Maternal Labor Supply, Childcare, and the Health of Preschool Children in Japan*

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Abstract

Due to a considerable shortage of after-school childcare services, mothers in Japan are likely to reduce their labor supply when their firstborn child enrolls in elementary school. By exploiting the timing of the firstborn child's school entry, this study explores how the health of younger preschool siblings responds to a decrease in maternal labor supply. A regression discontinuity design analysis is used to compare the health outcomes of preschool children whose eldest sibling enrolls in elementary school or remains in preschool. The results show that the maternal employment rate decreases by approximately 10% after the firstborn child's enrollment in elementary school. In addition, the reduction in maternal labor supply leads to an increase in parental care for the younger siblings. Despite these substantial decreases in maternal labor supply and increases in parental care, I find no improvement of children's subjective health status in both short-run and medium-run. The overall findings of this study indicate that the reduction in maternal labor supply is not associated with the health of preschool children.

Keywords : Japan, child health, maternal labor supply, hospitalization, regression discontinuity design

JEL classification : I0, J21, J13, C26

*This study was supported by a Grant-in-Aid from the Japan Society for the Promotion of Science (JSPS; Grant Number: 25780199). I am grateful for the comments and suggestions from Michihito Ando, Yukiko Asai, Yoshikazu Kenjoh, Sayaka Nakamura, Kazunari Shinpo, Isamu Yamamoto, and the participants of the Applied Econometric Conference in Osaka University, the annual meeting of the Japanese Health Economic Association, the Tokyo Labor Economics Workshop, and the Kansai Labor Economics Workshop. No ethical approval is required for the manuscript under the standard of the ethical review board in my research institute. The views expressed in this article do not necessarily reflect the official views of the JSPS and Hitotsubashi University. I declare that I have no conflict of interest. All errors are my own.

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1 Introduction

Over the past three decades, the number of mothers participating in the labor force has gradually increased in many developed countries. According to the Institute for Fiscal Studies (Roantree & Kartik, 2018), the share of working mothers in the UK has risen from 50% in 1975 to 72% in 2015. In particular, the most notable change is observed in the working pattern of women in several years before and after child birth. While maternal employment rate in Japan is one of the lowest in OECD countries, it begins to catch up with other developed countries; the maternal employment rate in Japan was only 34% in 1990, but had increased to 41% by 2010 (Asai *et al.*, 2015). However, little remains known about the costs and benefits associated with the increasing labor force participation of mothers. In particular, the causal effects of shifts in the maternal labor supply on child outcomes is a major topic of public debate that has not been sufficiently explored.

Of course, there can never be a general answer on how maternal labor supply affects child outcomes because they depend on various factors such as the child's age and the quality of alternative non-parental childcare (Becker, 1981). For example, maternal employment may harm children if mothers work very hard during pregnancy, but it may not have an effect if a child is already 15 years old when the mother returns to work. Similarly, the quality of non-parental childcare available for working mothers seems to be quite important. If children are left in a low-quality daycare center for very long hours per day, it may harm them. In addition, how maternal care improves child outcomes depends on the reason why the mother stays with her children. For example, if unexpected dismissal kicked out the mothers from labor market, the quality of maternal child care decreased due to higher stress level, and then their children can be also affected negatively (Hill *et al.*, 2011).

Due to the complex causal paths from maternal employment to child health, previous quasi-experimental studies that investigated the impact of maternal employment on child outcomes present fairly mixed results, even when focused on crucial periods in the child's development. For example, the estimated impact of expansions to maternity leave, which directly affect the working conditions of pregnant women and mothers with newborns, varies across outcomes (health or development) and scope (short-term effects or long-term effects) (Baker & Milligan, 2010; Liu & Skans, 2010; Rossin, 2011; Carneiro *et al.*, 2015). Unfortunately, few quasi-experimental studies focus on preschool children aged 2 years and older, with the notable exception of Dustmann & Schönberg (2012). Resolving the paucity of information on this age group may be crucial as the effects of maternal employment can vary according to children's age (von Hinke Kessler Scholder, 2008; Dustmann & Schönberg, 2012).

In order to present new evidence on the effects of maternal labor supply changes on the health outcomes among preschool children, this study exploits plausible variations in maternal labor supply that accrue from discontinuous reductions in childcare services for the eldest sibling. My research design is based on the fact that mothers in Japan experience discontinuous reductions in childcare services availability when their children enroll in elementary school at the age of 6. A typical daycare center in Japan keeps preschool children from 8:00 or 9:00 to 18:00 at low user fee. However, parents cannot use these services once their children enroll in elementary school. Although the first and second grades have very short school hours (e.g., 5 hours) with long seasonal vacations, there is limited availability of after-school childcare services for school-age children in Japan. This dearth of childcare services is notorious and has been referred to as the "wall for mothers with first graders" (in Japanese: *Shōgakkō Ichinensei no Kabe*) (Takaku, 2017). When faced with this "wall", many mothers choose to exit from the labor market to provide after-school care for their children, even if they had worked when their children were in preschool.

By exploiting the *Shōgakkō Ichinensei no Kabe*, this study seeks to establish novel regression discontinuity (RD)-based evidence for the impact of maternal labor supply on preschool-age child health. Specifically, this study applies the RD analysis to 6 waves of the Comprehensive Survey of Living Conditions (CSLC) conducted from 1995 to 2010, which provides a nationally representative sample of the Japanese population. With these data, I investigate how the firstborn child's school entry affects (1) maternal labor supply, (2) arrangement of childcare services for the younger siblings, and (3) the health status of the younger siblings. After implementing an intention-to-treat reduced-form regression discontinuity design analysis, I find large reduction of maternal employment at the timing of firstborn child's school entry. In addition, at the same timing, the younger siblings are exposed to parental care more than before. Despite the reduction of maternal employment and increase of parental care, I find little improvement of children's health status in both short-run and medium-run. While some indicators turn out to be statistically significant since I test many hypothese, none of these results survives after controlling for potential over-rejection.

One contribution of this paper is to explore the medium-run effects of mother's exit from labor market, as well as short-run effects under the consistent statistical methodology. In my central RD identification, I observe the younger sibling's health outcomes during short periods before and after the firstborn child's school entry. This strategy compares preschool children by whether their eldest sibling can just enter elementary school or not. However, in the next year, the youngest first grader moves to the second grade, and children slightly younger than him/her start school. Therefore, by focusing on the timing of one year after school entry, I can compare children just started school with children who have been in school for a year, and would therefore detect any change in the treatment effect over time. This point is noteworthy because previous studies which deal with the similar topic with my study (Gennetian *et al.*, 2010; Morrill, 2011) evaluate short-run effects of maternal employment and have no indication on the long-run effects. Contrary to them, my study indicates that the reduction of maternal labor supply does not have beneficial effects on child health in the long-run by showing null effects in both short- and medium-run.

2 Background

2.1 Prior Literature

This subsection reviews the previous studies that examine the causal effects of maternal employment on child outcomes. Earlier studies that explore this causality have employed the use of maternal fixed effects to address omitted variable bias (Waldfogel *et al.*, 2002; Anderson *et al.*, 2003; Aizer, 2004; Ruhm, 2004; Aughinbaugh & Gittleman, 2004; Gordon *et al.*, 2007). Using fixed-effects models, unobservable factors (such as the preferences of the mother) that affect both the maternal labor supply and child health can be successfully eliminated if they are time-invariant. As noted by Gordon *et al.* (2007), this assumption would be sufficiently plausible if additional covariates do not change the coefficient of maternal employment to a large extent.

Beyond the fixed-effects models, recent studies have exploited quasi-experimental changes in maternal employment in order to uncover causal effects. Here, previous studies are divided into 2 groups according to the timing of intervention. The first group comprises quasi-experimental studies that exploit plausible exogenous shocks for pregnant women and mothers with newborns. In many countries, maternity leave reforms provide a suitable quasi-experiment to evaluate the effects of maternal employment during very early childhood. These reforms may prevent overwork during pregnancy and increase the quality of parenting behavior for newborn children during the crucial first few months.

Although six studies were identified in this group, the results are fairly mixed. For example, Baker & Milligan (2008) exploit the expansion of mandatory maternal leave in Canada and compare the changes in outcome variables before and after the expansion. Their study reveals that the expansion of mandatory maternity leave reduced the employment rate of mothers after birth and sharply increased the duration of breastfeeding; the findings also indicate that these changes had a weak impact on the subjective health of mothers and children, as well as on the children's subsequent cognitive abilities. Using the same research design, Baker & Milligan (2010) report that increased maternal care had weak effects on children's developmental outcomes, and the reform resulted in the crowding out of home-based care by unlicensed non-relatives. In contrast, Rossin (2011) reports positive effects associated with the expansion of maternity leave in the US by evaluating the impact of unpaid maternity leave provisions mandated in the Family and Medical Leave Act (FMLA), which was enacted in 1993. The results of that study show that the reform was followed by slight increases in birth weights and decreases in the likelihood of premature births. It should be noted that both Baker & Milligan (2008) and Rossin (2011) evaluate relatively short-term consequences of maternity leave reforms. Long-term consequences are explored in Carneiro *et al.* (2015), Dustmann & Schönberg (2012), and Liu & Skans (2010). By exploiting maternity leave expansion in Norway, Carneiro *et al.* (2015) evaluate the long-term effects of maternity leave reform and note that the resulting increases in time spent between mothers and their children led to a decline in high school dropout rates and an increase in the children's eventual wages at age 30. However, an analysis of maternity leave reform in Germany by Dustmann & Schönberg (2012) finds that the expansion of maternity leave from 2 months to 6 months had no significant impact on the children's long-term educational outcomes, whereas an expansion from 18 months to 36 months deteriorated the children's educational attainment. A study of maternity leave reform in Sweden by Liu & Skans (2010) reveals that an expansion in leave coverage from 12 months to 15 months improved academic success only in children with highly educated mothers. In addition to these quasi-experimental studies, one study that utilizes maternal fixed effects (Bono *et al.*, 2012)[1] also finds that maternal employment during pregnancy is significantly associated with low birth weight.

The second group comprises four studies that evaluate the impact of maternal employment on children after the newborn period (Gennetian *et al.*, 2010; Morrill, 2011; Dustmann & Schönberg, 2012; Bettinger *et al.*, 2014), although three of these studies focus on school-age children. First, Gennetian *et al.* (2010) use experimental data from a welfare-to-work program implemented in the early 1990s in the US and show that a percentage point increase in employment induced by the program decreased the probability of an elementary school-age child being in very good or excellent health by 0.6 percentage points. Stronger negative effects are observed in the study by Morrill (2011), which focuses on the fact that labor participation rates discontinuously increase when the youngest sibling becomes eligible for kindergarten. The IV estimates in Morrill (2011) indicate that maternal employment greatly increases overnight hospitalizations, injuries and poisonings, and asthma episodes. Bettinger *et al.* (2014) exploit the introduction of a program that was intended to incentivize parents to stay at home with children under 3 years of age in Norway and investigate the effects of this program on the older siblings. The study reports a significantly positive treatment effect on the older siblings' grade point average in the tenth grade after their mothers reduced their labor force participation.[2]

[1]Beyond the use of a simple maternal fixed-effects estimator, Bono *et al.* (2012) presents an IV fixed-effects estimator that uses prenatal inputs during earlier pregnancies as instruments for differences in endogenous inputs between pregnancies.

[2]Recent studies on the effects of maternal employment on child health consistently link employment with detrimental health effects in the short term (Rossin, 2011; Gennetian *et al.*, 2010; Morrill, 2011; Bono *et al.*, 2012), but report comparatively mixed results in educational outcomes. This suggests that the health status of children may be more responsive to parental time input than educational outcomes.

2.2 Japanese Childcare System Before and After School Entry

In Japan, choices of childcare change with children's age. For children less than 3 years old, parents, especially mothers, generally take care of their children as full time childcare providers. If mothers work outside, their children can be left in daycare center. A typical daycare center in Japan keeps preschool children from 8:00 or 9:00 to 18:00 at low user fees. Once children of full time house wives become 3 years old, they can go to kindergarten. Japanese kindergartens typically have short business hours (e.g., 4 hours per day) (Oishi, 2002), which is mostly the same as the school hours for the first graders. In addition, kindergartens are closed during long seasonal vacations, like elementary school are, while there are no seasonal vacations at day care centers.[3] Since home care or home education for children aged 3 to 5 years old is not popular in Japan, preschool children are either in day care centers or kindergartens in the year prior to school entry. According to the Cabinet Office (2015), day care centers and kindergarten are used by 33.3% and 63.8% of children aged 5 years, respectively.

Once child enters elementary school at the age of 6 years old, employed mothers have to arrange for a variety of care options to ensure a safe after-school environment for their children. However, availability of afterschool child care is quite limited in Japan. According to an international comparative survey conducted by the OECD (2011), 80–90% of children in the Nordic countries such as Denmark and Sweden use out-of-school care services, whereas this rate is only 11.2% in Japan. According to the Council (2013), 33 % of the children who used daycare center cannot use after-school childcare when they start school. My previous study evaluated the quantitative impacts of this sudden dearth of childcare availability, which is widely known as "wall for mothers with first graders". Using longitudinal data of women with children, Takaku (2017) reveals that mothers from nuclear households reduce their labor supply and allocate more hours to childcare and housework when their child starts school. Quantitative impacts are also sizable. Labor market participation rate drops by 10.9 percentage points in the year of children's school entry. By exploiting this large reduction in maternal labor supply, the present paper uncovers the causal effects of maternal labor supply on child health.

[3]The childcare system in Japan is designed with the assumption that children have a stay-at-home parent in the household until 3 years of age and that they will attend kindergarten from April of the year in which they turn 3 years old, as long as their mother does not have a full-time job. If the mother has a full-time job or needs to work long hours, her children are eligible to be enrolled at a daycare center.

3 Data

3.1 Comprehensive Survey of Living Conditions

This study utilizes one of the most comprehensive databases of children's health status available in Japan. The CSLC is a nationally representative survey of a stratified random sample of the Japanese population. This survey has been conducted every three years since 1986, and data from 11 rounds of surveys are available with permission from the Ministry of Health, Labour and Welfare. In this study, I pooled data of children obtained from six survey waves spanning from 1995 to 2010 and used them as repeated cross sectional data. In the 2010 CSLC, the survey was sent to 289,363 households, and 229,785 households replied (response rate = 79.4 %). Since the CSLC provides exact birth day for respondents and their family members and it is held in June, I calculate age-in-months for all children and use it as running variable.

From the complete data set, I excluded children who received public welfare and those in single-parent households from analysis. Furthermore, children without siblings are excluded because my identification strategy is contingent on the change in maternal labor supply that accompanies the firstborn child's enrollment in elementary school. After all, the number of children less than 10 years old, who are potentially included in my analysis, is 113,356.

3.2 Dependent Variables

3.2.1 Maternal Labor Supply

The maternal labor supply is measured through an extensive margin (i.e. maternal employment).[4] In order to supplement the results on maternal employment, I construct three binary variables for the type of employment contract. The first variable takes a value of one when a mother works as a general employee (*ippan koyousya*). A general employee is a worker who is employed without term limits. The second variable is a binary variable for workers with short-term contracts of less than one year. In general, full-time workers are classified as general employees and part-time employees are more likely to be employed with short-term limits. At last, I also create a binary variable for self-employed and other workers.

3.2.2 Childcare

Childcare provision for the younger siblings is measured by two binary variables. In the first variable, children are defined as receiving parental care if they are enrolled in a kindergarten or

[4]Note that some previous studies investigate the effects of the mother's working hours (Anderson *et al.*, 2003; Gordon *et al.*, 2007; Datar *et al.*, 2014). As previously described, the firstborn child's school entry may affect the intensive margin of labor supply variables. Although this effect on working hours cannot be investigated in this study due to data limitations, it is a reasonable assumption that the firstborn child's school entry affects these variables in a similar manner.

if their parents are able to provide care in the daytime. Note that kindergarten enrollment is regarded as being equivalent as parental care since office hours of kindergarten is very short.[5] The second variable is the utilization of daycare center utilization. .[6]

3.2.3 Child Health

As a measure of child health, I use subjective symptoms, which are measured by asking subjects the following question: “In the last few days, have you experienced any symptoms of illness or injury?”. In the case of preschool children, their parents answered this question on their behalf. In addition to examining the effects of maternal employment on having any symptom of illness or injury, I also investigate the effects on specific symptoms such as fever and rash. Because maternal employment may indirectly result in an increase in respiratory problems and ear infections via utilization of daycare centers (Gordon *et al.*, 2007), it is possible that maternal employment has a significant effect on these symptoms.

4 Empirical Framework

4.1 Identification Strategy

Here, I formally explain my identification strategy. Core idea of the identification comes from the school system in Japan. In Japan, children who born in April (“April child”) start school at the age of 84 months in the end of April, and those who born in March (“March child”) enter school at the age of 73 months. Children who born from 73 months to 84 months in April consist of the same school grade. Thus, “April child” generally get older than “March child” within the same school grade. Note that the school admission dates are strictly enforced with almost complete compliance. Kawaguchi (2011) reports that the percentage of children who do not enroll in elementary school every April is only 0.03%. Therefore, “March child” cannot postpone school entry to the next year even if their parents worry about the fact that their children are the youngest within the first grade. Under this system, “April child” aged 72 months did not enter elementary school despite they were younger by only one month than “March child” aged 73 months. Due to the shortage of after school childcare, the mothers of the “March child” aged 73 months are likely to reduce their labor supply, while those of “April child” aged 72 months can continue to work.

[5]Since kindergarten is not a viable option for children aged less than 3 years old, mothers generally provide fulltime childcare if they do not work outside. Therefore, the combination of fulltime parental care until 3 years old and kindergarten enrollment is a typical trajectory of childcare environment for mothers who do not work outside. Therefore, this variable can represent if mothers can be categorized into this group.

[6]This variable takes a value of one if the child is always left in day care center during daytime in week days. Because mothers who are full-time homemakers are, in principle, not allowed to use licensed daycare centers, the use of day care center decreases as parental care increases.

Table 1: Birth Month and Age-in-Months at School Entry

	Children with the Same School Grade						Younger Grade Children	
Birth Month	April	May	June	· · ·	February	March	April	May
First Grade							**Preschool**	
April	84	83	82	· · ·	74	73	72	71
May	85	84	83	· · ·	75	74	73	72
June	**86**	**85**	**84**	· · ·	**76**	**75**	**74**	**73**
July	87	86	85	· · ·	77	76	75	74
August	88	87	86	· · ·	78	77	76	75
September	89	88	87	· · ·	79	78	77	76
October	90	89	88	· · ·	80	79	78	77
November	91	90	89	· · ·	81	80	79	78
December	92	91	90	· · ·	82	81	80	79
January	93	92	91	· · ·	83	82	81	80
February	94	93	92	· · ·	84	83	82	81
March	95	94	93	· · ·	85	84	83	82
Second Grade							**First Grade**	
April	96	95	94	· · ·	86	85	84	83
May	97	96	95	· · ·	87	86	85	84
June	**98**	**97**	**96**	· · ·	**88**	**87**	**86**	**85**
July	99	98	97	· · ·	89	88	87	86
August	100	99	98	· · ·	90	89	88	87
September	101	100	99	· · ·	91	90	89	88
October	102	101	100	· · ·	92	91	90	89
November	103	102	101	· · ·	93	92	91	90
December	104	103	102	· · ·	94	93	92	91
January	105	104	103	· · ·	95	94	93	92
February	106	105	104	· · ·	96	95	94	93
March	107	106	105	· · ·	97	96	95	94

Note: Columns represent the birth month and raws represent age in months in a given month. June, shaded in gary, is the survey month.

Of course, health status of children may change before and after school entry due to environmental changes. Thus I focus on the sample of the children whose eldest siblings are around school entrance age. These children are supposed not to be affected by their eldest sibling's school entry directly, but they may be affected by the reduction maternal labor supply due to the "wall". Since my data are held in June, I adopt 75 months as a cutoff age in months. Note that this strategy can measure short-run effects since "March child" aged 73 months in April were exposed to lower maternal labor supply during only 3 months (i.e. from April to June). To state the relationship between the timing of school entry and birth month more simply, I show Table 1. In this Table, columns represent birth month and rows represent the flow of time. In April in a given year, children aged 73 months to 84 months starts school. Since the survey is held in June, they become 75-86 months at the timing of the survey.

In addition to the short-run effects, it is possible to derive some suggestive evidences on medium-run effects even with the same strategy. Specifically, "March child" aged 73 months, who entered elementary school in April in a given year, moved to the second grade in the next April and "April child" aged 72 months, who stayed in preschool, entered elementary school in the next April. This setting indicates that "March child" in the second grade were exposed to greater maternal care during last one year but "April child" were exposed during one month, even if their age in months differed only by one month. Therefore, by comparing these two groups, the effects of longer exposure to reduced maternal labor supply can be reasonably estimated. Again, since the survey is held in June, I adopt 87 months as a cutoff age in months for this analysis.

4.2 Assumptions and Major Threats for the Identification

One important assumption of my strategy is the continuity of average potential outcomes around firstborn child's school entry. One thereat for this assumption comes from the manipulation of running variable (McCrary & Royer, 2011). In my context, this threat is not so consequential because the school admission dates are strictly enforced with almost complete compliance. Therefore, the manipulation of running variable is likely to occur when parents controls for the firstborn child's birth month (and not the timing of school entry). For example, parents may elect to deliver after April 2 due to the potential advantages in cognitive and non-cognitive performance over those born in March (Kawaguchi, 2011; Shigeoka, 2014b).[7] Since the seminal work by Huntington (1938), many epidemiological studies have shown that the season of birth predict a wide range of health outcomes in later life, such as the incidence of mental illness (Hare & Price, 1969) and mortality (Moore *et al.*, 1997).

[7] Kawaguchi (2011) shows that test scores are generally higher among older children than their younger counterparts in the fourth to eighth grades. Shigeoka (2014b) reports that there is considerable manipulation of birth timing around April 2 in an analysis of the universe of births in Japan between 1974 and 2010.

Regardless of the underlying reason, accounting for seasonality and heaping in the running variable is a challenge for RD analysis. To address this threat, I checked the robustness of the results by controlling for the fixed effects of the firstborn child's birth month using a method previously described in Shigeoka (2014a). As a result, my additional analysis deviates from the conventional wisdom that recommends choosing a smaller bandwidth and a linear polynomial function (Hahn *et al.*, 2001). However, it should be noted that a large bandwidth combined with birth month dummy variables may be the second-best strategy when seasonality and heaping bias is relevant, as a smaller bandwidth would increase the severity of non-random heaping bias (Barreca *et al.*, 2011).

4.3 Econometric Specification

4.3.1 Short-run Effects

The basic analytical framework in this article is based on "fuzzy" RD rather than "sharp" RD, the reduced form sharp RD can be used to estimate discontinuities in outcomes at the threshold, which may be considered analogous to an intention-to-treat (ITT) effect (Ludwig & Miller, 2007). [8] Specifically, the following equation is estimated by using the sample of children who are not firstborn,

$$Y_{it} \quad = \quad \alpha_0 + \alpha_1 75m_{it} + f(Z_{it}) + year + pref + \zeta_{it}, \tag{1}$$

where Y_{it} is child i's outcome variable such as maternal employment and health status, $75m_{it}$ is a binary variable that takes a value of one if the firstborn child is 75 months and more and a value of zero if otherwise, X_{it} is a vector of covariates, and ζ_{it} is an error term. $f(Z_{it})$ is a polynomial function of the firstborn child's age in months (Z_{it}), ζ_{it} is an error term. The variables $year$ and $pref$ represent year effects and prefecture (regional) fixed effects, respectively. In the baseline specification, I estimate this equation using a bandwidth of ± 18 months and a quadratic polynomial.

In addition, following conventional wisdom (Lee & Lemieux, 2010), I check robustness of the results by additionally controlling for a vector of covariates and the firstborn child's birth month fixed effects that absorb potential heaping-induced bias (Barreca *et al.*, 2011). In this specification, a variety of covariates such as age, sex, age of household head and spouse, and household size are controlled for. In addition, I control for the firstborn child's health condition as measured by

[8] This article focuses on reduced form ITT-style estimates because of the potential violation of exclusion restriction. In particular, mothers may reduce working hours for their part-time jobs or change their working times to night shifts or early morning shifts in order to provide after-school childcare when their child enrolls in elementary school. However, it is not feasible to understand these changes in a single variable of maternal labor supply.

subjective symptoms because their health condition may also change before and after entering elementary school. [9] Two additional robustness checks are then implemented. First, the results from cubic polynomials are also presented with the baseline bandwidth fixed at ±18 months and without covariates. Next, bandwidth is extended to ±36 months. In this specification, a vector of covariates and the firstborn child's birth month fixed effects are controlled for in order to address potential manipulation of running variable.

For the calculation of standard errors, I calculate the errors that are clustered at the firstborn child's age in months because the conventional standard error does not take into account the discreteness of the assignment variable, and therefore tends to overestimate the precision of the estimated effects (Lee & Lemieux, 2010). Finally, throughout this article, I do not use the optimal bandwidth calculation as proposed by Imbens & Kalyanaraman (2012).[10]

4.3.2 Medium-run Effects

In the analysis on medium-run effects, I estimate following equation,

$$H_{it} = \beta_0 + \beta_1 87m_{it} + f(Z_{it}) + year + pref + \kappa_{it}, \quad (2)$$

where H_{it} is child i's health outcomes, and $87m_{it}$ is a binary variable that takes a value of one if the firstborn child is 87 months and more and a value of zero if otherwise. As is previously explained, the youngest second grade children who born in March become 86 months and they are enrolled in school for 15 months at the timing of the survey. On the other hand, children aged 85 months has been enrolled in elementary school only during 3 months. Other covariates are just the same with equation (1). Choice of bandwidth and polynomial orders are also set similarly.

5 Results

5.1 Descriptive Statistics

The descriptive statistics are summarized in Table 2. The sample comprises preschool children whose eldest sibling has an age within 36 months before and after school entry. The number of observations is 65,556 at the maximum. The proportion of children who report symptoms is 26%

[9] Although this potentially violates the exclusion restriction (i.e., changes in the health condition among firstborn children directly affects the younger siblings' health status), I confirm that the inclusion of the firstborn child's subjective health measures does not alter the results. Therefore, I assume that the bias from this issue is negligible.

[10] This is because of the discreteness and limited range of the running variable. As the running variable is age in months and has a maximum of only 72 discrete values (36 months before and after enrollment in elementary school), it was preferable to provide RD estimates with varying bandwidths than to present a single RD estimate with an "optimal" bandwidth that would require additional assumptions.

and the proportion of working mothers is 38%. As shown in Panel C, the probability of receiving any parental care is approximately 68% and the daycare center utilization rate is 25%.

5.2 Identification Checks

Following conventional wisdom, I implement two standard validity checks (Lee & Lemieux, 2010). First, I examine whether the density of the assignment variable (age in months of the firstborn child) is smooth at the threshold. As age in months is a non-continuous discrete variable, I implement a parametric version of McCrary (2008)'s density test. Second, I implement standard continuity test which examines the discontinuities in all covariates such as age and sex.

The binned scatterplot of the number of children who are included in the analysis is presented in Figure 1-(a). The x-axis of the figure represents the age in months of the firstborn children relative to the month of enrollment in elementary school, which is standardized at "0". The y-axis represents the count of the younger siblings in each bin. When the value of the x-axis is -36 months this indicates that the mean age of the firstborn child is 36 months before their enrollment in elementary school. Initially, there are approximately only 500 children in this bin, but it gradually increases to approximately 1,100 children when the x-axis value is at 36 months. The observed upward slope in the count can be explained by the fact that the younger siblings had yet to be born when the firstborn child was an infant. The other scatterplots in Figure 1 are similar plots of the main covariates. As the running variable is the firstborn child's age, the mean age of the younger siblings should also increase as the running variable increases (Figure 1-(b)). However, there is no systematic jump around the threshold observed in the figure. Also, the share of girls and the age of the household head are found to be completely smooth. No systematic jumps are observed for the other main covariates.

The results of the parametric tests are presented in Table 3. Column (1) presents the results of baseline specification without firstborn child's birth month FEs. Columns (2) and (3) check the robustness of the results by controlling for control for firstborn child's birth month FEs (Column 2) and changing polynomial order to cubic (Column 3). In Columns (1) to (3), bandwidth is fixed at 18 months. Subsequently, Columns (4) provides a preferred robustness check for heaping induced bias which adopts broader bandwidth (36 months) and controls for firstborn child's birth month FEs.

In Panel A, which reports the results of the parametric density test, the coefficients are not statistically significant in all specifications. While null effects in the parametric density test is due to low statistical power, it suggests that there is no systematic bunching at the cut-off month. Panel B shows the discontinuities in covariates that are included in the main analysis. In short, covariates are found to be smoothly distributed around the threshold. While the discontinuity term is statistically significant for some covariates, no results are robust for alternative specification.

5.3 Effects on Maternal Employment

The effect of the firstborn child's school entry on maternal employment is graphically presented in Figure 2, and the corresponding RD estimates are summarized in Table 4. In Figure 2, the share of younger siblings with working mothers is plotted against the firstborn child's standardized age in months. Consistent with Takaku (2017), this figure shows clear evidence of the discontinuous reduction in maternal employment when a firstborn child enrolls in elementary school. First, the maternal employment rate increases from approximately 30% to 40% when the age in quarters increases from -12 to 0. Just after the cut-off month, however, maternal employment rate abruptly drops by approximately 5 percentage points, and subsequently begins to increase again. These findings are likely indicative of the *Shōgakkō Ichinensei no Kabe* effect, and directly show that mothers are exiting from the labor market when their firstborn child enters elementary school.

Next, Table 4 reports the corresponding RD estimates. First, four RD estimates are found to be highly significant, with the coefficients ranging from -0.041 in Column (2) to -0.087 in Column (3). In the baseline specification in Column (1), maternal employment rate drops by 6.5 percentage points when the firstborn child enrolls in elementary school. These results indicate a substantial decrease in the employment rate. While the average employment rate during the six months before the cut-off month is 39.5%, the probability of being employed decreases by approximately 16.4%. The impact on employment reduction is as large as that reported in Morrill (2011).[11]

In addition to the main results, Table 4 also presents the subsample results by the number of siblings and children's age. First, I find a relatively large reduction in maternal employment when the number of children in the household exceeds three. This is probably because the mother's burden of childcare is generally larger for this subsample than the mothers with two children, which makes it more likely that the firstborn child's school entry triggers an exit from labor market. On the contrary, results does not change by children's age (i.e. more or less than 3 years old) to a large extent.

Although the RD results suggest a sizable reduction in maternal labor market participation, it should be noted that working hours and employment status (regular or non-regular employee) may also be affected by the firstborn child's school entry. Empirically, my previous study found that working hours decrease by about 30 min at this time (Takaku, 2017). In other words, the firstborn child's school entry not only affects children's health via the extensive margin of labor supply, but also via the intensive margin. [12] Although the effects on the intensive margin cannot

[11] Morrill (2011) reports a discontinuous increase in maternal employment rate when the youngest sibling becomes eligible for kindergarten. The reported first-stage coefficients in Morrill (2011) range from 4-8 percentage points, which are similar to the values in this study.

[12] In particular, if the need for after-school childcare is concentrated in the first and second grades of elementary school, mothers are likely to adjust their labor supply by reducing their working hours rather than by completely exiting the labor market.

be quantified due to data limitations, the strong impact on the extensive margin indicates that the intensive margin is also substantially affected.

Next, Table 5 and Figure 3 show which types of employees exit from labor market. In general, I find no reduction in general employees and self-employed. However, Figure 3-(b) shows a discontinuous reduction in employees with short-term contracts at the threshold. In the baseline estimation in Column (1) in Table 5, the proportion of employee with short-term contracts decreases by 2.0 percentage points. Given that the mean of the proportion during the six months before the firstborn child's school entry, presented in the rightmost column, is 9%, the quantitative impacts on these workers seem to be sizable.

5.4 Effects on Childcare

Here, I investigate the parental response on childcare arrangements. As explained above, two dependent variables are constructed. The results are summarized in Figure 4 and Table 6. First, Figure 4-(a) shows a discontinuous increase in the probability of receiving parental care around the threshold. In contrast, the share of children who are enrolled in daycare centers and do not receive parental care in the daytime may decrease slightly after the firstborn child's school entry.

The parametric RD estimates in (Table 6) show the discontinuity estimates after controlling for fluctuations in the age-profile on childcare arrangement. The full sample results are presented in Panel A. In Column (1), the RD estimate indicates that the firstborn child's school entry significantly increases the probability of receiving parental care by 3.1 percentage points ($p<0.01$). This result is robust for alternative specification in Columns (2) to (4). In addition, I find a statistically significant decrease in daycare center use in Columns (1) and (2) in Panel A.

The results show fairly sizable effects among children with only one sibling. In Panel B, the firstborn child's school entry is associated with an increase in the probability of receiving parental care in all columns. In the baseline specification in Column (1), the probability of receiving parental care increases by 4.0% at the margin. In addition, the utilization rate of daycare centers decreases in Panel B. In contrast, there were no similar effects observed for children with two or more siblings. The RD coefficients on parental care and daycare center use for this subgroup are not significant, as shown in Panel C. Finally, I check whether the results changes by children's age in Panel D and E. In short, point estimates in these Panels suggest that there is no particular heterogeneity in the childcare arrangement by children's age.

Additional explanation is needed in regards to the large increase in parental care among children with one sibling. These results reflect the possibility that a mother easily cannot change childcare arrangements if she has multiple preschool children. Since childcare providers for young preschoolers change as they grow up, rearrangement of childcare for multiple preschoolers requires large adjustment costs. Thereby, mothers seem to take more time for themselves rather than increasing hours spent with their preschool children when the firstborn child enters elementary school. By contrast, a mother with two children can flexibly change the childcare arrangement for one preschool child when his or her elder sibling enters elementary school. The subsample results in Table 6 may reflect the differences in these adjustment costs.

5.5 Effects on Children's Health

5.5.1 Short-run Effects

As shown previously, reductions in maternal labor supply lead to an increase in parental care and lower utilization of daycare centers. Given that children in daycare centers experience higher rates of infectious diseases (Gordon *et al.*, 2007; Silverstein *et al.*, 2003; Kamper-Jorgensen *et al.*, 2011), it is likely that the firstborn child's school entry would reduce the incidence of subjective symptoms among their younger preschool siblings through the resulting increase in parental care. This effect may be stronger among children with one sibling since statistically significant increase of parental care is found in this group. However, I find no improvement in both full sample and subsample results.

The binned scatterplot of the probability of having any symptoms is presented in Figure 5-(a). The dots on the left-hand side of the vertical line exhibit a downward trend because children experience fewer symptoms as they get older, and the probability stabilizes at approximately 25% (as shown on the right-hand side of the figure). In addition, there does not appear to be any significant jump at the threshold. Indeed, the probability of having any symptoms at the margin of the threshold appears to be smooth regardless of the sudden decrease in maternal employment rate and increase in parental care. Table 7 summarizes the results of the ITT-RD specification. Coefficients from all specifications are not statistically significant in full sample, suggesting that the reduction in maternal labor supply does not reduce the probability of having symptoms.[13]

Next, the analysis focuses on selected symptoms. Table 8 reports the reduced form estimates on the 10 symptoms that were consistently surveyed in the CSLC from 1995 to 2010. Although the detailed reporting and interpretation of the results for each item would be too specific and beyond the scope of this study, the reduced form estimates are generally found to be non-significant. Only in "fever", which may be related to infectious diseases (Column 1), were negative effects observed in all specifications. The scatterplot in Figure 5-(b) also indicates a discontinuous reduction.

However, these results can be obtained by so-called "p-hacking" "since I examine the effects on multiple outcome variables. For example, the probability of falsely rejecting at least one null hypothesis exceeds 70% if one conducts 25 independent placebo tests at the 0.05 level when all

[13] Of course, it is likely that the health status of the first-born child changes after school entry, and that this affects the health of younger siblings through inter-household infection transmissions. However, results of the continuity tests in Table 3 suggest that subjective symptoms of the first-born child are continuous around the threshold month.

null hypotheses are true. Thus, it is not surprising that some of the estimates in this paper exhibit p-values less than 0.05. In order to address the issue of false rejection, recent papers are increasingly adopting multiple hypothesis testing. While there are several methods to implement multiple hypothesis testing, I show here how p-value in the main results decreases under multiple hypothesis testing by applying stepdown methods (Romano & Wolf, 2005). Compared with the methodologies in classical multiple hypothesis testing, Romano & Wolf (2005) implicitly allow a joint dependence structure of the test statistics. Table 8 report stepdown p values in the rows under p values calculated by standard method. While standard methods provide statistically significant effects on fever, stepdown p values for this outcome variable are larger than 0.05 in all specifications.

In addition, maternal employment is found to be irrelevant on chronic conditions such as asthma-related symptoms. Column (5) shows that the effects on the probability of experiencing "wheezing" are not significant. Given that asthma is a major cause of child hospitalization, the fact that there is no significant change in "wheezing" may suggest that the hospitalization rate is stable at the threshold. In addition, no effects are observed on "rash" in Column (9), which is indicative of skin problems. These findings suggest that short-term reductions in the maternal labor supply is not associated with chronic health conditions.

5.5.2 Medium-run Effects

In the RD-ITT approach in this study, the medium-run effects can be derived by setting cut-off months to 86 months instead of 75 months. The results are summarized in Table 9. In Column (1), I report the results on the probability to have any symptom. Here, as well as the short-run effects in Table 8, I find no improvement in this outcome. Rather, point estimates are consistently positive, strongly denying health benefit from reduced maternal labor supply in the medium-run. Results on the selected symptoms are reported from Column (2) to (11). Since I test 10 hypothesis jointly, stepdown p values which address potential over-rejection are presented as well as standard p values. While standard p values indicate statistically significant discontinuity in "fever" (Column (2)), 3 out of 4 stepdown p values are not statistically significant. In other outcomes, I find no statistically significant effects. Therefore, my results suggest that medium-run effects are also limited, while two previous studies consistently found statistically significant effects of maternal labor supply (Gennetian *et al.*, 2010; Morrill, 2011).

6 Robustness Check

6.1 Donut-hole RD

A potential threat to the analysis is a heaping-induced bias. In my context, parents may elect to deliver after April 2 due to the potential advantages for their children. The inclusion of firstborn child's birth month fixed effects is one way to alleviate this bias, but it is possible to use another way. Specifically, I conduct donut-hole RD by excluding a few observations around the threshold (Barreca *et al.*, 2011), and the results are presented in Online Appendix B.[14] Briefly, the results from the donut-hole RD analysis suggest that heaping-induced bias is limited because point estimates are stable across different sizes of the donut hole, although the standard errors become larger with increasing sizes of the donut hole.

6.2 Placebo Tests

As a robustness check on the main results, I conduct placebo tests using a series of alternative timings of the firstborn child's school entry. Following the approach described in Meyersson (2014), I use alternative timings of school entry, ranging from 24 months (2 years) before the actual school entry to 60 months (5 years) after entry. If the results described in the previous section are spurious, RD regression with alternative school entry months may show significant effects on maternal employment, parental care, and child hospitalization. In particular, these placebo tests directly address the threat that the seasonal pattern of the firstborn child's birth month generates irregular discontinuity. For the choice of bandwidth and polynomial functions, I analyze bandwidths from 18 to 36 months, and investigate the 2 polynomial functions individually (quadratic, cubic). In addition, I estimate each specification with and without covariates. Therefore, a total of 72 RD estimates (2 polynomial functions $\times$ 2 (with and without covariates) $\times$ 18 months) are generated for each potential treatment. After estimating all the results, I calculate the average t value of the 86 RD coefficients and graphically plot the values. If the t value exceeds 2 (or -2), this would suggest that the potential treatment significantly increases (or decreases) the estimate of the dependent variable.

The results of the placebo tests are summarized in Figure 6. Figure 6-(a) shows the results of maternal employment, and the solid line represents the average t value that corresponds to each alternative treatment. The solid vertical line represents 75 months and the dashed vertical line represents 87 months which are the cut-off month to measure the medium-run effects. Because I test numerous alternative timings, some of the average t values approach 2 or -2. However, the average t value of the effects on maternal employment is clearly below -2 only when I utilize the true

[14]The model is based on a quadratic polynomial age-profile fully interacted with a dummy variable for school-entry age or older. In all estimations, the bandwidth is fixed at 36 months and all covariates are controlled for.

timing of school entry, indicating that my results on maternal employment are not spurious. Next, Figure 6-(b) also shows that the probability of parental childcare increases only with the actual timing of the treatment, and not with other alternative treatments.[15] Figures 6-(c) suggests that there are no clear associations between the firstborn child's school entry and incidence of overall symptoms in both short-run and medium-run.

In addition, the potential treatments in -24, -12, 24, 36, 48 and 60 months are of particular importance since they compare "March child" with "April child" at the different developmental stages. If bias from first born child's birth month is severe, we would have found statistically significant effects from these potential treatments. However, I find no regular pattern across these timings. This suggests that the bias from unobservable factors which are associated with first born child's birth month is limited.

7 Conclusion

Despite the increase in labor force participation among women with children in the past 30 years, few studies seek to disentangle the causal effect of maternal employment on child health. By exploiting unique institutional settings in childcare availability in Japan, this study shows how the reduction of maternal employment affects health among preschool children. Specifically, the identification strategy in this study is based on the fact that mothers in Japan are likely to exit from the labor market to provide after-school childcare for their children after enrollment in elementary school because of the shortage of after-school childcare services. This discontinuous reduction in childcare availability is notorious in Japan, and is referred to as the" Wall for Mothers with First Graders" (Takaku, 2017). By exploiting this "Wall", this study establishes novel RD evidence on the impact of maternal labor supply on health outcomes among preschool children. The results show that the maternal employment rate drops by approximately 16% just after the firstborn child's school entry. In addition, there is a significant increase in parental care for the younger siblings. Despite the reduced maternal labor supply and increasing parental care, I find no improvement in subjective measures of children's health status. More importantly, I find no effects on asthma-related symptoms, while Morrill (2011) suggests that maternal employment is associated with asthma-related hospitalization. Overall, the decrease in maternal labor supply and the increase in parental care are not associated with the health status of their children in both short-run and medium-run among preschool children.

The important implication of these findings is that a large reduction in the maternal labor supply and an increase in parental care, caused by the "wall for mothers with first graders"(Takaku,

[15] Note that the results on parental care are based on a subsample of children with only one elder sibling because significant effects are detected only in this group.

2017), does not affect child health after the critical period *in utero* and in the first 12 months of life. As many studies that exploit maternity leave reforms have addressed the effects on these earlier periods (Baker & Milligan, 2010; Liu & Skans, 2010; Rossin, 2011; Carneiro *et al.*, 2015), it is of particular importance that my research design does not include pregnant women. Given that the average age of the sample at the cut-off month is 40 months (Figure 1-(b)), this article mainly evaluates the impact of maternal labor supply on children who are approximately 3 years old. In addition, this paper presents suggestive evidences on the changes in treatment effects over time, by showing null effects in both short-run and medium-run effects. With this regard, my results on both short-run and medium-run effects indicate that the reduction of maternal labor supply due to the limited child care availability will not lead to improvement of child health in the long-run, since the treatment effects in the medium-run, as well as these in the short-run, do not indicate the improvement of child health. While there are many reasons on the null effects, one plausible explanation may be that mothers' attitude to spend longer time with their children is not so positive, even if maternal time input increases due to the "wall". Although it is an extreme example, unexpected job loss among mothers may have negative effects on children' outcomes, while job loss generally increases maternal time input to children (Hill *et al.*, 2011). While exit from labor market due to the "wall" is probably less negative event than job loss, it is reasonable that mother's happiness level, which is an important determinant of child outcomes, is negatively affected by this treatment.

There remain several limitations to the study. First, my study focuses on the younger siblings and excludes firstborn children, which is similar to the approach employed by Morrill (2011) and Bettinger *et al.* (2014). Because studies have shown that children of different birth orders are raised differently and have different cognitive abilities (Haan *et al.*, 2014), the exclusion of firstborn children may limit the external validity of my analysis. Second, the results on subjective symptom are subject to selection bias since I exclude hospitalized children. This is potentially problematic if the hospitalization rate changes sharply at the threshold (Morrill, 2011). Note, however, that the number of hospitalized children in my data is too small (0.5 %) to obtain robust evidences on this outcomes. In addition, I directly check how hospitalization rate changes at the threshold and find no jumps in Online Appendix A. Therefore, the extent of the selection bias seems to be limited. Finally, we should also examine the effects on children's objective health status since self-reported health status may subject to detection and reporting bias.

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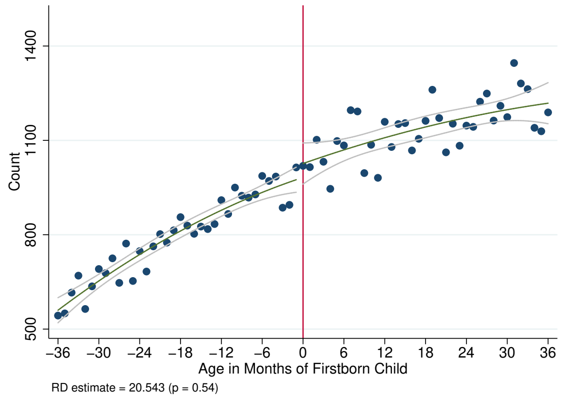
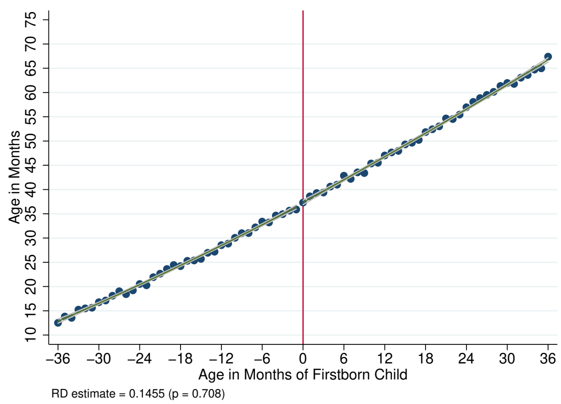
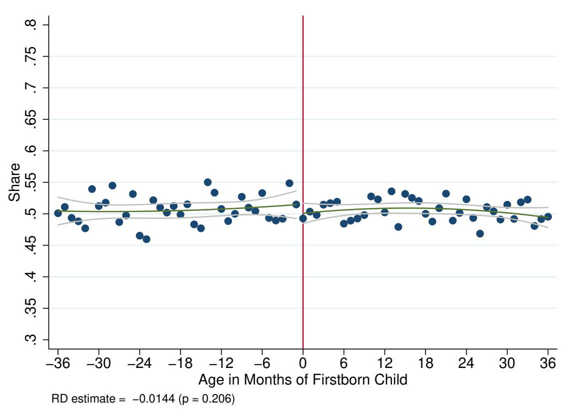


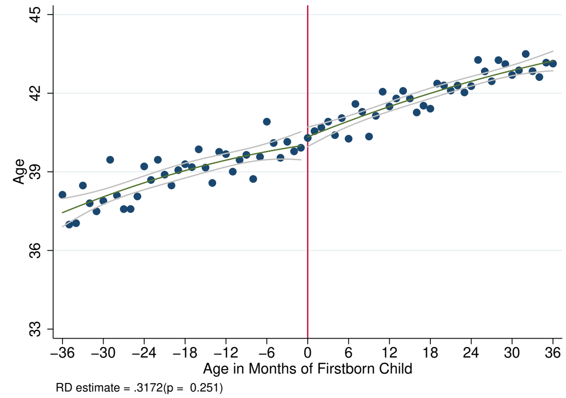
(a) Count



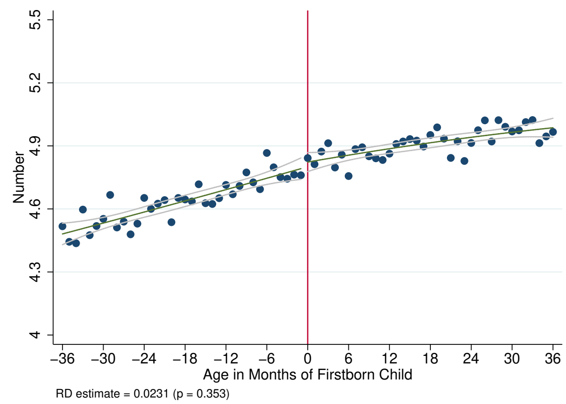
(b) Age in Months



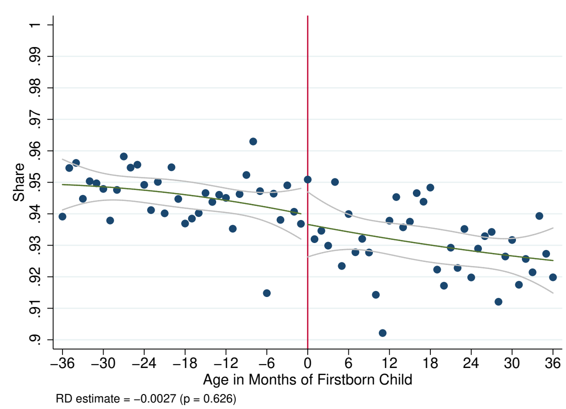
(c) Share of Girls



(d) Age of Household Head



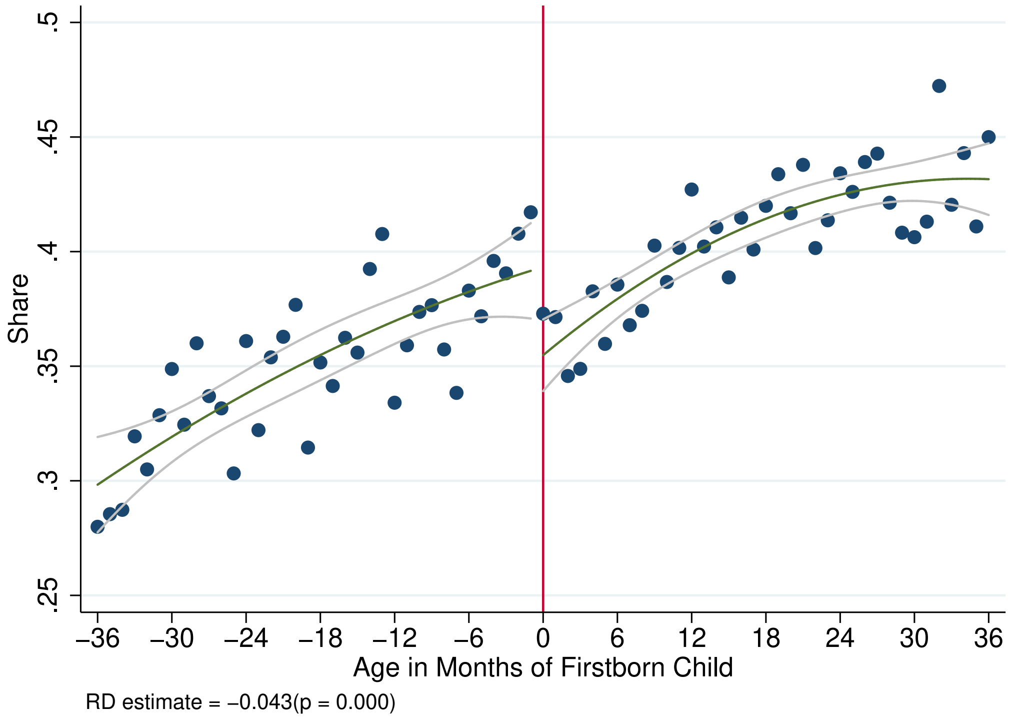
(e) Number of Household Members



(f) Household Head's Working Status

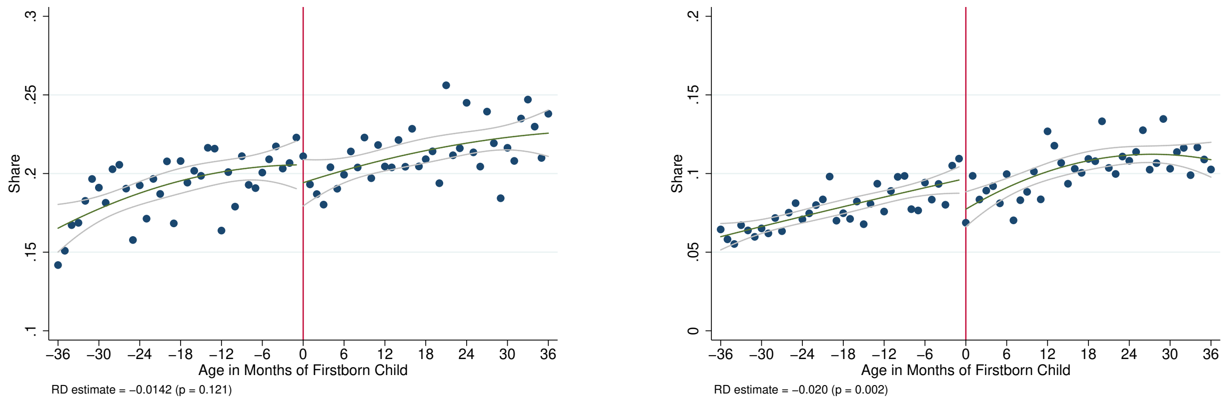
Note: The horizontal axis represents the age in quarters of the firstborn child standardized by the month that they enroll in elementary school. The count refers to the number of observations in each bin. Line represents quadratic fit and 95 % confidence intervals.

Figure 1: Identification Checks



Note: The horizontal axis represents the age in quarters of the firstborn child standardized by the month that they enroll in elementary school. Line represents quadratic fit and 95 % confidence intervals.

Figure 2: Share of Working Mothers

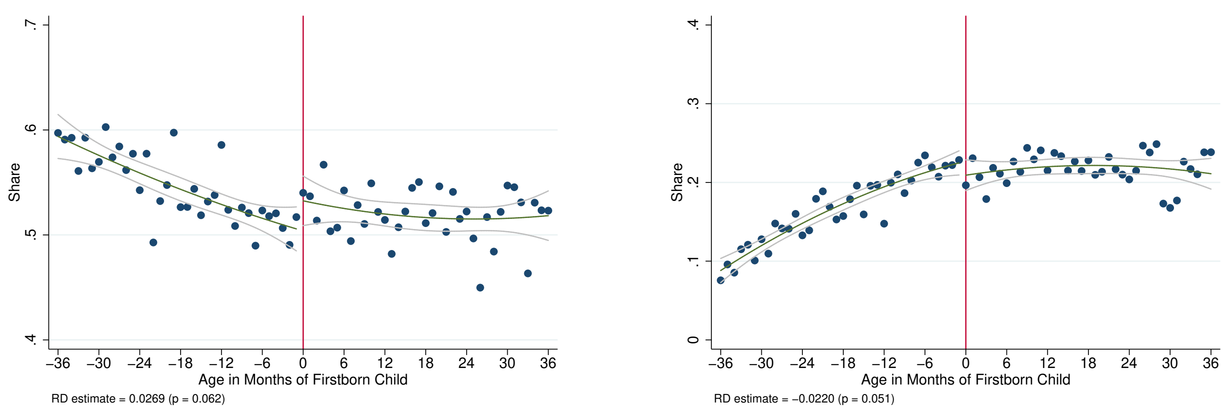


(a) General Employee

(b) Employee with Short-term Contract

Note: The horizontal axis represents the age in quarters of the firstborn child standardized by the month that they enroll in elementary school. Line represents quadratic fit and 95 % confidence intervals.

Figure 3: Type of Mother's Employment

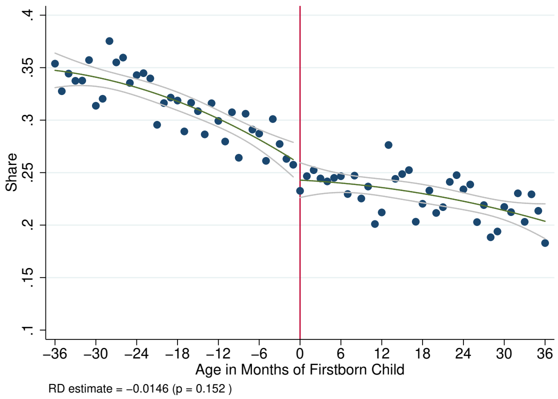


(a) Parental Care

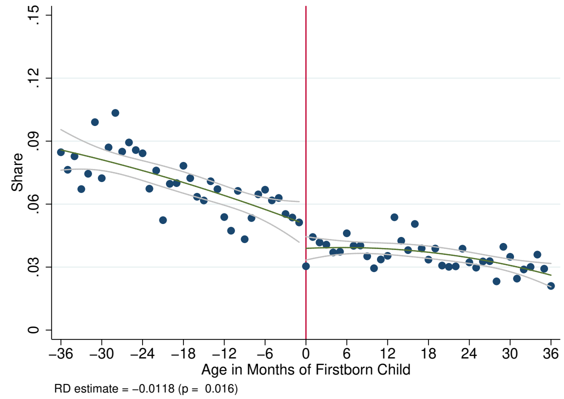
(b) Daycare Center Use

Note: The horizontal axis represents the age in quarters of the firstborn child standardized by the month that they enroll in elementary school. Line represents quadratic fit and 95 % confidence intervals.

Figure 4: Childcare Providers in the Daytime



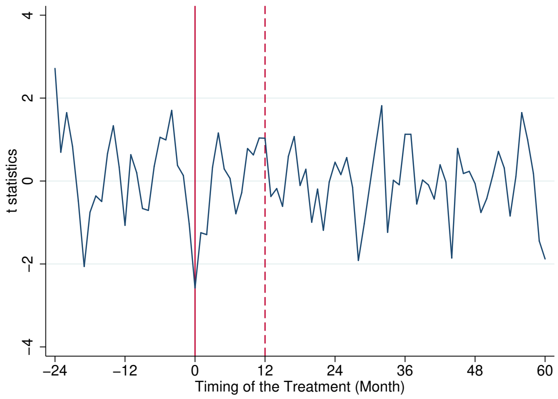
(a) Subjective Symptoms



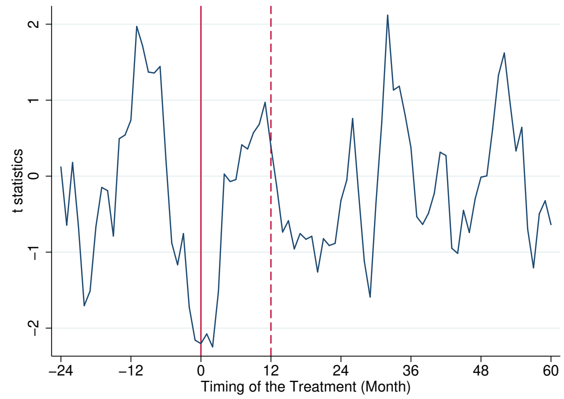
(b) Fever

Note: The horizontal axis represents the age in quarters of the firstborn child standardized by the month that they enroll in elementary school. Line represents quadratic fit and 95 % confidence intervals.

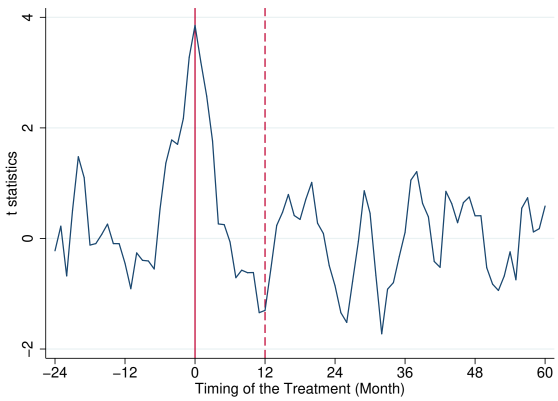
Figure 5: Probability of Subjective Symptoms



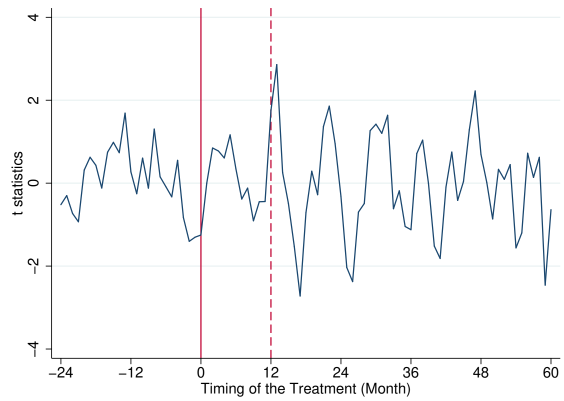
(a) Maternal Employment



(c) Daycare Center Use



(b) Parental Childcare



(d) Any Symptoms

Note: The vertical axis represents the average t statistics of placebo RD estimates at various potential cutoffs. The t statistics are averages of 2 different control function specifications: specifically, quadratic, and cubic control function is estimated on each side of the cut-off. For each polynomial function, the specification with and without extensive covariates is estimated. In addition, the bandwidth is changed from 18 months to 36 months. Therefore, each dot represents average t statistics of 72 $(2 * 2 * 18 = 72)$ RD estimates The horizontal axis represents the timing of the placebo treatments ranging from 24 months before the firstborn child's school entry to 60 months after the event. The solid vertical line represents 75 months and the dashed vertical line represents 87 months which are the cut-off month to measure the medium-run effects. The results on parental childcare are based on the subsample of children with only one elder sibling.

Figure 6: Placebo Tests for Robustness

Table 2: Descriptive Statistics

	Obs.	Mean	S.D
Panel A. Health Outcomes			
Probability of Symptom	65,556	0.26	0.44
Panel B .Maternal Labor Supply			
Mother Employed	65,556	0.38	0.49
General Employees	65,556	0.21	0.40
Employee with Short-term Contract	65,556	0.09	0.29
Panel C .Childcare Provision			
Parental Care	44,865	0.68	0.47
Daycare Center	44,865	0.25	0.43
Panel D .Covariates			
Age in Months	65,556	40.95	22.76
Girl	65,556	0.51	0.50
Number of Children	65,556	2.38	0.56
Household Size	65,556	4.81	1.10
Age of Household Head	65,556	40.74	12.20
Age of Spouse	65,556	38.27	11.70
Household Head Employed	65,556	0.94	0.24
Subjective Symptom of the Firstborn Child	65,556	0.26	0.44

Note: "Household Head Employed" takes a value of one if a household head works for remuneration. Number of observations for childcare is lower than the maximum (65,556) since the CSLC did not survey childcare in 1995.

Table 3: Identification Checks

	18 months (1)	18 months (2)	18 months (3)	36 months (4)
Panel A. Parametric Density Test				
Ln Count	0.044	0.011	0.051	0.042
	(0.037)	(0.032)	(0.056)	(0.039)
Number of Observations	36	36	36	72
Panel B. Discontinuity in Covariates				
Age in Month	0.341	0.088	0.545*	-0.085
	(0.239)	(0.185)	(0.312)	(0.214)
Share of Girl	-0.017	-0.009	-0.013	-0.01
	(0.011)	(0.007)	(0.016)	(0.011)
Age of Heads	0.348	0.273	0.075	0.204
	(0.238)	(0.265)	(0.215)	(0.267)
Age of Spouses	0.12	0.088	-0.103	0.06
	(0.283)	(0.366)	(0.310)	(0.350)
Number of Children	0.003	-0.014	0.057***	-0.005
	(0.016)	(0.015)	(0.021)	(0.016)
Household Size	0.019	-0.003	0.059**	-0.002
	(0.028)	(0.025)	(0.024)	(0.025)
Head's Working Status	-0.008	-0.013**	0.011*	-0.005
	(0.007)	(0.006)	(0.006)	(0.006)
Firstborn Child's Subjective Symptom	-0.014*	-0.012	-0.013	-0.002
	(0.008)	(0.008)	(0.012)	(0.008)
Number of Observations	33,725	33,725	33,725	65,556
Polynomial Order	Quadratic	Quadratic	Cubic	Cubic
Year and Prefecture Fixed Effect	Yes	Yes	Yes	Yes
Firstborn Child's Birth Months FEs	No	Yes	No	Yes

Note: This table summarizes the RD estimates based on alternative specifications. Columns (1) and (2) show the results of RD regression with a quadratic polynomial based on alternative bandwidths of 18 months, with and without the firstborn child's birth month fixed effects, respectively. Columns (3) and (4) show the results of RD regression with alternative polynomial orders. Bandwidth is extended to 36 months in Column (4). Standard error is clustered at the age in months of the firstborn child. ***, $p < 0.01$. **, $p < 0.05$. *, $p < 0.1$.

Table 4: Effect on Maternal Employment

	18 months (1)	18 months (2)	18 months (3)	36 months (4)	Mean of Dep.
Full Sample	-0.065***	-0.041**	-0.087***	-0.059***	0.395
	(0.017)	(0.020)	(0.023)	(0.019)	
Observations	33,725	33,725	33,725	65,556	
Children with One Sibling	-0.037*	-0.016	-0.038	-0.044**	0.402
	(0.020)	(0.027)	(0.028)	(0.021)	
Observations	21,902	21,902	21,902	42,818	
Children with Two or More Siblings	-0.118***	-0.093*	-0.173***	-0.094**	0.382
	(0.036)	(0.051)	(0.050)	(0.041)	
Observations	11,823	11,823	11,823	22,738	
Aged Less Than 3 Years Old	-0.056**	-0.044	-0.073**	-0.045*	0.361
	(0.023)	(0.032)	(0.031)	(0.025)	
Observations	15,390	15,390	15,390	30,206	
Aged More Than 3 Years Old	-0.074***	-0.031	-0.104***	-0.099***	0.427
	(0.022)	(0.032)	(0.032)	(0.029)	
Observations	18,335	18,335	18,335	35,350	
Polynomial Order	Quadratic	Quadratic	Cubic	Cubic	
Year and Prefecture Fixed Effect	Yes	Yes	Yes	Yes	
Other Covariates	No	Yes	No	Yes	

Note: This table summarizes the RD estimates based on alternative specifications. Columns (1) and (2) show the results of RD regression with a quadratic polynomial based on alternative bandwidths of 18 months, with and without extensive covariates which include sex, age in months, age of household head, number of children under 15 years of age, number of household members, working status of household head, survey year effects, firstborn child's subjective symptom, and the firstborn child's birth month fixed effects, respectively. Columns (3) and (4) show the results of RD regression with alternative polynomial orders. Bandwidth is extended to 36 months in Column (4). Standard error is clustered at the age in months of the firstborn child. In the rightmost column, means of the dependent variable during 6 months before the firstborn child's school entry are presented. ***, $p < 0.01$. **, $p < 0.05$. *, $p < 0.1$.

Table 5: Effect on the Type of Mother's Employment

	18 months (1)	18 months (2)	18 months (3)	36 months (4)	Mean of Dep.
Self-Employed , etc.	-0.009	-0.006	-0.009	0.007	0.09
	(0.012)	(0.015)	(0.017)	(0.012)	
General Employee	-0.014	-0.008	-0.030**	-0.003	0.21
	(0.007)	(0.010)	(0.010)	(0.015)	
Employee with Short-term Contract	-0.020***	-0.024***	-0.021**	-0.042***	0.09
	(0.010)	(0.015)	(0.014)	(0.012)	
Observations	35,665	35,665	35,665	66,975	
Year and Prefecture Fixed Effects	X	X	X	X	
Other Covariates	no	yes	no	yes	
Polynomial Order	Quadratic	Quadratic	Cubic	Cubic	

Note: This table summarizes the RD estimates based on alternative specifications. Columns (1) and (2) show the results of RD regression with a quadratic polynomial based on alternative bandwidths of 18 months, with and without extensive covariates which include sex, age in months, age of household head, number of children under 15 years of age, number of household members, working status of household head, survey year effects, firstborn child's subjective symptom, and the firstborn child's birth month fixed effects, respectively. Columns (3) and (4) show the results of RD regression with alternative polynomial orders. Bandwidth is extended to 36 months in Column (4). Standard error is clustered at the age in months of the firstborn child. In the rightmost column, means of the dependent variable during 6 months before the firstborn child's school entry are presented. ***, $p < 0.01$. **, $p < 0.05$. *, $p < 0.1$.

Table 6: Effect on the Choice of Childcare Provider

	18 months (1)	18 months (2)	18 months (3)	36 months (4)	Mean of Dep.
Panel A. Full Sample					
Parental Care	0.031***	0.046**	0.027**	0.028**	0.70
	(0.01)	(0.02)	(0.01)	(0.01)	
Daycare Center	-0.025**	-0.041**	-0.017	-0.014	0.27
	(0.01)	(0.02)	(0.01)	(0.01)	
Number of Observations	26,497	26,497	26,497	45,905	
Panel B. Children with One Sibling					
Parental Care	0.040***	0.049**	0.042***	0.034**	0.70
	(0.01)	(0.02)	(0.01)	(0.02)	
Daycare Center	-0.037***	-0.043**	-0.034**	-0.024	0.28
	(0.01)	(0.02)	(0.01)	(0.02)	
Number of Observations	17,500	17,500	17,500	29,973	
Panel C. Children with Two or More Siblings					
Parental Care	0.014	0.04	-0.005	0.008	0.71
	(0.02)	(0.04)	(0.03)	(0.03)	
Daycare Center	-0.001	-0.033	0.019	0.01	0.26
	(0.02)	(0.04)	(0.02)	(0.03)	
Number of Observations	8,997	8,997	8,997	14,892	
Panel D. Aged Less Than 3 Years Old					
Parental Care	0.023	0.040*	0.012	0.006	0.77
	(0.02)	(0.02)	(0.02)	(0.02)	
Daycare Center	-0.013	-0.029	-0.004	0.009	0.20
	(0.02)	(0.02)	(0.02)	(0.02)	
Number of Observations	12,327	12,327	12,327	24,174	
Panel E. Aged More Than 3 Years Old					
Parental Care	0.027*	0.062**	0.023	0.041*	0.65
	(0.02)	(0.03)	(0.02)	(0.02)	
Daycare Center	-0.021	-0.058**	-0.005	-0.012	0.34
	(0.02)	(0.03)	(0.02)	(0.02)	
Number of Observations	14,170	14,170	14,170	23,427	
Polynomial Order	Quadratic	Quadratic	Cubic	Cubic	

Note: This table summarizes the RD estimates based on alternative specifications. Columns (1) and (2) show the results of RD regression with a quadratic polynomial based on alternative bandwidths of 18 months, with and without extensive covariates which include sex, age in months, age of household head, number of children under 15 years of age, number of household members, working status of household head, survey year effects, firstborn child's subjective symptom, and the firstborn child's birth month fixed effects, respectively. Columns (3) and (4) show the results of RD regression with alternative polynomial orders. Bandwidth is extended to 36 months in Column (4). Standard error is clustered at the age in months of the firstborn child. In the rightmost column, means of the dependent variable during 6 months before the firstborn child's school entry are presented. ***, $p < 0.01$. **, $p < 0.05$. *, $p < 0.1$.

Table 7: Effect on the Probability of Having Any Subjective Symptoms

	18 months (1)	18 months (2)	18 months (3)	36 months (4)	Mean of Dep.
Full Sample	-0.015	-0.029	-0.004	-0.024	0.275
	(0.016)	(0.019)	(0.022)	(0.015)	
Observations	34,102	34,102	34,102	65,281	
Two Siblings	-0.017	-0.034	-0.014	-0.031*	0.275
	(0.018)	(0.023)	(0.026)	(0.018)	
Observations	22,219	22,219	22,219	42,818	
Three or More Siblings	-0.008	-0.013	0.008	-0.006	0.275
	(0.028)	(0.036)	(0.038)	(0.029)	
Observations	11,883	11,883	11,883	22,463	
Aged Less Than 3 Years Old	-0.03	-0.035	-0.022	-0.031	0.293
	(0.022)	(0.027)	(0.030)	(0.021)	
Observations	15,548	15,548	15,548	30,061	
Aged More Than 3 Years Old	-0.003	-0.031	0.015	0.006	0.257
	(0.021)	(0.026)	(0.029)	(0.024)	
Observations	18,554	18,554	18,554	35,220	
Polynomial Order	Quadratic	Quadratic	Cubic	Cubic	
Year and Prefecture Fixed Effect	Yes	Yes	Yes	Yes	
Other Covariates	No	Yes	No	Yes	

Note: This table summarizes the RD estimates based on alternative specifications. Columns (1) and (2) show the results of RD regression with a quadratic polynomial based on alternative bandwidths of 18 months, with and without extensive covariates which include sex, age in months, age of household head, number of children under 15 years of age, number of household members, working status of household head, survey year effects, firstborn child's subjective symptom, and the firstborn child's birth month fixed effects, respectively. Columns (3) and (4) show the results of RD regression with alternative polynomial orders. Bandwidth is extended to 36 months in Column (4). Standard error is clustered at the age in months of the firstborn child. In the rightmost column, means of the dependent variable during 6 months before the firstborn child's school entry are presented. ***, $p < 0.01$. **, $p < 0.05$. *, $p < 0.1$.

Table 8: Effect on Individual Symptoms

Polynomial Order	Covariates	Bandwidth Choice	Fever (1)	Cough (2)	Headache (3)	Wheezing (4)	Toothache (5)	Stuff nose (6)	Diarrhea (7)	Stomachache (8)	Rash (9)	Cut (10)
Quadratic	No	18 months	-0.022***	-0.012	0.001	-0.009	0.001	-0.012	0.003	-0.001	-0.001	0.001
			(0.007)	(0.011)	(0.002)	(0.007)	(0.002)	(0.011)	(0.004)	(0.002)	(0.006)	(0.003)
Standard p value			0.002	0.235	0.634	0.137	0.827	0.258	0.478	0.663	0.866	0.836
Stepdown p value			0.055	0.617	0.970	0.463	0.975	0.617	0.861	0.970	0.975	0.975
Observations			65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556
Quadratic	Yes	18 months	-0.026***	-0.016	-0.001	-0.003	0.002	-0.015	0.002	0.001	-0.009	-0.001
			(0.009)	(0.013)	(0.002)	(0.008)	(0.003)	(0.014)	(0.005)	(0.002)	(0.008)	(0.004)
Standard p value			0.004	0.217	0.772	0.731	0.449	0.238	0.707	0.710	0.204	0.743
Stepdown p value			0.075	0.473	0.950	0.950	0.786	0.473	0.950	0.950	0.453	0.950
Observations			35,567	35,567	35,567	35,567	35,567	35,567	35,567	35,567	35,567	35,567
Cubic	No	18 months	-0.009	0	0.001	-0.012	-0.001	0.004	0.002	0	0	0
			(0.010)	(0.014)	(0.002)	(0.009)	(0.003)	(0.016)	(0.006)	(0.003)	(0.009)	(0.004)
Standard p value			0.352	0.984	0.642	0.160	0.746	0.790	0.695	0.950	0.993	0.963
Stepdown p value			0.363	0.975	0.876	0.169	0.945	0.975	0.915	0.975	0.975	0.975
Observations			65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556
Cubic	Yes	36 months	-0.021***	-0.016	0.001	-0.004	0.002	-0.008	0.004	0.002	-0.003	0.006*
			(0.008)	(0.011)	(0.002)	(0.007)	(0.002)	(0.011)	(0.004)	(0.002)	(0.007)	(0.003)
Standard p value			0.004	0.129	0.634	0.511	0.463	0.444	0.301	0.385	0.611	0.077
Stepdown p value			0.055	0.488	0.876	0.876	0.876	0.876	0.771	0.861	0.876	0.373
Observations			65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556	65,556
Mean of Dep.			0.036	0.096	0.004	0.029	0.010	0.117	0.012	0.007	0.026	0.013

Note: This table summarizes the reduced form estimates based on alternative specifications. Each column corresponds to a specific symptom. The mean values of the dependent variables are reported in the bottom row. All equations control for survey year effects, and prefecture fixed effects. The firstborn child's birth month fixed effects are also controlled. Standard error is clustered at the age in months of the firstborn child. Romano & Wolf (2005)'s method is used for the calculation of stepdown p values. ***, $p < 0.01$. **, $p < 0.05$. *, $p < 0.1$.

Table 9: Medium-run Effect on Symptoms

Polynomial Order	Covariates	Bandwidth Choice	Any (1)	Fever (2)	Cough (3)	Headache (4)	Wheezing (5)	Toothache (6)	Stuff nose (7)	Diarrhea (8)	Stomachache (9)	Rash (10)	Cut (11)
Quadratic	No	18 months	0.018	0.011*	0.01	0.002	0.001	-0.005*	0	0	-0.003	0.002	0.001
			(0.015)	(0.006)	(0.010)	(0.002)	(0.006)	(0.003)	(0.010)	(0.003)	(0.002)	(0.005)	(0.004)
Standard p value				0.071	0.288	0.231	0.841	0.102	0.979	0.905	0.269	0.615	0.737
Stepdown p value				0.284	0.687	0.637	0.990	0.328	0.990	0.990	0.667	0.955	0.985
Observations			39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032
Quadratic	Yes	18 months	0.018	0.015*	0.005	0.004*	0.006	-0.007*	-0.002	0.002	-0.001	0.00	0.00
			(0.017)	(0.008)	(0.012)	(0.002)	(0.007)	(0.004)	(0.012)	(0.004)	(0.003)	(0.007)	(0.004)
Standard p value				0.057	0.641	0.124	0.451	0.081	0.919	0.613	0.618	0.953	0.989
Stepdown p value				0.229	0.960	0.383	0.915	0.294	0.990	0.960	0.960	0.990	0.990
Observations			39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032
Cubic	No	18 months	0.040*	0.017*	0.006	0.004**	-0.005	0.002	0.006	0.008*	-0.003	0.008	0.007
			(0.021)	(0.009)	(0.014)	(0.002)	(0.008)	(0.004)	(0.014)	(0.005)	(0.003)	(0.007)	(0.005)
Standard p value				0.052	0.717	0.106	0.475	0.591	0.645	0.118	0.469	0.303	0.204
Stepdown p value				0.229	0.906	0.318	0.881	0.906	0.906	0.328	0.881	0.647	0.508
Observations			39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032	39,032
Cubic	Yes	36 months	0.024*	0.020***	0.018*	0.002	0.01	-0.007**	0.005	0	-0.001	-0.001	-0.002
			(0.014)	(0.007)	(0.010)	(0.002)	(0.006)	(0.003)	(0.010)	(0.004)	(0.002)	(0.006)	(0.004)
Standard p value				0.002	0.048	0.342	0.093	0.037	0.549	0.897	0.596	0.905	0.533
Stepdown p value				0.045	0.254	0.846	0.363	0.254	0.945	0.975	0.945	0.975	0.945
Observations			75,090	75,090	75,090	75,090	75,090	75,090	75,090	75,090	75,090	75,090	75,090
Mean of Dep.			0.025	0.029	0.089	0.003	0.021	0.019	0.109	0.004	0.000	0.019	0.006

Note: This table summarizes the reduced form estimates based on alternative specifications. Each column corresponds to a specific symptom. The mean values of the dependent variables are reported in the bottom row. All equations control for survey year effects, and prefecture fixed effects. The firstborn child's birth month fixed effects are also controlled. Standard error is clustered at the age in months of the firstborn child. Romano & Wolf (2005)'s method is used for the calculation of stepdown p values. ***, $p < 0.01$. **, $p < 0.05$. *, $p < 0.1$.

Online Appendix

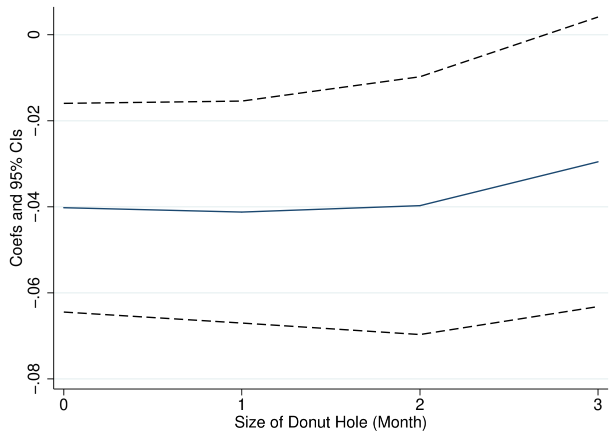
A Effects on Hospitalization

Table A1: Effect on Hospitalization

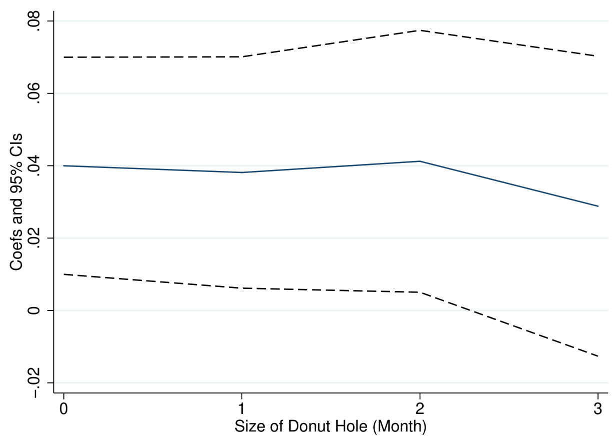
	18 months (1)	18 months (2)	18 months (3)	36 months (4)	Mean of Dep.
Full Sample	-0.003	-0.009***	-0.003	-0.008***	0.007
	(0.003)	(0.003)	(0.004)	(0.003)	
Observations	35,020	35,020	35,020	66,461	
Two Siblings	-0.002	-0.010**	-0.004	-0.009***	0.007
	(0.003)	(0.004)	(0.005)	(0.003)	
Observations	22,814	22,814	22,814	43,575	
Three or More Siblings	-0.003	-0.006	0.001	-0.004	0.008
	(0.005)	(0.006)	(0.007)	(0.005)	
Observations	12,206	12,206	12,206	22,886	
Aged Less Than 3 Years Old	-0.007	-0.011**	-0.006	-0.008*	0.009
	(0.004)	(0.006)	(0.006)	(0.004)	
Observations	15,975	15,975	15,975	30,633	
Aged More Than 3 Years Old	-0.001	-0.007*	-0.001	-0.002	0.006
	(0.003)	(0.004)	(0.004)	(0.006)	
Observations	19,045	19,045	19,045	35,828	
Polynomial Order	Quadratic	Quadratic	Cubic	Cubic	
Year and Prefecture Fixed Effect	Yes	Yes	Yes	Yes	
Other Covariates	No	Yes	No	Yes	

Note: This table summarizes the RD estimates based on alternative specifications. Columns (1) and (2) show the results of RD regression with a quadratic polynomial based on alternative bandwidths of 18 months, with and without extensive covariates which include sex, age in months, age of household head, number of children under 15 years of age, number of household members, working status of household head, survey year effects, firstborn child's subjective symptom, and the firstborn child's birth month fixed effects, respectively. Columns (3) and (4) show the results of RD regression with alternative polynomial orders. Bandwidth is extended to 36 months in Column (4). Standard error is clustered at the age in months of the firstborn child. In the rightmost column, means of the dependent variable during 6 months before the firstborn child's school entry are presented. ***, $p < 0.01$. **, $p < 0.05$. *, $p < 0.1$.

B Donut-hole RD



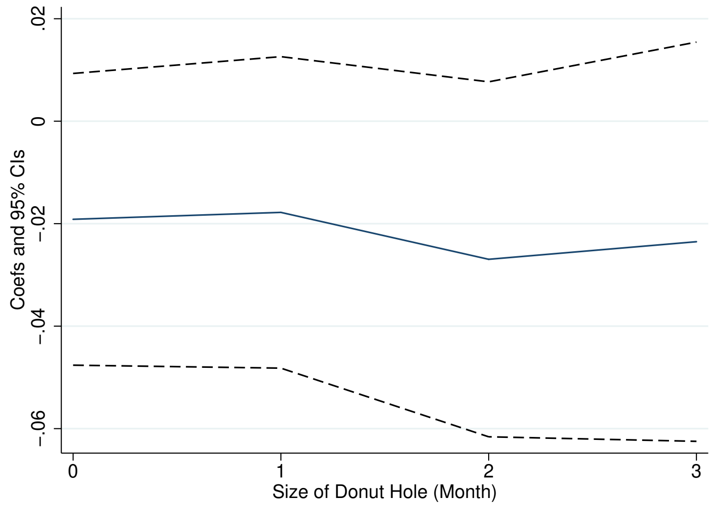
(a) Maternal Employment



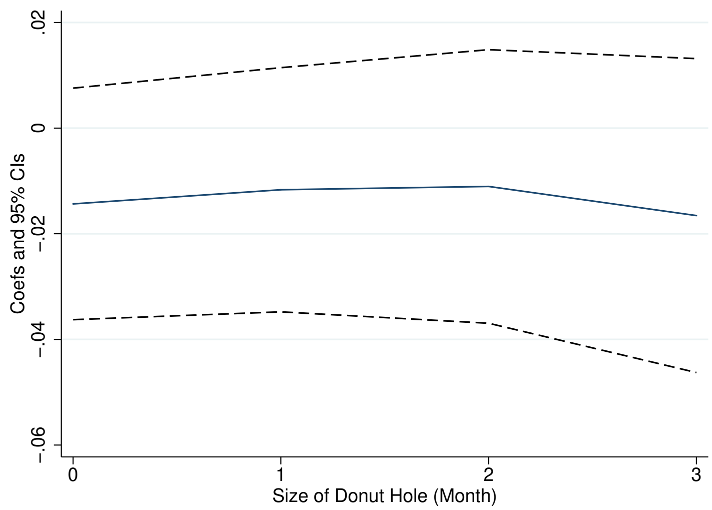
(b) Parental Care

Note: The horizontal axis represents the size of the donut hole, which is the number of months excluded from RD estimation. The value "0" represents the baseline specification where no observations are excluded. The model here is based on a quadratic polynomial age-profile fully interacted with a dummy variable for school-entry age or older. In all estimations, the bandwidth is fixed at 36 months. Dashed lines represent the 95% confidence intervals. Results in Figure (b) are based on the subsample of preschool children with one elder sibling.

Figure A1: Donut-hole RD Estimates



(c) Daycare Center Use



(d) Any Symptoms

Note: The horizontal axis represents the size of the donut hole, which is the number of months excluded from RD estimation. The value "0" represents the baseline specification where no observations are excluded. The model here is based on a quadratic polynomial age-profile fully interacted with a dummy variable for school-entry age or older. In all estimations, the bandwidth is fixed at 36 months. Dashed lines represent the 95% confidence intervals. Results in Figure (c) are based on the subsample of preschool children with one elder sibling.

Figure A1: Donut-hole RD Estimates