Parental Leave Reforms and the Employment of New Mothers: Quasi-experimental Evidence from Japan[☆]

Yukiko Asai[*]

Abstract

This study assesses the impact of changes in the income replacement rate of parental leave on new mothers' labour force participation after childbearing. The Japanese government increased the parental leave income replacement rate from 0% to 25% in 1995 and from 25% to 40% in 2001, creating 2 natural experiments. I identify the causal effect of these reforms by comparing the changes in the regular employment status of mothers who gave birth after the reform to the change for mothers who gave birth before the reform. The results suggest that the 2 reforms had no significant effects on the labour force participation of mothers who qualified for the reforms.

Keywords: Parental leave, maternal employment, income replacement, difference-in-difference

JEL: J13, J21, J22

1. Introduction

It has been 25 years since the Equal Employment Opportunity Law was enacted, but it remains difficult for mothers to continue working after childbearing in Japan. Among those who had a job before childbirth, only 38.0% of mothers continued to

have a job when the first child reached 1 year of age[1]. The Japanese female employment rate is approximately 65.7% for females aged 25 to 49, which is approximately 10 percentage points lower than the OECD average. The maternal employment rate is 29.8% for mothers with children 3 years of age or younger, which is approximately 30 percentage points lower than the average of OECD countries[2]. The percentage of women completing tertiary education is 59% for women aged 25 to 34 and 45% for women aged 25 to 64, although the employment rate for female university graduates is only 66.6%[3]. Seeking to promote a work-life balance, the Japanese government set a goal of increasing the return to work rate of mothers after the birth of their first child to 55% by 2017, an increase of 17 percentage points from the current situation. To accomplish this goal, the government has made numerous changes to the parental leave (PL) policy; therefore, it is crucial to assess the real impact of these changes and find an effective way to increase the rate of mothers who continue working after bearing children. In this paper, I investigate the effect of the policy reforms increasing the rate of PL income replacement on mothers' employment after childbearing to determine whether the policy changes increased the employment of those affected by the reform.

Maternity leave (ML) and PL programs vary by country and region[4]. In 2012, the Japanese ML program provides approximately 14 weeks of leave around the birth of the child. The income replacement during the ML is two-thirds of the

---

[1]The rate is calculated for mothers who gave birth to their first child between 2000 and 2004. National Institute of Population and Social Security Research(2005)Annual Population and Social Security Surveys

[2]OECD Family database(2011). Rate for Japan is in 2005.

[3]Number of 25 to 64 year-old in employment as a percentage of the population aged 25 to 64. OECD (2011)Education at a Glance 2011

[4]The definitions of 3 types of family leaves are the following. ML is a leave of absence for employed mothers surrounding childbirth and is mainly provided for maternal health reasons. Working during this period is prohibited in Japan and many other countries. Paternity leave is leave for fathers and is available in only small number of countries. PL is leave for parents and is provided as a supplement to maternity leave. The focus of this study is the PL taken by mothers.

previous income[5], and the PL program provides 50% of income replacement and the maximum duration is 16 months. The Japanese ML and PL programs are relatively generous compared to other OECD countries. The effects of ML and PL policies on maternal (or female) employment are hotly debated in many countries. Standard labour demand and supply theory predicts that an increase in maternal labour supply will reduce mothers' wages; therefore, studies on ML and PL policy investigate their effects on both subsequent employment and wages. Studies of ML are primarily conducted using United States data and the results are mixed. The United States has no mandated PL program and its ML is unpaid, therefore studies examine the effect of mandated ML. Klerman and Leibowitz [12] find that state ML statues have no statistically significant effect on the female employment rate. Conversely, Baum [3] finds that ML increased the number of mothers who eventually returned to their previous jobs. Waldfogel et al. [17], using data from United States, Britain and Japan, finds that ML coverage increases the probability of returning to work after childbirth in all 3 countries. Gruber [10] studies effect of ML costs on female labour market outcomes (wages), and finds the cost of ML is shifted to the wages of the group receiving benefits.

Studies on PL are mainly conducted on European countries and Canada. The Canadian ML program provides 55% income replacement and the duration is 15 weeks surrounding the birth, and the PL program provides 55% income replacement with a duration of 5 months plus additional months depending on the province in question. Canadian studies analyse the effect extending the duration of leave on the return to work rate. Baker and Milligan [2] find that the introduction of 17 to 18 weeks of PL expanding the duration to 29 to 70 weeks in some states increased the proportion of mothers employed and the proportion of mothers returning to work at the pre-birth employer. Hanratty and Trzcinski [11] find that an expansion of PL from 25 to 50 weeks has no effect on the return to work rate in the year following

---

[5]The ML income replacement rate was increased from 60% to two-thirds of the previous income beginning in April 2007

3

the birth. Germany, Austria and Sweden have very generous programs that provide approximately 100% income replacement during ML. Income replacement during PL varies by country: Germany: 67%, Austria: flat benefit of 340 Euro, Sweden: 80%, and the duration of PL is also generous in those countries. Germany for example has maximum of 3 years of leave, Austria provides 18 months and Sweden provides 16 months. For Austria, Lalive and Zweimuller [13] find that expansion of PL from 1 year to 2 years reduces the return to work rate and increases the probability that mothers have a second child. They also find that the negative effect on subsequent labour market outcomes does not persist in the long run (37 to 72 months after the birth). In Germany, Schonberg and Ludsteck [16] investigate the expansion of PL from 2 to 6 months in 1979, to 10 months in 1986, and to 3 years in 1992 and find that the changes induce mothers to delay their return to work. Ruhm and Teague [15] find that for 17 European countries, short to moderate durations of PL are positively associated with per capita incomes, employment rates and labour force participation rates. Ruhm [14], using data on 9 Western European countries, also finds that short and moderate lengths of leave increase female employment to population rates, however a moderate length of leave decreases wages by 3%.

The above research suggests that ML/PL coverage may increase the likelihood that mothers will return to work, however an excessively generous length of leave may decrease the maternal employment rate, and the introduction of ML/PL may reduce female wages. However, the effects on female wages may not persist in the long run. Those studies examine the difference between mothers who are covered by the program and those who are not, or regional differences in the timing of policy implementations. The outcome variables include the timing of a return to work, subsequent employment and subsequent wages. However, some of those studies may suffer from potential unobserved differences in mothers who select a company, country or region with ML/PL.

This paper focuses on PL policy reform enacted by the Japanese government and its effect on mothers' employment after childbearing. This study contributes to the previous literatures for 2 ways. First, this policy evaluation relies on an increase

in the rate of PL income replacement while the length of and the qualifications for PL are fixed. The PL income replacement rate for mothers is increased from 0% to 25% in 1995 and from 25% to 40% in 2001. As PL is a national program, PL eligibility varies by the timing of childbirth, rather than the mothers' choices of employers or region. Therefore, it is less likely to be correlated with the mothers' unobserved characteristics and presents evidence from a quasi-experiment. Second, this research focuses on a country where the female employment rate is low but the PL program is relatively generous. Previous studies tend to focus on European or North American countries; therefore this study contributes to the literature with new evidence from a developed country in Asia.

2. Parental Leave Policy in Japan

In Japan, pregnant women can take advantage of ML and PL programs, which afford them the right to return to their previous jobs after childbirth. Figure .1 is the time-line of a mother's childbirth, ML and PL. The ML Program is mandated by the Labor Standards Act (1947-) and allows mothers to take leave of 42 days before and 56 days after the birth of the child[6]. During ML, the ML income replacement is provided by the Health Insurance Program (1958-) and is equivalent to 60% of a mother's income at the time she takes her leave[7]. All working mothers who are covered by the health insurance, including non-regular employees, can utilise the program. The ML starting date can be adjusted based on the expected delivery date. Therefore, mothers cannot perfectly plan the timing of their ML and the following PL; their date of childbirth is the ending date of pre-birth-ML and the starting date of post-birth-ML. After ML, mothers can then take PL of up to 10

---

[6]It is illegal to work or allow a new mother work within 42 days of childbirth.

[7]In addition to the ML income replacement, 350,000 yen, a one-time maternity allowance is provided by the health insurance program (the allowance was increased to 420,000 yen in October 2010). The income replacement is provided as financial assistance for mothers because the costs of health screenings and delivery in Japanese gynaecology clinics are high. The ML income replacement rate was increased from 60% to two-thirds of the previous income beginning in April 2007. The current exchange rate is approximately 80 yen = 1 dollar and 100 yen=1 Euro.

months (up to 16 months from April 2005). The PL Program is mandated by the Child Care and Family Care Leave Act (1992-). A new mother must decide whether to take PL and to return to work after childbirth by 1 month before the birth at the latest and must submit the leave application form to her company. Based on her decision, her company submits the application form to the government with proof of the subsequent employment contract after the birth of her child. New mothers are also asked to submit the Maternal and Child Health Handbook, which is completed by the gynaecologist, to prevent pregnant mothers from providing a false expected delivery date. Although the submission deadline for this application is 1 month prior to the expected date of delivery, most mothers make their subsequent employment decision before the pre-ML period, which is at least 2 to 3 months before the birth due to the bureaucratic process and the social norms of Japanese companies[8].

A new mother takes a maximum of 10 months of PL and returns to work by the time child reaches exactly 12 months of age. The PL income replacement is paid through employment insurance, and therefore PL rights are only available to new mothers who are covered by the employment insurance program. The income replacement during PL is determined on the basis of the mother's average wage for the 6 months prior to the birth of her child[9]. The PL income replacement consists of 2 parts- one is a monthly payment during the leave and the other is paid as a lump-sum upon return to work. The lump-sum income replacement payment is a one-time payment conditional on the mother returning to work and is equal to the value of 10 months of monthly payments. A new mother must remain at her previous post for 6 months before receiving the lump-sum income replacement payment.

---

[8]New mothers who plan to quit their jobs must transfer their jobs to and train a replacement worker, therefore most expectant mothers provide their subsequent employment decision to the company at least 2 to 3 months before their expected delivery date.

[9]The 6-months average wage rate is calculated as follows. Sum the wages from 6 months before the birth, divide that number by 180, and then multiply by 30 days. The maximum wage rate is 430,200 yen and the minimum wage rate is 69,900 yen. The maximum and minimum amounts change every August.

[Figure 1 about here.]

Table .1 lists recent policy reforms. The government enacted a number of reforms over a short period of time to boost the maternal employment rate and declining birth rate. The original PL policy, which was developed in 1992, did not provide any income replacement. Income replacement from the employment insurance program began in 1995 and was 25% of mother's previous income at that time. The rate increased from 25% to 40% in 2001. The eligibility for PL was expanded to include non-regular employees in 2005, and the maximum duration of PL was extended to 16 months in 2005. The income replacement was increased from 40% to 50% in 2007. In 2010, monthly income replacement during PL and the lump-sum income replacement were combined, and currently only monthly PL income replacement payments are made.

My empirical analysis focuses on 2 policy reforms- (1) 1995 reform: an increase in replacement income during PL from 0% to 20% and after the return to work from 0% to 5% (25% in total, 25 percentage points increase, enforced beginning April 1st 1995), (2) 2001 reform: an increase in income replacement during PL from 20% to 30% and after the return to work from 5% to 10% (40% in total, 15 percentage points increase, enforced beginning January 1st 2001). As discussed previously, mothers must make the subsequent employment decision by 1 month before their expected delivery date (hence the choice depends on both the monthly and lump-sum PL income replacement payments). Therefore, I focus on the effect of the *total* amount of income replacement on mothers' subsequent employment rate.

Figure .2 is the graphical depiction of the 2 reforms and maternal income surrounding childbearing (the 1995 and 2001 reforms). After childbirth, ML income replacement covers 60% of a mother's previous income for 42 days before the birth and 56 days after the birth. Before the 1995 reform, income replacement during PL was 0%. Therefore, new mothers' incomes fell from 60% (ML income replacement) to 0% 3 months after childbirth. After the 1995 reform, the income replacement increased from 0% to 25%. After the 2001 reform, it increased by 15 percentage points to 40%. The gap between the previous income and the replacement income

during PL is the opportunity cost: the cost of childbearing. As the amount of income (actual wage plus PL income replacement) is higher for mothers who gave birth after the reform, the opportunity cost of childbearing is higher for mothers who gave birth before the reform. Note that during this period, the maximum length of and the qualifications for PL did not change. Additionally, the ML program did not change during this time; therefore I can assess the causal effect of changes in income replacement on maternal employment after childbearing. The research question is whether this extra 25 percentage points (1995 reform) and 15 percentage points (2001 reform) in income replacement incentivises mothers to return to work and increases the rate of subsequent employment after childbearing.

[Table 1 about here.]

[Figure 2 about here.]

3. Theoretical Framework

The theoretical framework is described in Figure .3 of (1). PL is a form of subsidised childcare that is provided to mothers who provide their own childcare. The PL reform's increase in the rate of income replacement will have a positive effect on maternal labour supply because it makes labour force participation more attractive. The Japanese PL program is paid for by the Employment Insurance program, and the length of PL remained fixed during both the 1995 and 2001 reforms. Therefore, the reforms do not increase labour costs. Thus the demand for female employment does not change, and the demand curve does not shift after the reforms. Taking supply (shift from S1 to S2) and demand (fixed as D1) effects into account, a small increase in maternal employment will be expected. In previous studies, labour supply responses are measured as the margin of participation (extensive margin) and hours worked (intensive margin). In this study I measure the extensive margin of labour supply responses to the reforms. If employers allow greater flexibility employee hours worked, it would be easier for individuals to adjust labour supply along the intensive margin. In Japan, the number of hours worked is not flexible for most

8

employees, especially for regular employees; therefore, individuals tend to adjust labour supply along the extensive margin.

[Figure 3 about here.]

Figure .3 of (2) is the static maternal labour supply framework. A mother will decide whether to participate in the labour market based on her market wage and her value of leisure. The opportunity cost of leisure time rises as the offered wage rate increases. During PL mothers care for a child. Therefore, PL is considered a leisure activity, and for every hour of work external childcare has to be provided. The line representing the upper budget constraint is for non-mothers, and utility is maximised when non-mothers work 'A hours'. For non-mothers, the utility of not working is less than the utility from working, and their wage is determined by the market wage that is solely determined by their real wage (w). The lower budget constraint is for mothers, and their market wage is determined based on their real wage and the cost of childcare (c)[10], therefore the budget line is (w-c) and shifts downward from the non-mother's budget line. The cost of childcare includes the actual cost of care and social norms. In this case, the utility at X, the point where no childcare needed, is above utility B; therefore mothers choose not to participate.

The mothers' budget constraint and its relationship to the PL reform are described in Figure .3 of (3). The PL reform increased the rate of income replacement and shifted the mothers' budget constraint upward (the middle budget constraint) because it increased the market wage (w-c+r), where (r) denotes the income replacement. However, depending on the cost of childcare (c) the utility at C will be lower than the utility at X. In Japan, there are fixed cost such as psychological and physical costs of using external childcare services. According to the Ministry of Health, Labour and Welfare (2010)[11], only 21.7% of children under age 3 are placed

---

[10]There are fixed cost for childcare facilities in Japan because most childcare facilities ask mothers to pay for a whole day rather than in terms of hours. For this reason, I assume childcare costs are independent of hours worked.

[11]The Ministry of Health, Labour and Welfare(2010) Report on Social Welfare Administration and Services

in external childcare facilities, demonstrating that there is still significant social pressure not to use external childcare services. It is also difficult to find external childcare facilities, especially in metropolitan areas, because there are long waiting lists for slots in these facilities, and thus some mothers have to use facilities far from their homes and workplaces. These fixed costs of using external childcare services will increase the value of leisure and increase mothers' reservation wages, thus presenting a barrier to maternal employment. By increasing the income replacement rate, the PL reforms would increase the offered wage rate and the opportunity cost of remaining at home and caring for children (substitution effects). Previous studies show that the extensive margin of the response is particularly important for low-income individuals, either because there are fixed costs of working or employers require employees to work a minimum number of hours. The details of theoretical framework for PL can be found in Boeri and vanOurs [4]. In this paper I investigate whether the 2 reforms to income replacement may reduce the opportunity cost of taking PL and remaining employed as a regular employee after childbirth; the extensive elasticity is expected to increase after the 2 reforms. In addition, I investigate how new mothers' extensive marginal responses differ by educational attainment.

Previous studies on labour supply responses to changes in wages, subsidies and tax rates find that an increase in wage/income subsidy or a decrease in the tax rate increase both the intensive and extensive margin of the labour response, however they find stronger responses on the extensive margin of labour supply[12]. A number of studies show that women's labour supply primarily responds to changes in the wage at the extensive margin rather than the intensive margin. Eissa and Liebman [9] and Eissa et al. [8] find that the US Earned Income Tax Credit(EITC) increased single mothers' extensive margin labour supply, but there were no changes in intensive margin labour supply. Card and Hyslop [5] find that a subsidy for full-time work increases the extensive margin of labour supply for single mothers, however the effects did not persist in the long run. In this study, I focus on the extensive

---

[12]See Chetty et al. [7] for more details

margin response of new mothers to the change in wages caused by an increase in the PL income replacement rate. The PL income replacement is a form of subsidy provided to new mothers. Therefore the extensive margin labour supply response by treatment mothers is expected to increase after the 1995 and 2001 reforms.

4. The Data

The data I use in this study come from the Japanese Employment Status Survey conducted in 1997 and 2002 by the Statistics Bureau of Japan. This is an administrative survey conducted on household members 15 years of age or older in approximately 440,000 households. Of the sample, 80% is available for research purposes, leaving approximately 800 thousand individuals for each year. The total number of individuals available after re-sampling is N=795,933 in 1997 and N=752,068 in 2002. Households containing more than 8 persons or with more than 3 household members with the same age are excluded from the re-sample. Sampling weights are provided to compensate for unequal selection probabilities. The survey is conducted on October 1st of each year. Age is counted in full years as of September 30.

Detailed employment history information is available in the data. Therefore, I create an individual panel data set based on the age of each newborn child, current and past employment status, tenure, quitting date and the starting dates for both current and past jobs. The children's birth dates are recorded on an annual basis, and hence a child's month of birth was unidentifiable. However, the child's age at the time of the survey as of September 30 is available, therefore it is possible to identify the timing of births and the dates new mothers were supposed to return to work because new mothers who take the PL must return on the date the child turns 1-year-old. I construct regular employment status from 3 years before the birth to 1 year after the birth of the child based on each mother's date of childbirth. For example, a mother in the 2002 data who has a child of age 0 is coded as giving birth between October 2001 and September 2002, and a mother who has a 2-year-old child is coded as giving birth between October 1999 and September 2000. Tenure information is available monthly for the 2002 data and yearly for the 1997 data.

11

The variables I use in this study are respondent age and sex, the age of newborn child, educational attainment, current job (employment status, type of contract, industry, company size, work start date, tenure), previous job (type of contract, industry, company size, work start date, quitting date and tenure). The sample used is mothers who are aged 20 to 39, are non-students and have had their first or second child (who have a 1 to 5 year-old child in the 1997 and 2001 data). Estimates are run separately for the first child and second child. The outcome variable is the mother's regular employment status, which takes a value of 1 if the mother is employed as a regular employee and 0 otherwise. Mothers who are on leave are included in the employed category.

There are 2 types of employment status in Japan, regular employment and non-regular employment. Regular employees are hired without a predetermined period of employment, work for scheduled hours, are full-time employees and are covered by social insurance programs. Conversely, non-regular employees are part-time or fixed/short term employees, are paid less and are eligible for fewer social insurance programs. I only focus on the regular maternal employment rate, as the Japanese lifetime employment and seniority based career advancement system make it difficult for workers to return as regular employees once they quit. Therefore the government designed the PL reforms to increase the mothers' regular employment rate.

Figure .4 clearly demonstrates why solely focusing on the regular employment is important. The employment to population ratio and regular-employment to population ratio are calculated from the 2002 data. The left-hand side figure is the employment to population ratio. This employment rate includes both regular and non-regular employment. The employment rate for females decreases during the childbearing years and jumps after 35. Those women who returned to the labour market often had a non-regular job. The figure on the right shows the regular employment to population ratio, and there is no jump after the childbearing years. In this paper, I examine whether the policy reform had any effect on improving mothers' regular employment rate.

Figure .5 is an example of how regular employment status changes over time. I

use women and men aged 25 to 29 and were employed as regular workers in 1997 and determine how many still had jobs 1 to 5 years later (they are aged 30 to 34 in 2002). Over the 5-year period, the male employment rate declines slightly, and approximately 7% men quit working or were fired. This trend is the same for fathers and non-fathers. However, the female employment rate decline drastically. The maternal employment rate declines to a greater extent than that of non-mothers: approximately 60% of mothers quit working, while 25% of non-mothers quit working after 5 years, strongly suggesting that childbearing is a major factor in job turnover for Japanese women.

[Figure 4 about here.]

[Figure 5 about here.]

There are 2 limitations to these data. First, wage information is only available for the survey year and was surveyed as a range of numerical values; therefore, wage information cannot be included in model. However, wages tend to be determined based on seniority, industry and company sizes in Japan. Therefore, including this information instead of wage information would reduce potential measurement error. Second, the respondent's age is recorded as 5-year range such as 20 to 24, 25 to 29, 30 to 34, or 35 to 39, but this is unlikely to cause biased estimates because I compare mothers who gave birth before and after the reform with 1-year time window. Bias resulting from these limitations is considered small.

To measure the effect of the change in the income replacement rate on new mothers' probability of continuing their jobs after childbirth, I further restrict my sample to mothers who are insured by the PL program. The employment insurance program, the financial source of PL income replacement, indicates that persons with (1) less than 1 year of continuous employment, (2) a contract that will terminate in less than a year, and (3) less than 2 days of work per week can be excluded from the PL program under labour-management agreements between employee and employer[13]. For this reason, regular workers are generally insured and qualify for

---

[13]Labor-management agreements in Japan are agreements between employees and employers. In

the PL program, and I therefore restrict my sample to mothers who were employed as regular employees 3 years before the childbirth. Those mothers will have more than 1 year of tenure before the birth and qualify to receive PL income replacement.

5. Identification Strategy

5.1. Identifying the Effects of the 2001 and 1995 Reforms

To measure the causal effect of the reforms, I use the variation in the income replacement rate created by the policy reforms as the identification strategy. I compare the change in the employment status surrounding childbirth of mothers who gave birth after the reforms, relative to control mothers who gave birth before the reforms; therefore this strategy employs difference-in-difference(DD) estimates[14]. The eligibility of mothers under the reform varies by the timing of childbirth and thus it is less likely to be correlated with potentially unobserved characteristics. Moreover, because of the timing of the policy reforms, it was very difficult for mothers to select the timing of a birth. Therefore the framework of this study represents a good random experiment.

The empirical design of this study is described in Table .2. For the 2001 reform, I use mothers who gave birth between October 2000 and September 2001 as the treatment group and compare the outcome for this group to that of control group mothers who gave birth between October 1999 and September 2000. The former group of mothers receive 40% income replacement while the later group of mothers receive 30%. To further determine the robustness of the results, I use mothers who gave birth between October 1998 and September 1999 as a second control group and compare their outcomes to those of the treatment group. The second group of control mothers only receive 25% income replacement. For the 1995 reform, I use mothers

---

contrast, collective bargaining agreements are negotiated by unions and employers at the company level. The percentage of workers who are members of labour unions is 18% and labour unions are primarily formed in companies with more than 1000 employees (50%). Companies tend to formulate rules on the basis of labour-management agreements.

[14]See Card and Krueger [6].

who gave birth between October 1995 and September 1996 as the treatment group and compare their outcomes to those of mothers who gave birth between October 1993 and September 1994. Because of the structure of the 1997 data I could not include mothers who gave birth between October 1994 and September 1995 in the estimates because these mothers could be in either the treatment or control group.

[Table 2 about here.]

Identifying the effect of the policy must satisfy 2 assumptions. One is local randomisation and the other is the common trend assumption[15]. For local randomisation, I investigate whether the birth date is random and if the characteristics of treated mothers and control mothers are identical. If mothers could change the timing of conception to be eligible for the reform, local randomisation might not be satisfied. Therefore I investigate whether mothers could change the timing of conception of a child to be eligible for the reform.

Figure .6 is the graphical presentation of the identification strategy for the 2001 policy reform. Based on the child's age in the 2002 data, I identify the timing of the birth, and based on the date of childbirth I group mothers into treatment and control groups. Year and month in the figure denote the childbirth date, the arrowed lines denote the policy amendment date and the policy enforcement date. I compare treatment mothers' subsequent employment outcomes, relative to those of control mothers that did not qualify the reform. If control mothers could change the timing of the birth based on the anticipation of the policy reform, the birth date may not be random. Mothers who gave birth after October 2000 (marked in black) qualify for the full increase in income replacement and receive 40% income replacement[16]. As average duration of a pregnancy is 10 months, to deliver a child on October 2000 or after, a mother has to become pregnant in December 1999 or after (denoted

---

[15]See Angrist and Pischke [1] for the assumptions

[16]Mothers who gave birth in October 2000 could be either the treatment or control group, although those mothers can use their unused paid holidays to delay their starting date of PL, thus they are placed in the treatment group. The average number of paid holidays is 20 days.

the pregnancy threshold in the figure). As the date that the policy was amended is May 12th, 2000, mothers who gave birth between October 1999 and September 2000 (became pregnant between December 1998 and November 1999) could not control the timing of their births to qualify for the 2001 reform. Control mothers who gave birth between October 1999 and September 2000 receive an income replacement bonus when they return to work due to the government's decision to give a 10% lump-sum payment that was previously 5%. However, it is unlikely that this lump-sum bonus could provide an incentive to return to work for this group of mothers because the subsequent employment decision has to be made at the very latest 1 month before the birth (between September 1999 and August 2000), and thus most mothers had already made the decision by the date that the policy was amended.

There is still a small possibility that mothers who gave birth between July 2000 and September 2000 (and made their subsequent employment decision at the very latest between June 2000 and August 2000) could control the timing their births. If there is a small number of mothers who were incentivised by the lump-sum bonus and changed their subsequent employment decisions the results will be biased. Therefore, I use mothers who gave birth between October 1998 and September 1999 as a second control group and compare their outcomes to those of the treatment group to further determine the robustness of the effect of the 2001 reform. This second group of control mothers could not control the timing of their births to qualify for the reform, as they had already given birth when the policy was amended.

[Figure 6 about here.]

[Figure 7 about here.]

Figure .7 graphically presents the identification strategy for the 1995 reform. For the 1995 reform, assignment to treatment and control groups differs from that of the 2001 reform because the policy was came into force in April 1995, and thus mothers who gave birth between October 1994 and September 1995 are excluded from the following estimates because they could be placed in either the treatment or control group. I use mothers who gave birth between October 1995 and September 1996

as the treatment group, and compare their outcomes to those of mothers who gave birth between October 1993 and September 1994. To deliver a baby on January 1995 or later[17], a mother had to be pregnant on March 1994 or later assuming a 10-month pregnancy (denoted pregnancy period in the figure). As the policy was amended on June 29th 1994, a mother who gave birth between October 1993 and September 1994 (became pregnant between December 1992 and November 1993) could not control the timing of her birth to qualify for the reform.

5.2. Robustness Checks

To further examine the local randomisation, I investigate whether there is any self-selection into the treatment group by comparing the frequency of births during this period. According to the vital statistics on Japan, I find no spike in the number of births around the threshold of the 2 reforms, further confirming that there is no significant self-selection into the treatment group detrimental to the comparison of treatment mothers and control group mothers.

Table .3 presents the means of the key characteristics of treatment and control mothers for the 2001 reform and the 1995 reform. If there is self-selection into the treatment group, means between treatment and control groups could be significantly different. Panel (1) presents means for the 2001 reform and panel (2) presents means for the 1995 reform. In each panel, column 1 presents the characteristics of mothers who gave birth to their first child before the reform (control); column 2 presents the characteristics of mothers who gave birth to their first child after the reform (treatment); column 3 presents t statistics for the mean difference between the control and treatment groups. Columns 4 and 5 present the characteristics of mothers who gave birth to their second child (control and treatment); column 6 presents t statistics for the difference in the means of the 2 groups. The 2 groups of mothers are almost identical in both panel (1) and panel (2). The only noticeable difference between

---

[17]Mothers who gave birth in January 1995 could be either the treatment or control group, although those mothers can use their unused paid holidays to delay their starting date of PL, thus they are qualified the 1995 reform.

the 2 groups is the proportion of working in the manufacturing and service industries for mothers who gave birth to their first child described in panel (1), and the education level for mothers who gave birth to their second child described in panel (2). The mean differences in those variables for treatment and control mothers are significant but small; therefore, I confirm that the local randomisation is satisfied. To further check the robustness of the results, I control for the effects of these key characteristics in the regression models in the following section.

[Table 3 about here.]

If there are any macroeconomics shocks detrimental to the comparison of the outcomes for the 2 groups during this period, the causal effect of the reform cannot be estimated. I use 2 methods to investigate the common trend assumption. First, I use a comparison group, a group of persons who are unaffected by the reform, both before and after, to take account for such macroeconomic shocks. Second, I run a placebo regression to determine whether there is any pre-existing trend detrimental to a comparison of the outcomes. After computing the DD estimates for mothers and the comparison group separately, the difference of those 2 DD-estimates is calculated; this is a triple difference estimate(DDD). The comparison group I use in this study is fathers who had a new child during the period under study. It would be best to use all women and all men as the comparison group. However, in this study using all women (non-mothers) or all men (non-fathers) as the comparison group was not possible because the sample was constructed from retrospective information based on child birth dates and past and current employment status in the cross-sectional data [18]. For those who did not have a new child during the period under study, assignment to the treatment or control group is not possible. A separate panel data set for fathers was created following the same procedure as that of the mothers: based on the age of each newborn child, current and past employment status, tenure, quitting date and the starting date for both current and past jobs

---

[18]The method used to construct the retrospective estimation sample is described in the data section above.

are identified. To determine the appropriateness of using fathers as the comparison group, I compare means of fathers and non-fathers to determine whether there is any significant difference between the groups, and I find no significant difference between fathers and non-fathers. In the following section, I first show the results of 2001 reform followed by the results from the 1995 reform. Because the employment history information is monthly in the 2002 data and yearly in the 1997 data, the potential measurement error may be smaller in the 2001 reform results.

6. Results

6.1. Graphical Comparison of the Treatment and Control Groups

Panels (1) and (2) of Figure .8 present a graphical comparison of the average regular employment rate for treatment and control mothers for the 2001 reform. This is the risk of job turnover conditional on having a regular job 3 years before the birth of a child. The employment rate 2 years before a mother's first birth is 85.7% for control mothers and 86.4% for treatment mothers (panel (1)). The employment rate 1 year after the first birth is 29.7% for control mothers and 29.6% for treatment mothers. Taking the difference in the employment rates between 2 years before birth and 1 year after birth, control mothers' employment rate decreased by 55.9 percentage points while the treatment mothers' employment rate decreased by 56.8 percentage points. The difference between the treatment and control groups does not seem to be significantly large. The difference in the average employment rate 1 year after the birth is also small for mothers who gave birth to their second child (panel (2)). Panels (3) and (4) of Figure .8 presents the regular employment rate for fathers who had a first or a second child. The difference in rates from 2 years before the birth and 1 year after the birth for fathers is also small.

For the 1995 reform, the treatment group is mothers who gave birth between October 1995 and September 1996 and the control group is mothers who gave birth between October 1993 and September 1994. Panels (1) and (2) of Figure .9 are graphical comparisons of the average regular employment rates for treatment and control mothers between 2 years before and 1 year after their first and second births.

This is not directly comparable to the results for the 2001 reform because the employment history variable in the 1997 data is yearly, not monthly, although the results are the quite similar to those for the 2001 reform. Around their first childbirth, 78.6% of control mothers are employed as regular employees while 78.1% treatment mothers are regularly employed (panel (1)). The rate drops to 20.4% for control mothers and 22.2% for treatment mothers 1 year after the birth. The difference in the rate before and after the birth is 58.2 percentage points for control mothers and 55.9 percentage points for treatment mothers. Around their second childbirth, the difference in the rate of regular employment before and after is 19.8 percentage points for control mothers and 16.3 percentage points for treatment mothers. Thus it seems that treatment mothers are slightly more likely to return as regular employees 1 year after a birth compared to control mothers. Panels (3) and (4) of Figure .9 present the regular employment rate for fathers. The differences in regular employment rates before and after the birth of the first child are 2.9 percentage points for control fathers and 0.9 percentage points for treatment fathers (panel (3)), those for the second birth are 3.1 percentage points for control fathers and 0.9 percentage points for treatment fathers (panel (4)), and the differences are small.

Figure .10 is a graphical comparison of the maternal regular employment rate by education level around the 2001 reform. Because the maximum wage rate for income replacement is 430,200 yen per month and the minimum wage rate is 69,900 yen per month, the reform would provide a stronger incentive to mothers who have lower incomes. Because the wage information could not be used in the estimates (the wage is surveyed in terms of categories of numerical values and only cover the survey year) and education is a strong determinant of wages in Japan, the rates are compared by the education level. Panels (1) and (2) present the maternal employment rates for college and university graduates and panel (3) and (4) are those rates for high-school graduates. Regarding the birth of the first child, the difference in employment rates before and after a birth is 50.1 percentage points for control mothers and 52.5 percentage points for treatment mothers who graduated college/university, and 64.0 percentage points for control mothers and 62.4 percentage points for treatment

mothers who graduated high-school. The means are higher for college/university graduate mothers, but the differences between the changes for treatment and control mothers are small. The difference between these 2 groups is similar when I compare the second control group and the treatment group.

Figure .11 presents a graphical comparison between the 2 groups by education level around the 1995 reform. The trends are similar to those of the 2001 reform, and mothers who graduated from college/university are more likely to remain employed than high-school graduate mothers. However, the difference between control mothers and treatment mothers is small. In the following section, I estimate the standard error of this difference in the outcomes of the 2 groups to investigate the significance of the difference.

[Figure 8 about here.]

[Figure 9 about here.]

[Figure 10 about here.]

[Figure 11 about here.]

6.2. *Basic Difference in Difference and Triple Difference Results*

To determine the significance of the difference in average outcomes between the treatment and control groups, DD estimates are conducted. Labour market participation is defined as working as a regular employee. The basic results for the 2001 reform are presented in Table .4. The upper panel (1) provides the results for mothers and the lower panel (2) those of fathers. Columns 1 through 3 provide the results for the birth of the first child and columns 4 through 6 those of the birth of the second child. Columns 1 and 4 present the average participation rate 2 years before the birth of a first and second child; columns 2 and 5 presents the average participation rate 1 year after the birth of the first child and second child; column 3 and 6 present the mean differences between before and after the childbirth. In each row, the average regular employment rate, standard errors (in parentheses), sample

21

sizes (in square brackets) are presented. Rows 1 and 5 present the outcomes for the treatment group; rows 2 and 6 present the outcomes for the control group; rows 3 and 7 present the difference in outcomes between the treatment and control groups; rows 4 and 8 present difference-in-difference estimates. Regarding the difference in the rates between 2 years before a birth and 1 year after a birth, there is a 56.8 percentage points decline in the employment rate for treatment mothers compared to a 55.9 percentage points decline in rate for control mothers. Thus, there is a 0.86 percentage points relative decline in the employment rate for treatment mothers with a standard error of 2.48%, and the difference is not significantly different from 0. This is a DD estimate of the 2001 reform impact. The result is similar for mothers who had their second child as shown in the right hand side of the panel, while there is a 21.5 percentage points decline in employment for the treatment mothers, there is a corresponding 22.4 percentage points decline for control mothers. Thus there is a 0.87 percentage points relative increase in the employment rate for treatment mothers, although the standard error is 3.18% and hence the difference is also not significantly different from 0.

If there are macroeconomic shocks or time trends in the labour market during this period, this basic estimate does not identify the causal impact of the reform. Therefore I include fathers as a comparison group. The results are shown in panel (2) of Table .4. There is a 0.34 percentage points relative increase in employment with a standard error of 0.88% for fathers who had their first child and a 0.95 percentage points relative decrease in employment with standard error of 0.76% for fathers who had their second child, therefore the differences are not significantly different from 0. Taking the difference between the mothers' and the fathers' DD estimates, the triple difference estimate in the bottom row will identify the causal effect of the treatment after accounting for the effect of macroeconomic shocks in the labour market. For mothers who gave birth to their first child, there is a 1.20 percentage points decline in the relative employment rate for treatment mothers, although the standard error is 2.63% and the difference is not significantly different from 0. For mothers who gave birth to their second child, there is a 1.83 percentage points relative increase in

employment compared to mothers and fathers who unaffected by the reform, however the standard error is 3.27% therefore the difference is not significantly different from 0. The basic DD and DDD estimates show that the 15 percentage points increase in the income replacement rate in 2001 does not have an effect on mothers' relative employment response, compared to those persons unaffected by the reform.

[Table 4 about here.]

6.3. Regression Results

The sampling variance of the basic DD and DDD estimates can be reduced by running a regression model and controlling for other covariates that potentially affect the employment response. The regression estimates will reduce bias arising from potential differences between the treatment and control groups and will provide more precise estimates. I run a linear probability model(LPM) and a probit model to estimate the effects of the reforms. By running 2 models, I can reduce errors resulting from my choice of model.

LPM for difference-in-difference estimates for mothers:

$$E_{itb} = \beta_0 + X_{itb}\beta_1 + \beta_2 After_{ib} + \beta_3 Reform_{it} + \beta_4 After_{ib} * Reform_{it} + u_{itb} \quad (1)$$

Probit model for difference-in-difference estimates for mothers:

$$P(E_{itb} = 1) = \phi(\beta_0 + X_{itb}\beta_1 + \beta_2 After_{ib} + \beta_3 Reform_{it} + \beta_4 After_{ib} * Reform_{it}) \quad (2)$$

where E_{itb} is a dummy variable that takes a value of 1 if a mother is working as a regular employee and 0 otherwise, X_{itb} denotes the set of covariates: education level, job tenure, the size of the company and industry. These covariates are measured as pre-birth (3 years before the birth) characteristics and control for observable characteristics that will affect the employment response. The parameter $After_{ib}$ takes a value of 1 for 1 year after the birth and represents the time fixed effect; $Reform_{it}$ is the group fixed effect and takes a value of 1 for treatment mothers. The second level interaction captures all variation in the employment of treatment mothers 1 year after childbirth. This is the impact of the reform. To correct for

heteroskedasticity, robust standard errors are used in the LPM model (1). ϕ in the model (2) is the standard normal density.

For the DDD estimate, I include both mothers' and fathers' samples and estimate the following LPM:

$$E_{itbs} = \beta_0 + X_{itbs}\beta_1 + \beta_2 After_{ibs} + \beta_3 Reform_{its} + \beta_4 Mother_{itb}$$

$$+\beta_5 After_{ibs} * Reform_{its} + \beta_6 After_{ibs} * Mother_{itb} + \beta_7 Reform_{its} * Mother_{itb}$$

$$+\beta_8 Reform_{its} * After_{ibs} * Mother_{itb} + u_{itbs} \quad (3)$$

Probit model for mothers and fathers:

$$P(E_{itbs} = 1) = \phi(\beta_0 + X_{itbs}\beta_1 + \beta_2 After_{ibs} + \beta_3 Reform_{its} + \beta_4 Mother_{itb}$$

$$+\beta_5 After_{ibs} * Reform_{its} + \beta_6 After_{ibs} * Mother_{itb} + \beta_7 Reform_{its} * Mother_{itb}$$

$$+\beta_8 Reform_{its} * After_{ibs} * Mother_{itb}) \quad (4)$$

where β_2 is the time fixed effect, β_3 is the group fixed effect, and β_4 is the eligibility (mothers=1) fixed effect. The fixed effects control for the time series changes in employment status (β_2), controls for the time-invariant characteristics of the treatment group (β_3), and controls for the time-invariant characteristics of the eligible group (β_4). The second level interactions control for changes over time in the treatment group (β_5), changes over time for mothers in all groups (β_6), and time-invariant characteristics of the mothers in the treatment group (β_7). The third level interaction captures all variation in the employment of mothers in the treatment group 1 year after childbirth: this is the impact of the reform after controlling for the macroeconomic shocks.

Table .5 presents the results from the regressions of the DD and DDD models for the 2001 reform. The coefficients from the LPM and the marginal effects from the probit model shown in the table are from DD and DDD terms in each model. Covariates are included in the model and the coefficients in models without covariates are equivalent to the basic DD and DDD estimates in Table .4. The difference in coefficients between the basic and regression DD and DDD estimates are due to the restricted sample in the regression estimates. Columns 1, 2, 5 and 6 present estimates from a comparison between treatment mothers (gave birth to their children

between October 2000 and September 2001) and control mothers (gave birth to their children between October 1999 and September 2000); columns 3, 4, 7 and 8 present estimates from a comparison between treatment mothers and the second group of control mothers (gave birth to their children between October 1998 and September 1999). The results reveal that there is no significant difference in the average change in the employment rates between the 2 groups even after controlling for the factors that affect the employment response (See the models with covariates). The signs and significances of the other covariates are as expected. The larger the company is, the more likely mothers are to remain employed; the longer the tenure, the more likely mothers are to return. The manufacturing industry has a higher employment rate than the service industry.

To further determine the policy effects, the extensive margin elasticity is calculated from the results of DD estimates and the changes in mothers' financial gains [19]. From Table .5, the percentage change in the employment rate after the birth of the first child for treatment mothers is 0.55% with a standard error of 2.32%. The employment rate for treatment mothers 2 years before their first childbirth is 86.38% with a standard error of 1.21%. Thus, there is a 0.63% change in the regular employment rate before and after childbirth for the treatment group. The rate of income replacement is 30% for control mothers and 40% for treatment mothers, therefore there is a 10%/30% = 33.3% increase in the rate, meaning that the estimated elasticity is 0.019 for their first child, therefore their employment response to an increase in income replacement is inelastic. For their second child, there is a 1.03% change (0.71%/69.13%) in the regular employment rate before and after childbirth for the treatment group, thus the estimated elasticity is 0.031. The estimated elasticity is higher when I compare the treatment mothers and the second group of control mothers; the rate of income replacement for treatment mothers is 40% and is 25% for the second group of control mothers, thus there is 60% increase in the rate of income replacement. The percentage change in the employment rate after a

---

[19]See Chetty et al. [7] for a meta analysis of the extensive margin elasticities.

first childbirth for treatment mothers is 2.17% with a standard error of 2.33%, thus there is a -2.51% change in the regular employment rate before and after childbirth for the treatment group. The estimated elasticity is 0.042 for the first child and 0.100 for the second child; therefore the employment response is also inelastic.

The results for the 1995 reform are shown in Table .6. The results are similar to those of the 2001 reform and show no significant difference between treatment and control groups in the probability of continued regular employment. From Table .6, the parentage change in the employment rate after the birth of the first child for treatment mothers is 1.79% with a standard error of 2.04%, and the employment rate for treatment mothers 2 years before their first childbirth is 78.07% thus there is a 2.29% change. The rate of income replacement for control mothers is 0% and that of the treatment mothers is 25%; therefore there is 25 percentage points increase in the employment rate and the elasticity is 0.092 for the first child[20]. For the second child, the percentage change in the employment rate after the birth of the second child for treatment mothers is 2.70% with a standard error of 2.43%, thus the elasticity is 0.174. I find no significant increase in the employment rate for treatment mothers after the 1995 reform relative to that of control mothers.

[Table 5 about here.]

[Table 6 about here.]

6.4. Robustness Check

To determine the existence of a pre-time trend, I obtain placebo DD and DDD estimates by using different 2 groups of mothers who gave birth to their children prior to the reforms. For the 2001 reform, the second group of control mothers used in the above estimates (who gave birth between October 1998 and September 1999) and the other group of mothers who gave birth before the reform (between October 1997 and September 1998) are used in the placebo estimates. I also use

---

[20]Because the income replacement rate changes from 0% to 25%, I use the percentage points change instead of the % change as the denominator.

mothers who gave birth between October 1996 and September 1997 and compare the result to that for mothers who gave birth between October 1997 and September 1998 to further determine the existence of a pre-existing trend. The results from the placebo DD and DDD estimates for the 2001 reform are shown in Table .7. All of the models show no significant differences in the relative outcomes of treatment mothers, and the magnitudes of the coefficients are very small, thus there seem to be no significant pre-existing time trends that would be detrimental to a comparison of the treatment and control groups for the 2001 reform. The placebo regression in Table .8 also shows no significant pre-existing time trends for the 1995 reform.

[Table 7 about here.]

[Table 8 about here.]

[Table 9 about here.]

[Table 10 about here.]

Table .9 presents DD and DDD estimates by education level. As there is a cap for income replacement, mothers with low education levels will be less disincentivised to return to their jobs after having children. The results show that the magnitude of the estimated difference in the employment responses of treatment and control group mothers is small, and thus this difference is not significant for mothers at any education level. No significant differences are found among the estimates for the 1995 reform by education level in Table .10. However, in contrast to the expected effects, the magnitudes of the coefficients are larger for college and university graduates for mothers who had their first child for the 1995 and 2001 reform and for mothers who had their second child for the 1995 reform.

These tests suggest that the probability of continuing regular employment after childbirth conditional on being employed as a regular employee 3 years before a birth is not significantly different between treatment mothers and those that did not qualify for the reform. The results are similar for mothers who gave birth to their first child and those who gave birth to their second child. The results do not

change even after controlling for the effect of macroeconomic shocks by running a DDD regression with fathers as the comparison group. The results from the placebo regression also confirm that there is no pre-existing trends to harm the comparison of 2 groups. I also find similar results for mothers at all education levels. I conclude that the increase in the PL income replacement rate from 0% to 25% in 1995 and from 25% to 40% in 2001 have no effect on the regular employment rate for mothers who qualified for the reform.

7. Conclusion

This study assesses the impact of changes in the PL income replacement rate on new mothers' regular employment status after having children. The Japanese government increased the PL income replacement rate from 0% to 25% in 1995 and from 25% to 40% in 2001. I identify the causal effects of these reforms by comparing the regular employment status of mothers who gave birth to their first or second child before and after each reform. The treatment and control groups are randomly assigned based on the date of the birth of their new children, which cannot be perfectly controlled by mothers. Because the government implemented the reform shortly after the date that the policy was amended, mothers were unable to anticipate the implementation date of the reform and were unable to control the timing of childbirth to qualify for the reform. Therefore the framework of this study is as good as a natural experiment. The outcome variable is being employed as a regular employee, which takes a value of 1 if mothers are employed as regular employees and 0 otherwise. I measure the difference in the maternal regular employment rate before and after childbirth between treatment and control group mothers, and hence this is a DD estimates. To control for macroeconomic shocks during this period, I include fathers who had their first or second child as a comparison group and take difference in relative outcomes between mothers and fathers; this is a DDD estimate. Based on the results from the DD and DDD estimates, I find that the 2001 and 1995 reforms both have no significant impact on probability of regular employment after childbirth for treatment mothers, that is, there is no significant difference in the rel-

ative outcomes of treatment mothers and those who did not qualify for the reform. The extensive margin elasticity is 0.019 for the first child and 0.031 for their second child for the 2001 reform, and 0.092 for the first child and 0.174 for the second child for the 1995 reform, thus the labour supply response of new mothers to the increase in income replacement is inelastic.

For future analysis, I propose examining the intensive margin of the labour response to the 1995 and 2001 reforms. In addition, the influence of the subsequent 2007 and 2010 reforms on the extensive and intensive margin labour supply responses should be investigated. The opportunity cost for employed women to have a child remains high in Japan. Only 24.5% of workplaces count leave periods as worked period; thus, they are not included in the calculation of seasonal salary increases. Additionally, mothers in most firms are at a disadvantage for promotions if they are absent to give birth. Their severance pay is also affected because 36.3% of workplaces indicate that employees who take leaves receive reduced severance compensation[21]. There are fixed costs in the use of childcare facilities, such as actual costs, psychological costs and distance costs. It is difficult to find slots in public facilities and those mothers who were unable to find a slot have to pay for private childcare facilities that are expensive compared to public ones. The results in this study could be caused by the high fixed cost of childcare in Japan. Therefore, the policy effects of reducing the fixed costs of childcare on new mothers' labour supply responses should be reviewed in future analyses.

References

[1] Angrist, J., Pischke, J., 2008. Mostly Harmless Econometrics: An Empiricist's Companion. Princeton University Press.

[2] Baker, M., Milligan, K., 2008. How does job-protected maternity leave affect mothers' employment? Journal of Labor Economics 26 (4), 655–691.

---

[21]The Ministry of Health, Labour and Welfare (2007) Basic Survey of Gender Equality in Employment Management.

[3] Baum, C. L., 2003. The effect of state maternity leave legislation and the 1993 family and medical leave act on employment and wages. Labour Economics 10 (5), 573–596.

[4] Boeri, T., vanOurs, J., 2008. Family policies. In: Boeri, T., vanOurs, J. (Eds.), The Economics of Imperfect Labor Markets. Princeton University Press, Ch. 7.

[5] Card, D., Hyslop, D. R., November 2005. Estimating the effects of a time-limited earnings subsidy for welfare-leavers. Econometrica 73 (6), 1723–1770.

[6] Card, D., Krueger, A. B., September 1994. Minimum wages and employment: A case study of the fast-food industry in new jersey and pennsylvania. American Economic Review 84 (4), 772–93.

[7] Chetty, R., Guren, A., Manoli, D., Weber, A., June 2012. Does Indivisible Labor Explain the Difference Between Micro and Macro Elasticities? A Meta-analysis of Extensive Margin Elasticities. University of Chicago Press.

[8] Eissa, N., Kleven, H. J., Kreiner, C. T., April 2008. Evaluation of four tax reforms in the united states: Labor supply and welfare effects for single mothers. Journal of Public Economics 92 (3-4), 795–816.

[9] Eissa, N., Liebman, J. B., May 1996. Labor supply response to the earned income tax credit. The Quarterly Journal of Economics 111 (2), 605–37.

[10] Gruber, J., June 1994. The incidence of mandated maternity benefits. American Economic Review 84 (3), 622–41.

[11] Hanratty, M., Trzcinski, E., 2009. Who benefits from paid family leave? impact of expansions in canadian paid family leave on maternal employment and transfer income. Journal of Population Economics 22, 693–711.

[12] Klerman, A., Leibowitz, A., 1997. Labor supply effects of state maternity leave legislation. In: Blau, F., Ehrenberg, R. (Eds.), Gender and Family Issues in the Workplace. Russell Sage Foundation, Ch. 3.

[13] Lalive, R., Zweimuller, J., 2009. How does parental leave affect fertility and return to work? evidence from two natural experiments. The Quarterly Journal of Economics 124 (3), 1363–1402.

[14] Ruhm, C., February 1998. The economic consequences of parental leave mandates: Lessons from europe. The Quarterly Journal of Economics 113 (1), 285–317.

[15] Ruhm, C., Teague, J., 1997. Parental leave policies in europe and north america. In: Blau, F., Ehrenberg, R. (Eds.), Gender and Family Issues in the Workplace. Russell Sage Foundation, Ch. 5.

[16] Schonberg, U., Ludsteck, J., Mar 2007. Maternity leave legislation, female labor supply, and the family wage gap. IZA Discussion Papers 2699, Institute for the Study of Labor (IZA).

[17] Waldfogel, J., Higuchi, Y., Abe, M., 1999. Family leave policies and women's retention after childbirth: Evidence from the united states, britain, and japan. Journal of Population Economics 12 (4), 523–545.
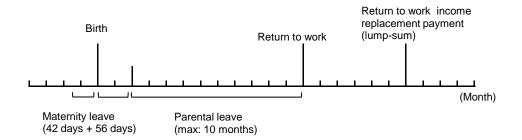
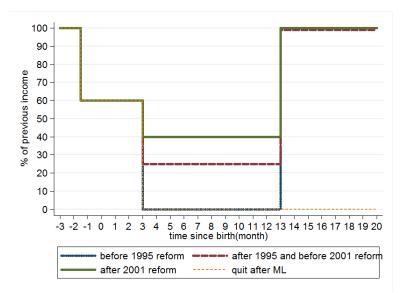Figure .1: Time-line of a Mother's Childbirth, Maternity Leave and Parental Leave

Figure .2: The PL Income Replacement Rates Before and After the 1995 and 2001 Reforms

Note: The Rates of PL income replacement are described as the total amount; monthly and lump-sum PL income replacement payment are combined.
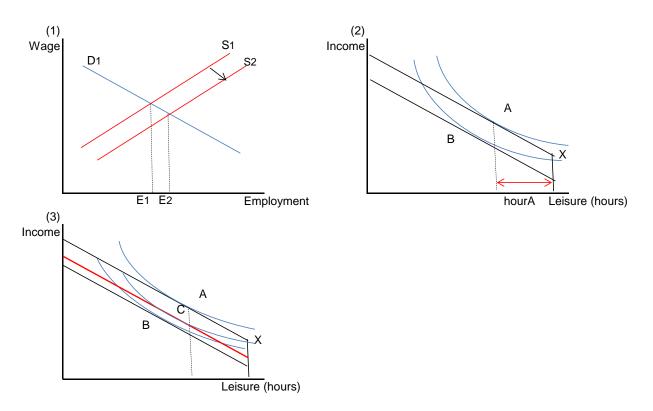
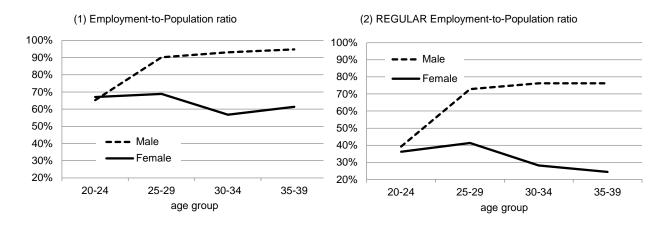Figure .3: Mothers' Labour Supply and Cost of Childbearing

Figure .4: Employment and REGULAR Employment Rates by Age and Sex Group

Note: Rates are calculated from the 2002 data. Employment-to-Population rate is the percentage of women and men who are employed, while the REGULAR employment-to-population rate is the percentage of women and men who are employed as regular employees. Means are weighted with the sampling weights.
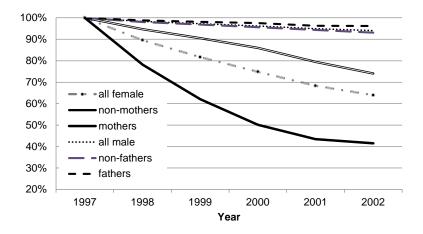
Figure .5: Job Turnover Hazard for Women and Men Aged 25 to 29 and were Employed as Regular Workers in 1997

Note: Data are from the 2002 Japanese Employment Status Survey. Regular employment rates from 1997 to 2002 are calculated for women and men aged 25 to 29 and were employed as regular workers in 1997. Means are weighted with the sampling weights.
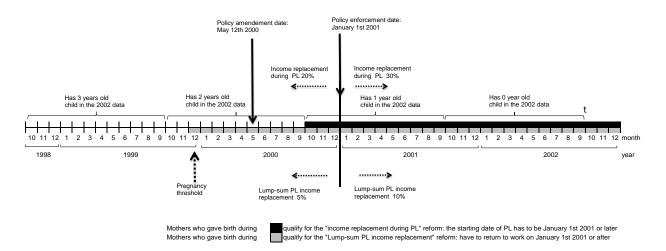

Figure .6: Identification Strategy and the Data Structure of the 2001 Reform

Note: Data are from the 2002 Japanese Employment Status Survey. Based on the child's age in the 2002 data, the date of childbirth (numbers under the horizontal line) is identified. The policy was amended in May 2000 and enacted in January 2001.
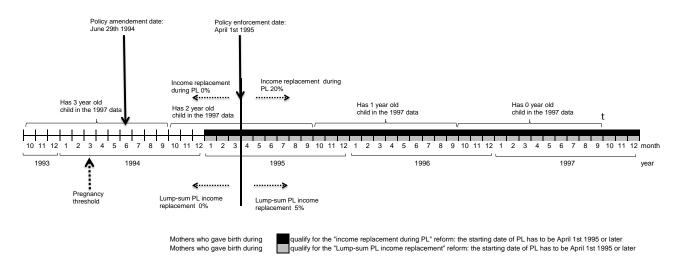
Figure .7: Identification Strategy and the Data Structure of the 1995 Reform

Note: Data are from the 1997 Japanese Employment Status Survey. Based on the child's age in the 1997 data, the date of childbirth (numbers under the horizontal line) is identified. The policy was amended in June 1994 and enacted in April 1995.
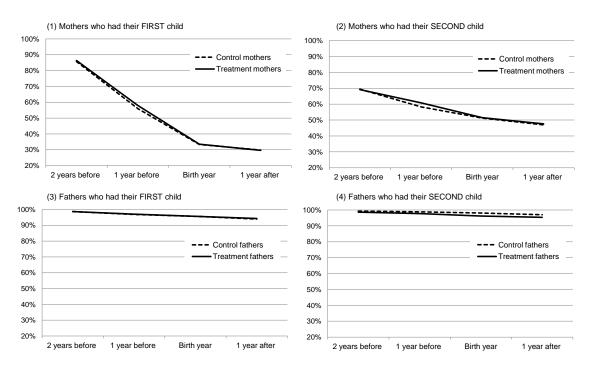

Figure .8: Regular Employment Rates for Treatment and Control Groups Before and After the Birth: the 2001 Reform

Note: Data are from the 2002 Japanese Employment Status Survey. Regular employment rates are calculated for mothers and fathers who were employed as regular employees 3 years before the childbirth. Treatment group: who gave birth and fathers who had a newborn child between October 2000 and September 2001, Control group: who gave birth and fathers who had a newborn child between October 1999 and September 2000. Means are weighted with the sampling weights.
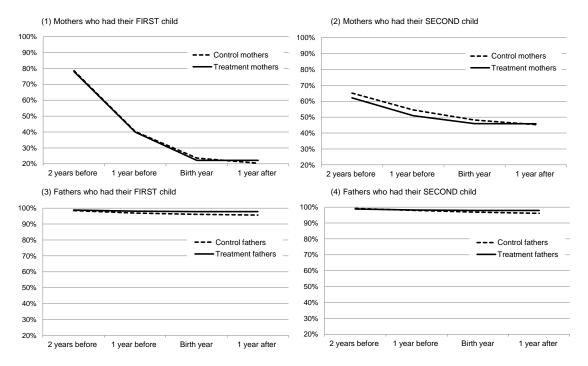
Figure .9: Regular Employment Rates for Treatment and Control Groups Before and After Childbirth: the 1995 Reform

Note: Data are from the 1997 Japanese Employment Status Survey. Regular employment rates are calculated for mothers and fathers who were employed as regular employees 3 years before the childbirth. Treatment group: who gave birth and fathers who had a newborn child between October 1995 and September 1996, Control group: who gave birth and fathers who had a newborn child between October 1993 and September 1994. Means are weighted with the sampling weights.
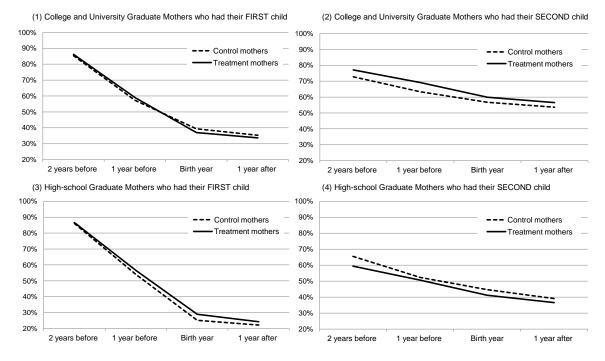
(1) College and University Graduate Mothers who had their FIRST child

(2) College and University Graduate Mothers who had their SECOND child

(3) High-school Graduate Mothers who had their FIRST child

(4) High-school Graduate Mothers who had their SECOND child

Figure .10: Regular Employment Rate for Treatment and Control Mothers Before and After the Birth by Education Level: the 2001 Reform

Note: Data are from the 2002 Japanese Employment Status Survey. Regular employment rates are calculated for mothers who were employed as regular employees 3 years before the childbirth. The upper panels are for mothers who attained college or university degree, and the lower panels are for mothers who graduated high-school or junior high-school. Treatment group: who gave birth between October 2000 and September 2001, Control group: who gave birth between October 1999 and September 2000. Means are weighted with the sampling weights.
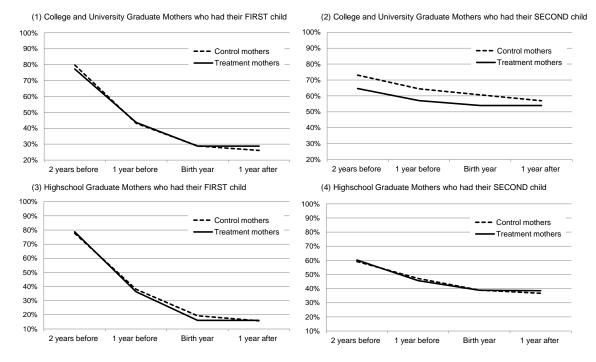
Figure .11: Regular Employment Rate for Treatment and Control Mothers Before and After Childbirth by Education Level: the 1995 Reform

Note: Data are from the 1997 Japanese Employment Status Survey. Regular employment rates are calculated for mothers who were employed as regular employees 3 years before the childbirth. Treatment group: who gave birth between October 1995 and September 1996, Control group: who gave birth between October 1993 and September 1994. The upper panels are for mothers who attained college or university degree, and the lower panels are for mothers who graduated high-school or junior high-school. Means are weighted with the sampling weights.

Policy amendment date	Policy enforcement date	Rate of income replacement			Eligibility	Maximum length
		During PL	Lump-sum	Total		
May 15, 1991	April 1, 1992	None	None	0%	Employees in more than 30 regular employees	10 months
June 29, 1994	April 1, 1995	20%	5%	25%	Only regular employees	10 months
May 12, 2000	January 1, 2001	30%	10%	40%	Only regular employees	10 months
December 8, 2004	April 1, 2005	30%	10%	40%	Regular and non-regular employees	16 months
April 23, 2007	April 1, 2007	30%	20%	50%	Regular and non-regular employees	16 months
March 30, 2009	April 1, 2010	50%	0%	50%	Regular and non-regular employees	16 months

Table .1: List of the Parental Leave Reforms

Date of childbirth	Rate of income replacement		
	During PL	Lump-sum	TOTAL
(1) 2001 reform			
October 1998-September 1999 birth	20%	5%	25%
October 1999-September 2000 birth	20%	10%	30%
October 2000-September 2001 birth	30%	10%	40%
(2) 1995 reform			
October 1993-September 1994 birth	0%	0%	0%
October 1995-September 1996 birth	20%	5%	25%

Table .2: Empirical Design

(1) 2001 reform

Mothers	First child			Second child		
	Control	Treat	t	Control	Treat	t
Education : University/college graduates	0.579	0.569	0.18	0.542	0.547	0.02
: Highschool graduates	0.421	0.431	0.18	0.458	0.453	0.02
Pre-birth characteristics(3 years before)						
Tenure(years)	4.167	4.192	0.03	5.498	5.384	0.19
Company size : Less than 30 employees	0.189	0.209	1.00	0.204	0.198	0.05
: 30-299 employees	0.303	0.299	0.04	0.264	0.257	0.04
: More than 300 employees	0.405	0.398	0.09	0.343	0.369	0.53
: Public office	0.102	0.095	0.26	0.189	0.176	0.22
Industory : Manufacturing	0.264	0.229	2.67	0.227	0.273	1.84
: Services	0.736	0.771	2.67	0.773	0.727	1.84
Sample size	1393	1298		623	637	

(2) 1995 reform

Mothers	First child			Second child		
	Control	Treat	t	Control	Treat	t
Education : University/college graduates	0.452	0.482	1.96	0.427	0.479	2.49
: Highschool graduates	0.548	0.518	1.96	0.573	0.521	2.49
Pre-birth characteristics(3 years before)						
Tenure(years)	3.299	3.472	1.90	4.372	4.608	1.02
Company size : Less than 30 employees	0.247	0.252	0.06	0.270	0.275	0.04
: 30-299 employees	0.278	0.273	0.07	0.257	0.218	2.08
: More than 300 employees	0.392	0.396	0.02	0.306	0.317	0.11
: Public office	0.082	0.079	0.08	0.167	0.189	0.82
Industory : Manufacturing	0.251	0.232	1.13	0.263	0.256	0.05
: Services	0.749	0.768	1.13	0.737	0.744	0.05
Sample size	1809	1634		741	650	

Table .3: Means of Key Characteristics for the 2001 Reform(Upper Panel) and the 1995 Reform(Lower Panel)

Note: Data are from the 1997 and 2002 Japanese Employment Status Survey. The 2001 reform: the treatment group is mothers who gave birth between October 2000 and September 2001, and the control group is mothers who gave birth between October 1999 and September 2000. The 1995 reform: the treatment group is mothers who gave birth between October 1995 and September 1996, and the control group is mothers who gave birth between October 1993 and September 1994. t is the test of equality of means between 2 groups. Means are weighted with the sampling weights.

2001 reform	First Child			Second Child		
Group/ Year	Before the birth	After the birth	Difference (after-before)	Before the birth	After the birth	Difference (after-before)
(1) Mothers						
Treatment group	0.8638	0.2958	-0.5680	0.6913	0.4759	-0.2154
	(0.0121)	(0.0171)	(0.0181)	(0.0246)	(0.0260)	(0.0242)
	[1314]			[643]		
Control group	0.8568	0.2974	-0.5594	0.6942	0.4700	-0.2242
	(0.0122)	(0.0153)	(0.0169)	(0.0245)	(0.0251)	(0.0207)
	[1411]			[632]		
Difference(treatment-control):	0.0070	-0.0016		-0.0029	0.0059	
	(0.0172)	(0.0230)		(0.0347)	(0.0361)	
Difference-in-difference:	**-0.0086**			**0.0087**		
	(0.0248)			**(0.0318)**		
(2) Fathers						
Treatment group	0.9868	0.9432	-0.0436	0.9863	0.9534	-0.0329
	(0.0029)	(0.0064)	(0.0058)	(0.0037)	(0.0072)	(0.0062)
	[2065]			[1754]		
Control group	0.9866	0.9396	-0.0471	0.9932	0.9698	-0.0233
	(0.0035)	(0.0074)	(0.0067)	(0.0020)	(0.0048)	(0.0043)
	[2102]			[1695]		
Difference(treatment-control):	0.0002	0.0036		-0.0069	-0.0165	
	(0.0045)	(0.0098)		(0.0042)	(0.0086)	
Difference-in-difference:	**0.0034**			**-0.0095**		
	(0.0088)			**(0.0076)**		
Triple difference:	**-0.0120**			**0.0183**		
	(0.0263)			**(0.0327)**		

Table .4: DDD Estimate of the Effect of the 2001 Reform on Regular Employment Rate One Year After Childbirth

Note: Data are from the 2002 Japanese Employment Status Survey. The outcome variable is the regular employment status, which takes a value of 1 if the mother worked as a regular employee and 0 otherwise. Treatment group: who gave birth between October 2000 and September 2001, Control group: who gave birth between October 1999 and September 2000. Before the birth denotes 2 years before childbirth. After the birth denotes 1 year after childbirth. Standard errors are in parentheses. Sample sizes are in square brackets. Means are weighted with the sampling weights.

	First child				Second child			
	Treatment group VS. Control group		Treatment group VS. Second control group		Treatment group VS. Control group		Treatment group VS. Second control group	
2001 reform	LPM	Probit	LPM	Probit	LPM	Probit	LPM	Probit
(1) Difference-in-Difference								
without covariates	-0.0106	-0.0056	-0.0235	-0.0234	0.0080	0.0099	-0.0469	-0.0472
	(0.0250)	(0.0234)	(0.0247)	(0.0247)	(0.0321)	(0.0307)	(0.0317)	(0.0321)
with covariates	-0.0106	-0.0055	-0.0224	-0.0217	0.0080	0.0071	-0.0374	-0.0413
	(0.0250)	(0.0232)	(0.0234)	(0.0233)	(0.0321)	(0.0304)	(0.0308)	(0.0308)
	[2691]		[2722]		[1260]		[1193]	
(2)Triple Difference								
without covariates	-0.0141	-0.0141	-0.0168	-0.0168	0.0185	0.0185	-0.0393	-0.0393
	(0.0270)	(0.0270)	(0.0260)	(0.0260)	(0.0330)	(0.0330)	(0.0326)	(0.0326)
with covariates	-0.0133	-0.0075	-0.0143	-0.0128	0.0231	0.0177	-0.0067	-0.0391
	(0.0245)	(0.0255)	(0.0256)	(0.0248)	(0.0192)	(0.0325)	(0.0191)	(0.0326)
	[6806]		[6793]		[4678]		[4395]	

Table .5: Regression DD and DDD Estimates of the Effect of the 2001 Reform on Regular Employment After Childbirth

Note: Data are from the 2002 Japanese Employment Status Survey. The outcome variable is the regular employment status, which takes a value of 1 if the mother or father worked as a regular employee and 0 otherwise. Estimates are coefficients from the linear probability model(LPM) and marginal effects from probit model. Treatment group: who gave birth between October 2000 and September 2001, Control group: who gave birth between October 1999 and September 2000, Second control group: who gave birth between October 1998 and September 1999. Standard errors are in parentheses. Sample sizes are in square brackets. Means are weighted with the sampling weights.

1995 reform	First child		Second child	
	LPM	Probit	LPM	Probit
(1) Difference-in-Difference				
without covariates	0.0230	0.0230	0.0370	0.0372
	(0.0215)	(0.0214)	(0.0253)	(0.0255)
with covariates	0.0182	0.0179	0.0277	0.0270
	(0.0205)	(0.0204)	(0.0245)	(0.0243)
	[3443]		[1391]	
(2)Triple Difference				
without covariates	0.0037	0.0037	0.0147	0.0147
	(0.0220)	(0.0220)	(0.0257)	(0.0257)
with covariates	0.0051	-0.0026	0.0137	0.0022
	(0.0217)	(0.0210)	(0.0253)	(0.0234)
	[8073]		[5149]	

Table .6: Regression DD and DDD Estimates of the Effect of the 1995 Reform on Regular Employment After Childbirth

Note: Data are from the 1997 Japanese Employment Status Survey. The outcome variable is the regular employment status, which takes a value of 1 if the mother or father worked as a regular employee and 0 otherwise. Estimates are coefficients from the linear probability model(LPM) and marginal effects from probit model. Treatment group: who gave birth between October 1995 and September 1996, Control group: who gave birth between October 1993 and September 1994. Standard errors are in parentheses. Sample sizes are in square brackets. Means are weighted with the sampling weights.

2001 reform	First child				Second child			
	October1997-September1998 VS. October1998-September1999		October1996-September1997 VS. October1997-September1998		October1997-September1998 VS. October1998-September1999		October1996-September1997 VS. October1997-September1998	
	LPM	Probit	LPM	Probit	LPM	Probit	LPM	Probit
(1) Difference-in-Difference								
without covariates	0.0044	0.0044	-0.0283	-0.0283	0.0208	0.0209	-0.0018	-0.0018
	(0.0238)	(0.0238)	(0.0241)	(0.0240)	(0.0301)	(0.0301)	(0.0315)	(0.0315)
with covariates	-0.0073	-0.0084	-0.0231	-0.0217	0.0175	0.0206	-0.0108	-0.0114
	(0.0230)	(0.0229)	(0.0232)	(0.0231)	(0.0293)	(0.0290)	(0.0311)	(0.0308)
	[2788]		[2702]		[1129]		[1135]	
(2)Triple Difference								
without covariates	0.0181	0.0181	-0.0368	-0.0368	0.0250	0.0250	-0.0058	-0.0058
	(0.0249)	(0.0249)	(0.0251)	(0.0251)	(0.0310)	(0.0310)	(0.0324)	(0.0324)
with covariates	0.0112	0.0057	-0.0313	-0.0299	0.0236	0.0251	-0.0100	-0.0175
	(0.0246)	(0.0240)	(0.0248)	(0.0241)	(0.0305)	(0.0311)	(0.0321)	(0.0324)
	[6691]		[6341]		[4037]		[3911]	

Table .7: Placebo Regression DD and DDD Estimates of the Effect of 2001 Reform on Regular Employment After Childbirth

Note: Data are from the 2002 Japanese Employment Status Survey. The outcome variable is the regular employment status, which takes a value of 1 if the mother or father worked as a regular employee and 0 otherwise. Estimates are coefficients from the linear probability model(LPM) and marginal effects from the probit model. Standard errors are in parentheses. Sample sizes are in square brackets. Means are weighted with the sampling weights.

1995 reform	First child				Second child			
	October1992-September1993 VS. October1993-September1994		October1991-September1992 VS. October1992-September1993		October1992-September1993 VS. October1993-September1994		October1991-September1992 VS. October1992-September1993	
	LPM	Probit	LPM	Probit	LPM	Probit	LPM	Probit
(1) Difference-in-Difference								
without covariates	-0.0124	-0.0124	-0.0076	-0.0076	-0.0182	-0.0182	-0.0154	-0.0154
	(0.0222)	(0.0222)	(0.0209)	(0.0209)	(0.0268)	(0.0268)	(0.0262)	(0.0263)
with covariates	-0.0036	-0.0036	-0.0033	-0.0023	-0.0147	-0.0133	-0.0101	-0.0057
	(0.0214)	(0.0213)	(0.0201)	(0.0201)	(0.0265)	(0.0263)	(0.0259)	(0.0257)
	[3192]		[3408]		[1240]		[1377]	
(2)Triple Difference								
without covariates	-0.0103	-0.0103	-0.0063	-0.0063	-0.0278	-0.0278	-0.0045	-0.0045
	(0.0228)	(0.0228)	(0.0216)	(0.0216)	(0.0277)	(0.0277)	(0.0270)	(0.0270)
with covariates	-0.0012	0.0015	-0.0063	-0.0010	-0.0267	-0.0243	-0.0044	0.0035
	(0.0225)	(0.0221)	(0.0214)	(0.0210)	(0.0275)	(0.0267)	(0.0268)	(0.0255)
	[7486]		[7926]		[4353]		[4820]	

Table .8: Placebo Regression DD and DDD Estimates of the Effect of the 1995 Reform on Regular Employment After Childbirth

Note: Data are from the 1997 Japanese Employment Status Survey. The outcome variable is the regular employment status, which takes a value of 1 if the mother or father worked as a regular employee and 0 otherwise. Estimates are coefficients from the linear probability model(LPM) and marginal effects from the probit model. Standard errors are in parentheses. Sample sizes are in square brackets. Means are weighted with the sampling weights. Means are weighted with the sampling weights.

2001 reform	First child				Second child			
	University/College graduates		Highschool graduates		University/College graduates		Highschool graduates	
	LPM	Probit	LPM	Probit	LPM	Probit	LPM	Probit
(1) Difference-in-Difference								
without covariates	-0.0269	-0.0269	0.0143	0.0142	-0.0142	-0.0142	0.0340	0.0340
	(0.0345)	(0.0344)	(0.0351)	(0.0351)	(0.0456)	(0.0456)	(0.0447)	(0.0447)
with covariates	-0.0142	-0.0128	0.0062	0.0055	-0.0071	-0.0097	0.0307	0.0309
	(0.0315)	(0.0313)	(0.0344)	(0.0344)	(0.0410)	(0.0398)	(0.0439)	(0.0434)
	[1463]		[1228]		[667]		[593]	
(2)Triple Difference								
without covariates	-0.0307	-0.0307	0.0125	0.0125	-0.0033	-0.0033	0.0443	0.0443
	(0.0376)	(0.0376)	(0.0376)	(0.0376)	(0.0474)	(0.0474)	(0.0451)	(0.0451)
with covariates	-0.0252	-0.0168	0.0158	0.0063	-0.0044	-0.0041	0.0438	0.0415
	(0.0362)	(0.0348)	(0.0372)	(0.0368)	(0.0462)	(0.0457)	(0.0448)	(0.0436)
	[3597]		[3209]		[2268]		[2410]	

Table .9: Regression DD and DDD Estimates of the Effect of 2001 Reform on Regular Employment After Childbirth by Education Levels

Note: Data are from the 2002 Japanese Employment Status Survey. The outcome variable is the regular employment status, which takes a value of 1 if the mother or father worked as a regular employee and 0 otherwise. Estimates are coefficients from the linear probability model(LPM) and marginal effects from the probit model. Treatment group: who gave birth between October 2000 and September 2001, Control group: who gave birth between October 1999 and September 2000. Standard errors are in parentheses. Sample sizes are in square brackets. Means are weighted with the sampling weights.

	First child				Second child			
	University graduates		Highschool graduates		University graduates		Highschool graduates	
1995 reform	LPM	Probit	LPM	Probit	LPM	Probit	LPM	Probit
(1) Difference-in-Difference								
without covariates	0.0497	0.0496	-0.0067	-0.0067	0.0542	0.0545	0.0145	0.0145
	(0.0319)	(0.0318)	(0.0281)	(0.0281)	(0.0335)	(0.0339)	(0.0367)	(0.0369)
with covariates	0.0519	0.0506	-0.0118	-0.0115	0.0478	0.0427	0.0140	0.0152
	(0.0302)	(0.0299)	(0.0277)	(0.0276)	(0.0316)	(0.0308)	(0.0361)	(0.0359)
	[1535]		[1908]		[626]		[765]	
(2)Triple Difference								
without covariates	0.0316	0.0316	-0.0268	-0.0268	0.0299	0.0299	-0.0058	-0.0058
	(0.0324)	(0.0324)	(0.0291)	(0.0291)	(0.0339)	(0.0339)	(0.0373)	(0.0373)
with covariates	0.0345	0.0299	-0.0255	-0.0302	0.0286	0.0103	-0.0042	-0.0057
	(0.0318)	(0.0303)	(0.0291)	(0.0288)	(0.0333)	(0.0296)	(0.0370)	(0.0340)
	[3755]		[4318]		[2295]		[2854]	

Table .10: Regression DD and DDD Estimates of the Effect of the 1995 Reform on Regular Employment After Childbirth by Education Levels

Note: Data are from the 1997 Japanese Employment Status Survey. The outcome variable is the regular employment status, which takes a value of 1 if the mother or father worked as a regular employee and 0 otherwise. Estimates are coefficients from the linear probability model(LPM) and marginal effects from the probit model. Treatment group: who gave birth between October 1995 and September 1996, Control group: who gave birth between October 1993 and September 1994. Standard errors are in parentheses. Sample sizes are in square brackets. Means are weighted with the sampling weights.