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

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Abstract

This study assesses the impact of changes in the parental leave income replacement rate on job continuity of new mothers' 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 two natural experiments. I identify the causal effects of these reforms by comparing the changes in the regular employment of mothers who gave birth after the reforms and those who gave birth before the reforms. My results suggest that the two reforms had no significant effects on the job continuity of mothers who qualified for the reforms.

Keywords: Parental leave, Income replacement, Job continuity, Labour supply

JEL: J13, J21, J22

1. Introduction

The focus of this study is job-protected Parental Leave (PL) for mothers and how the rate of PL income replacement affects mothers' job continuity surrounding childbirth. The Japanese government enacted a number of PL provision reforms over a short period of time to boost the low maternal employment rate. I conducted an empirical analysis of two policy reforms: the PL income replacement rate increase from 0% to 25% in 1995, and the increase from 25% to 40% in 2001. Before and after the reforms, the maximum duration for job protection and eligibility for benefits were unchanged. Therefore, I can measure the causal effects of an increase in income replacement on the job continuity of mothers. I study the mothers who gave birth before the reforms and those who gave birth after the reforms, and examine how they stay employed during PL (i.e. maternal employment includes mothers who are employed but on leave), and return to work to their previous employer after their PL expires. Because PL is a national program and paid for by the government, the mothers' eligibility depends on the timing of childbirth, and not on their choice of employer or region. Moreover, because of the timing of the reform enforcement, it was difficult for mothers to select the timing of childbirth. Therefore, it is less likely that the timing of births will be correlated with the mothers' unobserved characteristics, and thus, these policy reforms present us with a quasi-experiment. Finally, this research also adds to the previous literature in that it focuses on a country with a very low maternal employment rate and limited availability of childcare, but a

relatively generous PL program.

The PL provisions, such as financial support and duration of job-protection, differ greatly by country. Some programs are mandated by national or regional laws, and others voluntarily provided by employers. The changes in PL provisions and how they affect the labour supply of mothers form our central policy discussion. Job protection guarantees the right of a new mother to return to her previous employer after childbirth. Income replacement provides financial support for new mothers to remain employed but stay at home with their newborn child when the value of their time with the child is high. However, when mothers are not actually at work, their human capital might depreciate and their preferences might change. Therefore, a prolonged period of PL might decrease labour supply and harm the subsequent wages of mothers.

The Japanese paid job-protected PL allows new mothers to stay at home until the newborn child reaches the age of exactly one year, and guarantees the mothers' right to return to work with their previous employer after their PL expires. The provision has two particular features. First, mothers can take PL and receive income replacement if they commit to return to work with their previous employer. A new mother must decide whether to take PL at least one month before her expected delivery date. She must submit a leave application along with proof of her post-birth employment contract, which would then make it difficult for her to falsify and terminate her contract after taking PL. Second, PL is mandated by the government and paid for by the national employment insurance program, so when a mother takes

PL, there is no increase in her employer's insurance premiums. Companies do not discourage new mothers from taking PL because it will not result in a cost increase for them, and moreover, the cost is not likely to be shifted to the mothers' wages.[1] Unreasonable wage reductions and the discharge of female workers on account of childbirth are prohibited by law. The proportion of employers offering additional monthly compensation to new mothers is less than 10%; thus, the income of most mothers while on PL does not depend on their employers but on the rate of income replacement, determined on the mothers' pre-birth income.

Under these PL provisions, a rise in income replacement rate increases the incentive of expecting mothers who otherwise would have quitted their job to stay employed under PL and return to work to the same employer after their PL expires. Despite generous increases in the income replacement rate, I found no significant effect of the reforms on the job continuity of mothers who qualified for the reforms compared to those who did not qualify for the reforms. I confirmed the robustness of my results by checking for the presence of time trends and economic shocks with fathers and non-childbearing women as comparison groups, and by investigating for the presence of pre-reform trends.

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[1]In the United States, Gruber (1994) found that the costs of adding maternity benefit were shifted to the wages of the groups that benefited.

2. Theoretical Framework

New mothers decide to take PL and return to work to their previous employer when their expected utility from working U_w exceeds their expected utility from not working U_n. For simplicity, I assume that a mother will take PL for the maximum period, that is, until her child's first birthday. Mothers maximize their utility from the childbirth year ($t = 0$; 0-11 months since birth), to one year after birth ($t = 1$; 12-23 months since birth). Taking care of one's own child is deemed a leisure activity. When a woman has a child, she has to bear the cost of childcare. If she works, she has to use external childcare services. For simplicity, childcare cost is defined as a fixed cost, because childcare facilities in Japan require mothers to pay by the day rather than by the hour. If the mother does not work, she will provide childcare herself (Boeri and van Ours (2013)). In the Japanese system, a mother has to decide whether to take PL not later than one month prior to the expected delivery date. The mother will receive income replacement, r, only if she commits to return to work after childbirth; that is, $h_1 > 0$, where h denotes work hours. She compares the consumption and value of leisure from $t = 0$ to $t = 1$, and will return to work when $t = 1$ and the following equation is satisfied:

$$U_{w,0}(N_0 + r \cdot I[h_1 > 0], l_0) + U_{w,1}(W_1(T_1 - l_1) + N_1 - cc_1 \cdot I[h_1 > 0], l_1)$$
$$-U_{n,0}(N_0, l_0) - U_{n,1}(N_1, l_1) > 0 \tag{1}$$

where N is non-labour income, W is hourly wage, cc is cost of childcare and I is an indicator function. The introduction of r increases the incentive to work. When

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there is no r, a mother who does not want to work when her child is younger (when the marginal utility of leisure is high) has $U_{w,1} - U_{n,1} < 0$. However, when there is r, those mothers re-evaluate their expected utility, and some mothers might choose to work. When r is increased to $r + \triangle r$, some mothers might want to increase their leisure and not want to return to work. However, if a mother does not return to work, then $r = 0$. Note that if a mother quits the current job and takes a new job, then $r = 0$.[2] Therefore, the two reforms might encourage mothers who previously quit their jobs to stay employed under PL and return to their previous employer. If I relax the assumption that 'all mothers take PL for the maximum duration', mothers can either return to work at $t = 0$ or $t = 1$. The two reforms might induce the mothers to stay at home longer, and a greater proportion of mothers might return to work at $t = 1$ instead of $t = 0$.

The magnitude of increase in r is shown in Figure 1. Income replacement is paid as a monthly stipend consisting of two parts: one paid during the PL period, and the other paid as a lump-sum six months after returning to work. The 1995 reform, raised the income replacement rate from 0% to 25%, 20% paid during the leave period, and the remaining 5% paid as a lump-sum upon return to work. With the

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[2]It is also assumed that wages from alternative jobs will be lower than those of the current job. The Japanese labour market is not flexible-it is difficult to find regular employment, and non-regular jobs do not pay as well. Thus, wages offered from new employers tend to be considerably lower than those of one's current job. Therefore, mothers' wages are higher when they continue working at their pre-birth employer after giving birth than if they take a new job.
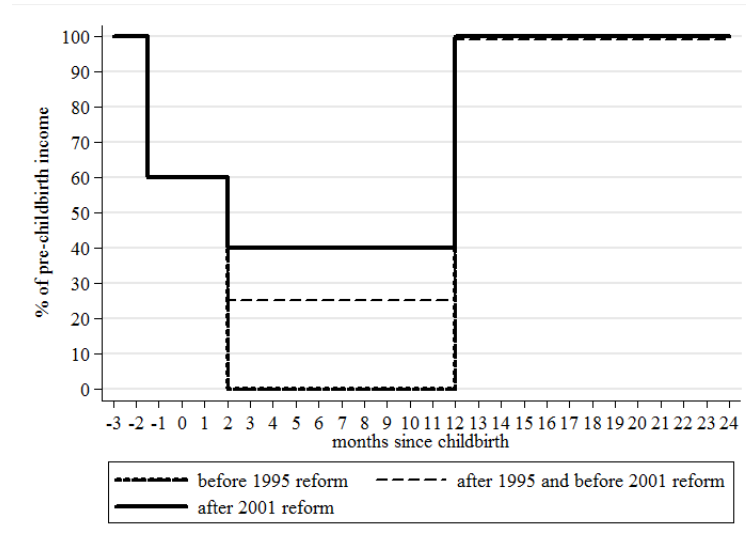
Figure 1: Mothers' Monthly Income Surrounding Childbirth

Note: The vertical axis denotes mothers' income as a % of pre-childbirth income and the horizontal axis is months since childbirth (childbirth month is denoted as 0). Mothers can only take PL and receive income replacement conditional on the promise of returning to work to the previous employer, hence, their decision to return to work is based on the total amount of income replacement. For this reason, I combine the income replacement during leave and the lump-sum payment for the total in the figure. Note that the Maternity Leave benefit will cover 60% of a mother's previous income for 42 days before and 56 days after the birth.

2001 reform, the rate was increased by 15 percentage points to 40%, 30% paid during the leave period and the remaining 10% paid as a lump-sum. As discussed previously, in order to receive r, mothers must commit to return to work not later than one month before their expected delivery date; hence, both the monthly and lump-sum PL income replacement payments depend on the mothers' pre-birth decision. For this reason, I focus on the effect of *total* income replacement on the job continuity of mothers.

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3. Previous Literature

PL provisions differ in terms of duration, amount paid, and degree of job protection. The maximum duration of job-protection as well as cash benefits under PL programs determine mothers' labour supply. Europe and Canada have generous programs, whereas the United States has a restricted program.[3] Previous Studies on PL and mothers' labour supply were conducted primarily using North American and European data.[4] Relatively few studies have examined job continuity (namely, mothers' returning to work at pre-birth employer).

Baker and Milligan (2008) find that the entitlement to both short and long job-protected leave increases job continuity with the previous employer.[5] Schonberg

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[3]In the United States, FMLA provides 12 weeks of unpaid job-protected maternity leave (ML) to employees in companies with more than 50 employees. Canada provides 15 weeks of paid ML and 35 weeks of paid PL, and job protection duration varies by province. Germany provides paid ML of 6 weeks before and 8 weeks after childbirth, and PL with flat transfer for a maximum duration of 24 months, and 36 months of job-protection. The Japanese program is generous and in line with the Canadian program.

[4]Previous Japanese studies identified the causal effects of PL provision by comparing women working for a company that voluntarily provided PL with women working for companies that did not (before 1992, PL was not mandated by the government). However, those estimates may suffer from unobserved differences in mothers who gain employment at companies offering PL.

[5]In one of their estimation results, they showed that extending job protected leave from 17-18 weeks to 29-52 weeks induces some women who previously returned to work but with other employers to now return to the pre-birth employer. Their dependent variable is an indicator for mothers being employed and at work the fourth months following the month of birth.

and Ludsteck (2007) find that an expansion of paid/job-protected period increases the probability that a woman will work for her pre-birth employer shortly after PL expires; however, many women leave their pre-birth employer soon after they return to work.[6] Waldfogel et al. (1999) find that leave coverage increase job continuity.

A number of studies on mothers' return-to-work decisions (including return-to-work with other employers) find that changes in PL provision affect their time away from work. An extension of the maximum duration of paid leave and/or job protection makes mothers stay at home longer and remain employed under PL and delays their return to work (Schonberg and Ludsteck (2007), Baker and Milligan (2008), Hanratty and Trzcinski (2009), Lalive and Zweimuller (2009), Lalive et al. (2013)).[7] The extension of PL duration increases the duration of leave taken and time spent at home; however, studies find that the likelihood of mothers returning to work after their PL expires does not change significantly because of such extension (Schonberg and Ludsteck (2007), Hanratty and Trzcinski (2009)). A prolonged paid job-protected PL may decrease the likelihood of mothers returning to work after

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[6]They interpret this result in two ways: first, some firms might lay off mothers soon after they return to work; second, mothers might play the system and return to work only in order to qualify for unemployment benefits. Whether either one of these hypothesis is true remains unknown.

[7]On the other hand, studies on the United States have shown that a short and unpaid job-protected PL does not have a significant impact on mothers' labour supply. For example, Klerman and Leibowitz (1997) investigated the labour supply effect of the United States' FMLA and found no statistically significant effect on employment, on leave, or at work. Baum (2003) found similar results of small and insignificant effects on employment.

the PL expires, partly because it may induce them to have another child and partly because when mothers are away from work, their human capital might depreciate and their preferences might change.[8] Lalive and Zweimuller (2009) shows that extending mothers' paid job-protected PL from one year to two years reduces the likelihood of their returning to work.

The present study contributes to the literature in two ways. First, it investigates how mothers' job-continuity surrounding childbirth -including after PL expired- varies with income replacement when the maximum duration of paid and job-protection remain unchanged. PL income replacement in Japan can be considered a government subsidy provided to new mothers conditional on their promise to return to work with their pre-birth employer. Therefore, it provides stronger incentives for mothers to stay employed during PL and to return to their previous employer after PL, which, in turn, could help mothers to preserve their job-specific human capital surrounding childbirth. Second, this study examines a country, Japan, in which the PL provision is generous but the maternal employment rate is very low and, therefore, public policies may have a larger impact on mothers' labour supply than in other countries. According to the OECD, the Japanese maternal employment rate for mothers with children three years of age or younger was 29.8%, which is approximately 30 percentage points lower than the average in OECD countries.

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[8]Using aggregate data, Ruhm (1998) finds that PL is associated with increases in female employment, but with reductions in their relative wages at extended durations in European countries.

The female employment rate in 2005 for those aged 25 to 49 was 65.7%, which is approximately 10 percentage points lower than the OECD average. This study also provides insights into the impacts of family policies on mothers' labour supply under limited availability of childcare facilities.

4. Parental Leave Policy in Japan

4.1. Job Protection and Income Replacement

Pregnant women can take advantage of paid job-protected PL in Japan. The PL program in Japan is mandated by the Child Care and Family Care Leave Act (1992).[9] Under this Act, mothers can take PL of up to 10 months after ML (which is 42 days before and 56 days after childbirth).[10] The leave starting date can be adjusted based on the expected delivery date, therefore mothers cannot perfectly

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[9]A list of recent reforms are in the appendix. The eligibility for PL was expanded to include non-regular employees, and the maximum duration of PL was extended to 16 months in 2005. The income replacement rate was increased from 40% to 50% in 2007. In 2010, the monthly income replacement during PL and the lump-sum income replacement were combined, and currently only monthly PL income replacement payments are made. These reforms are not the focus of my study, but should be reviewed in future analyses.

[10]ML is mandated by the Labour Standards Act (1947), and it is illegal to work or to allow a new mother to work within 42 days of childbirth. ML is mainly provided for maternal health reasons. During ML, the ML income replacement is provided by the Health Insurance Program (1958-) and is equivalent to 60% of a mother's pre-birth income. The ML income replacement rate was 60% from its inception in 1958 until April 2007, when it was increased to two-thirds of the pre-birth income. ML is available to all working mothers, including non-regular employees.

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plan the timing of their leave. Mothers have to return to work by the day after the child reaches exactly 12 months of age.[11] The actual PL take-up rate is unknown. MHLW (2002) showed that 64.0% of mothers who had a child during April 2001-March 2002 claimed PL, although the actual percentage of mothers who take PL out of those who qualify for it is estimated to be higher.

PL income replacement is paid through the national employment insurance, and PL rights are available only to the new mothers who are covered under the employment insurance program, i.e. the regular employees. The benefits are determined in accordance with the mother's average monthly wage for the six months prior to childbirth. The program has set the maximum monthly wage as 430,200 yen and the minimum as 69,900 yen.[12] According to the Basic Survey on Wage Structure (2010), the mean wage for females aged 20-39 is 219,300 yen, with the proportion of those earning above 400,000 yen only 1.69%. Therefore, most mothers receive income transfer proportional to their income. Note that income replacement is non-taxable, and therefore the incentive to mothers is proportionally dependent on where they stand in income distribution. Childcare cost is also proportional to income but varies by region, and by whether the service is public or private. Public childcare cost takes up about 10% of working mothers' income, averages 20,000-30,000 yen per month. Private childcare cost takes up about 20% of working mothers' income, av-

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[11]Fathers are also eligible to take PL, but the take-up rate by fathers was only 0.33% in 2001(MHLW (2002)), and thus this study only focuses on mothers.

[12]The maximum and minimum amounts change very slightly every August.

erages 30,000-50,000 yen per month. Most mothers' PL income replacement do not depend on their employer; the proportion of companies offering additional monthly income replacement was only 7.5%, with another 3.5% offering a lump-sum payment, such as a small amount of cash as a maternity gift (MHLW (2002)).

4.2. Return to Work Commitment

A new mother must decide whether or not to take PL and return to work after childbirth at least one month before the expected delivery date, and must submit her PL application to her company. Based on her decision, the company submits the leave application to the government along with proof of the woman's post-birth employment contract. New mothers are also asked to submit the Maternal and Child Health Handbook, which is completed by their gynaecologist, to prevent them from providing a false expected delivery date. Although the submission deadline for the leave application is one month prior to the expected date of delivery, most expecting mothers make their post-birth employment decision before the pre-ML period, which is at least two to three months before the expected delivery date, owing to the bureaucratic processes and social pressures of companies. For example, expecting mothers who plan to quit their job must transfer their responsibilities to and train a replacement.

Strictly speaking, there is no punishment for a new mother not returning to her previous employer. The mother will not receive the lump-sum payment, but she does not have to reimburse the PL benefit. However, due to the bureaucratic processes

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and social pressures in Japanese companies, mothers would find it very difficult not to return after making a commitment to return. The exact rate of mothers who commit to return but do not is not disclosed by the government, but the number of mothers who receive the PL payment and the lump-sum payment are close in number; the difference is only about 15%.[13] Furthermore, the psychological cost of not returning is very high. If a mother quits her job during PL, her company terminates her contract immediately, and thereafter she will not receive income replacement. Mothers who decide not to return after taking PL must inform the company their decision at least two to three months before their due date to return-to-work. If a mother does not return to her previous employer after PL, she will not be likely to find a new job, because during the hiring process, most companies ask for references from previous employers. Taking account of these factors, only rarely does a mother decide not to return to work at the end of her PL.

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[13]According to the Employment Insurance Report, the number of mothers who received PL payment for the first time was 77,944 from June 2001 to March 2002, and the number who claimed the return to work benefit was 66,422 from October 2002 to July 2003. Thus, the estimated return to work rate among who received PL payment is about 85%. Note that the rate is calculated based on the assumption that all mothers take leave until their child reaches age one, and submit the return to work payment application form soon after 6 months have passed since their return to work.

5. Data

5.1. Description of Data

The data in this study come from the Japanese Employment Status Survey (ESS), which was conducted by the Statistics Bureau on household members 15 years of age or older in approximately 440,000 households in 1997 and 2002. Of those, 80% of the responses are accessible for research purposes; the total number of individuals available after re-sampling was 795,933 in 1997 and 752,068 in 2002. Sampling weights are used to compensate for unequal selection probabilities.[14]

The survey is conducted on October 1st of each year and age is counted in full years as of September 30. The detailed retrospective accounts of the respondents' employment status information are available in the survey data, from which I have created individual panel data sets based on the age of each newborn child, and the mothers' current and past employment status, job tenure, quitting dates, and the starting dates for both the current and past jobs, for their first and second children separately. The individual birth months of the children are unidentifiable; however, since the children's age at the time of the survey are available, I can correctly identify the timing of each childbirth and the dates the new mothers were supposed to return to work, because all mothers must return to work by the day following the child's first birthday. The data on mothers' regular employment status from three years

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[14]Households containing more than eight persons or with more than three household members the same age are excluded from the re-sample.
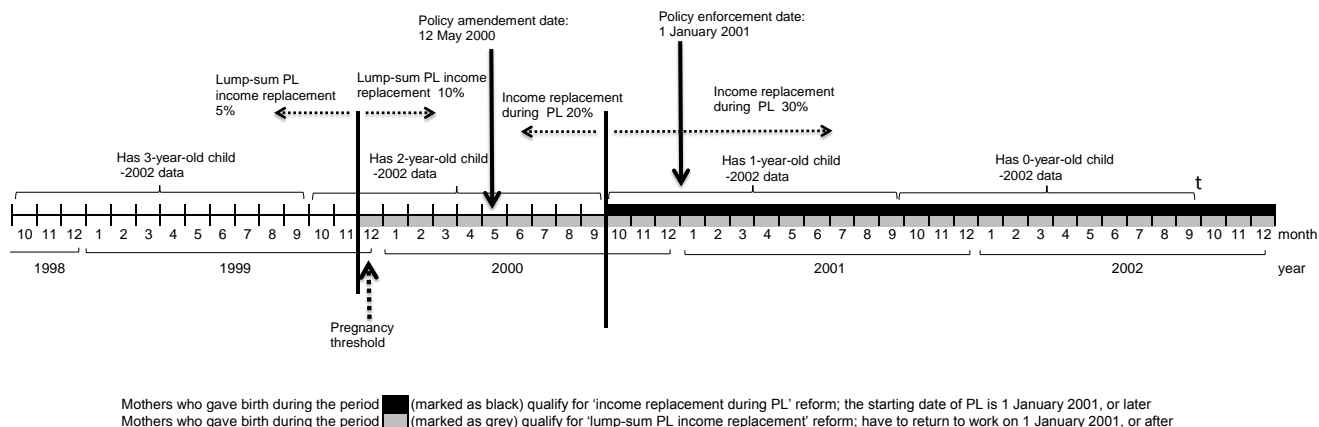
Figure 2: Identification Strategy and Data Structure of the 2001 Reform

Note: Data are from the ESS 2002. Based on the child's age in the 2002 data, the date of childbirth (horizontal axis) is identified. The policy was amended in May 2000 and enacted in January 2001. The figure for the 1995 Reform is shown in the appendix.

before childbirth to one year after are constructed based on each mother's childbirth date. The mothers on job-protected leave are recorded as employed.

The method used to construct the data is shown in Figure 2; for example, a mother who has a child aged 0 (as of 30 September 2002, the survey date) is coded as delivering between October 2001 and September 2002; a mother who has a two-year-old child is coded as delivering between October 1999 and September 2000, and so on. The year and month given under the horizontal line denote the childbirth date; the arrowed lines denote the policy amendment and policy enforcement dates. The solid line in the middle divides the mothers into two groups: those who qualify for the reform and those who do not.

Tenure information is available on a monthly basis for the 2002 data and yearly

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basis for the 1997 data; therefore, I first show the 2001 reform results, and then show the 1995 reform results. Note that because of differences in the questionnaires, we cannot directly compare the 1997 and 2002 data results. The data have two limitations: first, wage information is available only for the survey year, as a range of numerical values; therefore, wage information cannot be included in the model. However, wages tend to be determined based on seniority, industry, and company size in Japan. Therefore, including this information instead of wage information reduces the potential unobservable effects. Second, the respondents' age is recorded in five-year ranges (e.g. 20-24, 25-29, 30-34, or 35-39; however, each child's age is recorded as an exact number), but this is unlikely to cause biased estimates because, according to the Vital Statistics, in 2000, the proportion of mothers giving birth under the age of 19 years old was only 1.66% and the proportion of those aged above 40, 1.28%. Therefore, by restricting the sample to mothers who gave birth and constructing panel data for each mother for her first and second child separately, we can capture the data of mothers who gave birth when aged 20-39. In addition, I compare the mothers who gave birth before and after the reforms with a one-year time window, so the bias resulting from these limitations can be considered small. Furthermore, in Japan, a majority of newly hired regular employees acquire their jobs right after graduation, and a regular employment job transfer is unusual. Therefore, by including data on education level and tenure, we can capture the

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data on age.[15] Despite the limitations, this dataset has merits because it is based on a nationally representative government survey and can be used to investigate employment in Japan, which is still unfamiliar to most people outside Japan. The data can provide new insights into the labour supply of females and its relation with family leave policies from the perspective of a developed Asian country.

5.2. Overview of Employment and Job Continuity

I limit this study to mothers with regular employment, because only regular employees are eligible for PL. Regular employees are hired without a predetermined period of employment, work for scheduled hours, are full-time and covered by social insurance programs; the so-called 'lifetime' stable employment. The government introduced the PL reforms in order to increase mothers' regular employment rate, because the 'lifetime' employment and seniority-based career advancement system in Japan have made it difficult for workers to return as regular employees once they quit. Workers who quit their jobs and then return to the labour market, mostly take up new non-regular jobs. Non-regular employees are part-time or fixed/short term employees, are paid less and are eligible for fewer social insurance programs.

Figure 3(1) shows the employment-to-population ratio and regular-employment-

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[15]According to the *Monbu Kagaku Tokei Yoran*, the female high-school advancement rate is 97% (high school students are aged 16-18). High-school graduates either find employment right after graduation or go to higher education, and the college entry age is typically 19-20. University graduates also find employment upon graduation, as the labour market greatly values new graduates.

to-population ratio calculated for each age category from the 2002 data. The employment-to-population ratio includes both regular and non-regular employment. The employment rate for females in their 20s is about 68%, but it decreases to 57% during the childbearing years (age 30-34) and slightly rebounds to 61% after age 35. In contrast, the regular employment rate for females in their 20s is about 39%; the rate decreases to 28% for females aged 30-34, and become even lower, 24%, for those aged 35-39. The regular-employment-to-population ratio shows no increase after age 35, indicating that mothers either remain unemployed or take up non-regular employment after giving birth. Thus, it is important to improve the work incentives for mothers with regular employment so that they stay employed during their childbearing period, and maintain a life-long career.

Figure 3(2) gives an example of how the status of regular employment changes over time. The figure shows the number of women and men aged 25-29 with regular employment in 1997, and how many of them remained in the same jobs one to five years later (aged 30-34 in 2002). If the men/women had any children during those five years, they were categorized as fathers/mothers. Over the five-year period, while most males remained in their jobs, approximately 7% either left their companies or were fired. In contrast, female job continuity declined substantially. Japanese women are generally less attached to the labour market because the family's main breadwinner is typically a man, and there is a significant gender wage gap. According to the OECD, the gender gap in median earnings for full-time employees was 33.9% in 2000, almost twice the OECD average; Japanese women earn

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(1) Employment-to-Population Rate by Sex   (2) Regular Employment Rate Over 5 Years by Sex and Parenthood
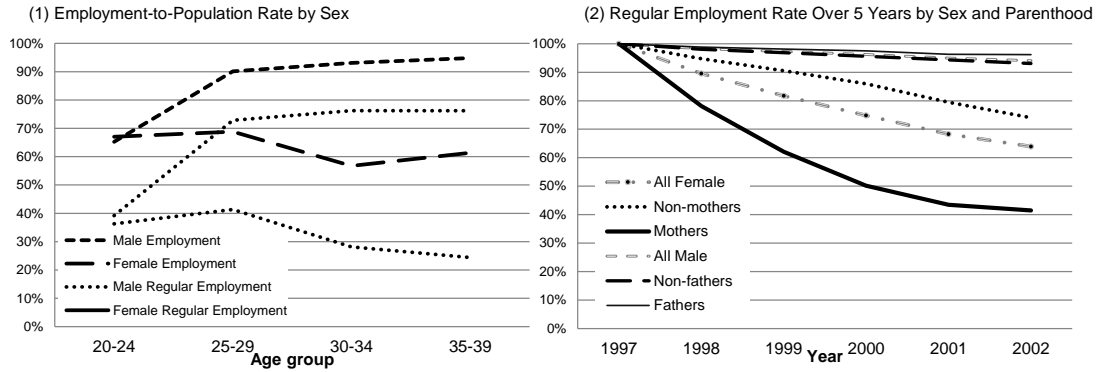
Figure 3: Employment-to-Population Rate and Job Continuity

Note: Rates are calculated from the ESS 2002. (1) Employment denotes Employment-to-Population rate, the percentage of women/men who are employed. Regular Employment denotes regular-employment-to-population rate, the percentage of women/men who are employed as regular employees. (2) Regular employment rates over 5 years (a measure of job continuity) are calculated for regularly employed women and men aged 25 to 29 in 1997 in the ESS 2002 data. If a person leaves the company, he/she is counted as having left the job. Means are weighted with the sampling weights.

only two-thirds of what men earn. The proportion of mothers remaining in regular employment declines to a greater extent than that of non-mothers: approximately 60% of mothers quit working as regular employees whereas only 25% of non-mothers quit working after five years, strongly suggesting that childbearing is a major factor in job turnover.

	Rate of income replacement		
Date of childbirth	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 1: Empirical Design

6. Identification Strategy

6.1. Empirical Design

To measure the reforms' causal effects, I consider the variations in the income replacement rates that resulted from the policy reforms. The mothers' job continuity surrounding childbirth -the number of mothers who remain employed under PL and return to work after the PL expires- of the treatment mothers who gave birth after the reforms are compared to a control group of mothers who gave birth before the reforms. The eligibility of mothers under the reforms varies by the timing of childbirth. Moreover, as I will explain later, because of the timing of the policy reforms, it was very difficult for mothers to select the timing of their childbirth to become eligible for the reforms.

The empirical design of this study is described in Table 1. The 2001 reform was amended on 12 May 2002, and enforced from 1 January 2001; the 1995 reform was amended on 29 June 1994, and enforced from 1 April 1995. One's eligibility to

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benefit from either reform is based on the starting date of one's PL. For the 2001 reform, I consider the mothers who gave birth between October 2000 and September 2001 as my treatment group, and compare the outcomes of this group with those of the control group mothers who gave birth between October 1999 and September 2000. The former group of mothers received 40% income replacement while the latter received 30%. However, although the control group of mothers received only 20% income replacement during PL, they received the same 10% lump-sum payment as the treatment mothers. To examine whether the increase in lump-sum payment had an effect on the labour supply of mothers and also determine the robustness of my results, I also compare a second control group with the treatment group, that is, the mothers who gave birth between October 1998 and September 1999 and received only 25% income replacement. For the 1995 reform, the assignment of mothers to the treatment and control groups differs from that of the 2001 reform because the policy came into force in April 1995, and the mothers who gave birth between October 1994 and September 1995 were excluded from the estimates because they could be placed in either the treatment or control group. Therefore, I consider the mothers who gave birth between October 1995 and September 1996 as the treatment group, and compare their outcomes with those of mothers who gave birth between October 1993 and September 1994.

In this study I measure the extensive margin of labour supply responses to the reforms. If employers allowed their employees greater flexibility in working hours, the individuals would find it easier to adjust their labour supply along the intensive

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margin. In Japan, the number of working hours is inflexible for most regular employees; therefore, mothers tend to adjust their labour supply along the extensive margin.

In the following section, I first consider the results of the 2001 reform and then those of the 1995 reform. The employment history information is recorded on a monthly basis in the 2002 data and yearly basis in the 1997 data; thus, I obtain a more accurate employment status for the 2001 reform. Furthermore, in comparison with the 1995 reform, the empirical design of the 2001 reform is well constructed to measure the impact around the reform.

6.2. Random Assignment Assumption

Random assignment makes treatment independent of potential outcomes and allows us to estimate the average causal effect of treatment (Angrist and Pischke (2008)). If mothers could change the timing of their conception in order to be eligible for the reform, the birth date would not be random. An increase in the replacement rate might increase the labour supply of females planning to become pregnant as well and thus might affect the composition of mothers. To confirm that the group assignment is random, I investigate (1) whether there was self-selection into the treatment group, (2) whether the characteristics of the treatment mothers and control mothers are identical, and (3) whether the reform induced more women to give birth.

Figure 2 shows the reform amendment date, the enforcement date, and the data

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structure, to confirm that the reform was not amended early enough to change the pregnancy timing of women. Mothers who took PL on or after 1 January 2001, qualified for the post-reform level of during PL income replacement (an increase from 20% to 30%), and those who returned to work on or after 1 January 2001, qualified for the post-reform lump-sum payment (an increase from 5% to 10%). As the duration of a pregnancy is 9-10 months,[16] mothers who gave birth after October 2000 (marked in black) qualified for the full increase in income replacement and received 40% income replacement. In order to deliver a child in October 2000 or later, a mother must conceive in December 1999 or later (the pregnancy threshold denoted in Figure 2). Since the policy was amended on 12 May 2000, the mothers who gave birth between October 1999 and September 2000 (i.e. those who conceived between December 1998 and November 1999) could not control the timing of their delivery in order to qualify for the 2001 reform. Note that the mothers who gave birth in October 2000 could be either in the treatment group or control group, but they could utilize their unused paid holidays to delay their PL starting date and thus be included in the treatment group. The average number of paid holidays is 20; by combining them with public holidays, a woman could easily delay her PL starting date by about one month. The second control group mothers, who gave birth between October 1998 and September 1999, could not control the timing of

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[16]The typical pregnancy lasts about 280 days (40 weeks) from the first day of last menstrual period, and the median is about 268 days from ovulation. However the period varies by up to 37 days. (Jukic et al. (2013)).

their delivery to qualify for the reform either, as they had already given birth when the policy was amended.

I also checked to see if there was any announcement effect. According to the four major Japanese newspapers,[17] the public became aware of the proposal for the 2001 reform only on 6 December 1999; note that the pregnancy threshold was 1 December 1999. Hence, mothers could not control the timing of conception to qualify for the reform. For the 1995 reform, control mothers could not control their timing of conception to qualify either.[18]

If there were self-selection into the treatment group, the means of the treatment and control groups could be significantly different. Table 2 shows the means of the key characteristics of the treatment and control mothers and the t-statistics for the

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[17]Nikkei, Yomiuri, Asahi and Mainichi

[18]Mothers who started their PL on April 1st 1995 or later qualified for both the post-reform during PL income replacement (increased from 0% to 20%) and the post-reform lump-sum PL replacement (increased from 0% to 5%), and thus mothers who delivered babies in January 1995 or later qualified for the reform. To deliver a baby January 1995 or later, a mother had to be pregnant in March 1994 or later. As the policy was amended on June 29, 1994, a control mother who gave birth between October 1993 and September 1994 (i.e. became pregnant between December 1992 and November 1993) could not control the timing of her birth to qualify for the reform. While mothers who gave birth in January 1995 could be either in the treatment or control group, those mothers could use their unused paid holidays to delay their PL starting date. Newspapers announced the reform proposal on December 1, 1993, which is a bit earlier than the pregnancy threshold. However, this will not pose a problem for this study because control mothers were already pregnant when the reform proposal was announced.

group differences in mean. The two groups of mothers are almost identical in both reforms. The only noticeable difference between the two groups is in the proportion of those working in the manufacturing and service industries for the 2001 reform. The mean differences in those variables for the treatment and control mothers are significant but small.

To examine whether the two reforms induced more women to give birth, I compare the frequency of births during the period. According to Vital Statistics, I found no spike in the number of births around the threshold of the 2001 reform,[19] further confirming that there was no significant self-selection into the treatment group that could detrimentally affect the comparison of the treatment and control group mothers. No increase in births was found in the ESS data either: 7.0% (first birth: 3.0%) of females aged 20-39 gave birth between October 1999 and September 2000, and 6.8% (first birth: 2.9%) gave birth between October 2000 and September 2001; the difference is not significantly different from zero. No spike was seen around the 1995 reform too.[20] I also checked the number of eligible mothers, that is, regular

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[19]The number of births from October 1999 to September 2000 was 1,190,077(103,131 in September 2000 only) and about 7.0% of females aged 20-39 gave birth; from October 2000-September 2001 was 1,173,366 (100,752 in October 2000 only) and about 6.9% of females aged 20-39 gave birth). Therefore, there was no significant increase in births around the 2001 reform. Note that while the number of births is the actual number taken from the birth registry, the monthly population is unavailable and is based on an approximation from the census population.

[20]The number of births from January 1994 to December 1994 was 1,238,328 (104,424 in December 1994 only) and about 7.4% of females aged 20-39 gave birth; the number from January 1995 to

employment, before and after the reform to verify whether the composition of this group changed. The proportion of regular employment three years before childbirth was not significantly different between the treatment and control groups, at about 45%, confirming the satisfaction of local randomisation.

		2001 Reform			1995 Reform		
		Control	Treat-ment	t	Control	Treat-ment	t
Education	University / College Graduates	0.579	0.569	0.18	0.452	0.482	1.96
	High School Graduates	0.421	0.431	0.18	0.548	0.518	1.96
Pre-birth Characteristics (3 Years Before)							
Tenure (years)		4.167	4.192	0.03	3.299	3.472	1.90
Company size	Less than 30 employees	0.189	0.209	1.00	0.247	0.252	0.06
	30-299 employees	0.303	0.299	0.04	0.278	0.273	0.07
	More than 300 employees	0.405	0.398	0.09	0.392	0.396	0.02
	Public office	0.102	0.095	0.26	0.082	0.079	0.08
Industry	Manufacturing	0.264	0.229	2.67	0.251	0.232	1.13
	Service	0.736	0.771	2.67	0.749	0.768	1.13
Sample size		1393	1298		1809	1634	

Table 2: Means of Key Characteristics

Note: For the 2001 reform, the treatment group comprises 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. For the 1995 reform, the treatment group comprises 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 two groups. Means are weighted with the sampling weights.

December 1995 was 1,187,064 (102,692 in January 1995 only) and about 7.1% of females aged 20-39 gave birth.

6.3. Estimating Models

Using the panel data structure, I examine how the women remain employed with their previous employers during the years around their giving birth. I restrict my attention to those with regular employment three years before giving birth. The employment insurance program states that persons with (1) less than one year of continuous employment, (2) a contract that will terminate in less than a year, and (3) less than two days of work per week can be excluded from the PL program under the labour-management agreements between the employees and employers.[21] Mothers with regular employment three years before childbirth will have more than one year of tenure before the birth and qualify to receive PL income replacement. The average age of first-time mothers was 28.2 in 2001; since most mothers will have worked for at least three years after completion of their education, this restriction is not strong, and does not create a serious selection bias. My focus is also on mothers having their first child.[22] I estimate the following probit equation for each reform separately:

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[21]Labour-management agreements in Japan are agreements between employees and employers. In contrast, collective bargaining agreements are negotiated by unions and employers at the company level. 18% of workers are members of labour unions, which are primarily formed in companies with more than 1,000 employees (50%). Companies tend to formulate their own rules on the basis of labour-management agreements.

[22]Results for mothers having their second child are similar and shown in the appendix.

$$P(E_{it} = 1) = \Phi(\rho \cdot Reform_t + X_{it}\beta) \tag{2}$$

where E_{it} is the outcome variable for individual i in childbirth group t, taking a value of 1 when the mother remains in regular employment with the same employer and 0 otherwise (this is a job continuity measure). Those on leave are included in the employed category. I define this variable for two years before childbirth, one year before childbirth, childbirth year, and one year after childbirth to capture how mothers remain employed with the same employers surrounding childbirth. $Reform$ represents a 0/1 indicator of reform eligibility and captures the average effect of the reform. Since job-protection lasts until the child's first birthday, all mothers have to return to work one year after the birth. A mother not having a regular employment one year after birth is one who did not return to her pre-birth employer. If the increased PL payments based on the reforms increase the number of mothers remaining employed under PL (and then quit when the PL expires), that can be captured from the composition of the pre-birth-year and birth-year employment rate. X denotes the vector of mothers' characteristics (education level, job tenure, size of the company, and industry) measured three years before childbirth;[23] it controls for the observable characteristics affecting the employment response. I separately estimate regressions for comparison of (1)the treatment and

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[23]Because of the data limitation mentioned in the previous section, the estimates shown do not include age information. Note that models including age category dummies at survey year did not change the results.

control groups for the 2001 reform, (2)the treatment and second control groups for the 2001 reform, and (3)the treatment and control groups for the 1995 reform.

To control for any time shocks that may have existed around the treatment period, I take fathers and non-mothers as the comparison groups and examine their employment status before and after the reforms compared to that of mothers. The panel data of fathers are created using the same procedure as that for mothers, based on the age of each newborn child, and their current and past employment status, job tenure, quitting dates, and the starting dates for both their current and past jobs. I create a panel data of non-mothers (who are unaffected by the reforms, either before or after) by randomly assigning them to one of two groups; using a matching algorithm, I chose a sample of women with regular employment who have not given birth but have characteristics similar to those of mothers.[24] This helps me solve the problem of some non-mothers who could possibly be in either group because the data are taken from retrospective accounts of employment data.

I include these two comparison groups using a difference-in-difference probit model that includes the interactions between a mother and reform indicator:

$$P(E_{itm} = 1) = \Phi(\beta_1 \cdot Reform_t + \beta_2 \cdot Mother_m + \gamma \cdot Reform_t \text{x} Mother_m + X_{itm}\beta) \ (3)$$

where $Mother$ takes a value of 1 for mothers and 0 otherwise, and represents the group fixed effect. The father or non-mother models run separately. γ captures all

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[24]I conducted 1 by 1 exact matching based on women's observed characteristics.

the variations in the job continuity of mothers in the treatment group, that is, the impact of the reform after taking into account the time and macroeconomic shocks.

Regressing the job continuity variables on group (cluster) level characteristics without considering the intragroup correlation of the errors result in a downward bias in the standard errors (Moulton (1990), Bertrand et al. (2004), Donald and Lang (2007), Conley and Taber (2011), Angrist and Pischke (2008)). The observations within the same group are not independent, and the inferences are likely to be overestimated. There are few clusters, and group size is large in this study; therefore, the inference is based on a t-distribution with G-L degrees of freedom, that is, number of groups minus the number of group constant variables (Donald and Lang (2007)), and compute robust standard errors clustered by group. The critical values for the tests of significance are drawn from a t-distribution with 2 degrees of freedom for model (3) with G=4. The critical values for the 1%, 5%, and 10% significance levels are 9.92, 4.30 and 2.92. For model (2) with G=2 case, it is harmless to include the unobserved cluster effect in the estimated treatment effect when the group assignment is randomized (Wooldridge (2010)). Therefore, I compute robust standard errors and use the standard inference method.

7. Results

7.1. A Graphical Comparison of Employment around the Time of Giving Birth

Figure 4(1) (upper-left corner panel) presents a graphical comparison of the average regular employment rate for the treatment and control groups for the 2001

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reform. The figure shows the job continuity of mothers- whether they remain employed at the same regular job over a four-year period before and after childbirth. The employment rate, which includes those mothers on PL, declines substantially from two years before childbirth to one year after childbirth. Most mothers choose not to remain employed before childbirth (approximately 70% of mothers terminated their job by the childbirth year). The proportion of mothers continuing work one year after childbirth (those who return to work after PL expires) for the 2001 reform is 29.7% for the control mothers and 29.6% for the treatment mothers.[25] The difference between the treatment and control groups does not seem to be significantly large. Panels (2) and (3) in Figure 4 present the regular employment rate for fathers who had a first child and non-mothers in regular employment three years before the treatment or control period. The group differences in job continuity for fathers and non-mothers are also small. The group difference for the 1995 reform is also small, as shown in the appendix.[26]

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[25]31.4% for the second control mothers.

[26]The 1995 reform graph is not directly comparable with the results for the 2001 reform; however, the results are quite similar to those for the 2001 reform. The employment rate drops to 20.4% for the control mothers and 22.2% for the treatment mothers one year after childbirth. Therefore, it seems that the treatment mothers are only slightly more likely to return to work after their PL expires. However, the non-mothers' and fathers' job continuity are also slightly different. Therefore, after differing out the time trends, the group difference is small.

7.2. Difference Results

Table 3 shows the difference results for the 2001 and 1995 reforms. The marginal effects from the probit model shown in the table are from separate regressions conducted for each year surrounding childbirth (marginal effect of *Reform* in model (2)). The coefficients from a linear probability model (LPM) are similar and shown in the appendix. The results reveal that there is no significant difference in job continuity between the two groups even after controlling for the factors that affect employment response-the models with covariates. Mothers do not terminate employment, either earlier or later. The likelihood of returning to work after PL shows no increase after the reform. The elasticity calculated from the D estimates results are almost zero for all models. From Table 3, the percentage change in employment rate one year after childbirth for treatment mothers is 0.39% (assuming 100% PL take-up rate).[27] The employment rate for the control mothers one year after childbirth is 29.7% and a 33.3% increase in income replacement rate, meaning that the estimated elasticity is 0.039. For the second group of control mothers, the estimated elasticity is -0.071. The elasticity for the 1995 reform is 0.276. The signs and significances of the other covariates are as expected. The larger the company, the more likely are mothers to remain employed; the longer the tenure, the more likely are mothers to remain. The manufacturing industry has a higher job continuity rate than the service industry.

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[27]The marginal effect is divided by 64% when a 64% take-up rate is assumed

7.3. Robustness Check

If there are macroeconomic shocks or time trends in the labour market during this period, the difference results do not identify the causal impacts of the reform. I use four methods to investigate the robustness of treatment effects: (1) difference in difference with the fathers having a new child during the period under study; (2) difference in difference with a group of non-childbearing women who have characteristics similar to those of mothers; (3) placebo regression to determine whether any pre-existing trends detrimental to a comparison of the outcomes exists; (4) estimates by education level, and for mothers having their second child.

Table 4 shows the difference-in-difference estimates from model(3), using fathers or non-mothers as the comparison group, running separate regressions. The marginal effects for the interaction terms ($Reform$x$Mothers$) are calculated as in Ai and Norton (2003). All the results show no significant difference in job continuity- not much of a time dimension to worry about when comparing the treatment and control mothers. There might be a mother-specific shock (affecting employment before childbirth) that systematically differs between the treatment and control groups, especially for the 1995 reform. For example, during the 1995 reform period, the spousal tax exemption (the tax deduction for the household head with dependent families) increased slightly. Some companies exert pressure on women to voluntarily quit their jobs once they got married and the intensity of the pressure could vary in accordance with economic conditions. These two time-varying factors could potentially affect the job continuity in a different way between the two groups for those

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women who got married within three years before the birth in the sample. To control for this, I take the differences between two years before childbirth (when the women are not yet pregnant and no policy effect is expected) and one year after childbirth (when all mothers have to return to work) for each group. The estimating models simply replace $Mother$ in model (3) with $After$, which takes the values of 1 for one year after childbirth and 0 for two years before childbirth. I also consider the triple difference with fathers and non-mothers, by including the full set of the second-level interaction with $Mother$ and third-level interaction in the estimation model. The results are shown in Table 5. None of the marginal effects is significantly different from zero, further confirming that the reform has no significant impact on mothers' job continuity.

Table 6 shows the placebo difference estimates and the placebo difference-in-difference estimates. For each reform, I compare two groups of mothers, all of who gave birth before the reform, and check for any differences in employment patterns during that period. The results show that between October 1998-September 1999 and October 1997-September 1998 (row 4 and column 1), the group differences are significantly different from zero. However, after taking into account the possible macroeconomic shocks through a difference-in-difference estimation for the comparison groups of fathers and non-mothers (row 4, column 2 or 3), I found no significant placebo treatment effects. None of the models shows any significant differences in the relative after-childbirth outcomes of the treatment mothers, and the magnitudes of the coefficients are very small. Thus, there seems to be no significant pre-existing

time trends that would be detrimental to a comparison of the treatment and control groups for either reform. Note that the difference in outcome between October 1997-September 1998 and October 1996-September 1997 shows that about 10% more mothers remain employed one year before birth, although job continuity during the birth year and one year after childbirth is not significantly different from zero.

The cost of quitting a job might be higher for mothers with higher-status occupations. To take this heterogeneity into account, I run the models separately by education level and obtain similar results: there are no significant differences between the treatment and control groups. The results for mothers who had their second child also show no significant differences between the two groups. The results are shown in Table 7.

8. Discussion

Time spent with a child is more valuable when children are younger (Klerman and Leibowitz (1997)). PL allows mothers to stay at home with their newborn child surrounding childbirth, and to return to work at their previous employer when the child grows older. Therefore, PL might encourage mothers who previously would have quit their jobs (or taken new jobs, mostly of an non-regular and part-time nature) to stay employed under PL and return to their previous employer. PL promotes job continuity of mothers and helps preserve their job-specific human capital surrounding childbirth. Previous studies have found that an increase in the duration of job protection increases job continuity at the pre-birth employer (Baker

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and Milligan (2008)), and an extension of the maximum duration of paid leave and/or job protection increases time spent with the newborn child (Schonberg and Ludsteck (2007), Baker and Milligan (2008), Hanratty and Trzcinski (2009), Lalive and Zweimuller (2009), Lalive et al. (2013)). Japanese PL reforms, investigated in this paper, (1) raise the rate of income replacement and (2) ask mothers to commit to return to their pre-birth employers in order to take PL. Compared to other countries' reforms, Japanese PL provides stronger financial incentives for mothers to return to their pre-birth employers. However, my results show that the reforms do not impact new mothers' job continuity surrounding childbirth. This is quite puzzling because women and mothers in particular are normally sensitive to taxation and benefits (Blundell and Macurdy (1999), Meghir and Phillips (2010)).[28]

Possible reasons for this are that the reforms increase mothers' marginal wages right after childbirth, but do not change anything beyond that period; that is, the reforms do not make it feasible for mothers to remain employed after PL. There are two major hardships for working mothers in Japan. First, access to childcare is insufficient; it is difficult to find slots in facilities, especially public ones. Public childcare facilities comprise only about half of total childcare facilities, and there are long waiting lists for places in both public and private facilities. Nannies, or alternative childcare services, are not widely available; fewer than 5% of Japanese

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[28]Many studies focus on single mothers and find that increase in a tax credit has a positive effects on their labour force participation (e.g. Eissa and Liebman (1996), Meyer and Rosenbaum (2001), Francesconi and van der Klaauw (2007)).

families use nannies. This means it is difficult for mothers to find someone who can provide childcare on short notice, such as when children are sick. In addition, when public childcare facilities are closed, mothers cannot rely on anyone but themselves to take care of their children. This is challenging for them as illness and accidents are unpredictable, and business needs are sometimes also unpredictable. The lack of childcare supply has been under discussion for many years in Japan but the government has not yet solved the problem. Compounding the issue is that there is still significant social pressure not to use external childcare services, because children are believed to suffer when mothers work. Another difficulty is that traditional family roles are strongly rooted and mothers are less likely to get support from husbands in housework and child-rearing. The share of total hours of housework and childcare performed by Japanese husbands is only 12.5%, which is 25 percentage points lower than in other OECD countries (GEBCO (2007)). In the majority of even two-income households, only mothers take care of both their children and housework. For mothers who work as regular employees in inflexible workplaces, which is the predominant situation in Japan, it is difficult to reconcile work and family responsibilities. Regular employees enjoy life long employment protection in exchange for a high degree of commitment to work and inflexible work hours. Short hours are not fully available, and it is difficult to take a day off on short notice. These cultural and labour market institutions make it infeasible for mothers to remain employed after childbirth.

In order to increase mothers' job continuity in the context of low maternal em-

ployment and limited availability of childcare in Japan, public spending on improving access to public childcare might be necessary. Studies have shown that improved access to public preschools (i.e. an implicit childcare subsidy) increases maternal employment when female employment is low (Berlinski and Galiani (2007) for Argentina in the late 1990s; Gelbach (2002) for the United States in the 1980s), and when both female employment is low and access to childcare is scarce (Nollenberger and Rodriguez-Planas (2011) for Spain in the early 1990s). A reduction in the cost of childcare via childcare subsidies might also be effective, certainly after the childcare supply shortage improves. Examining Spain's income tax reform in 2003, Sanchez-Mangas and Sanchez-Marcos (2008) and Azmat and Gonzalez (2010)) find that tax credits increased employment of mothers with children under the age of three. Public policy which increases childcare subsidies, together with expanding access to public childcare, might be ideal. For example, Lefebvre and Merrigan (2008) finds that public policy offering generous childcare subsidies together with free full-time kindergarten access has a substantial positive effect on the labour supply of mothers in Canada from 1993 to 2000. The importance of both providing sufficient access and reducing the costs of childcare can be seen from countries such as Sweden, the Netherlands and Norway. All three European countries provided mothers with extended access to public childcare alongside high childcare subsidies which resulted in high maternal employment rates. In those countries, further reduction in the price of childcare has led to only small or insignificant changes in mothers' labour supply (Lundin et al. (2008) for Sweden; Bettendorf et al. (2012)

for the Netherlands). This suggests the importance of public policy, especially in low maternal employment countries. It is worthwhile to note that when subsidies are provided regardless of whether one works, there will be a reduction in the labour supply of mothers even in high maternal employment countries (Schone (2004) and Naz (2004) for Norway). This implies that it is important to provide subsidies that are conditional on employment.

During the period of Japan's reforms from 1995-2001, neither access to childcare nor any institutional background changed. Therefore, the increase in PL income replacement might have benefited only those mothers who are lucky to have childcare facilities, nannies, or husbands to care for their children and who would have worked anyway regardless of the amount of income replacement. Also, taking into account the substantial opportunity cost of childbearing and childrearing,[29] the magnitude of the increase in income replacement as a result of the 1995 and 2001 reforms might be too small to impact the job continuity of mothers.[30]

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[29]Kato et al. (2013) found that childbearing will result in a considerable wage loss as well as substantial reduction in promotion odds in Japan. Only 24.5% of workplaces consider leave periods as worked period, and PL periods are not included in the calculation for seasonal salary increases. Severance pay is also affected; 36.3% of workplaces indicate that employees who take leave receive reduced severance compensation (MHLW (2007)). Also, childcare-related absenteeism reduces the promotion probability and wages of mothers.

[30]Yamaguchi (2013) evaluated an ex-ante policy of an increase in income replacement from 50% to 100% and estimated a small increase in job continuity.

9. Conclusion

This study assesses the impact of changes in the PL income replacement rate on mothers' job continuity surrounding childbirth. My focus is on Japan, where the rate of maternal employment is very low, but the PL program is relatively generous. Japanese job-protected PL allows mothers to stay at home until the child reaches the age of exactly one year old, and guarantees mothers' right to return to work to their previous employer. The Japanese government twice increased the PL income replacement rate substantially first in 1995, from 0% to 25%, and then in 2001, from 25% to 40%. Before and after the reforms, the maximum duration for job protection and eligibility for benefits were unchanged. Under the PL program, employed mothers receive income replacement only if they promise to return to work at their previous employers after childbirth. The income replacement is determined in accordance with a mother's average monthly wage in the months prior to childbirth, and is capped. The maximum cap is high enough to provide the income replacement proportional to monthly wages for most mothers. I identified the causal effects of these reforms by comparing the job continuity of regularly employed women who gave birth to their first child before and after each reform. The treatment and control groups are randomly assigned according to the birth date of their first child, which biologically cannot be perfectly controlled. Because the government implemented the reforms shortly after the policy was amended, the new mothers were unable to anticipate the reform and control the timing of their conception and childbirth to

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qualify for the reform. Therefore, the framework of this study is a quasi experiment.

The outcome variable was job continuity surrounding childbirth, which takes a value of 1 if the mother remains employed with the same employer, and 0 otherwise. I investigated the outcome from two years before childbirth to one year after childbirth. In this way, I was able to capture those mothers who remained employed before as well as after their PL expires (i.e. one year after the birth). My results suggest that the probability of continuing regular employment surrounding childbirth is not significantly different between the treatment mothers and those who did not qualify for the reform. The results do not change even after controlling for the effect of macroeconomic shocks by a difference-in-difference estimate with fathers or non-childbearing women as the comparison group. The results from placebo regression also confirm that there are no pre-existing trends to harm the comparison. I also find no significant effects when I run regression by education levels or by mothers with a second child.

Japan's PL reforms increased mothers' marginal wages only in the childbirth year; however, as discussed in the previous section, Japanese mothers face considerable hardships with childrearing when they return to work. There is a severe supply shortage of childcare, with long waiting lists in all regions of the country; thus, most expectant mothers find it infeasible to remain employed after childbirth. The lack of alternative childcare, such as nannies, and lack of help from their own husbands are also issues that compound Japanese mothers' hardship. Taking all this into account, it is understandable that the two reforms have not had an impact on the

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job continuity of mothers. In the current Japanese labour market, the only mothers who can stay employed are those lucky to find someone to care for their child (e.g. a childcare facility, husband, or nanny). The government has spent an enormous amount of money on the reforms, but they are not cost-effective. The Japanese reforms are a good example of why family policies for new mothers do not promote job continuity if they are not accompanied by a simultaneous expansion of access to public, private, and alternative childcare for mothers.

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APPENDIX
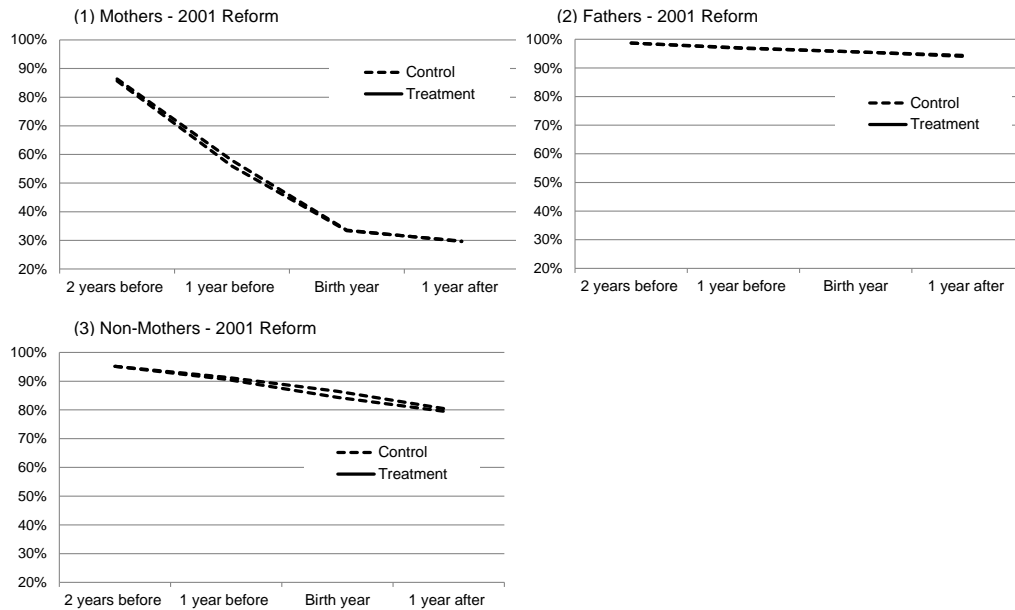
Figure 4: Job Continuity around the Time of Giving Birth for the Treatment and Control Groups: the 2001 Reform

Data are from the ESS 2002. Rates are calculated for mothers and fathers who were employed as regular employees three years before the childbirth, and for non-mothers with similar characteristics as mothers. The treatment group comprises mothers who gave birth and fathers who had a newborn child between October 2000 and September 2001; the control group comprises mothers who gave birth and fathers who had a newborn child between October 1999 and September 2000. Means are weighted with the sampling weights.

	2001 Reform				1995 Reform	
	Treatment vs. Control		Treatment vs. Second Control		Treatment vs. Control	
2 years before birth	0.0087	0.0092	0.0071	0.0083	-0.0038	-0.0009
	(0.0173)	(0.0171)	(0.0169)	(0.0167)	(0.0178)	(0.0176)
1 year before birth	0.0167	0.0195	0.0387	0.0418 *	-0.0023	-0.0012
	(0.0247)	(0.0237)	(0.0243)	(0.0235)	(0.0216)	(0.0209)
Birth year	0.0015	0.0070	-0.0057	-0.0030	-0.0129	-0.0165
	(0.0238)	(0.0212)	(0.0237)	(0.0216)	(0.0176)	(0.0156)
1 year after birth	-0.0020	0.0039	-0.0164	-0.0133	0.0191	0.0153
	(0.0232)	(0.0205)	(0.0232)	(0.0209)	(0.0171)	(0.0149)
Covariates	No	Yes	No	Yes	No	Yes
Sample size	2691		2722		3443	

Table 3: Difference Estimates

*Note: The outcome variable is job continuity, which takes a value of 1 if the individual remains as a regular employee with pre-birth employer and 0 otherwise. Estimates are average marginal effect of Reform in model (2). The 2001 treatment group comprises mothers who gave birth between October 2000 and September 2001; the 2001 control group comprises mothers who gave birth between October 1999 and September 2000; the second 2001 control group comprises mothers who gave birth between October 1998 and September 1999; the 1995 treatment group comprises mothers who gave birth between October 1995 and September 1996; the 1995 control group comprises those who gave birth between October 1993 and September 1994. Robust standard errors are in parentheses. Means are weighted with the sampling weights. ***p>0.01, **p>0.05, and *p>0.1.*

	2001 Reform				1995 Reform	
	Treatment vs. Control		Treatment vs. Second Control		Treatment vs. Control	
	Fathers	Non-Mothers	Fathers	Non-Mothers	Fathers	Non-Mothers
2 years before birth	0.0094	0.0103	0.0097	0.0172	-0.0054	0.0024
	(0.0173)	(0.0201)	(0.0169)	(0.0197)	(0.0175)	(0.0197)
1 year before birth	0.0187	0.0269	0.0476	0.0436	-0.0152	0.0232
	(0.0247)	(0.0279)	(0.0240)	(0.0278)	(0.0221)	(0.0252)
Birth year	0.0094	0.0287	0.0073	0.0180	-0.0363	-0.0088
	(0.0236)	(0.0285)	(0.0233)	(0.0295)	(0.0185)	(0.0230)
1 year after birth	0.0023	0.0147	-0.0017	0.0329	-0.0084	-0.0208
	(0.0235)	(0.0294)	(0.0233)	(0.0304)	(0.0185)	(0.0233)
Sample size	6806	5382	6793	5444	8073	6886

Table 4: Difference in Difference Estimates

Note: Note: The outcome variable is job continuity, which takes a value of 1 if the individual remains as a regular employee with pre-birth employer and 0 otherwise. Estimates are marginal effects for the interaction term from the probit model (3), and include covariates. Robust standard errors clustered by group are in parentheses. The critical values for 1%, 5% and 10% significance levels are 9.92, 4.30 and 2.92 (t-distribution with 2 degrees of freedom). Means are weighted with the sampling weights. ***p>0.01, **p>0.05, and *p>0.1.

| | 2001 Reform | | | | | | 1995 Reform | | |
| | Treatment vs. Control | | | Treatment vs. Second Control | | | Treatment vs. Control | | |
	Before after Birth DD	Fathers DDD	Non-mothers DDD	Before after Birth DD	Fathers DDD	Non-mothers DDD	Before after Birth DD	Fathers DDD	Non-mothers DDD
	-0.0055	-0.0075	-0.0058	-0.0217	-0.0128	-0.0271	0.0179	-0.0026	0.0359
	(0.0232)	(0.0255)	(0.0298)	(0.0233)	(0.0248)	(0.0293)	(0.0204)	(0.0210)	(0.0253)
Sample size	2691	6806	5382	2722	6793	5444	3443	8073	6886

Table 5: Before and After Childbirth Difference in Difference Estimates

Note: The outcome variable is job continuity, which takes a value of 1 if the individual remains as a regular employee with pre-birth employer and 0 otherwise. Estimates are marginal effects for the interaction term from the probit model and include covariates. Robust standard errors clustered by group are in parentheses. The critical values for 1%, 5% and 10% significance levels are 5.84, 3.18 and 2.35 (t-distribution with 3 degrees of freedom). Means are weighted with the sampling weights. $***p>0.01$, $**p>0.05$, and $*p>0.1$.
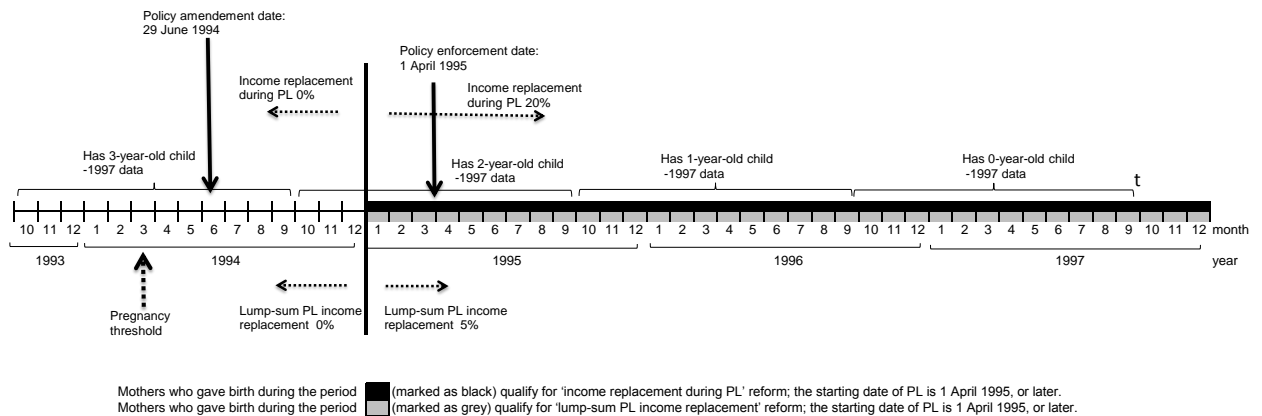

Figure A .1: Identification Strategy and the Data Structure of the 1995 Reform

Note: Data are from the ESS 1997. Based on the child's age in the 1997 data, the date of childbirth (horizontal axis) is identified. The policy was amended in June 1994 and enacted in April 1995.

	2001 Reform						1995 Reform					
	Oct.1998-Sep.1999 vs. Oct.1997-Sep.1998			Oct.1997-Sep.1998 vs. Oct.1996-Sep.1997			Oct.1993-Sep.1994 vs. Oct.1992-Sep.1993			Oct.1992-Sep.1993 vs. Oct.1991-Sep.1992		
	Difference	Fathers DD	Non-Mothers DD	Difference	Fathers DD	Non-Mothers DD	Difference	Fathers DD	Non-Mothers DD	Difference	Fathers DD	Non-Mothers DD
2 years before birth	0.050 ***	0.043	0.041	0.035 *	0.038	0.036	0.021	0.021	0.020	-0.005	-0.002	-0.024
	(0.018)	(0.018)	(0.021)	(0.020)	(0.020)	(0.024)	(0.018)	(0.018)	(0.022)	(0.019)	(0.019)	(0.021)
1 year before birth	0.018	0.012	0.023	0.109 ***	0.114 **	0.099 *	0.010	0.017	0.003	0.005	0.010	-0.007
	(0.023)	(0.024)	(0.026)	(0.022)	(0.024)	(0.028)	(0.020)	(0.021)	(0.025)	(0.021)	(0.021)	(0.025)
Birth year	0.045 **	0.044	0.044	0.020	0.023	0.020	0.022	0.027	0.028	-0.001	0.010	-0.035
	(0.020)	(0.022)	(0.027)	(0.019)	(0.021)	(0.027)	(0.015)	(0.018)	(0.023)	(0.015)	(0.018)	(0.023)
1 year after birth	0.042 **	0.050	0.024	0.013	0.012	0.005	0.018	0.021	0.033	-0.007	-0.001	-0.028
	(0.019)	(0.022)	(0.028)	(0.018)	(0.021)	(0.027)	(0.014)	(0.018)	(0.024)	(0.014)	(0.017)	(0.024)
Sample size	2788	6691	5576	2702	6341	5404	3408	7926	6816	3192	7486	6384

Table 6: Placebo Difference Estimates and Difference-in-Difference Estimates

Note: The outcome variable is job continuity, which takes a value of 1 if the individual remains as a regular employee with pre-birth employer and 0 otherwise. 'Difference' columns: estimates are average marginal effect of $Reform$ in model (2), and robust standard errors are in parentheses; 'Fathers DD' and 'Non-Mothers DD' columns: estimates are marginal effects for the interaction term from the probit model (3), and robust standard errors clustered by group are in parentheses. All regressions include covariates. The critical values for 1%, 5% and 10% significance levels for the DD models are 9.92, 4.30 and 2.92 (t-distribution with 2 degrees of freedom). Means are weighted with the sampling weights.***p>0.01, **p>0.05, and *p>0.1.

	2001 Reform			1995 Reform		
	First Child		Second Child	First Child		Second Child
	University and College Graduates	High School Graduates		University and College Graduates	High School Graduates	
Difference						
Birth year	-0.0118	0.0308	0.0048	0.0068	-0.0351 *	-0.0459
	(0.0286)	(0.0309)	(0.0320)	(0.0255)	(0.0186)	(0.0280)
1 year after birth	-0.0022	0.0118	0.0059	0.0365	-0.0009	-0.0171
	(0.0276)	(0.0300)	(0.0320)	(0.0249)	(0.0175)	(0.0277)
Sample size	1463	1228	1260	1535	1908	1391
Fathers DD						
Birth year	0.0007	0.0215	0.0243	-0.0075	-0.0610	-0.0550
	(0.0323)	(0.0344)	(0.0344)	(0.0282)	(0.0238)	(0.0304)
1 year after birth	-0.0012	0.0077	0.0225	0.0203	-0.0327	-0.0329
	(0.0322)	(0.0341)	(0.0348)	(0.0280)	(0.0234)	(0.0304)
Sample size	3597	3209	4678	3755	4318	5149
Non-Mothers DD						
Birth year	0.0227	0.0327	0.0321	-0.0054	-0.0135	-0.0262
	(0.0385)	(0.0408)	(0.0425)	(0.0352)	(0.0296)	(0.0378)
1 year after birth	0.0151	0.0135	0.0137	-0.0157	-0.0258	-0.0299
	(0.0391)	(0.0433)	(0.0448)	(0.0360)	(0.0295)	(0.0381)
Sample size	2926	2456	2520	3070	3816	2782

Table 7: Estimates by Education Level and for Mothers Having Their Second Child

Note: The outcome variable is job continuity, which takes a value of 1 if the individual remains as a regular employee with pre-birth employer and 0 otherwise. 'Difference' rows: estimates are average marginal effect of $Reform$ in model (2), and robust standard errors are in parentheses; 'Fathers DD' and 'Non-Mothers DD' rows: estimates are marginal effects for the interaction term from the probit model (3), and robust standard errors clustered by group are in parentheses. All regressions include covariates. The critical values for 1%, 5% and 10% significance levels for the DD models are 9.92, 4.30 and 2.92 (t-distribution with 2 degrees of freedom). Means are weighted with the sampling weights. ***p>0.01, **p>0.05, and *p>0.1.

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

Table A .1: List of the Parental Leave Reforms

Note: Before taking PL mothers can take ML, which is 42 days before and 56 days after the birth of the child. 'During PL' is the rate of income replacement during the leave (monthly stipend), 'lump-sum' is paid six months after return to work, the amount is provided for the length of leave.
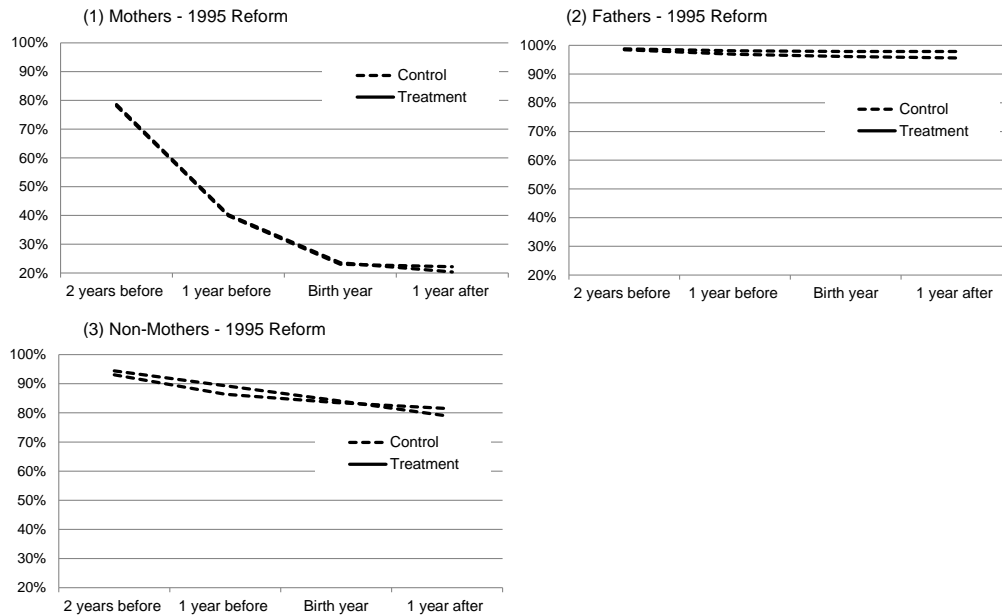
Figure A .2: Job Continuity around the Time of Giving Birth for the Treatment and Control Groups: the 1995 Reform

Note: Data are from the ESS 1997. Rates are calculated for mothers and fathers who were employed as regular employees three years before the childbirth, and for non-mothers with similar characteristics as mothers. The treatment group comprises mothers who gave birth and fathers who had a newborn child between October 1995 and September 1996; the control group comprises mothers who gave birth and fathers who had a newborn child between October 1993 and September 1994. Means are weighted with the sampling weights.

LPM	2001 Reform				1995 Reform	
	Treatment vs. Control		Treatment vs. Second Control		Treatment vs. Control	
2 years before birth	0.0086	0.0097	0.0071	0.0084	-0.0038	-0.0019
	(0.0173)	(0.0172)	(0.0169)	(0.0168)	(0.0178)	(0.0177)
1 year before birth	0.0167	0.0207	0.0387	0.0432 *	-0.0023	-0.0024
	(0.0247)	(0.0238)	(0.0243)	(0.0236)	(0.0216)	(0.0209)
Birth year	0.0015	0.0075	-0.0057	-0.0034	-0.0129	-0.0154
	(0.0238)	(0.0214)	(0.0237)	(0.0217)	(0.0176)	(0.0157)
1 year after birth	-0.0020	0.0041	-0.0164	-0.0140	0.0191	0.0163
	(0.0232)	(0.0206)	(0.0232)	(0.0210)	(0.0171)	(0.0151)
Covariates	No	Yes	No	Yes	No	Yes
Sample size	2691		2722		3443	

Table A .2: Difference Estimates (Linear Probability Model)

Note: The outcome variable is job continuity, which takes a value of 1 if the individual remains as a regular employee with pre-birth employer and 0 otherwise. Estimates are coefficients from the linear probability model. The 2001 treatment group comprises mothers who gave birth between October 2000 and September 2001; the 2001 control group comprises mothers who gave birth between October 1999 and September 2000; the second 2001 control group comprises mothers who gave birth between October 1998 and September 1999; the 1995 treatment group comprises mothers who gave birth between October 1995 and September 1996; the 1995 control group comprises those who gave birth between October 1993 and September 1994. Robust standard errors are in parentheses. Means are weighted with the sampling weights. ***p>0.01, **p>0.05, and *p>0.1.
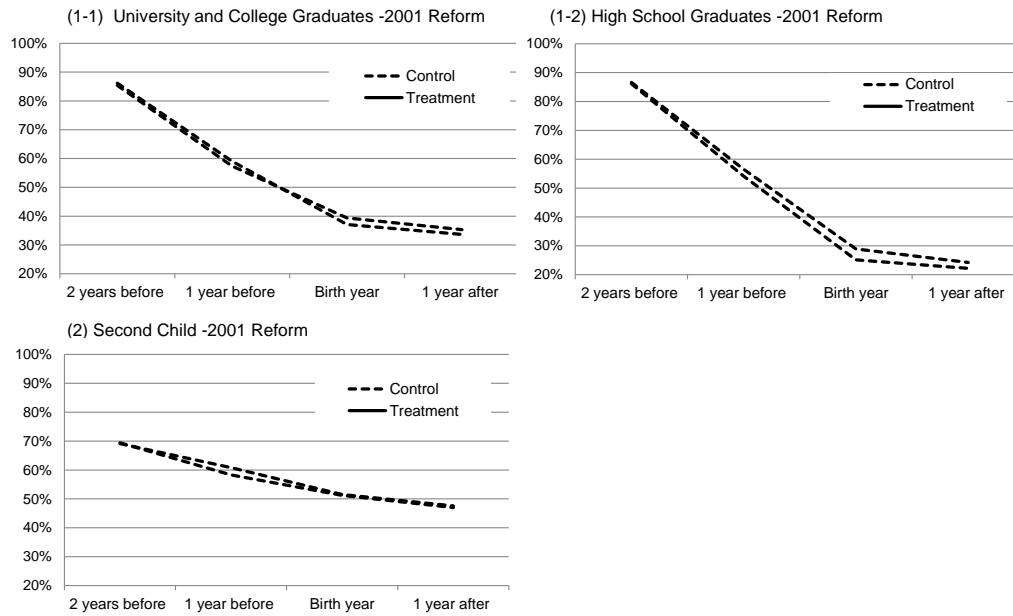
Figure A .3: Job Continuity around the Time of Giving Birth for the Treatment and Control Groups: the 2001 Reform- by Education Level (1), and Mothers Having Their Second Child (2)

Note: Data are from the ESS 2002. (1-1) and (1-2): Rates are calculated for mothers who were employed as regular employees three years before the childbirth by education level. (2): Rates are calculated for mothers who having their second child and who were employed as regular employees three years before the childbirth. Treatment group: mothers who gave birth between October 2000 and September 2001; Control group: mothers who gave birth between October 1999 and September 2000. Means are weighted with the sampling weights.

		2001 Reform			1995 Reform		
		Control	Treat-ment	t	Control	Treat-ment	t
Education	University / College Graduates	0.542	0.547	0.02	0.427	0.479	2.49
	High School Graduates	0.458	0.453	0.02	0.573	0.521	2.49
Pre-birth Characteristics (*3 Years Before*)							
Tenure (years)		5.498	5.384	0.19	4.372	4.608	1.02
Company size	Less than 30 employees	0.204	0.198	0.05	0.270	0.275	0.04
	30-299 employees	0.264	0.257	0.04	0.257	0.218	2.08
	More than 300 employees	0.343	0.369	0.53	0.306	0.317	0.11
	Public office	0.189	0.176	0.22	0.167	0.189	0.82
Industry	Manufacturing	0.227	0.273	1.84	0.263	0.256	0.05
	Service	0.773	0.727	1.84	0.737	0.744	0.05
Sample size		623	637		741	650	

Table A .3: Means of Key Characteristics of Mothers Having Their Second Child

Note: For 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 to their second child between October 1999 and September 2000. 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 to their second child between October 1993 and September 1994. t is the test of equality of means between two groups. Means are weighted with the sampling weights.