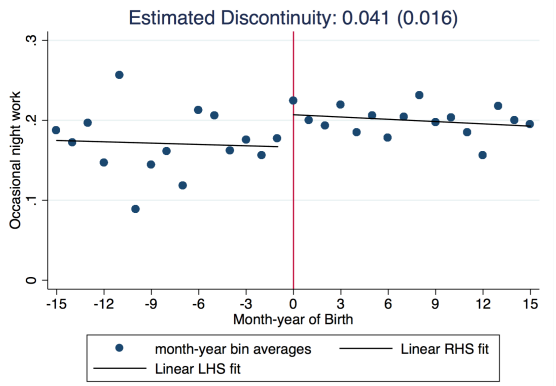
(b) Probability of being employed (versus unemployed)



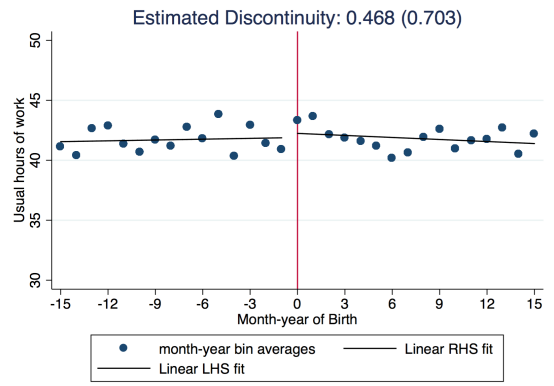
(c) Probability of working full-time (versus part-time)



(d) Weekly actual hours of work

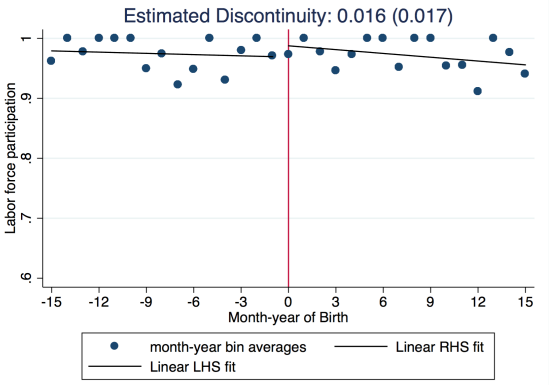


(e) Probability of working occasionally at night

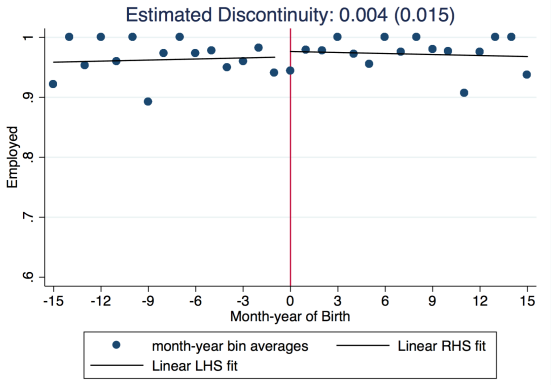


(f) Weekly usual hours of work

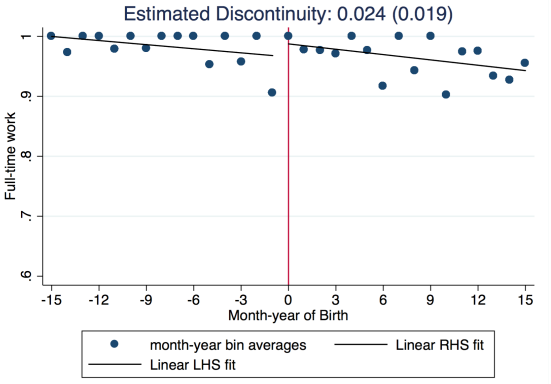
Figure 5: Labor supply for fathers with high school degree and more (Years 2 + 3)



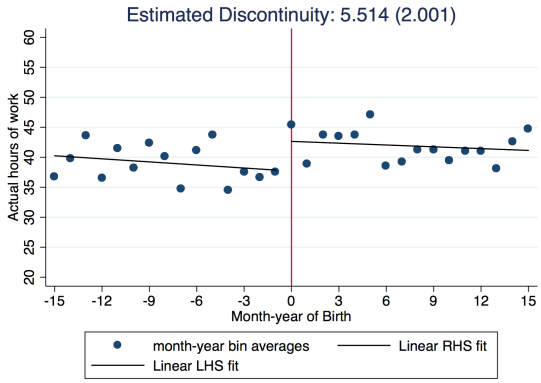
(a) Probability of being in labor force



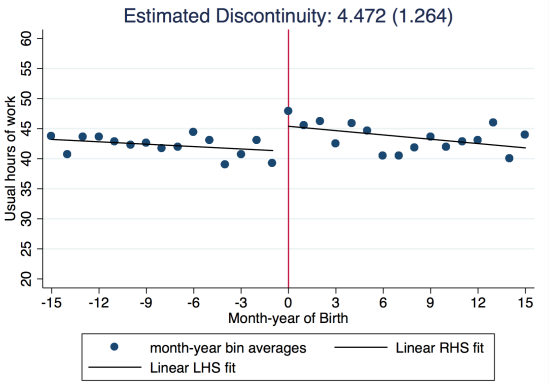
(b) Probability of being employed (versus unemployed)



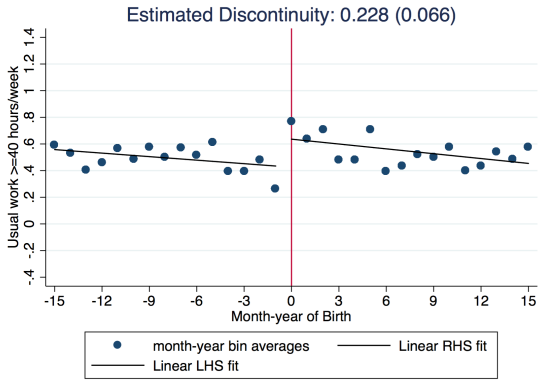
(c) Probability of working full-time (versus part-time)



(d) Weekly actual hours of work

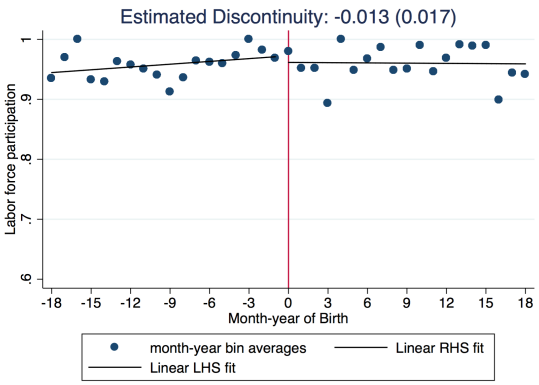


(e) Weekly usual hours of work

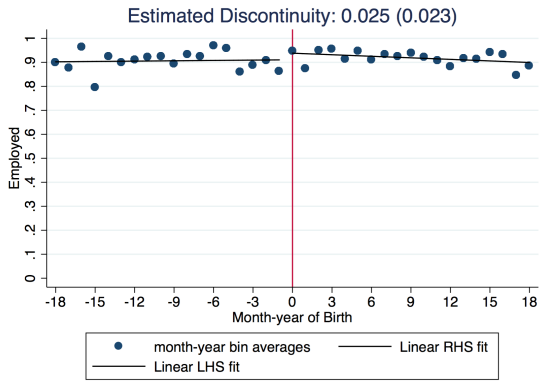


(f) Probability of usually working >= 40hours/week

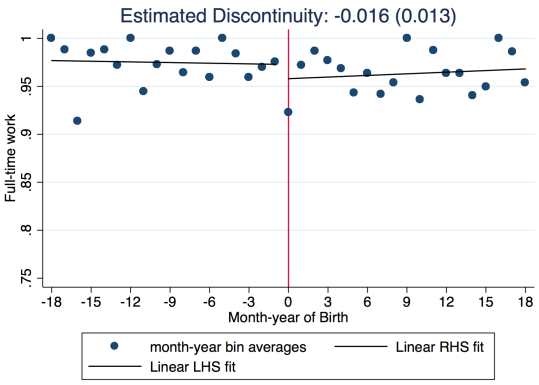
Figure 6: Labor supply for fathers with less than a high school degree (Years 2 + 3)



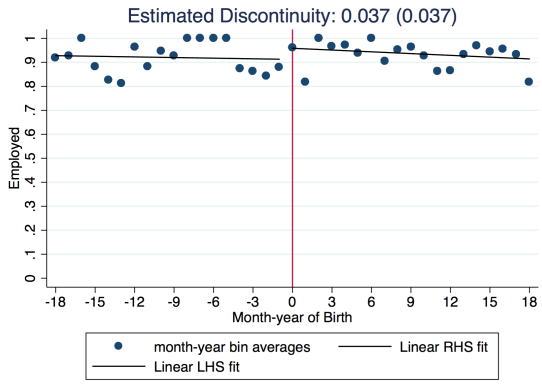
(a) Probability of being in labor force



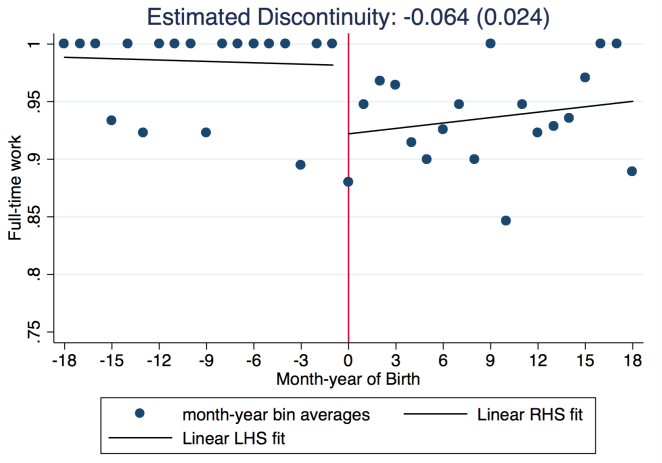
(b) Probability of being employed (versus unemployed)



(c) Probability of working full-time (versus part-time)

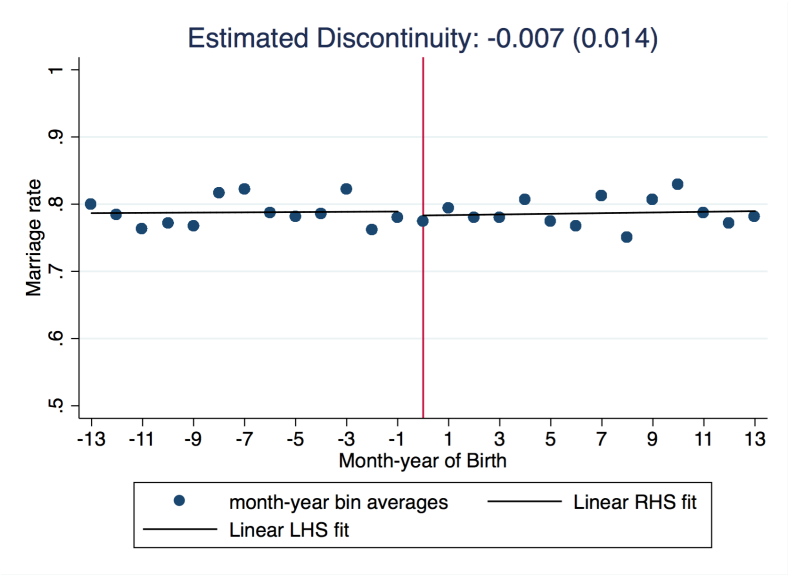


(d) Probability of being employed (lower middle class professions)

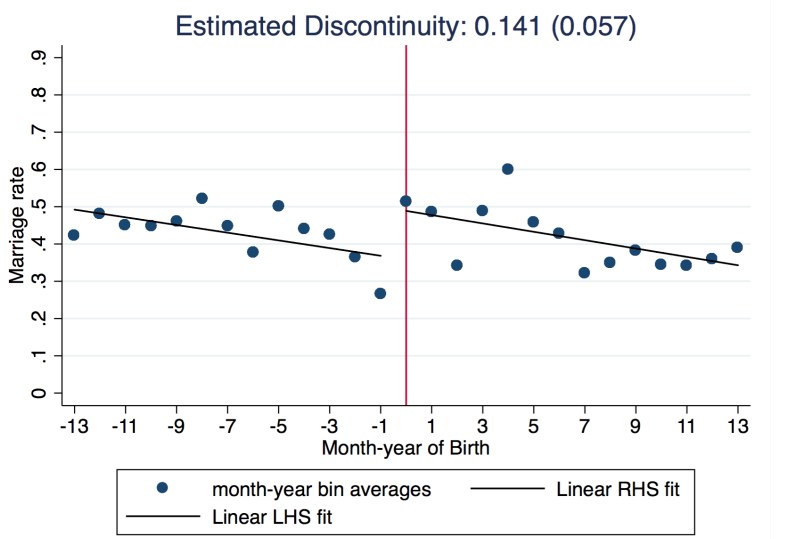


(e) Probability of working full-time (lower middle class professions)

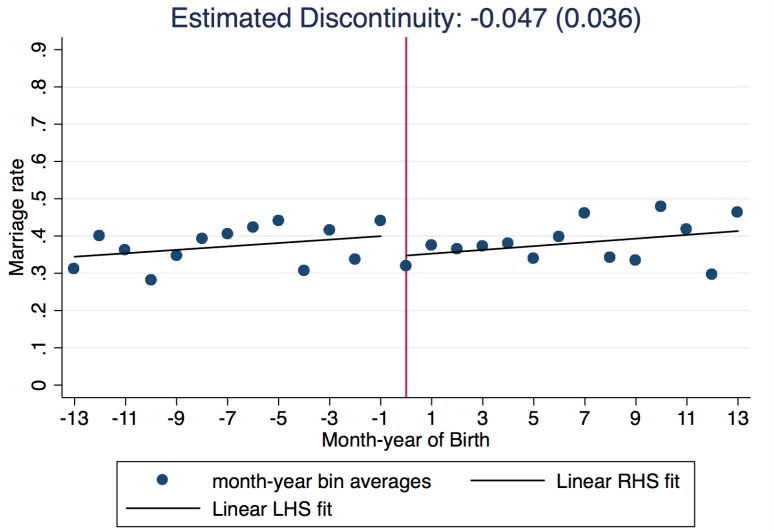
Figure 7: Marital Outcomes



(a) Probability of marriage (entire sample)

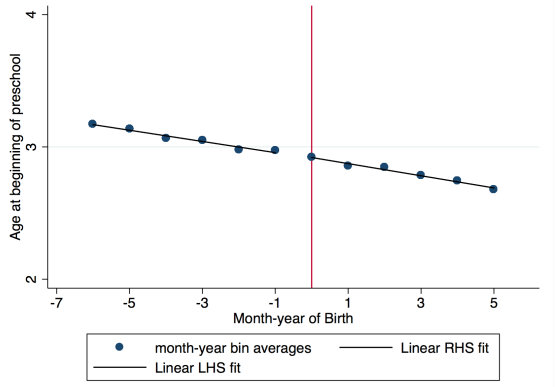


(b) Probability of marriage (fathers unmarried prior to birth of second child)

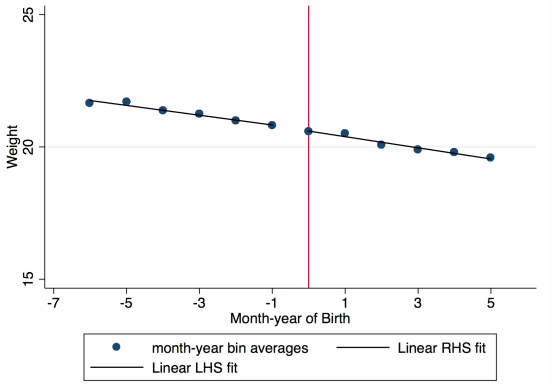


(c) Probability of marriage (mothers unmarried prior to birth of second child)

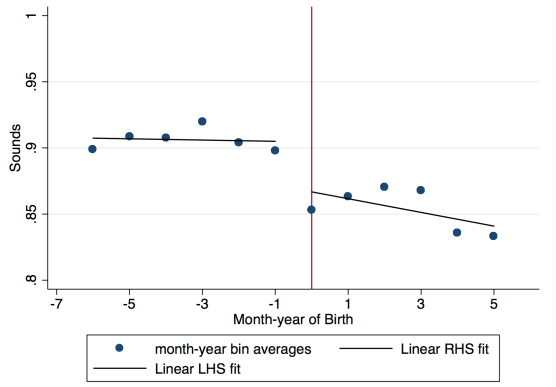
Figure 8: Children's short-run outcomes



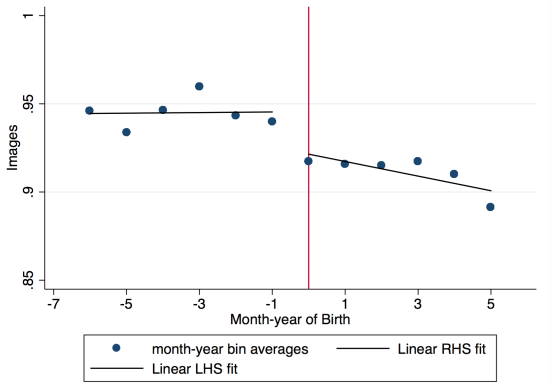
(a) Age at the beginning of preschool



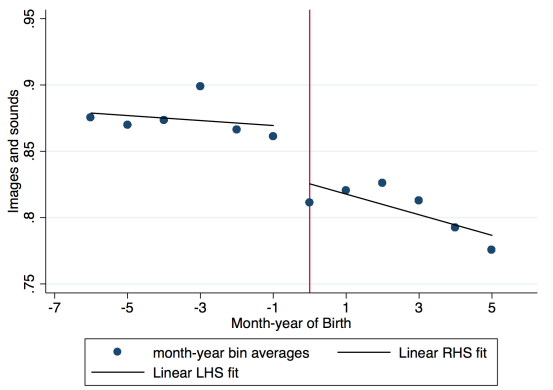
(b) Weight



(c) Probability of having a normal score on the phonological awareness test

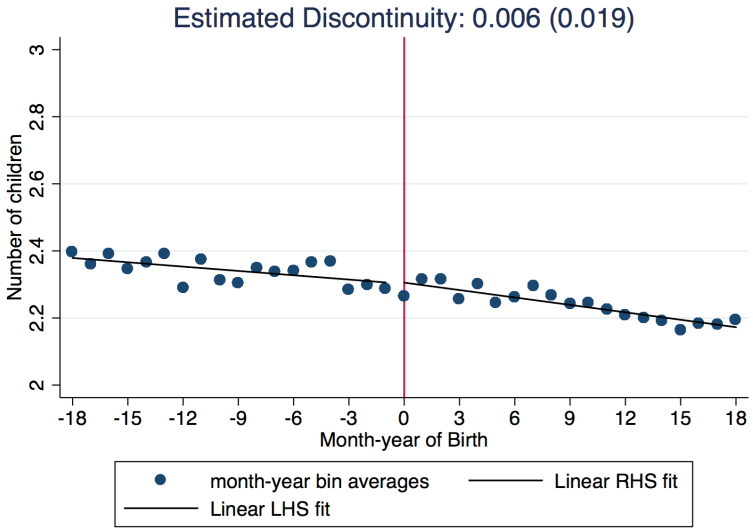


(d) Probability of having a normal score on the vocabulary development test

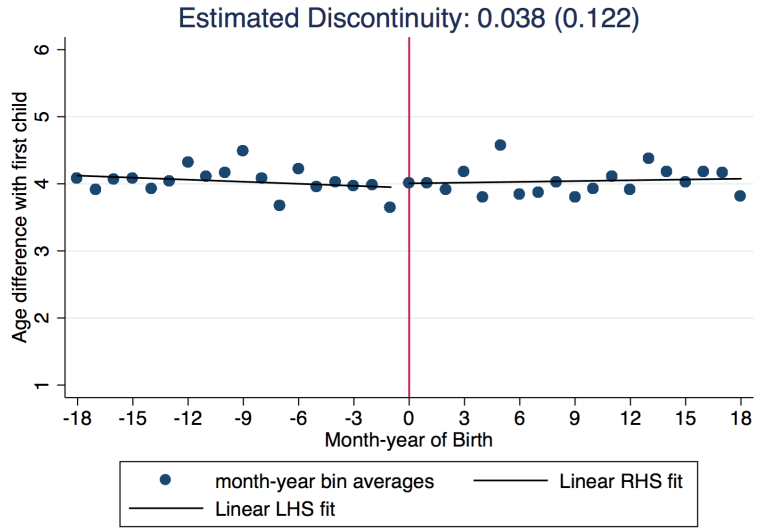


(e) Probability of having a normal score on both tests

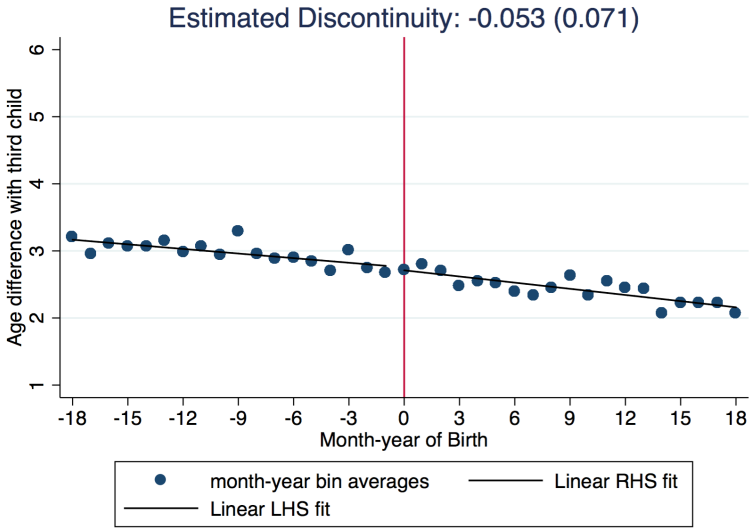
Figure 9: Fertility and infant mortality



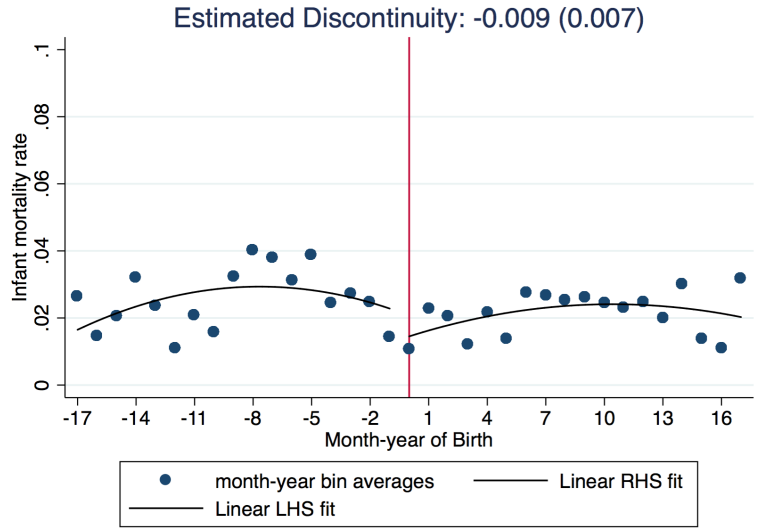
(a) Number of children



(b) Age difference between first and second child

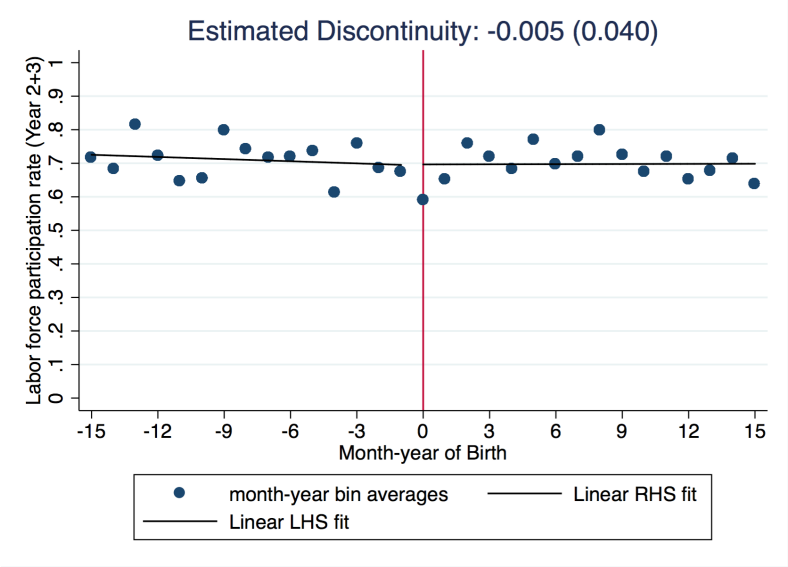


(c) Age difference between second and third child

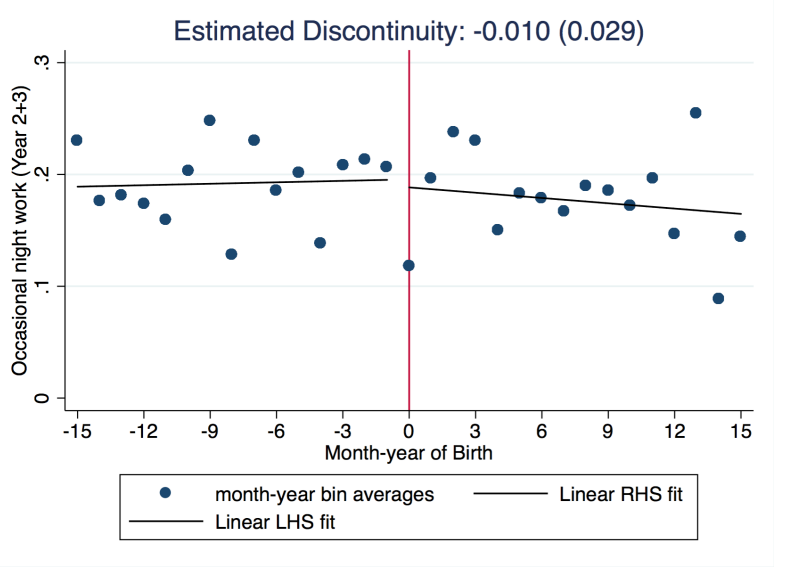


(d) Infant mortality

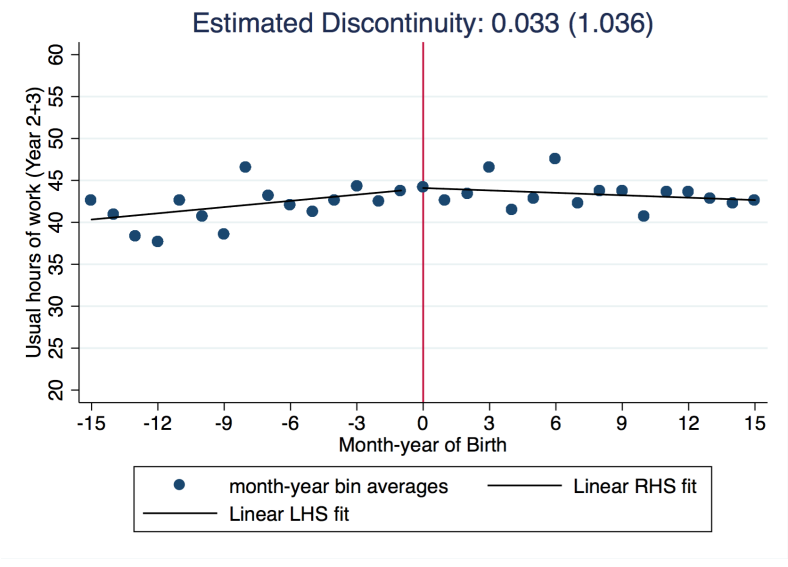
Figure 10: Parent's outcomes with birth year 1992



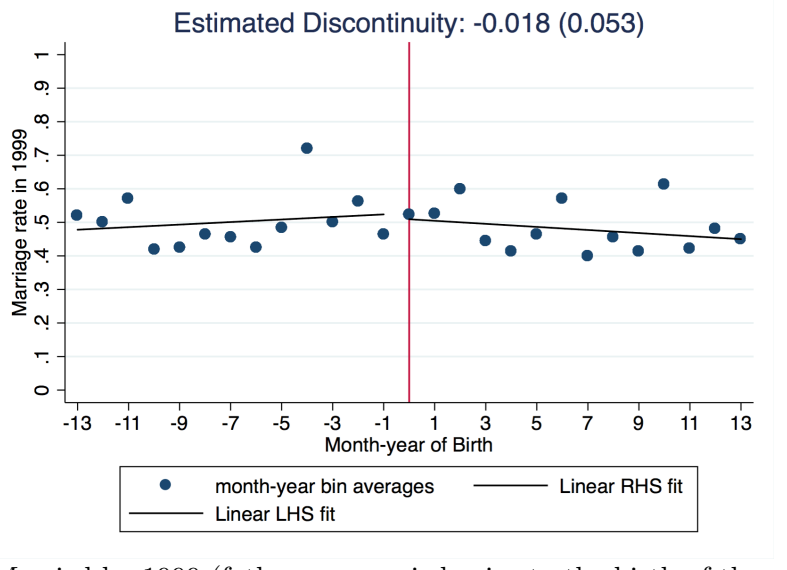
(a) Labor force participation (all mothers)



(b) Occasionally works at night (all fathers)



(c) Usual hours of work (Fathers high school degree and more)



(d) Married by 1999 (fathers unmarried prior to the birth of the second child)

B Tables

Table 1: Summary statistics

Panel A: LFS 1990-2002			**Panel B: Enquête Famille**	
	Year 2	Year 3		
All Mothers				
Out of labor force	.372	.356	Mother's age at birth	29.3
	(.483)	(.478)		(4.34)
	[4323]	[4386]		[13127]
Part-time work	.617	.601	Mother born in France	0.90
	(.486)	(.489)		(.303)
	[2267]	[2344]		[12780]
All Fathers				
Labor force participation	.964	.968	Father's age at birth	31.8
	(.185)	(.176)		(4.88)
	[4039]	[4055]		[8138]
Full-time work	.967	.970	Father born in France	.887
	(.178)	(.170)		(.317)
	[3628]	[3693]		[7904]
Actual hours of work	39.7	39.9	Father >= high school	.372
	(16.2)	(16.2)		(.483)
	[3628]	[3693]		[7674]
Usual hours of work	41.6	42.0	Second child is male	.511
	(9.28)	(9.38)		(.499)
	[3049]	[3099]		[21265]
Working >= 40hours/week	.377	.385	Second child deceased	.02
	(.485)	(.487)		(.139)
	[3049]	[3099]		[21265]
Panel C: Enquête Santé				
Age in Preschool	2.93			
	(.522)			
	[8849]			
Weight	20.7			
	(3.43)			
	[8849]			
Phonological awareness test is normal	.879			
	(.326)			
	[6215]			
Vocabulary development test is normal	.927			
	(.259)			
	[6215]			

For each variable, this table reports the mean, standard deviation in parantheses and the number of observations in brackets.

Table 2: Regression discontinuity estimates for baseline covariates

Bandwidth	-6 months	-3 months	18 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Age of mother at birth							
of second child	.258	.211	.196	.113	.190	.303	.293
	(.20)	(.18)	(.16)	(.19)	(.20)	(.20)	(.19)
Panel B:							
Age of father at birth							
of second child	.141	–.004	–.045	.241	.098	.102	–.032
	(.27)	(.25)	(.23)	(.28)	(.27)	(.25)	(.25)
Panel C:							
Mother born in France	–.023	–.020	–.009	–.007	–.002	–.003	–.010
	(.02)	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)
Panel D:							
Father born in France	.008	.011	.007	.021	.018	.020	.020*
	(.02)	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)
Panel E:							
Second child is male	.001	–.016	–.019	–.018	–.016	–.013	–.007
	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)
Polynomial (Panels A-B)	One	One	One	Two	Two	Two	Two
Polynomial (Panels C-E)	One	One	One	One	One	One	One
Obs. (Panel A)	5143	6511	7773	9047	10409	11798	13127
Obs. (Panel B)	3151	4005	4805	5598	6460	7303	8138
Obs. (Panel C)	5008	6339	7570	8803	10125	11483	12780
Obs. (Panel D)	3069	3901	4682	5450	6277	7094	7904
Obs. (Panel E)	8294	10516	12578	14645	16869	19101	21265

*** p <0.01 ** p <0.05 * p <0.1.

The table shows regression discontinuity estimates using bandwidths that are within 3, 6, 9 and 12 months of the preferred bandwidth of 18 months.

The variable "born in France" has missing observations.

Standard errors are clustered at the month-year of birth level.

Table 3: Regression discontinuity estimates for mothers' labor supply in years 2 and 3 after the birth of the second child

Bandwidth	-6 months	-3 months	15 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Out of the labor force	.210***	.161***	.129***	.121***	.111***	.134***	.137***
	(.02)	(.03)	(.03)	(.03)	(.03)	(.03)	(.02)
With Controls	.206***	.160***	.106***	.116***	.097***	.106***	.125***
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.02)
Panel B:							
Out of the labor force							
or working part-time	.146***	.121***	.105***	.100***	.100***	.116***	.106***
	(.04)	(.03)	(.03)	(.03)	(.03)	(.03)	(.02)
With Controls	.147***	.121***	.102***	.100***	.096***	.103***	.099***
	(.04)	(.04)	(.03)	(.02)	(.02)	(.02)	(.02)
Polynomial	One	One	One	One	One	One	One
Observations	2538	3419	4324	5262	6150	7024	7823

*** p <0.01 ** p <0.05 * p <0.1.
The table shows regression discontinuity estimates using bandwidths that are within 3, 6, 9 and 12 months of the preferred bandwidth i.e. 15 months.
Controls include the second child's gender and month of birth fixed effects as well as the mother's age at the birth of the second child and a dummy variable that is equal to 1 if she was born in France.
Standard errors are clustered at the month-year of birth level.

Table 4: Regression discontinuity estimates for all fathers' labor supply in years 2 and 3 after the birth of the second child

Bandwidth	-6 months	-3 months	15 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Labor force participation	–.033*	–.016	–.018	–.004	.002	–.004	.002
	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)
With Controls	–.033*	–.015	–.014	–.005	.006	.002	.006
	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Panel B:							
Employed	.030	.033*	.017	.019	.021	.014	.017
	(.02)	(.02)	(.02)	(.02)	(.02)	(.01)	(.01)
With Controls	.036	.037*	.018	.015	.019	.013	.020
	(.02)	(.02)	(.02)	(.02)	(.01)	(.01)	(.01)
Panel C:							
Full-time work	.005	–.002	.001	–.010	–.012	–.016*	–.010
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
With Controls	.007	–.002	.000	–.008	–.014	–.016	–.010
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Panel D:							
Actual hours of work	1.623	1.864*	1.447	1.814*	2.160**	1.980**	2.126**
	(.99)	(1.08)	(1.10)	(1.04)	(.96)	(.89)	(.86)
With Controls	1.715*	1.872	1.438*	1.435	1.975**	1.801*	2.067***
	(.98)	(1.09)	(.81)	(.91)	(.89)	(.90)	(.73)
Panel E:							
Occasional night work	.024	.034*	.041***	.041***	.036**	.038**	.051***
	(.01)	(.02)	(.01)	(.01)	(.01)	(.02)	(.01)
With Controls	.028	.037*	.055***	.044***	.046***	.049**	.064***
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Panel F:							
Usual hours of work	1.498**	.898	.469	.475	.680	.911	1.066**
	(.68)	(.71)	(.70)	(.72)	(.60)	(.55)	(.50)
With Controls	1.742**	.973	.240	.250	.368	.471	.951**
	(.68)	(.73)	(.56)	(.57)	(.56)	(.55)	(.46)
Polynomial	One	One	One	One	One	One	One
Obs. (Panel A)	2378	3191	4018	4902	5726	6542	7282
Obs. (Panel B)	2293	3078	3876	4728	5520	6307	7032
Obs. (Panel C-E)	2150	2886	3617	4408	5146	5890	6574
Obs. (Panel F)	1792	2415	3036	3679	4300	4939	5512

*** p <0.01 ** p <0.05 * p <0.1.

The table shows regression discontinuity estimates using bandwidths that are within 3, 6, 9 and 12 months of the preferred bandwidth i.e. 15 months.

Controls include the second child's gender and month of birth fixed effects as well as the father's age at the birth of the second child and a dummy variable for whether he was born in France.

Standard errors are clustered at the month-year of birth level.

Table 5: Regression discontinuity estimates of labor supply in years 2 and 3 after the birth of the second child for fathers with high school degree and more

Bandwidth	-6 months	-3 months	15 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Labor force participation	–.011	.017	.016	.016	.018	.008	.004
	(.02)	(.02)	(.02)	(.02)	(.01)	(.01)	(.01)
With Controls	–.013	.019	.022	.017	.022	.016	.006
	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)
Panel B:							
Employed	–.014	.018	.004	.008	.004	.006	.015
	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)
With Controls	–.010	.022	.003	–.000	.002	.000	.022
	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)
Panel C:							
Full-time work	.035	.027	.024	.003	–.001	–.008	.002
	(.03)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
With Controls	.036	.029	.020	.004	–.005	–.010	.003
	(.03)	(.02)	(.02)	(.02)	(.02)	(.02)	(.01)
Panel D:							
Actual hours of work	7.609***	6.346***	5.514**	4.795**	5.314***	4.799***	5.027***
	(2.11)	(2.08)	(2.00)	(1.84)	(1.74)	(1.59)	(1.46)
With Controls	7.689***	6.350***	5.008***	4.857**	4.575**	4.329**	4.692***
	(2.19)	(2.11)	(1.64)	(1.90)	(1.72)	(1.67)	(1.39)
Panel E:							
Usual hours of work	6.267***	5.190***	4.472***	4.038***	3.935***	4.025***	3.579***
	(1.40)	(1.30)	(1.26)	(1.31)	(1.17)	(.98)	(.91)
With Controls	6.642***	5.378***	3.554***	3.464***	3.088***	3.094***	3.095***
	(1.46)	(1.38)	(1.23)	(1.15)	(1.10)	(1.02)	(.88)
Panel F:							
Working >= 40 hours/week	.350***	.272***	.228***	.184***	.198***	.200***	.161***
	(.07)	(.07)	(.07)	(.07)	(.06)	(.05)	(.05)
With Controls	.361***	.273***	.195***	.167***	.160***	.149***	.126***
	(.08)	(.07)	(.05)	(.06)	(.05)	(.05)	(.05)
Polynomial	One	One	One	One	One	One	One
Obs. (Panel A)	764	1025	1276	1577	1821	2097	2405
Obs. (Panel B)	743	1000	1243	1538	1774	2043	2346
Obs. (Panel C-D)	719	967	1203	1486	1710	1974	2265
Obs. (Panel E-F)	559	762	954	1164	1347	1561	1797

*** p <0.01 ** p <0.05 * p <0.1.

The table shows regression discontinuity estimates using bandwidths that are within 3, 6, 9 and 12 months of the preferred bandwidth i.e. 15 months.

Controls include the second child's gender and month of birth fixed effects as well as the father's age at the birth of the second child and a dummy variable for whether he was born in France.

Standard errors are clustered at the month-year of birth level.

Table 6: Regression discontinuity estimates of labor supply in years 2 and 3 after the birth of the second child for fathers with less than high school degree

Bandwidth	-6 months	-3 months	18 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Labor force participation	–.000	–.033*	–.013	–.004	–.008	.001	–.002
	(.01)	(.02)	(.02)	(.02)	(.01)	(.01)	(.01)
With Controls	.001	–.033***	–.016	–.003	–.005	.004	.000
	(.01)	(.01)	(.02)	(.01)	(.01)	(.01)	(.01)
Panel B:							
Employed	.014	.022	.026	.032	.021	.023	.017
	(.01)	(.03)	(.02)	(.02)	(.02)	(.02)	(.02)
With Controls	.013	.014	.016	.024	.016	.022	.017
	(.01)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Panel C:							
Full-time work	–.012	–.010	–.016	–.017	–.020*	–.018*	–.015
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
With Controls	–.013	–.008	–.014	–.018	–.018	–.017	–.013
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Panel D:							
Employed							
(lower middle class professions)	.013	.030	.037	.062*	.027	.024	.018
	(.02)	(.04)	(.04)	(.03)	(.04)	(.03)	(.03)
With Controls	.008	–.017	.001	.025	.003	.008	.012
	(.02)	(.03)	(.03)	(.03)	(.03)	(.02)	(.02)
Panel E:							
Full-time work							
(lower middle class professions)	–.059***	–.059**	–.064**	–.076***	–.077***	–.069***	–.053***
	(.03)	(.03)	(.02)	(.02)	(.02)	(.02)	(.02)
With Controls	–.063***	–.053*	–.073**	–.085***	–.088***	–.071***	–.057***
	(.02)	(.03)	(.03)	(.03)	(.03)	(.02)	(.02)
Polynomial	Zero	One	One	One	One	One	One
Obs. (Panel A)	2175	2755	3341	3925	4468	4901	5431
Obs. (Panel B)	2078	2633	3206	3766	4287	4710	5222
Obs. (Panel C)	1927	2425	2935	3453	3934	4328	4806
Obs. (Panel D)	569	702	837	995	1131	1256	1389
Obs. (Panel E)	535	654	782	928	1050	1164	1290

*** p <0.01 ** p <0.05 * p <0.1.

The table shows regression discontinuity estimates using bandwidths that are within 3, 6, 9 and 12 months of the preferred bandwidth i.e. 18 months.

Controls include the second child's gender and month of birth fixed effects as well as the father's age at the birth of the second child.

Standard errors are clustered at the month-year of birth level.

Table 7: Regression discontinuity estimates for marital outcomes

Bandwidth	-6 months	-3 months	13 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Married (overall)	.013	–.005	–.007	–.002	.003	.010	.010
	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)
With Controls	.013	–.006	.014	.002	.010	.015	.013
	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)
Panel B:							
Married (Fathers unmarried before birth)	.174**	.172**	.141**	.128**	.087*	.174***	.152**
	(.06)	(.06)	(.06)	(.05)	(.05)	(.06)	(.06)
With Controls	.173**	.170***	.110***	.107***	.074*	.149**	.121**
	(.06)	(.06)	(.03)	(.03)	(.04)	(.06)	(.05)
Panel B:							
Married (Mothers unmarried before birth)	–.031	–.056	–.048	–.027	–.003	–.068*	–.068*
	(.05)	(.04)	(.04)	(.03)	(.03)	(.04)	(.04)
With Controls	–.030	–.057	–.019	–.008	.010	–.066	–.070*
	(.05)	(.04)	(.05)	(.03)	(.03)	(.04)	(.04)
Polynomial (Panel A)	One	One	One	One	One	One	One
Polynomial (Panels B+ C)	One	One	One	One	Two	Two	Two
Obs. (Panel A)	4877	6843	9035	11224	13290	15398	17631
Obs. (Panel B)	557	748	953	1194	1448	1675	1929
Obs. (Panel C)	886	1236	1626	2052	2455	2842	3207

*** p <0.01 ** p <0.05 * p <0.1.
Standard errors are clustered at the month-year of birth level.
Controls include the second child's sex and month of birth fixed effects as well as the individual's age at the birth of the second child.

Table 8: Regression estimates for children's short-run outcomes

Outcomes	Age in preschool	Weight	Phonological awareness	Vocabulary development	Phonological and Vocabulary
	(1)	(2)	(3)	(4)	(5)
Panel A: RD					
4 months	–.014	.020	–.040**	–.022**	–.042**
	(.02)	(.07)	(.00)	(.00)	(.01)
	[.188]	[.652]	[.038]	[.033]	[.048]
Obs.	6103	6103	4290	4290	4290
6 months	.005	–.045	–.038**	–.024***	–.042**
	(.02)	(.07)	(.01)	(.01)	(.01)
	[.266]	[.356]	[.018]	[.016]	[.018]
Obs.	8849	8849	6197	6197	6197
Panel B: RD-DID (control: First child)					
4 months	.010	.244	–.038**	–.027	–.037*
	(.02)	(.13)	(.02)	(.03)	(.02)
	[.202]	[.315]	[.023]	[0.213]	[.083]
Obs.	13673	13673	9486	9486	9486
6 months	–.001*	.266	–.031**	–.030*	–.039**
	(0.02)	(.14)	(.01)	(.01)	(.01)
	[.056]	[.432]	[.018]	[.058]	[.028]
Obs.	19962	19962	13815	13815	13815

*** p <0.01 ** p <0.05 * p <0.1.
Numbers in parentheses represent standard errors that are clustered at the month-year of birth level. Numbers in brackets are p-values derived from a clustered wild bootstrap-t procedure.

Table 9: Regression discontinuity estimates for fertility in 1999

Bandwidth	-6 months	-3 months	18 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Number of children	–.016	–.001	.006	–.005	–.000	–.001	.000
	(.03)	(.02)	(.02)	(.03)	(.03)	(.02)	(.02)
With Controls	–.013	.023*	.017	.012	.015	.015	.019
	(.03)	(.01)	(.01)	(.02)	(.02)	(.02)	(.02)
Panel B:							
Age difference (first child)	.259**	.092	.038	.196	.148	.186	.148
	(.11)	(.13)	(.12)	(.13)	(.13)	(.12)	(.12)
With Controls	.220**	–.033	–.051	.030	.010	–.000	–.032
	(.10)	(.12)	(.11)	(.13)	(.12)	(.13)	(.13)
Panel C:							
Age difference (third child)	–.013	–.027	–.053	–.021	–.063	–.057	–.135
	(.09)	(.08)	(.07)	(.10)	(.09)	(.09)	(.09)
With Controls	.003	–.071	–.051	–.026	–.092	–.093	–.194**
	(.08)	(.07)	(.06)	(.10)	(.09)	(.09)	(.09)
Polynomial	One	One	One	Two	Two	Two	Two
Obs. (Panels A-B)	8294	10516	12578	14645	16869	19101	21265
Obs. (Panel C)	2187	2749	3246	3747	4244	4746	5250

*** p <0.01 ** p <0.05 * p <0.1.
Standard errors are clustered at the month-year of birth level.
Controls include the second child's sex and month of birth fixed effects as well as the parent's age at the birth of the second child.

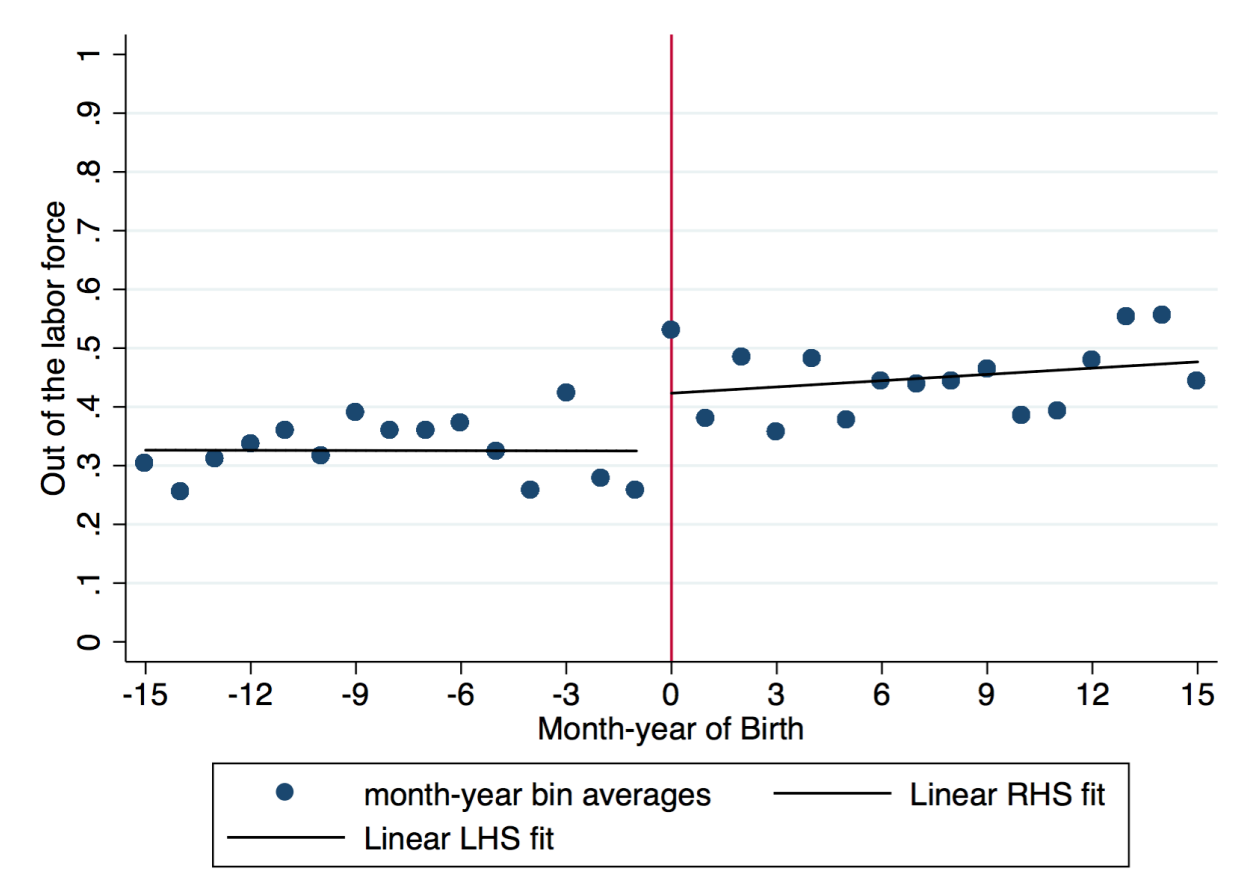
Table 10: Regression discontinuity estimates for infant mortality by age 3

Bandwidth	-6 months	-3 months	17 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Infant mortality	–.011*	–.011*	–.009	–.011	–.009	–.010	–.012*
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
With Controls	–.011*	–.007	.003	–.007	–.007	–.008	–.010
	(.01)	(.00)	(.01)	(.01)	(.00)	(.01)	(.01)
Polynomial	One	One	Two	Two	Two	Two	Two
Observations	7544	9811	11895	13939	16154	18422	20565

*** p <0.01 ** p <0.05 * p <0.1.
Standard errors are clustered at the month-year of birth level.
Controls include the second child's sex and month of birth fixed effects, as well as the parent's age at the birth of the second child.

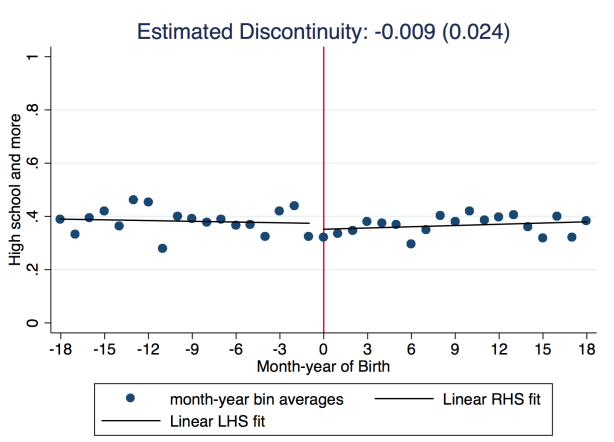
C Appendix Figures

Figure A1: Mothers' labor supply (Year 1)

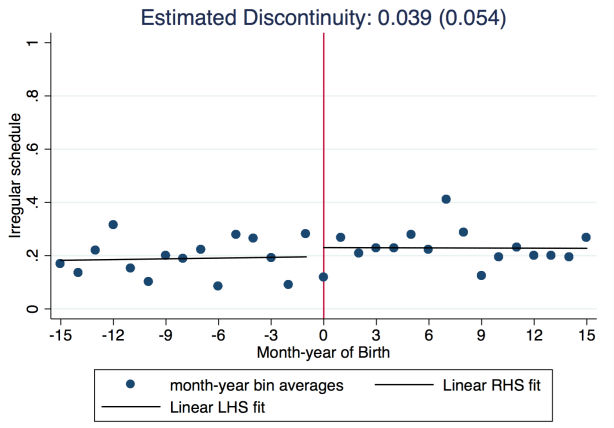


(a) Probability of being out of the labor force

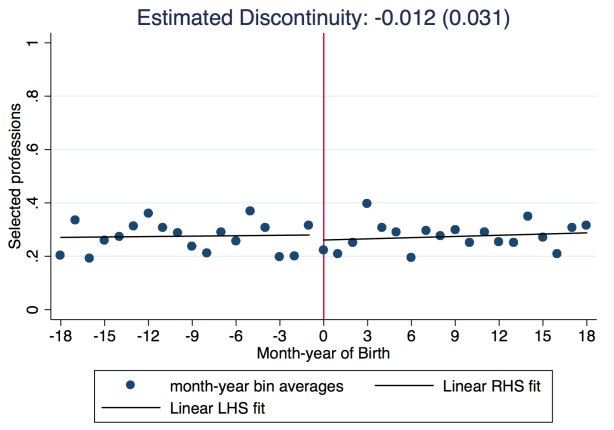
Figure A2: Additional results for fathers



(a) Probability of having a high school degree and more

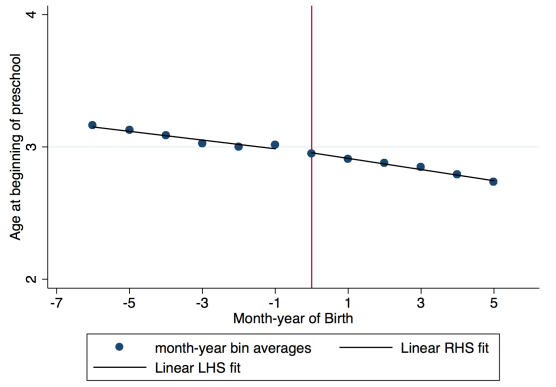


(b) Probability of having an irregular schedule (fathers with high school degree and more)

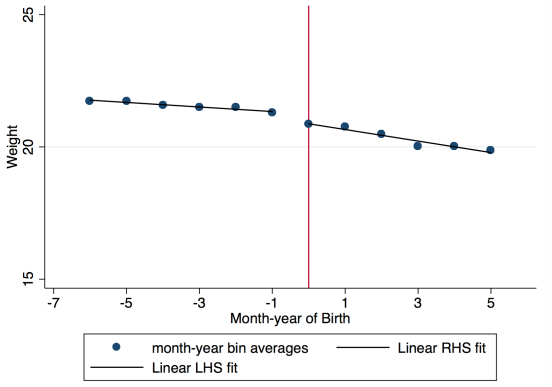


(c) Probability of being in lower middle class profession (fathers with less than high school degree)

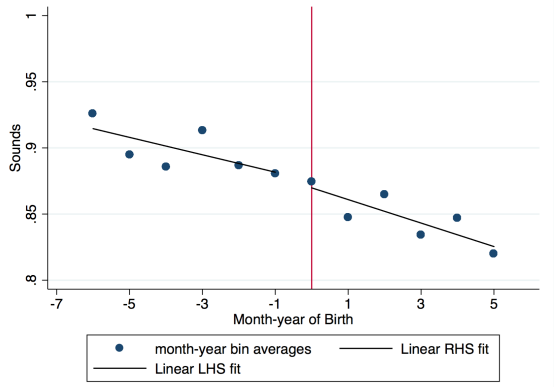
Figure A3: Children's short-run outcomes (First child)



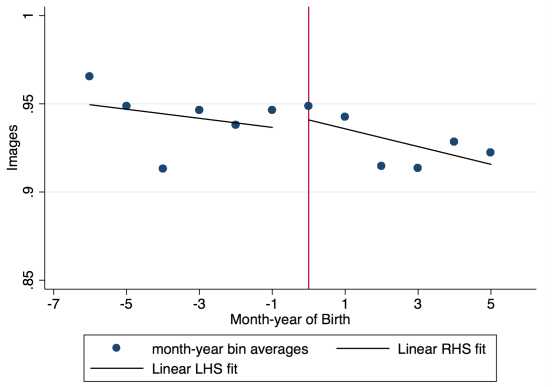
(a) Age at the beginning of preschool



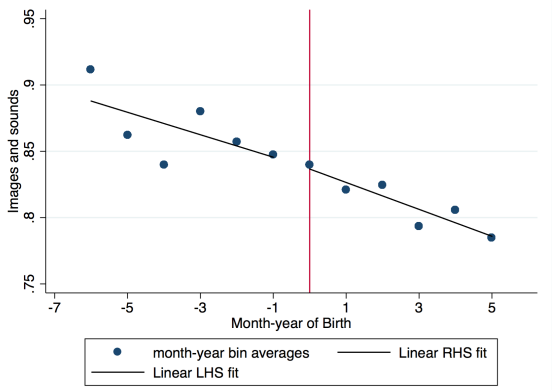
(b) Weight



(c) Probability of having a normal score on the phonological awareness test



(d) Probability of having a normal score on the vocabulary development test



(e) Probability of having a normal score on both tests

D Appendix Tables

Table A1: Regression discontinuity estimates for mothers' probability of being out of the labor force in the first year after the birth of the second child

Bandwidth	-6 months	-3 months	15 months	+3 months	+6 months	+9 months	+12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Out of the labor force	.180***	.152***	.093*	.108**	.121***	.129***	.117***
	(.05)	(.05)	(.05)	(.04)	(.04)	(.04)	(.03)
With Controls	.162***	.143***	.040	.064**	.081***	.084***	.094***
	(.05)	(.05)	(.03)	(.03)	(.03)	(.03)	(.03)
Polynomial	One	One	One	One	One	One	One
Observations	1197	1624	2028	2463	2900	3305	3672

*** p <0.01 ** p <0.05 * p <0.1.
The table shows regression discontinuity estimates using bandwidths that are within 3, 6, 9 and 12 months of the preferred bandwidth i.e. 15 months.
Controls include the second child's gender and month of birth fixed effects as well as the mother's age at the birth of the second child and a dummy variable for whether she was born in France.
Standard errors are clustered at the month-year of birth level.