

Mandated Paternity Leave and Fertility: Evidence from South Korea*

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Abstract

Low fertility is a pressing concern in many advanced economies, particularly those where gender gaps in work and caregiving persist. Recent research highlights that fertility increasingly depends on whether men can credibly share childcare responsibilities. This paper examines whether mandating paternity leave can raise fertility by institutionalizing fathers' caregiving. We study a 2017 company-wide mandate at a large South Korean conglomerate requiring all male employees to take one month of fully paid paternity leave. Using newly linked administrative data and an event-study design, we find that the mandate sharply increased leave uptake, lengthened leave durations, and generated spillovers among fathers not directly subject to the policy. The probability of having a child rose by about 15 percent, with the largest effects among dual-earner couples and those with higher-earning, longer-tenured wives. Wives' employment remained stable, indicating that fertility gains did not come at the cost of women's careers. Complementary survey evidence shows more supportive workplace norms toward fathers' leave-taking and greater paternal involvement in childcare at treated firms. These findings demonstrate that mandating short paternal leave can normalize fathers' caregiving and promote fertility without undermining women's employment.

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1 Introduction

Low fertility has become a defining demographic and policy challenge (Jones, 2022). The decline has coincided with women’s rapid gains in education and employment, making the ability to combine career and family a central determinant of fertility decisions (Feyrer et al., 2008; Goldin, 2021). Recent work argues that the main driver of fertility decline lies in the lagged adaptation of social institutions that support shared caregiving relative to women’s rapid labor-market progress (Doepke et al., 2023; Goldin, 2025a). A growing discussion highlights the role of fathers: men’s credible commitment to being dependable caregivers is now central to fertility decisions, underscoring the importance of policies that can make this commitment both credible and feasible (Doepke and Kindermann, 2019; Goldin, 2025b).

Cross-country evidence supports this view: countries where men spend more time on domestic work tend to exhibit both higher fertility and higher female labor force participation (Figure A.1). Reflecting this pattern, many governments have turned toward paternity leave policies as a means of strengthening fathers’ credible commitment to caregiving and thereby promoting higher fertility.¹ Nearly all OECD countries now offer father-specific paid leave entitlements.²

Yet whether such policies actually raise fertility remains unclear. Existing studies typically focus on fathers who already have a child, leaving the transition into parenthood—the margin most relevant for fertility dynamics—largely unexplored (Kotsadam and Finseraas, 2011; Cools et al., 2015; Farré and González, 2019; Canaan et al., 2022). Moreover, leave entitlements may have limited effects unless they succeed in shifting workplace and social norms that discourage fathers’ childcare participation. Even in countries with paid leave, many fathers abstain due to workplace norms or fear of career penalties (Dahl et al., 2014; Bartel et al., 2018; Patnaik, 2019; Kim and Lundqvist, 2023). In Korea, men cite career concerns and discouragement from supervisors as reasons for not taking leave, even when they wish to.³ This underscores that meaningful change may require stronger policy levers to shift workplace norms and caregiving practices.

¹Sociology literature finds that paternal involvement in childcare and housework is linked to higher second-birth rates, largely by easing work–family conflict and supporting maternal employment. See Cooke (2004); Kotila and Kamp Dush (2011); Fanelli and Profeta (2021); Lappegård and Kornstad (2020).

²As of 2023, 35 of 38 OECD countries offer paid, father-specific leave, with an average duration of 12.7 weeks.

³Presidential Committee on Aging Society and Population Policy (2024), Survey on Perceptions of Marriage, Fertility, and Childcare. <https://www.betterfuture.go.kr/front/notificationSpace/pressReleaseDetail.do?articleId=303>

This paper provides the first causal evidence on whether a paternity-leave mandate—a strong, binding intervention—can increase fertility. We study a unique natural experiment in which a large South Korean conglomerate (hereafter *C Group*) mandated one month of fully paid paternity leave for all male employees with children born after January 2017. The decision, made at the holding-company level, applied uniformly across 44 subsidiaries spanning nine industries and was exogenous to employees’ prior job choices. This quasi-random policy change allows estimation of effects on both first and higher-order births, as the mandate did not hinge on the timing of an existing child’s birth, and enables analysis of anticipatory responses driven by expectations of more generous leave benefits.

Building on this setting, the paper addresses three questions: (1) Does a mandate increase leave uptake and lead to more active utilization of paternity leave? (2) Does mandating paternal leave-taking lead to higher fertility? (3) If so, through which channels does the mandate affect fertility?

To answer these questions, we draw on a novel set of administrative datasets from Statistics Korea released in 2021—including the Child Registry, Population Census, Parental Leave Registry, Matched Employer–Employee Data, and Business Registry—to track both employees’ policy exposure and fertility outcomes. To complement the administrative analysis, we also field an original survey of male employees at treated and control firms, capturing child-care time, workplace culture, and fertility intentions.

Using these datasets, we first document that the mandate substantially increased paternity-leave uptake, providing evidence that it was binding and consistently enforced across subsidiaries. Prior to the policy, leave take-up within one year of childbirth remained low—2% among fathers at treated firms and 1.2% at control firms. Following the mandate, uptake among fathers at treated firms increased by 64.5 percentage points, more than 30 times the pre-policy level.⁴ While leave-taking at control firms also rose modestly—by 1.8 percentage points in the post-policy period—the increase was substantially smaller.

The mandate’s effects extended beyond its immediate requirements. Among treated fathers, the share taking more than one month of leave rose by 3.9 p.p. relative to the pre-policy baseline of 1.9%, three times the share of extended leave users. This is particularly notable given that, beyond the first month, federal wage replacement rates varied between 23–70% depending on leave duration and policy year, implying that men opted into partially paid leave despite limited financial incentives. Moreover, by requiring every father to take leave,

⁴Utilization does not reach 100% after implementation, even though the policy required all fathers to take at least one month of leave. Because our data contain limited information on contract type, we cannot separate employees ineligible for corporate benefits (e.g., part-time or fixed-term workers) from those who did not comply. The remaining non-take-up thus reflects both imperfect observation and noncompliance, which we discuss in greater detail in Section 4.

the mandate may have shifted workplace norms around leave-taking. To test this, we examine spillover effects among fathers at treated firms who were not directly covered by the corporate policy but remained eligible for federal leave—specifically, those with children born in 2015–2016. Among this group, the likelihood of taking leave in the post-policy period rose by 2.4 p.p. from a pre-policy average of 0.9%, compared to only a 0.8 p.p. increase among their counterparts at control firms (baseline 0.6%). Taken together, these findings suggest that the one-month mandate not only increased uptake among eligible fathers but also contributed to broader shifts in workplace attitudes toward paternity leave.

Does mandating paternity leave, then, affect households’ fertility decisions? To investigate this question, we implement an event study design that exploits the exogenous timing of the policy. We compare male employees at the treated conglomerate to those at other large conglomerates in similar industries who were not exposed to the mandate. The sample is restricted to men who were employed before the policy was under discussion and who were between ages 30 and 45 at that time—targeting individuals in their prime working and childbearing years. This baseline cohort definition allows us to follow the same group over time, beginning when the youngest was 27, and trace how their fertility rates evolved before and after implementation. This approach ensures that changes do not reflect compositional shifts among the treated group due to men with stronger fertility intentions selectively joining firms around the policy change. Moreover, if treated firms had employed men with stronger underlying fertility preferences even before the policy discussion, their plans could have gradually materialized over time regardless of the policy. In the data, however, fertility trends in treated and control groups were nearly identical prior to the reform and diverged only afterward, supporting the parallel-trends assumption of our empirical strategy.

Using this design, we find that the mandate significantly increased the probability of having a baby among affected employees. Specifically, the probability of having an infant rose by 1.4 percentage points, a 14.7% increase relative to the pre-policy average of 9.5% among treated employees. Similar results hold when restricting the outcome to births occurring while the father remained employed at the same firm, supporting that the observed increase reflects a response to the new leave benefit. A back-of-the-envelope calculation suggests the mandate generated roughly 1,459 additional births over six years among 17,366 treated men. To put this effect in context, the national total fertility rate (TFR)⁵ during the post-policy period was about 0.94. Our estimates imply an additional 0.084 births per treated man, equivalent to roughly 8.9% of the national TFR. Relative to actual births among same-aged

⁵The total fertility rate is the average number of children a woman of childbearing age is expected to have over her lifetime, summarizing the total number of births when fertility rates are tracked across age groups over time.

Korean men during the post-policy period—about 0.2 births per man—the effect represents an increase of nearly 41%.

Notably, fertility began to rise in the second half of 2016, shortly after the internal survey on policy implementation was circulated. This suggests that households adjusted fertility in anticipation of the policy, even before it formally took effect. These early responses underscore that fertility decisions are forward-looking: households make choices based on the child-rearing environment they expect to face and the anticipated commitment from both parents.

Additionally, we find that the fertility effect operates along both the extensive and intensive margins. The probability of a first birth increased by 0.8 percentage points—a 17% increase relative to a 4.7% baseline, while the probability of an additional birth rose by 0.6 percentage points—a 13% increase relative to a 4.7% baseline. Roughly half of the overall increase is driven by first births, providing novel evidence that paternity leave mandates can influence the transition into parenthood—a margin that previous studies could not capture given their policy contexts. The remainder reflects higher-order births, indicating that the policy supported both new parents and families expanding further. We also find no evidence of changes in parental age at birth or in birth spacing that would suggest shifts in birth timing. While we cannot yet observe completed fertility, the evidence points to the mandate generating additional fertility rather than simply accelerating births.

How could a one-month leave mandate generate such a large fertility response? One potential channel is that it relaxed families’ binding time constraints—not only through the required month of leave but also by shifting expectations that fathers would be more available for childcare. When fathers cannot credibly commit time to childcare, fertility may be constrained, particularly in households where mothers’ time is limited by work demands.

To probe this interpretation, we examine heterogeneity by wives’ baseline employment status. The effects are concentrated among dual-earner households. Fertility rose most in families where wives held stable wage jobs—rather than temporary or self-employed positions—where maternal time constraints are greatest, with larger gains among those with higher earnings and longer tenure. Importantly, these fertility gains did not come at the expense of wives’ labor supply: we find modest increases in both employment rates and months worked. Taken together, these patterns suggest that the mandate helped relax time constraints within dual-earner households, allowing them to have a child without undermining women’s careers.

While these results provide indirect evidence for this mechanism, administrative data cannot directly capture the underlying shifts in expectations that may have driven it. To directly assess this mechanism, we complement the administrative analysis with new survey

evidence. The survey was designed to test whether mandating leave can normalize workplace expectations and relax barriers to paternal caregiving. A broad literature highlights that social and workplace norms discourage fathers from taking leave (Dahl et al., 2014; Bartel et al., 2018; Patnaik, 2019), creating a constraint on their involvement in childcare. In this context, a mandate can serve as a powerful normalizing device, reshaping workplace attitudes toward caregiving and, in turn, amplifying fertility effects.

We fielded an original survey in August 2025—approximately eight years after the policy change—of 450 male employees aged 30–45 from treated and control firms. The survey captures direct evidence on fathers’ childcare time, perceptions of workplace culture, and expectations about leave-taking under both mandated and non-mandated scenarios. Relative to control firms, men at treated firms are far more likely to have taken leave when eligible and report more supportive norms: they expect higher coworker uptake and encouragement, perceive fewer career penalties for leave-takers, and report less coworker burden. Treated men also report spending more time on weekday childcare and express stronger fertility intentions.

To further isolate the role of workplace norms, we conducted a within-respondent vignette experiment among control workers. When asked to respond under a hypothetical mandate, their answers shifted toward greater support for leave-taking—lower perceived penalties and coworker burdens, higher peer support, and higher expected second-birth probabilities. Taken together, the survey evidence provides direct evidence that normalizing paternal leave can ease time-related barriers to childbearing by increasing expected paternal availability and support for caregiving in dual-earner households.

Lastly, as a complement to our household analysis, we examine whether promoting more family-friendly workplaces may also align with firms’ interests. Using Business Registry and Matched Employer-Employee Data, we find no significant changes in overall employment, revenue, or productivity following the mandate. Instead, the share of male employees aged 30–45 rose at treated firms, largely due to higher retention of incumbents. We also find evidence of additional sorting among new hires: men who entered treated firms after the mandate were more likely to become fathers during their tenure, suggesting that workers with stronger fertility preferences were more likely to join firms offering the benefit. Importantly, we do not find higher earnings for incumbents or new hires, indicating that these responses were driven by the benefit itself rather than by wage offers. Taken together, these patterns indicate that family-friendly mandates may help firms retain and attract employees who value such benefits, without imposing measurable short-run costs on company performance.

Taken together, these findings point to a promising policy lever for addressing low fertility. A brief, one-month paternity-leave mandate raised fertility meaningfully, without reducing

women’s employment or imposing detectable short-run costs on firms. Compared with other interventions, such as extending maternal or general parental leave—which can slow women’s career progression and disrupt firms—or large-scale cash or subsidy programs—which are fiscally expensive—the mandate represents a low-cost, high-return alternative. Beyond its direct effects, it also appears to have fostered more supportive workplace norms around fathers’ caregiving, suggesting that even a minimal intervention can initiate meaningful social change. In doing so, this study provides the first empirical evidence for [Goldin \(2025b\)](#) argument that bridging the gap between women’s career advancement and men’s credible commitment to childcare is essential to reversing fertility decline.

This study advances recent discussions on the causes of low fertility and policy responses by providing the first causal evidence that promoting fathers’ participation in childcare can raise fertility. A growing literature has examined the sources of fertility decline across contexts—from rising educational competition in Asia ([Kim et al., 2024](#)) to the increasing opportunity costs of childbearing for highly educated women ([Goldin, 2021](#)). Recent theoretical work highlights another critical dimension: the role of fathers ([Doepke et al., 2023](#)). Fertility increasingly depends on whether men can credibly commit to shared childcare within households ([Doepke and Kindermann, 2019](#); [Goldin, 2025b](#)). When workplace cultures evolve slowly and norms around paternal caregiving lag behind women’s advances in careers and labor-market participation, such institutional frictions can constrain fertility ([Goldin, 2025a](#)).

Empirical evidence on whether father-targeted leave policies can meaningfully foster such change remains limited and mixed. Studies of “daddy quotas” find modest or inconsistent effects on paternal involvement ([Kotsadam and Finseraas, 2011](#); [Ekberg et al., 2013](#); [Tamm, 2019](#); [González and Zoabi, 2021](#)), while workplace stigma and perceived career penalties continue to deter fathers from taking leave ([Dahl et al., 2014](#); [Johnsen et al., 2024](#)). This paper demonstrates that a binding, mandatory paternity-leave policy can overcome these barriers, promoting fathers’ caregiving and leading to higher fertility. In doing so, it provides empirical support for models that emphasize time constraints, gendered norms, and the credibility of paternal commitment as key determinants of fertility ([Erosa et al., 2010](#); [Doepke and Kindermann, 2019](#); [Goldin, 2025b](#); [Kim and Yum, 2025](#)).

Second, this study contributes to the literature on fertility and parental leave by providing the first causal evidence on the effects of paternity leave on overall fertility—including both first and higher-order births. Prior studies on parental leave have primarily focused on maternity leave reforms ([Lalive and Zweimüller, 2009](#); [Dahl et al., 2016](#); [Raute, 2019](#); [Kleven et al., 2024](#)), while those examining paternity leave typically exploit discontinuities around the birth of an existing child. These designs, by construction, cannot capture the

transition into parenthood—the margin most relevant for understanding fertility dynamics (Kotsadam and Finseraas, 2011; Dahl et al., 2014; Cools et al., 2015; Bartel et al., 2018; Farré and González, 2019). Our setting enables identification of effects on both first and subsequent births and reveals anticipatory behavioral responses, a previously unobserved margin through which expectations about future leave benefits can shape fertility decisions.

Finally, this paper contributes to a growing literature on the role of social norms and beliefs in shaping fertility behavior. A consistent finding across studies is that norms exert prescriptive power: individuals conform to perceived social expectations (Bursztyn et al., 2020), update their attitudes based on perceived peers’ attitudes (Cortés et al., 2024), and retain persistent gendered beliefs shaped by prior experiences (Exley et al., 2025). Within this context, policies can serve as salient signals that redefine social expectations. For instance, Patnaik (2019) shows that even subtle framing—such as labeling leave as a “daddy quota”—can increase fathers’ uptake by altering perceived norms. Our findings show that a one-month, binding leave requirement can act as a stronger institutional signal, establishing paternal caregiving as a new workplace default and easing a key constraint on fertility decisions. In this sense, we provide empirical evidence consistent with Goldin (2025b)’s hypothesis that fostering credible paternal commitment is central to sustaining fertility amid women’s rising economic agency.

The paper proceeds as follows. Section 2 describes the institutional setting and the corporate paternity-leave mandate. Section 3 details the administrative data. Section 4 presents the mandate’s effect on leave uptake. Section 5 outlines the empirical strategy and Section 6 reports the main fertility effects and heterogeneity analysis. Section 7 provides complementary survey evidence, and Section 8 examines firm-level outcomes. Finally, Section 9 concludes.

2 Institutional Background

2.1 Fertility rate and Parental Leave Policy in Korea

Among high-income countries with low fertility rates, South Korea shows one of the sharpest declines in recent decades. Its total fertility rate (TFR)—the average number of children a woman is expected to have over her lifetime—fell to 0.72 in 2023, far below the replacement level of about 2.1 children per woman needed to maintain a stable population. Other East Asian countries also face extremely low fertility rates, with Hong Kong at 0.75, Japan at 1.30, and China at 1.18 in 2021, compared to 1.67 in the United States.

In response to demographic challenges, the Korean government has expanded parental leave policies to improve work–family compatibility. Both female and male employees with

at least six months of tenure are eligible for parental leave until their child reaches age eight, with wage replacement funded by employment insurance. South Korea offers one of the most generous paid parental leave durations among OECD countries—52 weeks—far exceeding the OECD average of 13 weeks for paid paternity leave. However, wage replacement rates have historically been low. Table 1 summarizes the federal parental leave policy between 2015 and 2022, focusing on the case of the second leave user.⁶ Initially capped at 1.5 million KRW in 2015, the monthly benefit ceiling for the second parental leave user (typically fathers) was increased to 2 million KRW in 2018 and to 2.5 million KRW in 2019 for the first three months. During 2017–2019, the average monthly post-tax earnings⁷ for male workers aged 30–39 was 2.9 million KRW, raising the replacement rate from approximately 52% to 86%. Responding to continued criticism that the benefit ceiling still fell short of fathers’ average wages—and thereby discouraged paternal uptake—the government further increased the ceiling to 3 million KRW in 2022 for the first three months of paternity leave. This change made the potential replacement rate reach 100% for the first three months of leave for the average Korean male employee.

Despite the continuous expansion of benefits, paternity leave uptake in Korea remains low. Figure A.2 shows parental leave utilization rates by gender for parents of newborns in Korea between 2010 and 2023. In 2017, only 1.8% of eligible fathers used the benefit, compared to 62.2% of mothers. By 2023, fathers’ uptake rose modestly to 7.4%, compared to 73.2% for mothers.

2.2 Mandatory Corporate Paternity Leave Policy

In line with government efforts, a major Korean conglomerate—“C Group”—implemented a mandatory one-month paternity leave for all male employees with children born from January 2017 onward, offering 100% wage replacement during that month.⁸ The leave had to be taken within one year of childbirth, or within two years if both spouses were employed. Figure 1 summarizes the policy timeline. The idea originated in 2015 through an internal contest to improve the workplace environment and was later adopted by the executive board of the holding company. In early 2016, the board surveyed employees across subsidiaries, marking the first time workers learned about the potential mandate. The formal policy announcement

⁶Federal policy grants a higher replacement rate cap for the second leave user, reflecting the fact that in most cases the second user is the father. The policy is designed to promote more active uptake of leave among fathers by providing greater financial incentives.

⁷We compute the replacement rate using post-tax earnings, since the benefit is tax-exempt.

⁸The C conglomerate has implemented an automatic parental leave policy for female employees since 2012. Under this scheme, women are automatically enrolled in one year of parental leave following the end of their maternity leave. As a result, we do not observe changes in female parental leave usage at C conglomerate firms following the policy change.

was made in December 2016, and the mandate took effect for children born in January 2017 or later.

The corporate mandate complemented the federal parental leave scheme, which already entitled employees to up to one year of leave. Under the federal policy, wage replacement ranged from approximately 36% to 71% of the average monthly post-tax earnings of C Group employees aged 30–45. With the corporate mandate, however, fathers received full wage replacement for the first month of leave. For any leave beyond the first month, compensation reverted to federal rules, offering 36–71% for the next two months and 24–36% thereafter, depending on the year. Because C Group employees in our analysis sample earned above-average wages, their effective replacement rates remained lower than the statutory maximum despite the policy change.

The decision to mandate leave was implemented in a top-down manner, consistent with the governance structure of large Korean conglomerates. C Group owned 44 companies across nine industries and employed 0.6% of the national labor force in 2016. Its largest employment shares were in retail, followed by hospitality and restaurants, and then food, beverage, and chemical manufacturing. Like other conglomerates, it consisted of a parent company that set group-wide strategies—such as welfare policies, investment decisions, and brand management—while individual subsidiaries oversaw business operations, including human resources, logistics, and production.⁹

3 Data

We draw on rich administrative datasets from Statistics Korea that can be linked at both the individual and firm levels for our main analysis. In addition, we supplement the administrative data with an original survey of male employees conducted in August 2025. We describe each administrative dataset below and discuss the survey design and measures separately in Section 7.

3.1 Corporate and firm Data

We combine the Business Registry and Employer-Employee Matched Data for 2015–2020 to identify male employees working under each conglomerate. The Business Registry offers information on the relationships between conglomerates and their affiliated companies, and on each company including revenue, industry codes, and annual employment size. Because

⁹The Korean Fair Trade Commission (FTC) designates conglomerate status annually based on the total asset value of member firms; as of 2016, 31 groups held this designation, including C Group, which has maintained this status continuously since the category was first introduced.

conglomerate status can vary over time based on asset values, we include all companies that have ever been affiliated with a conglomerate during our observation period. In addition, we restrict our sample to companies that are observed continuously from 2015 to 2020, ensuring that companies are not differentially affected by events such as mergers, acquisitions, or business closures. We then link this list of companies with the Employer-Employee Matched Data using hiring date information to identify male employees working at these companies. The Employer-Employee Matched Data provides information on annual earnings and job spells, which we use to construct average monthly earnings.

3.2 Fertility and Childbirth Data

We integrate data from three sources to construct detailed information on fertility. First, we link the Child Registry with the employee dataset at the individual level to obtain data on the number of children and their ages. The Child Registry, which is based on birth certificates, provides information on parental relationships for all children under age 18 on annual basis. To obtain more granular data on birth dates, we supplement the Child Registry with information from the 1983–1995 Cohort Database¹⁰ and the Parental Leave Registry available for 2015–2022. Both sources provide child birth dates at the year-month level. This linkage process allowed us to identify the birth date for 98% of children born since 2012, which marks the start of our observation period for fertility outcome. By linking these three datasets, we construct an indicator for having an infant.

3.3 Household Composition and Spousal Employment Data

One advantage of our dataset is the ability to identify spouses using household relationship information from the Population Census. We use this information for heterogeneity analysis by wife employment status and for spousal labor supply analysis. The Population Census, available for 2015–2022, provides marital information based on cohabitation as observed every November. The earliest available marital status is from November 2015, which is two months prior to our baseline period.¹¹ We further link the wife’s identifier from the Census to the Employment Registry to obtain information on the wife’s working status. The Employ-

¹⁰The 1983–1995 Cohort Database only covers children born to individuals within that birth cohort, representing about 60% of our analysis sample, while the Parental Leave Registry captures birth dates for children whose parents utilized parental leave.

¹¹Because the Census records household members only if they are registered at the same home address at the time of the survey, data on partnerships may be missing when partners are registered at different addresses or when one partner is temporarily abroad. For our purposes, a meaningful relationship is one in which both parents live together and care for the same child. Therefore, in such cases, we treat the individual as unmarried.

ment Registry covers a longer time series, between 2015–2022, than the Employer-Employee Matched Data, 2015–2020, but does not include information on employers. It provides annual information on employment status, job type, annual earnings, and employment duration.

4 Effects on Leave Utilization

First, we show that the mandate substantially increased paternity leave uptake, with additional utilization beyond the required one month. We define treated firms as those belonging to C Group and controls as firms in other conglomerates operating in the same industries.¹² Table A.1 presents the industry composition of each group, measured by the share of employees in each industry. The treated group is most concentrated in the retail sector, while the control group is concentrated in manufacturing. Firm characteristics (Panel A of Table 2) are broadly comparable across groups in size, revenue per worker, and employee age, though treated firms pay lower wages on average. Before the mandate, paternity leave use was rare in both groups—about 2% among treated firms and 1.2% among controls.

Figure 2 shows the dramatic shift after the mandate. The figure shows the proportion of fathers by leave duration within one year after childbirth, including those who took no leave. The uptake among new fathers at treated firms rose to 67.6%, confirming that the policy was binding and enforced as intended. While most fathers initially adhered to the one-month minimum, the share taking longer leave grew modestly over time.¹³ The uptake in our data did not reach 100%. Extending the window to two years after childbirth, the observed take-up rate is about 78% among newborn fathers at treated firms post-policy. This shortfall reflects both non-compliance and data limitations. Our data distinguish only between daily/temporary contracts (shorter than one month) and longer contracts, without finer detail on hours worked or contract type (part-time vs. full-time; permanent vs. fixed-term). In the Korean labor market, part-time workers and many full-time employees on fixed-term contracts are ineligible for firm-level benefits such as this mandate, which likely explains part of the gap. Using external survey benchmarks to impute the share of ineligible workers, we estimate that true compliance ranges from nearly universal to about 82%, implying non-compliance of 0–18%.¹⁴ While precise compliance rates are uncertain, the evidence indicates

¹²We use section-level industry codes from the Korean Standard Industry Classification (KSIC), which categorizes industries into 21 sections (A–U), to identify firms operating in the same industry.

¹³In South Korea, about 80% of mothers spend 2–4 weeks in a postnatal care center after birth. Consistent with this, 87% of fathers in our data began leave after their child was at least one month old, and roughly half did so when the child was 1–3 months old (Appendix Figure A.3).

¹⁴According to the Korean Economic Census (2017–2020), 21.7% of male employees aged 20–49 were either full-time non-permanent contract workers or part-time workers. If we assume the same share among treated

that eligible workers' compliance was high. We therefore focus on the policy's impact on extended leave usage and fertility behavior.

We formalize the descriptive patterns using an event study framework to estimate the policy's causal effect on leave-taking behavior of fathers with newborns in each biannual time period. Specifically, we estimate:

$$Y_{ib} = \beta_0 + \sum_{k \neq -1} \beta_{1,k} Treat_{ib} \times \mathbf{1}(b + k = 2017H1) + \tau_b + \theta_{i(j)} + \varepsilon_{ib}, \quad (1)$$

where Y_{ib} is an indicator for whether father i with a child born in biannual period b took any leave (or leave longer than one month) within one year of childbirth. $Treat_{ib}$ equals 1 for fathers employed at C Group firms in period b —the firms subject to the mandate—and 0 for fathers employed at companies from other conglomerates within the same industry that were not subject to the mandate; this is the same sample used in Figure 2. $\mathbf{1}(b + k = 2017H1)$ denote event-time indicators relative to the policy implementation, with $k = -1$, the period immediately before the policy, omitted as the reference period. The interaction term captures the dynamic effects of the mandate over time. We control for biannual childbirth cohort fixed effects, τ_b , and firm fixed effects, $\theta_{i(j)}$. Figure 3 presents the dynamic event-study results, and Table 3 reports corresponding average difference-in-differences estimates.

Figure 3a shows a sharp and sustained increase in leave-taking within one year of childbirth at treated firms following the policy. Column 1 of Table 3 indicates that the mandate raised uptake by 64.5 percentage points from a pre-policy baseline of 2% at treated firms. We also find economically meaningful increases in the probability of taking more than the mandated one month of leave. Column 2 shows that, on average, treated fathers were 5.6 percentage points more likely to take extended leave relative to a pre-policy baseline of 1.9%. This estimate may be inflated, however, because the second half of 2020 saw a sharp Covid-related spike in extended leave-taking at C group, visible in Figure 2. To address this, we estimate a version truncated at 2020H2, reported in Column 3, which still shows a 3.9 p.p increase—about three times higher than the pre-policy baseline. We present this version in the event-study graph in Figure 3b, which improves visibility of the post-policy trend.

Does the mandate also influence fathers who were not directly covered by the policy? Fathers with children born before January 2017 were ineligible for the corporate mandate but still qualified for the federal parental leave benefit, which could be used until a child turned eight. To test for spillover effects, we estimate a difference-in-differences specification on an unbalanced panel of fathers with births in 2015–2016. The sample is restricted to periods in

fathers were ineligible, compliance reaches 99.5%. Restricting ineligibility to part-time workers only (5.1% of male employees) implies a compliance rate of 82.2%.

which these fathers remained employed at the same firm, as the goal is to assess whether leave-taking behavior changed for non-covered workers exposed to a mandate-affected workplace environment. We estimate the following equation:

$$Y_{it} = \beta_0 + \sum_{k \neq -1} \beta_{1,k} Treat_i \times \mathbf{1}(t + k = 2017H1) + \tau_t + \theta_{i(j)} + \varepsilon_{ib}. \quad (2)$$

Y_{it} is an indicator for whether father i took leave in biannual period t . To focus on ineligible fathers, we code the outcome as zero if leave was taken after the birth of another child born post-2017, since those fathers became eligible under the mandate. This specification is designed to isolate potential spillover effects on ineligible workers who were nevertheless exposed to a mandate-affected workplace environment. We include biannual time fixed effects, τ_t , to absorb time-varying common shocks, including changes to federal benefits, and firm fixed effects, $\theta_{i(j)}$, to account for persistent differences across firms.

Figure 3c and Column 4 of Table 3 show that ineligible fathers at treated firms were also more likely to take leave following the mandate. On average, their likelihood of taking leave rose by 2.4 percentage points relative to a pre-policy mean of 0.9%. By comparison, their counterparts at control firms saw only a 0.8 percentage point increase, from a similar baseline of 0.6%. In other words, ineligible fathers at treated firms were nearly twice as likely to take leave post-policy compared to similarly ineligible fathers at control firms.

5 Identifying the Effects of the Mandate on Fertility

Having established the policy’s impact on paternity leave uptake, we now turn to fertility behavior. To estimate these effects, we implement an event study design. We first define the treated and control groups, specify the timing of treatment, and establish a pre-policy baseline to capture anticipation effects.

5.1 Defining the Treated and Control Group

Our estimation strategy exploits random exposure to treatment via company affiliation at the time of the policy change. To capture both the impact on subsequent births, and the decision to have a first child, we include all men of child bearing age regardless of their current parental status. For the treatment group, we select male employees who were between 30 and 45 years old at that time. We chose the age range because the average age at first marriage for men in Korea was 32 during the post-policy period. Because the policy was implemented at the company level, an individual’s treatment status depends on their employer. This status

could be endogenous if workers joined a treated company after the policy was introduced. To address this, we restrict the sample to employees who were already at their firms before policy information first circulated. For the control group, we include male employees of the same age who, at baseline, were employed at companies in other conglomerates that did not implement the mandate.

5.2 Defining Treatment Timing and Baseline Period

We use the first half of 2016—the period when information about the mandate began circulating—as the baseline. Paternity leave can influence fertility behavior not only through actual uptake but also through anticipatory planning, as households may adjust childbearing decisions based on expected access to leave. As outlined in Section 2.2, an internal survey on the policy was circulated in early 2016, likely raising awareness among employees. If households responded, conceptions would have occurred in early 2016, with births later that year. Accordingly, we set 2016H1 as the reference period for subsequent analysis. To better capture such anticipation effects, we use biannual rather than annual time intervals.

5.3 Differences in differences Estimator

Our empirical approach employs an event study design to assess how the probability of having a baby changed among cohorts employed at treated versus control companies during the observation period. Specifically, we use a linear probability model to estimate the relative increase in the probability of having an infant for men at treated firms, compared to men at control firms of the same age, baseline earnings quintile, and baseline tenure. The model is specified as follows:

$$1(\text{Infant})_{it} = \beta_0 + \sum_{k \neq -1} \beta_{1,k} \text{Treat}_i \times \mathbf{1}(t + k = 2016H1) + X_i' \Gamma + X_{it}' \delta + \theta_t + \varepsilon_{it}, \quad (3)$$

where $1(\text{Infant})_{it}$ is an indicator for whether father i has an infant (aged 0) during biannual calendar time t .¹⁵ Treat_i equals 1 if father i was employed by a C Group company at the beginning of the baseline period. $\mathbf{1}(t + k = 2016H1)$ denote event time dummies relative to the policy announcement, with $k = -1$ omitted as the reference period. The interaction $\text{Treat}_i \times \mathbf{1}(t + k = 2016H1)$ captures the differential effect of the policy over time.

¹⁵This fertility indicator, when aggregated annually, corresponds closely to the conventional birth rate. We use biannual units instead, which better capture immediate fertility responses to the policy. As shown in Figure 4a, fertility increases every six months after the reference period. While annual aggregation yields similar overall patterns, it masks the initial jump that is visible only in the biannual specification.

The vector X_i includes time-invariant characteristics such as baseline tenure, baseline monthly earnings quintile indicators, and baseline firm fixed effects. Since employment is recorded at the company level—and each company can include multiple firms—we define firms at the company-by-commuting-area level. This definition reflects average commuting distances in Korea and groups workers at the minimal level that still captures the firms they are most likely to commute to.¹⁶ The firm fixed effects absorb time-invariant characteristics at the firm level, ensuring that results are not driven by pre-existing differences across firms—such as preferences for childbearing, career expectations, or work-life balance—that predate the policy change. The vector X_{it} captures time-varying characteristics. In the main specification, we include age. In robustness checks, we also control for additional demographic controls, such as an indicator for whether the man had a child in $(t - 2)$ and indicators for the child’s age groups (0, 1–3, 4–7, and 8 or above). We cluster robust standard errors at the firm level to account for potential within-firm correlation.

Our coefficients of interest, $\beta_{1,k}$, capture the causal impact of the paternity leave mandate on the probability of having an infant, a 0-year-old child in period k relative to the reference period. We track this probability from 2012H1 to 2022H2 using a balanced panel of men who were aged 30–45 at the time of the reference period, regardless of whether they remained at the same firm afterward. By following the same cohort over time—beginning when the youngest was 27 and continuing until the youngest reached 36 and the oldest 51—we observe changes in fertility behavior before and after the policy. This approach allows us to hold the cohort fixed, minimizing concerns about bias from changes in sample composition.

The key identifying assumption is that, absent the mandate, fertility behavior in treated and control groups would have followed similar trends. A potential violation arises if treated firms systematically employed men with stronger fertility preferences. In that case, fertility rates would have diverged from controls even before the policy was introduced, violating the parallel-trends assumption. In the data, however, formal tests reveal no evidence of differential pre-policy fertility trends between the two groups (Figure 4a).

5.4 Main Analysis Sample

Table 2 presents descriptive statistics for the treated and control groups in our fertility analysis sample, which includes all male incumbents at these firms regardless of their parental status as of the beginning of 2016—as opposed to the father sample used in the leave-taking analysis in Table 3. Characteristics are measured at baseline, with the exception of the birth rate, which is calculated as the average probability of having a child during the full pre-policy

¹⁶We construct a firm as a combination of a company identifier and a commuting-area pair, where the commuting area is based on employees’ province of residence. There are nine commuting regions nationwide.

period (2012–2015). The treated group comprises fewer individuals ($N = 17,366$) relative to the control group ($N = 378,689$) because the treated group consists of incumbents from a single conglomerate, whereas the control group draws from 30 other conglomerates. Both groups have a similar average age of 37 years and average tenure of about 8 years at baseline. Although both groups earn more than the national average for their age cohort (3.9 million KRW), treated individuals earn, on average, 1.7 million KRW less than control individuals.¹⁷ To account for these differences, we include baseline earnings quintile indicators in our main analysis, along with controls for age and baseline tenure.

Treated individuals were less likely to be married (60% versus 68%), even though the average age of spouses was identical across groups. Consistent with this lower marriage rate, a higher proportion of treated individuals were childless (40% versus 33%) and their pre-policy birth rate was about one percentage points lower. To address these differences in family characteristics, we conduct robustness checks controlling for marital status and number of children; the results remain very similar to our main estimates. We also reweight control-group observations to match the industry composition of the treated group, where weights are defined as the ratio of each industry’s share in the treated group to its share in the control group. Reweighting by industry makes the two samples more balanced in marriage rate, childlessness, and earnings (Appendix Table A.2) and yields qualitatively similar results (Appendix Figure A.4a).

6 Effects on Household Fertility

We examine whether the mandate affected the likelihood that male incumbents had an infant (a 0-year-old child) in each period. Figure 4a presents estimates from Equation 3, with the first dashed line marking our reference period—when information about the policy began circulating—and the second dashed line marking formal implementation.

Figure 4a shows that the probability of having an infant rose sharply after the policy among treated incumbents and remained consistently higher than that of the control group throughout the post-policy period. Pre-policy estimates are statistically insignificant and centered around zero, confirming that fertility trends evolved in parallel prior to the reform. To ensure that the observed fertility responses are attributable to the mandate, we regress an outcome variable that equals 1 only for births that occurred while fathers remained employed at the same firm, and 0 otherwise. Eligibility for the mandate depended on being employed at the firm at the time of childbirth, so this restriction ensures that the estimated effects reflect actual exposure to the policy. The results, presented in Figure 4b, closely mirror

¹⁷In 2016, 1 million KRW was equivalent to approximately \$862, and 3.9 million KRW to about \$3,362.

those in Figure 4a, reinforcing that the fertility increase is due to the mandate. We also observe a rise in the probability of having an infant beginning in the second half of 2016, shortly after employees first learned about the policy. This anticipatory response supports our choice of reference period: if households made childbearing decisions in anticipation of the mandate, the resulting births would appear by late 2016, as shown in both Figure 4a and Figure 4b. This pattern underscores that fertility decisions are forward-looking, with households deciding on childbearing based on expected leave availability.

Column 1 and 2 in Table 4 presents the corresponding difference-in-differences estimates - pooled average estimates across post-policy periods. On average, the probability of having an infant increases by 1.4 percentage points—a 14.7% increase relative to the treated group’s pre-policy average of 9.5%. Aggregating over six post-policy years, our back-of-the-envelope calculation yields about 1,459 additional births in the treated group. To our knowledge, this is the first study to show positive effects of paternity leave on fertility. To put the magnitude in context, the national total fertility rate (TFR)¹⁸ during the post-policy period was 0.94 on average. Our estimates imply an additional 0.084 births per treated man, or roughly 8.9% of the national TFR. If we interpret the additional 0.084 births per man as the total number of births these men will ever have, this provides a conservative lower bound on the effect size since it assumes no further births beyond our observation window. Additionally, we compare this to the actual number of births among same-aged Korean men in the post-policy period—0.204 births per man. On this margin, the mandate increased fertility by nearly 41%, indicating an upper bound on the effect size.

Relative to studies of maternity leave reforms, our effect size is similar in magnitude. Raute (2019) show that expanding financial incentives for maternity leave among high-earning mothers raised fertility by 16% relative to pre-reform birth rates, while Lalive and Zweimüller (2009) find that extending parental leave in Austria increased births by 21% within three years. Yet, because the paternity leave mandate involved a smaller policy change—one month of required leave, compared with six months of extended leave (Lalive and Zweimüller, 2009) or a year of higher replacement rates (Raute, 2019)—our estimates suggest a much larger effect per unit of policy expansion.

6.1 Bounding the Bias from Unobserved Eligibility

As discussed in Section 4, our data does not perfectly capture treatment status—eligibility for the mandate depends on workers’ contract types, which are not fully observed. As a

¹⁸The total fertility rate is the average number of children a woman of childbearing age is expected to have over her lifetime, summarizing the total number of births when fertility rates are tracked across age groups over time.

result, our main estimates are attenuated by the inclusion of workers who were ineligible, such as those on part-time or fixed-term full-time contracts. This type of misclassification is common in policy evaluation, and prior studies propose several correction approaches that hinge on whether the measurement error is related to outcomes (Negi and Negi, 2025). When misreporting is independent of outcomes conditional on true treatment status, the bias takes an attenuation form (Battistin and Sianesi, 2011).

In our setting, misreporting arises from imperfect information on pre-policy contract types. Conditional on true treatment status (working at the C group as an eligible employee), this error is plausibly independent of outcomes. Using national survey data, we proxy the share of fixed-term (21.7%) and part-time (5.2%) workers as lower and upper bounds for misclassification. Applying the method of Battistin and Sianesi (2011), and assuming independence of errors from treatment and observables, we impute an unbiased treatment effect ranging from a 1.5 p.p. to 1.9 p.p. increase in annual probability of having an infant, compared to the baseline estimate of 1.4 p.p. This yields potential bias from misclassification of roughly 5–28%.

6.2 Extensive vs. Intensive Margin

Next, since the policy could influence fertility both by encouraging first births (extensive margin) and by incentivizing higher-order births (intensive margin), we examine how much of the overall fertility effect comes from each channel. To isolate the extensive margin response, we regress an indicator for whether the infant in a given period is a first child—that is, the outcome variable equals 1 only if the individual has an infant in a given period and the infant is their first-born.

The results are reported in Column 3 and 4 of Table 4, with corresponding event study estimates shown in Figure 5. We find that the probability of having a first-born infant increases by 0.8 percentage points, relative to a pre-policy mean of 4.7% in the treated group—representing a 17% increase. The coefficient estimate is directly comparable to the overall fertility effect in Column 1; this estimate is about half the size of the overall effect (Column 1), implying that 53% of the fertility increase is driven by new parents. In other words, about half of the observed increase is driven by childless households having their first child, while the other half reflects households having additional children in the post-policy period. We construct a complementary indicator for second or higher-order births (intensive margin). Column 4 confirms a 0.6 percentage point effect—47% of the total.¹⁹ Together, these

¹⁹We also examine whether fathers whose first post-policy child was covered by the mandate were more likely to have a subsequent birth than otherwise similar fathers who just missed coverage following prior studies (Dahl et al., 2014; Cools et al., 2015; Bartel et al., 2018; Farré and González, 2019). We find no

results show that the policy increased fertility along both margins by similar magnitudes, affecting both new parents and those expanding their families.

6.3 Total Fertility vs. Fertility Timing

A key concern in interpreting the effect as a fertility gain is that the patterns might reflect shifts in fertility timing rather than increases in completed fertility. If households merely accelerated births they would have had anyway, our estimates would capture timing adjustments rather than additional childbearing. Under this scenario, we may observe any of two patterns: (i) accelerated entry into parenthood, reflected in fertility gains concentrated at first births and younger parental ages at childbirth, and (ii) shorter birth intervals among families with existing children. As shown in Section 6.2, our results already rule out the first scenario: fertility gains appear along both the extensive and intensive margins.

We next examine parental age at childbirth and birth spacing, two outcomes that would shift if timing effects were driving the results. For this analysis, we restrict the sample to incumbents at treated and control firms who had a newborn in each period, and estimate the difference-in-differences specification on three outcomes: (i) father’s age at childbirth, (ii) mother’s age at childbirth, and (iii) the age gap between the newborn and the first child. Table 5 reports the results: we find no significant differences between treated and control groups for any of these outcomes (Columns 1–3). Figure 6a illustrates nearly identical trends in paternal age across groups, with no evidence of bunching at younger ages. Similarly, Figure 6b shows no systematic changes in spacing between first and subsequent births, aside from a modest increase in first births in 2022H1.

Overall, these patterns are inconsistent with the view that the mandate simply accelerated births. While we cannot observe completed fertility within our data horizon, the absence of shifts in age or spacing provides supportive evidence that the policy generated additional fertility.

6.4 Heterogeneity by Wives’ Employment

How could a one-month leave mandate induce such large changes in fertility behavior? We argue that the mandate worked by relaxing constraints on fathers’ ability to devote time to childcare, constraints that are most binding in households where mothers hold inflexible or demanding jobs. In such settings, limited expectations of paternal involvement can discourage families from having additional children. Our simple household model (Appendix C)

statistically significant difference. This pattern suggests that households who “just missed” eligibility may still plan an additional child in anticipation of being covered for a future birth. See Appendix B for results.

formalizes this logic and predicts that effects should be strongest among dual-earner households and should increase with wives’ opportunity cost of time.²⁰ Guided by this framework, we examine heterogeneity by wives’ employment characteristics.

We classify households by wives’ baseline employment status prior to the policy introduction using linked administrative data from the Population Census and Employment Registry. This ensures that status is pre-determined and unaffected by the mandate. Table 7 presents descriptive statistics for married households. As in the main sample, treated couples earned somewhat less on average than their control counterparts, though treated wives had higher baseline employment rates. Following Statistics Korea’s classification, we define *any job* as any paid work (daily, temporary, or self-employment) and *regular job* as wage or salary employment with a contract longer than one month.

We estimate the baseline difference-in-differences model separately by wives’ employment status, retaining the full set of controls. Table 8 reports the results and corresponding event study estimates are shown in Figure 8.²¹ Fertility responses are concentrated among dual-earner households. Column 1 of Table 8 shows no significant effect among unmarried incumbents. Among married couples, the effects are driven by dual-earner households, as shown in Columns 2 and 3: households with employed wives experience a significant increase in fertility, while those with non-working wives show no response. For households where wives were employed prior to the reference period, treated households exhibit a 1 p.p. increase in fertility, equivalent to an 8.2% rise relative to their pre-policy average of 12.2%. The effect is strongest when the wife held a regular job (Column 4), which typically involves fixed schedules and limited flexibility. In these households, the probability of birth rises by 1.4 p.p., or 11.1% relative to the pre-policy average of 12.6%. By contrast, households with wives in temporary, daily, or self-employed jobs—typically characterized by more flexible work schedules—exhibit no effect. The point estimate for regular job holders is similar to the overall main effect, indicating that the fertility response is almost entirely driven by households where wives were employed in inflexible jobs.

We further test whether the effect increases with wives’ opportunity cost of time, proxied by their baseline annual earnings and tenure. Higher earnings capture the opportunity cost of time away from work, while longer tenure reflects stronger labor market attachment. To

²⁰The model treats fertility as a forward-looking decision shaped by three channels: fathers’ increased time at home, reduced utility costs of childcare time from workplace penalties, and an income effect from full wage replacement. It predicts that effects should be strongest among dual-earner households if the paternal time channels are driving the results.

²¹Because subgroup shares differ between treated and control groups, the weighted average of subgroup estimates is not equivalent to the main effect in Table 4. When $\Pr(g | T) \neq \Pr(g | C)$, subgroup treatment effects cannot be linearly combined to recover the overall effect. In our sample, the weighted composite effect matches the full-sample estimate.

do so, we interact the treatment indicator with these characteristics, allowing the treatment effect to vary accordingly.

$$Y_{it} = \gamma_0 + \gamma_1(Treat_i \times Post_t)\tilde{\lambda}_i + \gamma_2Treat_i \times Post_t + \gamma_3Treat_i \cdot \tilde{\lambda}_i + \gamma_4Treat_i + \gamma_5Post_t \cdot \tilde{\lambda}_i + \gamma_6Post_t + X_i'\Gamma + \varepsilon_{it}, \quad (4)$$

where λ_i is either the wife’s baseline earnings or tenure, and $\tilde{\lambda}_i = \lambda_i/SD(\lambda_i)$ standardizes the variable for comparability.

Table 9 reports the results. Columns 1–2 interact the treatment effect with baseline earnings and Columns 3–4 with baseline tenure. We present estimates for both the dual-earner subsample and the full sample of married households. We focus on the dual-earner specification, since the estimates for all wives are qualitatively similar. The interaction term γ_1 for employed wives, our coefficient of interest, is significant and positive: a one standard deviation increase in baseline annual earnings—27.6 million KRW, or roughly 2.3 million KRW in average monthly earnings if employed for a full year—is associated with a 0.8 p.p. increase in the likelihood of birth, or 6.7% relative to the pre-policy mean of 12%. Similarly, a one standard deviation increase in baseline tenure—equivalent to 4.5 years—is associated with a 0.9 p.p. increase, or 7.5% relative to the pre-policy mean. Taken together, these results show that fertility responses are driven almost entirely by households where wives face high opportunity costs of time, consistent with the view that expectations of greater paternal availability were central to the effects.

6.5 Effects on Spousal Labor Supply

Having shown that fertility responses are concentrated in households where wives face high time costs, we next examine whether these fertility gains came at the expense of women’s employment. If the mandate raised fertility by increasing fathers’ availability at home, wives’ labor supply should remain stable—or even rise—following the policy.

We use the same matched sample of married households from the heterogeneity analysis and track wives’ labor market outcomes over time. Specifically, we test whether wives in treated households experienced relative declines in employment after the policy change compared to those in control households. The dependent variable is the wife’s employment status in each biannual period. We include the same control variables as in the main analysis—husband’s age, baseline tenure, baseline earnings quintile indicators, baseline firm fixed effects, and calendar time fixed effects—together with wife’s age. A limitation of this analysis is the shorter pre-policy window, as Employment Registry data are available only from 2015, providing two pre-policy data points.

The analysis reveals consistently positive effects across several labor supply outcomes. Panel A of Table 10 presents the regression results and corresponding event study estimates are shown in Figure 9. Columns 1 and 2 show estimated effects on wives’ probability of being employed in any job and in a regular job (contract duration longer than one month), respectively. Column 3 examines labor intensity, measured as the number of months worked per biannual period. The results indicate that wives of treated incumbents were more likely to remain attached to the labor force. Their probability of being employed in any job increased by 2.1 p.p. from a pre-policy average of 45.4%. The likelihood of holding a regular job rose by 1.9 p.p. from a baseline of 40%. In terms of labor intensity, treated wives worked an additional 0.12 months per biannual period on average—about a 4.3% increase relative to a pre-policy mean of 2.8 months. These effect sizes represent a 4–5% improvement relative to the control group.

Additionally, we examine labor market outcomes for mothers who gave birth to children of treated incumbents before and after the policy. This exercise focuses on a selected subsample of wives who had a birth, allowing us to zoom in on post-childbirth employment dynamics. We track each mother’s employment status one, two, and three years after childbirth, including self-employment. Because the decision to work after childbirth is endogenous to fertility decisions, these results should not be interpreted as causal. Rather, they provide descriptive evidence on whether the fertility increase among treated households was accompanied by declines in maternal employment.

We estimate the following difference-in-differences model:

$$Y_{ib} = \pi + \gamma \cdot \text{Treat}_i \times \text{Post}_b + \tau \cdot \text{Treat}_i + X_i' \Gamma + \tau_b + \varepsilon_{ib}, \quad (5)$$

where Y_{ib} denotes the labor market outcome of mother i who gave birth in biannual period b . We control for parental ages, an indicator for first birth, the square root of the father’s earnings at childbirth, and biannual birth-period fixed effects (τ_b).

Due to data limitations, we observe employment outcomes for only 26% of all pre-policy mothers—specifically, those who gave birth in 2015 or early 2016 and did not have subsequent post-policy births. This restricts sample size and reduces statistical power. The corresponding results, shown in Panel B of Table 10, should therefore be interpreted with caution.

The estimates fail to reject the null that mothers experienced larger post-childbirth employment penalties. Several mechanisms may explain this null result. First, exposure spillovers may have affected pre-policy treated mothers: as shown in Section 4, fathers at treated firms increased their leave uptake even before being directly subject to the mandate, likely due to shifting workplace norms. Treated mothers could thus have benefited indirectly,

attenuating the contrast between pre- and post-policy treated groups and biasing estimates toward zero. Second, the one-month paternity leave itself may have been too short to meaningfully reduce the career costs of childbearing. These patterns are consistent with prior mixed evidence: positive effects in Norway, Spain, and Quebec (Cools et al., 2015; Farré and González, 2019; Patnaik, 2019), but limited or short-lived impacts in Germany (Tamm, 2019) and Sweden (Ekberg et al., 2013; Avdic and Karimi, 2018). Still, the absence of any detectable decline in maternal employment suggests that the fertility gains observed among treated households did not come at the expense of mothers’ post-childbirth labor market attachment.

Next, we examine whether the mandate affected mothers’ own parental leave-taking (Table A.4). The sample includes mothers of children born to treated and control fathers between 2015 and 2022 who were eligible for leave at childbirth—that is, employed at least six months and earning above the full-time minimum wage. We estimate a difference-in-differences model controlling for maternal age, parity, pre-birth earnings, and biannual birth cohort fixed effects. The results show no meaningful changes in leave usage among treated mothers, with only a marginal increase in leave duration of 0.6 months (about 2.5 weeks) relative to a pre-policy average of 12.2 months. Overall, we find no evidence that fathers’ additional time at home substituted for mothers’ leave-taking, suggesting that the policy eased household time constraints through greater joint availability rather than a reallocation of leave between parents.²²

In sum, the evidence shows that the fertility increase did not come at the expense of women’s employment, and that fathers’ mandated month of leave did not alter mothers’ return-to-work timing or leave-taking. Taken together, these results suggest that the policy enabled higher fertility without undermining women’s careers.

6.6 Robustness Test

We conduct robustness tests to assess whether baseline demographic differences drive the main results. We report these findings in Figure 7 and Table 6.

First, treated incumbents were less likely to be married at baseline. To account for this difference, we additionally control for baseline marital status inferred from Population Census data collected every November. As reported in Column (2), the results remain unchanged after controlling for baseline marital status.

²²Because parental leave data are available only from 2015, we observe three pre-policy periods for this outcome, compared with eight for fertility. This limits our ability to test for pre-trends, so results should be interpreted cautiously.

Second, to adjust for baseline parity differences (treated men had fewer children), we include indicators for the number of existing children—0, 1, or 2 or more—in period $t - 2$ (one year prior, given the biannual data). This adjustment reflects that most individuals have at most two children, and those with two or more are unlikely to have additional births. As shown in Column (3) of Table 6, including these controls yields estimates very similar to our main results. Additionally, to account for potential birth spacing, we include dummies for the age of the youngest child in $t - 2$ —categorized as no child, 0 years, 1–4 years, 5–7 years, and 8 or above—and again find similar results (Column 6). Overall, these checks confirm that our main findings are not driven by baseline differences in marital status or by the number and age of children.

Additionally, some effects may partly reflect couples in which the wife is also employed at a C Group company, since the conglomerate simultaneously extended its maternity leave provision to two years. Although C Group ranks fifth nationwide in employment size, dual employment within the group is rare: only 6.9% of treated men (1,192 out of 17,366) were ever married to a C Group employee. To address this concern, we re-estimate the main specifications excluding men ever married to a wife employed at a C Group subsidiary—for both treated and control samples—and find that results remain robust. Columns (1) and (2) of Table ?? present these estimates, which are nearly identical to the main results, along with the corresponding event-study patterns shown in Appendix Figures A.5a and A.5b.

Finally, to address compositional differences across industries, we reweight control-group observations to match the industry composition of the treated group, where weights are defined as the ratio of each industry’s share in the treated group to its share in the control group. Columns (3) and (4) of Table ?? report the reweighted estimates. Reweighting improves balance in marriage rate, childlessness, and earnings (Appendix Table A.2) and yields positive and statistically significant effects that are smaller in magnitude—roughly half the size of the main estimate for having an infant and about two-thirds for having an infant while remaining at the same firm. Nonetheless, the qualitative patterns remain very similar to the main results, as illustrated in Appendix Figure A.4a and Figure A.4b.

7 Survey Evidence on Workplace Norms

Administrative data show that time-constrained households were the most responsive to the policy, consistent with the interpretation that the mandate relaxed binding time constraints by increasing fathers’ effective availability for childcare. However, these patterns provide only indirect evidence on the underlying mechanism. Administrative records cannot directly capture how workplace norms and expectations shape parents’ decisions. To address

this limitation, we complement the administrative analysis with new survey evidence that directly measures attitudes, expectations, and leave-taking behavior within treated and control workplaces. The survey is designed to test whether mandating paternal leave normalized caregiving expectations at work and, in turn, eased time-related barriers to childbearing.

We surveyed 450 male employees aged 30–45—the same age range used in our main analysis—in August 2025. The sample was restricted to full-time, permanent-contract workers to ensure eligibility for corporate benefits. Respondents were recruited through three online survey providers operating quota-based panels, comparable to widely used international platforms such as Qualtrics and Prolific.²³ Quota sampling is a non-probability method in which invitations are sent to eligible individuals until specified quotas are filled. We selected individuals meeting the eligibility criteria and invited them via email to participate in the survey. The final sample includes 216 respondents from treated firms and 234 from control firms. We define treatment using two criteria: (i) the respondent works at a subsidiary of *C Group*, and (ii) reports that a mandate is in place at their firm. Control groups are defined symmetrically: (i) employed at large corporations unaffiliated with *C Group*, and (ii) reporting no mandate in place. This dual definition ensures that individuals categorized as “treated” are both exposed to—and aware of—the policy.

The survey comprised two parts. First, respondents reported their childcare time, perceptions of workplace culture, and experiences with leave-taking at their current workplaces. Second, we introduced a hypothetical male coworker, *Mingyu* (age 33), whose wife is employed full-time and who is expecting their first child. Respondents indicated (i) whether, on a 10-point scale, they would recommend that Mingyu take paternity leave, (ii) anticipated career penalties if he took leave, and (iii) the perceived likelihood that he would have a second child.

Table 11 presents descriptive statistics for the survey sample. Treated and control groups are similar in marriage rates, the share with infant or pre-K children, wives’ employment, and wives’ regular-job status, with no statistically significant differences. Treated respondents are more likely to have taken leave when eligible—74% compared to 17% in the control group, a difference of 57 percentage points—confirming that they are indeed employed at mandate-covered firms. Treated respondents are 0.8 years older on average (39.0 vs. 38.2, marginally significant at the 10% level). Job rank and tenure distributions are comparable, while own earnings are lower at treated firms: the share earning above 6 million KRW is 20 percentage points lower (0.3 vs. 0.5). By contrast, the share with high-earning spouses (wives earning more than 5 million KRW) is nearly identical. The lower own earnings at treated firms are

²³To enlarge the restricted pool, we relied on multiple providers to maximize sample size while controlling costs. Further details on survey implementation are provided in Appendix D.

consistent with our main sample. In subsequent regressions, we include controls for high-income status and age-by-marital group to address these differences.

Table A.5 compares descriptive statistics between the survey sample and the main incumbent sample. Relative to the main sample, the survey respondents are about two years older on average in both treatment and control groups, and they are more likely to be married. The main sample contains a larger share of workers with 5–8 years of tenure and a smaller share with 9 or more years of tenure. Because incumbent tenure is measured as of 2016 whereas the survey was fielded in 2025, this pattern indicates that some survey respondents were already employed at treated firms at the time of the policy change. In other words, part of the survey sample overlaps with the set of incumbents in the main analysis.

7.1 Differences in Perception, Childcare and Fertility Intention

Employees at treated firms were far more likely to have used paternity leave when eligible and were more supportive of leave-taking overall. Figure 10 plots average responses to leave-related questions. Asterisks appear next to the treated-group mean and indicate that the difference from the control mean is statistically significant at the 1% level, with corresponding p-values reported in parentheses. Among eligible employees, 74% at treated firms report having used paternity leave versus 17% at control firms. Leave eligibility is defined using reported child ages and tenure at the current employer. A respondent is classified as ever eligible if any child was under age eight during his tenure at the current firm. In our sample, 137 of 236 control employees and 136 of 214 treated employees are ever eligible. Treated respondents also report greater willingness to recommend leave to a hypothetical coworker expecting a newborn (*Mingyu*): 8.3 on average versus 6.0 for controls on a 10-point scale. The 2.3-point gap corresponds to 0.83 of the control-group standard deviation. The share willing to recommend leave (score > 5) is 83.6% for treated versus 54.7% for controls.

Similarly, treated respondents expect broader support from their coworkers. As shown in Figure 10, they believe 7 out of 10 eligible coworkers would take leave at their firm, compared with 2 out of 10 among controls—a difference of about 50 percentage points when expressed as a likelihood, mirroring the observed uptake gap. They also anticipate stronger peer encouragement: treated respondents expect about 7 out of 10 coworkers would recommend leave, compared with 4.7 out of 10 among controls. The 2.3-point gap corresponds to 0.8 of the control-group standard deviation.

Treated individuals also perceive fewer penalties and burdens associated with leave-taking. In Columns 1 and 2 of Table 12, we compare the share agreeing with each statement about situations a hypothetical coworker, *Mingyu*, would face if he took paternity leave at the

respondent’s company. We control for high-income status and the full set of age-by-marital indicators to account for demographic differences.²⁴ In Column 1, treated respondents are 45 percentage points less likely than controls to agree that *Mingyu* would face career penalties after leave; 58 percent of control respondents express such concerns. In Column 2, treated respondents are 52 percentage points less likely to agree that coworkers’ workload would increase due to *Mingyu*’s leave, compared to 74 percent of controls who agree. These gaps suggest that leave-taking is more normalized at treated firms and viewed as less costly for both the leave-taker and coworkers.

Next, we find that differences in perceived workplace support for leave-taking are associated with greater paternal involvement at home and stronger fertility intentions. First, treated men report spending more time on weekday childcare. We elicited childcare time—or expected time if the respondent was childless—in six intervals.²⁵ Figure 11 shows that 44.4% of treated men spend more than 120 minutes on childcare during weekdays, compared to 35.6% of control men. To compare average time, we converted interval responses to minutes using midpoints. Controlling for high-income status and the full set of age-by-marital indicators, the midpoint OLS estimate (Column 3 of Table 12) indicates that treated respondents spend 6.3 minutes more on childcare than controls, whose mean is 91.9 minutes. Using an interval regression that accounts for censoring at both ends (Column 4), the estimated difference is 8.8 minutes, relative to a control-group mean of 100.2 minutes. Second, treated respondents report higher fertility intentions. Among employees with fewer than two children, they are 7 percentage points more likely than controls to intend to have a child, relative to a control-group mean of 88 percent (Column 5). Their desired number of children is also higher by 0.11—about 9.4 percent of the control-group mean of 1.17 (Column 6). The differences are larger among childless employees. Treated respondents with no child are 16 percentage points more likely to intend to have a child, compared to a control-group mean of 77 percent (Column 7). Their desired number of children is higher by 0.19—about 17.4 percent of the control-group mean of 1.09 (Column 8). While cross-sectional, these patterns are consistent with the interpretation that the mandate acted as a normalizing device, with shifts in norms amplifying fertility effects beyond the one-month leave entitlement.

Additionally, because our survey was fielded in August 2025—eight years after the policy’s introduction—it is possible that some differences reflect sorting of workers with different preferences into treated firms. While such sorting may itself be a channel through which

²⁴We include four indicators for the 2×2 interaction of age (< 37 vs. ≥ 37) and marital status (single vs. married), omitting one as the reference. This flexibly adjusts for joint differences in age and marital composition while preserving statistical power in our sample of 450 respondents.

²⁵The intervals are: less than 10 minutes; 10–30 minutes; 30–60 minutes; 60–90 minutes; 90–120 minutes; and more than 120 minutes.

the mandate shapes workplace culture, we replicate the analysis restricting the sample to employees who joined their firms before the policy (i.e., tenure of nine years or more), representing about 50% of respondents. We regress survey responses on leave usage behavior and perceived penalties, controlling for high-income status and the full set of age-by-marital indicators as in the baseline specification. The results, presented in Table A.6, show that even among these pre-policy incumbents, treated employees report more favorable attitudes toward leave-taking and lower perceived penalties. This suggests that while worker composition may play some role, the documented differences are not solely driven by post-policy entrants.

7.2 Mandate Effects in Counterfactual Vignettes

How important is the mandate in shaping more favorable attitudes toward leave-taking? To assess this directly, we re-asked the *control* group the same vignette questions about a hypothetical coworker, *Mingyu* (age 33; wife employed full-time; expecting a first child), but instructed them to answer as if a corporate mandate were in place, analogous to the policy at treated firms. Again, respondents reported (i) whether they would recommend that Mingyu take paternity leave, (ii) expected career penalties if he did, (iii) expected coworker workload effects, (iv) expected peer support (share of coworkers who would recommend leave), and (v) the perceived probability that Mingyu would have a second child. We plot the within-respondent differences (mandate scenario minus status quo) alongside the initial responses in Figure 12.

The mandate scenario moves attitudes in the direction observed at treated firms. Under the hypothetical mandate, the share of control respondents willing to recommend leave rises by 28.8 percentage points from 54.6% to 83.4%. Expected peer support also increases: the expected share of coworkers who would recommend leave increases by 17.2 p.p. Concerns about penalties and coworker burden decline: the share agreeing that *Mingyu* would face career penalties decreases by 14.8 percentage points from 58%, while agreement that coworkers' workload would increase decreases by 9.7 percentage points from 74%. Finally, fertility expectations shift as well: the perceived probability that the hypothetical couple would have a second child increases by 13.8 percentage points relative to an initial mean of 39%.

These within-respondent results show that counterfactually assuming a mandate is in place substantially increases support for leave, lowers perceived penalties and burdens, and raises expected fertility. This pattern mirrors the cross-group differences documented above, underscoring how the policy environment shapes attitudes and expectations.

8 Effects on Firms

While the primary focus of this paper is on household fertility responses to a paternity leave mandate, scaling such a mandate to the national level highlights the importance of understanding firm-level costs. Offering paternity leave can impose costs through employee absences, yet firms may offset these costs by adjusting payroll—for example, lowering wages or reducing headcount—or by retaining experienced workers with high firm-specific productivity who value the benefit. Although our data cannot fully capture all firm responses, we use an event-study design with business registry and matched employer–employee data to examine potential impacts. Specifically, we assess whether the mandate affected firm performance and whether firms exhibited labor supply responses such as greater worker retention or additional sorting among new hires.²⁶ Because the decision to adopt the policy was made at the conglomerate level, these firm-level results should be interpreted with caution and do not provide causal inference.

To evaluate firm performance, we use company-level data on revenue, employment, and revenue per worker from the Business Registry between 2014 and 2020.²⁷ We estimate the following event-study specification:

$$Y_{jy} = \beta_0 + \sum_{k \neq -1} \beta_{1,k} \text{Treat}_j \times D_{y,k} + \sum_{k \neq -1} \beta_{2,k} D_{y,k} + \theta_j + \varepsilon_{jy},$$

where Y_{jy} is the outcome for company j in year y , θ_j is a company fixed effect, and $D_{y,k}$ are event time indicators (with $k = -1$ omitted). Standard errors are clustered at the company level.

Figure 13 shows no statistically significant changes in revenue, employment, or productivity (measured as revenue per employee) following the mandate. Estimates are less precise because standard errors are clustered at the company level, leaving only 23 treated clusters compared with the main analysis. Still, the absence of discernible negative effects suggests that companies may have partially offset costs.

To examine changes in workforce composition, we turn to matched employer–employee data (2015–2020), which provide detailed demographic information not available in the Business Registry.²⁸ Figure 14a shows an increase in the share of male employees aged 30–45 at

²⁶A companion paper, [Chu et al. \(2025\)](#), examines the mandate’s effects on worker sorting.

²⁷While the Business Registry reports firm-level employment, firm-level employment is imputed by Statistics Korea based on establishments’ revenue shares, which introduces measurement error. To minimize this issue, we focus on company-level variables directly reported in the data.

²⁸Because these data include employment start and end dates, we construct biannual observations and adopt the same half-year time unit as in the main analysis. The series begins in 2015h1, since employer–employee matched data are available only from 2015, whereas the company-level outcomes start in 2014.

treated firms post-mandate. This increase appears to be driven primarily by higher retention rather than new hiring. Figure 14b shows a parallel rise in the share of employees who were hired before the policy change and remain employed at the firm in each period, defined as the ratio of age 30–45 men hired before 2017 and still employed at the same firm to the total number of employees in that period. Moreover, male incumbents aged 30–45 at treated firms earned persistently less than their counterparts at control firms (Figure 14c), a gap largely driven by those who remained at the same firm (Figure 14d).²⁹ These patterns suggest that firms may have absorbed part of the mandate’s cost through lower wages while retaining workers who valued the benefit.

Lastly, we find suggestive evidence that the mandate attracted men who valued this benefit. To assess whether the new policy influenced recruitment, we examine the likelihood that newly hired men aged 25–40 had a newborn (or became a first-time father) after joining the firm. We estimate this probability for male new hires in each period, controlling for age and entry-year earnings to account for compositional differences. Because this analysis focuses on new hires, we use the second half of 2016—when the policy was officially announced externally—as the baseline period. Results shows that men who joined treated firms after the policy change were more likely to have a newborn (Figure 15a) and more likely to become first-time fathers (Figure 15b) during their tenure. The immediate increase following the announcement suggests that the effect is driven more by selection—that is, prospective fathers choosing to join treated firms—than by peer effects. If peer effects were the dominant mechanism (e.g., men being influenced by coworkers having children), we would expect similar responses among those hired before the announcement, since they were exposed to the same workplace environment. Importantly, this higher selection does not appear to be driven by wage offers. When we regress log entry-year earnings of new hires, we find no divergence between treated and control firms.³⁰ Taken together, these results suggest that men planning to have children were more likely to join firms offering the new benefit.

Overall, these findings imply that family-friendly workplace policies can help firms both retain and attract workers who value such benefits, even without additional financial compensation. This evidence supports the case for encouraging firms to expand paternity leave access and foster more family-friendly workplace cultures. At the same time, further research is needed to assess whether these recruitment and retention effects would persist if the policy were scaled nationally—a valuable direction for future work.

²⁹Earnings outcomes are measured on an annual basis because earnings are observed only at that frequency.

³⁰During this period, the Korean government raised the minimum wage at an average annual rate of about 10%. Because treated firms are concentrated in retail and hospitality sectors that were more exposed to these changes, we additionally control for industry-by-year fixed effects to account for differential minimum wage impacts.

9 Conclusion

This study leverages a corporate policy change in South Korea—a one-month, fully paid paternity-leave mandate introduced in 2017 at the corporate-group level—to estimate the causal effect of a paternal leave mandate on fertility. We find that the mandate substantially increased fathers’ leave-taking, extended leave durations beyond the required month, and raised fertility by 14.7% among affected employees. The gains were concentrated in dual-earner households and among wives with higher earnings—precisely where time constraints are most binding—and occurred without reducing women’s employment. Complementary survey evidence indicates that the policy fostered more supportive workplace norms around paternal leave, greater paternal involvement in childcare, and stronger fertility intentions. Even among workers at control firms, simply assuming a mandate increased support for leave-taking, reduced perceived career penalties, and raised expected fertility, underscoring the powerful norm-shifting role of the policy. Importantly, we find no evidence of adverse firm-level impacts: productivity and employment remained stable, while male retention and attraction of family-oriented workers increased without additional wage costs.

These findings have broader implications for fertility policy design. Across advanced economies, fertility continues to fall despite decades of interventions—from generous child subsidies to extended maternal or parental leave—each of which carries substantial costs. Cash or tax-transfer programs are fiscally expensive, while extending mothers’ already lengthy leaves can backfire by slowing their career progression and raising firms’ costs from prolonged absences. In contrast, a short, one-month paternity-leave mandate represents a low-cost yet effective alternative: it raised fertility, improved workplace retention, and shifted long-standing social norms about fathers’ caregiving. Changing norms is typically a slow and difficult process, yet this minimal intervention achieved it through a one-month of leave requirement.

Despite these encouraging results, several questions remain. Scaling such mandates nationally would require evaluating variation in firms’ capacity to accommodate fathers’ absences, especially among smaller employers. Future work should also assess longer-term effects on wages, recruitment, and workforce composition, as well as potential spillovers if competing firms adopt similar policies. Finally, if normalizing fathers’ caregiving also narrows gender wage or promotion gaps by reducing the stigma associated with mothers’ absences, the benefits of paternity-leave mandates could extend well beyond fertility itself.

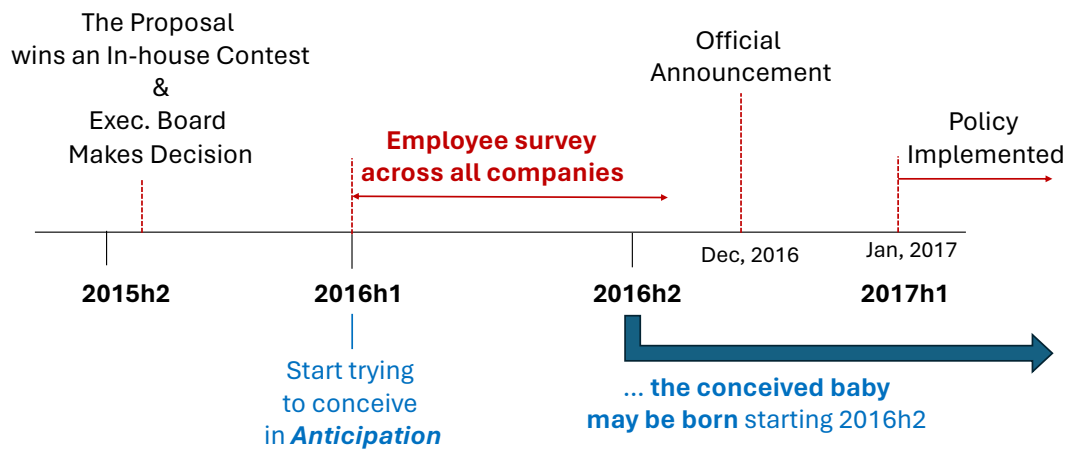
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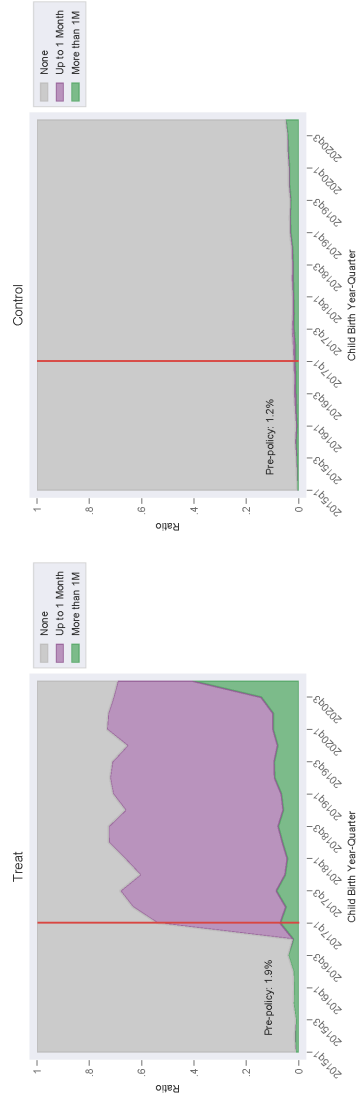
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Figure 1: Paternity Leave Mandate Policy Introduction



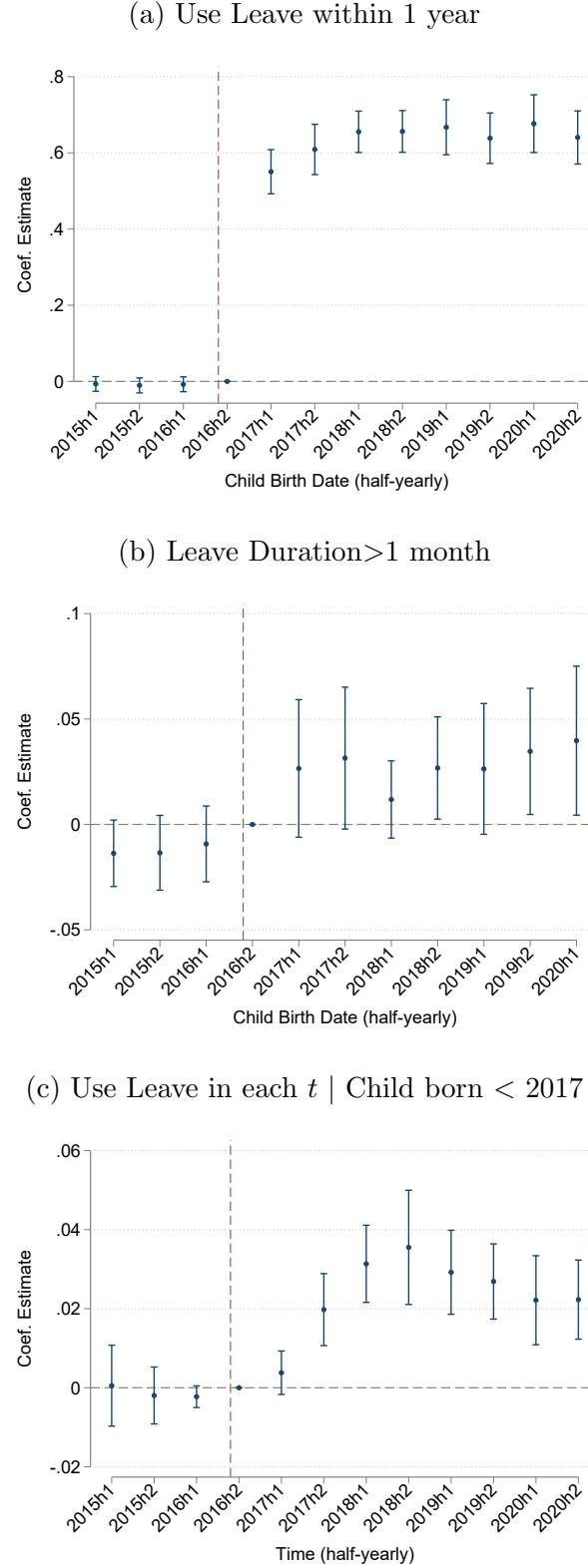
Note: The graph illustrates the timeline of C Group's corporate policy change, along with the expected timeline for conceived births among male incumbents in response to the new corporate paternity leave mandate. See Section 2.2 and Section 5.2 for further details.

Figure 2: Leave Utilization among Newborn Fathers



Note: The stacked area chart illustrates the proportion of fathers with newborns who took no leave (gray), up to one month of leave (purple), or more than one month of leave (green). The treated group consists of fathers employed by C group companies, while the control group includes fathers working at other conglomerates designated by the Korea Fair Trade Commission in the same industry as C group.

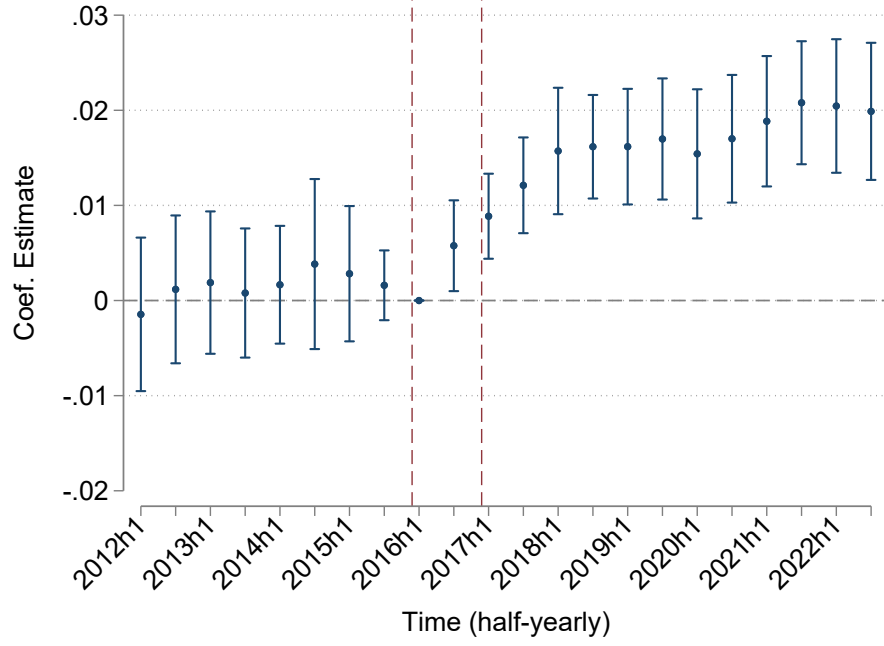
Figure 3: The Effect of the Mandate on Paternity Leave Taking



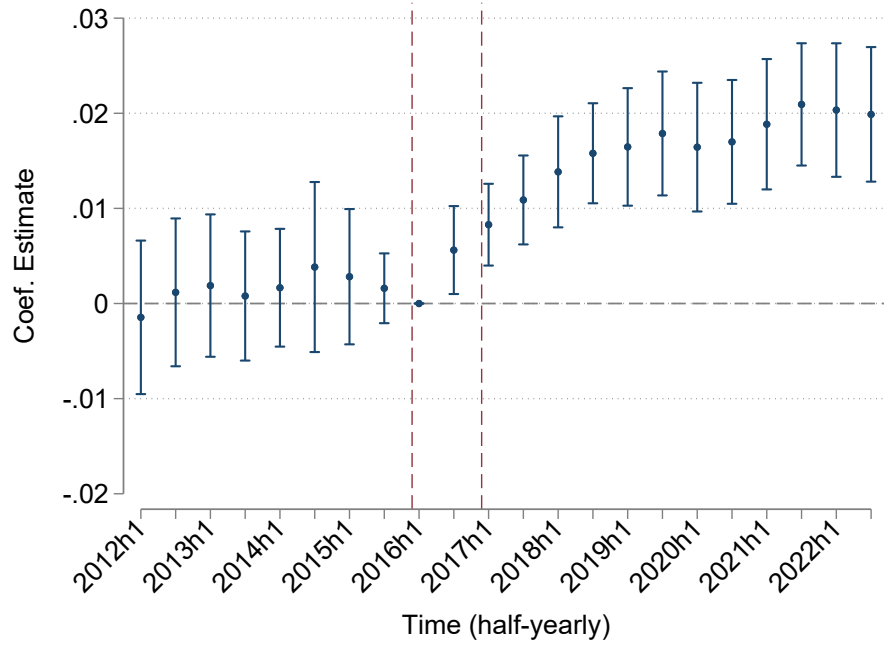
Note: The figures present event study estimates of the effects of the paternity leave mandate on male employees' leave-taking behavior. Panel (a) and (b) plot event time coefficients for fathers whose child was born in each period (x-axis) with (a) showing the probability of taking any paternity leave within one year of childbirth, and (b) the probability of taking leave longer than one month. Panel (c) shows leave usage in each period for fathers whose child was born before 2017. See Table 3 for more detail. Robust standard errors are clustered at the base

Figure 4: The Effect of the Paternity Leave Mandate on Fertility

(a) Probability to have an infant



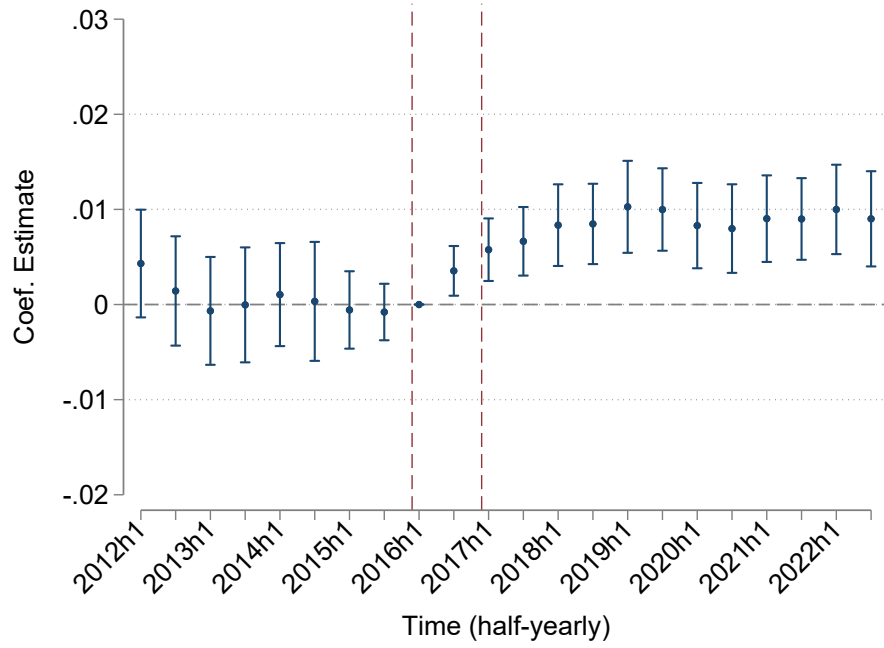
(b) Probability to have an infant and still employed at the same company



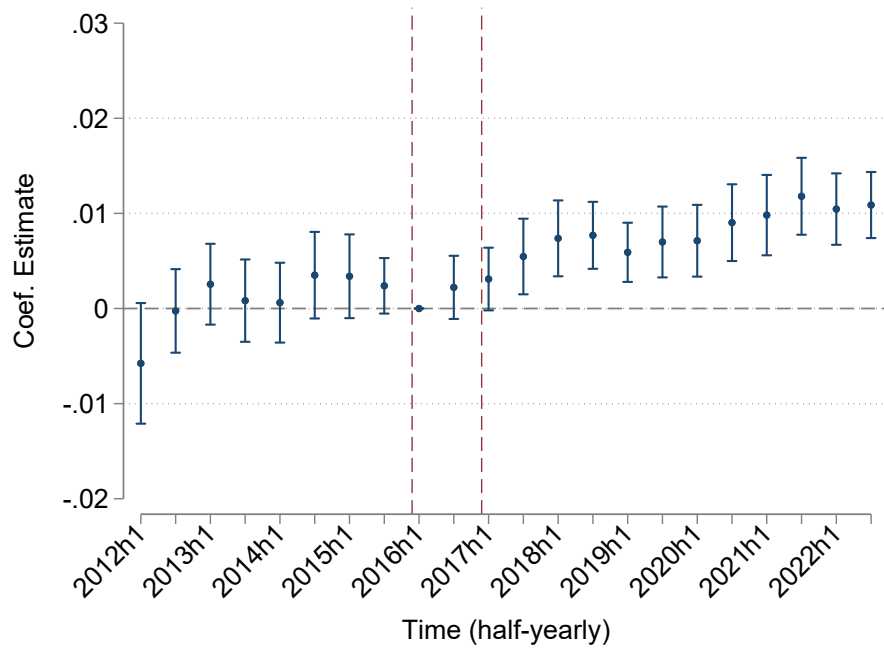
Note: The figures show event study estimates of the effects of the paternity leave mandate on the probability that male incumbents have an infant (age 0) in each period. Panel (a) uses an indicator for having an infant in each and (b) an indicator for having an infant and remaining at the same baseline firm. Each point plots the estimated coefficients corresponding to $\beta_{2,k}$ in Equation 3 and associated 95% confidence intervals. Robust standard errors are clustered at the baseline firm level.

Figure 5: The Effect of the Paternity Leave Mandate on Fertility by Birth Parity

(a) Probability to have an infant & first child



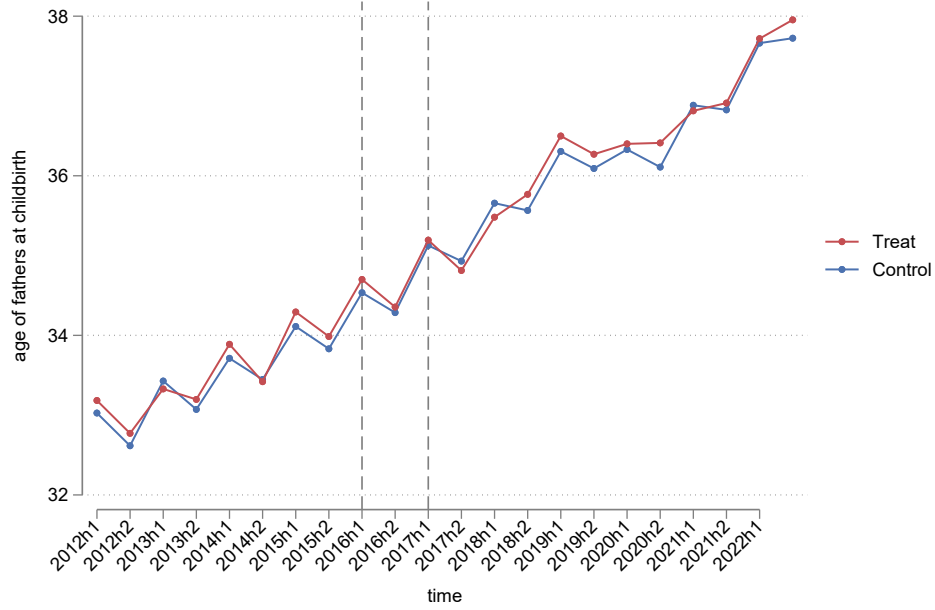
(b) Probability to have an infant & second or higher order birth child



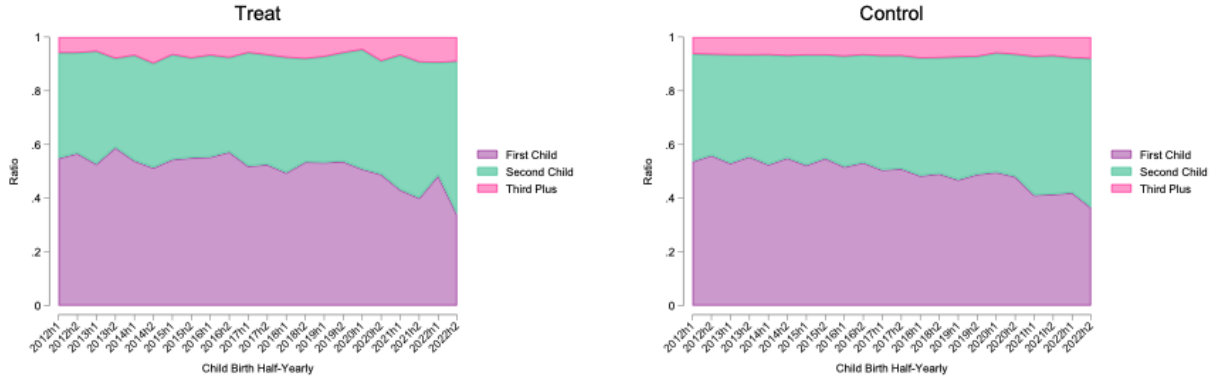
Note: The figures show event study estimates of the effects of the paternity leave mandate on the probability that male incumbents have an infant (0 years old) in a given period. Panel (a) uses an indicator for first-born infants and (b) an indicator for second or higher-order infants. Each point shows the estimated coefficients, corresponding to $\beta_{2,k}$ in equation 3 and associated 95% confidence intervals. Robust standard errors are clustered at the baseline firm level.

Figure 6: Average Paternal Age at Childbirth and Distribution of Birth Orders

(a) Average paternal age at child birth

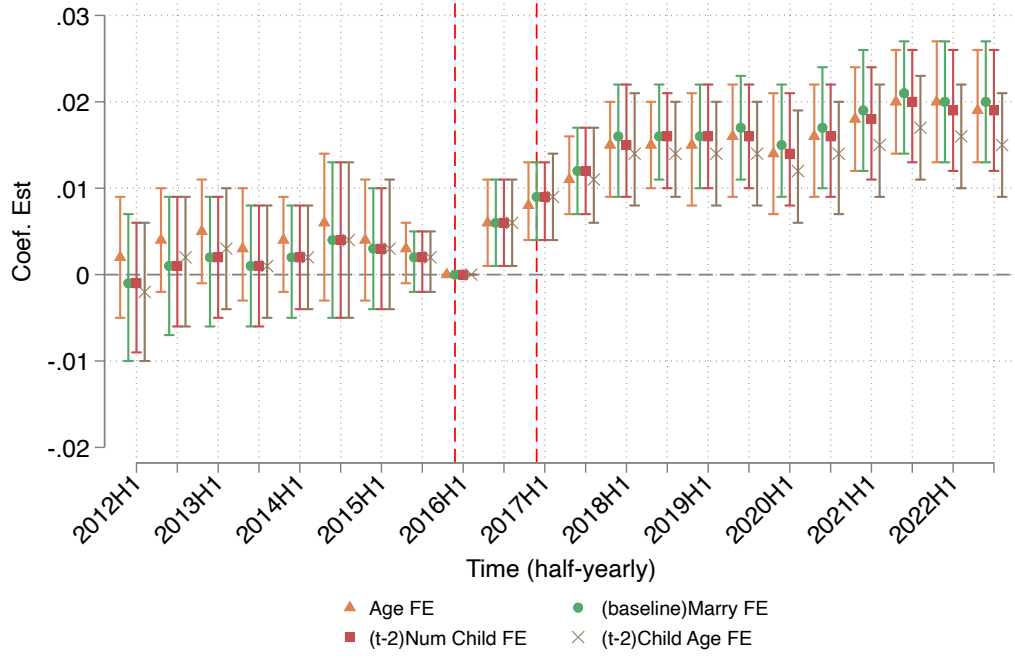


(b) The distribution of birth orders



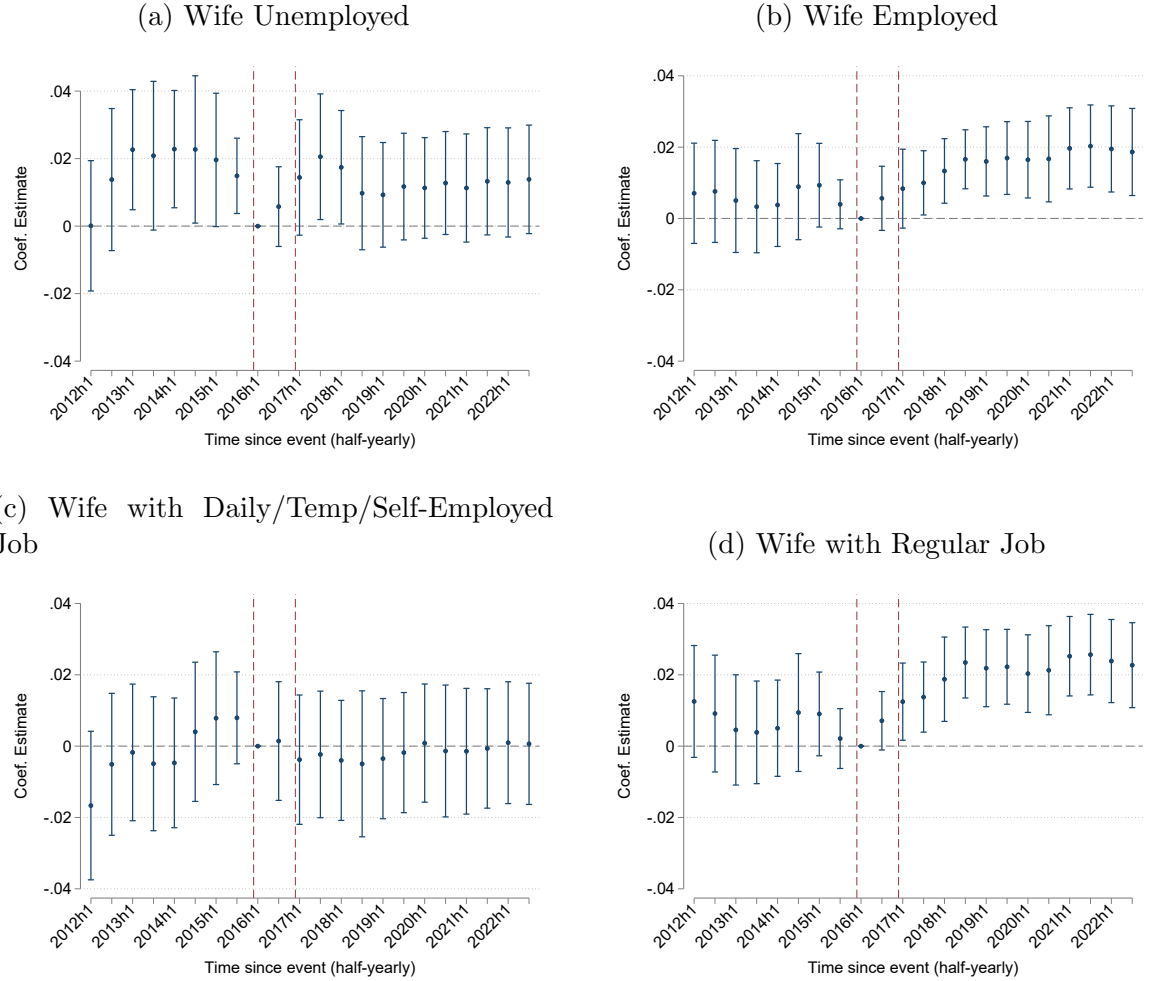
Note: Panel (a) plots the average paternal age at birth in each period for treated and control groups. Panel (b) shows the distribution of birth order among fathers with newborns—first births (purple), second births (green), and third or higher-order births (pink)—for treated and control firms in each period.

Figure 7: The Effect of the Paternity Leave Mandate on Fertility: Robustness



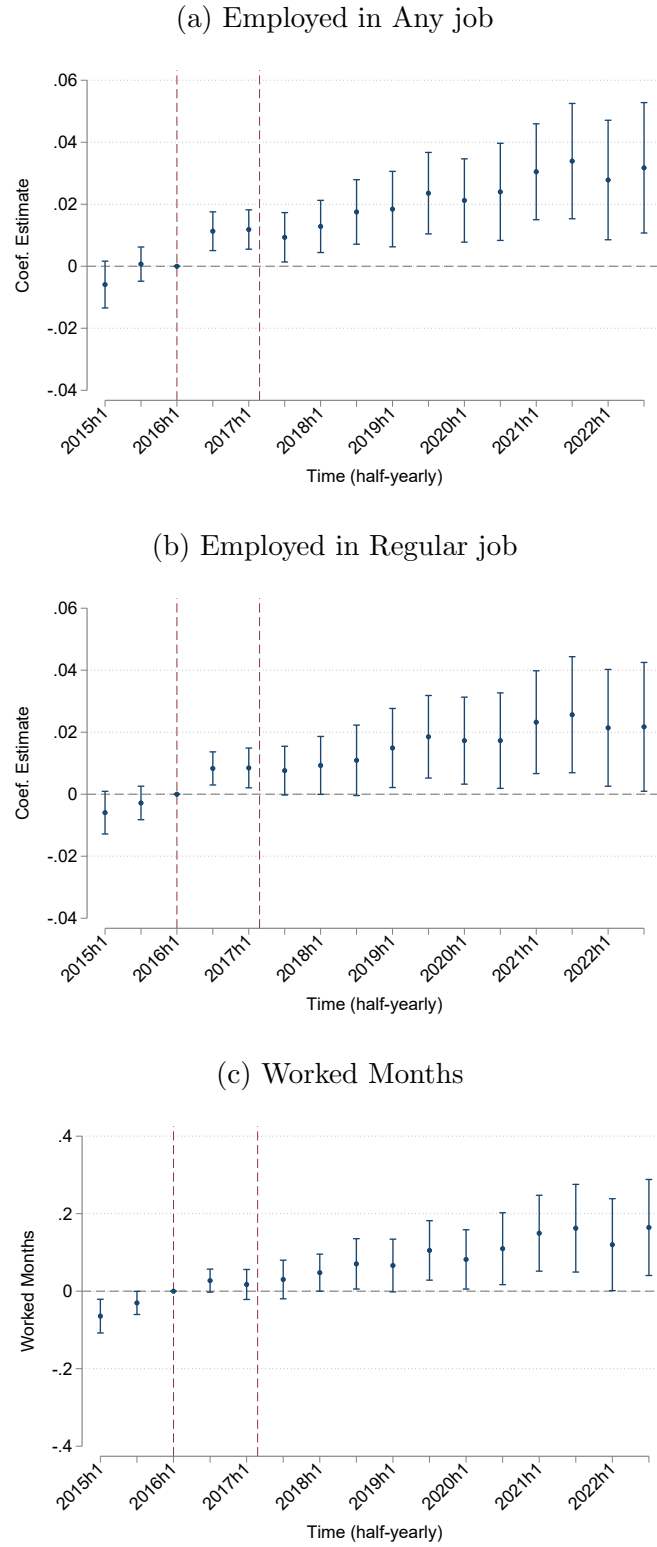
Note: The figures show event study estimates of the effects of the paternity leave mandate on the probability that male incumbents have an infant (0 years old) in a given period, using alternative sets of time-varying fixed effects. Each point plots the estimated coefficients corresponding to $\beta_{2,k}$ in Equation 3 and associated 95% confidence intervals. See Table 6 for included controls in each specification. Robust standard errors are clustered at the baseline firm level.

Figure 8: The Effect of the Paternity Leave Mandate on Fertility by Wife Employment



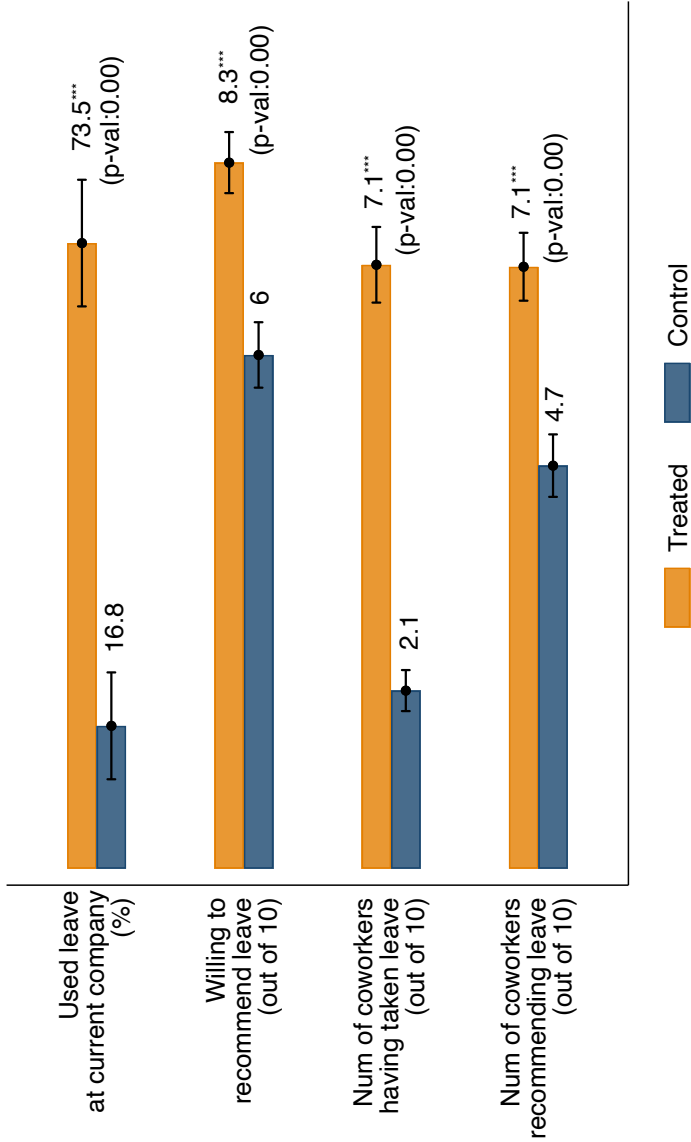
Note: The figure shows event-study estimates of the paternity leave mandate's effect on spousal labor supply of male incumbents. Panel (a) restricts to incumbents whose spouses were not employed at baseline; Panel (b) to those with spouses employed in any job; Panel (c) to those with spouses in daily, temporary, or self-employed work; and Panel (d) to those with spouses in wage and salary jobs in 2015. Each point plots the estimated coefficients $\beta_{2,k}$ from equation 3, with 95% confidence intervals. Standard errors are clustered at the baseline firm level.

Figure 9: The Effect of the Paternity Leave Mandate on Spousal Labor Supply



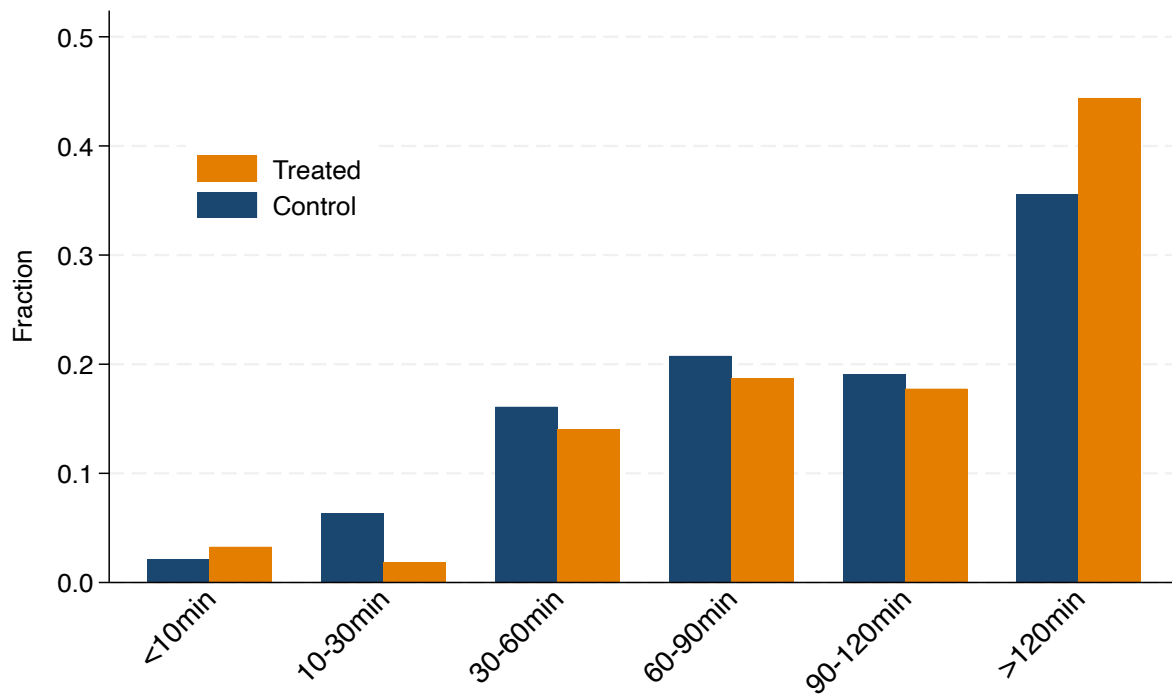
Note: The figure shows event-study estimates of the paternity leave mandate's effect on wives' labor supply. The sample includes wives matched to male incumbents in 2015. Panel (a) reports estimates for an indicator of employment in any job, Panel (b) for employment in wage and salary jobs, and Panel (c) for total months worked in biannual period t . Each point plots the estimated coefficient $\beta_{2,k}$ from equation 3, with 95% confidence intervals. Standard errors are clustered at the husband's baseline firm level. See Table 10 for additional results.

Figure 10: Paternity leave usage and attitudes toward paternity leave



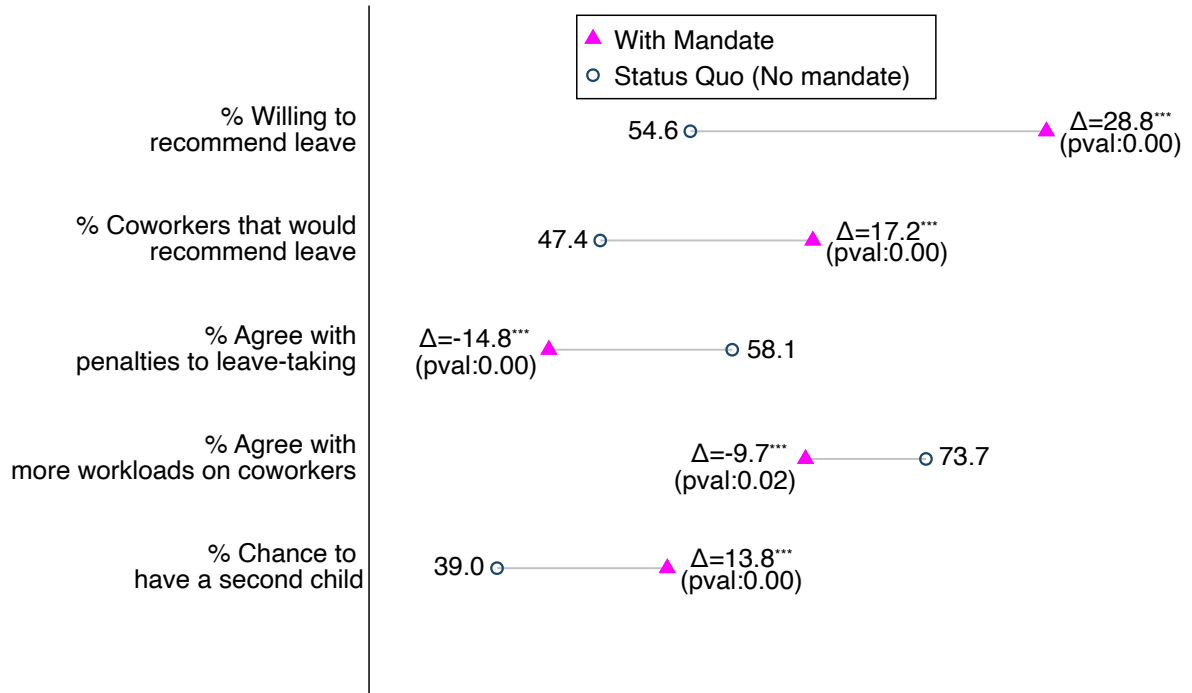
Note: The figure shows mean responses of treated and control individuals to survey questions on paternity leave-taking at their current companies. The y-axis lists each question, and bars represent group means with 95% confidence intervals shown as capped black lines. Asterisks next to the treated-group means indicate statistical significance of the difference from controls at the 1% level, with corresponding p-values reported in parentheses. See Section 7 for details.

Figure 11: Weekday time spent on childcare



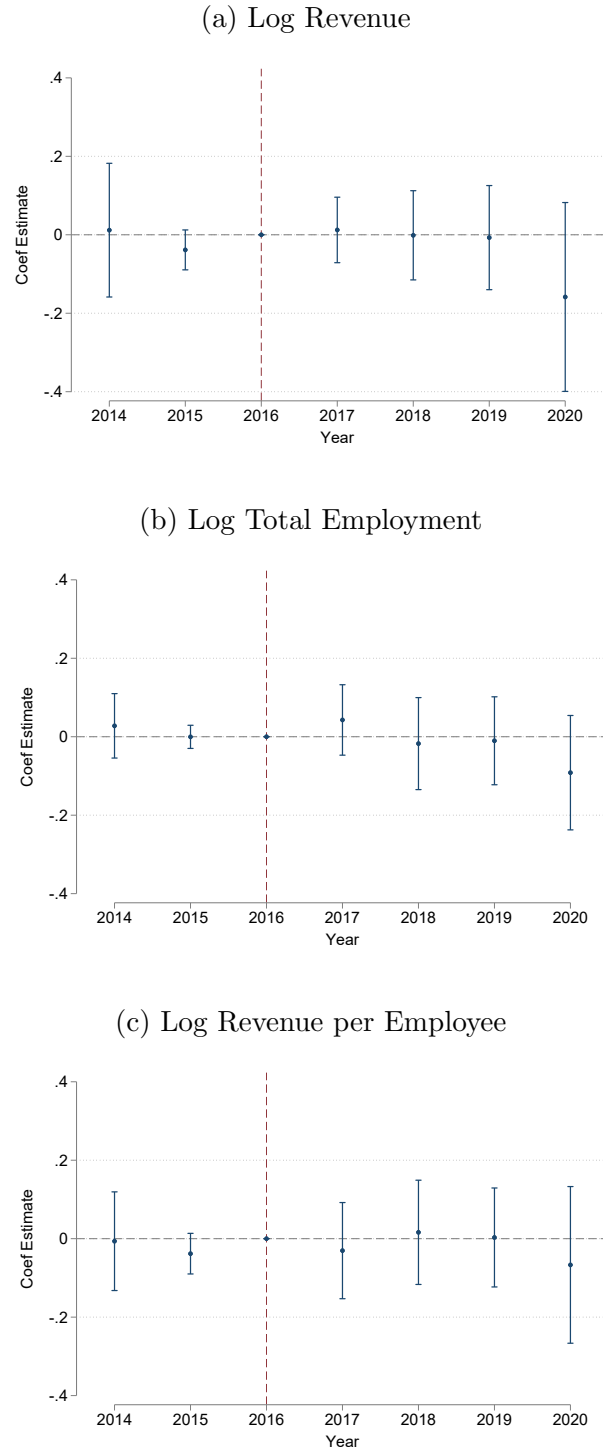
Note: The figure shows the distribution of responses to the survey question on weekday childcare time, comparing treated and control groups. Each bar represents the share of respondents in a given time category. See Section 7 for details.

Figure 12: Effects of a Mandate Scenario on Leave Norms and Subsequent Fertility



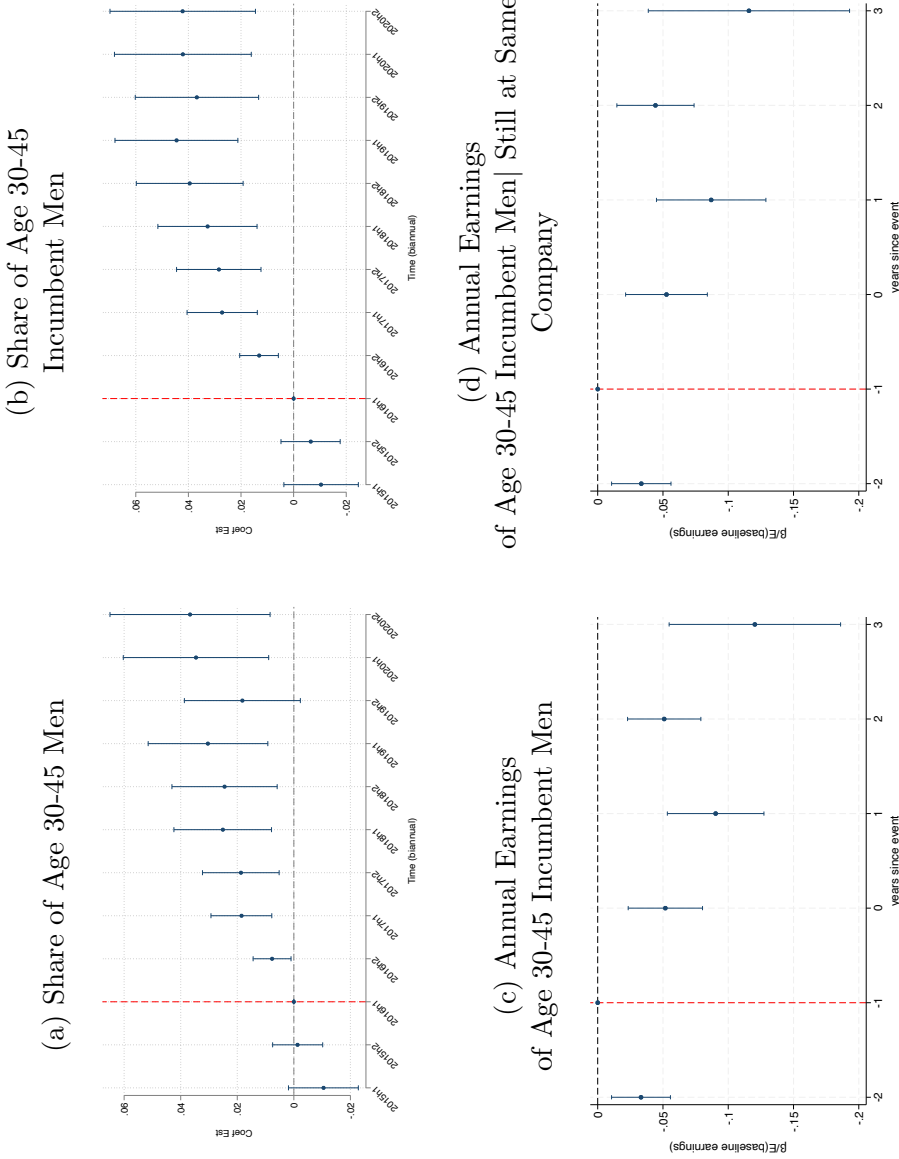
Note: The figure reports control-group responses to a vignette about a hypothetical coworker (“Mingyu”). For each outcome, the hollow circle shows the mean response under the status quo (no mandate), and the magenta triangle shows the mean response when the same question is answered assuming a mandate is in place. The average within-respondent difference is labeled as Δ , with p-values in parentheses. Asterisks (***) indicate significance at the 1% level. The survey questions are summarized on the y-axis. See Section 7 for details.

Figure 13: Company Level Outcomes



Note: The figures present event study estimates of the effect of the paternity leave mandate on company level outcomes. The sample consists companies owned by conglomerates. The outcome variables are observed at annual frequency on Business Registry 2014-2020, and are log revenue (Panel a), log total employment size (Panel b), and log revenue per employee (Panel c). The Treat dummy indicates whether a company is affected by the corporate policy change in 2017. We control for year dummies and company fixed effects. Each point represents the estimated coefficient on the interaction between the treatment indicator and year dummies (as shown on the x-axis) along with the associated 95% confidence intervals. Robust standard errors are clustered at the company level.

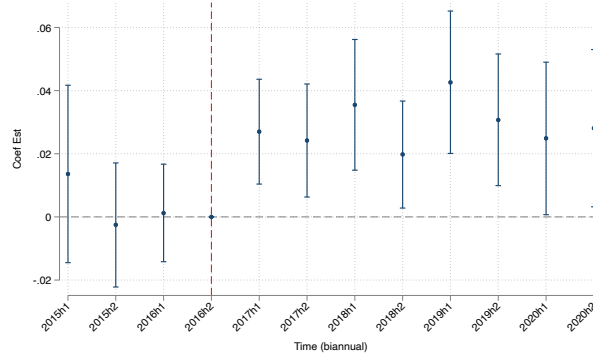
Figure 14: Effects on Firm Compositions and Employee Earnings



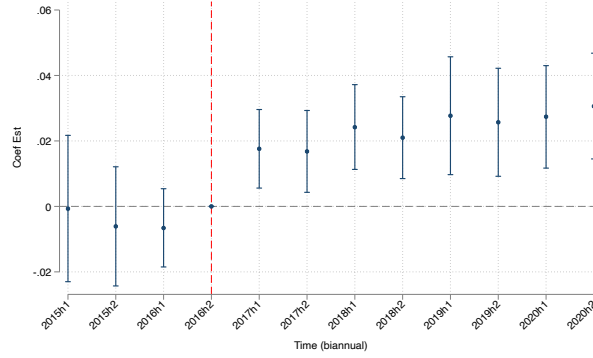
Note: The figures present event study estimates of the effect of the paternity leave mandate on firms. The sample consists of firms owned by conglomerates (Figure (a)-(b)) and age 30 to 45 male employees hired before 2017 (Figure (c)-(d)). In Figure (a)-(b), the outcome variables are aggregated at biannual frequency based on Matched Employee Employer data. In Figure (a), the outcome variable is the share of age 30-45 male employees among current employees in each given time period; In Figure (b), the outcome variable is the share of age 30-45 male employees who were hired before 2017 among current employees in each given time period. In Figure (c), the outcome variable is annual earnings of age 30-45 male and we control for individual fixed effects. In Figure (d), we estimate the same regression as (c) but using employees who are still working at the same baseline company in a given period. The Treat dummy indicates whether a firm is affected by the corporate policy change in 2017. We control for biannual calendar time dummies and firm fixed effects. Each point represents the estimated coefficient on the interaction between the treatment indicator and biannual calendar time dummies (as shown on the x-axis) along with the associated 95% confidence intervals. Robust standard errors are clustered at the firm level.

Figure 15: Effects on Fertility and Entry-Year Earnings of Male New Hires

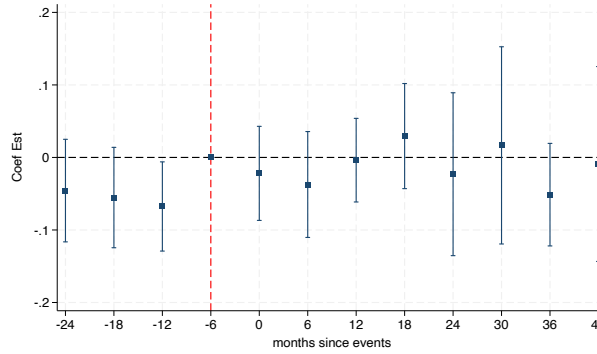
(a) Probability to have a baby



(b) Probability to become a father



(c) Log Entry-Year Earnings (Annualized)



Note: The figures present event study estimates of the effect of the paternity leave mandate on fertility and salary outcomes for male new hires aged 25–40. The sample includes men entering firms in each period. Panel (a) reports effects on an indicator for having a newborn while employed at the firm; Panel (b) reports effects on an indicator for becoming a father upon entry and while at the firm; and Panel (c) reports effects on log entry-year earnings (annualized). The treatment indicator equals one for firms covered by the corporate policy change in 2017. Specifications in Panels (a) and (b) control for biannual calendar time dummies, firm fixed effects, age at entry, and for entry-year earnings (annualized). Panel (c) additionally controls for industry-by-year fixed effects. Each point represents the estimated coefficient on the interaction between treatment status and biannual time dummies (x-axis), with 95% confidence intervals. Robust standard errors are clustered at the firm level.

Table 1: Federal Parental Leave Policy (2015-2022)

Condition		Eligible while child is aged 0–8 years; maximum duration: 12 months			
Earnings Replacement Basis		Calculated based on maximum benefit cap for second parental leave user for the same child.			
Year		2015–2017	2018	2019	2022
1–3 months	Maximum Monthly Benefit (KRW)	1.5 million	2.0 million	2.5 million	3.0 million
	<i>Benefit Relative to Average Post-tax Earnings (%)</i>				
	Male Employees, ages 30–39 (2.9 million)	52%	69%	86%	100%
	C group Male Employees, ages 30–45 (4.2 million)	36%	48%	60%	71%
4–12 months	Maximum Monthly Benefit (KRW)	1.0 million	1.0 million	1.2 million	1.5 million
	<i>Benefit Relative to Average Post-tax Earnings (%)</i>				
	Male Employees, ages 30–39 (2.9 million)	34%	34%	41%	52%
	C group Male Employees, ages 30–45 (4.2 million)	24%	24%	29%	36%

Note: This table summarizes the federal parental leave scheme including maximum monthly benefits and income replacement rates by year. Replacement rates are calculated as the percentage of average annual post-tax earnings of male employees aged 30 to 39 and of male employees aged 30 to 45 at C Group during the observation period.

Table 2: Descriptive statistics

	(1) Treat mean (sd)	(2) Control mean (sd)	(3) (1) - (2) mean (se)
<i>Panel A: Firm Characteristics</i>			
Number of Firms	104	1928	
Employment Size	456.3 (1289.2)	436.2 (1997.3)	20.1 (133.8)
Revenue per Employee (1 billion KRW)	1.05 (0.80)	1.02 (2.00)	-0.03 (0.18)
Average Monthly Pay (10,000KRW)	385.4 (129.2)	599.3 (210.7)	-213.9*** (36.0)
Paternity Leave Usage	1.97% (13.9)	1.21% (11.1)	0.007* (0.4)
<i>Panel B: Individual Characteristics</i>			
Number of Individuals	17,366	378,689	
Age	37.0 (4.5)	37.2 (4.6)	-0.3 (0.2)
Monthly Earnings (10,000KRW)	509.6 (172.0)	680.9 (280.0)	-171.3*** (34.8)
Tenure (years)	8.03 (4.95)	8.62 (5.47)	-0.59 (0.31)
Birth Rate (Pre-policy)	0.10	0.11	-0.01***
Married	0.60	0.68	-0.09***
Wife Age Married	35.5 (4.5)	35.5 (4.5)	0.00 (4.5)
<i>Number of Children</i>			
Childless	0.40	0.33	0.07***
One Child	0.24	0.25	0.01
Two or more	0.36	0.41	-0.05***

Note: Panel A reports baseline characteristics of firms in the treated and control groups. Panel B reports baseline characteristics of male employees aged 30–45 who were employed at these firms in the first half of 2016. Columns 1 and 2 show means and standard deviations for treated and control groups, and Column 3 reports the mean difference with standard errors. Firm size is measured using matched employer–employee data, revenue per employee from the Business Registry, age, earnings, and tenure from the Employer–Employee dataset, child information from the 2015 Child Registry, and marriage status from the 2015 Population Census. *** indicates significance at the 1% level.

Table 3: Paternity Leave Utilization: Differences-in-differences estimates

Dep. Var.	Leave Used within 1 year	1(Leave > 1 month)	1(Leave > 1 month) child born < 2020H2	Leave Used in each t child born < 2017
	(1)	(2)	(3)	(4)
Treat \times Post	0.645*** (0.026)	0.056*** (0.008)	0.039*** (0.009)	0.024*** (0.003)
Pre Avg (Treat)	0.0197	0.0191	0.0191	0.0091
Observations	182,644	182,644	173,121	1,409,187
Unit	Individual	Individual	Individual	Individual \times Time
Child Birth Date FE	Y	Y	Y	
Time (Bi-annual) FE				Y
Firm FE	Y	Y	Y	Y

Note: This table presents difference-in-differences estimates of paternity leave utilization by fathers of newborns employed at C group firms (treated) and other conglomerates (controls). The Treat dummy indicates C group firm employees, and the Post dummy indicates periods since the first half of 2017, when the policy was first implemented. Columns 1–3 use samples of fathers with children born in each corresponding period. Column 1 estimates the probability of taking any paternity leave within one year of childbirth. Columns 2 and 3 estimate the probability of taking more than one month of leave, with Column 3 restricting the sample to births before the second half of 2020. Column 4 uses a sample of fathers with children born before 2017 and regresses an indicator for any leave-taking during a given period. The unit of analysis is the individual in Columns 1–3, and an unbalanced individual-by-time panel in Column 4. Columns 1–3 control for bi-annual childbirth fixed effects and firm fixed effects. Column 4 includes bi-annual time fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. *** indicates significance at the 1%.

Table 4: Probability to have an infant: Differences-in-differences estimates

Dep Var	(1) 1(Infant)	(2) 1(Infant) × 1(same company)	(3) 1(Infant) × 1(First child)	(4) 1(Infant) × 1(Second+ child)
Treat x Post	0.014*** (0.003)	0.014*** (0.003)	0.008*** (0.002)	0.007*** (0.002)
Observations	8,713,232	8,713,232	8,713,232	8,713,232
Pre-policy Mean (Treat)	0.095	0.095	0.047	0.047
% rel. to Pre-Mean	14.7%	14.7%	17.0%	14.9%
Included Controls	Age, (baseline) Tenure			
Included Fixed Effects	Calendar Time, (baseline) Income Quintile, (baseline) Firm			

Note: This table reports differences-in-differences estimates from a linear probability model assessing the impact of a corporate policy change on the probability that male incumbents aged 30–45 have an infant. The sample includes men employed at subsidiary firms under conglomerates as of the first half of 2016. The Treat indicator identifies individuals at firms subject to the corporate policy change, while the control group comprises men at firms in other conglomerates. The interaction of Post and Treat indicators captures differential time trends between these groups. Column 1 reports estimates for having an infant; Column 2 adds the condition of remaining at the same baseline company; Column 3 focuses on having an infant who is the first child; and Column 4 covers having an infant of second or higher parity. All specifications control for biannual calendar time dummies, baseline monthly earnings quintile dummies, and baseline firm dummies. Robust standard errors clustered at the baseline firm level are reported in parentheses. *** indicates significance at the 1% level.

Table 5: Parental Age, Birth Order, and Age Difference: Differences-in-differences estimates

	(1) Father Age	(2) Mother Age	(3) Age Gap
Treat \times Post	-0.033 (0.084)	-0.024 (0.084)	0.003 (0.045)
Treat	0.118 (0.178)	-0.060 (0.166)	0.063 (0.053)
Post	2.029*** (0.043)	0.310*** (0.031)	0.539*** (0.013)
Constant	33.519*** (0.080)	33.109*** (0.076)	2.459*** (0.012)
Observations	255,834	255,834	116,255

Note: This table reports differences-in-differences estimates for parental age and the age gaps between children in households with a newborn in each period. The sample comprises households of male incumbents aged 30–45 who were employed at firms under C Group or other conglomerates as of the first half of 2016 and who subsequently had a newborn. The Treat indicator identifies firms affiliated with C Group, while Post marks the time since the first half of 2016, when information about the policy began circulating. The interaction term $Treat \times Post$ captures differential trends in these outcomes among treated households. Columns 1 and 2 estimate the effects on the ages of the father and mother of the newborn, respectively. Column 3 estimates the age difference between the newborn and the next oldest child (for non-firstborns). Robust standard errors, clustered at the baseline firm level, are reported in parentheses. *** indicates significance at the 1% level.

Table 6: Probability to have an infant: Differences-in-differences estimates

Dep Var	(1) 1(Infant)	(2) 1(Infant)	(3) 1(Infant)	(4) 1(Infant)
Treat x Post	0.014*** (0.003)	0.014*** (0.003)	0.013*** (0.003)	0.012*** (0.002)
Observations	8,713,232	8,713,232	8,713,232	8,713,232
Pre Avg (Treat)	0.095	0.095	0.095	0.095
% relative to Pre Avg	14.7%	14.7%	13.7%	12.6%
Included Controls	Age, (baseline) Tenure (baseline) (t-2) (t-2)			
Additional Fixed Effects	Age	Marital Status	Num. of children	Age of youngest child)
Included Fixed Effects	Time, (baseline) Income Quintile, Firm			

Note: This table presents differences-in-differences estimates from a linear probability model examining the effect of a corporate policy change on the likelihood that male incumbents aged 30–45 have an infant. The sample includes men employed at subsidiary firms under conglomerates in the first half of 2016. The *Treat* indicator identifies firms affected by the corporate policy change, while the control group consists of men employed at firms belonging to other conglomerates. The interaction term $Treat \times Post$ captures differential trends in fertility outcomes across groups. All columns use an indicator for having an infant as the dependent variable. All specifications control for bi-annual calendar time dummies, baseline monthly earnings quintile dummies, and baseline firm dummies. Column (1) additionally controls for age group dummies (rather than the continuous measure used in the main specification). Column (2) controls for a baseline married dummy. Column (3) includes dummies for the number of children at time $t - 2$ (0, 1, or 2+). Column (4) includes dummies for the age category of the youngest child at time $t - 2$ (no child, 0, 1–4, 5–7, or 8+ years). Robust standard errors are clustered at the baseline firm level and reported in parentheses. *** indicates significance at 1% level.

Table 7: Descriptive statistics: Households Characteristics

	(1) Treat mean (sd)	(2) Control mean (sd)	(3) (1) - (2) mean (se)
Wife age	36.61 (4.52)	36.65 (4.51)	-0.05 (0.18)
Husband age	38.20 (4.24)	38.16 (4.29)	0.00 (0.19)
Husband tenure	107.79 (61.68)	113.64 (67.23)	-5.49 (4.33)
Husband monthly earnings (10,000KRW)	545.64 (173.28)	721.56 (281.23)	-174.78*** (36.84)
Wife annual earnings (10,000KRW)	1,213.16 (1,947.08)	1,371.23 (2,430.08)	-157.06 * (95.38)
Wife annual earnings Employed	1,865.73 (2,148.21)	2,297.91 (2,786.88)	-429.18 *** (152.93)
<i>Employment Status in 2015</i>			
Wife with no job	0.35	0.40	-0.05***
Wife with any job	0.65	0.60	0.05***
Wife with regular job	0.47	0.45	0.02

Note: The table reports baseline household characteristics by treatment status. Columns 1 and 2 present the mean and standard deviation for treated and control incumbent households with matched spouse information. Age, monthly earnings, annual earnings data are sourced from the Employment Registry data. Information on marriage are drawn from the 2015 Population Census.

Table 8: Probability to have an infant by Wife Employment

Sample	(1) No spouse id	(2) Wife unemployed	(3) Wife employed	(4) Wife daily/temp/self-employed	(5) Wife regular wage job
Treat x Post	-0.003 (0.004)	-0.003 (0.005)	0.010*** (0.003)	-0.000 (0.005)	0.014*** (0.004)
Pre Avg (Treat)	0.029	0.164	0.122	0.113	0.126
% relative to Pre Avg	-10.3%	-2.9%	8.2%	-0%	11.1%
R-squared	0.022	0.104	0.070	0.081	0.068
Observations	2785054	2363658	3567520	884158	2683362
Included Controls	Age, (baseline) Tenure				
Include FE	Time, (baseline) Earnings Quintile, (baseline) Establishment				

Note: This table presents differences-in-differences estimates from a linear probability model examining the effect of a corporate policy change on the likelihood of male incumbents aged 30-45 having an infant. The sample include men employed at subsidiary firms under conglomerates in the first half of 2016. The Treat dummy indicates whether an individual works for firms affected by the corporate policy change, with the control group consisting of men working at firms belonging to other conglomerates. The interaction of Post and Treat indicators tests for differential time trends between these groups. All columns regress on an indicator for having an infant. In Column 1, the sample includes incumbents that are not matched to spouses at baseline. In Column 2, the sample includes incumbents whose matched spouses were not employed at the baseline. In Column 3 to 5, the sample includes incumbents whose matched spouses were employed in (3) any jobs, (4) daily, temporary contract jobs, or self-employed, and (5) wage and salary job in 2015. We control for bi-annual calendar time dummies, baseline monthly earnings quintile dummies, and baseline firm dummies. Robust standard errors are clustered at the baseline firm level and reported in parentheses. *** indicates significance at 1% level.

Table 9: Probability to have an infant by Wife Employment Characteristics

Wife Characteristics	Annual Earnings		Tenure (months)	
	(1)	(2)	(3)	(4)
Sample	Employed wives	All wives	Employed wives	All wives
Dep. Var	1(Infant)	1(Infant)	1(Infant)	1(Infant)
Treat x Post x $\tilde{\lambda}_i$	0.008** (0.003)	0.011** (0.003)	0.009*** (0.002)	0.006*** (0.002)
Treat x Post	0.007* (0.004)	0.004 (0.004)	0.002 (0.003)	0.005 (0.003)
Treat x $\tilde{\lambda}_i$	-0.003 (0.002)	-0.007*** (0.002)	-0.006*** (0.002)	-0.006*** (0.001)
Post x $\tilde{\lambda}_i$	0.015*** (0.001)	0.022*** (0.002)	-0.018*** (0.001)	-0.001 (0.001)
Pre Avg(Treat)	0.12	0.14	0.12	0.14
% change associated with 1SD increase	6.7%	7.9%	7.5%	4.3%
Observations	3,567,520	5,931,178	3,567,520	5,931,178
Included Controls	Husband age, (baseline) Husband tenure, (baseline) Husband Earnings Quintile			

Note: This table presents difference-in-differences estimates from a linear probability model examining the effect of a corporate policy change on the likelihood of male incumbents aged 30–45 having an infant. The sample includes men employed at subsidiary firms under conglomerates in the first half of 2016 and whose matched spouses were employed in 2015. The Treat dummy indicates whether an individual works for firms affected by the corporate policy change, with the control group consisting of men employed at firms in other conglomerates. All columns regress on an indicator for having an infant. In Columns 1 and 3, the sample is restricted to dual-earner households where the wife was employed in 2015; Columns 2 and 4 use the full sample of married incumbents. Columns 1 and 2 interact the treatment effect with the wife's total baseline annual earnings, while Columns 3 and 4 use her tenure at baseline. Both characteristics are normalized by their sample standard deviation for ease of interpretation. We control for bi-annual calendar time dummies, baseline monthly earnings quintile dummies, and baseline firm dummies. Standard errors are clustered at the firm level. *** indicates significance at 1% level.

Table 10: Effects on Wives' Labor Market Outcomes

Panel A: Wives of employees in main sample			
	(1) 1(any job)	(2) 1(regular job)	(3) Worked months
Treat \times Post	0.023*** (0.007)	0.019*** (0.007)	0.12*** (0.040)
Pre avg treated	0.45	0.4	2.79
% rel. to pre avg	5.1%	4.8%	4.3%
Observations	4,313,584	4,313,584	4,313,584
Demographic Controls: wife age, husband age, (baseline) husband tenure			
Included FE: time, (baseline) husband firm, (baseline) husband earnings quintile			
Panel B: Wives giving births in each quarter			
	1(any job) in 1 year since birth	1(any job) in 2 years since birth	1(any job) in 3 years since birth
Treat \times Post	0.016 (0.014)	0.009 (0.014)	0.005 (0.014)
Treat	0.029** (0.012)	0.049*** (0.012)	0.042*** (0.012)
Pre avg treated	0.47	0.45	0.44
% rel. to pre avg	3.4%	2%	1.1%
Observations	129,631	129,631	129,631
Demographic Controls: wife age, husband age, first child indicator, husband earnings			
Included FE: child birth date (biannual)			

Note: This table reports difference-in-differences estimates of the paternity-leave mandate's effect on wives' labor supply. The sample includes wives matched to male incumbents employed in 2015 at either treated or control firms. Column (1) reports results for an indicator of employment in any job, Column (2) for employment in a regular job (defined as wage or salary employment with contract duration longer than one month), and Column (3) for the total number of months worked in biannual period t . All specifications include biannual time fixed effects, baseline husband firm fixed effects, and baseline husband earnings-quintile fixed effects. Standard errors are clustered at the husband's baseline firm level. *** indicates significance at the 1% level, ** at 5%, and * at 10%.

Table 11: Descriptive Statistics of Survey Sample

	(1)	(2)	(3)	(4)
	Treated	Control	Difference	SE of Difference
Number of Respondents	214	236	450	
Age	39.04	38.21	0.84*	0.38
Married	0.82	0.75	0.07	0.04
Number of Children	0.98	0.96	0.02	0.08
Have an Infant	0.19	0.13	0.06	0.03
Have Age \leq 8 old	0.55	0.48	0.06	0.05
Wife Employed in Wage Job	0.51	0.45	0.06	0.05
Wife Non-Employed	0.23	0.23	0.00	0.04
Monthly Income \geq 6M KRW	0.30	0.50	-0.20***	0.05
Wife Monthly Income \geq 5M KRW	0.13	0.14	-0.01	0.03
Leave Used in current company	0.74	0.17	-0.57***	0.05
Tenure (years)				
Less than 1 year	0.02	0.01	0.01	0.01
1–4 years	0.23	0.16	0.07	0.05
5–8 years	0.19	0.21	-0.02	0.06
9 years or longer	0.56	0.61	-0.05	0.05
Job Level				
Staff/Associate	0.28	0.32	-0.04	0.04
Team Manager	0.22	0.15	0.07	0.04
Middle Manager	0.50	0.53	-0.02	0.05

Note: This table reports descriptive statistics for the survey sample of 450 male employees aged 30–45 on permanent contracts. The treated group is defined as employees at C Group firms where the paternity-leave mandate applied, and the control group as employees at large non-C Group firms without a mandate. Columns (1) and (2) present means for treated and control groups, Column (3) shows their differences, and Column (4) reports the standard errors of those differences. Variables cover demographic characteristics, spousal employment, income, leave use, tenure, and job level.

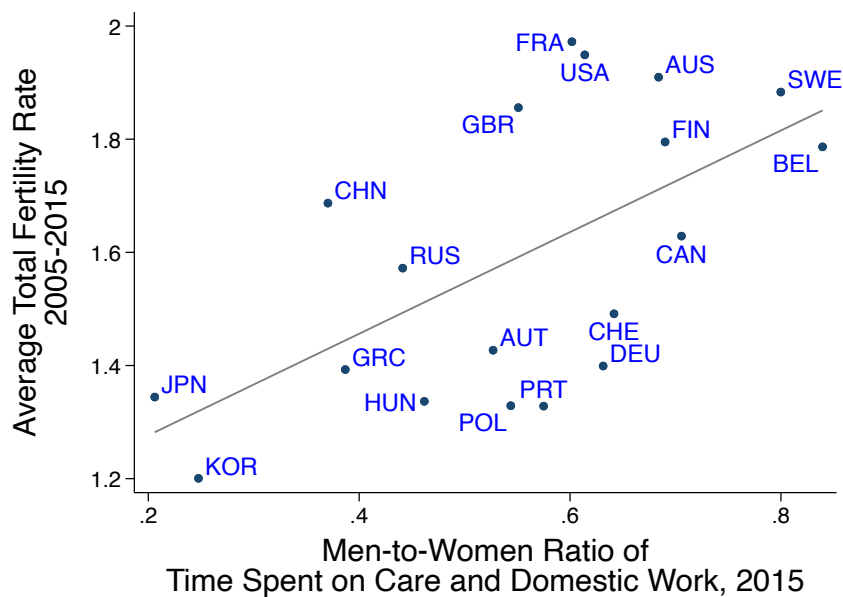
Table 12: Effects of the Mandate on Workplace Culture, Childcare Time, and Fertility Intentions

	(1) Experience penalties after leave-taking (Agree, or Strongly Agree)	(2) Coworkers' workload will increase (Agree, or Strongly Agree)	(3) Time on childcare OLS (midpoint)	(4) Time on childcare Interval regression (MLE)
Treated	-0.45*** (0.04) 450	-0.52*** (0.04) 450	6.34* (3.83) 450	8.80* (5.29) 450
Observations				
Treat mean	0.13	0.23	99.35	109.02
Control mean	0.58	0.74	91.95	100.22
Control SD	0.49	0.44	40.62	3.69
Controls: Age-by-marital FE, High-income FE				
	(5) Employees with Intend to have a child	(6) Employees with <Two Children Desired number of children	(7) Employees with No child Intend to have a child	(8) Desired number of children
Treated	0.07** (0.03) 313	0.11* (0.07) 313	0.16*** (0.06) 150	0.19* (0.11) 150
Observations				
Treat mean	0.98	1.50	0.93	1.30
Control mean	0.88	1.17	0.77	1.09
Control SD	0.27	0.64	0.43	0.74
Demographic Controls:	Age-by-marital status, High-income status		High-income status	

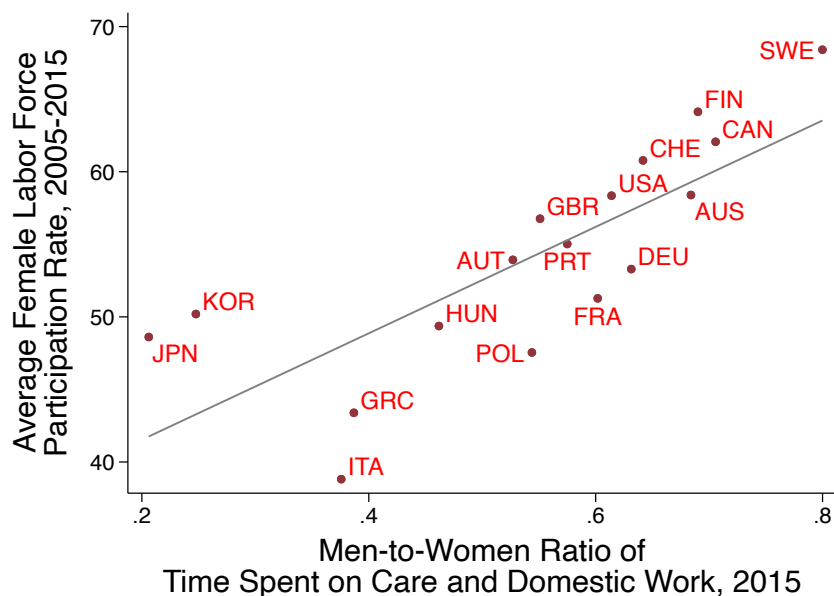
Note: This table reports regression estimates comparing survey responses of treated and control respondents. Columns (1)–(2) show the share agreeing that a hypothetical male coworker would face career penalties after leave-taking or increase coworkers' workload. Column (3) reports average weekday childcare minutes using OLS with midpoint coding, and Column (4) reports an interval regression (MLE) that accounts for censoring. Columns (5)–(6) present fertility intentions among respondents with fewer than two children. "Treated" equals 1 for respondents employed at mandate-covered firms and aware of the policy. Demographic controls include high-income status and four indicators for the age-by-marital interaction. Robust standard errors are in parentheses. *** indicates significance at the 1% level, ** at 5%, and * at 10%.

A Additional Figures and Tables

Figure A.1: Fertility and Female Labor Force Participation vs. Men's Share of Housework



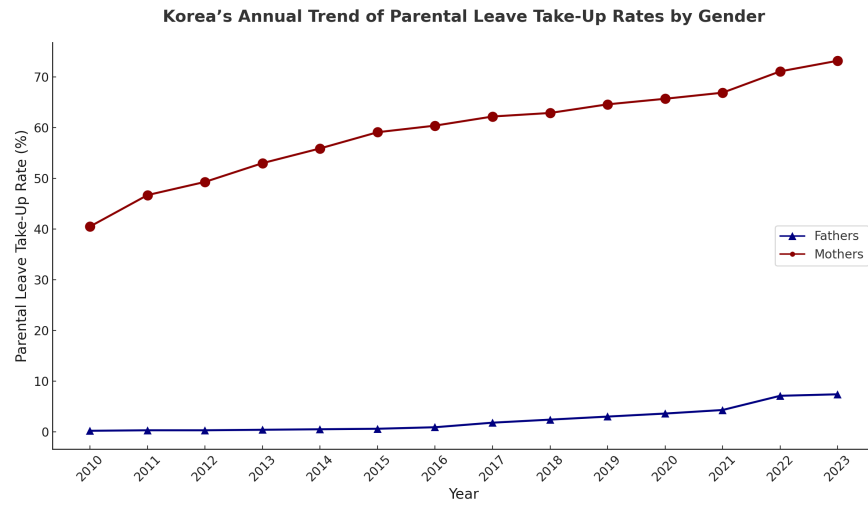
(a) Fertility vs. Men's Share of Housework



(b) Female LFP vs. Men's Share of Housework

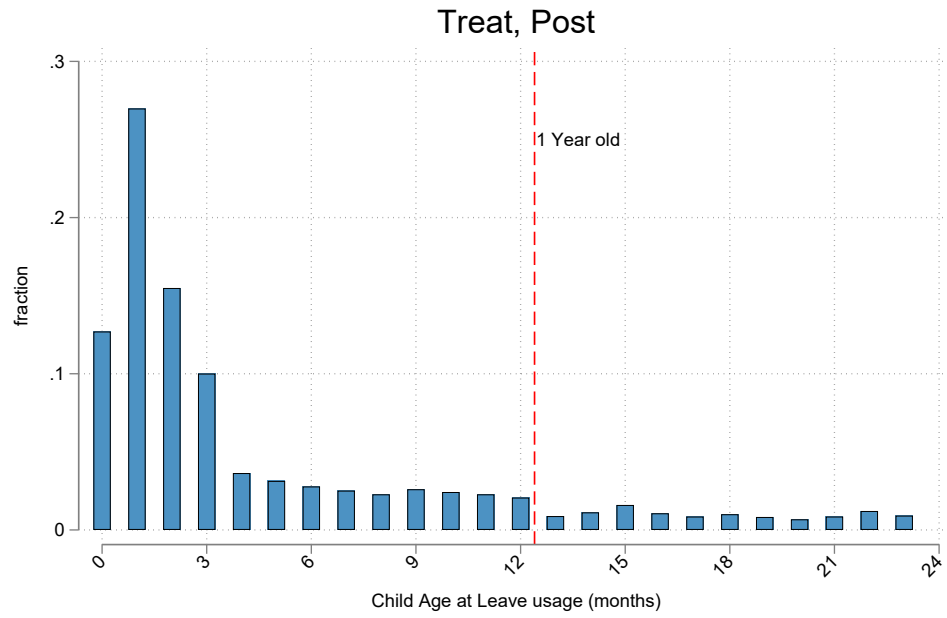
Note: Panel (a) plots the cross-country relationship between the men-to-women ratio of time spent on unpaid care and domestic work and the total fertility rate (Source: World Bank, UN Statistics Division). Panel (b) plots the relationship between the same ratio and female labor force participation (Source: OECD, UN Statistics Division).

Figure A.2: Parental Leave Utilization Rate in Korea by Gender



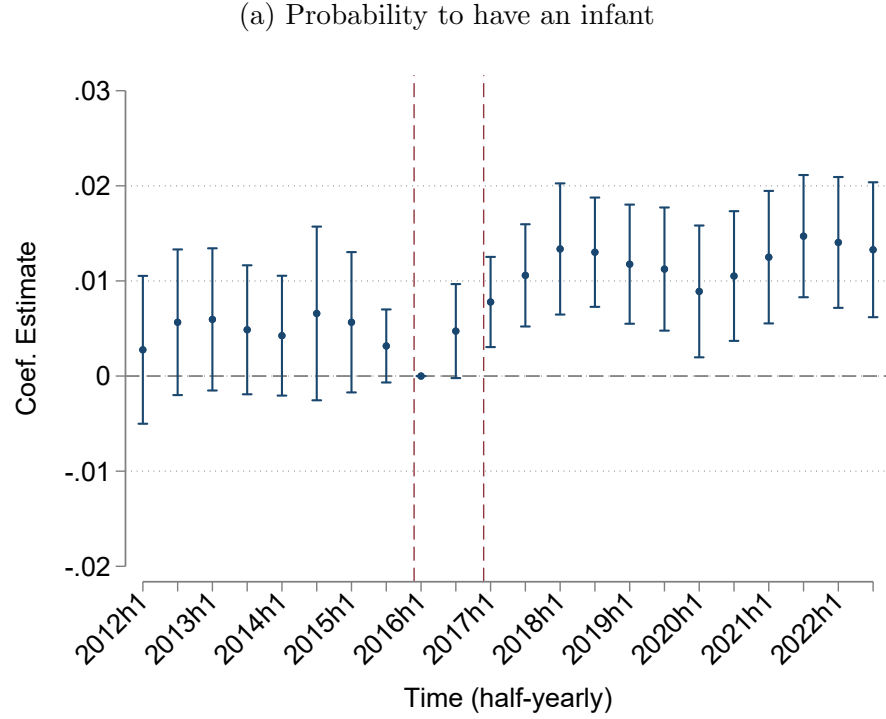
Note: The figure plots the average parental leave utilization rate by gender in Korea from 2010 to 2023. The utilization rate is calculated as the proportion of parents of newborns who used parental leave in a given year. Source: Statistics Korea.

Figure A.3: Child Age at Leave Usage

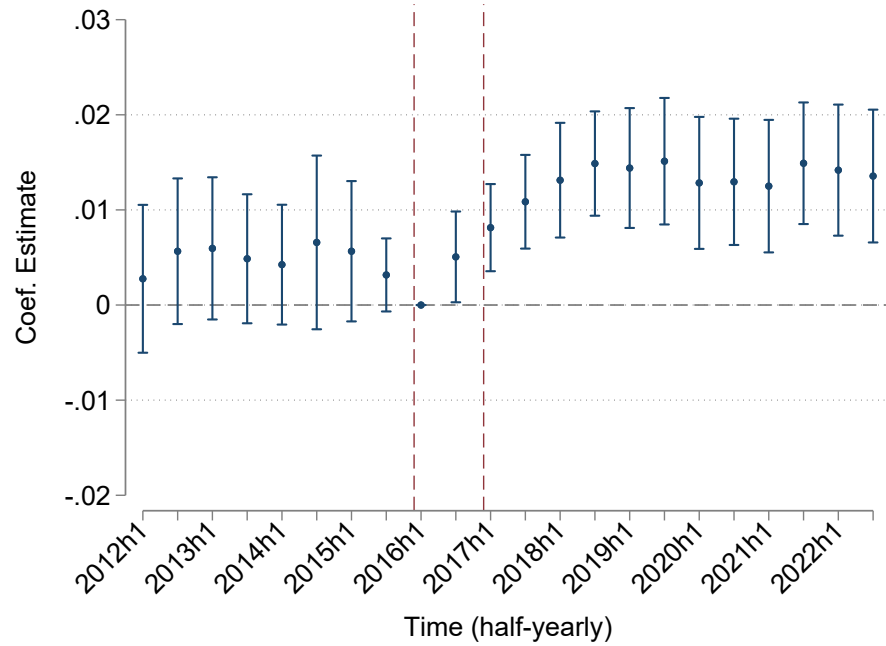


Note: The figure shows the distribution of child age at the time of leave-taking among fathers with newborns during 2017–2022 at treated firms. Treated firms refer to subsidiaries of conglomerate C Group, which implemented a mandatory one-month paternity leave policy in 2017.

Figure A.4: The Effect of the Paternity Leave Mandate on Fertility: Reweighted by Industry Composition



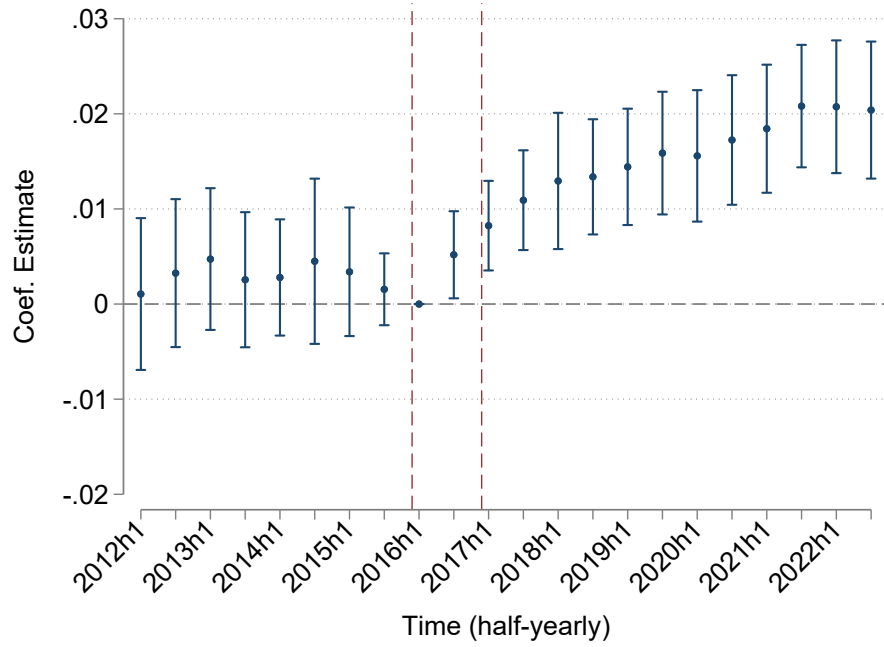
(b) Probability to have an infant and still employed at the same company



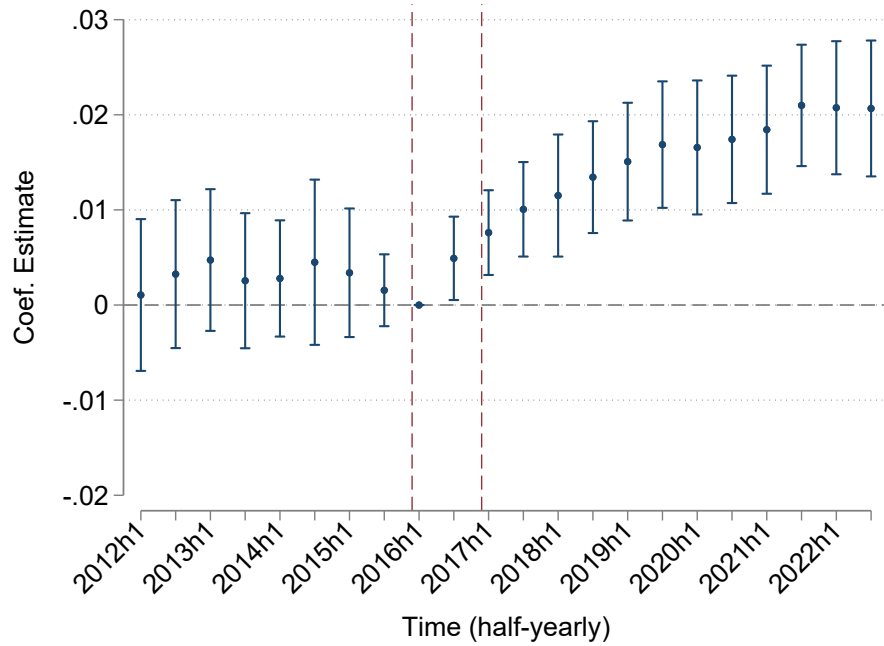
Note: These event-study figures present estimates of the effect of the paternity leave mandate on the probability that male incumbents have an infant (age 0) in a given period. Control group observations are reweighted to match the industry composition of treated firms. Panel (a) uses an indicator for having an infant in a given period, and Panel (b) uses an indicator for having an infant and remaining at the same baseline firm. Each point plots the estimated coefficients corresponding to $\beta_{2,k}$ in Equation 3. Robust standard errors are clustered at the baseline firm level.

Figure A.5: The Effect of the Paternity Leave Mandate on Fertility: Excluding those with wives working at the treated firms

(a) Probability to have an infant



(b) Probability to have an infant and still employed at the same company



Note: These event study figures show event study estimates of the impact of the paternity leave mandate on the probability that male incumbents have an infant (age 0) in a given period excluding incumbents who are ever married to wife working at the treated firms. Panel (a) uses an indicator for having an infant in a given period, and (b) uses an indicator for having an infant and remaining at the same baseline firm. Each point plots the estimated coefficients corresponding to $\beta_{2,k}$ in Equation 3. Robust standard errors are clustered at the baseline firm level.

Table A.1: Industry Composition of Treated and Control Firms

Industry	Treat (%)	Control (%)
Retails	55.48	9.58
Manufacturing	20.95	46.91
Hospitality, Restaurants	12.69	7.05
Construction	5.57	6.99
Logistics	2.26	5.72
Rentals, Operations	1.61	7.58
Real Estate	0.70	0.75
Finance, Insurance	0.67	15.10
Entertainments	0.07	0.32

Note: This table reports the share of employees in each industry for treated and control firms. Percentages are calculated within each group and may not sum to exactly 100 due to rounding.

Table A.2: Descriptive statistics: Reweighted by Industry Composition

	(1) Treat mean (sd)	(2) Control mean (sd)	(3) (1) - (2) mean (se)
Number of Individuals	17,366	378,690	
Age	37.0 (4.5)	36.8 (4.5)	0.1 (0.2)
Monthly Income (10,000 KRW)	509.6 (172.0)	574.0 (249.4)	-64.4** (26.0)
Tenure (years)	8.0 (5.0)	7.5 (5.1)	0.5 (0.3)
Married	0.64 (0.48)	0.66 (0.47)	-0.03 (0.01)
Birth Rate (Pre-policy)	0.04 (0.19)	0.04 (0.20)	-0.01*** (0.00)
Childless	0.40 (0.49)	0.39 (0.49)	0.01 (0.02)

Note: This table reports baseline individual characteristics for the main analysis sample, with control-group observations reweighted to match the industry composition of the treated group. The weights are defined as the ratio of each industry's share in the treated group to its share in the control group. Columns (1) and (2) present means and standard deviations for treated and control groups, respectively, while Column (3) shows the mean difference and its standard error. Age, monthly earnings, and tenure are drawn from the Matched Employer–Employee dataset; child information is from the 2015 Child Registry; and marriage data are from the 2015 Population Census.

Table A.3: Difference-in-Differences Estimates: Restricted and Reweighted Samples

	(1)	(2)	(3)	(4)
	<i>Excluding Incumbents with Wives at Treated Firms</i>		<i>Reweighted by Treated Industry Composition</i>	
Dependent Variable	1(Newborn)	1(Newborn, Same Firm)	1(Newborn)	1(Newborn, Same Firm)
Treat \times Post	0.012*** (0.003)	0.012*** (0.003)	0.007** (0.003)	0.008*** (0.003)
Constant	0.413*** (0.005)	0.394*** (0.006)	0.371*** (0.007)	0.348*** (0.008)
Observations	8,667,450	8,667,450	8,713,232	8,713,232
Included Fixed Effects	Calendar Time, Firm, Income Quintile			
Included Controls	Age, Baseline Tenure			

Note: This table reports difference-in-differences estimates from a linear probability model assessing the effect of a corporate policy change on the probability that male incumbents aged 30–45 have an infant. Columns (1) and (2) exclude incumbents ever married to wives employed at treated firms. The control group for Columns (3) and (4) is reweighted to match the industry composition of treated firms. The *Treat* indicator identifies individuals at firms subject to the corporate mandate, while the control group comprises men at firms in other conglomerates. The interaction of the *Post* and *Treat* indicators captures differential time trends between these groups. Columns (1) and (3) report estimates for having an infant, while Columns (2) and (4) report estimates for having an infant and remaining at the same baseline firm. All specifications include biannual calendar-time fixed effects, baseline earnings-quintile fixed effects, and baseline firm fixed effects. Robust standard errors clustered at the baseline firm level are reported in parentheses. * indicates significance at the 10% level; ** at 5%; and *** at 1%.

Table A.4: Effects on Mothers' Leave Taking

	(1) 1(Leave Use)	(2) 1(Leave Use)	(3) Leave Duration (months)	(4) Leave Duration (months)
Treat \times Post	0.014 (0.018)	0.011 (0.018)	0.657* (0.361)	0.617* (0.359)
Treat	-0.031** (0.015)		-0.875*** (0.311)	
Post	0.047*** (0.003)		0.657*** (0.073)	
Constant	0.799*** (0.003)	1.209*** (0.017)	13.110*** (0.339)	13.010*** (0.378)
Observations	68,541	68,541	68,541	68,541
Demographic Covariates	N	Y	N	Y
Child Birth Cohort FE	N	Y	N	Y

Note: This table reports difference-in-differences estimates of the paternity-leave mandate's effect on mothers' leave-taking. The sample consists of mothers matched to male incumbents employed at treated or control firms at baseline who had newborns during each period. Columns 1–2 report results for an indicator of leave use, and Columns 3–4 for leave duration (in months). Demographic covariates include maternal age at childbirth, an indicator for having two or more children, and the mother's annual earnings prior to childbirth. Childbirth cohort fixed effects are defined by bi-annual birth date indicators. Standard errors are clustered at the husband's baseline firm level. *** indicates significance at the 1% level, ** at 5%, and * at 10%.

Table A.5: Descriptive Statistics: Survey and Main Incumbent Samples

	Treat		Control	
	Survey	Main	Survey	Main
Number of Employees	214	17,366	236	378,690
Age	39.04	36.95	38.21	37.20
Married	0.82	0.64	0.75	0.72
Number of Children	0.98	0.63	0.96	0.73
Have Age ≤ 8 old	0.55	0.40	0.46	0.45
Wife Non-Employed	0.23	0.22	0.23	0.30
Tenure (years)				
Less than 1 year	0.02	0.01	0.01	0.02
1–4 years	0.23	0.21	0.16	0.21
5–8 years	0.19	0.36	0.21	0.32
9 years or longer	0.56	0.41	0.61	0.46

Note: This table reports descriptive statistics for two samples: (i) a survey sample of 450 male employees aged 30–45 on permanent contracts and (ii) the incumbent sample used in the main analysis. The treated group consists of employees at C Group firms subject to the paternity-leave mandate, while the control group includes employees at large non-C Group firms not covered by the mandate. Columns (1) and (3) show means for the survey sample, and Columns (2) and (4) show means for the main incumbent sample.

Table A.6: Effects of the Mandate on Leave Usage and Workplace Culture among Pre-Policy Employees

	(1)	(2)	(3)	(4)	(5)	(6)
	Leave Used (Had Newborn)	Expected # of Coworkers Use Leave (out of 10)	Willing to Recommend Leave (out of 10)	Expected # of Coworkers Recommend Leave (out of 10)	Experience penalties after leave-taking (Agree/Strongly Agree)	Coworkers' workload will increase (Agree/Strongly Agree)
Treated	0.59*** (0.06)	5.42*** (0.34)	2.10*** (0.33)	2.34*** (0.37)	-0.43*** (0.05)	-0.56*** (0.05)
Observations	204	265	265	265	265	265
Treat Mean	0.74	7.10	8.30	7.07	0.13	0.23
Control Mean	0.17	2.09	6.04	4.74	0.58	0.740
Control SD	0.38	1.89	3.01	2.88	0.49	0.44

Note: This table reports regression estimates comparing survey responses of treated and control respondents who had worked at the same firms since before the policy change (with more than nine years of tenure). Column 1 shows the share who used leave among those who had a newborn while employed at the same company. Columns 2-4 report average responses to leave-usage questions, consistent with Figure 10. Columns 5 and 6 report the share agreeing that a hypothetical male coworker would face career penalties after leave-taking or increase coworkers' workload. "Treated" equals 1 for respondents employed at mandate-covered firms and aware of the policy. Demographic controls include high-income status and four indicators for the age-by-marital interaction. Robust standard errors are in parentheses. *** indicates significance at the 1% level, ** at 5%, and * at 10%.

B Do Expectations or Experiences Drive the Effect?

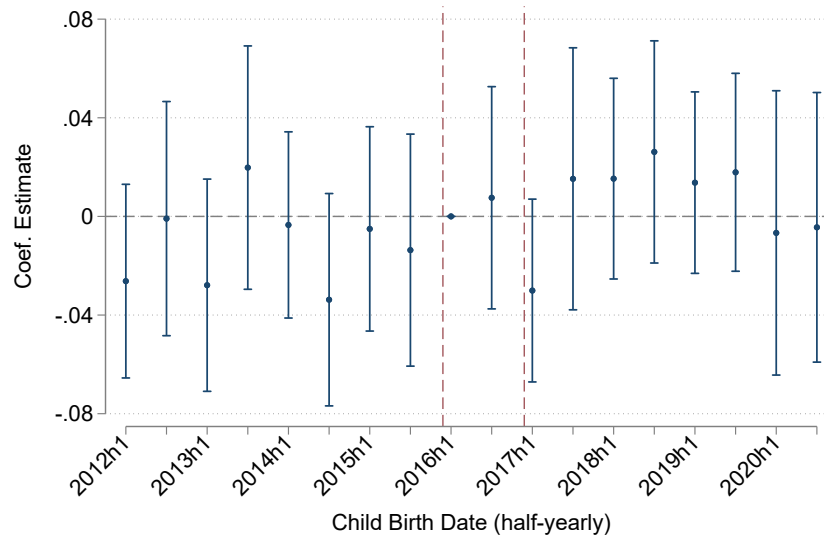
In this section, we assess how much of the observed fertility response reflects forward-looking behavior (changes in expectations before any leave is taken) versus ex-post effects from the lived experience of taking leave. A paternity-leave mandate can influence fertility either by shifting expectations about future paternal availability (*ex ante*) or by changing preferences after fathers experience leave (*ex post*). Prior work has largely targeted the latter and typically finds null or negative effects.

To isolate the ex-post channel, we follow the spirit of designs that study higher-order births around timing cutoffs and compare fathers whose current birth was covered by the mandate with otherwise similar fathers who just missed coverage. Concretely, within treated firms, we form cohorts of fathers with an infant whose birth occurred shortly after the mandate’s start (covered) and those with an infant born shortly before (just missed). Rather than a sharp regression discontinuity—which is underpowered here—we implement an event-study centered on the mandate start, tracking the probability of an additional birth following the current birth. We include father covariates (age, tenure, earnings-quintile fixed effects), firm fixed effects, and calendar-time fixed effects, in line with our main specification. This setup parallels prior “ex-post” tests while maintaining consistency with our baseline design.³¹

Figure B.1 shows no statistically significant increase in the likelihood of a subsequent birth for the covered cohort relative to the just-missed cohort. This suggests that the fertility response we document is not primarily driven by the experience of taking leave. Instead, the pattern is consistent with forward-looking fertility decisions: households that just missed coverage for the current child may still plan an additional birth in anticipation of being covered in the future. These findings align with our main evidence that expectations about paternal availability—rather than realized leave experiences—play a central role in the mandate’s fertility effects.

³¹We also benchmark against control firms to absorb seasonality and broader trends via time fixed effects; results are unchanged.

Figure B.1: Probability of subsequent births of fathers with newborn



Note: The figures present difference-in-differences estimates of the effect of the paternity leave mandate on subsequent births. The sample consists of male incumbents with infants in each time period. The outcome variable is an indicator for having another child. The treat dummy indicates whether a father is working at treated firms at the time of childbirth. We control for biannual calendar time dummies, firm fixed effects, monthly earnings of fathers, and number of children at the time. Each point represents the estimated coefficient on the interaction between the treatment indicator and biannual calendar time dummies (as shown on the x-axis) along with the associated 95% confidence intervals. Robust standard errors are clustered at the individual level.

C Formal Model of Fertility Decisions

This appendix presents the full household decision-making model that underlies the heterogeneity analysis in the main text (Section 6.4). We analyze household fertility decisions using a two-period utility maximization framework, following [Becker and Lewis \(1973\)](#). The household is modeled as a unitary decision maker. For clarity, we frame the problem as a two-stage decision solved by backward induction. Intuitively, the household first anticipates its situation if it were to have a child—how much time each parent would allocate, how much income would be forgone, and what additional costs would arise. Based on this anticipated allocation, the household then decides whether to have a child. Thus, the fertility decision reflects not only the desire to have a child but also the expected trade-offs between the benefits of childrearing and the associated costs. We begin with the second-stage problem: a utility maximization exercise conditional on having a child versus not. We then turn to the first-stage problem, which yields an inequality condition characterizing the fertility decision.

Second Stage: Utility Maximization Conditional on Fertility

Conditional on having a child ($b = 1$), the household allocates consumption c , maternal time t_f , and paternal time t_m to maximize utility:

$$U(1) = \delta c + \nu h(t_f, t_m) - \alpha t_m,$$

where δ is the marginal utility of consumption, $h(t_f, t_m)$ denotes child quality (concave in both inputs), and α captures stigma costs associated with paternal leave-taking.

The household faces both time and budget constraints. Each parent $g \in \{f, m\}$ has 12 months available, so $t_g \leq 12$. Earnings are reduced by foregone labor income $\lambda(t_g)w_g$, where w_g is the monthly wage and $\lambda(t_g)$ is the proportion of time withdrawn from market work. The budget constraint is:

$$c = 12w_f + 12w_m - \lambda(t_f)w_f - \lambda(t_m)w_m - \theta,$$

where θ captures fixed monetary costs of childrearing. Solving this problem yields the optimal allocation (t_f^*, t_m^*) of maternal and paternal time.

In the absence of a child ($b = 0$), the household maximizes:

$$U(0) = \delta c,$$

subject to the budget constraint:

$$c = 12w_f + 12w_m,$$

since no childrearing costs are incurred and no time is withdrawn from market work.

First Stage: Fertility Decision

Given the optimal allocation of parental time, the household chooses to have a child in the first period if and only if the expected utility from child rearing in the second period exceeds the utility from remaining childless. Analytically, the household chooses fertility to maximize expected utility:

$$b^* = \arg \max_{b \in \{0,1\}} \mathbb{E}[U(b) | \Omega],$$

where Ω denotes the information set, including wages (w_f, w_m) , stigma costs α , and fixed childrearing costs θ .

Fertility occurs if and only if:

$$U(1) \geq U(0).$$

Reordering terms yields the following fertility condition:

$$\nu h(t_f, t_m) \geq \delta [\lambda(t_f)w_f + \lambda(t_m)w_m] + \theta + \alpha \cdot t_m^*, \quad (6)$$

where t_m^* is the utility-maximizing paternal time input. The left-hand side represents the utility from time spent with a child, while the right-hand side reflects forgone consumption due to reduced earnings, direct child-related costs, and stigma costs. Intuitively, fertility occurs when the value of time spent with a child outweighs the forgone earnings and associated costs.

Fertility Threshold and the Role of Paternal Time

The household chooses to have a child if and only if the expected utility with a child exceeds that without one. Substituting the optimal allocation (t_f^*, t_m^*) into the budget constraints gives:

$$\begin{aligned} \delta \left[(12 - \lambda(t_f^*))w_f + (12 - \lambda(t_m^*))w_m - \theta \right] + \nu h(t_f^*, t_m^*) - \alpha t_m^* &\geq \delta [12w_f + 12w_m] \\ \iff \nu h(t_f^*, t_m^*) &\geq \delta [\lambda(t_f^*)w_f + \lambda(t_m^*)w_m] + \theta + \alpha t_m^*. \end{aligned}$$

The left-hand side captures the benefit from parental time spent with the child, while the right-hand side represents the costs of childbearing: foregone consumption from reduced labor supply, fixed childrearing expenses, and stigma costs associated with paternal leave.

The concavity of $h(t_f, t_m)$, combined with the linearity of the opportunity costs $\lambda(t_g)w_g + \alpha t_m$, generates a threshold level of paternal involvement, \bar{t}_m , such that households with $t_m^* > \bar{t}_m$ optimally choose not to have a child. Intuitively, beyond \bar{t}_m , the marginal cost of additional paternal time exceeds the marginal gain in child quality. Formally, let $\bar{t}_m = T$ satisfy

$$\nu h(t_f^*, T) = \delta[\lambda(t_f^*)w_f + \lambda(T)w_m] + \theta + \alpha T.$$

For any $t_m^* = T + \varepsilon$ with $\varepsilon > 0$, the additional benefit is

$$\nu[h(t_f^*, T + \varepsilon) - h(t_f^*, T)] \simeq \nu h_{t_m}(t_f^*, T)\varepsilon,$$

while the additional cost is

$$\delta\lambda_t(T)w_m\varepsilon + \alpha\varepsilon.$$

The first-order condition for t_m implies

$$\nu h_{t_m}(t_f^*, T) = \delta\lambda_t(T)w_m + \alpha,$$

so by concavity of $h(\cdot)$,

$$\frac{\nu h(t_f^*, T + \varepsilon) - \nu h(t_f^*, T)}{\varepsilon} \not\geq \nu h_{t_m}(t_f^*, T).$$

Thus, for \bar{t}_m , $U(1|t_m = \bar{t}_m + \varepsilon) < U(0)$ for all $\varepsilon > 0$, defining the threshold paternal time beyond which fertility is not optimal.

This formulation defines a threshold paternal time input, \bar{t}_m , conditional on $t_f^*w_f, w_m$, above which the marginal costs of childbearing outweigh the benefits. Households with $t_m^* > \bar{t}_m$ optimally choose not to have a child. Prior to the mandate, fertility was concentrated among households with $t_m^* = 0$, consistent with stigma and low wage replacement deterring even modest paternal leave-taking. By reducing the effective costs of early leave-taking, the mandate raises this threshold \bar{t}_m , expanding the set of households for whom childbearing is optimal.

As documented in Section 4, the share of fathers taking more than one month of leave also rises after the mandate. This suggests that by lowering the first-month leave cost, the policy shifted the paternal-time cutoff from $\bar{t}_m^{\text{pre}} = 0$ to $\bar{t}_m^{\text{post}} > 0$

Distinguishing Time vs. Income Channels: Single vs. Dual-Earner Households

Equation 6 shows that the policy lowers the fertility threshold through two distinct mechanisms: (i) an *income channel*—full wage replacement for one month—and (ii) a *time channel*—mandated leave-taking that reduces stigma. The importance of each channel depends on the mother’s availability of time for childcare. In single-earner households—where the mother is not employed and provides maximum childcare input ($t_f = \bar{t}_f^{\max}$)—the marginal utility from additional paternal time is negligible. In this case, the policy primarily operates through the income channel. In contrast, in dual-earner households, where maternal time is constrained, both channels are operative, with the time channel expected to be particularly important.

Single- versus Dual-Earner Households

The relevance of the time versus income channels depends critically on maternal time input t_f , which is shaped by the mother’s employment status. In single-earner households, where the mother allocates her maximum time \bar{t}_f^{\max} to childcare, the marginal value of paternal time is near zero. In such cases, the policy primarily affects fertility through the income channel. By contrast, in dual-earner households—where maternal time is limited due to employment—the marginal value of paternal involvement is higher, and both channels are operative.

Case 1: $t_m^* = 0$ (No Paternal Leave Pre-Policy)

For these households, pre-policy utility with a child was below utility without one, since the threshold \bar{t}_m was effectively zero. The policy can induce fertility if the combined benefit of one month of paternal time and wage replacement exceeds the pre-policy utility gap:

$$\nu [h(t_f, 1) - h(t_f, 0)] + \delta w_m \geq \nu h(t_f, 0) - \delta \lambda(t_f) w_f - \theta.$$

Because these households did not previously plan for paternal leave, the stigma component α is irrelevant. For single-earner households with $t_f = \bar{t}_f^{\max}$, the marginal gain from paternal time is negligible ($h_{t_m}(\bar{t}_f^{\max}, t_m) \approx 0$), so the effect operates almost entirely through wage replacement. In contrast, dual-earner households benefit from both channels, as paternal time has a higher marginal value when maternal involvement is constrained.

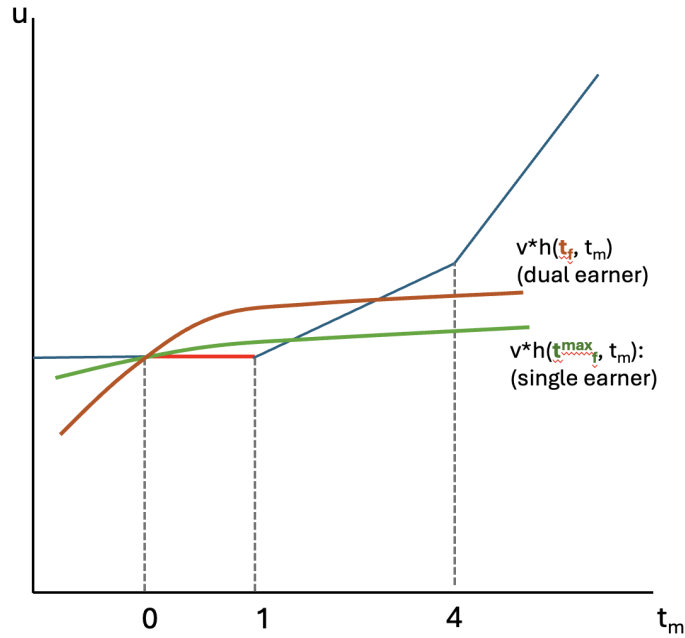
Case 2: $t_m^* > 0$ (Positive Paternal Leave Pre-Policy)

These households were previously deterred by the joint cost of forgone wages and stigma. The mandate reduces both components by $\delta w_m + \alpha$, shifting the fertility threshold \bar{t}_m^* defined by:

$$\nu h(t_f^*, \bar{t}_m^*) = \delta [\lambda(t_f^*)w_f + \lambda(\bar{t}_m^*)w_m] + \theta + \alpha \bar{t}_m^* - \delta w_m - \alpha.$$

Households with $t_m^* \leq \bar{t}_m^*$ may now optimally choose to have a child. The effect is again stronger in dual-earner households, where the marginal return to paternal time is higher. This is illustrated in Figure C.1.

Figure C.1: Policy Effects on Fertility Decision Threshold by Household Type



Finally, the model predicts that fertility responses increase with the mother's wage. Higher w_f raises the opportunity cost of maternal time, lowering maternal involvement ($\partial t_f / \partial w_f < 0$) and increasing the marginal value of paternal time ($\partial h_{t_m}(t_f(w_f), t_m) / \partial w_f \geq 0$). As a result, the availability of paternal leave is especially valuable in households where mothers face strong labor market incentives, amplifying fertility responses to the policy.

This framework yields two key testable predictions which are tested in Section 6.4. First, if the time channel is the dominant mechanism, fertility responses should be stronger among dual-earner households. Second, the effect should increase with the wife's market wage w_f ,

as higher wages imply a greater opportunity cost of maternal time and a correspondingly higher marginal return to paternal involvement. Formally, $h_{t_m}(t_f(w_f), t_m)$ is increasing in w_f , making the time channel more valuable in high-wage households. We test these predictions by examining heterogeneity in fertility responses by wives' employment status and earnings.

D Survey Sample Collection

We conducted a survey of 450 male employees aged 30–45, following the age range used in the main analysis. To ensure that respondents were eligible for corporate benefits, we restricted the sample to full-time employees on permanent contracts. Among these respondents, 216 are classified as treated and 234 as controls.

Treatment status was defined using two complementary criteria: (i) whether the respondent reported working at a subsidiary of C Group, and (ii) whether the respondent reported that a paternity-leave mandate was in place at their firm. Control status was defined symmetrically: respondents worked at large corporations unaffiliated with C Group and reported no mandate in place. This dual definition ensured that individuals categorized as “treated” were both exposed to—and aware of—the policy.

Respondents were recruited through three online survey providers: Embrain, Korea Policy and Research Group, and Remember. Embrain and Korea Policy and Research Group operate quota-sampling panels, similar to widely used international platforms such as Qualtrics and Prolific. Quota sampling is a non-probability sampling method in which invitations are sent to eligible individuals until specified quotas are filled. Remember is a business-oriented research firm that maintains a pool of Korean employees with verified information on their company affiliation.

From these panels, we selected individuals meeting the criteria above and invited them to participate via email or text message. Screening questions at the start of the survey confirmed that respondents (i) were full-time permanent workers, (ii) worked for either C Group or another large conglomerate (using a pre-specified list of firms), and (iii) reported whether their firm had a paternity-leave mandate in place. This allowed us to classify respondents consistently with our treatment and control definitions.

Participation was voluntary, and respondents were compensated between 5,000 and 14,000 KRW for completing the survey, depending on the platform. The full survey questionnaire is available at [Survey Questionnaire \(Korean\)](#) and [Survey Questionnaire \(English Translated\)](#).