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FUKAI, Taiyo
Gakushuin University

HARA, Hiromi
Meiji University



Research Institute of Economy, Trade & Industry, IAA

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Firms as Intermediaries in Social Policy:
Evidence from Japan's 2005 Employer Mandate for Work-Childcare Support*

Taiyo Fukai

Gakushuin University

Hiromi Hara

Meiji University

Abstract

This paper provides causal evidence that firms act as important intermediaries in the implementation of social policy. In 2005, Japan introduced a policy requiring large firms to develop action plans to establish or expand their company-specific childcare programs. Using pooled repeated cross-sectional survey data with retrospective fertility and employment histories and a Difference-in-Differences (DD) framework, we estimate the policy's impact on working mothers. The results show significant increases in the uptake of maternity and parental leave and improvements in post-birth employment outcomes, with mothers more likely to remain employed and to return as regular employees after the birth of their first child. However, we do not find robust evidence that the policy affected higher-order fertility. These results highlight the importance of firms as institutional channels for implementing family-friendly policies, while also suggesting that workplace-based measures alone may be insufficient to influence fertility behavior and address Japan's broader demographic challenges.

Keywords: firms as policy intermediaries; uptake of family-friendly programs; maternity and parental leave; maternal employment; fertility; barriers to program uptake.

JEL classification: J18, J13, J16

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1 Introduction

As many industrialized economies experience demographic aging and declining fertility, family policies have attracted increasing attention from policymakers and researchers. A large and growing literature examines the effects of such policies on household structure and fertility (Ruhm, 1998; Lalive and Zweimüller, 2009), child welfare (Dustmann and Schönberg, 2012; Carneiro et al., 2015; Ginja et al., 2020), and parental labor market outcomes (Lefebvre and Merrigan, 2008; Lalive and Zweimüller, 2009). Within economics, the empirical evidence on parental outcomes suggests that the effects of family policies are often modest. Reviews by Albanesi et al. (2023), Olivetti and Petrongolo (2017), and Hegewisch and Gornick (2011) indicate that while family policies can influence short-term labor supply decisions surrounding childbirth, their long-term impacts on maternal employment and fertility tend to be limited. Consistent with this broader evidence, findings from Japan point to similar patterns. In Japan, parental leave was introduced at the national level in 1992. While the initial reform strengthened mothers' labor market attachment (Fukai and Toriyabe, 2021), later policy expansions, such as increases in monetary benefits, had little impact on women's labor supply (Asai, 2015; Yamaguchi, 2019).

One possible explanation for these modest effects is that the existing literature has largely focused on governments and employees as the primary agents through which family policy operates, while paying comparatively little attention to the potential mediating role of the firm as employer. In theory, the employer is expected to play a crucial role in the successful implementation of family policies (Albanesi et al., 2023; Cortés and Pan, 2023) because the workplace is often where these policies come to life and are realized. Employers determine how policies are communicated, administered, and supported by facilitating employees' access to various forms of family support as well as providing workplace environments that help employees achieve a sustainable work-life balance. Nevertheless, despite their importance in theory, there remains little to no empirical evidence on whether firms do actively mediate the effectiveness of family policies.

This study attempts to fill this gap by examining the mediating role of firms as intermediaries in the implementation of family policy, and provides causal evidence that employer-targeted regulation can increase the uptake of family-related benefits and improve maternal

employment outcomes. We study the introduction of the 2005 *Act on Advancement of Measures to Support Raising Next-Generation Children*¹, which required private firms with 301 or more employees to formulate action plans supporting work–childcare balance. The policy targeted employers rather than workers directly, mandating firms to design workplace initiatives supporting childcare.

We exploit the size-based treatment eligibility threshold embedded in the law to identify the policy effects. Using repeated cross-sectional data from the National Fertility Survey (NFS) conducted by Japan’s Ministry of Health, Labour and Welfare (MHLW), we construct employment and leave histories at the time women recognize their first pregnancy. The NFS survey records the month and year when respondents became aware of their first pregnancy, allowing us to align policy exposure with the timing of key employment decisions. Our empirical strategy uses a difference-in-differences (DD) framework that exploits variation across firm size and the timing of pregnancy recognition relative to the policy implementation.

Our results show that the policy significantly increased the uptake of maternity and parental leave by working mothers following their first childbirth.² The reform also raised the probability that women remain employed—and in regular employment—after childbirth, suggesting that the enhanced access to leave facilitated continued labor market attachment rather than exit from employment, which has historically been common among mothers in Japan. In contrast, however, we do not find robust evidence that the policy affected the likelihood of women having additional children after the first child.

We further document substantial heterogeneity in the policy’s effects. The impact is concentrated among less-educated women, who may have faced greater informational barriers, workplace frictions, or other constraints to leave-taking prior to the reform. We also find stronger effects for mothers with better access to external childcare support such as assistance from relatives or public childcare services, suggesting complementarities between workplace-based policies and broader community-based childcare resources.

This study contributes to the literature in three ways. First, it provides to the best of our knowledge the first causal evidence that firms act as intermediaries in the implementation of

¹*Jisedai-ikusei-taisaku-shien-hou*, in Japanese

²In the case of mothers, parental leave can be taken from the day after the eight-week postnatal maternity leave ends until the day before the child’s first birthday.

family-friendly policies.³ While most research evaluates family policies through the behaviors of governments and households, we show that employer-targeted policies can significantly influence the ability of workers to utilize family-related benefits. In other words, companies are not necessarily the powerless agents of government directives that they are often assumed in research studies but can mediate government policies by embracing or resisting them, substantively improving worker well-being.

Second, the paper contributes to the growing literature on workplace determinants of parental leave utilization. While prior studies emphasize peer effects (Johnsen et al., 2024; Dahl et al., 2014), information flows, and employer attitudes, much of this evidence is indirect. By examining a policy that directly targets employer behavior, we provide quasi-experimental evidence on how workplace institutions shape leave-taking decisions.

Third, by examining both employment and fertility outcomes, this paper clarifies the scope and limits of employer-mediated policy effects. We show that while firm-level intervention can promote maternal employment and facilitate uptake of leave, evidence on higher-order fertility remains limited. This distinction highlights the limits of workplace-based policy instruments in addressing broader demographic challenges.

Lastly, this paper argues that the effectiveness of family policy depends not only on the policies themselves but also on how firms mediate implementation within the workplace. We interpret the policy as operating through two related mechanisms. First, the requirement that firms formulate and publicly communicate action plans may have reduced informational frictions surrounding the availability and procedures of family-friendly programs. Second, the formalization of employer commitments may have alleviated employees' prior concerns about potential career penalties associated with leave-taking such as unfavorable evaluations, reassignment, or slower promotion. Although we do not directly observe changes in workplace culture or employer attitudes, the heterogeneous effects we document are consistent with these channels.

The remainder of the paper proceeds as follows: Section 2 describes the institutional background and policy reform, while Section 3 outlines the conceptual framework and Section 4

³Two studies have shown that the introduction of family policies can influence corporate human resource management. Bartel et al. (2025) showed that the introduction of paid family leave for small and medium-sized enterprises in New York State in 2018 made it easier for them to manage long-term employee absences, and Ginja et al. (2023) revealed that Swedish employers responded to extended maternity leave by hiring new employees or extending the working hours of existing staff.

presents the data and empirical framework. Section 5 provides a descriptive analysis and Section 6 presents the estimation results. Section 7 describes several robustness checks, Section 8 explores possible mechanisms for the policy effect, and Section 9 discusses the complementarity of the reform and broad public access to childcare services. Section 10 concludes the paper.

2 Institutional Background

2.1 Motives behind the 2005 Policy

Looking at the social and economic conditions in Japan before the 2005 *Act on Advancement of Measures to Support Raising Next-Generation Children*,⁴ by the mid-1990s, Japan was already anticipating a contraction in the labor force, placing increased emphasis on the fuller utilization of women. Yet, structural constraints remained. In 2002, the overall female labor force participation rate stood at nearly 48.5%, but participation among women in their 30s lagged behind other age groups, reflecting the persistence of the well-known “M-shaped” employment profile,⁵ indicating that many women exited the labor market during childrearing years. Consistent with this, among women employed prior to the birth of their first child between 2000 and 2004, only 40.6% remained in the workforce after childbirth.⁶

At the same time, fertility had long been below the replacement level of 2.08, a threshold not reached since 1974. Despite numerous policy efforts such as the 1994 Childcare Support Plan (*Angel Plan*), the birth rate continued to decline, reaching 1.32 by 2002.⁷ Moreover, demographic trends pointed not only to delayed marriage but also to a decline in the number of children within marriage. Together, these developments underscored the need for a coordinated policy response aimed at both supporting women’s continued labor force attachment and addressing persistently low fertility.

At the same time, several family-related programs were already in place. Maternity leave had long been guaranteed under the Labor Standards Act, parental leave had been introduced in 1992, and reduced-hours arrangements had also been institutionalized. In addition, these

⁴For details of the policy, Roumu-Gyousei, ed (2004) and Iki (2011) were consulted.

⁵Japan Bureau of Statistics, *Labor Force Survey*.

⁶MHLW, *The National Fertility Survey 2016*.

⁷In 2005, the birth rate reached an all-time low of 1.26.

programs had already been incorporated into the employment regulations of many large firms: according to the 1999 Basic Survey on Women's Employment Management, parental leave provisions were included in employment regulations at 94.5% of firms with 300–999 employees and 99.3% of firms with 1,000 or more employees (JILPT, 2006). Nevertheless, actual use and employment continuation remained limited. Among eligible mothers whose first child was born between 2000 and 2004, only 26.9% took parental leave (NFS, 2005, 2010 and 2015). Details on these programs are provided in Section 4.3.

2.2 The 2005 Policy

Against this backdrop, Japan enacted the *Act on Advancement of Measures to Support Raising Next-Generation Children* in 2005. The policy aimed to address declining fertility by promoting work-childrearing balance and improving conditions for raising children. Its distinguishing feature was a direct intervention in firms' employment practices. Large private employers (301 or more employees) were required to formulate *action plans* and submit notifications of these plans to the Minister of Health, Labor and Welfare via regional Labor Bureaus,⁸ while smaller firms were exempt.⁹ We use this variation in firm size-based eligibility, and in the timing of awareness of first pregnancy, to identify the policy's effects.

The *Act* imposed a standardized planning process on all treated firms. Each plan had to specify: i) a duration of three to five years, ii) concrete goals, and iii) the measures to achieve them, including implementation timelines. Compliance was mandatory and monitored through formal submission to the regional Labor Bureau, as described below. The emphasis on specificity was intended to translate broad policy objectives into actionable, near-term workplace changes.

Within this framework, rather than a one-size-fits-all solution, firms retained discretion over content, actively reviewing what was lacking in their workplaces and addressing those deficiencies. This generated cross-firm heterogeneity in implementation according to the needs of each company. Typical measures included: i) the introduction or expansion of family-friendly programs such as flexible work arrangements, spousal leave, and extended parental leave; ii)

⁸Japan has 47 prefectures, with each having a regional Labor Bureau that serves as a local office of the MHLW.

⁹However, in 2008, an amendment of the *Act* made it mandatory for employers with 101 or more employees to also create an action plan, and this went into effect in 2011.

facilitation of existing benefits through information campaigns and managerial training;¹⁰ and 3) broader improvements in working conditions for all workers such as facilitating paid leave. As a result, action plans varied according to firm-specific needs.

Our treatment definition is based solely on statutory exposure to the *Act* rather than realized firm-level measures. Accordingly, cross-firm heterogeneity in plan content does not constitute endogenous variation in treatment assignment. Instead, identification relies on the exogenous size threshold that determines policy coverage, combined with time variation in pregnancy awareness, to construct a quasi-experimental comparison between treated and control firms.

2.3 Policy Implementation

Although the Act did not impose penalties for non-compliance, uptake was nearly universal: by September 2007, 97.6% of firms with 301 employees or more had submitted notifications of action plans.¹¹ This high compliance likely reflects several factors. First, the formal submission requirement to the Minister of Health, Labour and Welfare itself created reputational and administrative pressure on companies. Second, the Equal Employment Offices of regional Labour Bureaus actively supported and guided non-compliant firms. Third, firms that achieved their stated goals could obtain certification and display an official mark in corporate communications, providing them with potential commercial benefits. Consequently, among early-filers (by June 2005), 24.3% were planning to seek certification.

Institutionally, an action plan in Japan would typically be drafted by the firm's HR department, but since a filing with the Minister of Health, Labor, and Welfare is a serious procedure, this would require approval from top management prior to submission. Given the formal nature of the reporting process, this approval likely increased firms' commitment to the policy, with the HR department responsible for implementation once plans were authorized.

Survey evidence suggests substantive changes in employer behavior. A 2012 survey by the Japan Institute for Labour Policy and Training (JILPT) reports that 60% of treated firms introduced new family-friendly programs following action plan formulation.¹² A smaller (N=103)

¹⁰The requirement to publicize and inform employees of the action plan was not mandated in 2005 but implemented in the 2009 policy revision.

¹¹MHLW press release in October 2007. <https://www.mhlw.go.jp/houdou/2007/10/h1019-2.html>.

¹²JILPT, "A survey on the careers of male and female regular employees and work-life balance support," October 12–31, 2012 (<https://www.jil.go.jp/press/documents/20130312.pdf>).

2005 JILPT survey¹³ similarly indicates active implementation: 63% of responding firms reported new programs to promote parental leave uptake (e.g. information campaigns, managerial seminars, securing substitute personnel, and skill development during leave), while others introduced new measures related to maternity leave (9.6%) and reduced hours (21.7%).

Overall, the evidence suggests that most treated firms not only complied with the reporting requirement but also put the action plans into practice, implementing concrete workplace changes, particularly in areas related to maternity and parental leave and reduced hours.

3 Conceptual Framework

This section presents a simple framework for how the Next-Generation Law may affect the uptake of statutory family policies with firms as intermediaries. Although statutory rights such as parental leave exist formally, uptake is often incomplete and diffuses only gradually, indicating that legal availability does not automatically translate into effective use.

We argue that this gap arises because standard policy models typically abstract from firms, implicitly treating statutory rights as frictionlessly usable once legislated, ignoring the agency of firms in their uptake. By incorporating firms as active intermediaries, our framework explains both the underutilization of statutory rights and the channels through which the Next-Generation Law might increase uptake. The model is intentionally parsimonious, as it is designed to guide the empirical analysis rather than provide a structural estimation framework.

3.1 Benchmark without Firms: Government + Individual

Consider a benchmark in which a statutory family policy (e.g., parental leave or reduced hours) is introduced and can be used without frictions. A working mother derives baseline utility U from continued employment. Experiencing a childcare burden imposes a cost $\kappa > 0$. Let $x \in \{0, 1\}$ denote the decision to take up the statutory policy. If she continues employment, her utility is

$$U_C = U - \kappa + Bx, \tag{1}$$

¹³JILPT, "Business Labour Monitor Survey," June 13–July 6 in 2005 (<https://www.jil.go.jp/kokunai/blt/bn/2005-10/p58-67.pdf>).

where $B > 0$ represents the benefit from policy use. Exiting employment yields outside utility U_0 . In the benchmark, two strong assumptions hold:

1. The worker is fully informed about the policy and understands her eligibility.
2. Policy take-up entails no firm-specific penalty or retaliation.

Under these assumptions, the continuation condition is

$$U - \kappa + Bx \geq U_0. \quad (2)$$

Because workers are fully informed and face no workplace penalties from policy uptake, they optimally choose $x = 1$ whenever $B > 0$.

If B is sufficiently large, introducing a statutory policy mechanically increases employment continuation among mothers facing childcare costs. Because usage is frictionless, the model cannot easily generate persistent under-utilization of existing statutory rights, for whenever the policy exists and is beneficial, workers optimally choose $x = 1$. Consequently, the model leaves little room for heterogeneity across firms or gradual diffusion over time.

3.2 Firms and Two Frictions

The benchmark model above, which characterizes the literature, abstracts from firms and implicitly assumes that statutory rights translate directly into effective options. We now relax this by allowing workplace conditions to shape effective access. Specifically, we introduce two firm-level frictions that do not alter legal entitlement but reduce its effective payoff. For simplicity, these workplace conditions are taken as given, for the framework is intended to guide our empirical interpretation, not model firm behavior.

Returning to the continuation condition in (2), we allow the benefit from policy use to be mediated by workplace conditions. If a mother remains employed and uses the statutory policy, her utility becomes

$$U_C = U - \kappa + Bx - \phi, \quad (3)$$

where $\phi \geq 0$ captures the effective cost of using the policy within the firm.

We decompose this friction as

$$\phi = \phi_I + \phi_C, \quad (4)$$

where ϕ_I reflects workplace information and permissibility conditions, and ϕ_C reflects credibility conditions regarding future career consequences.

Information and permissibility. Even when a statutory right exists, workers may lack clear information about eligibility or procedures or be uncertain whether use is acceptable in their workplace. Let $\lambda \in [0, 1]$ denote the probability that a worker is both (i) aware of the policy and (ii) perceives its use as permissible, so that she can effectively access it when desired.

A simple way to capture this is to assume that imperfect information or ambiguity about permissibility reduces the effective value of the statutory arrangement. When workers are unsure about eligibility, procedures, or workplace acceptance, the realized benefit from using the policy may be lower than the statutory benefit B . For simplicity, we represent this friction as a proportional reduction in the effective policy benefit so that the realized benefit from policy use becomes λB . For notational convenience, we represent the implied loss from imperfect access as

$$\phi_I \equiv (1 - \lambda)Bx,$$

so that the deterministic representation in (3) yields the effective payoff λBx .

Credibility of non-retaliation. Even when informed, a worker may anticipate future penalties from policy use, such as slower promotion or unfavorable assignments. Let $\pi > 0$ denote the utility cost of such a penalty, and let $q \in [0, 1]$ denote the perceived probability that the penalty occurs. We represent the expected penalty as $\phi_C \equiv q\pi x$.

Substituting these expressions into (3), the employment continuation condition becomes

$$U - \kappa + \lambda Bx - q\pi x \geq U_0. \tag{5}$$

Equivalently, the continuation condition can be written as

$$U - \kappa + x(\lambda B - q\pi) \geq U_0.$$

Because the policy benefit and the expected penalty apply only when the worker chooses $x = 1$,

policy uptake occurs whenever the net payoff from policy use is sufficiently positive.

$$\lambda B - q\pi \geq 0.$$

Thus, λ and q jointly shape both the likelihood of policy uptake and the decision to remain employed.

Relative to the benchmark model, the effective payoff from policy use is now attenuated by imperfect information or questionable permissibility ($\lambda < 1$) and expected career penalties ($q\pi > 0$). This framework generates two features that the benchmark model cannot easily explain. First, statutory rights may remain underutilized if λ is low or q is high, even when B is positive, for workers may rationally refrain from policy uptake when the effective benefit λB is small or the expected penalty $q\pi$ is large. Second, policy diffusion may be gradual. If workplace communication improves slowly, norms evolve or credibility builds gradually over time, then λ may increase and q may progressively decline. As these frictions are relaxed, the effective payoff from policy use rises, generating increasing uptake and employment continuation even without additional statutory reform.

3.3 Interpreting the Next-Generation Law

The Next-Generation Law requires large firms to formulate action plans concerning work–family measures, often including internal communication strategies, numerical targets, and formal procedures. Within this framework, the law affects workplace conditions by:

1. Increasing λ through greater transparency, internal communication, and clarification of permissible use.
2. Decreasing q by strengthening the credibility of non-retaliation through formal planning, monitoring, and reputational mechanisms.

Formally, let the law shift workplace conditions from (λ, q) to (λ', q') such that

$$\lambda' > \lambda, \quad q' < q.$$

Proposition 1 (Policy uptake and employment continuation). *If the Next-Generation Law increases λ and decreases q , then it weakly increases:*

(i) *the probability of policy uptake ($x = 1$), and*

(ii) *the probability of employment continuation among mothers facing childcare burdens.*

Moreover, the effect is strictly positive whenever the pre-policy continuation condition binds for a non-zero measure of workers and the law induces a non-trivial shift in (λ, q) .

Proof. An increase in λ and a decrease in q weakly increase the term $(\lambda B - q\pi)$, thereby expanding the set of workers for whom $x = 1$ is optimal and for whom the continuation condition holds. □

Heterogeneity Implication. In this framework, the law shifts workplace conditions from (λ, q) to (λ', q') , raising the net return to uptake, $\lambda B - q\pi$. Whether this generates larger empirical effects in “high-friction” environments is not mechanical: if $(\lambda' - \lambda, q - q')$ were constant across groups, the induced index shift would be similar.

However, larger effects can arise under two conditions. First, if the law relaxes frictions proportionally or more strongly where baseline frictions are larger—for example, because communication, monitoring, or reputational mechanisms matter more in workplaces with weaker pre-existing systems—then environments with lower initial λ or higher q may experience larger effective improvements. Second, larger effects may emerge when a greater mass of workers is located near the uptake or continuation margin in those environments, so that a given index shift translates into more switching behavior.

Accordingly, we use heterogeneity analyses to assess whether the estimated patterns are consistent with these channels. The framework does not require observables that uniquely proxy for (λ, q, π) ; rather, it provides a disciplined way to interpret stronger estimated effects as evidence of larger pre-policy frictions and greater scope for improvement under the law.

Lastly, we do not attempt to identify changes in λ and q but instead test whether the reform reduced firm-mediated frictions in a manner consistent with this conceptual framework.

4 Econometric Framework

4.1 Data

We use the *National Fertility Survey* (NFS) conducted by Japan's Ministry of Health, Labour and Welfare (MHLW),¹⁴ a major official survey fielded roughly every five years since 1952. The NFS consists of a survey of married women and a survey of unmarried individuals, and we use the married women's survey.¹⁵ The NFS (the married women's survey) collects detailed information on marriage, fertility, and child-rearing. Our analysis pools the married-women samples from 2005, 2010, 2015, and 2021, yielding repeated cross-sectional data. Although not a true panel, the survey's retrospective questions allow us to reconstruct key life events, including women's fertility histories, uptake of family-related leave, and employment status before and after childbirth.

The sample consists of married women under age 50, who report information for themselves and their husbands. Despite its sampling design, the NFS survey is highly representative, with large sample sizes and high response rates: 6,836 (85.7%) in 2005, 7,847 (86.7%) in 2010, 6,598 (87.8%) in 2015, and 6,834 (72.7%) in 2022.¹⁶ These features make it well-suited for analyzing policy-relevant behavior around the 2005 reform.¹⁷

The NFS has been widely used in prior studies of fertility and employment in Japan (e.g. Ghaznavi et al., 2022; Uchikoshi et al., 2026), but an additional advantage of the NFS for this study is its retrospective design, which allows us to reconstruct key life-course events and link outcomes from pregnancy through the post-birth period for the same individual. Within samples of women who have experienced childbirth, it also provides a relatively large sample (N=3,460, as detailed below).¹⁸ The survey records the timing of first birth (month and year) of married women, employment status at pregnancy, firm size at the time of pregnancy awareness, and the date when the first child turned one year old. It also captures uptake of maternity

¹⁴The survey is commissioned by the National Institute of Population and Social Security Research.

¹⁵The effects on childbirth and work for single women would be different for married women; however, this is beyond the scope of this study.

¹⁶The NFS uses a stratified random two-stage cluster sampling method to select a sample of married women under 50 years of age nationwide, with the census tract as the sampling unit.

¹⁷The only other data available for this analysis is the Japanese Panel Survey of Consumers, which began in 1993 with a sample of 1,500 women aged 24 to 34, making its age profile and much smaller size less suitable for this study.

¹⁸For example, even in the U.S. NLSY97, the initial sample included 4,385 women, but the number of women who experienced a first birth during the observation period is substantially smaller and falls below the sample size used in this study.

leave, parental leave, or reduced hours during the first three years after childbirth.¹⁹ These data allow us to identify whether women working at treated versus control firms at the time of pregnancy used family-friendly programs and remained employed one year after childbirth.²⁰ We exploit this data structure in a difference-in-differences (DD) framework to estimate the causal effect of the policy.

We define the treatment group as women employed as regular workers in firms with 300 or more employees at the time they became aware of their first pregnancy, and the control group those in firms with 299 or fewer employees. While the statutory threshold is 301 employees (not 300), the NFS reports firm size in coarse categories, which necessitates this approximation. Any misclassification due to this difference of one employee is negligible, however. According to the Establishment and Enterprise Census, which is a complete count survey conducted by the Japan Bureau of Statistics, firms with exactly 300 employees accounted for only 2.1% of all firms with 300 or more employees as of April 2004 and 1.5% as of 2006. This implies that the contamination from non-treated firms being included in the treatment group is minimal.²¹

Concerns about strategic firm resizing are also limited. Census data show little change in the number of firms above the threshold (11,400 in 2004 versus 11,803 in 2006), suggesting no systematic avoidance. A remaining concern is a potential sorting of workers across firms around pregnancy, which we address later in the paper.

4.2 Analysis Sample

We pool four waves of the NFS married-women survey, yielding an initial sample size of 28,115, and restrict the sample to women who became aware of their first pregnancy between FY2000 and FY2010²² to secure a sample of five years before and after the policy was introduced, reducing the sample size to 5,454. We use the timing of first pregnancy awareness as the key reference point in our analysis, as that is when women begin forming expectations and mak-

¹⁹The NFS asks the same questions for the second and third childbirths; however, reports using published NFS data show that the impact of the first childbirth is significant. Additionally, due to the insufficient sample size for births of the second child and beyond, this study focuses only on the first childbirth.

²⁰While the survey relies on respondents' retrospective reporting, recall bias is likely limited because the questions concern salient life events such as the birth of the first child and employment status at specific points in time, which respondents tend to remember accurately.

²¹As noted in [Pischke \(2007\)](#), when the measurement error in an explanatory variable is small, the attenuation bias in the OLS estimates due to such error is also small.

²²The Japanese fiscal year t begins in April in t and ends in March in $t + 1$. The policy was introduced on April 1st, 2005, in fiscal year (FY) 2005.

ing decisions about their careers following pregnancy and childbirth. Accordingly, we restrict the analysis sample to women who became aware of their first pregnancy between FY2000 and FY2010. The endpoint (FY2010) avoids contamination by a policy change in FY2011 that expanded coverage to also include firms with 101-300 employees.

As the policy targeted private-sector firms, we further restrict the sample to those employed in the private sector—either as regular or non-regular employees—when they became aware of their first pregnancy. The treatment group is restricted to regular employees in large firms, while the control group includes both regular and non-regular employees in smaller firms. This asymmetry reflects evidence that firms had limited initial awareness that non-regular workers should be covered (MHLW policy report, 2013), although the *Act* formally covers both employment types. Accordingly, our main specification focuses on married female regular employees in treated firms, yielding an analysis sample of 3,460 observations (973 treated; 2,487 control).

For men’s outcomes, the corresponding sample includes 3,975 observations (1,871 treated; 2,104 control).²³ Table A1 in the Appendix provides details of the main analysis sample, including women’s survey year and date of awareness of pregnancy with their first child. For a robustness check on our main specification, we use a sample in which the control group is also limited to regular employees so as to align the expected length of employment for the treatment and control groups. As most regular employees in Japan work full-time under a permanent (time-indefinite) contract, they are considered to be highly motivated to continue working even after childbirth. Because relatively few women work as regular employees in Japan (around 20% of all Japanese employees between 2000 and 2010),²⁴ this restriction leaves us with a sample size of 2,376 for the robustness check.

Table 1 reports descriptive statistics for key individual characteristics by treatment status and sex. For both women and men, the treatment group is older and more likely to hold a university degree, and more likely to live in urban and surrounding areas.²⁵ As highly educated women are more likely to work at large companies, which are typically located in urban areas,

²³Men’s outcomes are available only from the 2010 NFS onward. The sample size is larger for men because their baseline employment rate is substantially higher than that of women.

²⁴For example, in 2010, the number of both male and female employees was 51.1 million, and that of female regular employees was 10.5 million (*Labor Force Survey* of the Japanese Bureau of Statistics).

²⁵In the NFS, urban and surrounding areas can be identified separately from rural areas. Urban and surrounding areas correspond to Densely Inhabited Districts (DID) as defined by the Statistics Bureau of Japan, where the population density is 4,000 or more people per square kilometer, and the total population of adjacent basic unit areas is 5,000 or more. These areas are designated based on the *National Census*. More details can be found at <https://www.mhlw.go.jp/toukei/list/118-1b.html>.

these variables were included as controls in the estimations.

4.3 Outcomes

We examine three outcomes: i) uptake of family-friendly programs, ii) post-birth employment, and iii) subsequent fertility. For mothers, all outcomes are directly observed. For fathers, data are limited to the uptake of parental leave and reduced working hours.

The first set of outcomes comprises dummy variables for use of maternity leave (a short-term leave for the protection of the mother before and after childbirth), parental leave (taken for childcare from the day following the end of postnatal leave until the child turns one year old), and reduced working hours. In Japan, these are all statutory entitlements or employer obligations. Maternity leave, established under the 1947 *Labor Standards Act*, includes pre- and postnatal periods and prohibits work during the six weeks following childbirth, even if at the mother's request, so our measure largely captures prenatal leave.²⁶ Parental leave became a statutory right for men under the 1992 *Childcare Leave Act* and in 2005 was further extended to non-regular employees in addition to regular employees, provided that certain conditions were met.²⁷ Employers are thus required to grant parental leave upon request regardless of gender or employment status. We assess the implications of this extension in Section 5 and show that its impact is likely minimal.

The 1992 *Act* also required employers to establish work-family accommodations such as reduced hours, flexible schedules, or staggered start/finish times. We focus on reduced working hours because it was the most common accommodation in 2005²⁸ and is the only one captured in the NFS.²⁹ This program primarily targets regular employees, allowing shorter hours for childcare while retaining regular employee status.³⁰

²⁶The number of days of maternity leave is up to six weeks before childbirth, including the expected delivery date, and up to eight weeks after childbirth if the woman has a single pregnancy. The postnatal leave during the 7th and 8th weeks after childbirth is based on the worker's request so, more precisely, our variable also includes the last two weeks of postnatal leave.

²⁷At the time, eligibility required at least one year of continuous employment with the same employer, an expectation of continued employment until the child's first birthday, and not being a day laborer.

²⁸The 1995 *Childcare and Family Care Leave Act* amended and expanded the 1992 *Childcare Leave Act*, making workers who had a child under the age of three, were not employed on a daily basis, and whose scheduled working hours per day exceeded six hours eligible for reduced hours.

²⁹According to the 1999 MLHW *Basic Survey on Women's Employment Management (JILPT, 2006)*, the programs most commonly introduced by firms were reduced working hours (29.9%), exemption from overtime (22.9%), adjusted starting and ending work hours (21.7%), and flextime (8.9%).

³⁰In July 2012, the reduced working hours program, which had previously been one of several options, became mandatory but that does not affect our analysis.

The second outcome is employment status one year after first birth, measured by dummy variables for i) regular employment and ii) employment of any type. Given that statutory parental leave in Japan extends for one year, these measures include mothers who take parental leave and subsequently return to work. Transitions from small to large firms are rare in this context: in our analysis sample, only 0.92% of women employed at firms with fewer than 300 employees at awareness of first pregnancy are observed in firms with 300 or more employees one year after first childbirth, implying minimal post-birth switching from control to treatment.

The final outcome proxies fertility responses. While the NFS does not report completed fertility, it does elicit the lifetime planned and ideal number of children³¹ which we use as an approximation. In our sample—women observed at first birth—this captures intentions for higher-order fertility, not the decision to have a first child.

4.4 Empirical Model

We employ a Difference-in-Differences (DD) design that exploits policy exposure varying with firm size. Our empirical strategy estimates the reduced-form effect of the policy on mothers' outcomes. Though not a true longitudinal panel, we use pooled repeated cross-sectional data with retrospectively reconstructed life histories to estimate the following equation:

$$Y_i = \alpha + \beta(D_i \times PostAwarePreg_i) + \theta D_i + \rho_{t(i)} + X_i' \gamma + u_i, \quad (6)$$

where i is an individual and u_i is the error term. Y indicates the three outcome variables: i) uptake of family-friendly programs, ii) post-birth employment status, and iii) higher-order fertility, as explained in section 4.3.

$PostAwarePreg$ indicates exposure to the policy and is a binary variable equal to 1 if the woman became aware of her pregnancy in April 2005 or later (FY2005 or later) and 0 otherwise. D indicates treatment status, and is a binary variable taking 1 if she was a regular employee at a firm with 300 or more employees at the time of first pregnancy awareness, and 0 otherwise. $\rho_{t(i)}$ denotes fiscal-year fixed effects for the fiscal year in which the woman became aware of her first pregnancy. The coefficient of interest is β . X is a vector of control variables including the ages of the mother and her husband at the time of first pregnancy awareness, indicators

³¹These are the responses to the questions “How many children do you and your spouse intend to have in total?” and the ideal number of children for the couple, respectively.

for whether each completed a four-year university degree or higher, residential region,³² city size,³³, survey year, and a full set of birth-year fixed effects.

For fathers, information is available only on the uptake of parental leave and reduced working hours after the birth, so we focus on these outcomes. Here, D_i is defined by the father's firm size at marriage, the only point at which firm-size information is available. $PostPreg$ is defined as in the analysis for women, and the vector X_i includes the same covariates.

Identification within our DD design arises from the assumption of a parallel trend between the treatment and the control group. To test for a pre-existing trend, we conduct a Dynamic DD estimation using the following equation:

$$Y_{i,2005-j} = \alpha + \sum_j \beta_j (D_i \times AwarePreg_{i,2005-j}) + X'_{it} \gamma + \lambda_k + \rho_j + u_{i,2005-j}, \quad (7)$$

Here, 2005 refers to FY2005, so $j = 0$ corresponds to FY2005 and $j \in (-5, \dots, 0, 1, 3, \dots, 5)$.³⁴ λ_k and ρ_j indicate firm size and event time fixed effects, respectively, with k indexing individual i 's firm-size category at pregnancy awareness. This specification allows us to visually and statistically assess the validity of the parallel trends assumption prior to policy implementation.

Figure 1 plots $\hat{\beta}_j$ for women, the estimation results of Eq. (7), for uptake of parental leave, probability of employment one year after first childbirth, and lifetime number of expected children.³⁵ Panels A and B show no clear differences in pre-policy trends between the treatment and control groups, suggesting no major concerns regarding the validity of the parallel trend assumption. While Panel C shows greater variability, there is no evidence of a systematic or clear divergence in the trends between the two groups before the policy. It is also worth noting that in Panels A and B, the policy effects become statistically significant after FY2009, which will be discussed in the following section.

³²The NFS divides the 47 prefectures into seven regional blocks: Hokkaido, Tohoku, Kanto, Chubu, Kinki, Chugoku / Shikoku, and Kyushu / Okinawa.

³³The NFS has seven categories for city size: Non-Densely Inhabited District (DID), and DIDs with populations of less than 100,000, 100,000–199,999, 200,000–499,999, 500,000–999,999, one million–1,999,999, and more than 2 million.

³⁴We use $j = 2$ (FY2003) as the base year because FY2004 is after the policy announcement.

³⁵In Figure 1, we set the reference period as FY2003 rather than FY2004 because the policy was announced in July 2003, which may have induced anticipatory responses before its formal implementation. See Figure A2 in the Appendix for graphs of other maternal outcome variables.

5 Descriptive Evidence

Before presenting the estimation results, we examine graphical and descriptive statistic evidence of post-policy differences between the treatment and control groups, using the main analysis sample as described in section 4.1.

Figure 2 plots year-quarter means and nonparametric fits of outcome variables by the timing of pregnancy awareness.³⁶ The treatment group (300 employees or more) is shown in blue and the control group (fewer than 300 employees) in red. We focus on parental leave uptake, employment one year after childbirth, and expected lifetime fertility.³⁷

Panel A shows that parental leave uptake is consistently higher in the treatment group, and parental leave, maternity leave, and reduced hours all increase more for the treated than the control group (Figure 2 and Appendix Figure A3), indicating that firms' responses to the 2005 reform led working mothers to increase their use of family-friendly programs.

In 2005, an amendment to the *Child Care and Family Care Leave Law* extended coverage to nonregular employees such as daily workers and fixed-term employees. However, Panel A shows no discrete change for the control group—which includes both regular and non-regular employees—around April 2005, suggesting a limited impact of this amendment. Eligibility conditions (such as expected employment beyond the child's first birthday) likely constrained coverage among non-regular employees.³⁸

Panel B of Figure 2 shows that employment one year after first childbirth rises more for treated than control mothers post-reform. This reflects a higher probability both of employment and of regular employment (Appendix Figure A3). In contrast, Panel C of Figure 2 shows no clear post-policy difference between treated and control mothers in expected lifetime fertility. We also see that the effects in Panels A and B of Figure 2 become more pronounced after FY2009, consistent with the *Act's* requirement that action plans span three to five years. As initial plans reached renewal, firms may have strengthened implementation, amplifying the observed effects.³⁹

³⁶The local constant regression is used.

³⁷See Figure A3 in the Appendix for graphs of other outcome variables.

³⁸Furthermore, although the Ministry of Health, Labour and Welfare provided guidelines for determining whether an employee meets this requirement, in practice, even when there was a possibility of contract renewal, some non-regular employees were still excluded from parental leave eligibility. As a result, non-regular employees may have felt that they could not take advantage of parental leave.

³⁹After the completion of the first action plan, firms were required by the *Act* to update their plans by formulating the second, third, and subsequent action plans.

Table 2 reports pre/post-policy means and DD estimates. For women, Panel A shows statistically significant increases of about 10 percentage points in the uptake of maternity leave, parental leave, and reduced hours. Panel B shows a 10-12 percentage point rise in post-birth employment, and Panel C shows a statistically significant change in planned fertility (about 0.105 children), although the difference is not visually pronounced in Figure 2. As shown below, these fertility-related patterns are not estimated precisely in the regression analysis.

For men (Table 2), the uptake of parental leave and reduced hours remains extremely low (0.1 to 0.4 percent) across firms and throughout the sample period. Although there is a slight post-reform increase at large firms, the magnitude is negligible. Overall, the descriptive evidence suggests that the reform led to meaningful effects for women's uptake of benefits and employment but little change in men's behavior.

6 Estimation Results

Table 3 shows the point estimates of $\hat{\beta}$ from Equation (6), capturing the policy's effects on our main outcome variables. Column (1) presents the baseline specification using the main sample, where the control group includes both regular and non-regular employees at firms with 299 or fewer employees. To address any concerns about including non-regular employees in the control group, Column (2) reports a robustness check restricting the control group to regular employees only.

Panel A presents the estimated policy effects on the uptake of the three family-friendly (FF) programs featured in this study: maternity leave, parental leave, and reduced hours. As explained in section 4.3, while prenatal leave may be granted upon request by the employee, post-natal leave is not permitted in Japan for six weeks after childbirth even if requested.⁴⁰ The policy significantly increased uptake of maternity leave by 9.5 percentage points (pp) which, given the post-natal constraint, implies that the proportion of pregnant women requesting prenatal leave increased.

Parental leave can be taken by both male and female employees upon request to their employer, which the firm cannot refuse under the *Child and Family Care Leave Law*. The estimation results show that the 2005 reform increased uptake of parental leave by 11.6 pp. The control

⁴⁰The legal postnatal leave period is within 8 weeks in the case of a singleton pregnancy; however, women may engage in duties deemed appropriate by a physician after six weeks have passed and upon their request.

mean in Column (3) is 47.5%, indicating that without the policy, only 47.5% of mothers would have taken parental leave. However, the policy raised uptake to 59.1%, representing a 24.4% increase ($=11.6/47.5 \times 100$). The reform also significantly increased uptake of reduced work hours. While this program is only available to mothers returning to work after childbirth, our analysis sample also includes those who did not return to work. Among women after first childbirth, 16.4% used reduced hours prior to the reform, but the policy shift raised this by 8.9 pp to 25.3%.

Panel B reports the policy effects on employment one year after first childbirth. Column (1) shows statistically significant increases in both overall employment and regular employment. The employment rate rises by 10.0 pp—from 54% pre-reform to 64% post-reform—and the regular employment rate increases by 11.3 pp. Because regular employment is usually permanent (not a fixed-term contract), these gains tend to lead to longer tenure, an additional benefit over time. Overall, the results indicate that the 2005 reform improved mothers' post-birth labor market attachment, particularly in stable long-term employment, after the birth of their first child.

To sum up, the policy increased working mothers' uptake of maternity and parental leave and strengthened post-birth employment. More mothers returned to work after childbirth and used reduced-hours arrangements, suggesting that these programs might have facilitated the return to work after childbirth.

A remaining concern is the inclusion of non-regular employees in the control group. Column (2) of Panels A and B of Table 3 addresses this concern by restricting the control group to only regular employees working at firms with fewer than 300 employees. The coefficients are smaller than in Column (1) but remain statistically significant, confirming the robustness of our results.

Turning to fertility, Columns (1) and (2) of Table 3 show that the estimated effects on the planned and ideal number of children are not statistically significant. While some point estimates are positive, they are imprecisely estimated, so the results do not provide robust evidence that the policy affected higher-order fertility beyond the first child.

Overall, the 2005 reform increased leave uptake and employment outcomes for women after first childbirth, while the evidence on fertility remains inconclusive.

7 Robustness

The results in Section 6 show that the 2005 policy increased mothers' uptake of family-friendly programs and their likelihood of remaining employed after the birth of their first child. While these findings are robust across outcomes, several potential identification and specification concerns merit further examination.

First, identification exploits variation in firm size at the time of first pregnancy awareness. If women planning to continue working after childbirth selectively moved from small to large firms around childbirth, the estimates may be upward biased.

Second, as shown in the summary statistics (Table 1), the treatment and control groups differ systematically in observable characteristics such as education and residential location. While these differences are controlled for in the baseline linear DD specification, the estimated effects may still depend on functional form assumptions.

Third, the identification strategy relies on the parallel trend assumption between women employed at large and at smaller firms. Although the event-study estimates provide no evidence of different trends prior to the reform, large and small firms may nonetheless have been exposed to different time-varying shocks. Moreover, because treatment timing varies across cohorts within a multi-year window, recent concerns in the Difference-in-Differences literature regarding staggered treatment adoption may be relevant. This section addresses these concerns through a series of robustness and specification checks.

7.1 Check of Whether Women Transferring to Treated Firms Influenced the Results

After the reform, if women who intended to remain employed after childbirth disproportionately transferred from small to large firms, our estimates of parental leave uptake and post-childbirth employment by treated women could be inflated. Given the difficulty that employees in Japan face in transferring from small to large firms,⁴¹ this scenario appears unlikely. Nevertheless, to formally assess this possibility, we conduct an analysis that restricts the sample to women whose firm size at the time of labor market entry coincides with the firm size

⁴¹For example, in 2011, the number of individuals whose previous workplace was a small or medium-sized enterprise (SME, with 1–299 employees) and who were still employed at an SME was 1.4099 million, whereas 0.6154 million were employed at firms with 300 or more employees. This implies that only about 30% of workers who left SMEs moved to large firms, while the majority transitioned from one SME to another (Source: *White Paper on Small and Medium Enterprises 2016*, based on the Ministry of Health, Labour and Welfare's *2011 Survey on Employment Trend*).

at which they were employed when they became aware of their first pregnancy. By excluding these individuals for whom firm size differs between these two points, we remove women who may have changed employer size prior to childbirth.

This restriction is particularly relevant because, for the vast majority of women in our sample, initial labor market entry occurred well before 2005. In Japan, as job mobility from a small to large firm is very limited, for those women who became aware of their pregnancy after the 2005 policy reform, firm size at pregnancy awareness largely reflects the employer chosen prior to the reform. Applying this restriction reduces the sample size to 2,624, which is approximately three quarters of the main sample.

Table 4 presents the results. We find no substantial differences in the point estimates compared to the main results reported in Table 3 and only a modest increase in standard errors. These findings suggest that it is unlikely that selective job transitions across firm-size categories drive our main results.⁴²

7.2 Specification Robustness

The parallel trend assumption underlying the DD estimation requires that the mean outcome for the treatment group would have evolved in parallel with that of the control group even in the counterfactual absence of the policy intervention. However, it is conceivable that time-varying confounding factors may have affected the treatment and control groups differently in ways correlated with firm size. For example, large and small firms may have been exposed to different time-varying macroeconomic shocks.

Figure A1 of the Appendix summarizes the evolution of real wages for the treatment and control groups from 2000 to 2010. The trends are very similar, suggesting that the two groups were likely exposed to comparable macroeconomic environments both before and after the policy change, alleviating concerns about differing aggregate shocks. Moreover, the dynamic DD estimates presented above indicate no systematic differences in pre-trends across the two groups.

Nevertheless, even if the parallel trends hold on average, it remains possible that the assumption is satisfied only conditional on observable characteristics. As shown in the summary

⁴²Table 4 reports the corresponding estimates for men, also with no evidence of any effects driven by transitions across firm-size categories.

statistics (Table 1), the treatment and control groups differ systematically in several covariates such as education and residential location, raising the possibility that residual differences in outcome dynamics may persist after linear adjustment in the baseline specification.

To address this concern, following [Sant'Anna and Zhao \(2020\)](#), we first employ a doubly robust Difference-in-Differences (DRDID) estimator. This approach allows for consistent estimation of the average treatment effect on the treated (ATT) under a conditional parallel trend assumption, provided that either the outcome regression model or the propensity score model is correctly specified. The set of covariates used in the DRDID estimation is identical to that used in the baseline OLS specification, ensuring comparability across methods.

Table 5 reports the DRDID estimation results, which are quantitatively very similar to those obtained from the baseline OLS specification. In particular, we continue to find statistically significant positive effects of the reform on the uptake of leave and on mothers' post-childbirth employment outcomes. For the planned number of children, the point estimates are somewhat larger than in the baseline specification, but these estimates are not statistically significant and are imprecisely estimated. This suggests limited statistical power and no robust evidence of effects on fertility-related outcomes.

We further examine robustness to treatment timing and dynamic treatment effects by applying the estimator proposed by [Callaway and Sant'Anna \(2021\)](#), which explicitly accounts for staggered treatment adoption in multi-period DD settings (CSDID). This approach is particularly relevant in our context, as the analysis sample spans five years before and after the 2005 policy reform.

As shown in Table A1, the number of observations within each conception-year cell is relatively small, which limits the statistical precision in cohort-specific comparisons. Accordingly, to mitigate excessive loss of effective sample size, we exclude residential area and city size dummies from the covariate set in the CSDID specification. We report average post-treatment ATT estimates.

The CSDID results, also reported in Table 5, exhibit substantially larger standard errors and do not yield statistically significant estimates. Nevertheless, for key labor market outcomes—particularly mothers' post-childbirth employment and regular employment—the point estimates remain broadly consistent in sign and magnitude with those obtained from both the baseline OLS and DRDID specifications.

Taken together, these robustness checks indicate that the main findings regarding mothers' uptake of family-friendly programs and post-childbirth employment are not driven by functional form assumptions, violations of conditional parallel trends, or artifacts of treatment timing.

8 Why the Policy Was Effective: Mechanism

8.1 Hypotheses

This section examines why the 2005 reform increased mothers' uptake of family-friendly (FF) programs. As discussed in Section 2.1, statutory family-related programs were already widely available prior to the reform, yet their use remained limited. For example, only 26.9% of eligible mothers took parental leave. This gap between formal availability and actual take-up suggests the presence of informal barriers, such as uncertainty about workplace norms or concerns about career penalties.

However, the 2005 reform required firms to formulate action plans and submit notifications, which seems to have led to their implementation in practice, as shown in 2.3. Although we do not observe firm-level implementation directly, this requirement likely induced changes in workplace practices. In large Japanese firms, new personnel systems are typically communicated company-wide, raising employee awareness and signaling managerial support. Such communication might have reduced concerns about career consequences while also shifting workplace norms toward greater acceptance of leave, reduced hours and other family-friendly benefits.

In sum, the 2005 reform operated through firm-mediated channels: the creation and implementation of action plans introducing new company-specific programs assisted employees in exercising workers' rights such as maternity and parental leave. Further, while we cannot directly observe changes in perceived penalties, it is plausible that the formulation, official submission and workplace communication of these plans reduced employees' concerns that they would suffer disadvantages if they used the programs. In addition, the introduction of these programs may have also contributed to a more accommodating workplace culture that made it easier to take leave. Taken together, these employer actions appear to have increased the likelihood that women continued working after childbirth. In the analysis below, we do not

directly identify these mechanisms; rather, we examine whether the heterogeneity in treatment effects is consistent with this interpretation.

8.2 Testing the Hypothesis: Heterogeneous Effects by Educational Attainment

To test the validity of this hypothesis, we examine whether the effects are larger for those less informed about the HR systems, which would make uptake more difficult, or more likely to be influenced by workplace culture because they are in a relatively vulnerable position in the labor market. We examine whether the policy effect differed by level of education, with the lower education group including women with less than a university degree⁴³ and the higher education group those women who graduated from university or graduate school. This classification was chosen because, since the mid-1990s in Japan, the enrollment rate of women in junior colleges has decreased while the rate for university has increased.⁴⁴

Table 6 presents the pre-policy program uptake by the treatment group, with the results estimated using Eq. (6) for women with lower and higher levels of education. The point estimates are the ATT and, comparing pre-policy program uptake by the treatment group, the average uptake was lower for lower-educated women. For instance, the average uptake of parental leave for lower- and higher-educated women was 45.5% and 53.2%, a difference of 7.7 pp. However, among highly educated women, the policy had no effect on either program participation or employment. In contrast, the policy significantly increased the uptake of family-friendly programs by women with lower education as well as significantly raising their probability of employment after childbirth. Although the differences in the estimated effects between lower- and higher-educated women are not statistically significant (Column (5)), this is likely due to limited sample size when the sample is split by educational attainment. Notably, across outcomes, the point estimates are consistently larger for women without a university degree, suggesting that the policy effects are concentrated among lower-educated women. While alternative explanations, such as ceiling effects among highly educated women, cannot be fully ruled out, pre-policy uptake remains far from universal even for this group, suggesting that differential scope for improvement alone is unlikely to account for the observed heterogeneity.

⁴³This includes women whose highest level of education was junior high school, high school, vocational school/technical college, or two-year junior college.

⁴⁴In 2005, for example, 36.8% of female high school graduates went to university while only 13.0% entered a two-year college (*School Basic Survey*, Ministry of Education, Culture, Sports, Science and Technology).

In sum, the findings provide suggestive support for the view that the reform may have alleviated employees' concerns about potential disadvantages associated with parental leave and fostered a more supportive workplace culture conducive to the uptake of family-friendly benefits.

9 Complementarity with Access to Childcare Services

This study has thus far demonstrated that the 2005 policy, which encouraged companies to create a work environment in which family-friendly programs were easily accessible, was effective in promoting the uptake of parental leave and continued employment after childbirth. Needless to say, support for mothers provided solely by companies is insufficient, so this section further investigates how the availability of external childcare support such as public childcare support and co-residence with grandparents complemented and enhanced the effectiveness of the 2005 policy.

A related study by [Kleven et al. \(2019\)](#) reveals that in Denmark, where paid parental leave and public childcare services are well developed, a significant "child penalty," or decline in women's earnings following childbirth, still persists. This finding suggests that the mere presence of supportive policies is insufficient; rather, it highlights the critical importance of creating an environment in which such policies can function effectively.

Building on this perspective, this section focuses not on the presence or absence of policies but on whether those policies are embedded in an environment that makes them truly accessible. Specifically, we examine whether the regional availability of formal public childcare services or presence of informal external childcare support such as co-residence or close proximity to a grandmother complemented the effects of the 2005 policy.⁴⁵ It is conceivable that, upon becoming aware of their pregnancy, some women might have moved to areas where public childcare services were more accessible or, alternatively, chose to live with their parents, but for our purposes it is reasonable to assume that even if this was prominent, it occurred not only among women working at the treated companies.

Regarding formal childcare support, from the NFS, we can identify the prefecture where the

⁴⁵Prior research on Japan has also indicated that better access to external childcare support, including co-residence with grandparents and nearby public childcare facilities, positively influences women's labor force participation ([Sasaki, 2002](#)).

mother resided until her first child turned three years old, so we classify the prefectures into those with high and low childcare capacity per child.⁴⁶ Panel A of Table 7 shows the estimation results for areas with high and low availability of public childcare services. Owing to data availability, the sample size for this analysis is smaller than in the previous estimations.⁴⁷

In areas with greater availability of public childcare services, treated mothers were more likely than control mothers to increase their uptake of parental leave. The policy also increased their likelihood of post-childbirth employment (not statistically significant) and employment as a regular worker (statistically significant). In other words, access to public childcare services facilitated the return to work after parental leave. Because public childcare, unlike informal childcare, provides extended-hour coverage, this likely explains why there was no increase in the use of reduced-hour arrangements.

By contrast, in areas with lower availability of public childcare, the policy had no effect on uptake of parental leave or post-childbirth employment. Instead, it statistically significantly increased the use of reduced working hours, suggesting that where public childcare is limited, the main benefit of the reform was not leave-taking at childbirth but facilitating the return to work later.

We next examine informal childcare. The NFS asks whether the couple lived with either mother until their first child turned three. We define "living nearby" as either living together or within the same municipality, and "living apart" otherwise, and examine the heterogeneity of effects based on proximity to grandmothers. As shown in Panel B of Table 7, among women living with or near their mothers, the policy increased uptake of maternity and parental leave and significantly raised post-childbirth employment outcomes. However, for mothers living apart from grandmothers, the policy had no effect.

These patterns suggest that access to grandmothers impacts policy effectiveness. Women with nearby mothers were more likely to take parental leave after the reform, likely because they had greater certainty about receiving informal childcare assistance after returning to work. However, given the limits of full-day informal care from an aging grandmother, these mothers likely combined informal care at home with reduced-hour arrangements at work to remain in

⁴⁶The number of public childcare slots per child was calculated for each prefecture as of 2005, and then prefectures with a value above the national median were categorized as "high," while those below the median were categorized as "low" (source: Ministry of Health, Labour and Welfare, 2016 *Summary of the Situation Related to Daycare Centers*).

⁴⁷In the 2021 NFS survey, prefecture-level information was no longer collected; therefore, the 2021 survey wave is excluded from this analysis, resulting in a smaller sample size.

the labor force.

Overall, the 2005 policy appears complementary to external childcare support such as formal public childcare or informal assistance from grandmothers. Although equality of coefficients across subgroups (Column (3)) cannot be rejected,⁴⁸ the consistently larger point estimates suggest complementarity. Access to non-workplace childcare support thus may have amplified the effects of the 2005 policy, particularly on regular employment after first childbirth (0.113 in Table 3 vs. 0.132 and 0.147 in Table 7). While the analysis timeframe is limited to a few years after the first birth, increased regular employment offers long-term career development and thus the potential for longer-term reductions in gender disparities in the labor market.

10 Conclusion

This paper examines whether employer-targeted measures can enhance the effectiveness of social policy. We study the 2005 *Act on the Advancement of Measures to Support Raising Next-Generation Children* in Japan, which required firms with 301 or more employees to develop action plans supporting work-childcare balance. Using repeated cross-sectional data from the National Fertility Survey and a difference-in-differences design that exploits variation in firm-size eligibility and the timing of pregnancy recognition, we estimate the causal effects of the reform on leave utilization, maternal employment, and fertility outcomes.

We find that the policy increased the uptake of maternity and parental leave, while reducing the working hours of mothers following the birth of their first child. The reform also increased the probability that mothers remain employed and continue working as regular employees after childbirth, indicating stronger maternal labor market attachment. However, we do not find robust evidence that the policy affected fertility, as measured by a woman's expected lifetime number of children. The effects are stronger for women with greater access to external childcare support, suggesting complementarities between workplace policies and broader childcare resources in the community.

These results provide causal evidence that firms play an important intermediary role in the implementation of government policy. By requiring firms to develop and communicate workplace support measures, the reform appears to have reduced informational and work-

⁴⁸The lack of statistical significance in the difference across groups likely reflects limited statistical power due to the smaller subsample sizes in the heterogeneity analysis.

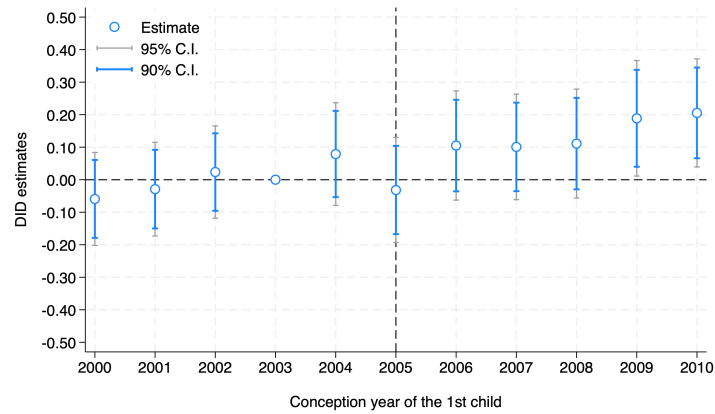
place cultural barriers to using family-friendly programs. More broadly, the results suggest that the effectiveness of social policy depends not only on its formal design but also on how firms mediate policy implementation within the workplace. A robust understanding of the mechanisms through which firms mediate public policy thus represents an important direction for future research on labor markets, workplace institutions and the effectiveness of social policy.

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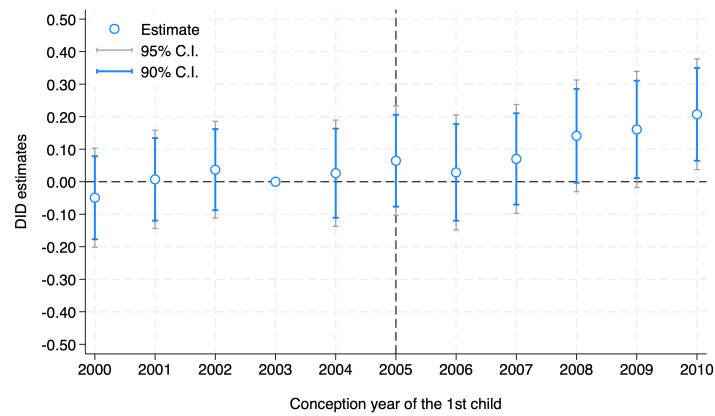
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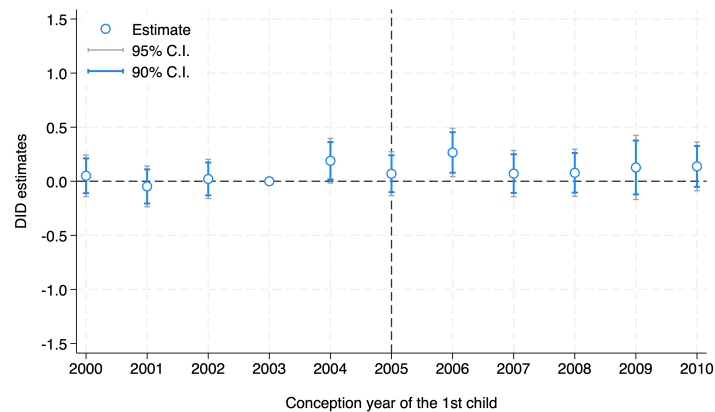
Figure 1: Results of Dynamic DD (Women)



(A) Parental Leave



(B) Probability of Working One Year after First Childbirth

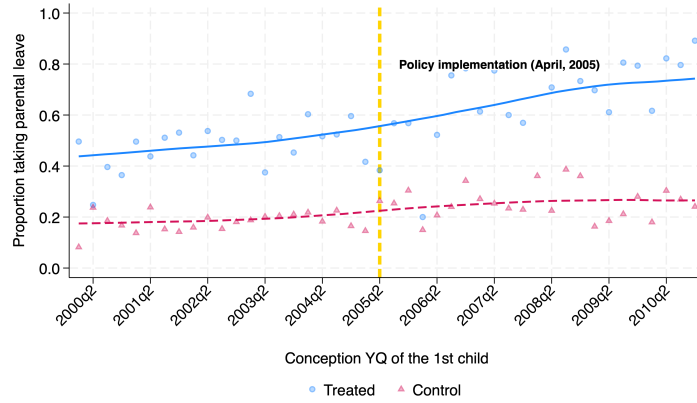


(C) Number of Planned Children

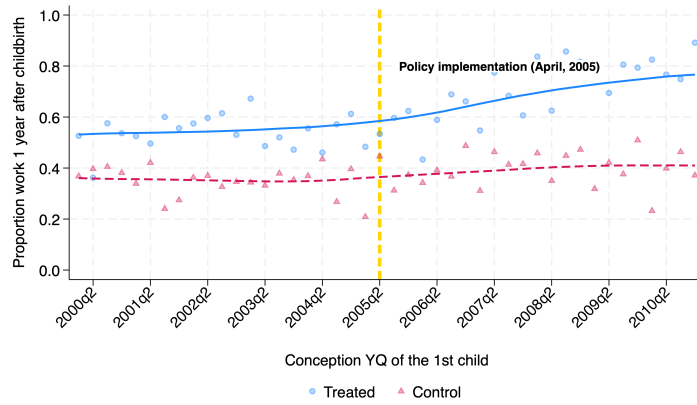
Notes. The x-axis represents the fiscal year when women became aware of their pregnancy with their first child, and FY2005 is the timing of the policy introduction. The reference period is FY2003 rather than FY2004 because the policy was announced in July 2003. Circles indicate the point estimates of the difference between the treatment and control groups (in percentage points), and vertical bars indicate 95% CI.

Sources. The National Fertility Survey 2005, 2010, 2015, and 2021.

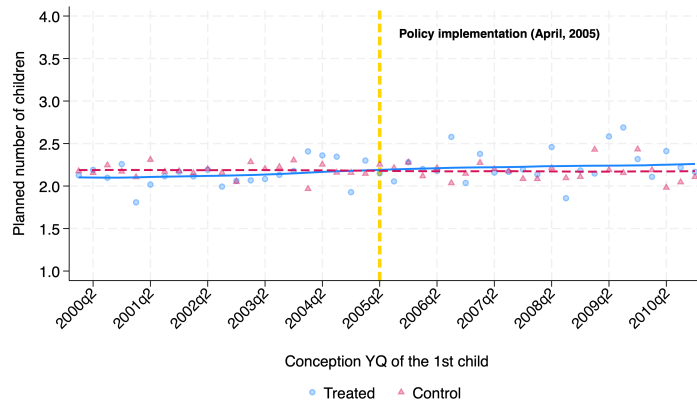
Figure 2: Sample Mean of Each Outcome



(A) Parental Leave



(B) Employment one year after childbirth



(C) The Number of Planned Children

The x-axis represents the quarter when women became aware of their pregnancy with their first child, and the vertical yellow line represents the quarter of the policy introduction (the second quarter of 2005). The blue line and dots indicate the treatment group, and the red ones indicate the control group.

Sources. The National Fertility Survey 2005, 2010, 2015, and 2021.

Table 1: Summary Statistics

Treatment status	$D = 1$	$D = 0$	
	Mean[SD]	Mean[SD]	Diffs(SE)
Analysis for mothers outcomes	(1)	(2)	(3)
Age of mothers	29.43 [4.03]	28.89 [4.31]	0.55 (0.16)
Age of fathers	31.08 [4.83]	30.57 [5.18]	0.51 (0.19)
Mother's Education: College+	0.30 [0.46]	0.19 [0.39]	0.11 (0.02)
Father's Education: College+	0.49 [0.50]	0.35 [0.48]	0.15 (0.02)
Urban and surrounding areas	0.74 [0.44]	0.65 [0.48]	0.09 (0.02)
Survey years	2014.08 [5.22]	2013.94 [5.07]	0.14 (0.19)
Observations	973	2487	3460
Treatment status	$D = 1$	$D = 0$	
	Mean[SD]	Mean[SD]	Diffs(SE)
Analysis for fathers outcomes	(4)	(5)	(6)
Age of mothers	29.82 [4.09]	29.18 [4.42]	0.64 (0.14)
Age of fathers	31.57 [4.91]	30.92 [5.24]	0.65 (0.16)
Mother's Education: College+	0.30 [0.46]	0.17 [0.38]	0.13 (0.01)
Father's Education: College+	0.57 [0.50]	0.31 [0.46]	0.26 (0.02)
Urban and surrounding areas	0.76 [0.43]	0.66 [0.48]	0.10 (0.01)
Survey years	2015.16 [4.33]	2014.98 [4.21]	0.17 (0.14)
Observations	1871	2104	3975

Notes. $D = 1$ is the treatment group (firms with 300 or more employees) and $D = 0$ is the control group (firms with 299 or fewer employees). Column (3) is the difference between Columns (1) and (2).
Source. The National Fertility Survey 2005, 2010, 2015, and 2021.

Table 2: Sample Mean of Main Outcomes

Women	Before	After	D	DD
	[SD]	[SD]	(SE)	(SE)
By firm size	(1)	(2)	(3)	(4)
Panel A. Uptake of family-friendly programmes				
Maternity leave				
-299 (D=0)	0.240[0.427]	0.309[0.462]	0.069(0.018)	0.105(0.035)
300- (D=1)	0.533[0.499]	0.707[0.456]	0.174(0.032)	
Parental leave				
-299 (D=0)	0.185[0.388]	0.263[0.441]	0.079(0.017)	0.124(0.034)
300- (D=1)	0.470[0.500]	0.673[0.470]	0.202(0.032)	
Reduced work hours				
-299 (D=0)	0.036[0.187]	0.074[0.261]	0.037(0.009)	0.099(0.022)
300- (D=1)	0.162[0.369]	0.298[0.458]	0.136(0.027)	
Panel B. Employment status one year after the birth of the first child				
Probability of working				
-299 (D=0)	0.348[0.477]	0.400[0.490]	0.052(0.020)	0.109(0.037)
300- (D=1)	0.535[0.499]	0.696[0.460]	0.162(0.032)	
Probability of working as regular employees				
-299 (D=0)	0.207[0.405]	0.245[0.430]	0.038(0.017)	0.124(0.034)
300- (D=1)	0.492[0.500]	0.654[0.476]	0.162(0.032)	
Panel C. Having additional children				
Number of planned children				
-299 (D=0)	2.180[0.694]	2.175[0.740]	-0.005(0.029)	0.105(0.054)
300- (D=1)	2.130[0.665]	2.230[0.687]	0.100(0.044)	
Ideal number of children				
-299 (D=0)	2.475[0.683]	2.440[0.702]	-0.035(0.028)	0.023(0.052)
300- (D=1)	2.446[0.662]	2.434[0.606]	-0.012(0.042)	
Men				
	Before	After	D	DD
	[SD]	[SD]	(SE)	(SE)
By firm size	(5)	(6)	(7)	(8)
Panel A. Uptake of family-friendly programmes				
Parental leave				
-299 (D=0)	0.003[0.051]	0.003[0.056]	0.001(0.003)	0.010(0.006)
300- (D=1)	0.004[0.062]	0.014[0.119]	0.011(0.006)	
Reduced work hours				
-299 (D=0)	0.001[0.036]	0.000[0.000]	-0.001(0.001)	0.001(0.003)
300- (D=1)	0.002[0.044]	0.002[0.045]	0.000(0.003)	

Notes. $D = 1$ indicates the treatment group, and $D = 0$ indicates the control group. "After" indicates women who became aware that they were pregnant with their first child after April 2005 (between FY2005 and FY2010), and "before" indicates women who became aware before March 2005 (between FY2000 and FY2004). Column (3) is the difference between Columns (1) and (2), and Column (4) is the difference-in-differences.

Source. The National Fertility Survey (women: 2005, 2010, 2015, and 2021; men: 2010, 2015, and 2021).

Table 3: Estimation Results of 2005 Policy Effects

Workers in control group	Women			Men		
	All (1)	Regular (2)	(pre-mean) (3)	All (4)	Regular (5)	(pre-mean) (6)
Panel A. Uptake of family-friendly programmes						
Maternity leave	0.095 (0.036)	0.071 (0.041)	0.536	-	-	-
Parental leave	0.116 (0.036)	0.089 (0.041)	0.475	0.003 (0.004)	0.003 (0.004)	0.005
Reduced work hours	0.089 (0.029)	0.070 (0.031)	0.164	0.001 (0.002)	0.001 (0.002)	0.002
Observations	3460	2347		3975	3813	
Panel B. Employment status one year after the birth of the first child						
Probability of working	0.100 (0.037)	0.089 (0.041)	0.540	-	-	-
Probability of working as regular employees	0.113 (0.036)	0.087 (0.041)	0.498	-	-	-
Panel C. Having additional children						
Number of planned children	0.069 (0.050)	0.033 (0.055)	2.129	-	-	-
Ideal number of children	0.007 (0.050)	-0.017 (0.054)	2.447	-	-	-
Observations	3460	2347		-	-	

Notes. This table reports the point estimate of $\hat{\beta}_1$ using Eq. (6) for each outcome variable. The mother's and father's age at birth and education, residential area dummies, residential city size dummies, and survey year dummies are controlled. Control groups in (1) and (3) are all employees (both regular and non-regular employees) in companies with 299 or fewer, and those in (2) and (5) are regular employees in companies with 299 or fewer employees. The means in (3) and (6) are calculated for the treatment group in the pre-policy period. Robust standard errors are in parentheses.

Source. The National Fertility Survey (women: 2005, 2010, 2015, and 2021; men: 2010, 2015, and 2021).

Table 4: Check of Whether Transfers to Treated Firms Influenced the Main Results

Workers in control group	Women			Men		
	All (1)	Regular (2)	(pre-mean) (3)	All (4)	Regular (5)	(pre-mean) (6)
Panel A. Uptake of family-friendly programmes						
Maternity leave	0.098 (0.041)	0.066 (0.046)	0.547	-	-	-
Parental leave	0.138 (0.040)	0.104 (0.046)	0.473	-0.007 (0.005)	-0.004 (0.005)	0.002
Reduced work hours	0.108 (0.033)	0.092 (0.035)	0.178	-0.001 (0.006)	-0.001 (0.006)	0.007
Observations	2624	1880		2349	1687	
Panel B. Employment status one year after the birth of the first child						
Probability of working	0.120 (0.042)	0.097 (0.046)	0.543	-	-	-
Probability of working as regular employees	0.134 (0.041)	0.098 (0.046)	0.499	-	-	-
Panel C. Having additional children						
Number of planned children	0.017 (0.056)	-0.031 (0.061)	2.140	-	-	-
Ideal number of children	-0.025 (0.056)	-0.046 (0.060)	2.450	-	-	-
Observations	2624	1880		-	-	

Notes: $D = 1$ is the treatment group (firms with 300 or more employees) and $D = 0$ is the control group (299 or fewer employees). “After” indicates women who became aware that they were pregnant with their first child after April 2005, and “before” indicates women who became aware before March 2005. Robust standard errors are in parentheses. The mother’s and father’s age at birth and education, residential area dummies, residential city size dummies, and survey year dummies are controlled. The analysis sample of this table is women where the company size at the time of the first childbirth matched the company size immediately after the last school graduation.

Source. The National Fertility Survey 2005, 2010, 2015 and 2021.

Table 5: Robustness Check (Women)

Workers in control group	All			Regular		
Estimation method	OLS	DRDID	CSDID	OLS	DRDID	CSDID
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Uptake of family-friendly programmes						
Maternity leave	0.095 (0.036)	0.084 (0.038)	0.051 (0.171)	0.071 (0.041)	0.080 (0.043)	0.049 (0.170)
Parental leave	0.116 (0.036)	0.101 (0.038)	0.061 (0.158)	0.089 (0.041)	0.080 (0.043)	0.023 (0.157)
Reduced work hours	0.089 (0.029)	0.080 (0.030)	0.075 (0.151)	0.070 (0.031)	0.051 (0.034)	0.053 (0.149)
Panel B. Employment status one year after the birth of the first child						
Probability of working	0.100 (0.037)	0.087 (0.038)	0.057 (0.168)	0.089 (0.041)	0.082 (0.043)	0.076 (0.161)
Probability of working as regular employees	0.113 (0.036)	0.097 (0.038)	0.083 (0.156)	0.087 (0.041)	0.083 (0.043)	0.100 (0.157)
Panel C. Having additional children						
Number of planned children	0.069 (0.050)	0.088 (0.051)	-0.064 (0.281)	0.033 (0.055)	0.084 (0.056)	-0.067 (0.276)
Ideal number of children	0.007 (0.050)	0.013 (0.052)	-0.107 (0.173)	-0.017 (0.054)	0.011 (0.057)	-0.080 (0.139)
Observations	3460	3460	3460	2347	2347	2347

Notes: Columns (1) and (4) report the point estimates of $\hat{\beta}_1$ from Eq. (6), repeated from Table 3. Columns (2) and (5) report doubly robust difference-in-differences estimates (DRDID) following Sant'Anna and Zhao (2020). Columns (3) and (6) report average post-treatment ATT estimates from Callaway and Sant'Anna (2021) estimator (CSDID). All specifications target the average treatment effect on the treated (ATT). The DRDID and CSDID estimates were obtained using the `drdid` and `csdid` commands in Stata, as provided by Pedro H. C. Sant'Anna (see <https://psantanna.com/did-resources/>). The mother's and father's age at birth and education, residential area dummies, residential city size dummies, and survey year dummies are included in the baseline OLS and DRDID specifications. For the CSDID estimation, we omit residential area and city size dummies to avoid loss of precision due to limited effective sample sizes in year-by-year comparisons. Control groups in columns (1)–(3) include all employees (both regular and non-regular) in firms with 299 or fewer employees, while those in columns (4)–(6) restrict the control group to regular employees only. Robust standard errors are reported in parentheses.

Source. The National Fertility Survey 2005, 2010, 2015, and 2021.

Table 6: The Effects of the Policy on Employment by Mothers' Education

	High-educated		Low-educated		Diff.
	(1)	pre-mean	(2)	pre-mean	(3)
Panel A. Uptake of family-friendly programmes					
Maternity leave	0.029 (0.074)	0.589	0.145 (0.043)	0.519	-0.115 (0.084)
Parental leave	0.069 (0.074)	0.532	0.153 (0.043)	0.455	-0.085 (0.084)
Reduced work hours	0.041 (0.059)	0.255	0.099 (0.034)	0.137	-0.059 (0.066)
Panel B. Employment status one year after the birth of the first child					
Probability of working	0.103 (0.074)	0.582	0.124 (0.044)	0.525	-0.021 (0.085)
Probability of working as regular employees	0.086 (0.073)	0.532	0.145 (0.043)	0.486	-0.059 (0.083)
Panel C. Having additional children					
Number of planned children	-0.016 (0.097)	2.092	0.115 (0.060)	2.139	-0.131 (0.112)
Ideal number of children	-0.128 (0.100)	2.482	0.055 (0.059)	2.433	-0.183 (0.114)
Observations	769		2691		

Notes. This table reports the point estimates of $\hat{\beta}_1$ using Eq. (6) for each outcome variable. The mother's and father's age at birth and education, residential area dummies, residential city size dummies, and survey year dummies are controlled. The means are calculated for the treatment group in the pre-policy period. Robust standard errors are in parentheses. "Yes" indicates university-educated women, and "no" indicates women with other educational backgrounds.

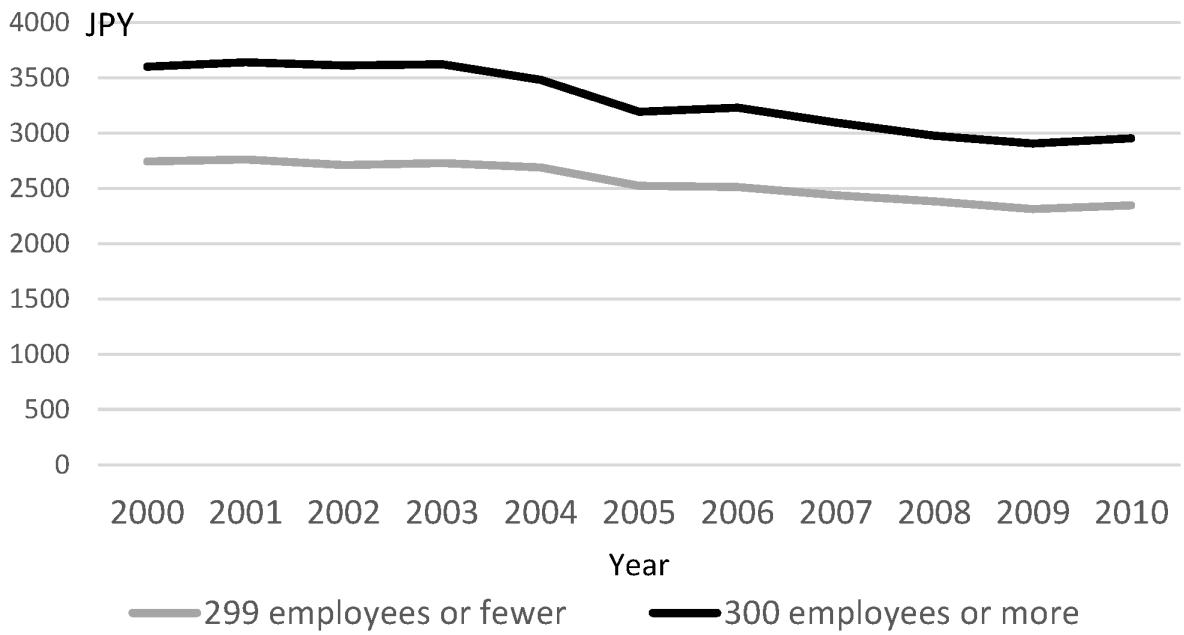
Source. The National Fertility Survey 2005, 2010, 2015, and 2021.

Table 7: The Effects of the Policy on Employment by Access to Childcare Services

Panel A. Regional Availability of Public Childcare Services (Formal Childcare Services)					
Childcare availability	(1)	High pre-mean	(2)	Low pre-mean	Diff. (3)
Panel A. Uptake of family-friendly programmes					
Maternity leave	0.080 (0.063)	0.557	0.100 (0.065)	0.503	-0.020 (0.089)
Parental leave	0.150 (0.062)	0.489	0.064 (0.066)	0.460	0.087 (0.089)
Reduced work hours	0.030 (0.050)	0.190	0.119 (0.055)	0.182	-0.089 (0.073)
Panel B. Employment status one year after the birth of the first child					
Probability of working	0.111 (0.066)	0.517	0.099 (0.070)	0.529	0.012 (0.094)
Probability of working as regular employees	0.132 (0.063)	0.489	0.076 (0.067)	0.481	0.056 (0.091)
Panel C. Having additional children					
Number of planned children	0.031 (0.087)	2.184	0.000 (0.087)	2.107	0.031 (0.121)
Ideal number of children	-0.037 (0.087)	2.541	0.034 (0.092)	2.390	-0.070 (0.125)
Observations	1215		990		
Panel B. Co-residence or Close Proximity to a Grandmother (Informal Childcare Services)					
Living close to grandmothers	(1)	Yes pre-mean	(2)	No pre-mean	Diff. (3)
Panel A. Uptake of family-friendly programmes					
Maternity leave	0.138 (0.047)	0.540	0.029 (0.056)	0.531	0.109 (0.072)
Parental leave	0.172 (0.047)	0.471	0.031 (0.055)	0.481	0.142 (0.072)
Reduced work hours	0.091 (0.037)	0.129	0.063 (0.046)	0.216	0.028 (0.058)
Panel B. Employment status one year after the birth of the first child					
Probability of working	0.150 (0.049)	0.540	0.018 (0.057)	0.539	0.131 (0.074)
Probability of working as regular employees	0.147 (0.048)	0.490	0.058 (0.055)	0.510	0.089 (0.072)
Panel C. Having additional children					
Number of planned children	0.094 (0.069)	2.099	0.033 (0.075)	2.174	0.061 (0.101)
Ideal number of children	0.031 (0.067)	2.447	-0.011 (0.078)	2.446	0.042 (0.101)
Observations	2182		1278		

Notes: This table reports the point estimates of $\hat{\beta}_1$ using Eq. (6) for each outcome variable. The mother's and father's age at birth and education, residential area dummies, residential city size dummies, and survey year dummies are controlled. The means are calculated for the treatment group in the pre-policy period. Robust standard errors are in parentheses. For Panel A, the number of public childcare slots per child was calculated for each prefecture in 2005, and then prefectures with a value above the national median were categorized as "high," while those below the median were categorized as "low." Regarding Panel B, "Yes" indicates living together with a woman's parents/in-laws or living close to them within the same municipality, otherwise "no." Source: The National Fertility Survey 2005, 2010, 2015, and 2021.

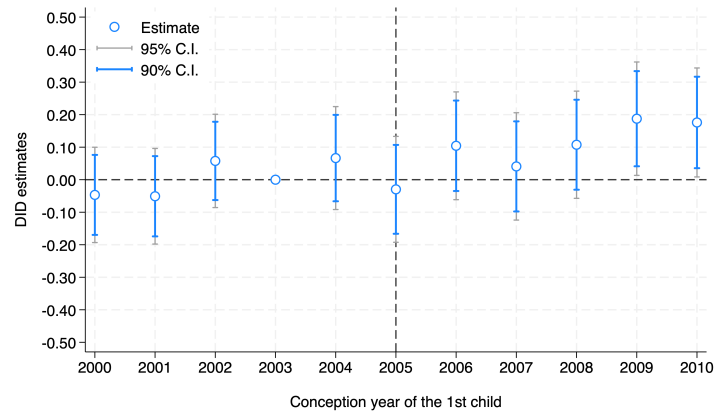
Figure A1: Real Wage by Firm Size (2000–2010)



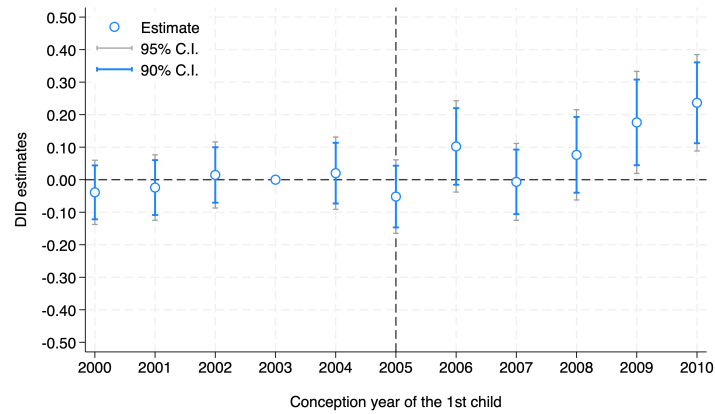
Notes: The Y axis indicates Japanese yen. Real wages are calculated by dividing nominal wages by the CPI (Consumer Price Index).

Sources: The Basic Survey of Wage Structure (Ministry of Health, Labour, and Welfare), Consumer Price Index based on the year 2020 (The Cabinet Office).

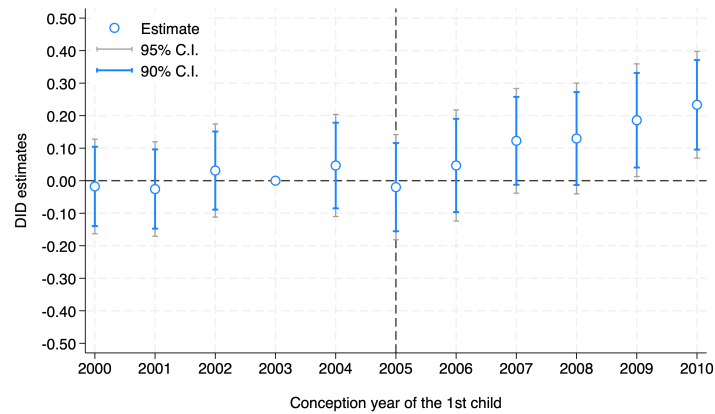
Figure A2: Results of Dynamic DD for Other Outcomes (Women)



(A) Maternity Leave



(B) Reduced Work Hours

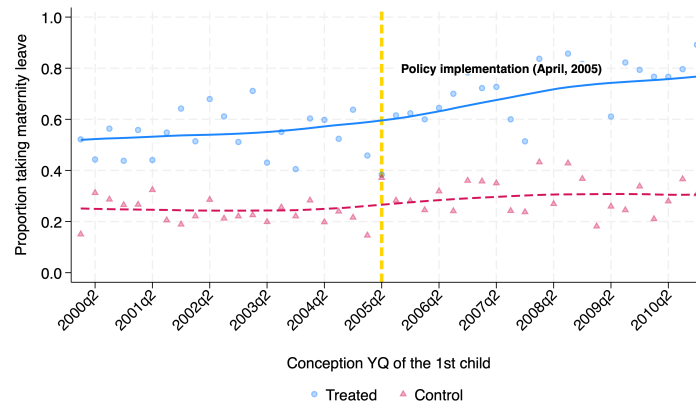


(C) Regular Employment One Year After Childbirth

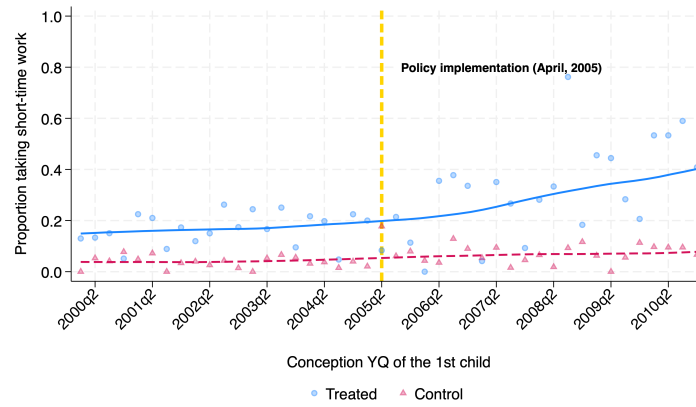
Notes. The x-axis represents the fiscal year when women became aware of their pregnancy with their first child, and FY2005 is the timing of the policy introduction. The reference period is FY2003 rather than FY2004 because the policy was announced in July 2003. Circles indicate the point estimates of the difference between the treatment and control groups (in percentage points), and vertical bars indicate 95% CI.

Sources. The National Fertility Survey 2005, 2010, 2015, and 2021.

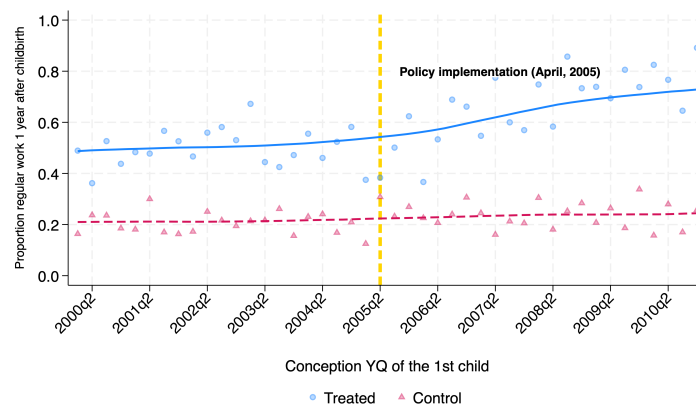
Figure A3: Sample Mean (Women)



(A) Maternity Leave



(B) Reduced Work hours



(C) Regular Employment One Year After Childbirth

Notes: The x-axis represents the quarter when women became aware of their pregnancy with their first child, and the vertical yellow line represents the quarter of the policy introduction (the second quarter of 2005). The blue line and dots indicate the treatment group, and the red ones indicate the control group.
Sources: The National Fertility Survey 2005, 2010, 2015, and 2021.

Table A1: Sample Size by Fiscal Year of First Pregnancy Recognition and Survey Year

Women FY	Total	All		Survey year							
		Treated	Control	2005		2010		2015		2021	
				Treated	Control	Treated	Control	Treated	Control	Treated	Control
Total	3,460	973	2,487	109	265	276	730	325	881	263	611
2000	427	121	306	28	76	40	88	26	95	27	47
2001	421	121	300	27	71	38	99	39	86	17	44
2002	424	132	292	31	71	43	95	29	68	29	58
2003	367	108	259	23	43	31	74	25	91	29	51
2004	312	88	224	0	4	34	88	33	84	21	48
2005	288	80	208	-	-	24	74	31	77	25	57
2006	277	64	213	-	-	23	78	27	75	14	60
2007	311	73	238	-	-	24	88	28	88	21	62
2008	237	65	172	-	-	19	46	24	73	22	53
2009	194	52	142	-	-	0	0	26	77	26	65
2010	202	69	133	-	-	-	-	37	67	32	66

Men FY	Total	All		Survey year							
		Treated	Control	2005		2010		2015		2021	
				Treated	Control	Treated	Control	Treated	Control	Treated	Control
Total	3,975	1,871	2,104	-	-	606	680	711	863	554	561
2000	427	203	224	-	-	75	82	69	96	59	46
2001	413	196	217	-	-	81	90	73	84	42	43
2002	419	210	209	-	-	92	96	63	70	55	43
2003	408	191	217	-	-	72	81	70	82	49	54
2004	387	162	225	-	-	57	93	67	86	38	46
2005	392	195	197	-	-	75	65	55	77	65	55
2006	362	159	203	-	-	59	66	67	82	33	55
2007	389	183	206	-	-	63	72	63	88	57	46
2008	286	148	138	-	-	32	35	63	62	53	41
2009	245	101	144	-	-	0	0	60	75	41	69
2010	247	123	124	-	-	-	-	61	61	62	63

Notes: This table shows which survey source year for the analysis sample. FY indicates the fiscal year when women noticed their first pregnancy.

Sources: The National Fertility Survey (women: 2005, 2010, 2015, and 2021; men: 2010, 2015, and 2021).