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Essays in Labour Economics: Empirical Evaluations of Japanese Family Policies

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Essays in Labour Economics: Empirical Evaluations of Japanese Family Policies

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Abbreviations

CSR	**C**orporate **S**ocial **R**esponsibility
DD	**D**ifference-in-Difference
ESS	**E**mployment **S**tatus **S**urvey
JLPS	**J**apanese **L**ife Course **P**anel **S**urvey
PL	**P**arental **L**eave
SMEs	**S**mall and **M**edium-sized Enterprises
ML	**M**aternity **L**eave

Chapter 1

Introduction

A number of studies have examined family policies, especially in European and North American countries, and the majority have concluded that their particularities and effects vary according to the prevailing economic environment, culture, and labour market institutions. The Japanese labour market is different from those in other countries in terms of its work hours (i.e. full-time and part-time), workplace flexibility, and corporate culture. Because of the particular institutions in the Japanese labour market, policies that have successfully been implemented in other countries rarely work in Japan. It is therefore important to understand these context-specific institutions when we evaluate family policies.

This dissertation empirically evaluates how Japanese family policies influence employment and work hours. In particular, I investigate the factors behind the low employment rates of women, especially mothers, and examine how family policies affect their behaviour in the Japanese labour market. Further, because

the long work hours of men are also an issue, I examine the effects of work hour regulations on their labour supply. The results clarify the fundamental causes of the low employment rate of females and long work hours of Japanese workers and offer suggestions for how family policies should be formulated in particular institutions.

Chapter 2 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.

Chapter 3 examines both the supply and demand effects of introducing the short work hour provisions. Since June 2010, firms in Japan with more than 100 employees have been required to formulate a short work hour provisions for use by employees with children under the age of three. Qualified employees are allowed to work six hours per day instead of eight, and they are also exempt from overtime work. The new scheme makes it possible for new parents to manage work and childcare demands while maintaining their generous employee benefits plan; in this way, the scheme can contribute to job continuity among new parents. On the other hand, since there are fixed labour costs independent of work hours, the short work hour provisions will increase the labour cost per unit of time. Based

on corporate social responsibility data from Japanese firms, I examine the effects of scheme introduction on hiring, job turnover, and overtime hours. The causal effects are identified by comparing the outcomes of firms that already had the scheme in place before the reform to those of firms that had not. Contrary to expectations, the results show an increase in the proportion of newly hired female college graduates among firms that had introduced the provisions after the reform.

Chapter 4 assesses how the increase in the overtime premium paid to workers that carry out excessive overtime influences work hours and the incidence of overtime. In April 2010, the Japanese government reformed the Labour Standards Act, which increased the overtime premium that companies have to pay their employees from 25% to 50%. This reform, which only applied to overtime of more than 60 hours per month and workers in large firms, generated an exogenous variation in the marginal cost to employers of assigning extra overtime. Based on data derived from the Japanese Life Course Panel Survey conducted by the University of Tokyo from 2007 to 2013, the presented findings suggest that despite the overtime premium doubling, there has been no change in work hours or in the incidence of overtime since the introduction of the reform, suggesting the prevalence of unpaid overtime in Japan.

Chapter 5 concludes this dissertation.

Chapter 2

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

2.1 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 is required to 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

[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.

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.

2.2 Theoretical Framework

New mothers derive utility U from consumption c and leisure l; $U = U(c, l)$. They 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.[2] 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

[2]The theoretical setting comes from Killingsworth and Heckman (1986), Klerman and Leibowitz (1997) and Boeri and van Ours (2013).

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 at $t = 1$; that is, $h_1 > 0$, where h_1 denotes work hours at $t = 1$. 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:

$$\begin{aligned} U_{w,0}(N_0 + r \cdot I[h_1 > 0], l_0) + U_{w,1}(W_1(T_1 - l_1) + N_1 - cc_1 \cdot I[h_1 > 0], l_1) \\ -U_{n,0}(N_0, l_0) - U_{n,1}(N_1, l_1) > 0 \end{aligned} \tag{2.1}$$

where N is non-labour income, W is hourly wage, T is total time allocation and cc is cost of childcare.[3] Let $I(A)$ denote the usual indicator function, which assumes the value one if A holds true, zero otherwise. The introduction of r increases the incentive to work. When 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

[3]The subscripts are suppressed for brevity.

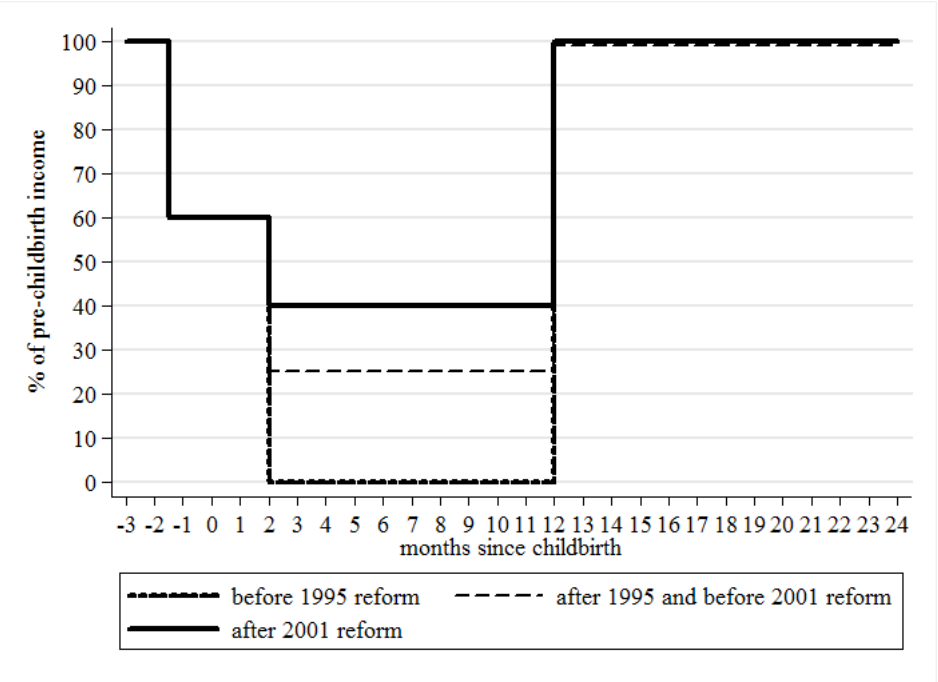

FIGURE 2.1: Mothers' Monthly Income Surrounding Childbirth

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.

to work, then $r = 0$. Note that if a mother quits the current job and takes a new job, then $r = 0$.[4] 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 $t = 0$.

[4]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.

The magnitude of increase in r is shown in Figure 2.1. 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 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, I assume that both the monthly and lump-sum PL income replacement payments depend on the mothers' pre-birth decision.[5] For this reason, I focus on the effect of *total* income replacement on the job continuity of mothers.

2.3 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.[6] Previous Studies

[5]The validity of the assumption is discussed in the Section 2.4.2.

[6]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.

on PL and mothers' labour supply were conducted primarily using North American and European data. 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.[7] 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.[8] Schonberg 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.[9] Waldfogel et al. (1999) find that leave coverage

[7]Most previous studies in Japan focused on the socio-economic characteristics of persons who took PL. For example, Abe (2002) using data from the Japanese Panel Survey of Consumers 1993-2003, shows that women who work in large-scale companies and employees with high seniority benefits from the system are more likely to take PL. Higuchi (1994), Suruga and Zhang (2002) and Shigeno and Matsuura (2003) indicate that employees who work at a company with PL have higher marriage and fertility rates than employees who do not enjoy this benefit. Few studies have reviewed the PL policy and its effect on women's labour participation. Sato and Ma (2007), using KHPS's first wave of data in 2004, reveal that women who have PL coverage have a higher return-to-work rate than those who are employed by companies without PL. However, Sato and Ma (2007) results might suffers from potential selection bias since women who are willing to return to work may look for jobs proving PL. Morita (2005) reviewed the effect of PL on company's labour demand of women and found that the 1995 reform reduced women's new employment in less than 30 workers workplaces.

[8]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.

[9]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.

increase job continuity in the United States, Britain and Japan.

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)).[10] 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 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.[11] 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

[10]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.

[11]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.

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.[12] 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. 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.

[12]Women and mothers in particular are normally sensitive to taxation and benefits, because women are substituting among market work, home production and leisure while men are substituting between market work and leisure (Blundell and Macurdy (1999), Blau and Kahn (2007), Meghir and Phillips (2010)).

2.4 Parental Leave Policy in Japan

2.4.1 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). A list of recent reforms are in Table 2.1.[13] Under this Act, mothers can take PL of up to 10 months after ML (which is 42 days before and 56 days after childbirth).[14] The leave starting date can be adjusted based on the expected delivery date, therefore mothers cannot perfectly plan the timing of their leave. Mothers have to return to work by the day after the child reaches exactly 12 months of age.[15] The actual PL take-up rate is unknown. Kosei-rodosho (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

[13]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 (see Asai (2014b) for details.). 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.

[14]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.

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

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.[16] 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, averages 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 (Kosei-rodosho (2002)).

2.4.2 Return to Work Commitment

A new mother needs to 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

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

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 2.1: List of the Parental Leave Reforms

Note: Mothers can take PL of up to 10 months after ML which is 42 days before and 56 days after childbirth. '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. The eligibility for PL was expanded to include non-regular employees, and the maximum duration of PL was extended to 16 months in 2005, however before and after the 1995 and 2001 reforms, the maximum duration for job protection and eligibility for benefits were unchanged.

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 and social pressures in Japanese companies, mothers would find it very difficult not to return after making a commitment to return. According to the Employment Insurance Report, the number of mothers who started to receive PL payment between June 2001 and March 2002 was 77,944, 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%.[17] Furthermore, the psychological cost of not returning is high. If a mother quits her job during PL, her company terminates her contract immediately, and thereafter she will not receive income replacement.[18] 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. Taking account of these factors, only rarely does a mother decide not to return to work at the end of her PL.

2.5 Data

2.5.1 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,

[17]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.

[18]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 and who have the post-birth employment contract with their company. For this reason, if a mother quits her job during PL, she is no longer qualified for the benefit.

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.[19]

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 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 2.2; 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

[19]Households containing more than eight persons or with more than three household members the same age are excluded from the re-sample.

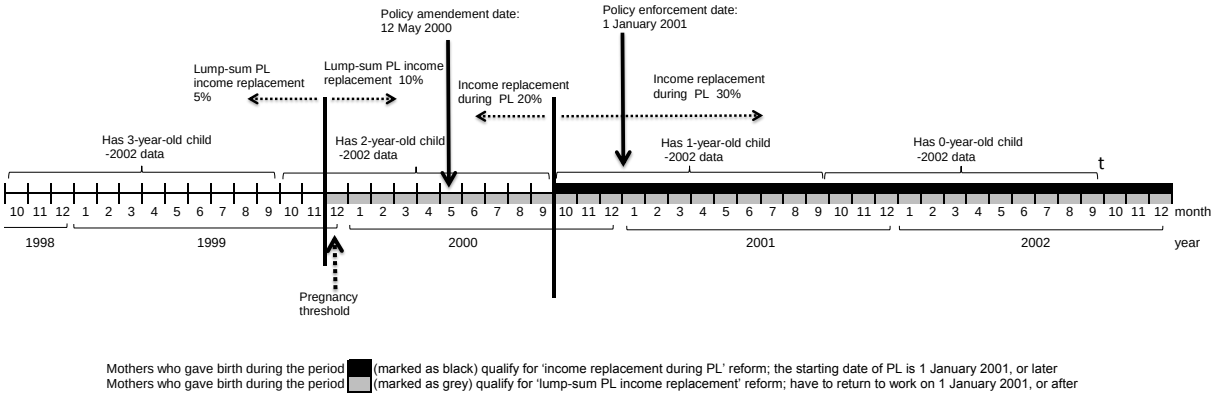

FIGURE 2.2: Identification Strategy and Data Structure of the 2001 Reform

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. 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. The policy was amended in May 2000 and enacted in January 2001. The figure for the 1995 Reform is shown in the appendix.

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 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 not very common (Kosei-rodosho (2011a), Kosei-rodosho (2011b)). Therefore, by including data on education level and tenure, we can capture the data on age.[20] 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.

[20] 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.

2.5.2 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 2.3(1) shows the employment-to-population ratio and regular-employment-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 2.3(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 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.

2.6 Identification Strategy

2.6.1 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

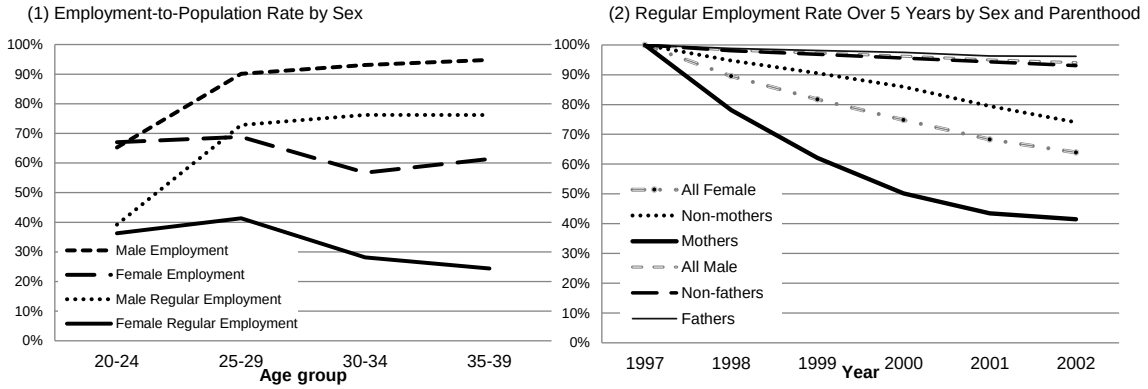

FIGURE 2.3: Employment-to-Population Rate and Job Continuity

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.

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 2.2. 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 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

	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 2.2: Empirical Design

Note: The eligibility of mothers under the reforms is defined by the timing of childbirth. 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.

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 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.

2.6.2 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 mothers of the control group and those of the treatment group are not statistically different, and (3) whether the reform induced more women to give birth.

Figure 2.2 shows the reform amendment date, the enforcement date, and the data 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,[21] 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 2.2). 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

[21]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)).

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 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,[22] 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.[23]

If there were self-selection into the treatment group, the means of the treatment and control groups could be significantly different. Table 2.3 shows the means of the key characteristics of the treatment and control mothers and the t-statistics for the 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

[22]Nikkei, Yomiuri, Asahi and Mainichi

[23]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.

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,[24] 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.[25] I also checked the number of eligible mothers, that is, regular 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.

[24]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.

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

		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 2.3: 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.

2.6.3 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.[26] Mothers with regular employment three years before childbirth will have more than one year of tenure before the birth and qualify to receive PL

[26]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.

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.[27] I estimate the following probit equation for each reform separately:

$$P(E_{it} = 1) = \Phi(\rho \cdot Reform_t + X_{it}\beta) \tag{2.2}$$

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).[28] 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. $Reform_t$ represents a 0/1 indicator of reform eligibility of the childbirth group t and captures the average effect of the reform- t indexes the childbirth group (1 if treatment group, 0 if control group). 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

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

[28] For the definition of the childbirth group t, see Table 2.2. The eligibility of mothers under the reforms is defined by the timing of childbirth.

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;[29] it controls for the observable characteristics affecting the employment response. I separately estimate regressions for comparison of (1)the treatment and 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.[30] 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.

[29]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.

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

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

$$P(E_{itm} = 1) = \Phi(\beta_1 \cdot Reform_t + \beta_2 \cdot Mother_m + \gamma \cdot Reform_t \text{x} Mother_m + X_{itm}\beta) \tag{2.3}$$

where $Mother$ takes a value of 1 for mothers and 0 otherwise, and represents the group fixed effect. t indexes the childbirth group (1 if treatment group, 0 if control group), and m indexes family status (1 if mothers, 0 if fathers/non-mothers). The father or non-mother models run separately. γ captures all 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.(for difference-in-difference model, see for instance Card and Krueger (1994); Gruber (1994); Angrist and Krueger (1999); Angrist and Pischke (2008))

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 (2.3) with G=4. The critical values for the 1%, 5%, and 10% significance levels are 9.92, 4.30 and 2.92. For model (2.2) 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.

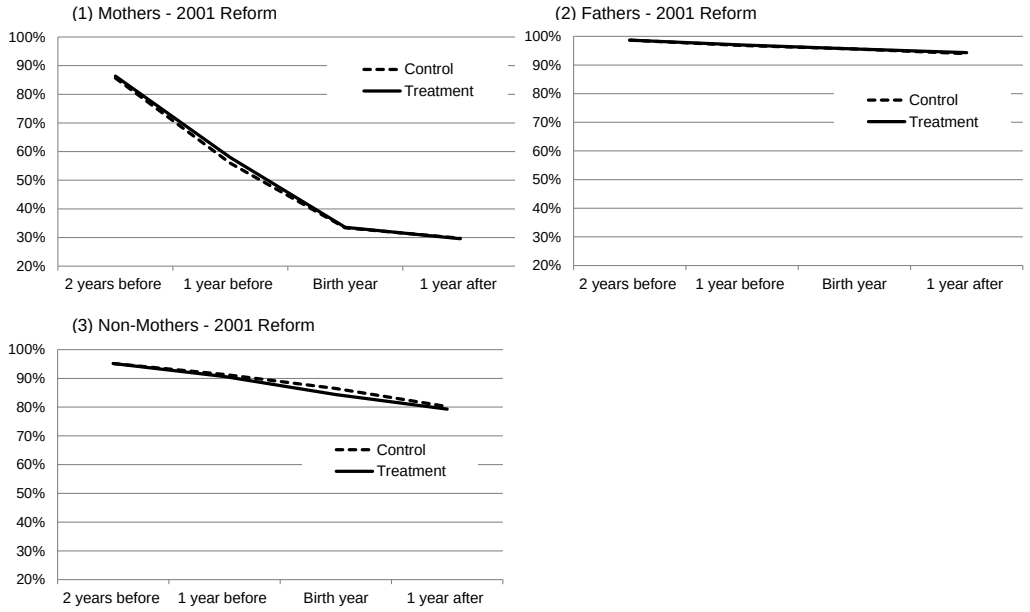

FIGURE 2.4: Job Continuity around the Time of Giving Birth for the Treatment and Control Groups: the 2001 Reform

Note: 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.

2.7 Results

2.7.1 A Graphical Comparison of Employment around the Time of Giving Birth

Figure 2.4(1) (upper-left corner panel) presents a graphical comparison of the average regular employment rate for the treatment and control groups for the 2001 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.[31] The difference between the treatment and control groups is negligible. Panels (2) and (3) in Figure 2.4 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.[32]

[31] 31.4% for the second control mothers.

[32] 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.

	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 2.4: 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 (2.2). The marginal effects are from separate regressions conducted for each year surrounding childbirth. 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.*

2.7.2 Difference Results

Table 2.4 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 (2.2)). 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 (i.e. job continuity at $t = 1$) shows no increase after the reform. The elasticity

calculated from the D estimates results are almost zero for all models. From Table 2.4, the percentage change in employment rate one year after childbirth for treatment mothers is 0.39% (assuming 100% PL take-up rate).[33] 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.

2.7.3 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.

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

	2001 Reform				1995 Reform	
	Treatment vs. Control		Treatment vs. Second Control		Treatment vs. Control	
Comparison group	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 2.5: 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 (2.3), and include covariates. The marginal effects are from separate regressions conducted for each year surrounding childbirth. 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 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.*

Table 2.5 shows the difference-in-difference estimates from model(2.3), using fathers or non-mothers as the comparison group, running separate regressions. The marginal effects for the interaction terms ($Reform \times Mothers$) 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.[34] 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 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 (2.3) 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.[35] The results are shown in Table 2.6. None of the marginal effects is significantly different from zero, further confirming that the reform has no significant impact on mothers' job continuity -not much of a mother-specific shock (affecting employment before childbirth) to worry about

[34]For spousal tax exemption and female labour supply, see for instance Yamada (2011), Akabayashi (2006), and Yokoyama (2013)

[35]I estimate the following triple difference equation:

$$P(E_{itma}=1)=\Phi(\beta_1\cdot Reform_t+\beta_2\cdot Mother_m+\beta_3\cdot After_a+\beta_4\cdot Reform_t\times Mother_m$$
$$+\beta_5\cdot Reform_t\times After_a+\beta_6\cdot Mother_m\times After_a+\gamma\cdot Reform_t\times Mother_m\times After_a+X_{itma}\beta_8)$$

where t indexes the childbirth group (1 if treatment group, 0 if control group), m indexes family status (1 if mothers, 0 if fathers/non-mothers), and a indexes years (1 if one year after childbirth, 0 if two years before childbirth).

	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 2.6: 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 (γ from the triple difference equation in footnote 35). Estimates come from regressions that control for mothers' characteristics (education level, job tenure, size of the company, and industry) measured three years before childbirth. 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. Before after Birth DD: To control for mother-specific shock (affecting employment before childbirth) that systematically differs between the treatment and control groups, 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 are taken. Fathers DDD: difference in difference with the fathers having a new child during the period under study. Non-mothers DDD: difference in difference with a group of non-childbearing women who have characteristics similar to those of mothers. 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.*

when comparing the treatment and control mothers.

Table 2.7 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

	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 2.7: 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. The marginal effects are from separate regressions conducted for each year surrounding childbirth. 'Difference' columns: estimates are average marginal effect of $Reform$ in model (2.2), 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 (2.3), 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.*

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.

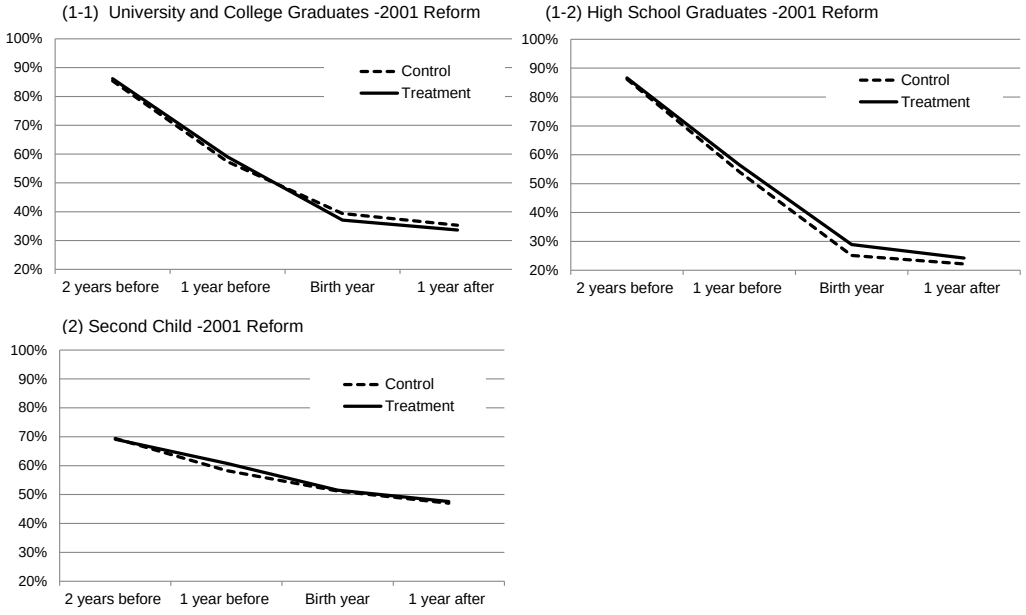

FIGURE 2.5: Job Continuity around the Time of Giving Birth for the Treatment and Control Groups: the 2001 Reform- by Education Level (1), and Mothers Having Their Second Child (2)

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.

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 Figure 2.8.

	2001 Reform			1995 Reform		
	First Child			First Child		
	University and College Graduates	High School Graduates	Second Child	University and College Graduates	High School Graduates	Second Child
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 2.8: 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 (2.2), 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 (2.3), and robust standard errors clustered by group are in parentheses. The marginal effects are from separate regressions conducted for each year surrounding childbirth. All regressions include covariates. 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 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. 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.

2.8 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 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), Blau and Kahn (2007), Meghir and Phillips (2010)).[36]

[36] A number of existing studies focused on single mothers or married women and found that an increase in a tax credit had a positive effects on their labour force participation (e.g. Eissa and Liebman (1996), Meyer and Rosenbaum (2001), Francesconi and van der Klaauw (2007)). The elasticities were larger for single mothers and married women than other groups and was especially large when female labour force participation was low (Blundell and Macurdy (1999), Olivier et al. (2014)). Olivier et al. (2014) found that the elasticities estimate for married women in France, Finland, Portugal, Sweden, the NMS, the UK and the US ranged from 0.1 to 0.2, while they were larger in countries with low female labor force participation-such as Ireland, Greece and Spain- ranged from 0.4 to 0.6. In Japan, female employment rate has been relatively low

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 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

in the recent decades and a considerable proportion of married women work part-time as secondary earners in households. The Japanese spousal tax exemption system provides low-income secondary earners in households greater tax deduction (the amount of deduction decreases proportionally with spousal income) which makes them sensitive to taxation and benefits (Yamada (2011), Akabayashi (2006), and Yokoyama (2013)). For this reason, in Japan the elasticities of married women, especially those of mothers tend to be larger than in other countries, ranging from 0.03 to 0.81. The elasticities were smaller in the early 1990s (Akabayashi (2006)), but became larger after the mid-1990s (Yamada (2011)). Although a detailed investigation on the changing trends of elasticities of female labour force participation in Japan is beyond the scope of this study, the elasticities were likely smaller in the early 1990s than in the late 1990s and afterwards because an entry to the labour force was not a feasible option for many married women due to lack of desirable employment opportunities and shortage of affordable childcare services. The variation in elasticities found in the above-mentioned previous studies is thought to be due largely to differences in types of data used for their analyses, groups those analyses focused, the periods of observation, and methods used for their estimation. It is therefore important to net out possible measurement differences when we compare elasticities of female labour force participation estimated by various studies (Olivier et al. (2014)).

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 (Naikakufu (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 employment 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, 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.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. 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 (Kosei-rodosho (2007)). Also, childcare-related absenteeism reduces the promotion probability and wages of mothers.

2.9 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 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 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.

Chapter 3

Labour Demand Effects of Short Work Hour Provisions in Japan

3.1 Introduction

Over the past 10 years, the work–life balance of regular employees in Japan has shown virtually no improvement. Although work hours have decreased over the past decade, this fact merely reflects the substantial increase in the number of part-time and other non-regular workers. According to Somusho-tokeikyoku (2010), 53.8% of workers were in non-regular employment in 2009 compared with 46.4% in 2000, a seven percentage point increase over that decade.

Traditionally, regular employees in Japan have enjoyed lifetime employment and high levels of social security in exchange for their willingness to fulfil

extensive corporate needs, such as an overload of work assignments, frequent relocations, and long work hours. Persons unable to meet these corporate demands are left with no option but to work as non-regular employees. This has been one of the major reasons why the majority of female workers quit their jobs after giving birth. Non-regular employees are paid significantly less than regular employees and they do not qualify for most of the social security benefits that regular employees enjoy. However, the introduction of so-called short work hour provisions has allowed workers to adjust their labour supply through the intensive margin as their monthly wages are affected by the reduction in hours to a lesser degree (Boeri and van Ours (2013)).

To provide regularly employed new parents with a better work environment in which they retain their employee benefits, the Japanese government created short work hour provisions in June 2010. Under these provisions, regular employees with children under the age of three may work six hours per day instead of eight; they are also exempt from overtime work. The new provisions benefit new parents, as they allow them to pick up their children from childcare facilities before they close. This is especially important in large cities where commuting times are excessively long.[1] In addition, the provisions allow parents to spend time with their children after work rather than being compelled to work overtime hours, which may contribute favourably to child development.[2] Hence, short work hour provisions incentivize parents who, before the reform, would have preferred not to

[1]Childcare services in Japan are regulated by the government. Typical operating hours for childcare facilities are from 7:00 am to 6:00 pm or 11 hours on weekdays.

[2]For child development and maternal employment, see Baker et al. (2008) and Ruhm (2008).

work as regular employees and make taking and/or staying in regular employment more attractive.

However, if workers incurred fixed labour costs independent of work hours, short work hour provisions would increase the labour cost per unit of time. These fixed costs might include insurance, social security, and the costs related to hiring, management, and training. Additionally, co-workers might need to cover for short-time workers, because in Japan the scope of responsibility of each individual tends not to be clearly delineated. The introduction of short work hour provisions may thus increase the cost of hiring new regular employees relative to the cost of increasing the work time of regular workers.

This study examines both the supply and the demand effects of introducing short work hour provisions by examining data taken from Japanese firms' corporate social responsibility (CSR) reports. I measure the impact of the introduction of short work hour provisions on both supply outcomes (measured as job turnover in a firm) and demand outcomes (measured as (1) changes in the number of newly hired graduates, (2) proportion of non-regular employees, and (3) overtime hours). This approach is close to that used by Fernandez-Kranz and Rodriguez-Planas (2013) who analyse the labour demand effects of the law that granted all workers with children younger than seven years protection against layoff if the worker had previously asked for a reduction in his or her working week because of his or her family responsibilities.

3.2 Background and Theoretical Framework

3.2.1 Short Work Hour Provisions

To help provide parents with an environment that enables them to care for their newborn children while still working, the Japanese government reformed the Child Care and Family Care Leave Act on 30 June 2010. Under that reform, firms with more than 100 employees are required to offer short work hour provisions to, and exempt from overtime work, those employees with children under the age of three. Firms with fewer than 100 employees are excluded from the 2010 reform; however, they were asked to meet the provision by 1 July 2012 (see Yajima (2013), Matsubara (2012), Takeishi (2013), and Sato and Takeishi (2014) for details of formulation and implementation in firms as well as some of the growing concerns about short work hour provisions).[3]

Before the reform, a number of firms had already taken up the provision. According to Kosei-rodosho (2008), among workplaces with five or more regular employees, 38.9% were already offering short work hour provisions. To identify the causal effects of the reform on labour supply and demand, I examine the employee turnover, hiring practices, and overtime hours of those firms that were already offering short work hour provisions before the reform and compare them with those of firms that introduced the provision only after the reform. The identification issue that arises here is that firms that were already offering short work hour provisions before the reform are relatively large and have better corporate

[3]Data are not yet available to investigate the 2012 reform.

performance. To estimate the causal effect of the reform under the non-random assignment of treatment, this study therefore utilizes matching estimates.

3.2.2 Labour Supply

Workers choose their work hours on the basis of an hourly wage rate and their preferences for leisure and consumption. A worker can choose from the following three employment statuses: (1) regular employment, (2) non-regular employment, and (3) having no job. If the reservation wage is too high for some workers to enter the regular employment market, they may choose a non-regular job or not work at all. However, if regular short-time employment is added to their options, these higher wages may induce such workers to work as regular short-time employees, since regular short-time employment comes with a higher hourly wage rate, better employee benefits, and stable employment compared with non-regular employment. In brief, short work hour provisions provide workers with a higher reservation wage along with an optimal combination of work hours and leisure time (see Boeri and van Ours (2013) for a discussion). Therefore, these provisions are expected to increase labour market participation by regular employees.

3.2.3 Labour Demand

A firm's production function is $F = f(E, H_t)$, where F is output, E is the number of employees, and H_t is total work hours. The firm will choose E and H_t to

maximize its profit at the lowest cost. The firm's total labour cost of hiring short-time workers is described as follows (Trejo (2003), Hart (2004)):

$$C_s = wEH_s + b_sE \tag{3.1}$$

$$C_l = wEH_l + pwE(H_t - H_l) + b_lE \tag{3.2}$$

where w is the hourly wage rate, p denotes the overtime premium paid to workers, and b represents he fixed labour costs,[4] such as training and hiring costs and various employee benefits. H_s is the threshold at which the maximum hours for short-time workers (i.e. six hours) applies and H_l is the standard work hours (i.e. eight hours). w, H_s, and H_l are exogenous, and the firm chooses E and H_t to maximize profits. Unless $b_s < b_l$, the cost of hiring new workers who potentially use short work hour provisions may increase relative to the cost of increasing the work time of regular employees. In Japan, the majority of parents who are responsible for childcare are female; therefore, the provisions may affect female labour demand. Additionally, they may discourage the hiring of regular employees, while firms might substitute regular employees with a lower-cost labour force, non-regular employees, or employees with lower education levels.

[4]See Hart (2004) for a detailed discussion of these costs.

3.3 Previous Studies

3.3.1 Female Labour Demand

The labour demand effect of employee programmes remains uncertain. Labour demand theory suggests that generous programmes may increase labour costs and thus affect the wages of those who benefit from such policies. Gruber (1994) did actually find that the benefit costs shift to the wages of the workers who benefited; however, he also found that in the United States, the benefit did not have a significant effect on the labour demand of the group who benefited. One study in Japan has investigated the introduction of parental leave policy on labour demand. Morita (2005) found that the introduction of parental leave policy in 1992 had no effect on the labour demand of women; however, that study also found that for firms with fewer than 30 employees, there was a significant decrease in female labour force participation following the introduction of the parental leave programme. Fernandez-Kranz and Rodriguez-Planas (2013) evaluated the impact of a 1999 law that granted all workers with children younger than seven years protection against lay-off if the worker had previously asked for a reduction in his or her working week because of his or her family responsibilities. The authors found that employers shift childbearing-aged women out of good jobs, which decreases women's relative wages.

3.3.2 Female Labour Supply

It is generally recognized that turnover is costly for companies because it causes human capital loss and additional investment requirements in recruiting and re-training new workers. Some workers are much more likely than others to change jobs. Workers have different tolerance levels for working conditions such as night work, long hours, inflexible work schedules, and jobs with a higher risk of injury and disease. Holding differences in workers' preferences constant, and assuming that actual wages are fixed, companies with unpleasant working conditions would attract fewer workers than companies with good working conditions, unless they pay a compensating wage.

Studies have found that workers receive wage premiums for taking on hazardous jobs (Viscusi (1978), Hamermesh and Wolfe (1990)), roles that carry a higher risk of unemployment (Moretti (2000)), seasonal employment (Del Bono and Weber (2008)), and night work (Kostiuk (1990)). Overtime work and inflexible work schedules also disrupt family interaction and cause physical and psychological health issues such as stress and depression. The overtime premium is the compensating wage paid for overtime hours and this, in turn, affects workers' choices of work hours (Hamermesh and Trejo (2000)). Just as employees need compensation to work long hours, employees with children under the age of three accept a reduction in their work hours and wages to spend more time with their families (Boeri and van Ours (2013)).

3.3.3 Studies in Japan

Various programmes in Japan target workers with newborn children; the most frequently used of these is parental leave. Asai (2014b) examined the increase in the parental leave income replacement rate — from 0% to 25% in 1995 and from 25% to 40% in 2001 — and found the reform to have no significant effect on mothers' job continuity. Additionally, Asai (2014a) evaluated the subsequent 2007 reform and found, similarly, that it had no effect on mothers' job continuity. For the results of previous studies of parental leave, see Chapter 2.[5] Other studies have examined the effect of introducing flexible work provisions. Sakazume (2002) showed that companies that offer family-friendly programmes[6] tend to have lower turnover rates than companies that do not. Additionally, Yanadori and Kato (2009) investigated how four family-friendly practices, namely flexitime, maternity leave, childcare leave, and nursing care leave, affect employee turnover. They found that companies that offer a programme by which employees can take leave exceeding that legally required have lower female employee turnover rates.

[5]Most previous studies focus on the socio-economic characteristics of persons who take parental leave. For example, Abe (2002), using data from the Japanese Panel Survey of Consumers 1993–2003, showed that women who work in large firms and high-ranking employees, who tend to benefit from the system, are more likely to take parental leave. Higuchi (1994), Suruga and Zhang (2002), and Shigeno and Matsuura (2003) all indicated that employees who work at a firm that offers parental leave have higher marriage and fertility rates than employees who do not enjoy this benefit. Besides Asai (2014b) Asai (2014a), however, few researchers have reviewed parental leave policy and its effect on women's labour force participation. Sato and Ma (2007), using the first wave of data from the Keio Household Panel Survey of 2004, revealed that women who have parental leave coverage have a higher return-to-work rate than those employed by firms lacking parental leave. However, their results might have suffered from potential selection bias, since women seeking a return to work may look for jobs with firms that offer parental leave benefits.

[6]She created a notional measure using seven family-friendly indexes. For example, this measure includes whether a firm has a workplace development programme that encourages work-life balance or a programme to make employees more time-aware and promote work styles that use time more productively.

Compared with the aforementioned literature on parental leave and other flexible work programmes, very little research has focused on the relatively new short work hour provisions. Fukahori (2013) used Japan Household Panel Survey data to show that the provisions may have reduced labour turnover after the reform; however, his sample size was small and he did not investigate its effect on demand. The present study, which is similar to that of Fernandez-Kranz and Rodriguez-Planas (2013), contributes to the literature by investigating the policy effects of the introduction of short work hour provisions on labour demand in Japanese firms. The Japanese labour market is highly segregated, in that the wages and benefit levels of full-time and part-time workers are significantly different. The introduction of short work hour provisions which provides parents with the possibility of reducing their work hours without losing their full-time equivalent employee benefits thus provides new insights into how firms and employees react to work hour regulations.

3.4 Data

This study uses CSR data drawn from Japanese firms' CSR reports for the 2009 and 2010 fiscal years; 1,051 firms can be found within the data of both years.[7] I limit my analysis to firms with more than 100 employees, and among those, only 455 firms answered all the employment-related questions.[8] Job turnover is measured as the proportion of male and female turnover, which is calculated as

[7]The Japanese fiscal year starts on 1 April and ends on 30 March; therefore, the 2009 fiscal year denotes April 2009–March 2010, inclusive. I manually inputted data from the CSR reports and constructed a panel dataset for each firm.

[8]Note that the respondent firms are mainly from the manufacturing industry.

the number of male and female workers who quit divided by the total number of male and female employees (this is a supply side measure). The measures of hiring are as follows: (1) changes in the number of newly hired college graduates, which measures the proportion of college graduates among all newly hired employees;[9] (2) the proportion of non-regular employees; and (3) average overtime hours (these are the female labour demand measures). For each outcome, the difference between the 2009 and 2010 data was taken.

3.5 Framework

3.5.1 Random Assignment

This study compares the pre-implementation (i.e. 2009) ($t = 0$, where t is time) and post-implementation (i.e. 2010) ($t = 1$) outcomes of treatment firms (i.e. firms that took up the provision after the reform) and compares them with those of firms that had formulated the provision before the reform. Whether a firm offered short work hour provisions before the reform is denoted as $a_i = 0, 1$. y_i denotes the outcome of firm i; y_{1i}, the outcome after exposure to treatment; and y_{0i}, the outcome without exposure to treatment. The treatment effects can be measured as $E[y_{1i}|a_i = 1] - E[y_{0i}|a_i = 1]$; however, we cannot observe both y_{0i} and y_{1i} for each firm. For this reason, this study compares the expected outcome of the treatment and comparison groups and measures the treatment effects.

[9]The proportion is calculated as the number of college graduate female workers divided by the number of newly hired graduate workers at all education levels.

If there is a difference in the pre-enforcement outcome for the two groups, selection bias arises – that is, $E[y_{0i}|a_i = 1] - E[y_{0i}|a_i = 0] \neq 0$ – and the causal effects of the treatment cannot be identified (Angrist and Pischke (2008)). The random assignment of treatment firms will resolve this problem; however, in this study, treatment firms are not randomly assigned, as shown in Table 3.1. The first and second rows show the mean and standard deviation (SD) of the characteristics at $t = 0$. The fifth row shows the t-statistics of the test of equality of means between the two groups. Treatment firms are significantly smaller in size and offer lower average incomes than those in the comparison group.

	Treatment group		Comparison group		
	Mean	SD	Mean	SD	t
Number of employees	1063.11	1221.12	3346.88	6012.08	3.66 ***
Changes in number of employees (From 2008 to 2009)	22.18	235.97	23.42	579.49	0.02
Average income (thousand)	5626.7	1093.7	6232.0	1445.6	3.79 ***
Changes in average income (From 2008 to 2009)	-229.1	402.2	-390.6	459.8	3.11 **
Average age of employees	38.93	3.71	39.29	2.92	1.02
Average tenure of employees	14.17	4.68	14.82	4.04	1.34
Proportion of female employees	0.19	0.13	0.19	0.12	-0.12
Proportion of non-regular employees	0.27	0.56	0.38	0.92	1.13
Industry Agriculture, mining, construction	0.07	0.26	0.04	0.21	-1.19
Manufacturing (food, textile, medical, etc.)	0.18	0.39	0.22	0.41	0.75
Manufacturing (hardware, machinery)	0.30	0.46	0.37	0.48	1.37
Electricity and gas	0.00	0.00	0.02	0.15	1.46
Transport and information-communication	0.09	0.28	0.09	0.28	0.11
Commerce and trade	0.20	0.40	0.15	0.36	-1.24
Finance and insurance	0.09	0.28	0.06	0.23	-1.07
Real estate and service	0.07	0.26	0.05	0.22	-0.93
Male turnover rate	0.04	0.04	0.04	0.05	-1.00
Female turnover rate	0.07	0.06	0.06	0.08	-1.36
Proportion of newly hired college graduates male	0.58	0.27	0.57	0.24	-0.24
Proportion of newly hired college graduates female	0.17	0.20	0.19	0.19	0.71
Overtime hours	16.10	10.58	16.50	9.56	0.36
Sample size	94		361		

TABLE 3.1: Characteristics of the Treatment and Comparison Groups

*Note: The treatment group comprises firms that were already offering short work hour provisions before the reform and the control group is firms that took up the provisions after the reform. ***p>0.01, **p>0.05, and *p>0.1. t is the test of equality of means between two groups.*

To estimate the causal effect of the programme under the non-random assignment of treatment, this study utilizes a panel-data structure and matching

estimates, and thus focuses on the comparability of the treatment and comparison groups in terms of the pre-enforcement variables (see Rosenbaum and Rubin (1983), Dehejia and Wahba (1999), and Angrist and Pischke (2008) for details.)

3.5.2 Estimation Model

The implementation of short work hour provisions might increase a firm's labour cost per unit of time. Therefore, large firms with ample human resources were more likely to offer this provision before the reform than smaller firms, and firms with particular observables X_i have a higher probability of treatment. Matching estimates have a causal interpretation assuming that, conditional on the individual characteristics by which the pre-enforcement formulation of short work hour provisions is determined, the pre-enforcement formulation is independent of potential outcomes (Angrist and Pischke (2008), Becker and Ichino (2002)):

$$[y_{0i}, y_{1i} \perp \alpha_i] \mid X_i \tag{3.3}$$

Therefore, under the conditional independence assumption (CIA), we can estimate the effects of certification in the following manner:

$$\delta = E[E[y_i \mid X_i, a_i = 1] - E[y_i \mid X_i, a_i = 0] \mid \alpha_1 = 1] \tag{3.4}$$

First, the propensity score p_i, which is the probability of treatment, conditional on the pre-intervention observable characteristics, X_i, is estimated by using a Probit model:

$$p_i = Pr(a_i \mid X_1) \tag{3.5}$$

$$0 < p_i < 1 \tag{3.6}$$

It is important to include all the variables that will determine pre-enforcement formulation as well as employee turnover at X_i. This study includes the following variables: number of employees, average income, average employee age, average employee tenure, proportion of female employees, proportion of non-regular employees, industry, male turnover rate, female turnover rate, and proportion of newly hired college female graduates at time $t = 0$. It also includes changes in the number of employees and in average income from 2008 to 2009, as Ashenfelter and Card (1985), Angrist and Pischke (2008), and Angrist and Krueger (1999) indicated that it is important to examine several years of pre-intervention outcomes before determining the programme effects. Firms with promising prospects will be more likely to implement such programmes. In addition, an increase in pay might reduce employee turnover.

The density distribution of the estimated p_i is shown in Figure 3.1. The horizontal line denotes a score of $0 < p_i < 1$. In the following sections, firms with an estimated propensity score lower than the minimum or greater than the maximum are discarded, because the matching is justified when performed over the

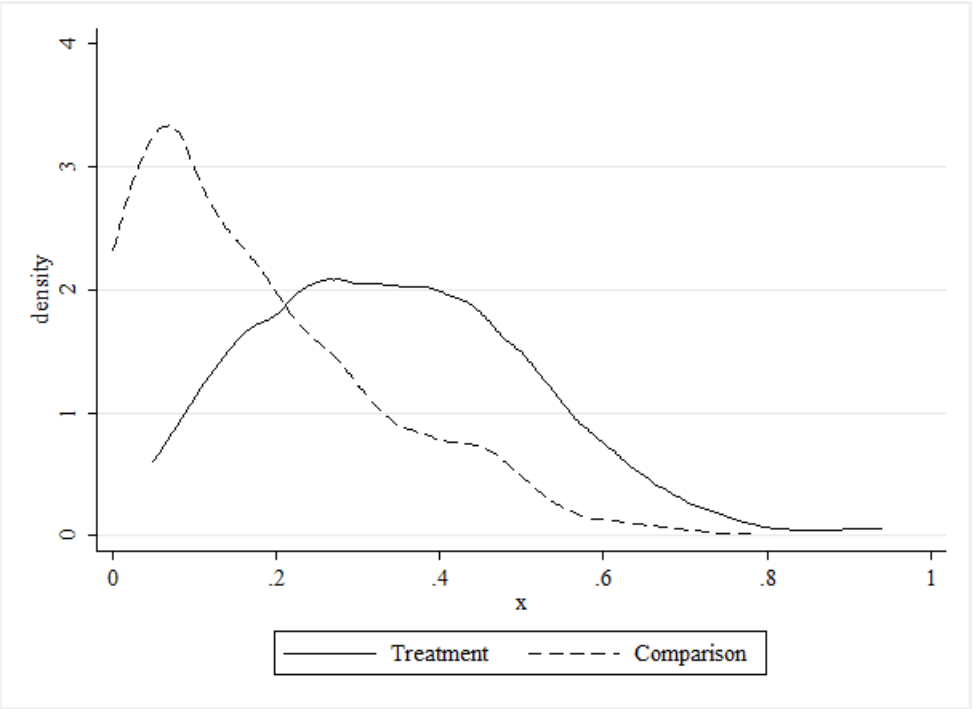

FIGURE 3.1: Density of the Probability of Treatment

Note: This figures illustrates the distribution of the propensity score p_i, which is the probability of treatment, conditional on the pre-intervention observable characteristics, X_i. The horizontal line denotes a score of $0 < p_i < 1$. In the following sections, firms with an estimated propensity score lower than the minimum or greater than the maximum are discarded, because the matching is justified when performed over the common support region; I also exclude firms that have an estimated propensity score outside the common support region $0.1 < p_i < 0.9$ (Dehejia and Wahba (1999), Smith and Todd (2005)). The treatment group comprises firms that were already offering short work hour provisions before the reform and the control group is firms that took up the provisions after the reform.

common support region; I also exclude firms that have an estimated propensity score outside the common support region $0.1 < p_i < 0.9$ (Dehejia and Wahba (1999), Smith and Todd (2005)). For firms inside the common support region, the average treatment effect on treated firms (DID-ATT) is estimated in the following manner:

$$\widehat{\delta_{ATT}} = \frac{\sum_{i \in T}[y_i^T - \sum_{j \in M_{(i)}} w_{ij} y_j]}{N^T} \tag{3.7}$$

That is, for each treatment firm i, y_i^T, the difference in the means of outcomes between comparison group firms, $M(i)$, is taken. The weight w_{ij} is defined as

$$if \quad j \in M(i); then, w_{ij} = \frac{1}{N_i^C}, \qquad and \quad if \quad not, then \quad w_{ij} = 0 \tag{3.8}$$

3.6 Results

The results of the propensity score estimation are shown in Table 3.3. The smaller the firm, the lower is its average income, while the lower its proportion of regular employees, the higher is the probability of treatment. The average probability of treatment among all employees is 29.0%. Matching estimates have a causal interpretation assuming that, conditional on the individual characteristics by which treatment is determined, treatment is independent of the potential outcomes. To determine the success of the matching, the mean and SD of the observables for the two groups in the common support region are considered (Table 3.2). Compared with Table 3.1, the observables of the two groups are balanced. The difference in characteristics is not significantly different from zero.

The results of the DID-ATT estimation using Equation (3.7) are presented in Table 3.4. I changed the number of matches from one to three in order to check the robustness of the results. The coefficients shown in Table 3.4 are ($\widehat{\delta_{ATT}}$), the treatment effect on the treated. Supply measures, male turnover rate,

	Treatment group		Comparison group		
	Mean	SD	Mean	SD	t
Number of employees	965.52	1128.64	1228.98	1519.38	1.45
Changes in number of employees (From 2008 to 2009)	25.99	245.70	46.98	655.84	0.29
Average income (thousand)	5617.4	1101.3	5806.7	1162.9	1.29
Changes in average income (From 2008 to 2009)	-214.2	350.7	-294.8	423.5	-1.56
Average age of employees	38.83	3.83	39.09	3.17	0.61
Average tenure of employees	14.14	4.73	14.50	4.20	0.65
Proportion of female employees	0.19	0.13	0.19	0.12	0.19
Proportion of non-regular employees	0.25	0.54	0.26	0.49	0.03
Industry Agriculture, mining, construction	0.08	0.28	0.06	0.23	-0.80
Manufacturing (food, textile, medical, etc.)	0.17	0.38	0.22	0.42	0.89
Manufacturing (hardware, machinery)	0.30	0.46	0.32	0.47	0.36
Electricity and gas	0.00	0.00	0.00	0.00	-
Transport and information-communication	0.08	0.28	0.09	0.29	0.22
Commerce and trade	0.19	0.39	0.18	0.39	-0.06
Finance and insurance	0.09	0.29	0.08	0.26	-0.51
Real estate and service	0.08	0.28	0.05	0.22	-0.98
Male turnover rate	0.04	0.04	0.04	0.06	0.29
Female turnover rate	0.07	0.06	0.07	0.10	0.02
Proportion of newly hired college graduates male	0.58	0.03	0.58	0.02	-0.14
Proportion of newly hired college graduates female	0.18	0.20	0.18	0.21	0.15
Overtime hours	15.53	9.74	15.58	9.77	0.04
Sample size	86		213		

TABLE 3.2: Characteristics of the Treatment and Comparison Groups for Firms in the Common Support Region

*Note: The treatment group comprises firms that were already offering short work hour provisions before the reform and the control group is firms that took up the provisions after the reform. ***p>0.01, **p>0.05, and *p>0.1. t is the test of equality of means between two groups.*

and female turnover rate did not show significant increases after the introduction. Additionally, changes in the labour-demand measures of the proportion of non-regular employees and in overtime hours were not significantly different from zero. On the contrary, the proportion of newly hired female college graduates increased by 5–9% among treatment firms, whereas the proportion of newly hired male college graduates did not increase significantly.

This result suggests the possibility of 'sorting' skilled women to firms that offered generous programmes before the reform. That is, the reform helped treatment firms that could not encourage the recruitment of skilled women before the introduction of short work hour provisions attract such workers.

	Marginal Effects	SE	
Number of Employees (log)	-0.100	0.019	***
Changes in number of employees (From 2008 to 2009)	0.000	0.000	
Average income (thousand, log)	-0.305	0.111	***
Changes in average income (From 2008 to 2009)	0.000	0.000	***
Average age of employees	-0.098	0.119	
Average age of employees*Average age of employees	0.001	0.002	
Average tenure of employees	0.005	0.030	
Average tenure of employees*Average tenure of employees	0.001	0.001	
Proportion of female employees	-0.252	0.198	
Proportion of non-regular employees	-0.087	0.034	**
Industry Agriculture, mining, construction	0.145	0.124	
Manufacturing (food, textile, medical, etc.)	-0.021	0.050	
Manufacturing (hardware, machinery) Ref.			
Transport and information-communication	0.005	0.077	
Commerce and trade	0.137	0.081	*
Finance and insurance	0.383	0.143	***
Real estate and service	0.135	0.132	
Male turnover rate	-0.333	0.640	
Female turnover rate	0.007	0.390	
Proportion of newly hired college graduates male	-0.010	0.081	
Proportion of newly hired college graduates female	-0.058	0.114	
Overtime hours	-0.005	0.006	
Overtime hours*Overtime hours	0.000	0.000	
Sample size	447		

TABLE 3.3: Marginal Effects on the Probability of Treatment

*Note: The marginal effects on the probability of treatment from the Probit model. ***p>0.01, **p>0.05, and *p>0.1.*

3.7 Conclusion

Since June 2010, firms in Japan with more than 100 employees have been required to formulate and offer short work hour provisions to employees with children under the age of three. Qualifying employees are allowed to work six hours per day instead of eight and they are also exempt from working overtime hours. I investigated the effect of introducing these provisions on labour supply and demand, by using a matching framework.

Labour demand theory predicts that if workers incur a fixed labour cost

independent of work hours, short work hour provisions will increase the labour cost per unit of time. However, the results presented herein did not show any decrease in the hiring of skilled employees (i.e. college graduates). Additionally, the reduced hours were not found to be offset by other workers' overtime hours: there was no increase in overtime hours.

Two explanations might clarify these results. First, the labour productivity of short-time users might not have fallen. In the majority of firms, short-time users are expected to produce the same amount of output in fewer hours; therefore, their non-productive time might have been reduced. Second, the number of short-time users might be too small to affect firm behaviour; to the best of my knowledge, the take-up rate has not been surveyed to date. Because short work hour provisions are available only to employees with children under the age of three, these employees do not represent a majority of employees and not all of them make use of the provision in any case.

For this reason, even if short work hour provisions were to increase the labour cost per unit of time, firms might be able to offset these costs elsewhere; in fact, it might even increase the per-hour productivity of provision users. There have been growing concerns over the introduction of short work hour provisions in terms of the cost increase incurred by firms (Yajima (2013); Matsubara (2012); Takeishi (2013); Sato and Takeishi (2014)); however, the results of the current study indicated that the introduction of the provisions benefits firms in that it helps them attract a well-educated and skilled female labour force.

	Number of matches			
Outcome	**1**	**2**	**3**	**N**
Male turnover rate	0.003	0.002	-0.001	263
	(0.009)	(0.007)	(0.007)	
Female turnover rate	0.020	0.020	0.019	263
	(0.016)	(0.014)	(0.014)	
Proportion of newly hired college graduates male	0.054	0.003	-0.003	227
	(0.034)	(0.032)	(0.032)	
Proportion of newly hired college graduates female	0.047 *	0.052 **	0.045 *	227
	(0.026)	(0.025)	(0.024)	
Proportion of non-regular employees	-0.024	-0.022	-0.019	281
	(0.017)	(0.016)	(0.014)	
Overtime hours (log)	-0.142	-0.177	-0.107	259
	(0.256)	(0.254)	(0.238)	

TABLE 3.4: DID-ATT on the Outcome Variables

*Note: The coefficients ($\widehat{\delta_{ATT}}$) are from the DID-ATT estimation using Equation 3.7 (i.e. the treatment effect on the treated). The treatment group comprises firms that were already offering short work hour provisions before the reform and the control group is firms that took up the provisions after the reform. Heteroscedasticity-consistent standard errors are shown in parentheses.***p>0.01, **p>0.05, and *p>0.1.*

Chapter 4

Overtime Premium and Work Hours: An Evaluation of the Labour Standards Act Reform in Japan

4.1 Introduction

Based on a standard 40-hour working week, almost one in 10 Japanese workers works more than 20 hours of overtime every week (Somusho-tokeikyoku (2010)). This proportion rises to 18% for men in their thirties. In order to reduce work hours and thus promote work/life balance as well as reduce cases of death from overwork (Karoshi), the Japanese government reformed the Labour Standards Act

in April 2010.[1] This reform required that large firms double the overtime premium paid to workers that work more than 60 hours of overtime per month (i.e. an average of 15 hours every week) from 25% to 50%. Note, however, that small and medium-sized enterprises (SMEs) remained uncovered this by Act. In Japan, companies in different industries are classified as SMEs according to their number of employees as follows: (i) retail industry: under 50; (ii) services industry: under 100; (iii) wholesale industry: under 100; and (iv) other industries: under 300. The government is classified as a large company.

This study examines how the reform has affected average overtime hours worked and the incidence of overtime in Japan. In this regard, the presented findings contribute to the literature in two ways. First, the study assesses the Japanese government's decision to regulate excessive overtime in order to provide evidence on how such a regulation affects overtime hours. Second, I investigate whether strong governmental regulation is able to lower the incidence of overtime in a country where the culture of long work hours is deeply rooted, which is the intended effect of the reform.

The studies that have focused on how regulations on overtime pay affect work hours and the incidence of overtime have presented inconsistent results. Trejo (2003) examines the impact of an increase in the coverage of the overtime premium during 1970-1989 in the United States and finds no impact on overtime incidence or overtime hours. He explains this result by stating that the overtime premium could have been offset by changes in standard hourly wage rates. On the other

[1]The details of the reform can be found on the Ministry of Health, Labour, and Welfare website at www.mhlw.go.jp/stf/seisakunitsuite/bunya/koyou_roudou/roudoukijun/roukikaitei/index.html.

hand, Hamermesh and Trejo (2000) investigate the extension of the time-and-a-half pay for work hours above eight hours per day and find that the overtime penalty substantially reduced the amount of daily overtime worked compared with other states; this finding is consistent with the labour demand model presented in Section 4.2.

A number of studies have also focused on reductions in standard work hours, which have been shown to increase the marginal cost of an additional worker and thereby decrease employment at the firm level (Hunt (1999); Crepon and Kramarz (2002)). In this vein, Kawaguchi et al. (2008) investigate how a reduction in legal work hours influences actual hours worked by using Japanese data and find that that a one-hour reduction in weekly legal work hours reduced actual work hours by 0.14 hours, while it did not decrease monthly compensation to workers.

The remainder of this chapter is organised as follows. Section 4.2 presents the theoretical framework and data sources, section 4.3 provides an overview of overtime work in Japan, section 4.4 shows the descriptive analysis, section 4.5 presents the estimation models, section 4.6 discusses the results, and section 4.7 concludes.

4.2 Theoretical Framework and Data Sources

4.2.1 Labour Demand Model

Trejo (2003) uses a labour demand model to show that the regulation of overtime

pay would reduce the proportion of workers that regularly work more than the standard 40-hour working week. In this study, I extend Trejo (2003) model to explain the distinction between working either fewer or more than 60 hours of overtime per month following the 2010 reform. A firm's production function is $F = f(E, H_t)$, where F is output, E is the number of employees, and H_t is monthly work hours. The total labour cost is written as follows:

$$C_1 = wEH_t + bE, \qquad if \qquad H_t \leq H_s \tag{4.1}$$

$$C_2 = wEH_s + pwE(H_t - H_s) + bE, \qquad if \qquad H_x \geq H_t > H_s \tag{4.2}$$

$$C_3 = wEH_s + pwE(H_x - H_s) + xwE(H_t - H_x) + bE, \qquad if \qquad H_t > H_x \tag{4.3}$$

where w is the hourly wage rate, p denotes the overtime premium paid to workers that work up to 60 hours of overtime per month (overtime workers hereafter), and x denotes the overtime premium paid to workers that work more than 60 hours of overtime per month (extra overtime workers hereafter). Further, H_s is the threshold at which the overtime premium applies and H_x is the threshold at which the extra overtime premium applies, while b represents fixed labour costs [2] such as training costs and employee benefits. w, l, p, x, H_s, and H_x are exogenous and the firm chooses E and H_t. The profit maximisation problem including this extra overtime premium is thus

$$Y_1(w, b) = \max_{E, H_t} f(E, H_t) - wEH_t - bE \tag{4.4}$$

[2] See Hart (2004) for a detailed discussion of these costs.

$$Y_2(w, b, p, H_s) = \max_{E,H_t} f(E, H_t) - wEH_s - pwE(H_x - H_s) - bE \tag{4.5}$$

$$Y_3(w, b, p, x, H_s, H_x) = \max_{E,H_t} f(E, H_t) - wEH_s - pwE(H_x - H_s) - xwE(H_t - H_x) - bE \tag{4.6}$$

If the firm profit when assigning extra overtime, Y_3, is higher than that without assigning overtime or with assigning some overtime, Y_1 and Y_2, respectively, the firm assigns extra overtime to employees; otherwise, the firm lets employees work less than H_x. The rise in the overtime premium above H_x (i.e. from 25% to 50%) increases the marginal cost to employers of assigning overtime; therefore, firms respond by reducing employees' extra overtime hours. The increase in the extra overtime premium should thus reduce the proportion of employees that work more than 60 hours of overtime per month (i.e. the government's intended effect of the reform).

4.2.2 Labour Supply Model

Individuals choose their work hours based on the hourly wage rate as well as on their preferences for leisure and consumption. In other words, they work until the marginal value of non-market hours meets the wage rate. Therefore, taking into

account the counterbalance between demand and supply effects, the new labour market equilibrium is unknown.

When a firm wants to increase an employee's work hours, it must pay an overtime premium. Overtime work and inflexible work schedules disrupt family interaction and cause physical and psychological health issues such as stress. The overtime premium is the compensating wage paid for overtime hours and this, in turn, affects workers' choice of work hours (Hamermesh and Trejo (2000)). If employees have control over their work hours, the reform incentivises employees to increase the number of overtime hours they work because this increases their marginal wage (even though it reduces their leisure time; Trejo (2003); Hart (2004); Boeri and van Ours (2013)). So-called 'service overtime' in Japan is unpaid overtime that employees typically perform voluntarily. In this study, because I cannot identify which overtime is paid and which is not, I assume that all overtime is paid.

Trejo (2003) predicts that without the regulation on the straight-time wage, standard hourly wage rates are flexible; therefore, changes in the overtime premium could be offset by changes in standard hourly wages, leaving work hours and earnings unchanged.[3] The labour supply model thus predicts that a rise in the overtime premium might either (i) increase overtime hours or (ii) have no effect on overtime hours.

In this study, I investigate whether the supply effects dominate the demand effects. I also explore the reform effects by examining workers' preferences.

[3] Trejo (2003) considers that workers and firms can negotiate their own compensation packages of work hours and earnings.

In Japan, some workers are forced to work overtime, whereas others do so voluntarily because their base salaries are low and/or they have a strong preference for work.[4] The Labour Standard Law states that when an employee's activities are under employer supervision (including implicit and indirect supervision), the employer must pay for these hours. However, there is a grey area in this law. For example, when an employee voluntarily chooses to work overtime but nobody stops him or her from doing so, this is considered to be under supervision. Therefore, in a firm without strict time management (of which there are many in Japan), employees can work as many extra hours as they want.

By contrast, some employees are forced to work overtime because of staff shortages or because employers value workers who work long hours and thus favour them for promotion. Compared with temporary and non-regular workers, regular workers in Japan enjoy lifelong employment protection and generous benefits. Hence, firms tend to consider regular workers to be obliged to work as often as they demand. Some firms even cut the fixed cost of labour by reducing their numbers of regular employees, causing chronic staff shortages. To capture the degree to which employees' overtime choices are autonomous, workers' preferences are therefore used in this study.

For those workers that voluntarily choose to work overtime, the labour supply model is considered to provide a better prediction of the reform effect, namely employees increase their overtime hours or the reform has no effect on overtime hours, unless firms have strictly tried to reduce overtime work. On the

[4] Hamermesh and Slemrod (2008) argue that for workaholics, current work increases the marginal utility for future work, lowers the utility from a given amount of working, and imposes future costs in terms of health and other problems.

other hand, for employees that want to reduce their overtime hours, the labour demand model predicts that the reform would allow them to do so; however, any change also depends on the degree to which the firm has tried to reduce extra overtime.

4.2.3 Data Sources

The data used in this study are derived from the Japanese Life Course Panel Survey (JLPS) conducted by the Institute of Social Science at the University of Tokyo from 2007 to 2013.[5] The first wave of this survey was conducted on 4800 individuals aged 20-40, whereas the seventh wave included 3076 respondents.[6] The survey is conducted every January.

In this study, I limit my focus to regular workers that did not change their jobs during the study period of 2009-2012.[7] I also exclude workers who are discretionary workers (e.g. executives and managers), because they are not paid an hourly rate and thus they do not qualify for the overtime premium. The total sample size in this analysis is therefore 1027.

The work hours and workdays are recorded as 'usual work hours per day (including overtime)' and 'the number of workdays in usual month' in the questionnaire. Therefore, monthly work hours are calculated as 'usual work hours

[5]The response rate in the first wave is discussed by Miwa (2008), and the nonresponse rate in the subsequent waves is discussed by Tanabe (2013). The response rate is slightly lower for younger generations therefore the results from this study should be interpreted with caution.

[6]A new cohort was added in 2011; however, I did not include this in this analysis.

[7]Respondents who do not work or who work as a non-regular workers are excluded from the analysis.

per day (including overtime)' multiplied by 'the number of workdays in usual month'.

4.3 Overview of Overtime Work in Japan

Excessive overtime work causes physical and psychological fatigue and creates negative impacts on the family and private lives of workers (Eurofound (2012)). A number of governmental and private-sector projects have tried to tackle this phenomenon, which is a major social problem in Japanese society. As shown in Table 4.1(a), according to JLPS data, female work hours decreased slightly in 2013; however, there was no substantial change in work hours between 2009 and 2013. Table 4.1(b) illustrates that the proportion of extra overtime workers[8]. The 220 hours is calculated as four workweeks of 40 standard hours, plus 60 hours of overtime per month.[9] Although the proportions of male extra overtime workers decreased slightly between 2010 and 2011, it is unclear whether this decline was the effect of the reform or a macroeconomic shock.

Given that some workers have a strong preference to overwork and others are rather forced to overwork, I categorise workers' preferences as follows: (1) those that want to reduce the number of hours worked, (2) those that want to work the same number of hours, and (3) those that want to increase the number of hours. These preferences are defined according to worker status in January 2009.[10] Table

[8]The 220 hours is calculated as four workweeks of 40 standard hours, plus 60 hours of overtime per month.

[9]The proportion is slightly higher than that presented in the Labour Force Survey because the age distribution of sample universe is skewed to younger people in the JLPS.

[10]The 2008 questionnaire did not ask such a question.

	Men		Women	
	(a)	(b)	(a)	(b)
Jan. 2009	211.2	31.9%	188.2	12.8%
Jan. 2010	213.4	32.5%	190.3	14.2%
Jan. 2011	213.6	32.7%	191.1	12.8%
Jan. 2012	212.5	32.2%	191.8	14.8%
Sample size	661		366	

TABLE 4.1: (a) Average monthly work hours and (b) proportion of extra overtime workers by sex.

Note: Averages are calculated for each year. The extra overtime workers is defined as workers that work more than 220 hours (i.e. 40 standard hours, plus 60 hours of overtime per month). Respondents who did not answer the question are excluded from the analysis.

4.2 shows the changes in working hour preferences from 2009 to 2013 for extra overtime workers (left) and others (right). This table shows that almost 65% of extra overtime workers want to reduce their hours compared with only 40% of those employees that do not work extra overtime (standard worker hereafter). Nevertheless, approximately 35% of extra overtime workers are happy to continue working at the same rate. In the following, I separately analyse (i) those who wish to reduce their hours (those who prefer leisure to overworking) and (ii) those who wish to increase their hours or work the same hours (those who prefer overworking to leisure). In the next section, I run separate regressions for these two groups to investigate whether the reform impact varies by working hour preferences.

4.4 Data Analysis

First, I examine changes in work hours based on respondents' work hours before the reform and the size of firm for which they work. The treatment group of this study is extra overtime workers that worked in large firms in January 2009 (the

	Extra overtime worker			Standard worker		
	Wish to Increase Hours	Wish to work the same Hours	Wish to reduce hours	Wish to Increase Hours	Wish to work the same Hours	Wish to reduce hours
2009	0.4%	27.2%	72.4%	3.1%	61.3%	35.6%
2010	1.2%	30.0%	68.9%	3.0%	58.4%	38.6%
2011	1.6%	31.9%	66.5%	1.6%	59.1%	39.4%
2012	1.2%	34.2%	64.6%	1.6%	55.8%	42.7%
Sample size		257			762	

TABLE 4.2: Distribution of workers' preferences towards work hours according to whether a respondent worked extra overtime in January 2009

Note: The changes in working hour preferences from 2009 to 2013 for extra overtime workers (left) and others (right), which is defined according to worker status in January 2009. The workers' preferences is categorized as follows: (1) those that want to reduce the number of hours worked, (2) those that want to work the same number of hours, and (3) those that want to increase the number of hours. Respondents who did not answer the question are excluded from the analysis.

reform was amended in December 2008 and enforced in April 2010). The control group is workers that did not work extra overtime hours or with those in SMEs

Figure 4.1 shows average monthly work hours by firm size (solid line) and worker status (dashed line). This figure shows only slight changes in work hours between 2009 and 2012 by firm size, with an approximately 2.9-hour increase in large firms and a 2.1-hour decrease in SMEs between 2010 and 2011. On the other hand, the average work hours of extra overtime workers decreased by 6.1 hours, whereas they increased by 2.7 hours for standard workers between 2010 and 2011.[11]

Figure 4.2 shows the average monthly work hours for four groups of respondents divided by firm size and worker status in January 2009. Of these four, the extra overtime workers in a large firm group is the treatment group.

[11] Work hours significantly reduced from 2008 to 2009, possibly because of the ramifications of the collapse of Lehman Brothers and the ensuing global financial crisis. Because of this reason, I did not define the treatment group status based on 2008 work hours. Note that the results were similar when the treatment group was defined based on 2008 work hours.

There was an approximately 0.6-hour decrease in work hours for the treatment group from 2010 to 2011, but a 10.5-hour decrease for extra overtime workers in SMEs, suggesting that the decrease in the work hours of overtime workers shown in Figure 4.1 was mainly driven by a reduction in work hours in SMEs.

Figure 4.3 shows average monthly work hours by workers' preferences. It shows that there was a 1.8-hour increase in work hours for the treatment group who want to work more compared with a 1.4-hour decrease for the treatment group who want to reduce their hours in 2011, suggesting that the reform might have had a heterogeneous effect on work hours according to different workers' preferences.[12] However, the reduction in work hours mainly derived from workers in SMEs. Indeed, there was a 9.9-hour decrease in work hours for extra overtime workers in SMEs who want to work more compared with a 10.8-hour decrease for extra overtime workers in SMEs who want to reduce their hours.

Figure 4.4 shows the incidence of overtime working by group. The incidence of overtime decreased by 1.6 percentage points in the treatment group, while there was an 8.7 percentage point increase in the proportion for extra overtime workers in SMEs. Again, the difference in this average level shows that the reduction in the incidence of extra overtime was mainly from extra overtime workers in SMEs.

[12]In addition, the graph for weekly overtime hours is calculated from the monthly work hours minus the standard working hour category. I recode the standard hours category from the JLPS with the median of each category and exclude workers in a firm with no standard hours or with varying standard hours by seasons. The sample size decreases because of non-responses or no-fixed standard hours. The results are similar to those for monthly work hours.

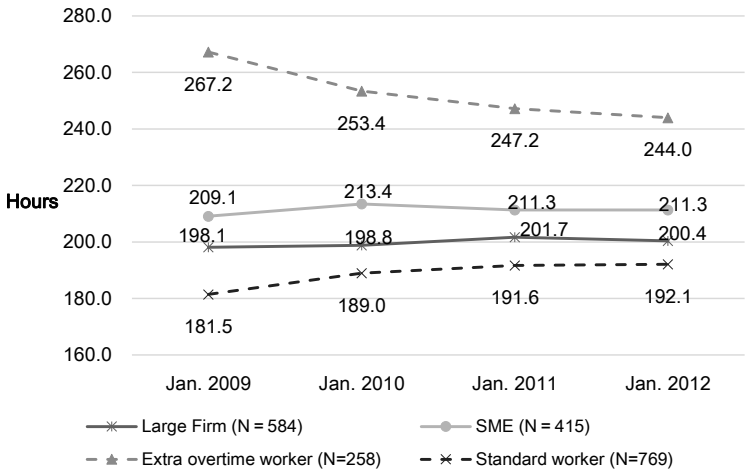

FIGURE 4.1: Average monthly work hours by firm size (solid line) and worker status (dashed line)

Note: Workers are divided into two groups by firm size- (1) Large firm and (2)SMEs (solid line) and by worker's extra overtime work status -(1)Extra overtime worker, (2)Standard worker (dashed line). Respondents who did not answer the work hours question are excluded from the analysis.

Figure 4.5 shows the incidence of overtime working by group and workers' preferences. For workers who want to work more (left panel), there was a 3.1 percentage point increase in the incidence of excessive overtime for the treatment group. On the contrary, for workers who want to reduce their hours (right panel), there was only a 3.3 percentage point decrease in incidence immediately after the reform (i.e. in 2011). However, the incidence decreased by 6.5 percentage points in 2012 compared with 2010. Note that the left panel shows that there was a 5.4 percentage point increase in the incidence of extra overtime workers in SMEs in 2011 and a 10.1 percentage point decrease for extra overtime workers in SMEs who want to reduce hours, suggesting the existence of a business trend.

The above figures suggest that firms may not have realised the cost of the increased overtime premium in 2011, right after the reform; however, they did so slightly a year later, when then they might have tried to reduce work hours.

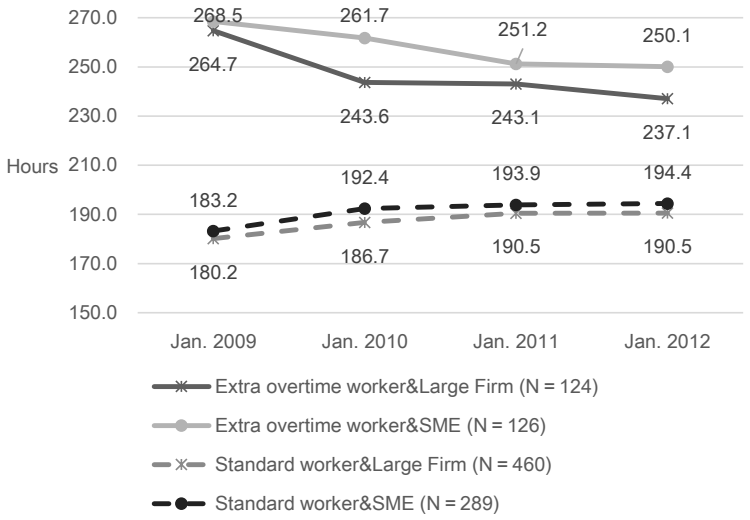

FIGURE 4.2: Average monthly work hours for four groups of respondents (defined by worker status and firm size)

Note: The average monthly work hours for four groups of respondents divided by firm size and worker status in January 2009. The treatment group of this study is extra overtime workers that worked in large firms in January 2009. Respondents who did not answer the work hours question are excluded from the analysis.

The reduction of work hours mainly comes from extra overtime workers in SMEs. In the following section, I thus examine the reform effects on work hours and the incidence of overworking by using a regression framework.

4.5 Estimation Models

The effects of the reform on changes in monthly work hours and the incidence of overtime work are investigated in this section by adopting a policy evaluation framework (For difference-in-difference methods, see for instance Card and Krueger (1994); Gruber (1994); Angrist and Krueger (1999); Angrist and Pischke (2008)). Here, I compare the relative outcomes of workers that worked extra overtime hours in large firms with those that did not work extra overtime hours or with those in SMEs, before and after the reform.

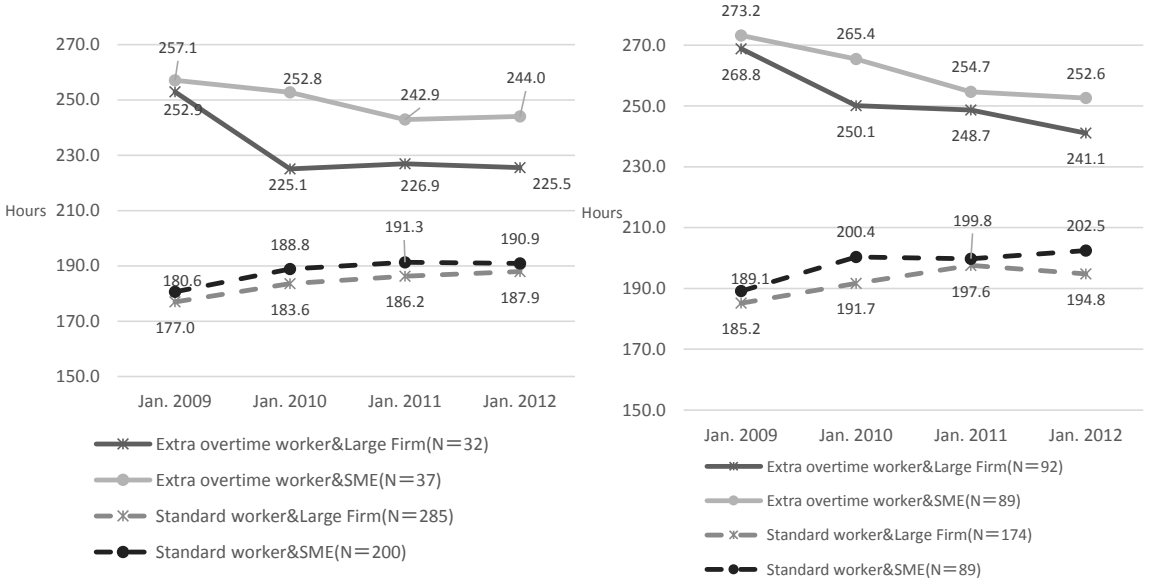

FIGURE 4.3: Average monthly work hours by workers' preferences: (Left) Workers who want to work more (Right) Workers who want to reduce their hours

Note: The average monthly work hours for four groups of respondents divided by firm size and worker status in January 2009. The treatment group of this study is extra overtime workers that worked in large firms in January 2009. Respondents who did not answer the work hours question are excluded from the analysis.

The outcome is changes in monthly work hours or the incidence of extra overtime. Since the reform was only enforced in large firms, the outcomes of workers in large firms are compared with those in SMEs before and after the reform (recall that the reform does not apply to workers in SMEs). Moreover, I include a dummy variable for whether an individual worked more than 60 hours of overtime in the pre-reform period. Then, the outcomes of workers who work extra hours are compared with those of workers who did not by using a difference-in-difference (DID) model.[13] In addition, the interaction of these two effects (extra overtime hours and firm size) is included in a triple difference (TD) model. The effect of the reform is then measured as coefficients of the treatment dummy in

[13]Difference between before and after, and between an individual worked more than 60 hours of overtime and those of workers who did not.

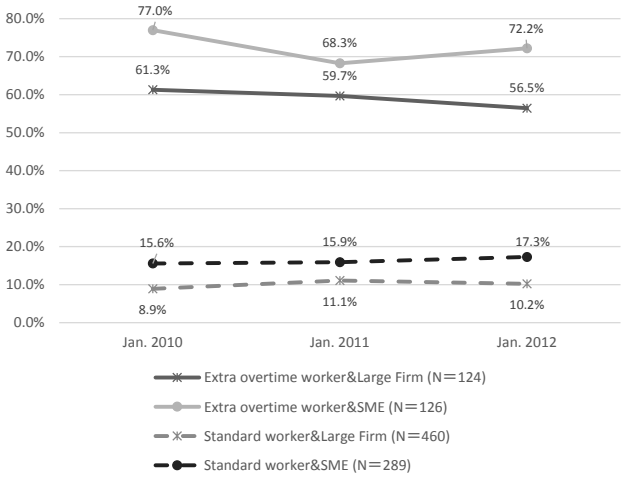

FIGURE 4.4: Incidence of extra overtime by worker status and firm size

Note: The incidence of extra overtime for four groups of respondents divided by firm size and worker status in January 2009. The treatment group of this study is extra overtime workers that worked in large firms in January 2009. Respondents who did not answer the work hours question are excluded from the analysis.

the interaction term in a TD model-average treatment effects on the treated. The DID estimation equation is as follows:

$$\triangle Y_{ib} = \rho R_b + X_{ib}\tau + \epsilon_{ib} \tag{4.7}$$

where $\triangle Y_{ib}$ represents changes in the outcomes of individual i in group b. I estimate two outcomes: monthly work hours in the logarithmic form and the incidence of excessive overtime work (taking the value of 1 if a worker is deemed to be an extra overtime worker and 0 otherwise). X_{ib} a vector of observed characteristics before the reform (sex, age, job title, industry). R_b is either an overwork dummy (when a worker was an extra overtime worker in January 2008) or a firm size dummy. The estimator in equation (4.7) assumes that if there were no reform, the outcome changes for workers in SMEs would have been similar across all worker types. The TD model is stated in equation (4.8):

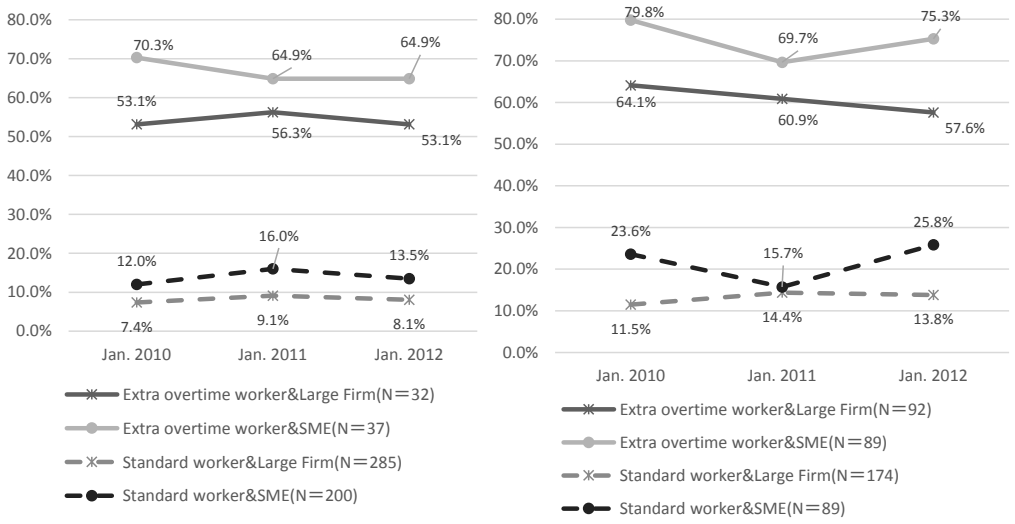

FIGURE 4.5: Extra overtime work by workers' preferences: (Left) Workers who want to work more (Right) Workers who want to reduce their hours

Note: The incidence of extra overtime for four groups of respondents divided by firm size and worker status in January 2009. The treatment group of this study is extra overtime workers that worked in large firms in January 2009. Respondents who did not answer the work hours question are excluded from the analysis.

$$\triangle Y_{ibw} = \rho Firm_b + \beta Over_w + \gamma Firm_b * Over_w + X_{ibw}\tau + \epsilon_{ibw} \tag{4.8}$$

where $Over$ is the indicator variable that identifies extra overtime workers before the reform and Firm is the indicator variable of large firms. γ is thus the average treatment effects on the treated. Further, I estimate equation (4.8) according to workers' preferences, namely those who want to work more hours and those who want to reduce their hours. Models by workers' preferences were investigated in order to examine how the reform effects vary by the autonomous nature of work hours. All models are estimated by using a linear probability model.

	Extra overtime worker &Large Firm	Extra overtime worker &SME	Standard worker &Large Firm	Standard worker &SME
Female (=1)	10.6%	24.4%	39.1%	44.1%
Age	34.1	34.1	34.0	33.5
Job Title				
No Title	44.7%	69.1%	69.5%	78.3%
Team Leader	8.9%	7.3%	7.0%	8.9%
Section Leader	26.0%	6.5%	13.6%	6.8%
Department Chief	17.9%	8.1%	6.6%	3.2%
Department manager	1.6%	5.7%	0.9%	2.1%
Non-respondent	0.0%	0.8%	0.0%	0.4%
Industry				
Agricultural	0.0%	0.0%	0.0%	0.4%
Construction/mining	4.9%	22.0%	0.7%	6.8%
Manufacturing	24.4%	21.1%	28.6%	29.9%
Electricity/water	0.0%	0.0%	1.3%	0.7%
Transportation/operator	5.7%	8.9%	2.4%	6.8%
Wholesale	4.1%	8.1%	4.6%	5.3%
Retailing/Food Service	10.6%	5.7%	5.5%	4.6%
Insurance/Banking/Real Estate	7.3%	2.4%	5.7%	4.6%
Other services	38.2%	31.7%	38.9%	38.4%
Government	4.9%	0.0%	12.3%	2.5%
Sample Size	123	123	455	281

TABLE 4.3: Descriptive statistics of workers by worker status and firm size (characteristics are at the time of January 2009)

Note: The treatment group of this study is extra overtime workers that worked in large firms in January 2009 (the reform was amended in December 2008 and enforced in April 2010). The control group is workers that work in SMEs or that do not work extra overtime. Respondents who did not answer the work hours question are excluded from the analysis.

4.6 Results and Discussion

The descriptive statistics of workers by worker status and firm size in January 2009 are shown in Table 4.3. Although the four groups show a similar age, their other characteristics differ. For example, extra overtime workers are more likely to be men and have a job title above the section leader. The industry classification also varies according to groups; however, the majority of workers work in either the manufacturing or the other services industries. The models estimated below include these covariates to control for the pre-reform difference in characteristics.

The estimated coefficients on changes in monthly work hours or the

	Working hour 2011-2010			Working hour 2012-2010			Incidence of extra overtime 2011-2010			Incidence of extra overtime 2012-2010		
	DID extra overtime	DID firm size	TD	DID extra overtime	DID firm size	TD	DID extra overtime	DID firm size	TD	DID extra overtime	DID firm size	TD
Extra overtime worker(=1)	-0.0444***		-0.0569***	-0.0547***		-0.0613***	-0.0722*		-0.0794	-0.0687*		-0.0642
	(0.0155)		(0.0192)	(0.0162)		(0.0207)	(0.0391)		(0.0532)	(0.0353)		(0.0497)
Large firm(=1)		0.0327**	0.0227		0.0266*	0.0188		0.0266	0.0174		-0.00398	-0.00651
		(0.0146)	(0.0160)		(0.0151)	(0.0166)		(0.0280)	(0.0288)		(0.0264)	(0.0279)
Extra overtime worker*Large firm			0.0290			0.0165			0.0174			-0.0101
			(0.0284)			(0.0298)			(0.0717)			(0.0669)
Female (=1)	-0.0193	-0.00996	-0.0163	-0.00960	0.00100	-0.00736	-0.0255	-0.0121	-0.0234	-0.00442	0.00632	-0.00536
	(0.0146)	(0.0137)	(0.0144)	(0.0152)	(0.0146)	(0.0150)	(0.0268)	(0.0263)	(0.0269)	(0.0262)	(0.0261)	(0.0261)
Age	0.00286	0.00180	0.00224	-0.0157	-0.0169	-0.0160	0.0451	0.0435	0.0447	0.0214	0.0201	0.0216
	(0.0160)	(0.0159)	(0.0160)	(0.0163)	(0.0162)	(0.0164)	(0.0278)	(0.0278)	(0.0278)	(0.0279)	(0.0278)	(0.0280)
Age square	-0.00710	-0.00506	-0.00600	0.0241	0.0264	0.0248	-0.0728*	-0.0698*	-0.0721*	-0.0307	-0.0280	-0.0310
	(0.0235)	(0.0234)	(0.0235)	(0.0240)	(0.0237)	(0.0241)	(0.0420)	(0.0420)	(0.0421)	(0.0422)	(0.0420)	(0.0424)
Job title												
Team Leader (ref: no title)	-0.0294	-0.0308	-0.0317	-0.0433*	-0.0446**	-0.0448**	0.0145	0.0131	0.0131	-0.0734	-0.0738	-0.0726
	(0.0232)	(0.0231)	(0.0232)	(0.0222)	(0.0223)	(0.0223)	(0.0502)	(0.0500)	(0.0501)	(0.0456)	(0.0457)	(0.0458)
Section Leader	-0.00912	-0.0205	-0.0169	-0.00598	-0.0175	-0.0116	-0.00386	-0.0174	-0.00928	0.0152	0.00769	0.0176
	(0.0167)	(0.0181)	(0.0176)	(0.0187)	(0.0197)	(0.0194)	(0.0421)	(0.0420)	(0.0426)	(0.0443)	(0.0440)	(0.0450)
Department Chief	-0.00532	-0.0206	-0.0131	-0.0138	-0.0301	-0.0194	0.113*	0.0932	0.108	0.0473	0.0336	0.0497
	(0.0250)	(0.0257)	(0.0258)	(0.0235)	(0.0243)	(0.0244)	(0.0666)	(0.0661)	(0.0674)	(0.0574)	(0.0583)	(0.0590)
Department manager	-0.0462	-0.0467	-0.0392	-0.0431	-0.0465	-0.0380	-0.0736	-0.0797	-0.0687	-0.0421	-0.0538	-0.0443
	(0.0362)	(0.0349)	(0.0341)	(0.0326)	(0.0329)	(0.0317)	(0.0762)	(0.0745)	(0.0757)	(0.0905)	(0.0929)	(0.0903)
Non-respondent	-0.181	-0.174	-0.161	-0.119	-0.118	-0.104	-0.00470	-0.00792	0.00940	0.00747	-0.0128	0.00129
	(0.112)	(0.125)	(0.107)	(0.0888)	(0.106)	(0.0862)	(0.0364)	(0.0308)	(0.0439)	(0.0387)	(0.0284)	(0.0415)
Industry												
Agricultural (ref: Manufacturing)	0.208***	0.237***	0.221***	0.322***	0.350***	0.333***	-0.0726**	-0.0411	-0.0624	-0.0347	-0.0215	-0.0385
	(0.0208)	(0.0194)	(0.0197)	(0.0212)	(0.0200)	(0.0204)	(0.0338)	(0.0387)	(0.0381)	(0.0295)	(0.0327)	(0.0328)
Construction/mining	0.00766	0.00417	0.0220	0.0116	0.00154	0.0220	-0.0489	-0.0657	-0.0389	0.0395	0.0113	0.0350
	(0.0239)	(0.0234)	(0.0241)	(0.0236)	(0.0233)	(0.0238)	(0.0664)	(0.0648)	(0.0684)	(0.0581)	(0.0556)	(0.0590)
Electricity/water	0.171	0.171	0.166	0.137	0.140	0.133	-0.166	-0.160	-0.169	-0.139	-0.128	-0.138
	(0.140)	(0.139)	(0.139)	(0.114)	(0.114)	(0.114)	(0.109)	(0.112)	(0.110)	(0.121)	(0.122)	(0.120)
Transportation/operator	0.0361	0.0346	0.0410	0.0338	0.0293	0.0377	-0.0555	-0.0631	-0.0519	-0.0593	-0.0722	-0.0608
	(0.0456)	(0.0457)	(0.0451)	(0.0471)	(0.0469)	(0.0467)	(0.0667)	(0.0655)	(0.0668)	(0.0605)	(0.0597)	(0.0609)
Wholesale	0.0233	0.0209	0.0256	0.0311	0.0274	0.0326	-0.103*	-0.108*	-0.101*	-0.0400	-0.0467	-0.0407
	(0.0587)	(0.0587)	(0.0586)	(0.0569)	(0.0570)	(0.0570)	(0.0610)	(0.0603)	(0.0611)	(0.0733)	(0.0726)	(0.0738)
Retailing/Food Service	-0.0112	-0.0201	-0.0148	-0.0331	-0.0429*	-0.0356	0.00856	-0.00381	0.00609	0.0335	0.0240	0.0346
	(0.0217)	(0.0219)	(0.0221)	(0.0244)	(0.0251)	(0.0248)	(0.0566)	(0.0554)	(0.0569)	(0.0561)	(0.0556)	(0.0560)
Insurance/Banking/Real Estate	-0.0297	-0.0358	-0.0341	-0.0215	-0.0275	-0.0247	-0.0514	-0.0584	-0.0545	-0.0121	-0.0156	-0.0108
	(0.0229)	(0.0237)	(0.0232)	(0.0236)	(0.0242)	(0.0240)	(0.0706)	(0.0722)	(0.0708)	(0.0605)	(0.0606)	(0.0600)
Other services	0.00568	0.00166	0.00417	-0.0198	-0.0242	-0.0209	-0.0305	-0.0360	-0.0315	-0.0117	-0.0159	-0.0113
	(0.0154)	(0.0156)	(0.0155)	(0.0164)	(0.0166)	(0.0165)	(0.0330)	(0.0329)	(0.0329)	(0.0320)	(0.0320)	(0.0321)
Government	-0.0341	-0.0406	-0.0424	-0.0145	-0.0181	-0.0211	-0.0494	-0.0515	-0.0556	-0.00350	0.00386	-0.00102
	(0.0304)	(0.0312)	(0.0312)	(0.0312)	(0.0319)	(0.0320)	(0.0425)	(0.0430)	(0.0432)	(0.0412)	(0.0415)	(0.0419)
Constant	0.0203	0.00444	0.0154	0.287	0.274	0.281	-0.622	-0.635	-0.627	-0.339	-0.337	-0.338
	(0.263)	(0.261)	(0.263)	(0.270)	(0.267)	(0.272)	(0.450)	(0.449)	(0.449)	(0.455)	(0.454)	(0.456)
Observations	982	982	982	982	982	982	982	982	982	982	982	982
R-squared	0.025	0.023	0.030	0.029	0.022	0.031	0.021	0.016	0.021	0.013	0.008	0.014

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 4.4: Estimated coefficients on changes in work hours and the incidence of extra overtime

Note: The estimated coefficients on changes in monthly work hours or the incidence of overtime work from models 4.7 and 4.8. Covariates are measured in January 2009. Robust standard errors are in parentheses. Note that the clustered standard errors were smaller than the robust standard errors (Donald and Lang (2007); Angrist and Pischke (2008))

incidence of overtime work from models 4.7 and 4.8 are shown in Table 4.4. The DID model in rows 1 and 2 shows a 4.4% decrease in work hours for overtime workers and a 3.3% increase for workers in large firms. By contrast, the TD model shows no difference in work hours for extra overtime workers in treatment firms. No significant treatment effects were thus found for total work hours.

In order to check the robustness of the results, an alternative model

(i.e., outcome is changes in monthly work hours from 2010 to 2012) was run to investigate the mid-term treatment effects. Rows 1 and 2 shows similar results to the main model; again, the TD model estimates show no significant effect of the reform. The incidence of extra overtime models shows similar effects. There is no significant effect of the reform on the changes in incidence of extra overtime.

To assess whether the effect of the reform varies by workers' preferences, I run models 4.7 and 4.8 separately for workers who want to work more and those who want to reduce their hours. The results show no significant effect of the reform (Table 4.5). The coefficients shown are the treatment effect terms ρ in model 4.7 and γ in model 4.8. As shown in row 2, there was a 6.6% decrease in work hours for extra overtime workers and a 6.5% increase in work hours for workers in large firms after the reform. However, the TD model results show no significant treatment effect, while the models of the incidence of overtime show no significant effect of the reform either.

The presented results are consistent with those of Trejo (2003). Because the 2010 reform did not restrict standard wages, changes in the overtime premium might have been offset by changes in standard hourly wages, leaving work hours and total earnings unchanged. The coefficients from the changes in wages regressions show no significant effect of the reform either (Table 4.6).

The other possible explanation of the presented result is the existence of unpaid overtime. So-called 'service overtime' is prevalent in Japan, where workers do not report their actual work hours. According to the Ministry of Health, Labour, and Welfare, 1312 firms in fiscal year 2011 received a penalty for using

	Working hour 2011-2010			Working hour 2012-2010			Incidence of extra overtime 2011-2010			Incidence of extra overtime 2012-2010		
	DID extra overtime	DID firm size	DID	DID extra overtime	DID firm size	TD	DID extra overtime	DID firm size	TD	DID extra overtime	DID firm size	TD
Workers who want to work more (n=545)	-0.0209	0.0147	0.0603	-0.0133	0.0180	0.0422	-0.0315	-0.0120	0.0797	-0.000488	-0.00593	0.0427
	(0.0312)	(0.0138)	(0.0670)	(0.0329)	(0.0153)	(0.0695)	(0.0667)	(0.0344)	(0.124)	(0.0674)	(0.0333)	(0.126)
Workers who want to reduce their hours (n=436)	-0.0663**	0.0650**	0.00162	-0.0800***	0.0452	0.00837	-0.0656	0.0950**	-0.0872	-0.0968**	0.00689	-0.0508
	(0.0263)	(0.0292)	(0.0424)	(0.0265)	(0.0289)	(0.0428)	(0.0535)	(0.0470)	(0.100)	(0.0470)	(0.0426)	(0.0914)

TABLE 4.5: Estimated coefficients of work hours and worker status

Note: The estimated coefficients on changes in monthly work hours or the incidence of overtime work from models 4.7 and 4.8. The models are estimated separately for workers who want to work more and those who want to reduce their hours. Covariates are measured in January 2009. Robust standard errors are in parentheses. Note that the clustered standard errors were smaller than the robust standard errors (Donald and Lang (2007); Angrist and Pischke (2008))

service overtime, with these unpaid overtime hours eventually paid to 117,002 employees.[14]. The findings of this study thus confirm the importance of the government considering stronger regulations towards unpaid work, while regulating the standard wage might also be an effective strategy considering that some workers overwork because their wages are so low. Because long work hours are deeply rooted in Japanese culture, a drastic reform is thus necessary to improve working conditions considerably.

4.7 Conclusion

In this study, I evaluated the reform of the Labour Standards Act enacted in April 2010, which tried to reduce excessive overtime and thus promote better work life balance for employees by doubling the overtime premium for extra overtime workers, as they are described herein, that work in large firms. The reform generated

[14]The details can be obtained from the Ministry of Health, Labour, and Welfare website http://www.mhlw.go.jp/stf/houdou/2r9852000002lrsc.html.

an exogenous variation in the marginal cost of assigning extra overtime, which I exploit in order to examine how the reform affected work hours and the incidence of excessive overtime.

The estimation results from the TD models showed that there were no changes in work hours or in the incidence of overworking following the introduction of the reform for the treatment group. This finding also applied to workers that wanted to either increase or reduce their total number of hours worked. The findings presented herein suggest the prevalence of unpaid overtime in Japan. For example, workers might choose to work longer hours in order to gain recognition from their bosses and/or earn future promotions. This study thus highlights the importance of the Japanese government considering regulation on unpaid overtime.

For further research, it is important to investigate how standard wages have changed after the reform, because as Trejo (2003) suggested, the examined changes in the overtime premium might have been offset by changes in standard hourly wages, leaving work hours and earnings unchanged.[15] It is also important to document changes in unpaid work. Unfortunately, this study fails to capture how many overtime hours are paid and unpaid. Further investigation on the possibility that the reform might have reduced paid extra overtime hours is needed.

[15]The coefficients from the changes in wages regressions show no significant effect of the reform, however the wage measure of this study includes overtime premium (Table 4.6).

	Hourly wage 2011-2010			Hourly wage 2012-2010		
	DID extra overtime	DID firm size	TD	DID extra overtime	DID firm size	TD
Extra overtime worker(=1)		-0.0865	-0.230		-0.0169	-0.158
		(0.193)	(0.213)		(0.235)	(0.253)
Large firm(=1)	-0.0131		-0.331	-0.361		-0.621
	(0.250)		(0.359)	(0.281)		(0.418)
Extra overtime worker*Large firm			0.584			0.481
			(0.476)			(0.545)
Female (=1)	-0.139	-0.144	-0.137	-0.160	-0.105	-0.154
	(0.217)	(0.209)	(0.216)	(0.245)	(0.233)	(0.242)
Age	-0.177	-0.177	-0.186	0.0992	0.0909	0.0931
	(0.196)	(0.195)	(0.197)	(0.247)	(0.246)	(0.246)
Age square	0.293	0.292	0.307	-0.197	-0.181	-0.187
	(0.306)	(0.305)	(0.307)	(0.383)	(0.382)	(0.382)
Job title						
Team Leader (ref: no title)	0.698*	0.699*	0.673	0.443	0.440	0.425
	(0.410)	(0.410)	(0.410)	(0.549)	(0.552)	(0.550)
Section Leader	0.363	0.377	0.341	0.296	0.248	0.271
	(0.313)	(0.320)	(0.317)	(0.401)	(0.421)	(0.409)
Department Chief	-0.399	-0.386	-0.417	-0.276	-0.355	-0.295
	(0.444)	(0.452)	(0.448)	(0.398)	(0.401)	(0.413)
Department manager	0.897	0.879	0.904	0.228	0.177	0.239
	(0.895)	(0.895)	(0.899)	(1.214)	(1.220)	(1.228)
Non-respondent	0.101	0.0468	0.118	0.505**	0.398*	0.538*
	(0.226)	(0.238)	(0.313)	(0.213)	(0.226)	(0.280)
Industry						
Agricultural (ref: Manufacturing)	0.0663	0.0190	-0.0712	0.160	0.229	0.0652
	(0.257)	(0.268)	(0.283)	(0.277)	(0.278)	(0.272)
Construction/mining	0.386	0.344	0.415	0.893	0.740	0.931
	(0.546)	(0.523)	(0.533)	(0.585)	(0.567)	(0.588)
Electricity/water	1.146	1.166	1.179	3.264	3.320	3.284
	(2.128)	(2.126)	(2.134)	(2.713)	(2.715)	(2.719)
Transportation/operator	0.606	0.588	0.577	0.545**	0.485*	0.526**
	(0.420)	(0.405)	(0.416)	(0.259)	(0.249)	(0.264)
Wholesale	0.134	0.127	0.150	0.950**	0.911**	0.964**
	(0.214)	(0.205)	(0.214)	(0.435)	(0.431)	(0.440)
Retailing/Food Service	-0.0812	-0.0776	-0.0906	-0.705	-0.749	-0.707
	(0.573)	(0.566)	(0.573)	(0.626)	(0.613)	(0.625)
Insurance/Banking/Real Estate	0.629	0.637*	0.618	0.714	0.695	0.703
	(0.382)	(0.377)	(0.387)	(0.688)	(0.684)	(0.686)
Other services	0.0344	0.0362	0.0353	0.322	0.298	0.320
	(0.246)	(0.242)	(0.248)	(0.296)	(0.291)	(0.297)
Government	0.157	0.185	0.203	0.669**	0.709**	0.699**
	(0.377)	(0.384)	(0.380)	(0.292)	(0.300)	(0.298)
Constant	2.352	2.398	2.618	-1.408	-1.380	-1.225
	(3.001)	(3.013)	(3.029)	(3.870)	(3.900)	(3.913)
Observations	964	964	964	956	956	956
R-squared	0.016	0.017	0.018	0.026	0.024	0.026

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

TABLE 4.6: Estimated coefficients on changes in hourly wage

Note: The estimated coefficients on changes in hourly wage from models 4.7 and 4.8. Covariates are measured in January 2009. Robust standard errors are in parentheses. Note that the clustered standard errors were smaller than the robust standard errors (Donald and Lang (2007); Angrist and Pischke (2008))

Chapter 5

Conclusion

In this dissertation, I evaluated three governmental policies related to families: parental leave, the short-time hours scheme for employees with children, and the increase in the overtime premium. Although this dissertation focused on Japan, its findings add to the body of knowledge on how family policies affect particular economic environments, cultures, and labour market institutions. It also showed the importance of investigating the fundamental causes of labour market issues such as the lack of childcare availability and prevalence of unpaid overtime before implementing family policies. Although the Japanese family policies investigated in this dissertation are well structured and generous, the national government has failed to examine these fundamental causes prior to implementation, which has resulted in unintended outcomes.

Chapter 2 assesses the impact of changes in the PL income replacement rate on mothers' job continuity surrounding childbirth. 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 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. Compared to other countries' reforms, Japanese PL provides stronger financial incentives for mothers to return to their pre-birth employers. 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 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. 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. Japan's PL reforms increased mothers' marginal wages only in the childbirth year; however Japanese mothers face considerable hardships with childrearing when they return to work. This reform is 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.

Chapter 3 examines both the supply and demand effects of introducing

the short work hour provisions. Since June 2010, firms in Japan with more than 100 employees have been required to formulate a short work hour provisions for use by employees with children under the age of three. Qualified employees are allowed to work six hours per day instead of eight, and they are also exempt from overtime work. The new scheme makes it possible for new parents to manage work and childcare demands while maintaining their generous employee benefits plan; in this way, the scheme can contribute to job continuity among new parents. On the other hand, since there are fixed labour costs independent of work hours, the short work hour provisions will increase the labour cost per unit of time. Based on corporate social responsibility data from Japanese firms, I examine the effects of scheme introduction on hiring, job turnover, and overtime hours. The causal effects are identified by comparing the outcomes of firms that already had the scheme in place before the reform to those of firms that had not. Contrary to expectations, the results show an increase in the proportion of newly hired female college graduates among firms that had introduced the scheme after the reform. I also found reform implementation to have no effect on labour supply. This result suggests the possibility that skilled females had been 'sorted' to firms that featured generous programmes before the reform. I conclude that the short-time working scheme may have contributed to attracting skilled female labour into treatment firms.

Chapter 4 assesses how the increase in the overtime premium paid to workers that carry out excessive overtime influences work hours and the incidence

of overtime. In April 2010, the Japanese government reformed the Labour Standards Act, which increased the overtime premium that companies have to pay their employees from 25% to 50%. This reform, which only applied to overtime of more than 60 hours per month and workers in large firms, generated an exogenous variation in the marginal cost to employers of assigning extra overtime. Based on data derived from the Japanese Life Course Panel Survey conducted by the University of Tokyo from 2007 to 2013, the presented findings suggest that despite the overtime premium doubling, there has been no change in work hours or in the incidence of overtime since the introduction of the reform, suggesting the prevalence of unpaid overtime in Japan. For example, workers might choose to work longer hours in order to gain recognition from their bosses and/or earn future promotions. This study thus highlights the importance of the Japanese government considering regulation on unpaid overtime.

In the context of the low employment rate and long work hours of mothers, governmental policies related to families are necessary. The female employment rate in Japan is approximately 65% for women aged 25 to 49, which is 10 percentage points lower than the OECD average. Moreover, the maternal employment rate is approximately 30% for mothers with children three years of age or younger, which is 30 percentage points lower than the average of OECD countries. The long work hours in Japan are one barrier to increasing female employment. Most jobs are demanding, with approximately 30% of women working 50 hours or more per week and 10% working 60 hours or more. As discussed in this dissertation, Japanese family policies have not had the intended effect, largely because of

the lack of prior investigation into fundamental issues such as the unavailability of childcare and prevalence of unpaid overtime. Hence, this dissertation contributes to the literature by clarifying that the effects of family policies vary according to the economic environment, culture, and labour market institutions.

Appendix A

An Appendix

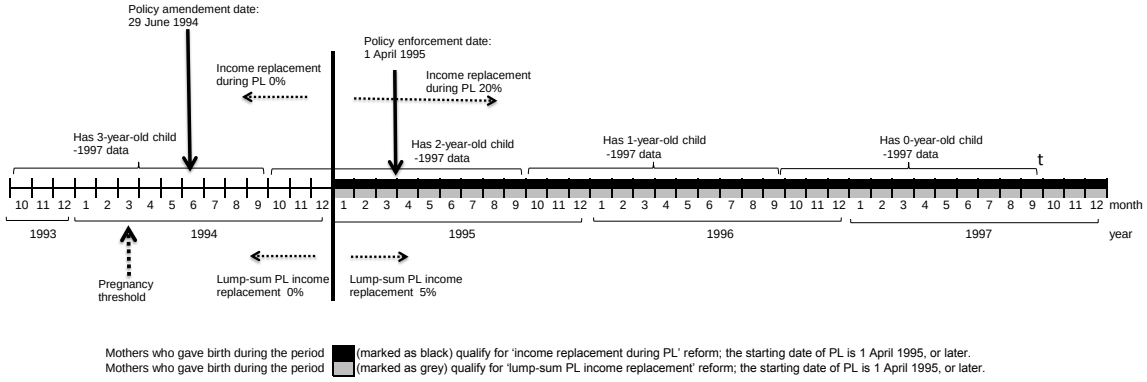

FIGURE A.1: Identification Strategy and the Data Structure of the 1995 Reform

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 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. The policy was amended in June 1994 and enacted in April 1995.

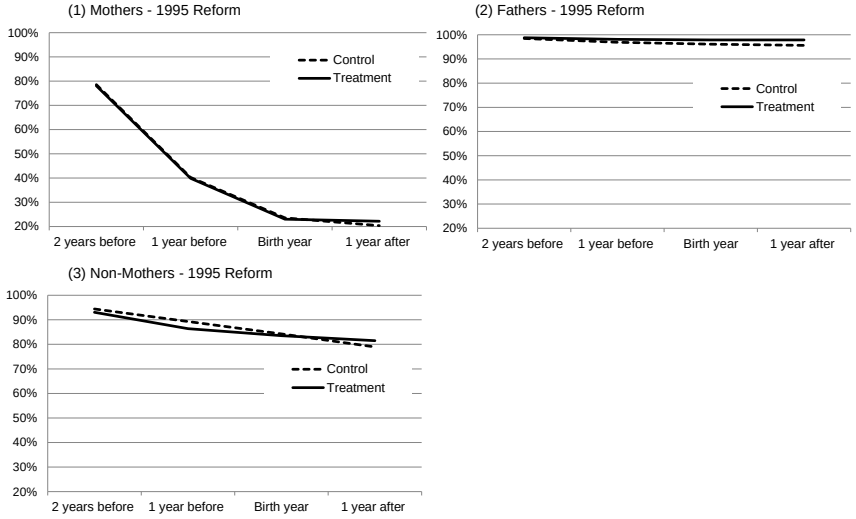

FIGURE A.2: Job Continuity around the Time of Giving Birth for the Treatment and Control Groups: the 1995 Reform

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.

	2001 Reform				1995 Reform	
	Treatment vs.		Treatment vs.		Treatment vs.	
LPM	Control		Second Control		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.1: 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.*

		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.2: 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.

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