Effect of introducing a parental leave policy on long-run maternal employment

By

Taiyo Fukai
(The University of Tokyo)

Takahiro Toriyabe
(The University of Tokyo and The University College London)

September 2021

CREPE DISCUSSION PAPER NO. 108

CENTER FOR RESEARCH AND EDUCATION FOR POLICY EVALUATION (CREPE)
THE UNIVERSITY OF TOKYO
http://www.crepe.e.u-tokyo.ac.jp/

Effect of introducing a parental leave policy on long-run maternal employment*

Taiyo Fukai† Takahiro Toriyabe‡

September 27, 2021

Abstract

Parental leave is one of the most popular policies for promoting postpartum career continuation for mothers. Drawing on micro data from the Japanese population census, we evaluated the long-run impact of taking parental leave on maternal employment, using as natural experiments the parental leave reforms of 1992 and 1995. We found that both of these reforms increased full-time employment while decreasing part-time employment in the long-run. Our results suggest the reforms unambiguously strengthened the labor market attachment of mothers, allowing those who would otherwise engage in part-time work after childbirth to continue in full-time positions.

Keywords: Parental leave, Female labor supply, Policy take-up
JEL codes: J08, J13, J21

---

*This research is supported by JSPS KAKENHI Grant Numbers 15H05692, 16J08836 and 18J21486. The use of data in this paper has been approved by the Ministry of Internal Affairs and Communications and the Ministry of Health, Labour and Welfare. For valuable comments, we thank Stacey Chen, Christian Dustmann, Andrew Griffen, Ryo Kambayashi, Hyejin Ku, Daiji Kawaguchi, Keisuke Kawata, Ayako Kondo, Teresa Molina, Masato Oikawa, Fumio Otake, Uta Schönberg, Ryuichi Tanaka and seminar participants at the University of Tokyo, the Japan Economics Association Meeting, Tokyo Labor Economics Workshop, Tohoku University, Sophia University, Society of Labor Economists 2021 Conference, Asian and Australasian Society of Labour Economics 2018 Conference, WEAI 15th International Conference, and Centre for Research and Analysis of Migration. We also thank the Ministry of Health, Labour and Welfare for its guidance on Japan's parental leave policies. Editing services have been provided by Philip C. MacLellan. Any remaining errors and omissions are the responsibility of the author.

†Department of Economics, The University of Tokyo. E-mail: fukai@e.u-tokyo.ac.jp

‡Department of Economics, The University of Tokyo and Centre for Research and Analysis of Migration, The University College London. E-mail: ttoriyabe@g.ecc.u-tokyo.ac.jp

1 Introduction

While the expansion of family-friendly policies in recent years has coincided with a shrinking gender gap in various labor market outcomes, the causal relationship is not obvious, and family policies sometimes reinforce traditional gender roles (Albrecht et al., 2003; Blau and Kahn, 2013; Fernández-Kranz and Rodríguez-Planas, 2013; Canaan, 2019). At the same time, the gender gap is still non-negligible, especially when measured across the wage distribution, and there still exist examples such as the "child penalty", which is a sharp and persistent drop in earnings after childbirth despite a prenatal earnings trajectory similar to that of women without children (Kleven et al., 2019). This dip is associated with changes in occupation, a shift to a "family-friendly" work environment, reduced work time and effort of mothers, and/or the shift to the "mommy track" at work (Fernández-Kranz et al., 2013; Kleven et al., 2018). In line with these observations, Goldin (2014) points out that the gender wage gap is closely related to the compensated wage differential in terms of work flexibility. As these studies suggest that childbirth changes the work attitudes and environments of women as they attempt to reconcile work and family responsibilities, what, then, would be an effective family policy to promote the careers of women after childbirth?

One of the most popular policy instruments for this purpose is parental leave, and according to the OECD family database, most developed countries have been expanding their parental leave programs for decades. A parental leave policy typically guarantees a mother's right to be reinstated in her original post or its equivalent after the leave, and provides cash benefits during the leave to smooth income fluctuations. However, although a generous parental leave policy is expected to facilitate the career continuation of mothers, it is costly for employers as it requires additional human resource management. One concern is that this extra cost might potentially induce firms to shift the costs of parental leave (whether monetary or not) onto the wages of female workers (Gruber, 1994), or to statistically discriminate female workers (Thomas, 2018). In addition, there is also a concern that a leave of long duration may result in human capital depreciation. Considering these potential negative aspects, an effective policy must successfully navigate an appropriate level of generosity; one that is too generous will be harmful to the labor market opportunities of mothers, whereas one that is too limited will not be useful for mothers.

Following the expansion of parental leave programs, a number of empirical studies have evaluated the policy effects on maternal labor market outcomes. Cross-country studies find mixed results in that the parental leave increases the female employment rate but widens the gender wage gap, and these policy impacts are heterogeneous across educational or skill levels (Ruhm, 1998; Thévenon and Solaz, 2013; Olivetti and Petrongolo, 2017; Kawaguchi and Toriyabe, 2018). On the other hand, a majority of recent studies using a discontinuous feature of the policy reforms to ensure clean identification find the policy impact to be large in the short-run but negligible in the long-run. In the short-run, the extension of the maximum duration of parental leave causes a delay in mothers' return to work and increases their time with their children (Baker and Milligan, 2008; Lalive and Zweimüller, 2009; Dustmann and Schönberg, 2012; Schönberg and Ludsteck, 2014; Dahl et al., 2016).[1,2] However, despite the delay in the return to work, the impact becomes attenuated over time, and their market outcomes catch up in the long-run (Lalive and Zweimüller, 2009; Schönberg and Ludsteck, 2014; Carneiro et al., 2015; Dahl et al., 2016).

However, these findings of negligible long-run effects do not necessarily mean that parental leave does not help the career continuation of mothers, because most previous studies have only examined the effect of extending parental leave. In other words, the take-up rate of parental-leave is not well considered in the literature, and this extensive margin seems crucial, particularly when the parental leave program is first introduced or when the conditions of a limited parental leave program are expanded. Although some studies have evaluated the extension of short parental leave, they do not investigate the effect through the extensive margin because the take-up rate was already high before the reform (Carneiro et al., 2015) or because of a sample restriction focusing only on those who took maternity leave (Schönberg and Ludsteck, 2014). One German study by Kluve and Schmitz (2018) which does estimate the effect of a policy reform as a mixture of the extensive and intensive margins finds a positive long-run impact. Therefore, while the impact of the introduction of a policy seems to be distinct from one that is merely extended, the policy impact of the extensive margin is understudied in the literature.

To investigate the effect of taking parental leave, this study evaluated the long-run labor market outcomes of two major parental leave reforms in Japan; namely, the job protection policy in 1992 and the paid leave policy in 1995. The 1992 reform introduced unpaid leave with job protection until the child's first birthday, while the 1995 reform introduced cash benefits and exemption from paying social insurance premiums during the leave, which together amounted to about 37 percent of pre-leave earnings. Additionally, eligibility for parental leave was expanded in 1995. The first reform is particularly useful for separating the effect of job protection from that of cash benefits, while the second reform allows us to estimate the impact of cash benefits and also of eligibility expansion that increased the take-up rate of parental leave.

Our empirical analysis draws on the micro data of the Japan population Census between 1990 and 2010, with the dataset from each Census containing information on more than one million

[1]The policy impact on child development tends to negligible (Gregg et al., 2005; Baker and Milligan, 2010; Rasmussen, 2010; Dustmann and Schönberg, 2012; Dahl et al., 2016), though some studies in the U.S. find a positive impact (Berger et al., 2005; Rossin, 2011).

[2]Baum (2003) and Baker and Milligan (2008), who find no short-run impact of a short job protection policy, attribute their null results to private arrangements that mothers could make even in the absence of formal job protection.

newborns. The large data set is indispensable in the Japanese context, for only a small proportion (about 5 percent) of mothers took parental leave in the 1990s. Asai (2015), for example, who evaluates the short-run impact of the 1995 reform in Japan using the Employment Status Survey, which has a sampling rate of only about 1 percent, does not find any policy impact, presumably due to lack of statistical power. Thus, our population data facilitates the detection of the policy impact, and to evaluate both the short- and long-run policy impacts, the maternal market outcomes are measured by the employment status 0–15 years after childbirth.

In order to estimate the policy impact, we exploited two different identification strategies. First, using the 2000 and 2005 Census, we conducted a difference-in-differences (DID) estimation in which those who gave birth several years after the policy was implemented were compared with those who gave birth in 1991, one year before the job protection policy reform. Since the child's age in 2005 depends on the birth year, the policy effect is not separated from the child age effect in this simple comparison, so we controlled for the child age effect by using mothers with children of the same age in the 2000 Census who were not eligible for the parental leave. The estimation results show that while the 1992 job protection policy did not affect the mother's labor market outcome in the reform year, it had a positive impact 1–2 years after implementation, and this effect was further amplified by the second reform in 1995. In particular, the parental leave policies increased full-time employment in the long-run while decreasing part-time employment, thus not affecting the employment rate since these two impacts offset each other. However, as our empirical evidence suggests that the provision of unpaid leave helped mothers to continue in their full-time jobs, the main beneficiaries of the parental leave policy were those who, in absence of the policy, would have quit their jobs at childbirth, only to return to the labor market later. Given the rigid labor market in Japan whereby the port of entry to a career job is concentrated around school graduation (Imai and Itami, 1984; Genda et al., 2010), mothers quitting their jobs would have difficulty obtaining positions equivalent to their original posts and would thus likely end up working part-time. Thus, by protecting their full-time positions with income compensation, the dual parental leave policies were effective in enabling women to navigate the labor structure in Japan and continue their careers.

Since this DID analysis would be invalidated if mothers giving birth several years after the reform are selective in terms of market outcomes, following the literature (e.g. Lalive and Zweimüller (2009)), we also conducted a regression discontinuity Design (RDD)-DID analysis. Given that the accessibility to paid and unpaid leave depends on the birth timing, we identified the policy impact through the regression discontinuity design (RDD) using the birth timing as running variable, combined with a difference-in-differences (DID) method to control for potential seasonality in birth timing. While this RDD-DID method provides sharper identification than the first estimation method, it tends to result in imprecise estimates because it focuses on the local part of the data, so in this sense, the two methods complement each other. From the RDD-DID estimation, we found that the paid leave policy increased the take-up rate of parental leave in the short-run and increased full-time employment in the long-run while decreasing part-time employment, leaving no impact on overall employment. Furthermore, the two-sample Wald estimate constructed from these short-run and long-run effects indicates that the take-up of parental leave increased (decreased) the probability of being a full-time (part-time) worker in the long-run by around 30 percentage points. Therefore, the findings of this study contrast sharply with previous studies that have found only a negligible effect of extending the duration of parental leave.

Turning to the policy implications, in most developed countries, the duration of parental leave is at most 3 years, and thus a mother needs to make some childcare arrangement to continue her career after the parental leave ends. In Japan and some European countries such as Denmark, Germany and Norway, a major option is highly subsidized public childcare services that offer full-time daycare for pre-school-age children, but those countries have struggled to expand the capacity of childcare centers, particularly for 1–2 year old children, as the care of toddlers requires intensive human resources. Despite the important connection between parental leave and public childcare provision, to the best of our knowledge, no study to date has evaluated the interaction of these policies, so to fill this gap in the literature, we conducted a heterogeneity analysis using regional variation in public childcare availability and found suggestive evidence that the parental leave policy is more effective when public childcare is accessible. Therefore, given the negative consequences of a parental leave that is too long (Canaan, 2019), an effective policy design would be a combination of parental leave and early childcare.

Our study is closely related to the British study by Stearns (2018), who also evaluates the effect of parental leave by focusing on the extensive margin. She finds that the introduction of job-protected leave with some cash benefits increased maternal employment but did not affect work hours in the long-run, and she also finds some negative impact on promotion, particularly among the high-skilled group. One advantage of our study is that the temporal separation of the two Japanese reforms allows us to make a more fine-grained analysis since the first reform provided job protection but not cash benefits. While Stearns (2018) argues that the cash benefit was not the main channel of her results as its magnitude was small, we are able to actually isolate the effect of unpaid job-protected parental leave. Another difference between the two studies is that the Japanese and British policies seem to have different eligibility criteria. While most working mothers are eligible in Britain, the Japanese system requires mothers to have a non-fixed-term contract and tenure in the position greater than one year. Therefore, the characteristics of the mothers affected by the policy reforms seem to be different in the two countries, and this may be the reason why the Japanese parental leave reform had a different impact on maternal employment.

Other study related to this paper is Yamaguchi (2019), which also evaluates the effect of parental leave using Japanese data. He estimates a dynamic discrete choice model, and his simulation results show that the legislation of job-protected leave for a year is effective for maintaining the maternal labor supply but any duration additional to one year is ineffective. Since our study took a design-based approach using the policy reforms as a natural experiment while Yamaguchi (2019) takes a model-based approach to reveal the mechanism of the policy impact, we believe that these two studies complement each other. Furthermore, since the parental leave policies that we investigated in this study entitled mothers to take the parental leave up to one year, our findings of the positive policy impact is consistent with his study.

2 Background information

2.1 Parental leave system in Japan

This section describes the childbirth leave policy in Japan, which makes a clear distinction between maternity leave and parental leave. The maternity leave policy dates back to the early 20th century, and was first legislated by the amended Factory Act in 1926. This maternity leave protection was continued by the successor Labor Standards Act in 1947, which prohibits a woman from working 6 weeks before and 8 weeks after childbirth (Article 65). Furthermore, an employer is not allowed to dismiss a woman during maternity leave or within 30 days thereafter (Article 19). During the maternity leave, a woman receives cash benefits that amount to two-thirds of the average earnings in the past 12 months if she is covered by national Health Insurance (Health Insurance Act, Articles 99 and 102).[3]

In contrast to maternity leave, the parental leave system is relatively new, with the 1972 Working Women Welfare Act requiring employers to use "best efforts" to adjust maternal work conditions, but not making any adjustments mandatory. Then, in 1975, job protection was provided for women working in public educational and medical sectors until their child's first birthday, but the first universal job protection was not enacted until May 1991 and implemented in April 1992, with the Act on Childcare Leave, Caregiver Leave, and Other Measures for the Welfare of Workers Caring for Children or Other Family Members.[4] This legislation allowed a mother to take leave until the first birthday of her child if her job tenure was one year or longer.[5] Establishments with 30 employees or

[3]Health Insurance typically covers full-time employees whose labor contract is expected to last for more than a year, while it does not cover most part-time employees (Health Insurance Act, Article 3).

[4]This applied to establishments with 30 or more employees at this time, and this was extended to all establishments in April 1995.

[5]This legislation was for private employees, but quite similar laws were implemented for public employees.

fewer, however, were exempted from this legislation until the second reform in April 1995, which also abolished the establishment size restriction. Importantly, this law was grandfathered in, so those who gave birth before the enactment of the law were still eligible for job protection as long as they satisfied the above eligibility conditions, though they needed to wait until April 1, 1992 to take the job-protected leave. The duration of the job protection was then extended from 10 to 16 months in April 2005 and to 22 months in October 2017 only for mothers whose children are not able to enroll in a childcare center due to capacity constraints. This study focuses on the 1992 reform that initiated universal parental leave job protection and not the 2005 and 2017 reforms that extended its duration.

While the parental leave reform of 1992 was unpaid, cash benefits were introduced in April 1995, allowing a mother to receive these benefits during a leave period after April 1, 1995 and until the first birthday of her child if she was employed as a full-time worker for at least 12 months within the past two years,[6], with the amount of the benefits being a quarter of the average monthly income during the past 6 months. In addition to this income replacement, a mother is also exempted from paying pubic health and pension insurance premiums during the leave and, further, the parental leave benefits are not taxed (Health Insurance Act, Article 76 and Welfare Pension Insurance Act, Articles 12, 82-2, 139 (5) and (6)). According to the 1995 Annual Health and Welfare Report of the Ministry of Health, Labour and Welfare (MHLW), these public insurance premiums, on average, amounted to 12 percent of monthly earnings, indicating an effective replacement rate of around 0.37.[7]

Similar to the job protection policy, the cash benefit policy was also grandfathered in, so that those who gave birth before the enactment of the law are also eligible as long as they satisfy the eligibility conditions. As a result, the effective (or average) replacement rate depends on the timing of the birth and the duration of the leave (Figure 5), because a leave after April 1995 entails cash benefits while a leave before the enactment does not. Thus, this policy reform does not provide a sharp discontinuity but rather provides a gradual transition in the policy regime, which contrasts to the introduction of parental leave systems in some other countries where eligibility was not given

[6]More precisely, eligibility is determined by the total period covered by Employment Insurance. A worker is covered by this insurance if his/her working hours are at least 20 hours per week and his/her employment is expected to continue for one month. A mother covered by Employment Insurance for 12 months within the past 2 years is eligible for the cash benefits (Employment Insurance Act, Articles 4, 6 and 61-4).

[7]One-fifth of the cash benefits are paid 6 months after the leave, conditional on return to work (Employment Insurance Act, Article 61-5), but this aspect does not seem to affect the interpretation of the policy impact, as the MHLW reports that about 85 percent of mothers taking the leave returned to work even before the introduction of the cash benefits and this proportion did not change after the cash benefit policy was introduced, presumably because a mother is required to commit to return to work when applying for the parental leave. This delayed payment system was abolished in 2010.

Table 1: Proportion of mothers taking parental leave, 1996-2000

Year	1996	1997	1998	1999	2000
	4.8%	5.4%	5.8%	6.3%	7.0%

Data source: Monthly Report on Employment Insurance Services (*Koyou hoken jigyou geppou*), the Ministry of Health, Labour and Welfare.

to those who gave birth before the enactment of the policy. However, since the policy regime is still discontinuous across birth months, and the difference in the effective replacement rate leads to different take-up rates across child birth months, this Japanese parental leave reform is still effective in identifying the causal impact.

Although the parental leave system was introduced in the 1990s, the proportion of mothers taking up parental leave was very small at that time, as we can see from Table 1, which shows that the percentage of mothers who took up parental leave from 1996 to 2000[8] ranged from less than 5 percent in 1996, one year after the second reform, and increased only to 7 percent even five years after the second reform. This low take-up rate is partly attributed to the narrow coverage of the policy, as Japanese women tended to quit their jobs at marriage or childbirth at that time. Indeed, the employment rate of married women was around 50 percent,[9] and the eligibility rate among those with an infant younger than one year old was 30 percent (Table A1).

Meanwhile, in addition to this legally enacted parental leave, some firms had their own maternity or parental leave systems, but availability was limited before the introduction of the 1992 legislation (Ministry of Labour, 1991, 1993). The Ministry of Labour (1996) reports that most establishments did not provide additional leave on top of the legislated leave, with about 90 percent of establishments simply following the legislation, while 5 percent provided additional unpaid leave and another 5 percent provided additional paid leave. Therefore, in contrast to the U.S., where private parental leave is highly salient (Baum, 2003), in Japan, the official parental leave policies enacted by legislation were the relevant policies in effect for most firms during our analysis period.

Finally, we briefly describe formal childcare services in Japan. As in many European countries, publicly subsidized childcare centers are a major provider of formal childcare services in Japan. Although these institutions could potentially be an alternative to parental care, the childcare slots for infants have been very limited, with only about 5 percent of the 1990 cohort of infants aged

[8]Unfortunately, the number of mothers taking up parental leave up until 1995 is not available from the Ministry of Health, Labour and Welfare.

[9]According to the 1990 census, the employment rate of married women between the ages of 16 and 45 was 52 percent. Among them, the full-time employment rate was 27 percent.

0–1 enrolled in childcare centers. From 1990 to 1995, the proportion increased from 5 percent to 6.6 percent, but it was still rare for women to use childcare centers for their 0–1 year old infant children. Given such limited access to formal childcare, parental leave seems necessary for a mother to continue her career after childbearing during our analysis period, unless other informal childcare arrangements were available.

2.2 Data

For this study, we used data from the 1990 through 2010 waves of the Japanese Census, which takes place in October every five years. The Census dataset covers all individuals in Japan and collects basic demographic characteristics such as age, gender, marital status and household structure as well as employment status, occupation and industry. This is supplemented by data from the decennial census which collects years of education. The Census collects employment status for the week prior to the survey (i.e., from September 24–30), and choices are (1) mostly worked; (2) worked besides doing housework; (3) worked besides attending school; (4) absent from work; (5) unemployed; or (6) out of labor force. The survey instructions categorize full-time workers as those who "mostly worked" while part-time workers are considered to be those who either "worked besides doing housework" or "worked besides attending school." If a mother is on parental leave, her employment status is supposed to be "absent from work."[10] Hereinafter, we refer to the first category as "full-time," the second category as "part-time," the third category as "on leave", finally "employed" as the union of all three categories. The analysis sample is restricted to mothers in non-institutional households who were not engaged in schooling at the time of the survey and who gave birth between age 16 and 45.[11]

Before going into the detailed analysis, we first briefly describe the industrial/occupational composition of Japanese female workers as it facilitates to interpret our results later. Figure A1 shows the joint distribution of occupation (horizontal axis) and industry (vertical axis) of women by their marriage status and employment type. Firstly, the composition of occupation and industry is quite similar between full-time workers and part-time workers, and also between married women and single women, suggesting that job transitions for women are likely to occur within the same occupation and industry. In particular, typical jobs for women include professional positions such as teachers and nurses in the service industry, manufacturing (especially, the production of food,

[10]This category also includes employees and self-employed workers whose absence from work did not exceed 30 days up to the census date or who received or expected to receive wages or salary during the week before the census date.

[11]For the detailed definition of non-institutional households, see the following web page: http://www.stat.go.jp/english/data/kokusei/2010/pdf/ex.pdf

clothing, and crafts), and sales and clerical work in the retail and wholesale industries. Although the distribution of full-time and part-time workers is similar, a difference, if any, is that full-time workers are more likely to have professional occupations in the service sector and clerical jobs in government agencies than part-time workers.

3 Effect of parental leave policy

3.1 Identification strategy

This study adopts two identification strategies to examine the effect of parental leave policies on long-run maternal employment. Our first identification strategy relies on the assumption that the timing of the birth is random. In particular, we compare mothers who gave birth in 1992 or afterwards with those who gave birth in 1991, a year before the introduction of the job protection policy, and our focus is the mothers' employment status in the 2005 Census. Since employment status is measured in 2005, both groups of mothers face the same labor market conditions irrespective of their treatment status and consequently, the comparison is unlikely to be confounded by any macroeconomic shocks.

A major concern in this comparison, however, is the child's age in 2005, which differs depending on the year of birth. Since children of treated mothers are younger than children of untreated mothers, and since mothers with small children may shorten their work hours, the effect on full-time employment could be downward biased and that of part-time employment upwardly biased. The one-to-one relationship between treatment status and child's age prevents us from identifying the policy impact separately from the effect of the age of the child.

We address this issue by using the cohorts 5 years earlier, which allows us to partial out the child age effect. Letting $Y_{t,a}$ be the employment status in year t of a mother with an a-year-old child, then the policy effect on a mother with a-year-old child is

$$\text{Policy effect} = \underbrace{\left(E\left[Y_{2005,a}|X\right] - E\left[Y_{2005,14}|X\right]\right)}_{\text{Policy effect + child age effect}} - \underbrace{\left(E\left[Y_{2000,a}|X\right] - E\left[Y_{2000,14}|X\right]\right)}_{\text{child age effect}}$$

where X is a vector of control variables and $a \leq 13$, as the age of a child born in 1991 is 14. The essential assumption here is that the child age effect is the same across the two cohorts, and this assumption is analogous to the parallel trend assumption in the standard difference-in-differences analysis.

Given the increasing trend in the parental leave take-up rate (Table 1), the policy impact is likely to evolve overtime. Therefore, we estimate the following dynamic DID equation:

$$y_{it} = \sum_{\substack{9 \le a \le 18 \\ a \neq 14}} \delta_a D_i(a) \cdot T_t + \sum_{9 \le a \le 18} \tau_a D_i(a) + x_i' \gamma + \eta_t + u_{it}, \tag{1}$$

where T_t is a dummy variable that takes 1 if a mother gives a birth between 1987 and 1999, η_t is survey-year fixed effects, which absorb macroeconomic shocks in 2000 and 2005, and x_i is a collection of demographic characteristics, including mother's age, the number of household members, the number of children (household members aged 15 or less), an indicator for a nuclear household, an indicator for a single mother, and immigrant status. $D_i(a)$ is an indicator that takes 1 if the child is a years old. The parameter of interest is δ_a, which represents the policy impact on a mother with an a-year-old child for $a \le 13$ and the placebo impact for $a \ge 15$, as the parental leave is not available in 1990 or earlier (the mothers of 14-year-old children are the baseline). For this analysis, treatment status, T_t and $D_i(a)$, was assigned on the basis of the birthday of the first child to ensure the treatment and control groups were mutually exclusive.

3.2 Estimation results

We first show the descriptive evidence to confirm the assumption that the child age effect is constant across cohorts. Figure 1 plots the full-time employment rate relative to the baseline cohort, i.e., $\hat{E}\left[Y_{2005,a}\right] - \hat{E}\left[Y_{2005,14}\right]$ (solid line) and $\hat{E}\left[Y_{2000,a}\right] - \hat{E}\left[Y_{2000,14}\right]$ (dashed line). This figure has two notable features. First, the solid and dashed lines have downward slopes, which is natural because a mother with a younger child is less likely to work full time than a mother with an older child. This shows that the child age effect is so substantial that a simple comparison, $\hat{E}\left[Y_{2005,a}\right] - \hat{E}\left[Y_{2005,14}\right]$, will not accurately represent the causal impact of the parental leave policy. Second, the solid line is almost identical with the dashed line before the introduction of the parental leave policy, but deviates from it after the policy is introduced. This suggests that the child age effect is likely to be constant across cohorts; otherwise, the solid line would deviate from the dashed line even before 1992. Further, consistent with the increasing trend in the policy take-up rate, the difference between the two lines becomes larger over time.

To complete the descriptive analysis, we repeat the same exercise in terms of part-time employment and the overall employment rate. In contrast to full-time employment, part-time employment decreased after the introduction of parental leave (Figure 2), whereas the employment rate at each age relative to the baseline cohort is the same across the two cohorts (Figure 3). This suggests that the positive impact on full-time employment is completely offset by the negative impact on

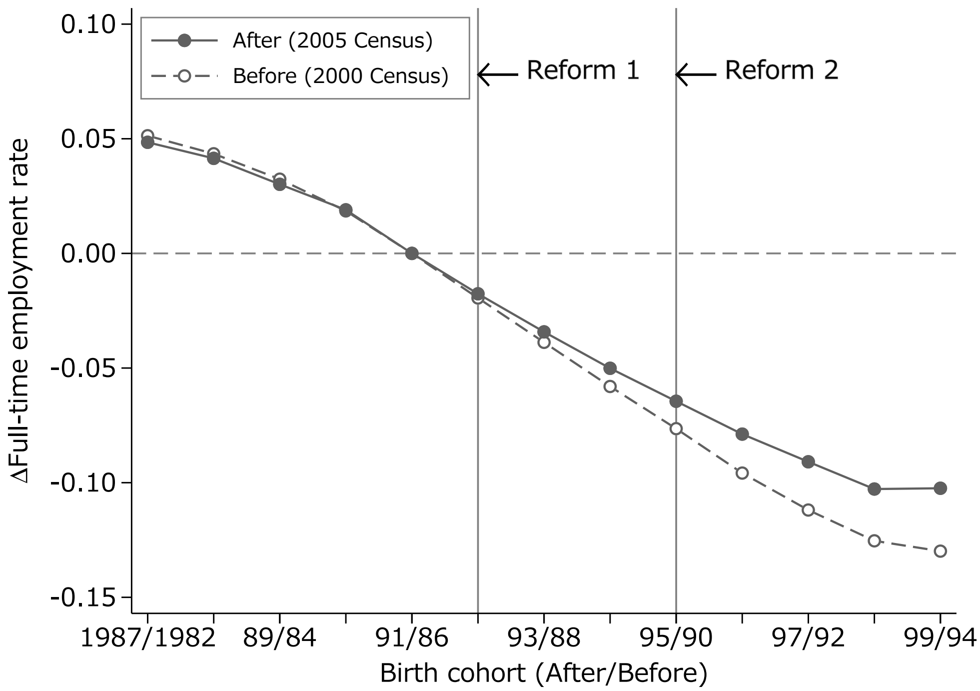


Figure 1: Trend in full-time employment rate relative to the baseline cohort

Note: This figure plots $\hat{E}\left[Y_{2005,a}\right] - \hat{E}\left[Y_{2005,14}\right]$ and $\hat{E}\left[Y_{2000,a}\right] - \hat{E}\left[Y_{2000,14}\right]$, where Y is the full-time employment and child age a is between 6 and 18. The vertical lines, JP and CB, indicate the job protection and cash benefit policies of 1992 and 1995, respectively.

part-time employment, resulting in no change to the employment rate.

Although the graphical analysis is suggestive of the policy impact, it does not control for any demographic characteristics or provide statistical inference, and so we now turn to the regression analysis. Figure 4 plots the estimated coefficients $\hat{\delta}_a$ with their 95 percent confidence intervals resulting from our estimation of equation (1). In 1992, when the policy was first introduced, the job protection policy did not affect the mother's employment status 13 years after childbirth. However, the policy impact grows over time and becomes positive for the full-time employment of mothers giving birth in 1993 and 1994. This implies that the job-protection policy increases full-time employment. While the magnitude of the impact appears small at 0.4–0.8 percentage points, this is likely attributable to the low take-up rate of the parental leave, as mentioned above. Although we do not have the take-up information for 1993 or 1994, the trend was monotonically increasing, and so we can assume that it would be less than the 4.8 percent of 1996. We thus suspect that the job protection policy has a non-negligible impact on those who do take the parental leave, and the policy impact

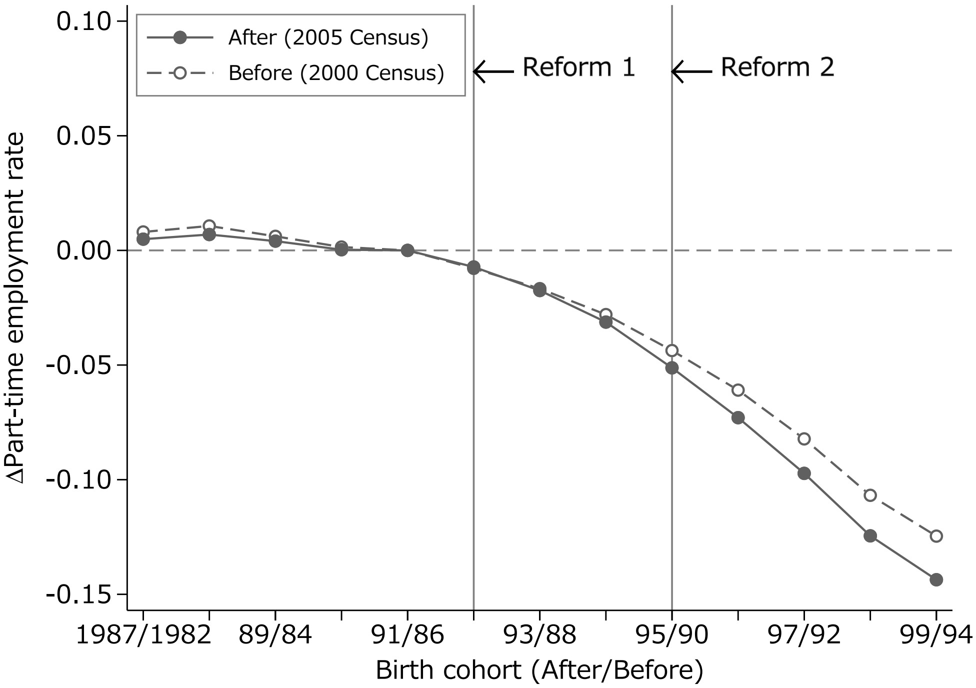


Figure 2: Trend in part-time employment rate relative to the baseline cohort

Note: This figure plots $\hat{E}\left[Y_{2005,a}\right] - \hat{E}\left[Y_{2005,14}\right]$ and $\hat{E}\left[Y_{2000,a}\right] - \hat{E}\left[Y_{2000,14}\right]$, where Y is the part-time employment and child age a is between 6 and 18. The vertical lines, JP and CB, indicate the job protection and cash benefit policies of 1992 and 1995, respectively.

continued to grow after 1995, when the second reform was implemented. Similarly, the negative policy impact on part-time employment also grew over time, leaving the overall employment rate unaffected. Therefore, the regression results corroborate our original graphical analysis.

Our empirical evidence implies that the parental leave policy works in favor of mothers who would otherwise quit their jobs after childbearing and return to the labor market as part-time workers, but has a minimal effect for mothers who do not intend to return to the labor market after childbirth. While the size of the policy impact is at most 1.5 percentage points, this does not immediately mean that the parental leave has little benefit for the career continuation of mothers. In particular, we need to consider the policy take-up rate to evaluate the magnitude of the treatment effect of those taking parental leave. Since an official parental leave system did not exist until 1992, and firm-provided parental leave was not prevalent (Ministry of Labour, 1991, 1993), the policy impact on the take-up rate of parental leave is well approximated by the official take-up rate. Hence, this effect can be obtained from the Wald estimate, namely, the reduced-form estimates $\hat{\delta}_a$ divided by

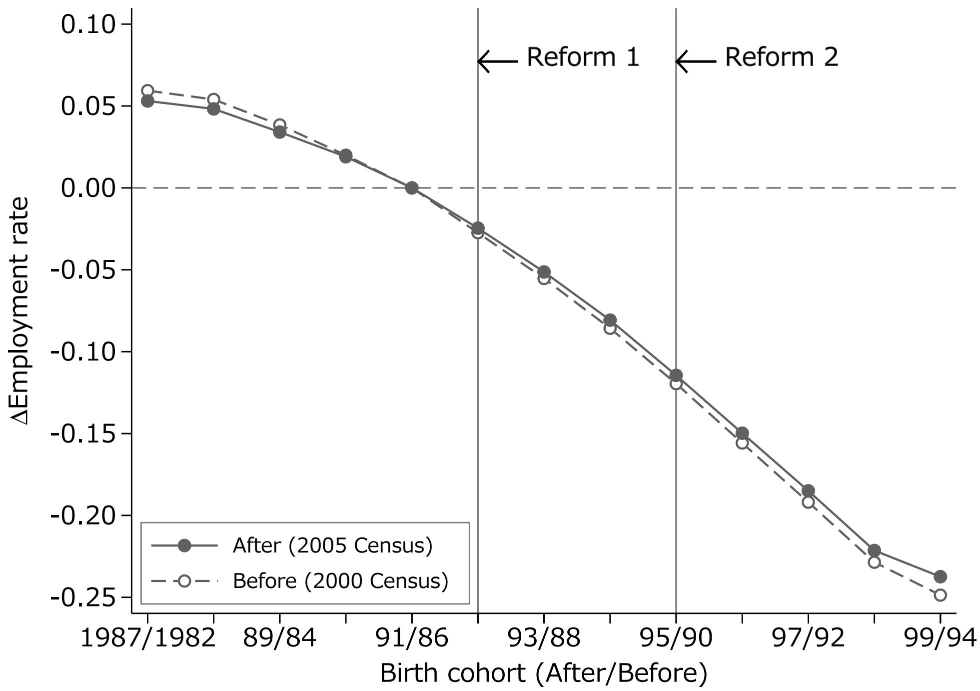


Figure 3: Trend in employment rate relative to the baseline cohort

Note: This figure plots $\hat{E}\left[Y_{2005,\,a}\right] - \hat{E}\left[Y_{2005,\,14}\right]$ and $\hat{E}\left[Y_{2000,\,a}\right] - \hat{E}\left[Y_{2000,\,14}\right]$, where Y is the employment and child age a is between 6 and 18. The vertical lines, JP and CB, indicate the job protection and cash benefit policies of 1992 and 1995, respectively.

the take-up rate. Calculating this using 1996 values, we found that a substantial impact of taking parental leave, increasing full-time employment by about 30 percentage points while decreasing part-time employment by the same amount.

3.3 Potential threats to identification

3.3.1 Violation of the common trend assumption

A fundamental assumption of a DID analysis is that the treatment and control groups exhibited a common trend prior to the policy reform, and since we obtained some significant effects for the placebo years that, while small in magnitude, could indicate a violation of our identification assumption, we confirmed the robustness of our results by controlling for any group-specific linear

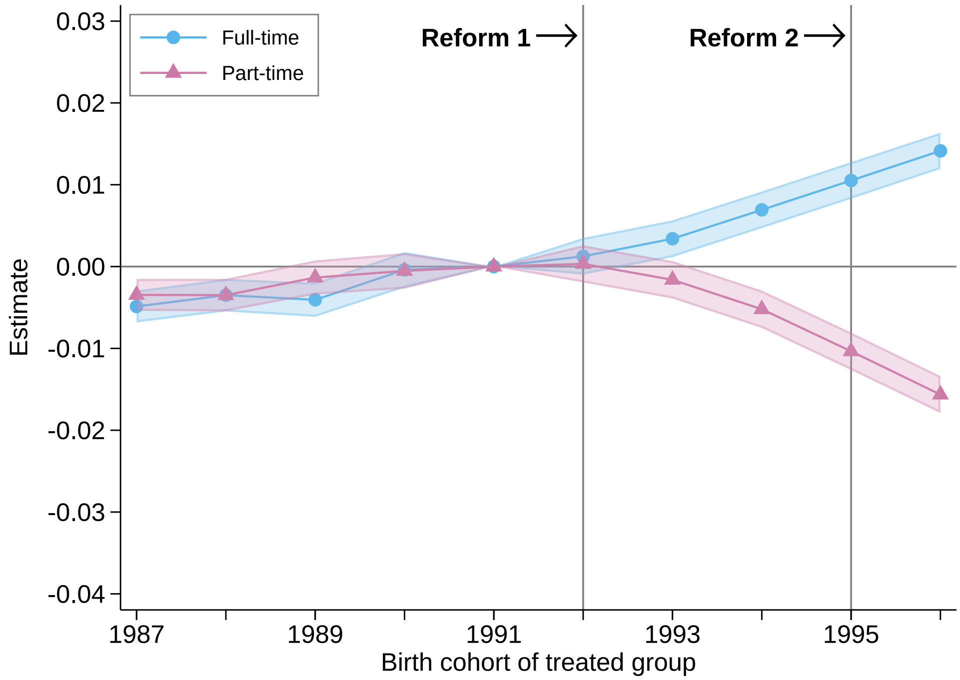


Figure 4: The effects of the parental leave policies by child birth years

Note: This figure shows the results of estimating equation (1). The shaded areas represent the 95 percent confidence intervals of each estimate.

age effect. In particular, we estimated

$$y_{it} = \sum_{9 \leq a \leq 13} \delta_a D_i(a) \cdot T_t + \sum_{9 \leq a \leq 18} \tau_a D_i(a) + \beta ChildAge_i \cdot T_t + x_i' \gamma + \eta_t + u_{it}, \quad (2)$$

where β addresses the child age effect specific to the treatment cohorts. As β is identified by the mothers with 15–18-year old children in the treatment cohort, the placebo terms $(\delta_{15}, \ldots, \delta_{18})$ are excluded from the model.

The dotted lines in Figure A2 show the estimation results with and without the linear age effect, and we see that the estimated policy impact becomes smaller on full-time employment and larger (in absolute value) on part-time employment compared to the baseline estimates. These results are expected, as the placebo estimates in the baseline model have upward slopes both in terms of full-time and part-time employment. Since the policy impacts remain significant after controlling for the child's age with a linear term, our main estimation results cannot simply be explained by a violation of the assumption of a constant child age effect.

3.3.2 Composition change in mothers

Another essential assumption for the identification of the treatment effect is that the mother's decision to have a child must be unaffected by the policy, at least during the analysis period. On its face, this assumption seems restrictive, because it is conceivable that the parental leave policy may induce career-oriented women to have a child. If so, the work propensity of these women may be higher after the reforms than before, and this selection effect would make the counterfactual market outcomes between the treated and control groups non-comparable, leading to a spurious positive impact on maternal labor market outcomes. If this condition is salient, we will observe an increase in the 1st births (relative to 2nd or 3rd births) particularly in regions with high parental-leave take-up. We thus divided the prefectures into two groups using the take-up rate in 2000 calculated from the Census, and plotted the proportion of 1st births among those groups (Figure A3). While the proportion of 1st births has an increasing trend due to Japan's declining fertility rate, this trend is not specific to high-take-up regions but is common across all groups. We also conducted a DID analysis using these two groups to confirm that the selection effect is statistically and economically negligible (Figure A3).[12]

We are aware that the DID analysis performed in this section is vulnerable to selection bias and that it is difficult to completely negate the potential biases in this research design. As discussed in the literature,[13] we can address this difficulty in controlling birth timing by alleviating the difference in maternal (un)observed characteristics between the treatment and control groups by focusing on a small window around the policy implementation. In the next section, we explain how we confirm the robustness of our main results to this selection issue by using 3-month and 6-month windows. Finally, we note that, aside from the selection issue, the DID analysis presented in this section is preferable to other approaches such as RDD in our context, because the maternal response to the policy is not immediate, unlike that of some European countries. Our dynamic DID framework can capture change in policy impact over time, but regression discontinuity design misses it because its focus is limited to a small window.

[12]In this DID analysis, we controlled for the birth cohort of mothers and their marital status, and the standard errors were clustered by municipality $\times$ mother's birth cohort to address serious correlation of birth outcomes.

[13]For example, Lalive and Zweimüller (2009).

4 Effect of parental leave policy: More robust approach

4.1 1992 Reform

The preceding analysis shows the substantial long-run impact of the parental leave policy, but the identification assumption seems restrictive particularly because it rules out the possibility that the policy effect on fertility is heterogeneous depending on labor market attachment. As pointed out in the literature (Lalive and Zweimüller, 2009), this selection issue is alleviated by focusing on mothers who became pregnant without knowing about the policy change, which allows us to make a causal inference between mothers guided by the new policy regime and those guided by the old policy regime. While this argument suggests that regression discontinuity design (RDD) might be an appropriate identification strategy, the literature is also concerned about the seasonality in births, for the RDD window tends to be relatively wide.[14] As a result, a preferred alternative combines RDD and DID to partial out any potential seasonality. In the analysis below, we apply this RDD-DID method to confirm the robustness of our empirical findings.

In this subsection, we analyze the effect of the 1992 job protection policy and investigate the 1995 paid leave policy in the next subsection. Since it seems essential for mothers to access job protection immediately after maternity leave, mothers with a child born in February 1992 or later were the main population treated. In addition, job protection was also continuously accessible to mothers with a child born in January 1992 because the Labor Standards Act prohibits employers from dismissing their employees within 30 days after maternity leave. On the other hand, a mother with a child born in December 1991 or earlier is eligible for job-protected leave from the April 1992, but her job is not protected until then. Therefore, the earlier the child was born, the more difficult it would have been to utilize the legislated leave system. However, because the reform does not give a sharp threshold but a fuzzy one, for our source of identification, we use the accessibility of job protection depending on the child's birth month. In particular, we set the policy cutoff at January 1991 which, as explained above, is the cutoff for continuously utilizing the job protection policy.

The identification assumption is that counterfactual maternal market outcomes (in absence of the reform) are identical between two mothers giving a birth just before and after the policy implementation (RDD assumption) or, if not identical, their differences are the same as the corresponding differences of the cohort five years before (DID assumption). Given those assumptions, the policy impact is estimated from the following empirical model:

$$y_{ijt} = \beta_0 + \beta_1 D_i + \beta_2 D_i \cdot T_t + x'_{ijt}\gamma + \xi_j + \tau_t + u_{ijt}, \tag{3}$$

[14] For example, the RDD window is 3 months in the baseline specification of Schönberg and Ludsteck (2014).

Table 2: Maternal characteristics (Child born in 1988-1990 and 1993-1995)

Birth quarter of child	Q3 and Q4 (1)	Q1 and Q2 (2)	Difference (2) - (1) (3)
Mother's age	32.39	31.91	-0.488
	[4.22]	[4.22]	(0.005)
Household members	4.65	4.58	-0.068
	[1.23]	[1.24]	(0.002)
Children	2.20	2.14	-0.060
	[0.76]	[0.77]	(0.001)
Nuclear household(%)	73.32	73.99	0.661
	[44.23]	[43.87]	(0.057)
Twin(%)	0.68	0.69	0.006
	[8.22]	[8.26]	(0.011)
Immigrant(%)	1.18	1.24	0.059
	[10.80]	[11.07]	(0.014)
Observations	1248082	1174540	

Note: This table shows the demographic characteristics of the mothers whose children were born in 1986-1997 and 1991-1992 using 1990 and 1995 Census data. Standard deviations are in brackets and robust standard errors are in parentheses.

where i, j and t indicate individuals, prefectures and years, and ξ_j and τ_t are prefecture- and year-fixed effects. The vector of control variables, x_{ijt} is the same as before. An indicator variable T_t takes one if mother belongs to the reform-year cohort, and D_i takes one if the child birth month is after the threshold month, i.e., January 1992. In the baseline analysis, the sample is restricted to 3 months before and after the threshold month. We also conducted a robustness check using a 6-month window, which gave similar magnitude of the estimates with smaller standard errors. The outcome variable, y_{ijt}, is employment status 3–13 years after childbirth. For the job protection policy, we do not analyze its impact on the take-up of parental leave, for the Census was not conducted in the reform year, 1992.

To validate the RDD assumption, we compared the demographic characteristics of mothers across the child birth quarters, because the quarter of delivery could be correlated with market outcomes and this self-selection would complicate the causal inference. Tables 2 and 3 describe the demographic characteristics of mothers in the sample for the analysis of the unpaid and paid leave, respectively. The average age of the mothers at childbirth is around 29, and the 3rd and 4th quarter groups were the oldest while the 1st and 2nd quarter groups were the youngest by construction. To see this, notice that if all mothers gave birth when they became 29 years old,

Table 3: Maternal characteristics (Child born in 1988-1990 and 1993-1995)

Birth quarter of child	Q3 and Q4 (1)	Q1 and Q2 (2)	Difference (2) - (1) (3)
Mother's age	29.60	29.14	-0.451
	[4.31]	[4.32]	(0.006)
Household members	4.17	4.15	-0.013
	[1.24]	[1.24]	(0.002)
Children	1.76	1.76	-0.004
	[0.83]	[0.83]	(0.001)
Nuclear household(%)	77.26	77.98	0.719
	[41.92]	[41.44]	(0.055)
Twin(%)	0.76	0.73	-0.026
	[8.68]	[8.53]	(0.011)
Immigrant(%)	1.54	1.61	0.062
	[12.33]	[12.57]	(0.016)
Observations	1182735	1108509	

Note: This table shows the demographic characteristics of the mothers whose children were born in 1988-1990 and 1993-1995, using 1990 and 1995 Census data. Standard deviations are in brackets and robust standard errors are in parentheses.

for example, then the 1st and 2nd quarter group would be younger than the 3rd and 4th quarter group by 0.5 years (or 6 months). The remaining rows report the household structure. While we found statistically significant differences for some characteristics due to the huge sample size, the difference is not economically meaningful. To sum up, these descriptive statistics suggest that demographic characteristics are balanced across the child birth quarters.

Table 4 presents the estimation results for maternal employment 3–13 years after childbirth, and we found no impact on maternal employment, irrespective of the timing of evaluation. Although one estimate, for part-time employment 3 years after childbirth with a 6-month window, is significant at the 5 percent level, it can be expected that one out of 24 estimates in the table might show significance. Considering that the estimation sample consists of those who gave birth between the 3rd quarter of 1991 and the 2nd quarter of 1992, the null (or tiny) policy impact seems consistent with the earlier DID result in which the estimates for 1992 were not statistically significant and the estimate gradually becomes larger after 1993 (Figure 4).

There are two possibilities for the null policy effect immediately after the reform. First, it is possible that the provision of job-protected leave has no impact on maternal market outcomes because, for example, the public parental leave system crowds out a private parental leave system (Baum,

Table 4: The effects of the 1992 reform on maternal employment

Yrs since birth	3 years		8 years		13 years	
Window	3m	6m	3m	6m	3m	6m
	(1)	(2)	(3)	(4)	(5)	(6)
Employed	-0.0011	-0.0014	-0.0001	-0.0006	-0.0007	-0.0005
	(0.0017)	(0.0012)	(0.0017)	(0.0012)	(0.0017)	(0.0012)
Full-time	0.0002	0.0003	-0.0000	-0.0009	0.0004	0.0008
	(0.0013)	(0.0009)	(0.0015)	(0.0011)	(0.0017)	(0.0012)
Part-time	-0.0015	-0.0023	-0.0004	-0.0000	-0.0013	-0.0013
	(0.0014)	(0.0010)	(0.0016)	(0.0011)	(0.0017)	(0.0012)
On leave	0.0002	0.0005	0.0003	0.0003	0.0002	0.0000
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0002)
Observations	1188478	2422622	1196338	2436685	1180916	2404013

Note: This table shows the estimation results of equation (3), focusing on the effects of the reform in 1992. We only reported the estimates of interest, that is, the estimated coefficients of $D_i \times After_i$. The heteroskedasticity-robust standard errors are reported in parentheses. In this analysis, we used mothers who gave birth between October 1986 and March 1987 or between October 1991 and March 1992 for the 3-month window, and between July 1986 and June 1987 or between July 1991 and June 1992 for the 6-month window.

2003). However, this explanation is unlikely to apply to our case because firm-provided parental leave programs were rare when the public parental leave policy was introduced. Furthermore, this explanation is not compatible with the growing policy impact observable in Figure 4. The second possibility is that the policy has no impact when first introduced due to low policy take-up. Although we do not have any direct evidence, we speculate that social pressure or uncertainty about the consequences of taking parental leave might have led to a low take-up rate, which is observed in parental leave for fathers in European countries (Dahl et al., 2014). Given that Japan relies on traditional gender norm, and the mothers in the estimation sample are the first cohort with access to the formal parental leave system, the non-pecuniary cost of taking up parental leave could be substantial, as there are no "peers" taking parental leave before.

4.2 1995 Reform

We next apply the RDD-DID method to examine the 1995 reform that provided cash benefits during parental leaves. Like the job protection reform, the paid leave reform is not "sharply" designed, for since those who gave birth before the policy was enacted can receive cash benefits from April 1995 to the child's first birthday as long as the work experience condition is satisfied, the effective

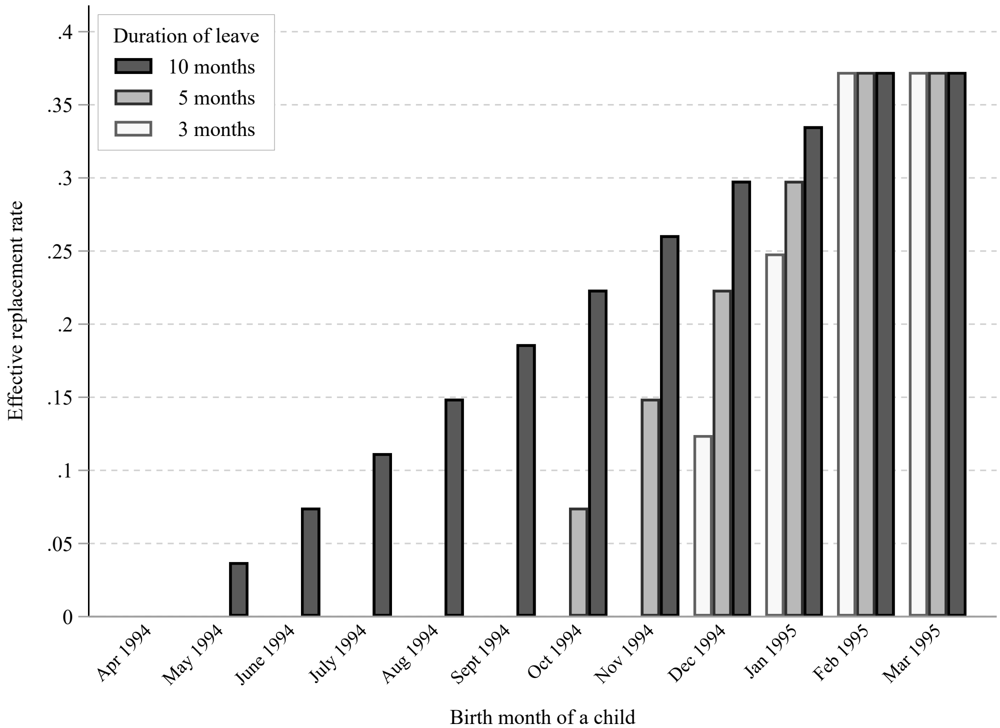


Figure 5: Effective replacement rate across the birth month of a child

Note: This figure shows the effective replacement rates when a mother takes parental leave for 10, 5 and 3 months.

replacement rate depends on the child's birth month. For example, mothers with children born in the 4th quarter of 1994 and the 1st quarter of 1995 faced, on average, replacement rates of 0.26 and 0.36 if the parental leave was fully taken, and this difference is further widened if the parental leave was not fully taken (Figure 5). In order to make the analysis comparable with the job-protection policy, we set the policy cutoff here at January 1995. In this comparison, parental leave is fully associated with cash benefits in the treatment group (except for January), whereas initial months of parental leave are unpaid in the control group. The amount of unpaid benefits is not negligible. For example, if a mother gave birth in December 1994 and began the leave from February 1995, her first two months of the leave were unpaid, which amounts to about 3 quarters of her monthly earnings. In addition to the cash benefits, the 1995 reform allows mothers working in small establishments to take the parental leave, and similarly to the 1992 reform, they could continuously access job protection during maternity and parental leave if they gave birth in the 1st quarter of 1995 or later, their jobs were not protected for several months after the maternity leave if they gave birth in the 4th quarter of 1994 or earlier. The estimation equation is the same as before.

Table 5: The effects of the reform in 1995 on maternal employment

Yrs since birth	5 years		10 years		15 years	
Window	3m	6m	3m	6m	3m	6m
	(1)	(2)	(3)	(4)	(5)	(6)
Employed	0.0021	0.0019	-0.0011	0.0004	0.0014	0.0005
	(0.0018)	(0.0012)	(0.0018)	(0.0012)	(0.0017)	(0.0012)
Full-time	0.0024	0.0027	0.0024	0.0041	0.0049	0.0022
	(0.0015)	(0.0010)	(0.0016)	(0.0012)	(0.0018)	(0.0013)
Part-time	-0.0002	-0.0008	-0.0037	-0.0040	-0.0033	-0.0018
	(0.0015)	(0.0011)	(0.0017)	(0.0012)	(0.0018)	(0.0013)
Observations	1143134	2315223	1140160	2306228	1079598	2193557

Note: This table shows the estimation results of equation (3), focusing on the effects of the reform in 1995. We only reported the estimates of interest, that is, the estimated coefficients on $D_i \times After_i$. The heteroskedasticity robust standard errors are reported in the parentheses. In this analysis, we used mothers who gave birth between October 1989 and March 1990 or between October 1994 and March 1995 for 3-month window, and between July 1989 and June 1990 or between July 1994 and June 1995 for 6-month window.

We performed a RDD-DID estimation of the policy impact 5–15 years after childbirth, and Table 5 presents the results. The effect on full-time employment tends to be stable regardless of the age of the child, which implies that a mother taking parental leave continues to work full-time after childbirth, and she would have had some difficulty finding or returning to a full-time job in the absence of the parental leave policy. While the estimates for the 3-month window range between 0.0022–0.0049, this seems to be due to a lack of precision, and indeed, the 6-month window results are within this range with less variability. On the other hand, the impact on part-time employment is small when the child is 5 years old. Since this child is still of pre-school age, those mothers who quit their job around childbirth may not have yet returned to the labor market to take care of their children. In such a case, the policy impact on part-time employment is negligible because the affected margin is working full-time or not working. In contrast, for older children, we found a negative impact on part-time employment, possibly because those mothers began to work in a part-time job after their children started schooling. Since these two effects offset each other at least in the long-run, the employment rate is not affected.

While the magnitude of the estimates in this analysis is smaller than the earlier DID result in Figure 4, this is due to a difference in the baseline group. The baseline in the earlier analysis is mothers who gave birth in 1991, a year before the introduction of legislated parental leave. The baseline in the current analysis, however, is those who gave birth between the 3rd or 4th quarter of 1994, and they had access to parental leave with lower effective replacement rate than the treatment group. Thus, the current analysis focuses on the subtle margin given by the effective replacement rate, resulting in smaller estimates than the previous analysis. Since this small observed impact masks a substantial impact of "taking-up" parental leave due to a small difference in take-up rate between the treatment and control group, we next construct the two-stage least square estimator in Section 4.3.

4.3 Short-run effect of taking parental leave

Our reduced-form evidence shows a statistically significant impact of the paid leave policy, but the estimated coefficients appear small and thus difficult to interpret. As discussed earlier, due to the low take-up rate, these small estimated coefficients do not necessarily indicate that the impact of taking up parental leave is negligible, and we need the first-stage estimate to infer the effect of parental leave take-up. However, this estimation is not straightforward due to a lack of direct information on parental leave take-up in 1994 and 1995. Instead, what is available from the 1995 Census is only whether or not a mother is taking leave in October 1995. Though not ideal, this information still helps us infer the first-stage impact.

The first stage estimation regarding the leave status in the 1995 Census as parental leave take-up needs careful interpretation because it results in an artificial difference in the take-up rate by birth month. To illustrate, compare a mother who gave birth in June 1995 with a mother who gave birth in October 1994, and consider the minimum duration of the parental leave required to be observed as taking leave in the 1995 Census. The former needs 2 months of parental leave (after 2 months of maternity leave), whereas the latter needs 10 months. Put differently, the latter mother is not counted in the take up of the Census unless the parental leave is fully taken. Furthermore, the leave status for those who gave birth in the 3rd quarter of 1994 cannot be observed in the Census, irrespective to their parental leave duration, for their children are older than 1 year in October 1995. As a result, the first-stage impact is over-estimated, and the degree of the bias is serious in the case of a 6-month window compared to a 3-month window.

Given this first-stage estimate, we calculate the 2nd-stage estimate by dividing the reduced-form estimate by the first-stage estimate, which is referred to as the two-sample Wald estimator (Angrist, 1990; Dee and Evans, 2003). Since our first-stage estimate is upwardly biased, the resulting 2nd-stage estimate is the lower bound (in absolute value) of the 2nd-stage impact of taking parental leave. The standard errors are calculated by the delta method.[15]

[15]In calculating the standard error, we relied on the assumption of a null correlation between the first-stage and second-stage estimates as in Dee and Evans (2003). However, our estimates may be correlated since we used the whole population in Japan for both estimates, though we could not match individuals across years. In such cases, the standard error may be over- or under-evaluated, depending on the correlation between u_1 and u_r as well as the sign

Table 6: The effects of the 1995 reform on maternal employment

Yrs since birth	0 years	
Window	3m	6m
	(1)	(2)
On leave	0.0065	0.0187
	(0.0006)	(0.0004)
Employed	0.0020	0.0016
	(0.0015)	(0.0011)
Full-time	-0.0016	-0.0109
	(0.0011)	(0.0008)
Part-time	-0.0029	-0.0063
	(0.0011)	(0.0008)
Observations	1131508	2291244

Note: This table shows the estimation results of equation (3), focusing on the effects of the reform in 1995. We only reported the estimates of interest, that is, the estimated coefficients of $D_i \times After_i$. The heteroskedasticity-robust standard errors are reported in parentheses. In this analysis, we used mothers who gave birth between October 1989 and March 1990 or between October 1994 and March 1995 for the 3-month window, and between July 1989 and June 1990 or between July 1994 and June 1995 for the 6-month window.

Table 6 shows the policy impact of the leave status using the 1990 and 1995 Census. As expected, we obtained much larger estimates with a 6-month window than with a 3-month window. Given the relatively similar reduced-form estimates regardless of the window choice, the 6-month result for the first-stage estimate seems to be severely biased. Aside from the leave status, we found suggestive evidence of a positive impact on employment in the short-run, which is marginally significant when using a 6 month window. This positive short-run impact on employment is consistent with our view that the parental leave prevented mothers from quitting their job at childbirth. Since "employment" includes those on leave as well as those who are employed full-time and part-time, the mis-measurement of parental leave take-up does not bias the employment result as long as those who take-up the parental leave do not quit their job after the leave period, which is actually rare because the mother is required to commit to return to work when applying for parental leave (see footnote 7).

Using the first-stage and reduced-form estimates, we calculated the two-sample Wald estimate (Table 7), which is interpreted as the effect of taking parental leave on maternal employment,

of the coefficients. If one is concerned that the OLS estimate obtained by regressing y on D would be over-estimated (given β_1 and β_r are positive), then the standard error would be over-estimated, and the statistical inference based on our assumption becomes rather conservative.

Table 7: The two-sample Wald estimate of the effect of taking parental leave

Yrs since birth	5 years		10 years		15 years	
Window	3m	6m	3m	6m	3m	6m
	(1)	(2)	(3)	(4)	(5)	(6)
Employed	0.325	0.100	-0.170	0.024	0.218	0.025
	(0.273)	(0.066)	(0.270)	(0.066)	(0.259)	(0.063)
Full-time	0.368	0.142	0.364	0.220	0.754	0.116
	(0.226)	(0.055)	(0.256)	(0.062)	(0.287)	(0.068)
Part-time	-0.026	-0.041	-0.571	-0.214	-0.504	-0.095
	(0.233)	(0.057)	(0.269)	(0.064)	(0.280)	(0.067)

Note: This table shows the two-sample Wald estimate using the 1st-stage and reduced-form estimation results. The standard errors are calculated by the delta method. In this analysis, we used mothers who gave birth between October 1989 and March 1990 or between October 1994 and March 1995 for the 3-month window, and between July 1989 and June 1990 or between July 1994 and June 1995 for the 6-month window.

and the size of the impact is non-negligible. For full-time employment, the positive impact is between 33–75 percentage points and for part-time employment, the negative impact is around 50 percentage points, using a 3-month window. The variability of the estimates is due to the lack of precision in the reduced-form estimates, and we obtained more stable results by using the the 6-month window. Although the 6-month window results could be severely downwardly biased (in magnitude), we still found a sizable impact on both full-time (about 18–20 percentage points) and part-time (12–23 percentage points) employment in the long-run. Furthermore, the size of these estimates is compatible with the DID estimate in Figure 4. In 1996, the estimates for full-time and part-time employment are 0.015 and -0.014, respectively, and rescaling these estimates by the take-up rate of 0.048, we obtain 0.318 for full-time and -0.303 for part-time employment, which are comparable to the Wald estimates evaluated at age 10 in Table 7.

While we found a positive policy impact on maternal career continuation, an important policy question is whether or not the parental leave policy is fiscally balanced. To obtain a rough estimate of the increase in tax revenue obtained by the policy, we first note that a part-time worker in Japan typically pays no income tax due to tax deductions, and the marginal income tax rate of a typical female full-time worker is only 10 percent. According to the Basic Survey on Wage Structure, the mean annual earnings of female full-time workers are 3.34 million JPY and given the tax deduction of 1.03 million JPY, her income tax is 0.23 million JPY.[16] On the other hand,

[16]In addition to her own income tax, a full-time worker's husband's income tax also increases since the spousal exemption no longer applies.

aside from the administration costs, the cost of the parental leave policy is 37 percent of income replacement for at most 10 months, and thus amounts to 1.03 million JPY when the mother takes the maximum parental leave. This back-of-the-envelope calculation suggests that the cost of the parental leave policy would be recovered if a mother works full-time instead of part-time for 4.5 years. Since our estimation result indicates a persistent long-run positive impact, the increased income tax revenue seems to surpass the policy cost, though the discussion here should be taken only as a first approximation due to the numerous assumptions. A more rigorous evaluation using tax data would be necessary to provide a definitive conclusion about the fiscal appropriateness of the parental leave policy, but that is beyond the scope of this study.

4.4 Placebo test

The RDD-DID framework used in this study is recognized in the literature (e.g. Lalive and Zweimüller 2009) as a means of addressing the limitations of the RDD or DID methods when used alone, but as a further check of the validity of our RDD-DID framework, we estimated equation (3) using less policy-relevant birth quarters; namely, the 2nd and 3rd quarter of 1994 and 1989, where the 3rd quarter is regarded as the treatment group (Table A2). In contrast to the positive impact on the 1st-quarter group, we did not find any policy impact in this exercise, with the estimates being not statistically significant and also not showing the systematic pattern found in the 1st-quarter group. Indeed, the estimates for full-time and part-time employment are sometimes positive and sometimes negative, but this finding is consistent with the policy design in which the effective replacement rate depends on the birth month of children and the duration of parental-leave take-up. For example, if a mother gives birth on September 1994, her first 5 months of parental leave is unpaid (after 2 months of post-birth maternity leave), which makes the parental leave less affordable. Furthermore, if she is working in a small-sized establishment, her job is not protected between December 1994 and March 1995. It is for these reasons that we did not find any impact on this less policy-relevant group.

4.5 Anticipation of the policy reform

Although the RDD-DID analysis is more robust than our initial DID analysis in terms of the identification assumption, it could be invalidated by any anticipation of the policy changes. If the reform were predictable and mothers could completely control the timing of childbearing, those who gave birth just after the cutoff date would not be comparable with those who did so just before the cutoff. Specifically, such a comparison would lead to an over-estimation of the policy impact, as the treated are likely to have a higher propensity to work than the untreated in that case. Thus, as with our initial analysis, the identification assumption could be violated due to self-selection.

In practice, it would seem difficult to anticipate the introduction of cash benefits at the time of conception if the child birth month was around the date of the policy reforms. The bill on job protection was first deliberated in the Diet on April 12, 1991 and promulgated on May 15, 1991, while the bill on the cash benefits was first deliberated on May 31, 1994 and promulgated on June 29, 1994. In order to give birth on February 1995 and start taking the leave from the following April, a woman would need to conceive around May 1994, before the bill was deliberated in the Diet. Although it was still possible that she may have anticipated the policy change on March 11, 1994 when it was first submitted to the Diet, she would have had only a few months to conceive.

However, the expansion of eligibility for those working in small establishments was announced at the time of the reform in 1992, so this was completely predictable. Thus, although we cannot completely negate the possibility of self-selection, we believe the RDD-DID analysis with a 3-month window is relatively safe from potential biases. As discussed in Lalive and Zweimüller (2009), even if the policy change was predictable, manipulation of birth timing is difficult. If a mother tried to avoid giving birth in the 4th quarter of 1994, she had only 3 months to conceive in order to give birth in the 1st quarter of 1995. In order to confirm this intuition, we investigated the number of births around the cutoff. Figure A5 shows the average number of births per day in each ten day window for each fiscal year, and we see no evidence that the number of births increased after the cut-off. Therefore, we believe that self-selection, if any, would have only a minor impact on our analysis.

5 Policy mechanism

5.1 Do cash benefits matter?

As described in Section 2.1, the 1995 policy reform both introduced cash benefits and expanded eligibility. Based on the literature, an important policy question is whether cash benefits encouraged mothers to take parental leave or not. Dahl et al. (2016), studying the Norwegian parental leave system, argues that eligible workers are a socially advantaged group, for they are highly educated and have a high family income compared to those who are ineligible, and so the parental leave system is a form of regressive income redistribution. Since the Japanese parental leave system has a restrictive eligibility requirement in favor of a high income population, and we also observe that the coverage rate was higher among college graduates than high-school graduates (Table A1), The Japanese reform might also be regressive. On the other hand, it is also possible that parental leave

Table 8: Do cash benefits matters? Subsample analysis by exposure to policy expansion

Yrs since birth	0 years		5 years		10 years		15 years	
Frac. women in	Low	High	Low	High	Low	High	Low	High
small est.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed	0.0020	0.0012	0.0026	0.0012	0.0017	-0.0009	0.0014	-0.0005
	(0.0015)	(0.0015)	(0.0017)	(0.0018)	(0.0017)	(0.0018)	(0.0016)	(0.0017)
Full-time	-0.0125	-0.0091	0.0029	0.0024	0.0045	0.0039	0.0016	0.0030
	(0.0011)	(0.0011)	(0.0014)	(0.0015)	(0.0016)	(0.0017)	(0.0018)	(0.0018)
Part-time	-0.0054	-0.0072	-0.0005	-0.0011	-0.0028	-0.0054	-0.0003	-0.0035
	(0.0010)	(0.0011)	(0.0015)	(0.0015)	(0.0017)	(0.0017)	(0.0018)	(0.0018)
On leave	0.0199	0.0176	0.0002	-0.0002	0.0001	0.0006	0.0001	0.0000
	(0.0006)	(0.0006)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Observations	1159549	1130563	1183412	1130656	1186752	1118309	1137949	1054467

Note: This table shows the estimation results of equation (3), focusing on the effects of the reform in 1995. We only reported the estimates of interest, that is, the estimated coefficients on $D_i \times After_i$. The heteroskedasticity-robust standard errors are reported in the parentheses. In this analysis, we used a 6-month window, with mothers who gave birth between July 1989 and June 1990 or between July 1994 and June 1995.

is not affordable to many workers without the cash benefit. Thus, it is worth investigating whether the cash benefits facilitate parental leave take up.

The Japan Census, unfortunately, does not ask the size of the establishment at which people work, and so we instead conducted a subsample analysis by region. Using the establishment census, we calculated the proportion of women working in an establishment with 30 or fewer employees at the city level, and then divided the sample by its median value. Since this proportion is regarded as a proxy for exposure to the expansion in eligibility, if the policy impact is completely driven by the eligibility, we would see a larger impact in the high exposure group than in the low exposure group.

The first two columns of Table 8 show the short-run impact, and we see that the policy impact is larger in the low exposure group than in the high exposure group. The remaining columns report the long-run impact, and we do not find any systematic differences in the estimates between the two groups. Therefore, the empirical evidence indicates that the main driver of the policy impact is the provision of cash benefits, not the expansion in eligibility. It is notable that cash benefits made parental leave affordable even though the cash benefits represented an effective income replacement rate of only 37 percent.

5.2 Interaction between parental leave and publicly-provided childcare

Although the parental leave policy helps mothers take care of their child until his/her first birthday, they thereafter need to make some childcare arrangement to return to work. There are two main options for parents: formal childcare, or informal childcare by grandparents or other family members. In reality, however, the latter option does not seem available for most households, as more than 75 percent of households in Japan with a child less than 1 year old do not cohabitate with their parents. On the other hand, since formal childcare in Japan is heavily subsidized by the government, it would be a more accessible arrangement for these households as long as the local government offers sufficient childcare capacity.

However, to the best of our knowledge, there is no empirical evidence on the interaction between parental leave and the provision of public childcare. Thus, to analyze that relationship, we divide the sample by the city-level capacity of childcare centers in 1990 and conduct a subsample analysis using the number of seats available in childcare centers per child between the ages of 0 and 5 as an indicator of the availability of public childcare. In Japan, childcare center availability varies considerably across municipalities, with the 10th percentile 11.7 percent, the median 23.2 percent, and the 90th percentile 42 percent.[17] To determine whether the effects of parental leave depend on the accessibility of public childcare, we divided the analysis sample into two groups using the median of childcare availability, and re-ran the RDD-DID analysis.

Table 9 shows the RDD-DID estimates for parental leave take-up. Although the difference is relatively small (14 percent), the results suggest that the impact of the parental leave policy on parental leave take-up is positively correlated with public childcare availability. In terms of long-run maternal market outcomes, we found that the positive impact on full-time employment is larger in regions with high availability of public childcare than in regions with low availability, though the difference is not statistically significant (Table 9).[18] Overall, the subsample analysis provides suggestive evidence that the parental leave policy is more effective when it is associated with availability of public childcare, but given the imprecise estimates, further evidence is required to definitively confirm any synergies between parental leave and public childcare policies.

5.3 Heterogeneity by household type

In the analysis to this point, we have focused on the average effects of the paid leave policy across the maternal population, but it is possible that the treatment effects could vary across household

[17]The distribution of public childcare availability is shown in Figure A6.

[18]For this subsample analysis, we chose a 6 month window, as the results with 3 month window were imprecise and thus uninformative.

Table 9: Heterogeneity by public childcare availability

Yrs since birth	0 years		5 years		10 years		15 years	
Availability	Low	High	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed	0.0021	0.0009	0.0005	0.0034	-0.0007	0.0019	-0.0009	0.0020
	(0.0014)	(0.0016)	(0.0017)	(0.0018)	(0.0018)	(0.0017)	(0.0017)	(0.0016)
Full-time	-0.0095	-0.0123	0.0029	0.0024	0.0024	0.0061	0.0013	0.0031
	(0.0010)	(0.0012)	(0.0013)	(0.0016)	(0.0016)	(0.0017)	(0.0017)	(0.0019)
Part-time	-0.0059	-0.0068	-0.0023	0.0009	-0.0035	-0.0046	-0.0021	-0.0014
	(0.0010)	(0.0011)	(0.0014)	(0.0016)	(0.0017)	(0.0017)	(0.0018)	(0.0018)
On leave	0.0175	0.0200	-0.0001	0.0001	0.0003	0.0003	-0.0001	0.0003
	(0.0006)	(0.0006)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Observations	1174067	1117177	1174812	1140411	1170484	1135744	1117920	1075637

Note: This table shows the estimation results of equation (3), focusing on the effects of the reform in 1995. We only reported the estimates of interest, that is, the estimated coefficients on $D_i \times After_i$. The heteroskedasticity robust standard errors are reported in the parentheses. In this analysis, we used mothers who gave birth between July 1989 and June 1990 or between July 1994 and June 1995 for 6-month window. The sample was divided according to the public childcare availability in 1990.

situations depending on the availability of childcare and financial support. For example, mothers co-residing with their parents may receive childcare assistance or financial support even if they do not receive paid leave. Thus, the paid leave policy might be more effective for nuclear households than for other households. To investigate this potential heterogeneity, we re-ran the analysis by each household type, restricting the sample to mothers with children born between the 3rd quarter of 1994 and the 2nd quarter of 1995. In the subsample analyses hereafter, we prefer the 6-month specification, as the 3-month specification tends to provide relatively imprecise estimates, which makes it difficult to compare the policy impact across groups. The estimation results with a 3-month window are reported in Appendix A.

The subsample analysis demonstrates that the propensity to take up the leave tends to be higher in nuclear households than others, while not showing substantial heterogeneity in the long-run effects (Table 10). Although we found the estimated short-run impact to be identical across household types in terms of percentage points, this does not mean that nuclear households have the same demand for parental leave as other types of households, because the proportion of households eligible for parental leave is different between the two groups. In fact, the 1990 Census shows that the proportion of married women without children working full time is 0.37 among households consisting of a wife and husband, while the corresponding proportion is 0.45 among households consisting of a wife, husband and other family members. Thus, the eligibility rate is likely to be lower among nuclear households than other households, and the identical estimates rather indicate higher take-up rate by nuclear households among those eligible. In terms of the long-run policy impact, the estimates were similar across household types, indicating that the effect of taking the parental leave is similar across household types. This finding possibly suggests that those who do not have access to informal care tend to take parental leave irrespective of household type, suggesting that informal care is not crowded out by the parental leave policy.

We also investigated the possibility of a heterogeneous policy impact according to the mothers' educational attainment for the two reasons. First, the opportunity costs of quitting a job (or taking non-paid leave) after childbirth depends on the level of human capital. Second, mothers with higher educational attainment may put higher value on time spent with their children (Guryan et al., 2008), and this preference could result in a higher take-up rate among them. Given these possibilities, we divided the sample into two groups depending on whether mothers graduated from a four-year/junior college or not. Since the Census asks for years of education only every ten years, we investigated the policy impact 5 and 15 years after childbirth.[19]

The subsample analysis by education provides suggestive evidence that college graduates are a main beneficiary of the paid leave policy (Table 11), as the magnitude of the policy impact is larger for college graduates than for high-school graduates for full-time and part-time employment both 5 and 15 years after childbirth. In addition, we also found this pattern with the 3-month window specification (Table A4), though the difference in the estimates is not statistically significant for either a 6 or 3-month window. Since college graduate wages are likely to be higher than that of high-school graduates, the cost of quitting one's job seems higher as well, leading to higher take-up rates. A second possibility is that college graduates may have a higher take-up rate due to strong preferences for time with their children. The long-term effect of taking parental leave also depends on labor market attachment, because those who continue their jobs after childbirth can possibly exit the labor market after 10 or 15 years. If such tendency is more common among high-school graduates than college graduates, the long-run impact on high-school graduates becomes less striking than the impact on college graduates. Although we found a larger impact on more highly educated mothers, this does not necessarily indicate that the parental leave policy reduces the gender gap in market opportunities, as Stearns (2018) finds a positive impact of parental leave on the employment rate but a negative impact on promotion. Thus, further research is necessary to reach a conclusion about the policy impact on the mother's career.

[19]Also, due to the survey design, we used mothers with children born in 1984/1985 as the control group in this analysis to eliminate seasonality in maternal employment by birth quarter. As shown in the next subsection , use of this cohort did not change our baseline results.

6 Conclusion

This study evaluated two parental leave policies introduced in the 1990s in Japan, and the RDD-DID estimation results indicate that the introduction of job protection had no effect but the introduction of cash benefits, together with an expansion in eligibility, positively affected maternal employment. More specifically, this latter reform led mothers to take-up the leave in the short-run, and in the long run, increased full-time employment while decreasing part-time employment, leaving the overall employment rate unaffected. This suggests that the main beneficiaries of the policy are those mothers who, in the absence of the policy, would quit their jobs at childbirth and return to the labor market later when their children become older. According to our two-sample Wald estimates, the long-run effect of taking paid leave on full-time employment is more than 30 percent. Therefore, we find that the policy impact is substantial at the extensive margin, in contrast to the intensive margin (i.e. the extension of policy duration) that is often studied in the literature. Although our RDD-DID analysis did not find any significant effect of the 1992 job protection policy, the DID analysis using the child's birth year indicated that the job-protected leave was not immediately taken up by mothers but unfolded gradually, and we do find a positive policy impact 1–2 years after the policy was implemented, indicating that even unpaid leave encourages a mother to continue her full-time job. While this study shows that the introduction of parental leave is useful for strengthening the labor market attachment of mothers, the policy impact on gender gaps in labor market opportunities is still little known, and future study will examine this issue.

Table 10: The effects of the cash benefit policy on maternal employment

Household type		Nuclear			Other		
		Reduced form	Wald estmates	Mean	Reduced form	Wald estmates	Mean
Yrs after birth		(1)	(2)	(3)	(4)	(5)	(6)
0 years	Employed	0.0003	0.0151	0.1732	0.0030	0.1616	0.3354
		(0.0011)	(0.0606)		(0.0026)	(0.1408)	
	Full-time	-0.0119	-0.6311	0.0531	-0.0116	-0.6246	0.1696
		(0.0008)	(0.0446)		(0.0022)	(0.1225)	
	Part-time	-0.0067	-0.3538	0.0691	-0.0040	-0.2139	0.1142
		(0.0008)	(0.0439)		(0.0018)	(0.1001)	
	On leave	0.0189	1.0000	0.0510	0.0185	1.0000	0.0516
		(0.0005)	(0.0348)		(0.0009)	(0.0692)	
	Observations	1778164			513080		
5 years	Employed	0.0008	0.0408	0.4012	0.0041	0.2236	0.5771
		(0.0014)	(0.0752)		(0.0025)	(0.1373)	
	Full-time	0.0016	0.0829	0.1992	0.0042	0.2280	0.3362
		(0.0011)	(0.0586)		(0.0024)	(0.1305)	
	Part-time	-0.0010	-0.0509	0.1929	0.0004	0.0226	0.2317
		(0.0012)	(0.0639)		(0.0023)	(0.1224)	
	On leave	0.0002	0.0088	0.0091	-0.0005	-0.0269	0.0093
		(0.0003)	(0.0134)		(0.0005)	(0.0251)	
	Observations	1753996			561227		
10 years	Employed	0.0005	0.0282	0.6046	0.0008	0.0408	0.7362
		(0.0015)	(0.0769)		(0.0023)	(0.1252)	
	Full-time	0.0040	0.2114	0.2658	0.0045	0.2427	0.4055
		(0.0013)	(0.0689)		(0.0026)	(0.1382)	
	Part-time	-0.0038	-0.1993	0.3320	-0.0041	-0.2231	0.3251
		(0.0014)	(0.0733)		(0.0025)	(0.1340)	
	On leave	0.0003	0.0161	0.0068	0.0004	0.0212	0.0057
		(0.0002)	(0.0123)		(0.0004)	(0.0204)	
	Observations	1750590			555638		
15 years	Employed	0.0007	0.0391	0.7233	-0.0000	-0.0010	0.8044
		(0.0014)	(0.0730)		(0.0022)	(0.1205)	
	Full-time	0.0020	0.1064	0.3860	0.0024	0.1287	0.4984
		(0.0014)	(0.0761)		(0.0027)	(0.1482)	
	Part-time	-0.0013	-0.0664	0.3321	-0.0028	-0.1530	0.3020
		(0.0014)	(0.0764)		(0.0026)	(0.1407)	
	On leave	-0.0000	-0.0009	0.0051	0.0004	0.0232	0.0040
		(0.0002)	(0.0121)		(0.0004)	(0.0199)	
	Observations	1690017			503540		

Note: This table shows the estimation results of equation (3), focusing on the effects of the cash benefits policy. We only reported the estimates of interest, that is, the estimated coefficients on $D_i \times After_i$. The heteroskedasticity robust standard errors are reported in the parentheses. In this analysis, we used mothers who gave birth between April 1989 and March 1990 or between April 1994 and March 1995.

Table 11: The effects of the cash benefit policy on maternal employment

Yrs after birth	5 years		15	
	HS	College	HS	College
Employed	0.0013	0.0016	0.0017	0.0012
	(0.0015)	(0.0020)	(0.0015)	(0.0019)
Full-time	0.0011	0.0036	0.0010	0.0043
	(0.0013)	(0.0017)	(0.0016)	(0.0020)
Part-time	-0.0000	-0.0024	0.0007	-0.0033
	(0.0013)	(0.0016)	(0.0016)	(0.0020)
Observations	1583855	901410	1453335	864777

Note: This table shows the estimation results of equation (3), focusing on the effects of the reform in 1995. We only reported the estimates of interest, that is, the estimated coefficients of $D_i \times After_i$. The heteroskedasticity-robust standard errors are reported in the parentheses. In this analysis, we used mothers who gave birth between July 1989 and June 1990 or between July 1994 and June 1995.

References

Albrecht, James, Anders Bjorklund, and Susan Vroman, "Is there a glass ceiling in Sweden?," *Journal of Labor Economics*, January 2003, *21* (1), 145–177.

Angrist, Joshua D., "Lifetime earnings and the Vietnam era draft lottery: evidence from social security administrative records," *The American Economic Review*, 1990, *80* (3), 313–336.

Asai, Yukiko, "Parental leave reforms and the employment of new mothers: Quasi-experimental evidence from Japan," *Labour Economics*, 2015, *36*, 72 – 83.

Baker, Michael and Kevin Milligan, "How does job-protected maternity leave affect mothers' employment?," *Journal of Labor Economics*, 2008, *26* (4), 655–691.

_ and _ , "Evidence from maternity leave expansions of the impact of maternal care on early child development," *Journal of Human Resources*, 2010, *45* (1), 1–32.

Baum, Charles L., "The effect of state maternity leave legislation and the 1993 Family and Medical Leave Act on employment and wages," *Labour Economics*, 2003, *10* (5), 573 – 596.

Berger, Lawrence M., Jennifer Hill, and Jane Waldfogel, "Maternity leave, early maternal employment and child health and development in the US," *The Economic Journal*, 2005, *115* (501), F29–F47.

Blau, Francine D. and Lawrence M. Kahn, "Female labor supply: Why is the United States falling behind?," *The American Economic Review*, 2013, *103* (3), 251–256.

Canaan, Serena, "Parental leave, household specialization and children's well-being," *IZA Discussion Papers*, 2019.

Carneiro, Pedro, Katrine V. Løken, and Kjell G. Salvanes, "A flying start? Maternity leave benefits and long-run outcomes of children," *Journal of Political Economy*, 2015, *123* (2), 365–412.

Dahl, Gordon B., Katrine V. Loken, and Magne Mogstad, "Peer effects in program participation," *American Economic Review*, July 2014, *104* (7), 2049–74.

_ , Katrine V. Løken, Magne Mogstad, and Kari Vea Salvanes, "What Is the case for paid maternity leave?," *The Review of Economics and Statistics*, 2016, *98* (4), 655–670.

Dee, Thomas S. and William N. Evans, "Teen drinking and educational attainment: Evidence from two-sample instrumental variables estimates," *Journal of Labor Economics*, 2003, *21* (1), 178–209.

Dustmann, Christian and Uta Schönberg, "Expansions in marternity leave coverage and children's long-term outcomes," *American Economic Journal: Applied Economics*, July 2012, *4* (3), 190–224.

Fernández-Kranz, Daniel, Aitor Lacuesta, and Núria Rodríguez-Planas, "The motherhood earnings dip: Evidence from administrative records," *Journal of Human Resources*, 2013, *48* (1), 169–197.

__ **and Núria Rodríguez-Planas**, "Can parents' right to work part-time hurt childbearing-aged women? A natural experiment with administrative data," *IZA Discussion Paper*, 2013.

Genda, Yuji, Ayako Kondo, and Souichi Ohta, "Long-term effects of a recession at labor market entry in Japan and the United States.," *Journal of Human Resources*, 2010, *45* (1), 157 – 196.

Goldin, Claudia, "A grand gender convergence: Its last chapter," *American Economic Review*, April 2014, *104* (4), 1091–1119.

Gregg, Paul, Elizabeth Washbrook, Carol Propper, and Simon Burgess, "The effects of a mother's return to work decision on child development in the UK," *The Economic Journal*, 2005, *115* (501), F48–F80.

Gruber, Jonathan, "The incidence of mandated maternity benefits," *American Economic Review*, June 1994, *84* (3), 622–641.

Guryan, Jonathan, Erik Hurst, and Melissa Kearney, "Parental education and parental time with children," *Journal of Economic Perspectives*, September 2008, *22* (3), 23–46.

Imai, Ken-ichi and Hiroyuki Itami, "Interpenetration of organization and market: Japan's firm and market in comparison with the U.S.," *International Journal of Industrial Organization*, 1984, *2* (4), 285 – 310.

Kawaguchi, Daiji and Takahiro Toriyabe, "Parental leaves and female skill utilization:Evidence from PIAAC," *RIETI Discussion Paper Series 18-E-003*, 2018.

Kleven, Henrik, Camille Landais, and Jakob Egholt Søgaard, "Children and gender inequality: Evidence from Denmark," *NBER Working Paper*, 2018.

__ **,** __ **, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller**, "Child penalties across countries: Evidence and explanations," *NBER Working Paper*, 2019.

Kluve, Jochen and Sebastian Schmitz, "Back to work: Parental benefits and mothers' labor market outcomes in the medium run," *ILR Review*, 2018, *71* (1), 143–173.

Lalive, Rafael and Josef Zweimüller, "How does parental leave affect fertility and return to work? Evidence from two natural experiments," *Quarterly Journal of Economics*, 2009, *124* (3), 1363–1402.

Ministry of Labour, "Report on Basic Survey of Employment Management of Women 1991," 1991. (*Heisei 3 Nenndo Joshi Koyou Kanri Kihon Chosa Kekka Houkokusho* in Japanese).

__ , "Report on Basic Survey of Employment Management of Women 1993," 1993. (*Heisei 5 Nenndo Joshi Koyou Kanri Kihon Chosa Kekka Houkokusho* in Japanese).

— , “Report on Basic Survey of Employment Management of Women 1996,” 1996. (*Heisei 8 Nenndo Joshi Koyou Kanri Kihon Chosa Kekka Houkokusho* in Japanese).

Olivetti, Claudia and Barbara Petrongolo, “The economic consequences of family policies: Lessons from a century of legislation in high-income countries,” *Journal of Economic Perspectives*, February 2017, *31* (1), 205–30.

Rasmussen, Astrid Wurtz, “Increasing the length of parents’ birth-related leave: The effect on children’s long-term educational outcomes,” *Labour Economics*, 2010, *17* (1), 91 – 100.

Rossin, Maya, “The effects of maternity leave on children’s birth and infant health outcomes in the United States,” *Journal of Health Economics*, 2011, *30* (2), 221 – 239.

Ruhm, Christopher J, “The economic consequences of parental leave mandates: Lessons from Europe,” *Quarterly Journal of Economics*, 1998, *113* (1).

Schönberg, Uta and Johannes Ludsteck, “Expansions in maternity leave coverage and mothers’ labor market outcomes after childbirth,” *Journal of Labor Economics*, 2014, *32* (3), 469–505.

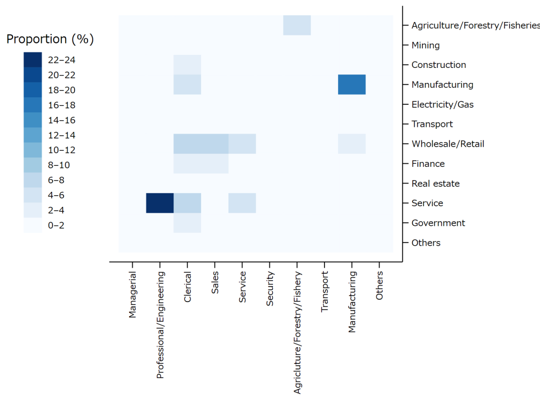
Stearns, Jenna, “The long-run effects of wage replacement and job protection: Evidence from two maternity leave reforms in Great Britain,” 2018. (Unpublished).

Thévenon, Olivier and Anne Solaz, “Labour market effects of parental leave policies in OECD countries,” OECD Social, Employment and Migration Working Papers 141, OECD Publishing January 2013.

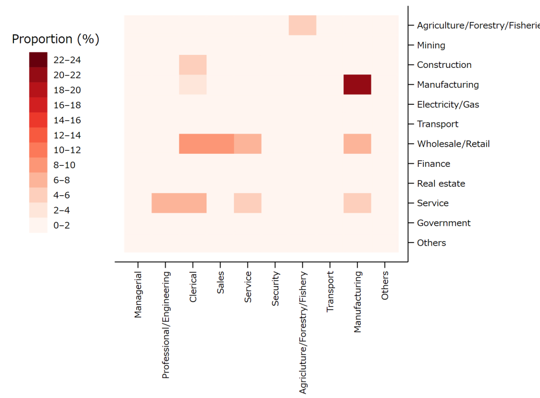
Thomas, Mallika, “The impact of mandated maternity benefits on the gender differential in promotions: Examining the role of adverse selection,” 2018. Mimeo, Cornell University.

Yamaguchi, Shintaro, “Effects of parental leave policies on female career and fertility choices,” *Quantitative Economics*, 2019, *10* (3), 1195–1232.

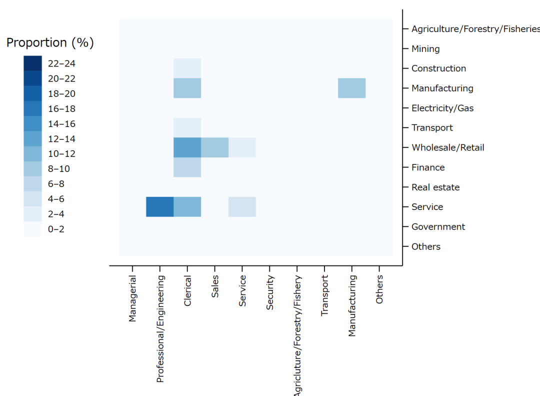
A Supplementary figures and tables



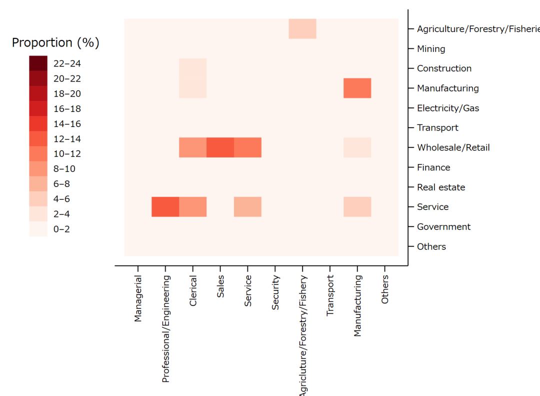
(a) Mother, Full-time



(b) Mother, Part-time



(c) Single, Full-time



(d) Single, Part-time

Figure A1: Industry and occupation of working women in 1990

Data source: *Japanese Census* (Ministry of Internal Affairs and Communications)
Note: This figure shows the percentage of industry and occupation combinations among working women, using the 1990 Census. The horizontal axis shows occupation and the vertical axis shows industry. The definition of a mother is a woman who had a child between the ages of 16 and 45 and who had a child between the ages of 0 and 10 as of 1990. The definition of single women for comparison is never-married women between the ages of 16 and 50 who have no children under the age of 15 in their households. Note that Manufacturing occupations include the production of food, clothing, and crafts, as well as metal fabrication and the assembly of electrical equipment. It also includes laborious work such as transportation and cleaning.

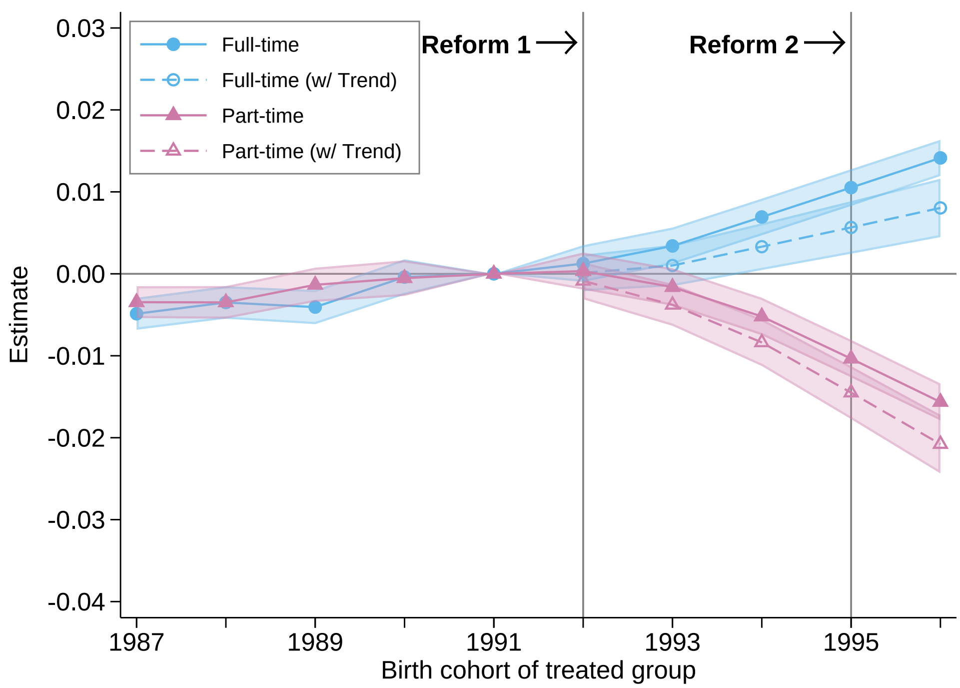


Figure A2: The effects of the parental leave policies by child birth years with trend

Note: This figure shows the estimation results of equation (1) with and without the cohort-specific linear age effect. The shaded areas represent the 95 percent confidence interval of each estimate.

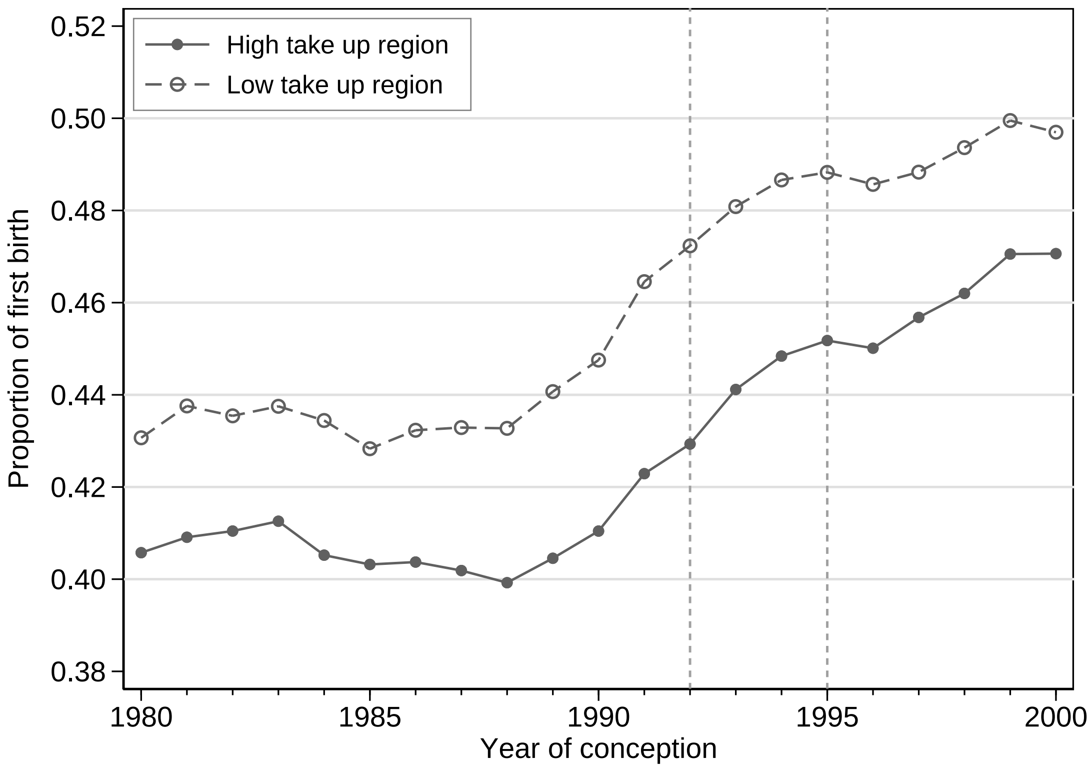


Figure A3: Proportion of the 1st births by regional take-up rate

Data source: *Vital Statistics* (Ministry of Health, Labour and Welfare)
Note: This figure shows the proportion of the 1st births. The regional group is defined by the median of prefecture-level take-up rate in 2000 calculated from the 2000 Census data.

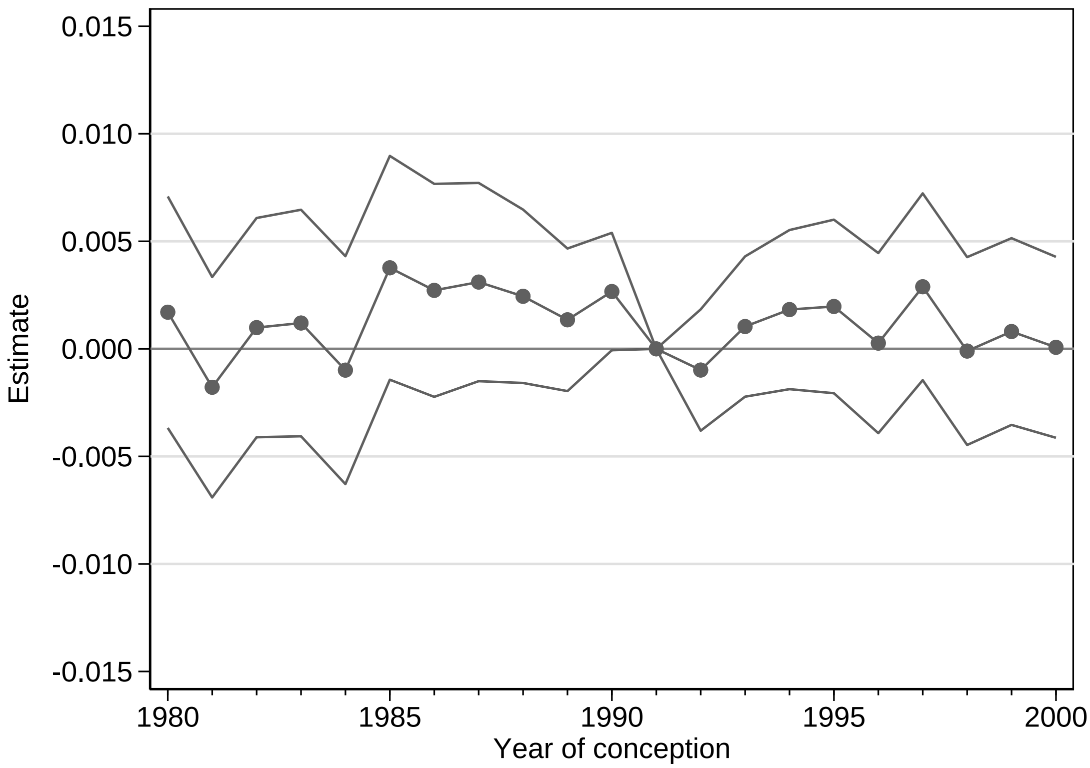


Figure A4: DID estimation on proportion of the 1st births

Data source: *Vital Statistics* (Ministry of Health, Labour and Welfare)
Note: This figure shows the DID estimate on the proportion of the 1st births, in which the treatment group is the high take-up prefectures and the control group is the low take-up prefectures. The unit of analysis is year-prefecture, and the base year is 1991. The control variables used in this analysis are cohort dummy variables of the mothers, dummy variables of the conception year, prefecture dummy variables, and indicator for the single mother.

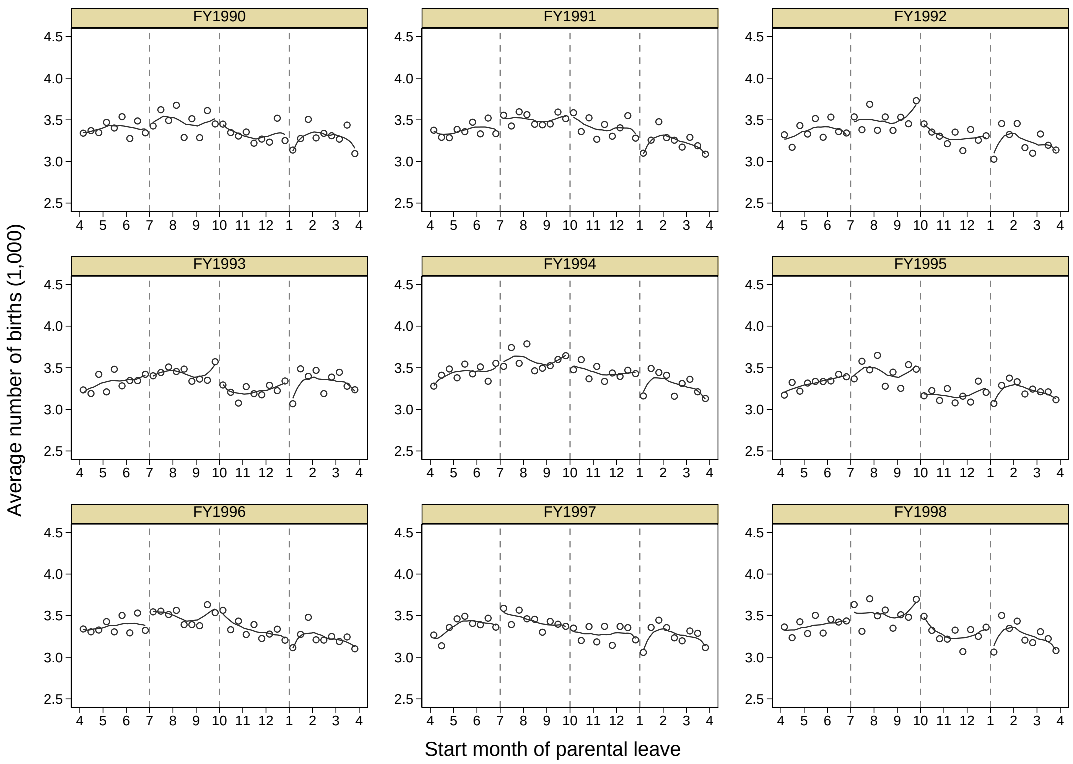


Figure A5: Number of childbirths around policy cut-offs

Data source: *Vital Statistics* (Ministry of Health, Labour and Welfare)
Note: This figure shows the average number of births per day for each 10 days between 1990 and 1999. Since the parental leave is taken after the 56 days of maternity leave, the number of births is calculated on the birth date plus 56 days.

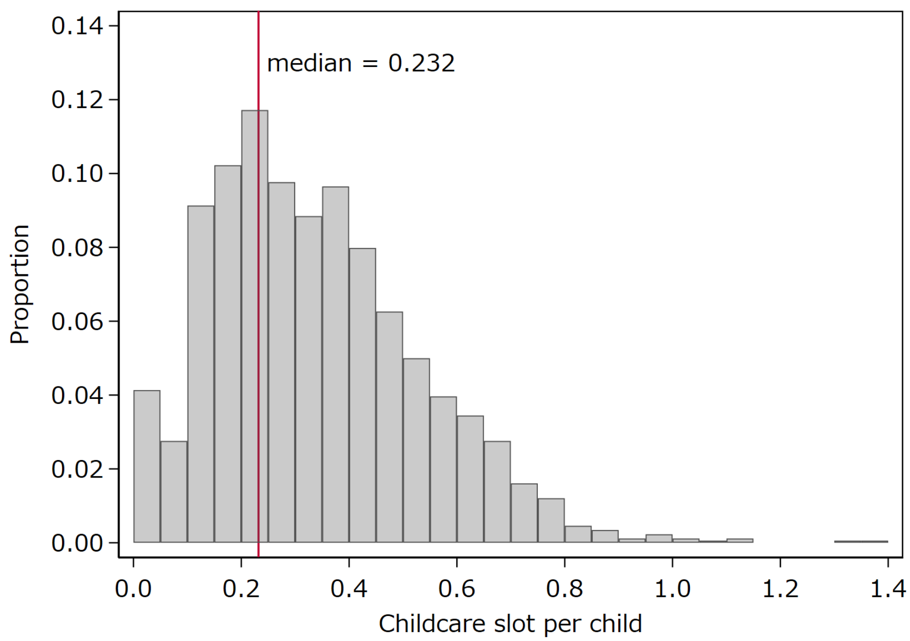


Figure A6: Variation in the availability of childcare centers by municipality in 1990

Data source: *Japanese Census* (Ministry of Internal Affairs and Communications) and *Survey of Social Welfare Institutions* (Ministry of Health, Labour and Welfare)
Note: This figure shows the distribution of the number of seats in accredited childcare centers per child by municipality in 1990. The median is calculated by using the number of children in the municipality as a weight.

Table A1: Coverage of parental leave system

Employment Status Survey	1992	1997
All	29.7%	30.5%
High school grad.	27.6%	28.1%
College grad.	35.0%	42.7%

Table A2: Placebo test

	1994Q3 vs 1994Q2		
Years after birth	6 years (1)	11 years (2)	16 years (3)
Employed	-0.0007	-0.0012	-0.0009
	(0.0017)	(0.0017)	(0.0016)
Full-time	-0.0003	-0.0020	0.0013
	(0.0015)	(0.0016)	(0.0018)
Part-time	-0.0010	0.0005	-0.0020
	(0.0015)	(0.0017)	(0.0018)
Observations	1181206	1178172	1118171

Note: This table shows the estimation results of equation (3), focusing on the effects of the reform in 1995. We only reported the estimates of interest, that is, the estimated coefficients on $D_i \times After_i$. The heteroskedasticity robust standard errors are reported in the parentheses. In this analysis, we used mothers who gave birth between April 1989 and September 1989 or between April 1994 and September 1994.

Table A3: The effects of the cash benefit policy by household type (Q1 and Q4)

		Nuclear households			Non-nuclear households		
		Reduced form	Wald estimates	Mean	Reduced form	Wald estimates	Mean
Years after birth		(1)	(2)	(3)	(4)	(5)	(6)
0–1 years	Employed	0.0010		0.2060	0.0050		0.3753
		(0.0016)			(0.0037)		
	Full-time	-0.0030		0.0870	0.0016		0.2167
		(0.0011)			(0.0031)		
	Part-time	-0.0026		0.0953	-0.0031		0.1346
		(0.0012)			(0.0027)		
	On leave	0.0066		0.0237	0.0064		0.0240
		(0.0007)			(0.0013)		
	Observations	879246			252262		
5–6 years	Employed	0.0011	0.1629	0.4150	0.0050	0.7709	0.5926
		(0.0020)	(0.3069)		(0.0036)	(0.5833)	
	Full-time	0.0013	0.1903	0.2020	0.0041	0.6306	0.3428
		(0.0016)	(0.2388)		(0.0034)	(0.5469)	
	Part-time	-0.0002	-0.0330	0.2053	0.0013	0.2051	0.2421
		(0.0017)	(0.2604)		(0.0032)	(0.5053)	
	On leave	0.0000	0.0057	0.0077	-0.0004	-0.0649	0.0077
		(0.0004)	(0.0538)		(0.0007)	(0.1024)	
	Observations	867457			275677		
10–11 years	Employed	-0.0009	-0.1410	0.6225	-0.0014	-0.2169	0.7462
		(0.0021)	(0.3137)		(0.0033)	(0.5169)	
	Full-time	0.0023	0.3525	0.2740	0.0022	0.3392	0.4129
		(0.0018)	(0.2821)		(0.0036)	(0.5698)	
	Part-time	-0.0034	-0.5203	0.3420	-0.0039	-0.6136	0.3282
		(0.0020)	(0.3031)		(0.0035)	(0.5633)	
	On leave	0.0002	0.0268	0.0065	0.0004	0.0576	0.0052
		(0.0003)	(0.0495)		(0.0005)	(0.0831)	
	Observations	867245			272915		
15–16 years	Employed	0.0021	0.3252	0.7306	-0.0005	-0.0767	0.8058
		(0.0020)	(0.2997)		(0.0032)	(0.4972)	
	Full-time	0.0054	0.8149	0.3931	0.0027	0.4185	0.4990
		(0.0020)	(0.3214)		(0.0039)	(0.6158)	
	Part-time	-0.0030	-0.4606	0.3322	-0.0029	-0.4472	0.3030
		(0.0021)	(0.3157)		(0.0037)	(0.5866)	
	On leave	-0.0002	-0.0292	0.0054	-0.0003	-0.0480	0.0037
		(0.0003)	(0.0497)		(0.0005)	(0.0822)	
	Observations	833704			245894		

Note: This table shows the estimation results of equations (3) and the two-sample Wald estimate, focusing on the effects of the cash benefits policy. We only reported the estimates of interest, that is, the estimated coefficients on $D_i \times After_i$. The heteroskedasticity robust standard errors are reported in the parentheses. In this analysis, we used mothers who gave birth between April 1989 and March 1990 or between April 1994 and March 1995. The reported mean value is among the treatment group in the post-reform year.

Table A4: The effects of the cash benefit policy by mothers' education (Q1 and Q4)

		High-school grad.		College grad.	
		Estimate	Mean	Estimate	Mean
Yrs after birth		(1)	(2)	(3)	(4)
5 years	Employed	-0.0004	0.4549	0.0040	0.4171
		(0.0022)		(0.0028)	
	Full-time	-0.0005	0.2268	0.0042	0.2257
		(0.0018)		(0.0024)	
	Part-time	0.0003	0.2209	-0.0005	0.1805
		(0.0019)		(0.0023)	
	On leave	-0.0001	0.0072	0.0002	0.0109
		(0.0003)		(0.0006)	
	Observations	773328		444347	
15 years	Employed	0.0018	0.7448	0.0036	0.7380
		(0.0021)		(0.0027)	
	Full-time	0.0032	0.4136	0.0064	0.4062
		(0.0023)		(0.0029)	
	Part-time	-0.0013	0.3262	-0.0027	0.3271
		(0.0022)		(0.0028)	
	On leave	-0.0001	0.0050	-0.0001	0.0047
		(0.0003)		(0.0004)	
	Observations	711773		427386	

Note: This table shows the estimation results of equation (3), focusing on the effects of the cash benefits policy. We only reported the estimates of interest, that is, the estimated coefficients on $D_i \times After_i$. The heteroskedasticity robust standard errors are reported in the parentheses. In this analysis, we used mothers who gave birth between April 1984 and March 1985 or between April 1994 and March 1995. The reported mean value is among the treatment group in the post-reform year.