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Spillover of Female Labor Protection: Evidence from China

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Abstract

This study examines the gendered consequences of China's 2012 Female Labor Protection Policy (FLP), introduced to enhance workplace protections through extended maternity leave, standardized allowances, job restrictions, and penalties for non-compliant employers. Using a triple-differences (DDD) identification strategy and data from the China Family Panel Studies (CFPS), we separate the effects of the FLP from contemporaneous family-planning reforms. Although the FLP aimed to improve women's work–life balance, our findings reveal large wage penalties that fall disproportionately on women—especially older female workers—rather than being confined to those of childbearing age. We find no evidence that the policy increases women's labor force participation or fertility. These results suggest that well-intentioned gender-focused labor regulations can generate unintended adverse consequences, reinforcing workplace inequality and exacerbating the motherhood penalty. Our study contributes to feminist analyses of labor regulation by highlighting how protective legislation can inadvertently strengthen age- and gender-based wage biases.

Keywords Gender · Female labor protection · Policy evaluation · China

JEL Classification J16 · J22 · J88

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Introduction

Although female labor participation has experienced huge progresses worldwide since 1960s due to increasing access to education and the rise of feminist movement, gender disparities still remain in the labor market, as well as structural inequalities in wages. Females still face inferior job opportunities compared to their male counterparts. According to the World Bank, the global labor force participation for women is just over 50%, while that for men is around 80% in 2022.[1] Moreover, women earn 16% less than men on average in prime working age, with the gap widening to older groups.[2] To promote inclusive progress in the labor market, many countries have launched policies protecting women's labor rights, such as anti-discrimination laws, paid maternity leave, and equal pay. However, the effectiveness of these policies remains debated. While some studies suggest that labor protections improve gender equality in labor force participation and earnings (Hyland et al. 2020), others highlight unintended consequences, such as employment discrimination against women of childbearing age (Becker et al. 2019; Fernández-Kranz and Rodríguez-Planas 2021; Timpe 2024; Liu et al. 2024a). A further strand of research observes modest increases in fertility alongside negligible impacts on women's labor-force participation (Hiriscau 2024). The potential trade-offs between labor protections and labor market distortions raise important questions: Do those female labor protection policies achieve their intended goals for targeted groups? Do they lead to spillover effects on other demographic groups?

This paper answers these questions by evaluating the effects of female labor protection provisions (hereafter: FLP) in the context of China. China's rapid aging population and declining fertility rates began to pass pressures to its labor market around the 2010s. In 2012, the female labor protection provisions, such as paid maternity leave, were introduced to improve the balance between work and family for female workers. Meanwhile, China began gradually relaxing its one-child policy, introducing a selective two-child policy in 2011, which is followed by further expansions, ultimately leading to the universal two-child policy in 2016 aimed at promoting fertility.

While FLP primarily aims to enhance workplace protections for females, and the two-child policy directly removes restrictions on fertility incentives, it is challenging to disentangle the causal effects of FLP from the two-child policy, as labor and childbirth behaviors are naturally interwoven, and their targeted groups and implementing periods overlap.[3] To address this problem, we employ a triple-difference (DDD) strategy, leveraging variations in sectoral coverage of FLP and the universal application of the two-child policy. The two-child policy applies broadly to all women, while FLP specifically affects female workers in formally covered sectors. By comparing labor market outcomes across different sectors and demographic groups before and

[1] Soucre: https://genderdata.worldbank.org/en/data-stories/flfp-data-story

[2] Source: https://www.americanprogress.org/article/fact-sheet-the-state-of-women-in-the-labor-market-in-2023/

[3] For example, evidence shows that the paid family or maternity leave would increase fertility but decreases employment and wage for young women (Golightly and Meyerhofer 2022; Chang 2021).

after the FLP implementation, the DDD approach allows us to isolate the effects of FLP from those of the two-child policy.

Besides, we provide a simple labor searching and matching model to understand the effects of FLP. The enactment of FLP will increase the benefits for FLP-covered female workers, but also lead to potential productivity loss for employers. Those cost-benefit through both the labor supply and labor demand sides determines the labor participation, as well as wages of females. Additionally, by considering the heterogeneity in benefit claiming probability and bargaining power, we infer the spillover effects of childbearing age targeted FLP to workers beyond those targeted ages.

Our findings indicate that FLP lead to unintended negative consequences, particularly wage declines for women, without significantly influencing female labor force participation. Furthermore, the impact of FLP extended beyond its targeted demographic, as wage reductions were also observed among older women beyond childbearing age. This suggests that employer responses to FLP resulted in unexpected labor market distortions. Additionally, FLP negatively affected job switchers, particularly older females, but did not increase fertility rates.

We contribute to a strand of rapidly growing literature that highlights the paradox in which policies intended to protect female workers may inadvertently disadvantage them in the labor market. Although some research documents a positive relationship between female labor protection and their labor performance[4], more and more recent studies found the opposite results. Policies aimed at addressing gender inequality in the workplace, such as FLP, are designed to mitigate the disadvantages women face due to fertility-related career interruptions. However, as seen in various contexts, these policies can introduce unintended labor market consequences. For example, Liu et al. (2024a) find that maternity leave entitlement extensions in China negatively affected employment and wages for women of childbearing age, highlighting that even well-intended labor protections can alter employer hiring behavior. Similarly, broader policy shifts like the two-child policy or paid maternity leave, which directly influence fertility, also produce unintended labor market distortions (Huang et al. 2021; Chang 2021; Liu et al. 2024b). These policies, while seeking to bridge gender-related gaps, demonstrate that interventions in fertility and labor protection policies are interconnected and may do more harm than good to child-bearing age female workers.

Another highly related literature is measuring the labor market outcomes on different horizons, for example, the effects of paid maternity leave in a short run versus in a long run, or the effects on new mothers versus on other related groups (Regmi and Wang 2022).[5] Most existing efforts answered the questions in a short-term window by investigating the immediate employment and wage changes, either prior to birth (Girsberger et al. 2023), or post childbirth (Liu et al. 2024a). Some recent focus has

[4] Byker (2016) explored the impacts of parental leave in the United States and suggested that the paid leave laws could significantly increase women's labor market attachment, especially for low-skilled women. Bergemann and Riphahn (2023) found the reform in German parental leave benefit program sped up mother's labor market return.

[5] Besides employment outcomes in labor market of mothers, there are also research on paid maternity leave on mother's health, child health, child human capital accumulation, father's leaving taking, as well as fertility and marriage choice. For a more detailed literature review, please see Regmi and Wang (2022).

documented negative effects from a more longer-term horizon: Bailey et al. (2025) observes the paid leave programs still make mothers suffer loss in wage even in ten years after giving the birth; Thomas (2021) provides evidence that paid leave reduces women's chance to get promotion; Hiriscau (2024) finds the increased fertility persist for seven years. However, Regmi and Wang (2025) observes the U.S. paid maternity leave that it benefits children's education by enabling these exposed children to get college degrees. Our conclusion aligns with the findings of those studies that have overall negative employment effects resulting from extended maternity leave entitlements in the short run, such as reduced likelihood of working, promotion, formal employment, and wages (Azmat et al. 2006; Han et al. 2009; Nandi et al. 2018; Nivorozhkin and Romeu-Gordo 2019; Thomas 2021; Del Rey et al. 2021; Timpe 2024; Liu et al. 2024a). However, most of them have focused on the labor outcomes, either for only mothers post-childbirth or for woman group at childbearing age. Few efforts have been made to examine unintended demographic groups, such as older females beyond childbearing age. The focus of this paper contributes to the discussion of short-term effects of FLP, and its spill over effects to other groups, especially the unintended effects to older female workers.

In addition to broadening the affected groups, we draw subtly different conclusions: no significant effects on labor participation but notable wage declines. Liu et al. (2024a) exploit China's provincial variation and find that the maternity leave entitlement extension deteriorates the labor market outcomes of women of childbearing age, including both employment and wages. The documentation of wage reductions for women, including those beyond childbearing age, suggests that protective labor policies may contribute to systemic wage disparities. This paper offers evidence indicating that gender-based employment discrimination persists in China's labor market, adversely affecting women's labor performance in a form of intensive margins rather than extensive margins.

Our study also contributes to the broader discussion on gendered labor protections and their economic implications. Existing research has shown that while maternity benefits encourage job retention, working-hour restrictions reduce women's overall employment (Zveglich et al. 2003). The persistence of gendered household specialization suggests that women continue to bear a disproportionate burden of caregiving responsibilities (Mu and Xie 2016). Additionally, fertility policies have long-term socioeconomic trade-offs, affecting not just labor supply but also parental well-being (Islam and Smyth 2015). Evidence from Hong Kong further indicates that family-friendly policies involving fathers, rather than focusing solely on mothers, yield better fertility and employment outcomes (Lui and Cheung 2021). These insights align with our findings that labor protections must be carefully designed to balance gender equity and labor market efficiency (Österbacka and Räsänen 2022; Casarico et al. 2023).

The paper is organized as follows: Section 2 outlines the background of LPF. Section 3 discusses the conceptual framework underpinning these policies. Section 4 describes the data sources used, and Section 5 details the methodology for assessing the impact of LFP; Section 6 presents our empirical results. Section 8 concludes with a summary of findings and policy implications.

Background

On April 28, 2012, the State Council of the People's Republic of China promulgated the *Special Provisions on Labor Protection of Female Employees* (hereafter, Female Labor Protection, FLP). The Provisions took effect immediately upon issuance and substantially raised the statutory requirements for protecting the rights of female employees, while also strengthening penalties for non-compliance. Relative to earlier regulations, the FLP introduced several primary changes.

First, the Provisions extended paid maternity leave from 90 to 98 days, with additional leave in some localities. Second, the rules standardized the calculation of maternity allowances, which are funded by maternity insurance and are tied to the employer's average wage in the preceding year. This design tends to benefit low-wage female employees, whose maternity allowances can exceed their regular wages, while local regulations often require employers to top up the allowance for higher-wage employees so that their income during leave does not fall below pre-leave earnings. Third, the FLP expanded the list of jobs and working conditions deemed unsuitable for female employees during menstruation, pregnancy, and lactation, tightening restrictions on night work, heavy lifting, and exposure to certain hazards. While intended to protect health, these expanded restrictions may increase the perceived cost of employing women and thus induce potential productivity loss for hiring child-bearing wage women. Finally, the Provisions specify explicit monetary penalties: for example, employers who overwork breastfeeding employees or assign them night shifts can be fined between RMB 1,000 and 5,000 per violation.

Legally, the Provisions apply to "employing units" and their "female employees" in registered work units, including state agencies, enterprises, public institutions, and social organizations. Enforcement is mediated through formal labor contracts, maternity insurance schemes, and workplace inspections. In practice, this institutional machinery is concentrated in the *formal* sector, reviewed as urban (or non-rural) employment in registered organizations. By contrast, rural agricultural workers in the China Family Panel Studies (CFPS) are overwhelmingly self-employed or unpaid family workers without written contracts without employer-provided benefits. Although the Provisions do not explicitly exclude these unregistered non-rural or agricultural organizations, women working in such settings are effectively outside the contract-based enforcement system of the FLP. In the empirical analysis we therefore refer to urban employees in registered work units as the *formal sector*, and to rural agricultural and other unregistered work as the *informal sector*, and we treat women in the informal sector as a comparison group who are de facto unexposed to the FLP. The Provisions do not directly target male workers.

In evaluating the impact of the FLP on women's labor market outcomes, it is important to account for contemporaneous changes in fertility policies and related institutions. China's one-child policy had long included limited exceptions, including the possibility of a second birth for some couples in which both spouses were only children, and these exceptions were gradually generalized prior to our sample period. More substantial relaxations occurred in the 2010s and are often referred to collectively as the "two-child policy." As summarized in Fig. 1, the first major nationwide relaxation during our sample period was the selective two-child reform announced

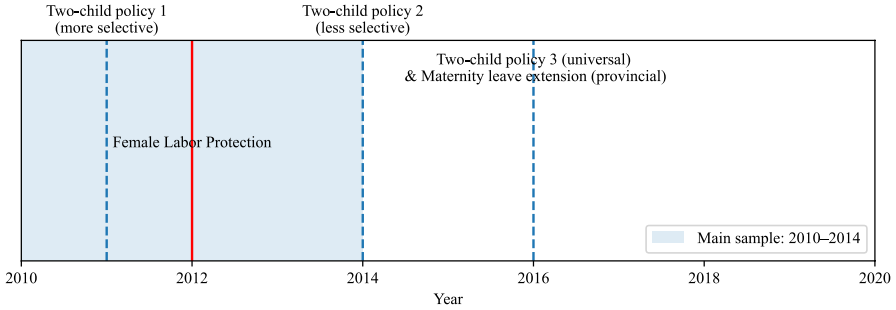

Fig. 1 Timeline of Relevant Policies. *Notes:* The red vertical line marks the introduction of the Female Labor Protection (FLP) Provisions, which became effective in 2012. The other dash lines indicate gradual relaxations of China's birth-planning regime in our sample period. The earliest relaxation we consider, labelled "Two-child policy 1" in the figure, corresponds to the long-standing exception that allows couples in which *both* spouses are the only child to have a second birth. This exception was introduced earlier and gradually generalized across provinces; we anchor it around 2011 in the timeline, by which point it had effectively been adopted nationwide. "Two-child policy 2" refers to the selective two-child reform announced in late 2013, which allowed a second child if *either* spouse is an only child and was implemented by provinces from 2014 onward; the figure therefore places its effective year at 2014. "Two-child policy 3" denotes the universal two-child reform announced in late 2015, with the amended Population and Family Planning Law taking effect on January 1, 2016; the figure marks 2016 as the first full effective year of the universal two-child regime. Maternity leave extensions were further expanded across provinces after 2016 and are not shown separately

in late 2013, which allowed a second child if either the husband or the wife was an only child and was implemented by provinces from 2014 onward. A further reform in late 2015 was a universal two-child policy that became effective on January 1, 2016.

Maternity leave policies evolved alongside these fertility reforms. Before 2015, statutory maternity leave was relatively uniform and the FLP provided the main national framework. After the universal two-child policy took effect, many provinces extended maternity leave and introduced additional benefits, leading to substantial regional heterogeneity from 2016 and onward. To avoid conflating the FLP with these later changes, our main analysis focuses on the 2010–2014 period, which covers the introduction of the FLP in 2012 and the initial implementation of the selective two-child reform. Within this window, we interpret our estimates as capturing the effect of the FLP in an environment where fertility policy is gradually relaxing. Existing work suggests that two-child reforms tend to worsen female labor market outcomes: for example, He et al. (2023) document increased hiring discrimination against women following the two-child policy using a correspondence study, and Wu (2022) show that additional births reduce maternal labor supply in urban China. Our contribution is to isolate the FLP-related component of these changes by exploiting variation across formal and informal sectors and between women and men.

Conceptual Framework: A Labor Search and Matching Model

This section develops a simple framework proposed by Del Rey et al. (2017), and formalizes the mechanism through which the Female Labor Protection (FLP) can influence the labor participation(i.e., employment) and wage of female workers through the productivity loss of employers (indicated as ψ in the model), as well as the benefits of workers (indicated as z in the model).

Economic Environment

The economy consists of a measure 1 of risk-neutral workers and employers.[6] All representative workers and employers discount future payoffs at a common interest rate r from a perfect capital market. In this economy, u and v represent the unemployment rate, and the number of job vacancy, respectively. The matching between unemployed workers and job vacancies is costly, captured by the function m(u,v).[7] The aggregate rate that unemployed workers find a job is $p(\theta) = \frac{m(u,v)}{u} = m(1, \frac{v}{u})$, and the aggregate rate that a job vacancy is filled is $q(\ theta) = \frac{m(u,w)}{v} = m(\frac{u}{v}, 1)$, where $\theta = \frac{v}{u}$ measures the tightness of labor market, satisfying $p(\theta) = \theta q(\theta)$.[8] If a job vacancy is filled at the probability $q(\theta)$, it will generate a positive value $J - V$, where J is the value of a filled job position, while V is the value of a vacant position. If the position is not filled, it will incur a flow cost, implied by c.

Each representative employer produces exclusively with labor by a constant-returns-to-scale function, and hence the profit is the difference between labor productivity A and labor cost w. Filled position can either be closed by employers at a probability of s or be interrupted by employees at a probability of σ due to FLP. For example, the FLP covered workers take the paid maternity leave, switch to some less labor-intensive position, or even sue the employers of breaking the FLP. The capital loss of closing a job position is $J - V$, while is $J - X$ by interruption, where X is the hiring cost of employers attributed to FLP. Besides, we suppose the net loss in productivity due to FLP is represented by ψ per period until the workers returns back to work normally at the probability γ. Thus, the value of a vacant position V, filled position J, and interruption position X can be represented as follows:

$$rV = q(\theta)(J - V) - c \tag{1}$$

$$rJ = A - w - \sigma(J - X) - s(J - V) \tag{2}$$

[6] We ignored the heterogeneity of workers and employers here, say the gender of worker, and ownership of firms. The reason is all these effect heterogeneity will be implied by the comparative static analysis later when we discuss the interest variable: the expansion of FLP.

[7] It is increasing in both of it arguments, concave, and homogeneous of degree 1.

[8] The probability of unemployed workers finding a job is increasing with respect to labor market tightness, while the probability of job vacancies being filled by a worker is decreasing in tightness, i.e. $p^{'}(\theta) > 0, q^{'}(\theta) < 0$.

$$rX = \gamma(J - X) - \psi \tag{3}$$

An unemployed individual is endowed with value *b*, and are with the probability to find a job at $p(\theta)$, which yields net value $W - U$ where *W*, *U* are the values she attributes to employment and unemployment, respectively. For an employed worker, she earns wage *w*, and can either lose her job at probability *s* due to employers or temporarily quit at probability σ due to FLP. A worker covered by FLP enjoys value *z* and returns to her previous position at rate γ. Hence, the net gain generated by FLP is $W - L$, where *L* stands for the value that she claims the benefits of FLP. Accordingly, the values associated with different working status (i.e., unemployed *U*, employed *W*, and claiming the benefits of FLP *L*) can be represented as follows:

$$rU = b + p(\theta)(W - U) \tag{4}$$

$$rW = w - s(W - U) - \sigma(W - L) \tag{5}$$

$$rL = z + \gamma(W - L) \tag{6}$$

To solve out the equilibrium of the system above, we impose two assumptions below.

Assumption 1-Free Entry for Job Vacancies Employers can open vacancies until its value becomes 0, i.e.,

$$V = 0 \tag{7}$$

Assumption 2-Bilateral Nash Bargaining A matching between employers and workers results in a surplus, which is defined as $S = J + W - U$. The allocation of this surplus between employers and employees is based on the result of a bilateral Nash Bargaining, where the wage maximizes the weighted product of workers' and employers' return from the job match, the first order condition satisfying

$$(1 - \beta)(W - U) = \beta J \tag{8}$$

where $\beta, 1 - \beta \in (0, 1)$ represent the bargaining power of workers and employers, respectively.

Equilibrium

Given labor market tightness, the unemployment rate (i.e, *u*) and employment rate *e* evolve based on backward-looking differential equations:

$$\dot{u} = se - p(\theta)u, \tag{9}$$

$$\dot{e} = -se + p(\theta)u. \tag{10}$$

When the economy reaches equilibrium, $\dot{u} = \dot{e} = 0$. Also, in the economy, individuals are either employed or unemployed, i.e.,

$$e + u = 1 \tag{11}$$

Then with Eqs. 9 and 11 we have the unemployment rate at equilibrium.

$$u = \frac{s}{s + p(\theta)} \tag{12}$$

$$e = \frac{p(\theta)}{s + p(\theta)} \tag{13}$$

With Eqs. 1 and 7, we know the expected benefits of filling a position must equal its cost of opening a vacancy, and thus:

$$J = \frac{c}{q(\theta)} \tag{14}$$

Also, derived by Eqs. 2 and 3, we know:

$$J = \frac{(r + \gamma)(A - w) - \sigma\psi}{r(r + \sigma + s) + \gamma(s + r)} \tag{15}$$

With Eqs. 4, 5, 6, and 15, as well as Eq. 8, the wage at equilibrium can be derived as

$$w = (1 - \beta)b + \beta(A + c\theta) + \frac{\sigma}{r + \gamma}\left(\beta\left(\frac{c\,(p(\theta) + r)}{q(\theta)} - \psi\right) - (1 - \beta)(z - b)\right) \tag{16}$$

In the Eq. 16, the first two terms on the right-hand side are the elements derived from the standard searching and matching model, while the last term indicates how wages are affected by FLP through the benefits and costs to both workers and employers: (1) employers face a productivity loss ψ due to FLP, but it saves the cost of opening a vacancy for a position by $B = \frac{c\,(p(\theta)+r)}{q(\theta)}$. If $B - \psi$ is positive (negative), then employers tend to pay a higher (lower) wage for FLP covered workers; (2) workers need to compare the benefits of claiming their FLP (i.e, z) with the benefits of being unemployed (i.e, b). If $z - b$ is positive (negative), she tends to bear wage reduction (not work unless wage increases).[9]

[9] Detailed calculation can be found in Appendix 9.

Testable Implications: Effect of FLP on Employment and Wages

In order to analyze the effects of FLP on employment and wages, we rewrite the equations at equilibrium Eqs. 14, 15 and 16 which determines the labor market tightness θ and wage w:

$$\frac{(r+\gamma)(A-w)-\sigma\psi}{r(r+\sigma+s)+\gamma(s+r)}q(\theta)=c$$

$$w=(1-\beta)b+\beta(A+c\theta)+\frac{\sigma}{r+\gamma}\left(\beta\left(B-\psi\right)-(1-\beta)(z-b)\right)$$

We focus on discussing the parameters ψ, z, which measures the productivity loss of employers and benefits of workers attributed to FLP, respectively. The details of comparative static analysis can be found in the Appendix A.

Propositions 1. $\frac{d\theta}{d\psi}>0, \frac{du}{d\psi}<0$, the productivity loss caused by FLP makes the labor market more tight, and raises the employment.

2. $\frac{d\theta}{dz}>0, \frac{du}{dz}<0$, the benefits of FLP makes the labor market more tight, and raises the employment.
3. $\frac{dw}{d\psi}<0$, the productivity loss due to FLP decreases wages.
4. $\frac{dw}{dz}<0$, the benefits of FLP decreases wages.
5. If $D_\sigma<\frac{D}{\sigma}, D_\beta<\frac{D}{\beta}, D\left(Y_w+\sigma Y_{w\sigma}\right)<\sigma Y_w D_\sigma$ hold, then $\frac{\partial^2\theta}{\partial z\partial\sigma}, \frac{\partial^2\theta}{\partial z\partial\beta}, \frac{\partial^2\theta}{\partial\psi\partial\sigma}, \frac{\partial^2\theta}{\partial\psi\partial\beta}<0$ and $\frac{\partial^2 w}{\partial z\partial\sigma}, \frac{\partial^2 w}{\partial z\partial\beta}, \frac{\partial^2 w}{\partial\psi\partial\sigma}, \frac{\partial^2 w}{\partial\psi\partial\beta}>0$. The positive impact of z and ψ on tightness would be smaller as β, σ get higher, while the negative impact of z and ψ on wage would be smaller as β, σ get higher.[10]

Intuitively, FLP increased the attachment of child-bearing age women in the labor market. It may have a positive direct effect on job creation: the benefit of opening a vacancy increases and thus the tightness of labor market and labor participation increases. However, the direct effect on wages is negative since the loss in productivity may outweigh the cost savings of opening a vacancy for employers, as well as the coverage benefits of workers tend to bear wag compression.

The FLP doesn't explicitly target the female workers based on their age, but in fact women at child-bearing age are mostly influenced by FLP. Women at other age ranges (i.e. older) are not primarily targeted by FLP, and hence they experience no increase or a relatively smaller increase in ψ, z than those younger female workers. Additionally, the main heterogeneity between female worker at child-bearing age and beyond child-bearing age is the probability that they claim the benefit provided by FLP, which is indicated as σ in this model: the child-bearing age females are characterized with a higher benefit-claiming probability σ, as well the enhanced

[10] Where $D=\frac{\partial Y}{\partial\theta}\frac{\partial Z}{\partial w}-\frac{\partial Z}{\partial\theta}\frac{\partial Y}{\partial w}, D_\sigma=\frac{\partial D}{\partial\sigma}, \quad D_\beta=\frac{\partial D}{\partial\beta}, Y_w=-\frac{(r+\gamma)}{r(r+s)+(\sigma+\gamma)r+\gamma s}$, $Y_{w\sigma}=\frac{(r+\gamma)r}{(r(r+s)+(\sigma+\gamma)r+\gamma s)^2}$.

bargaining power β. Hence, $\partial^2 w/(\partial\sigma\,\partial z) > 0, \partial^2 w/(\partial\beta\,\partial z) > 0$ represent that wage compression may also spill over to those female workers will little probability to claim the FLP benefits, or relatively smaller bargaining power (i.e, older workers). The reason underlying is the probability of taking-up (bargaining power) may reshape the mapping between labor market tightness and wage: σ (or β) and FLP generosity *z* behave more like substitutes rather than complements, and thus a higher probability σ (or bargaining power β) to claim FLP makes the wage compression effects of *z* weaker instead of stronger. The same is true for outcomes on labor participation ($\partial^2\theta/(\partial\sigma\,\partial z) > 0, \partial^2\theta/(\partial\beta\,\partial z) > 0$), as well as considering the effects of $\psi(i.e., \partial^2 w/(\partial\psi\partial\beta), \partial^2\theta/(\partial\psi\partial\beta), \partial^2 w/(\partial\psi\partial\sigma), \partial^2\theta/(\partial\psi\partial\sigma))$.

Accordingly, we propose the following hypothesis to test in empirical analysis:

Hypothesis 1. The Direct Effects of FLP on Labor Participation: For female workers of child-bearing age, their labor participation in response to FLP is increasing.

Hypothesis 2. The Direct Effects of FLP on Wage: For female workers of child-bearing age, their wage in response to FLP is decreasing.

Hypothesis 3. The Spill-over Effects of FLP on Labor Participation: For female workers beyond child-bearing age, their labor participation in response to FLP is increasing.

Hypothesis 4. The Spill-over Effects of FLP on Wage: For female workers beyond child-bearing age, their wage in response to FLP is also decreasing.

Data

Data Source

This study uses data from the China Family Panel Studies (CFPS), a national survey conducted by the Institute of Social Sciences Survey (ISSS) at Peking University. The CFPS is nationally representative, covering 25 provinces, cities, and municipalities, and includes information from approximately 16,000 households. The first comprehensive wave of data collection began in 2010 after a pilot survey conducted in major cities (Beijing, Shanghai, Shenzhen) during 2008-2009. Subsequent waves were conducted every two years, allowing longitudinal tracking of individual, family, and community-level information. This study focuses on the period 2010-2014 to avoid the confounding effects of policy changes introduced after 2016, such as changes in maternity leave policies. Data from 2008-2009 are used to perform placebo tests, providing a baseline without policy impacts.

Sample Selection and Grouping

The analytical sample includes people aged 20 to 60 years, excluding those who are retired, self-employed, or unable to work due to physical disabilities. This ensures the inclusion of the working-age population most likely to be affected by the policies under examination. Respondents are classified according to the industry, occupation, and worker classification codes designated by CFPS.

Workers in urban registered organizations—state agencies, state-owned or collective enterprises, public institutions, social organizations, and other formal employers—are classified as being exposed to both the two-child policy reforms and the Female Labor Protection (FLP) Provisions. The control group consists of individuals working in rural agricultural work and other unregistered jobs, who are primarily affected only by the two-child policy changes because the FLP does not apply to them. This formal–informal split allows us to compare groups that face the same fertility-policy environment but differ in their exposure to the FLP, and thus to isolate the incremental effect of the FLP. Male counterparts in both sectors are included as an additional control group to account for the systematic difference between sectors.

Summary Statistics

Table 1 provides summary statistics for the key variables used in the analysis, disaggregated by sex and employed sectors. The sample consists of individuals aged 20 to 60, with an average age of approximately 41 years. From column (5) of Table 1, it displays that individuals work in formal sectors are noticeably younger than those in informal sectors (38.3 versus 44.0 years), and are much less likely to have rural hukou (41.3% versus 95.4 %). Also, they tend to have better health and higher educational achivement, less and younger children than those in informal sectors. Their family background differs as well: formal workers are more likely to have mothers alive (74.5% versus 55.4%). Although they are less likely to have support from live spouse(84.1% versus 94.3%), their spouse are more likely to be at man-

Table 1 Descriptive statistics

	(1)	(2)	(3)	(4)	(5)
Variable	Overall	Female	Male	Informal Sector	Formal Sector
Age (years)	41.38 (10.79)	40.77 (10.65)	41.99 (10.90)	44.01 (9.67)	38.31 (10.29)
Married (%)	88.7	90.3	87.1	94.2	83.8
Rural Hukou (%)	73.2	75.5	70.8	95.4	41.3
Health (1–5 scale)	1.71 (0.91)	1.80 (0.97)	1.62 (0.83)	1.81 (1.00)	1.54 (0.70)
Education (years)	2.72 (1.32)	2.53 (1.35)	2.91 (1.27)	2.16 (1.01)	3.65 (1.32)
Children (count)	1.63 (0.95)	1.69 (0.94)	1.58 (0.96)	2.00 (0.91)	1.14 (0.72)
Children under 3 (%)	10.3	10.4	10.2	8.9	10.9
BMI	22.60 (3.26)	22.26 (3.25)	22.95 (3.23)	22.53 (3.18)	22.85 (3.27)
Mother Alive (%)	63.2	61.1	65.3	55.4	74.6
Spouse Alive (%)	88.9	90.4	87.3	94.3	84.1
Spouse Manages (%)	4.5	6.8	2.2	2.7	7.5
Adjusted Income (Yuan)	12,098.08 (22,850.35)	7,811.74 (16,436.75)	16,382.76 (27,153.94)	5,680.60 (17,194.71)	25,234.46 (27,752.21)
Participation (%)	84.8	77.8	91.8	96.1	93.1
Giving Birth (%)	5.3	5.3	5.4	3.6	7.3
Observation	8,009	3,786	4,223	5,040	2,969

Notes: Robust standard errors are in parentheses

agerail positions. Adjusted income and labor force participation vary significantly across employed sectors. Individuals employed in the formal sector earning an average adjusted income of 25,234 Yuan and exhibiting high labor force participation (93.1%), compared to 5,681 Yuan and 96.1% participation for those in the informal sector. The probability of giving birth in the last 2 years is higher in the formal sector (7.3% versuse 3.6%).

Birth Rate Analysis and Subgroup Motivation

Figure 2 presents the the weighted counts of live birth per 1000 women of given age, stratified by total births and birth order, using data from the China Family Panel Studies (CFPS) 2010. It is calculated as the number of births within a specific age group and birth order (e.g. first child, second child, or third and higher) divided by the total population of mothers in the same age group, and wieghted by the provided sampling weights. This approach provides a clear depiction of how fertility patterns vary by age and birth order.

It reveal clear trends that women's child bearing in China is compressed into their 20s and early 30s. First births are most common among younger mothers in their early twenties, but decline sharply thereafter. In contrast, second and subsequent births show delayed peaks, with higher-order births occuring later in the reproductive lifecycle. After age 35, the total chilrbirth declines significantly, highlighting both biological and societal constraints on fertility as women age.

These patterns provide a compelling basis for age-based subgroup analysis in the following empirical section. Women aged 20 to 35 constitute the prime birth

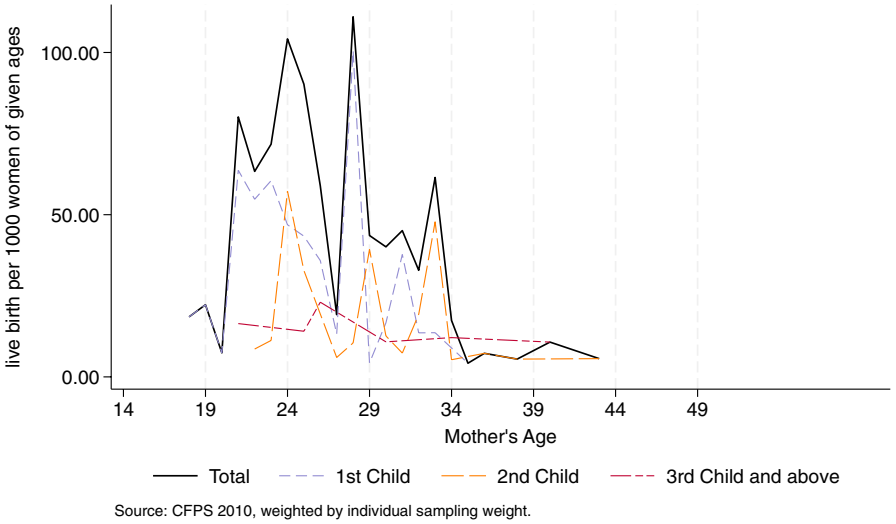

Fig. 2 Live Birth Per 1000 Women by Age. *Notes:* This figure shows the birth rate distribution by maternal age, classified by birth order. Birth rates peak between ages 21-34, with first births occurring earlier on average. Data: CFPS 2010

age group, accounting for the majority of childbearing behavior, while those over 35, who exhibit markedly fewer child-bearing, represent a transitional group less engaged in reproductive roles. These distinctions are crucial for understanding how fertility decisions interact with policy interventions, as the timing and likelihood of childbearing are inherently linked to labor perfromance.

Empirical Strategy

Setup and Identification Intuition

The goal is to estimate the causal effect of the Female Labor Protection (FLP) Provisions on women's labor market outcomes. The FLP applies to employees in registered urban entities ("employing units") with formal contracts, maternity insurance schemes, and workplace inspections. We refer to these jobs as the *formal* sector. Rural agricultural work and other unregistered jobs in the CFPS are overwhelmingly self-employment or unpaid family labor without formal contracts or maternity insurance, and are excluded from the contract-based enforcement system of the FLP. We group these jobs as the *informal* sector and treat them as not directly covered by the Provisions.[11]

Let $g \in \{\text{female}, \text{male}\}$ index gender, $s \in \{\text{formal}, \text{informal}\}$ index sector, and $t \in \{\text{pre}, \text{post}\}$ index time, where the pre-policy period is 2010 and the post-policy period includes 2012 and 2014. Denote by Y_{gst} the mean outcome for group (g, s, t). A simple difference-in-differences (DID) for women compares the change in outcomes for women in formal jobs to the change for women in informal jobs:

$$\text{DID}_{\text{female}} = \left(Y_{\text{female,formal,post}} - Y_{\text{female,formal,pre}}\right) - \left(Y_{\text{female,informal,post}} - Y_{\text{female,informal,pre}}\right). \quad (17)$$

This comparison differentiate out shocks that affect all women similarly over time, but it still allows for sector-specific trends that could confound the policy effect (for example, if formal-sector wages would have risen faster than informal-sector wages even in the absence of the FLP).

To net out such sector trends, we exploit the fact that the FLP directly targets women and should not affect men. Men provide an additional control group for sector-specific shocks. The analogous DID for men is

$$\text{DID}_{\text{male}} = \left(Y_{\text{male,formal,post}} - Y_{\text{male,formal,pre}}\right) - \left(Y_{\text{male,informal,post}} - Y_{\text{male,informal,pre}}\right), \quad (18)$$

and the triple-difference (DDD) estimator is the difference between these two DIDs:

$$\text{DDD} = \text{DID}_{\text{female}} - \text{DID}_{\text{male}}. \quad (19)$$

[11] In robustness checks, we show that restricting the analysis to urban workers and to urban households fully subject to the strict one-child policy yields very similar wage results, suggesting that the main findings do not hinge on rural-urban split.

This statistic measures how the formal–informal gap changes for women relative to men after the policy. Under the assumption that, in the absence of the FLP, the evolution of the formal–informal gap would have been the same for women as for men (a group-specific parallel-trends assumption), DDD identifies the causal effect of the FLP on women's outcomes in the policy environment prevailing after 2012.

It is important to note that this design does not require fertility incentives or birth-planning rules to be identical across formal and informal sectors. Any sector-specific time trend that affects men and women in the same way—including heterogeneous exposure to the staged relaxation of the one-child policy across urban and rural hukou holders—is differenced out by comparing women to men within sectors. The triple-difference parameter therefore captures the net change of FLP to female workers from sector differences. Moreover, even though some informal jobs are partially exposed to the FLP in practice, treating them as fully untreated biases the estimates toward zero; the wage effects we report can therefore be interpreted as conservative, in the sense of providing a lower bound on the impact of the FLP relative to a fully untreated control group.

Triple-difference regression

We implement the DDD estimator at the individual level using three binary indicators. The variable Post_t equals 1 in post-policy waves (2012 and 2014) and 0 in the pre-policy wave (2010). The variable Formal_i equals 1 if individual i works in the formal sector (wage employment in registered urban organizations where the FLP applies) and 0 if i works in the informal sector (rural agricultural and other unregistered jobs). The variable Female_i equals 1 for women and 0 for men.

Our baseline specification is

$$\begin{aligned} Y_{it} = {} & \alpha + \beta_1 \,\mathrm{Post}_t + \beta_2 \,\mathrm{Formal}_i + \beta_3 \,\mathrm{Female}_i \\ & + \beta_4 \left(\mathrm{Post}_t \times \mathrm{Formal}_i\right) + \beta_5 \left(\mathrm{Post}_t \times \mathrm{Female}_i\right) + \beta_6 \left(\mathrm{Formal}_i \times \mathrm{Female}_i\right) \\ & + \beta_7 \left(\mathrm{Post}_t \times \mathrm{Formal}_i \times \mathrm{Female}_i\right) + X_{it}'\delta + \rho_p + \varepsilon_{it}, \end{aligned} \tag{20}$$

where Y_{it} is the outcome for individual i in year t; X_{it} is a vector of individual controls (age, age squared, education, marital status, health, number and age of children, the existence of live mother, spouse, and whether the spouse has a managerial job); ρ_p denotes province fixed effects; and ε_{it} is an error term. Standard errors are robust to heteroskedasticity.[12]

The coefficient of interest is β_7, on the triple interaction $\mathrm{Post}_t \times \mathrm{Formal}_i \times \mathrm{Female}_i$. This term measures the additional change in the outcome for women in formal jobs after the FLP, relative to (i) women in informal jobs, (ii) men in formal jobs, and (iii) men in informal jobs. For binary outcomes such as labor force participation, β_7 is a change in probability on the 0–1 scale (a change in percentage points). For wage outcomes, the dependent variable is the natural logarithm of income, so a coefficient β_7 can be interpreted approximately as a percentage change in wages. Specifically,

[12] In robustness checks we show that the main results are very similar when standard errors are clustered at the province level.

$$\%\Delta Y \approx 100 \times \left(e^{\beta_7} - 1\right). \tag{21}$$

The FLP was introduced in a period when fertility policies were gradually being relaxed, especially through the selective and universal two-child policies. Our design does not attempt to separately identify the impact of these fertility-policy changes. Instead, the Post indicator is defined around the introduction of the FLP, and the triple-difference parameter β_7 is interpreted as the effect of the FLP on women in formal jobs in this evolving policy environment, relative to men and to informal workers. Year fixed effects absorb aggregate changes common to all groups, including those induced by fertility-policy reforms.

We assess the credibility of the identifying assumptions in three ways. First, we estimate placebo versions of the model using male outcomes only and find no corresponding effects for men; for wages, formal-sector men actually see faster wage growth over the period, which works against our main result. Second, we re-estimate the triple-difference specification on urban-only samples and on an urban subsample that excludes households with pre-existing exemptions from the one-child policy (where the first-born child is a daughter in rural area, or the household is from ethinic minority), obtaining wage effects that are very similar to the baseline and participation effects that remain small. Third, we examine fertility outcomes, using both the probability of having a birth and the number of children as dependent variables, and find no evidence of increased fertility among treated women. Taken together, these exercises support the view that β_7 is capturing a genuine deterioration in labor market outcomes for formally employed women after the FLP, rather than spurious patterns driven by sectoral trends, contamination from fertility policies.

Results

This section presents the results from the triple-difference (DDD) analysis, aimed at isolating the distinct effects of the FLP.

Table 2 reports the estimated triple-difference coefficient on Post × Female × Formal from Eq. 20 for labor force participation (columns 1–3) and log annual income (columns 4–6), for the full sample and for women aged 20–35 and 35–55.

For participation, the DDD estimates are small and statistically insignificant for the full sample and for women aged 20–35, the reason underlying might be that the young female labor force participation in China is relatively high and even saturated (Meng et al. 2023). For women aged 35–55, the coefficient is around -0.058 and significant at the 10% level, which corresponds to a 5.8 percentage-point lower probability of being in the labor force for formally employed women after the FLP, relative to the combined comparison groups. This pattern is consistent with a modest reduction in participation among older women, while younger women's participation appears largely unchanged.

The wage response are much stronger. Across all specifications and age groups, the DDD coefficients on log annual income are large, negative, and precisely estimated, with values around -2.0 for the full sample and between -2.25 and -2.23 for the 20–35 and 35–55 age groups. Interpreted literally, a coefficient of about -2 in

Table 2 Regression results for participation and log wage

Dependent Variable/Age group	Participation			Log Wage		
	(1)	(2)	(3)	(4)	(5)	(6)
All Groups						
DDD Coefficient	-0.015	-0.014	-0.014	-2.020***	-2.015***	-2.014***
	(0.023)	(0.023)	(0.023)	(0.260)	(0.258)	(0.257)
Ages 20–35						
DDD Coefficient	0.002	0.005	0.005	-2.251***	-2.259***	-2.261***
	(0.043)	(0.042)	(0.042)	(0.452)	(0.450)	(0.446)
Ages 35–55						
DDD Coefficient	-0.058*	-0.058*	-0.058*	-2.227***	-2.227***	-2.227***
	(0.030)	(0.029)	(0.029)	(0.350)	(0.350)	(0.350)
Control Variables:						
Demographic Characteristics	No	Yes	Yes	No	Yes	Yes
Province Dummies	No	No	Yes	No	No	Yes

Notes: Each row reports the estimated triple-difference coefficient for the interaction Post × Female × Formal from specification Eq. 20. Columns (1)–(3) use a 0–1 indicator for labor force participation as the dependent variable; in these columns, the coefficients are measured in probability units, so a value of -0.058 corresponds to a 5.8 percentage-point change in participation. Columns (4)–(6) use the natural logarithm of annual labor income as the dependent variable; in these columns, coefficients can be interpreted approximately as percentage changes in income using $100 \times \left(e^{\beta} - 1\right)$. Demographic controls include age, age squared, education, marital status, health, and the number and age of children, whether the mother is alive, the spouse is alive, and whether the spouse has the managerial job. All regressions include the control variables indicated at the bottom of the table and province fixed effects. All regressions are weighted by survey weights. Robust standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

log income implies that, after the FLP, annual earnings for treated women in formal jobs are roughly 85–90% lower than those of the corresponding comparison groups ($100 \times (e^{-2} - 1) \approx -86\%$). The pattern across columns is clear: after accounting for common time shocks, sector-specific trends, and baseline gender gaps, women in formal jobs experience a steep relative drop in earnings in the post-policy period, while their labor force participation changes only modestly.

Table 3 examines how the FLP affects earnings separately for workers who stay with the same employer and sector between waves ("No New Job") and those who change employer but remain in the same sector ("Switch New Job"). Among stayers, the DDD coefficient is about -0.83 for the full sample, which corresponds to roughly a 55–60% decline in annual income for formally employed women relative to the comparison groups.[13] This effect is concentrated among older women: for ages 35–55, the coefficient is around -1.20, implying a drop on the order of 70%, while the estimates for ages 20–35 are smaller and not statistically significant.

For job switchers, the estimated income losses are even larger. The overall DDD coefficient of about -1.18 implies a roughly 70% reduction in income for formally employed women who change jobs after the FLP, relative to men and informal workers

[13] A coefficient β in logs can be translated as a percentage change using $100 \times \left(e^{\beta} - 1\right)$.

Table 3 Regression results for log income

Dependent Variable/Age group	No New Job			Switch New Job		
	(1)	(2)	(3)	(4)	(5)	(6)
All Groups						
DDD Coefficient	-0.827***	-0.824***	-0.824***	-1.180**	-1.184**	-1.183**
	(0.432)	(0.425)	(0.413)	(0.517)	(0.510)	(0.505)
Ages 20–35						
DDD Coefficient	-0.462	-0.469	-0.462	-1.027	-1.027	-1.026
	(0.845)	(0.817)	(0.794)	(0.913)	(0.903)	(0.890)
Ages 35–55						
DDD Coefficient	-1.202**	-1.200**	-1.199**	-1.880***	-1.879***	-1.878***
	(0.601)	(0.595)	(0.576)	(0.676)	(0.669)	(0.655)
Control Variables:						
Demographic Characteristics	No	Yes	Yes	No	Yes	Yes
Province Dummies	No	No	Yes	No	No	Yes

Notes: The dependent variable is the natural logarithm of income. Entries report the triple-difference coefficient on Post × Female × Formal from the specification in Eq. 20. Columns (1)–(3) use individuals who remain with the same employer between survey waves and stay in the same sector (formal or informal); this group is labelled "No New Job". Columns (4)–(6) use individuals who change employer between waves but remain within the same sector; this group is labelled "Switch New Job". Workers who move between the formal and informal sectors, or who enter or exit employment between waves, are excluded from this table and are instead captured in the participation analysis. Demographic controls include age, age squared, education, marital status, health, the number and age of children, whether the mother is alive, the spouse is alive, and whether the spouse has the managerial job. Province dummies are included where indicated. All regressions are weighted by survey weights. Robust standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

who also switch jobs. Among older women (35–55), the coefficient is close to -1.88, suggesting an income loss of more than 80%, whereas the corresponding estimates for younger women (20–35) are imprecise and not statistically distinguishable from zero. Taken together, these results indicate that the FLP is associated with very large relative earnings penalties for formally employed women, especially older workers and those who change jobs.

Table 4 examines whether the wage effects of the FLP differ across the income distribution by estimating the triple-difference specification separately for low- and high-income workers. The left panel reports results for the low-income subsample (i.e., lower than the medium wage). Across all urban workers, the DDD coefficient is -1.486 (column 1), implying that, after the policy, women in formal jobs experience an additional decline in annual income of roughly 75–80 percent. The effects are similar in magnitude for women aged 20–35 and 35–55 (columns 2 and 3), with coefficients around -1.5 and -1.4, respectively, and all three estimates are highly statistically significant.

The right panel shows the corresponding estimates for the high-income subsample (i.e., higher than the medium wage). Here, the triple-difference coefficients are positive but imprecisely estimated, and none are statistically different from zero at conventional levels. For example, in the full high-income sample (in column 4) the DDD estimate is 0.574, which would correspond to a sizable income gain if it were precisely estimated, but the standard error is large and the effect is not statistically signif-

Table 4 Triple-difference estimates for log income, by income group

	Low-income			High-income		
	All urban	Age 20–35	Age 35–55	All urban	Age 20–35	Age 35–55
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Female × Formal (DDD)	-1.486***	-1.541***	-1.414***	0.574	1.023	0.387
	(0.251)	(0.457)	(0.330)	(0.618)	(0.974)	(0.650)
Observations	23,284	5,747	13,721	4,055	1,616	2,194

Notes: The dependent variable is the natural logarithm of annual labor income. Entries report the triple-difference coefficient on the interaction Post × Female × Formal from specification Eq. 20. The sample is split into low- and high-income groups based on the medium wage in our sample. Columns (1)–(3) use the low-income subsample; columns (4)–(6) use the high-income subsample. Coefficients on the log scale can be interpreted approximately as percentage changes in income, using $100 \times \left(e^{\beta} - 1\right)$. For example, a coefficient of -1.5 corresponds to roughly a 75–80 percent lower income relative to the comparison groups. All regressions include the full set of individual controls and province fixed effects. All regressions are weighted by survey weights.Robust standard errors are in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

icant. The pattern is similar for younger and older high-income women in columns 5 and 6. The reason behind it might be only those lower wage earners are some susceptible to FLP benefits. Hence, it provides another evidence of the unintended effects of FLP that it ampflies the wage disparity within gender across different income groups.

Table 5 reports the triple-difference estimates for log monthly working hours. For the full sample, the DDD coefficient is 0.054 (s.e. 0.040), implying an increase of roughly 5–6% in hours for women in formal jobs relative to the comparison groups, but this effect is not statistically significant at conventional levels. The point estimate is similar for women aged 20–35 (0.059, s.e. 0.075) and essentially zero for those aged 35–55 (0.003, s.e. 0.054). Overall, there is no strong evidence that the FLP systematically reduced or increased monthly working hours for formally employed

Table 5 Effect of FLP on log monthly working hours (DDD estimates)

	All	Age 20–35	Age 35–55
	(1)	(2)	(3)
Post × Female × Formal (DDD)	0.054	0.059	0.003
	(0.040)	(0.075)	(0.054)
Observations	24,614	6,568	14,447

Notes: The dependent variable is the natural logarithm of total monthly working hours. Each coefficient is the estimate on the triple interaction Post × Female × Formal from specification Eq. 20, which measures the additional post–pre change in log hours for women in formal jobs, relative to women in informal jobs and to men in both sectors. Because the outcome is in logs, coefficients can be interpreted approximately as percentage changes using $100 \times \left(e^{\beta} - 1\right)$; for example, 0.054 corresponds to about a 5–6% change in hours. All regressions include the full set of demographic controls and province fixed effects. All regressions are weighted by survey weights. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

women, consistent with the interpretation that the large income losses documented in the wage regressions primarily reflect lower returns per hour rather than sizable adjustments in hours worked.

Figure 3 examines whether older women are disproportionately concentrated in sectors where FLP enforcement is likely to be stricter. The figure plots, for 2014, the distribution of employed women across three work types—public sector, formal private, and self-employed/family—separately for ages 20–35 and 35–55. Within each work type, the bars show the share of women accounted for by each age group.

Two patterns stand out. First, older women (35–55) represent a larger share of employed women than younger women (20–35) in all three work types, not just in the public sector. Second, the age composition of public-sector jobs is very similar to that of formal private jobs: in both cases, the share of older women is only modestly higher than the share of younger women. The most pronounced age skew appears in self-employed and family-based work, where older women account for the majority of workers. Thus, older women are not disproportionately clustered in public-sector jobs relative to other formal jobs, and the wage penalties documented in Section 6 cannot be explained solely by the composition of older women in formal sectors where FLP enforcement might be stricter.

Table 6 reports how the FLP relates to fertility. Panel A focuses on the extensive margin, where the dependent variable is an indicator for having at least one birth between survey waves. For the full sample of women (columns 1–3), the estimated coefficients are exactly zero up to three decimal places, with standard errors around

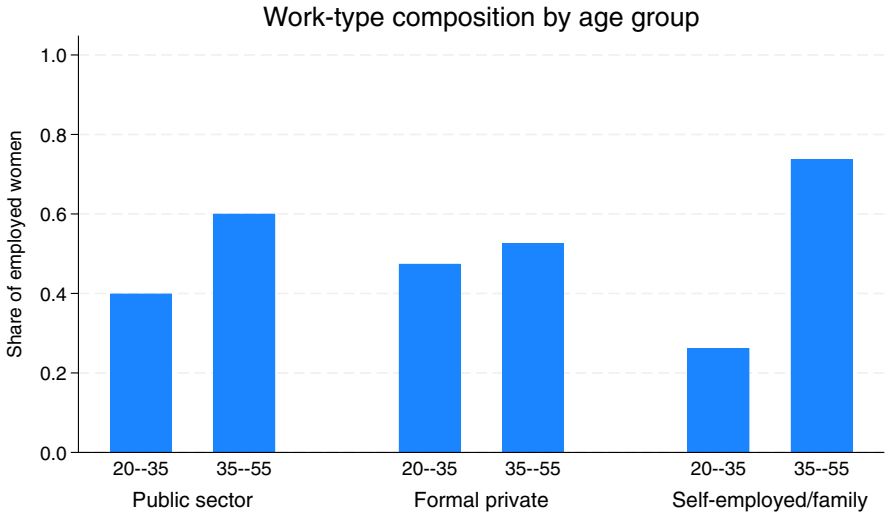

Fig. 3 Work-type composition by age group. *Notes:* The figure uses the CFPS 2014 wave to illustrate the distribution of employed women across work types by age group. "Public sector" includes government/party organizations, state-owned or collectively owned public institutions, and state-owned or state-controlled enterprises. "Formal private" includes private enterprises and firms invested in by foreign or Hong Kong/Macau capital. "Self-employed/family" includes individual and family businesses and village/community organizations

Table 6 Effect of FLP on fertility outcomes

	All			Age 20–35	Age 35–55
	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable = indicator for having a birth					
Post × Female × Formal (DDD)	0.000	0.000	0.000	0.000	-0.000
	(0.020)	(0.019)	(0.019)	(0.093)	(0.007)
Panel B: Dependent variable = number of children					
Post × Female × Formal (DDD)	-0.032	-0.032	-0.032	-0.194***	0.001
	(0.058)	(0.043)	(0.042)	(0.074)	(0.054)
Control Variables					
Demographics	No	Yes	Yes	Yes	Yes
Province Dummies	No	No	Yes	Yes	Yes
Observations	27,654	27,654	27,654	2,328	16,011

Notes: In Panel A, the dependent variable equals 1 if the respondent has at least one birth between survey waves and 0 otherwise (extensive margin). In Panel B, the dependent variable is the total number of children at the time of the survey, which within an age group is analogous to a cohort-specific total fertility rate (intensive margin). Each column reports coefficients from the triple-difference specification in Eq. 20. Column (1) includes only the treatment indicators and their interactions; column (2) adds individual demographic controls; column (3) further includes province fixed effects. Columns (4) and (5) restrict the sample to women aged 20–35 and 35–55, respectively, and include the full set of individual controls and province fixed effects. The coefficient labelled "Post × Female × Formal (DDD)" is the triple-difference estimator of interest. Coefficients in Panel A are on the 0–1 probability scale; coefficients in Panel B are in number of children. All regressions are weighted by survey weights. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

0.02. This means there is no detectable change in the probability of giving birth for women in formal jobs relative to the comparison groups. The same pattern appears when the sample is split by age. Among women aged 20–35 (column 4), who are in their prime childbearing years, the estimate is again essentially zero, although the standard error is larger because the sample is smaller. For women aged 35–55 (column 5), the estimate is very close to zero and estimated quite precisely. Nothing in Panel A suggests that the FLP made women in formal employment more likely to have a birth during the study period.

Panel B turns to the intensive margin, using the number of children as the outcome, which within an age group is a simple analogue to a cohort-specific total fertility rate. In the full sample (columns 1–3), the triple-difference coefficient is about -0.03 and not statistically significant, so there is no clear change in completed fertility when all women are pooled. The coefficient for women aged 35–55 (column 5) is also extremely small and imprecise, again pointing to no meaningful effect. The one

exception is the group of women aged 20–35 in the formal sector (column 4): here the coefficient is around -0.19 children and statistically significant at the 1% level. This estimate suggests a modest reduction in the number of children for younger women with formal jobs, relative to same-age women in informal work and to men in both sectors.

Putting the two panels side by side, the picture is quite clear. The FLP does not increase the fertility either in extension nor intensive margins. Among prime-age women in the formal sector, the point estimates actually tilt in the opposite direction, with a small but significant decline in the number of children. This pattern fits naturally with the wage results: stronger formal protections that are accompanied by wage penalties and hiring concerns do not encourage more childbearing and may instead give some younger women an additional reason to postpone or avoid an extra birth.

Placebo Test

Use Pilot Datat of 2008-2009

The validity of the Difference-in-Differences (DID) and Difference-in-Difference-in-Differences (DDD) models relies on the parallel trends assumption, which posits that the treated and control groups would have followed similar trends in the absence of the intervention. To test this assumption, we performed a placebo analysis using data from 2008 and 2009, which preceded the implementation of the policies. The results are summarized in Table 7.

Table 7 Regression results for participation and log income (placebo test)

Dependent Variable	Participation			Log Income		
	(1)	(2)	(3)	(4)	(5)	(6)
All Groups						
DDD Coefficient	0.1477	0.1105	0.1107	0.0710	0.1298	0.1460
	(0.111)	(0.124)	(0.124)	(0.214)	(0.204)	(0.202)
R-squared	0.2966	0.3579	0.3579	0.1579	0.2620	0.2678
Ages 20–35						
DDD Coefficient	0.1023	0.1047	0.1046	-0.1264	0.1698	0.0659
	(0.026)	(0.026)	(0.026)	(0.475)	(0.398)	(0.425)
R-squared	0.4374	0.4690	0.4698	0.0718	0.1973	0.2037
Ages 35–55						
DDD Coefficient	0.1155	0.1408	0.1378	-0.0642	-0.1106	-0.0683
	(0.155)	(0.153)	(0.152)	(0.302)	(0.296)	(0.291)
R-squared	0.2419	0.3160	0.3168	0.1408	0.2468	0.2542
Control Variables:						
Demographic Controls	No	Yes	Yes	No	Yes	Yes
Province Dummies	No	No	Yes	No	No	Yes

Notes: Demographic controls include age, age squared, education, marital status, health, the number and age of children, whether the mother is alive, the spouse is alive, and whether the spouse has a managerial job. All regressions are weighted by survey weights. Robust standard errors are in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 7 includes the regression results for DDD models, incorporating demographic and provincial controls. Across all groups, the DDD coefficients are statistically insignificant for both participation and log wage outcomes during the placebo period. For example, the DDD coefficient for participation in the 20–35 age group is 0.1046 (SE = 0.026, p = 0.475), and for log wages in the 35–55 age group, it is -0.0683 (SE = 0.291, p = 0.814). These findings further confirm that treated and control groups exhibited similar trends in the pre-policy period.

The results of the placebo test provide robust evidence supporting the assumption of parallel trends for both the DID and the DDD models. The lack of significant Diff-in-Diff and DDD coefficients during the pre-policy period implies that any observed differences in outcomes post-intervention can be attributed to the policy effects rather than preexisting trends. These results validate the use of the DID and DDD frameworks for analyzing the impacts of the policy intervention.

Male Labor Market Outcomes

As an additional credibility check, we examine whether the policy period is associated with similar changes in labor market outcomes for men. Since the female labor protection rule directly targets women of childbearing age, we do not expect to find analogous treatment effects among male workers. If our baseline estimates were instead driven by sector-specific shocks or general macro trends, we would expect to observe comparable patterns for men in formal and informal sectors.

We therefore estimate a placebo difference-in-differences specification restricting the sample to men and using the interaction between a post-policy indicator and the formal-sector indicator as the treatment variable. The specification is otherwise identical to our baseline regressions: we consider male labor force participation and log income as outcomes, control for the same set of individual characteristics, include province fixed effects, and apply survey weights. Appendix Table 8 reports the results.

Columns (1)–(3) of Appendix Table 8 show the estimates for male labor force participation. The coefficient on the Post × Formal interaction is small in magnitude

Table 8 Placebo: male labor market outcomes

Dependent Variable	Participation			Log Income		
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.068***	-0.071***	-0.071***	0.222***	0.207***	0.207***
	(0.011)	(0.010)	(0.010)	(0.059)	(0.057)	(0.057)
Formal sector	0.121***	0.148***	0.156***	0.925***	0.627***	0.551***
	(0.008)	(0.011)	(0.011)	(0.040)	(0.044)	(0.043)
Post × Formal	0.001	0.004	0.004	0.175***	0.190***	0.192***
	(0.012)	(0.012)	(0.012)	(0.065)	(0.063)	(0.062)
Demographic Controls	No	Yes	Yes	No	Yes	Yes
Province Dummies	No	No	Yes	No	No	Yes
Observations	13,353	13,353	13,353	8,504	8,504	8,504

Notes: These results are restricted to men. Demographic controls include age, age squared, education, marital status, health, the number and age of children, whether the mother is alive, the spouse is alive, and whether the spouse has a managerial job. All regressions are weighted by survey weights. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

and statistically insignificant across all specifications. This indicates that the policy period did not induce differential changes in participation between men in formal versus informal sectors, alleviating concerns that our main participation results for women are driven by broader sectoral or macroeconomic shocks.

Columns (4)–(6) report the placebo results for log income. Here, the Post × Formal coefficient is positive and statistically significant, implying that male workers in formal sectors experienced faster wage growth than men in informal sectors after the policy period. This pattern contrasts sharply with our main finding that wages for women of childbearing age in formal sectors decline relative to appropriate control groups following the policy. If anything, the male placebo suggests that general wage trends in formal sectors work against our main result. Taken together, the absence of a participation effect for men and the opposite-signed wage response reinforce the interpretation that the negative wage effects we document are specific to women and arise from the interaction of the female labor protection rule with fertility-related costs, rather than from general shocks to formal sectors or the macroeconomy.

Robustness Check

Urban Only Samples

A first concern is that the baseline comparison between formal urban workers and informal rural workers might pick up broader urban–rural differences in institutions or labor market conditions rather than the effect of the FLP itself. To address this, Table 9 re-estimates the triple-difference specification on an urban-only sample. The data are restricted to individuals with urban hukou, and the formal sector is defined as wage employment in registered urban organizations; the comparison group consists of urban informal workers and men in both sectors.

Table 9 Urban-only sample: effect of FLP on participation and log income

	All	Age 20–35	Age 35–55
	(1)	(2)	(3)
Panel A: Dependent variable = participation (0–1)			
Post × Female × Formal (DDD)	0.024	0.223	-0.036
	(0.056)	(0.182)	(0.074)
Observations	7,035	954	3,951
Panel B: Dependent variable = log income			
Post × Female × Formal (DDD)	-3.720***	-4.113***	-4.186***
	(0.498)	(0.995)	(0.633)
Observations	7,098	2,211	3,958

Notes: Each entry reports the coefficient on the triple interaction Post × Female × Formal from regressions estimated on the urban subsample (urban hukou only). The formal sector is defined as wage employment in registered urban organizations; the comparison group consists of urban informal workers (self-employment and other unregistered sector jobs) and men in both sectors. Panel A uses a 0–1 indicator for labor force participation as the dependent variable. Panel B uses the natural logarithm of income. All specifications include the full set of individual demographic controls and province fixed effects. All regressions are weighted by survey weights. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A of Table 9 uses labor force participation as the outcome. The triple-difference coefficient on Post × Female × Formal is small and statistically insignificant estimated in all cases: it is close to zero for the full urban sample and for women aged 35–55, and, although the point estimate is positive for women aged 20–35, the standard error is large and the estimate is far from conventional significance thresholds. These estimates do not point to a systematic change in participation for women in formal urban jobs after the FLP.

Panel B turns to log income. Here the pattern is much clearer. The triple-difference coefficients are large, negative, and highly significant for the full urban sample and for both age groups. The estimates are about -3.7 for all urban workers and roughly -4.1 to -4.2 for women aged 20–35 and 35–55. Urban formal workers earn more and experience faster wage growth than urban informal workers on average, but these gains are markedly weaker for women than for men. The negative Post × Female × Formal terms show that, even when the sample is limited to urban hukou holders facing similar institutional environments, women in formal jobs see their wages fall sharply relative to men and to urban informal workers after the FLP.

Table 10 examines fertility outcomes in the same urban setting. Panel A uses an indicator for having at least one birth between survey waves, and Panel B uses the number of children as an intensive-margin measure. Across the full sample and within the 20–35 and 35–55 age groups, the triple-difference coefficients are very small and statistically indistinguishable from zero. The estimates for the probability of a birth are essentially zero with tight confidence intervals, and the coefficients for the number of children are modest and not statistically significant, including for women in their prime childbearing years. Within the urban sample, where fertility-policy rules are relatively homogeneous, there is no indication that the FLP raised fertility on either the extensive or the intensive margin.

Table 10 Urban-only sample: effect of FLP on fertility

	All	Age 20–35	Age 35–55
	(1)	(2)	(3)
Panel A: Dependent variable = indicator for having a birth			
Post × Female × Formal (DDD)	0.000	0.000	-0.000
	(0.036)	(0.192)	(0.010)
Panel B: Dependent variable = number of children			
Post × Female × Formal (DDD)	-0.013	-0.116	0.009
	(0.078)	(0.174)	(0.107)
Observations	7,098	2,211	3,958

Notes: Each entry reports the coefficient on the triple interaction Post × Female × Formal from regressions estimated on the urban, policy-included subsample. The sample is restricted to individuals with urban hukou. The formal sector is defined as wage employment in registered urban organizations; the comparison group consists of urban informal workers and men in both sectors. In Panel A, the dependent variable equals 1 if the respondent has at least one birth between survey waves and 0 otherwise (extensive margin). In Panel B, the dependent variable is the total number of children at the time of the survey (intensive margin). All specifications include individual demographic controls and province fixed effects. All regressions are weighted by survey weights. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11 Urban, strict one-child sample: effect of FLP on participation and log income

	All	Age 20–35	Age 35–55
	(1)	(2)	(3)
Panel A: Dependent variable = participation (0–1)			
Post × Female × Formal (DDD)	0.019	0.185	-0.035
	(0.057)	(0.184)	(0.074)
Panel B: Dependent variable = log income			
Post × Female × Formal (DDD)	-3.902***	-4.096***	-4.185***
	(0.492)	(1.053)	(0.640)
Observations	6,805	2,111	3,793

Notes: Each entry reports the coefficient on the triple interaction Post × Female × Formal from regressions estimated on a restricted urban subsample. The sample is limited to individuals with urban hukou who were fully subject to the strict one-child policy in 2010. The formal sector is defined as wage employment in registered urban organizations; the comparison group consists of urban informal workers and men in both sectors. Panel A uses a 0–1 indicator for labor force participation as the dependent variable. Panel B uses the natural logarithm of income. All specifications include individual demographic controls and province fixed effects. All regressions are weighted by survey weights. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Urban Sample Subject to Strict One-Child Policy

China's birth-planning rules were not applied uniformly across all households. Rural couples with a first-born daughter and many ethnic minority households were effectively allowed more than one birth even before the selective and universal two-child reforms. Hence, there exists a possibility that those exempt groups may not respond to FLP because they face with more relaxed birth control policy rather than they are not covered by CFP. To rule out the possibility that results are driven by these pre-existing exemptions, and make individuals more comparable, we restrict sample to include only individuals with urban hukou and households that were fully subject to the one-child rule in 2010.[14] In this subsample, all treated and comparison groups face a common, strict fertility-policy environment prior to the FLP.

Table 11 reports the triple-difference estimates for participation and log income in this restricted urban sample. Panel A shows that the participation effects remain small and imprecisely estimated: the coefficient on Post × Female × Formal is close to zero for the full sample (0.019 with a standard error of 0.057) and for women aged 35–55 (-0.035, s.e. 0.074). For women aged 20–35, the point estimate is positive (0.185), but the standard error is large and the estimate does not reach conventional significance thresholds. There is little evidence that the FLP altered extensive-margin participation once we focus on urban workers who were fully constrained by the one-child policy. Panel B paints a much sharper picture for wages. In all three columns, the triple-difference coefficient on Post × Female × Formal is large, negative, and highly significant, with values around -3.9 to -4.2. In this strict urban sample, women in formal jobs experience a substantial deterioration in log income relative

[14] Concretely, we keep respondents with urban hukou and drop households that were exempt from strict one-child enforcement. We define an indicator policyexclude2010 equal to one if either the respondent or the spouse has rural hukou and the first child is a girl, or if the respondent belongs to an ethnic minority group, and zero otherwise. The strict subsample used in Tables 11 and 12 restricts to observations with urban hukou and policyexclude2010 = 0.

Table 12 Urban, strict one-child sample: effect of FLP on fertility

	All	Age 20–35	Age 35–55
	(1)	(2)	(3)
Panel A: Dependent variable = indicator for having a birth			
Post × Female × Formal (DDD)	-0.000	0.000	0.000
	(0.037)	(0.196)	(0.011)
Panel B: Dependent variable = number of children			
Post × Female × Formal (DDD)	-0.011	-0.144	0.008
	(0.080)	(0.179)	(0.105)
Observations	6,858	945	3,798

Notes: Each entry reports the coefficient on the triple interaction Post × Female × Formal from regressions estimated on the urban, strict one-child subsample. The sample is restricted to individuals with urban hukou and households that were fully subject to the one-child policy in 2010. The formal sector is defined as wage employment in registered urban organizations; the comparison group consists of urban informal workers and men in both sectors. In Panel A, the dependent variable equals 1 if the respondent has at least one birth between survey waves and 0 otherwise (extensive margin). In Panel B, the dependent variable is the total number of children at the time of the survey (intensive margin). All specifications include individual demographic controls and province fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

to men and to urban informal workers after the FLP, closely mirroring the baseline wage results.

Table 12 turns to fertility outcomes in the same restricted sample. Panel A uses an indicator for having at least one birth between survey waves as the dependent variable, and Panel B uses the number of children as an intensive-margin measure. In both panels, the triple-difference coefficients are small and statistically indistinguishable from zero for the full sample and for both age groups. The estimates for the probability of a birth are essentially zero with tight confidence intervals, and the coefficients for the number of children are modest in magnitude and far from statistical significance, including for women aged 20–35. When the analysis is confined to urban households that were fully subject to the one-child rule, the FLP does not appear to raise fertility on either the extensive or the intensive margin. Viewed alongside the wage results in Table 11, these patterns underscore the main finding: the policy is associated with large wage penalties for formally employed women, while participation and fertility responses remain weak even when fertility-policy differences between treatment and comparison groups are tightly controlled.

Conclusion

This study investigates the impact of the Female Labor Protection (FLP) policy on labor participation and wages, particularly for women of and beyond childbearing age. The theoretical model predicts two key mechanisms: productivity loss of employers (ψ) and covered beneifts for employees (z) would increase employment but decrease wage. Additioanlly, heterogeneity among wokrers to claim the beneifts and their bargaining power will spill over the effects to older female workers. Our empirical findings align with these predictions and offer additional insights into how these effects differ across age groups.

We find strong evidence that the FLP puts downward pressure on wages for covered women, and that this effect extends beyond women of childbearing age to older women (ages 35–55). This pattern suggests that older women, who are closer to the end of their working lives and may have weaker bargaining power, can also face substantial wage penalties even though they are less likely to claim FLP-related benefits. This reinforces the need for careful consideration of how labor protection policies interact with age-related dynamics in the workforce.

Our placebo test supports the validity of these findings. By examining trends in participation and wages prior to the implementation of FLP, and only examining the response of male wokrers, we confirm the parallel trends assumption required for causal inference. This strengthens the credibility of our results and suggests that the observed effects are indeed driven by FLP rather than underlying trends in the labor market. Additionaly, the robutsness checks with only urban samples and those samples who are more susceptible, we confirm the repsonse in labor partipciation and fertility is minor, but the wage panelty is profound, even spilling over to older groups.

These findings have important policy implications. While FLP provides necessary protections for women, the wage penalties observed—especially for older women—highlight potential inefficiencies in its design. Policymakers may consider strategies to reduce firms' hiring costs, such as subsidies or tax credits, to mitigate wage penalties while preserving the policy's protective benefits. Moreover, policies that promote shared parental responsibilities could alleviate the perceived risks of hiring women, reducing the disproportionate wage penalties for older cohorts.

In summary, by contextualizing these results within China's unique demographic and institutional setting, we provide critical insights into that labor protection policies may lead to unintended negative effects, and even towards groups beyond targets. It underscores the importance of designing policies that balance the goals of fostering not only gender equity but also integenerational equity. Future research should explore how complementary policies, such as mandatory paternity leave, may mitigate unintended negative consequences and promote more inclusive labor market outcomes.

A Appendix: Model

In this section, we will show the details of calculation of the labor searching and matching model. Keeping all assumptions and notations the same as in the main text, the optimal wage would maximize the Nash product between workers and employers:

$$\max\ [W(w) - U]^{\beta}[J(w)]^{1-\beta} \tag{A.1}$$

and the first order condition would be

$$(1-\beta)(W-U) = \beta J \tag{A.2}$$

Combining Eqs. 2 and 3, we can get a system for employers' value for a filled vacant position J, and employers' value for FLP X.

$$J = \frac{A - w + \sigma X}{r + \sigma + s}, \tag{A.3}$$

$$X = \frac{-\psi + \gamma J}{r + \gamma}. \tag{A.4}$$

Plugging Eq. A.4 into Eq. A.3, we can solve out the employers' value for a filled vacant position J

$$J = \frac{(r + \gamma)(A - w) - \sigma\psi}{r(r + \sigma + s) + \gamma(s + r)}. \tag{A.5}$$

Based on Eq. 5, we can get $W - U$

$$(r + s)(W - U) = w - \sigma(W - L) - rU. \tag{A.6}$$

and subtracting Eq. 6 from Eq. 5, we can get $W - L$

$$W - L = \frac{w - z - s(W - U)}{r + \sigma + \gamma} \tag{A.7}$$

By plugging Eqs. A.7 and 4 into Eq. A.6, we can get

$$W - U = \frac{(r + \gamma)(w - b) + \sigma(z - b)}{(r + p(\theta))(r + \sigma + \gamma) + (r + \gamma)s} \tag{A.8}$$

Then, plug Eqs. A.5 and A.8 into Eq. A.2, we have

$$(w - b) + \frac{\sigma(z - b)}{r + \gamma} = \beta\left[A - b + \left(\frac{(r + \gamma)(A - w) - \sigma\psi}{r + \gamma}\right)\frac{p(\theta)(r + \sigma + \gamma) + \sigma r}{r(r + \sigma + s) + \gamma(s + r)} + \frac{\sigma(z - b - \psi)}{r + \gamma}\right].$$

Using the job creation condition,

$$\frac{(r + \gamma)(A - w) - \sigma\psi}{r(r + \sigma + s) + \gamma(s + r)} = \frac{c}{q(\theta)}$$

we can finally write the optimal wage as

$$w = b - \frac{\sigma(z - b)}{r + \gamma} + \beta\left[A - b + \frac{\sigma(z - b - \psi)}{r + \gamma} + \frac{c}{q(\theta)}\frac{p(\theta)(r + \sigma + \gamma) + \sigma r}{r + \gamma}\right] \tag{A.9}$$

Based on the job creation function and the optimal wage function, we can rewrite the system equation as

$$Y: \quad \frac{(A-w)(r+\gamma)-\sigma\psi}{r(r+s)+(\sigma+\gamma)r+\gamma s}-\frac{c}{q(\theta)}=0, \tag{A.10}$$

$$Z: \quad w-b+\frac{\sigma(z-b)}{r+\gamma}-\beta\left[A-b+\frac{\sigma(z-b-\psi)}{r+\gamma}+\frac{c}{q(\theta)}\frac{p(\theta)(r+\sigma+\gamma)+\sigma r}{r+\gamma}\right]=0. \tag{A.11}$$

For Y it's easy to get

$$Y_\theta := \frac{\partial Y}{\partial \theta}=\frac{q'(\theta)c}{q(\theta)^2}<0, \tag{A.12}$$

$$Y_w := \frac{\partial Y}{\partial w}=-\frac{(r+\gamma)}{r(r+s)+(\sigma+\gamma)r+\gamma s}<0, \tag{A.13}$$

$$Y_\psi := \frac{\partial Y}{\partial \psi}=\frac{-\sigma}{r(r+s)+(\sigma+\gamma)r+\gamma s}<0, \tag{A.14}$$

$$Y_z := \frac{\partial Y}{\partial z}=0. \tag{A.15}$$

For Z we have

$$Z_\theta := \frac{\partial Z}{\partial \theta}=-\beta c\left[\frac{p'(\theta)(r+\sigma+\gamma)}{q(\theta)(r+\gamma)}-\frac{\big(p(\theta)(r+\sigma+\gamma)+\sigma r\big)q'(\theta)(r+\gamma)}{\big(q(\theta)(r+\gamma)\big)^2}\right]<0, \tag{A.16}$$

$$Z_w := \frac{\partial Z}{\partial w}=1, \tag{A.17}$$

$$Z_\psi := \frac{\partial Z}{\partial \psi}=\frac{\sigma\beta}{r+\gamma}>0, \tag{A.18}$$

$$Z_z := \frac{\partial Z}{\partial z}=(1-\beta)\frac{\sigma}{r+\gamma}>0. \tag{A.19}$$

Hence, $\frac{\partial Y}{\partial \theta}\frac{\partial Z}{\partial w}-\frac{\partial Z}{\partial \theta}\frac{\partial Y}{\partial w}$ is negative. With all known information above, we can use the Cramer's rules that

$$\frac{d\theta}{d\psi}=\frac{-\frac{\partial Y}{\partial \psi}\frac{\partial Z}{\partial w}+\frac{\partial Z}{\partial \psi}\frac{\partial Y}{\partial w}}{\frac{\partial Y}{\partial \theta}\frac{\partial Z}{\partial w}-\frac{\partial Z}{\partial \theta}\frac{\partial Y}{\partial w}}$$

Hence, we know

$$\text{sign}\left(\frac{d\theta}{d\psi}\right) = \text{sign}\left(\frac{\partial Y}{\partial \psi}\frac{\partial Z}{\partial w} - \frac{\partial Z}{\partial \psi}\frac{\partial Y}{\partial w}\right) = \text{sign}(\frac{\sigma(1-\beta)}{r(r+s)+(\sigma+\gamma)r+\gamma s}) > 0, \tag{A.20}$$

Similarly, we know[15][16]

$$\text{sign}\left(\frac{d\theta}{dz}\right) = \text{sign}\left(\frac{\partial Y}{\partial z}\frac{\partial Z}{\partial w} - \frac{\partial Z}{\partial z}\frac{\partial Y}{\partial w}\right) > 0, \tag{A.21}$$

$$\text{sign}\left(\frac{dw}{d\psi}\right) = \text{sign}\left(\frac{\partial Y}{\partial \theta}\frac{\partial Z}{\partial \psi} - \frac{\partial Z}{\partial \theta}\frac{\partial Y}{\partial \psi}\right) < 0, \tag{A.22}$$

$$\text{sign}\left(\frac{dw}{dz}\right) = \text{sign}\left(\frac{\partial Y}{\partial \theta}\frac{\partial Z}{\partial z} - \frac{\partial Z}{\partial \theta}\frac{\partial Y}{\partial z}\right) < 0, \tag{A.23}$$

$$\text{sign}\left(\frac{du}{d\psi}\right) = \text{sign}\left(-\frac{d\theta}{d\psi}\right) < 0, \tag{A.24}$$

$$\text{sign}\left(\frac{du}{dz}\right) = \text{sign}\left(-\frac{d\theta}{dz}\right) < 0, \tag{A.25}$$

With information provided in Eqs. A.12 - A.19, we can get the signs in Eqs. A.20 - A.25 as follows, and we can interpret them as:

- $\frac{d\theta}{d\psi} > 0$, the increase of productivity loss makes the labor market more tight.
- $\frac{d\theta}{dz} > 0$, the benefits of FLP makes the labor market more tight.
- $\frac{du}{d\psi} < 0$, the increase of productivity increases the labor force participation.
- $\frac{du}{dz} < 0$, the benefits of FLP makes the labor increases the labor force participation.
- $\frac{dw}{d\psi} < 0$, the increase of productivity loss decreases wages.
- $\frac{dw}{dz} < 0$, the benefits of FLP makes the labor decreases wages.Further, we can discuss the heterogeneity of bargaining power β and probability of claiming FLP benefits β among workers. In default, the FLP targets on those females at child-bearing age, and hence we suppose their bargaining power and probability of taking FLP benefits would be higher than their older female colleagues.

Based on known results from the Cramer's rules above, we can calculate the second derivatives with respect to the probability σ or bargaining power β. more precisely

15 $\frac{d\theta}{d\psi} = -\frac{xp'(\theta)}{\sigma+p(\theta)^2}\frac{d\theta}{dz}$

16 $\frac{du}{d\psi} = -\frac{xp'(\theta)}{(\sigma+p(\theta))^2}\frac{d\theta}{dz}$

$$\frac{\partial^2 w}{\partial\sigma\,\partial z} = \frac{1-\beta}{r+\gamma}\,\frac{\frac{\partial Y}{\partial\theta}\left(D-\sigma D_\sigma\right)}{D^2}$$

and since $\frac{1-\beta}{r+\gamma} > 0, \frac{\partial Y}{\partial\theta} < 0, D^2 > 0.$ $\operatorname{sign}\left(\frac{\partial^2 w}{\partial\sigma\,\partial z}\right) = \operatorname{sign}\left(D-\sigma D_\sigma\right),$ where $D = \frac{\partial Y}{\partial\theta}\frac{\partial Z}{\partial w} - \frac{\partial Z}{\partial\theta}\frac{\partial Y}{\partial w}, D_\sigma = \frac{\partial D}{\partial\sigma}.$ $\frac{\partial^2 w}{\partial z\partial\sigma} > 0$ if $D_\sigma < \frac{D}{\sigma}$. the increasing probability σ changes the tightness-wage system so that the negative wage impact of z is weakened as σ rises. The take-up probability and benefits generosity then act like substitutes in their wage effect.

Similarly, we can get

- $\frac{\partial^2 w}{\partial z\partial\sigma} > 0 \iff D_\sigma < \frac{D}{\sigma},$
- $\frac{\partial^2 w}{\partial z\partial\beta} = -Y_\theta\,\frac{Z_{z\beta}D - Z_z D_\beta}{D^2} > 0,$
- $\frac{\partial^2 w}{\partial\psi\partial\sigma} > 0 \iff D_\sigma < \frac{D}{\sigma},$
- $\frac{\partial^2 w}{\partial\psi\partial\beta} > 0 \iff D_\beta < \frac{D}{\beta},$
- $\frac{\partial^2\theta}{\partial z\partial\sigma} < 0 \iff D\left(Y_w + \sigma Y_{w\sigma}\right) < \sigma Y_w D_\sigma,$
- $\frac{\partial^2\theta}{\partial z\partial\beta} = \frac{Z_{z\beta}Y_w D - Z_z Y_w D_\beta}{D^2} < 0,$
- $\frac{\partial^2\theta}{\partial\psi\partial\sigma} < 0 \iff D\left(Y_w + \sigma Y_{w\sigma}\right) < \sigma Y_w D_\sigma,$
- $\frac{\partial^2\theta}{\partial\psi\partial\beta} < 0 \iff D_\beta < \frac{D}{\beta}.$

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Declarations

Conflicts of Interest The authors declare no conflict of interest.

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Informed Consent N/A.

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