The Unintended Impacts of the One-child Policy Relaxation in China on Women's Labor Market Outcomes

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Abstract

This study investigates the impacts of China's relaxation of the one-child policy on women's labor market outcomes by analyzing the relaxation timing across different couples in a staggered difference-in-differences design and using the China Family Panel Survey (CFPS) from 2010 to 2020. The results reveal that affected women had higher birth rates but experienced lower labor participation by 2.6 percentage points. They worked 2.164 fewer hours per week, earned less salary, and were 0.9 percentage points (90%) more likely to be forced to leave their previous jobs. The analysis further indicates that these impacts were most pronounced among women at prime age and mothers of an only child, particularly an only daughter. This suggests that employers' mistreatment of newly eligible women and their overestimation of female employees' fertility willingness were the main contributors to the negative impacts on women's labor market outcomes, but not just their actual childbearing. Dynamic analysis using multiple methods shows that affected women quickly returned to work after two years, with long-lasting impacts on work time and salary earnings. This study contributes to the literature by providing suggestive evidence that fertility-encouraging policies could harm female employees through misconceptions. It also provides one of the first comprehensive empirical analyses of the universal two-child policy in China, particularly on women's labor market outcomes.
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1 Introduction

Over the past several decades, there has been a significant convergence in gender inequalities within labor market outcomes. This notable shift, often termed the "quiet revolution", is primarily attributed to the rise in women's labor force participation and their relative earnings (Goldin (2006)). Despite these advancements, the convergence has stalled since the 1990s, with gender disparities persisting in both developed and developing nations, especially among higher-skilled women workers (Blau and Kahn (2017)). While anti-discrimination measures, job protection policies, and inclusive educational initiatives have made strides, the enduring gender inequalities are primarily accounted for by the disparate roles in parenthood between men and women (Kleven and Landais (2017); Kleven et al. (2019); Cortés and Pan (2023)).

Empirical studies examining the "child penalty" have predominantly utilized exogenous shocks to childbearing and the number of offspring[1]. In this paper, I exploit a unique policy change, China's one-child policy relaxation, as a shock to the household fertility decision to study how families react to the policy change and what further direct and indirect impacts this policy shock has on women's labor market outcomes. To identify the causal effects of the policy changes, I utilize the timing variation in eligibility for a second child among couples as the identification strategy. This variation exists due to the staggered adoption of the policy shift and different regulations under the one-child policy for different couples in different provinces.

Population policies aimed at either promoting or curbing fertility rates are prevalent worldwide. As documented by United Nations Department of Economic and Social Affairs: Population Division (2021), as of 2019, nearly three-quarters of global governments (143 out of 197) had implemented fertility policies, with 55 promoting higher fertility rates. The Chinese scenario offers a unique opportunity to examine familial responses to shifts in government fertility policies and the subsequent impacts on the labor market, particularly on women. While China was not alone in enforcing mandatory population controls[2], it is distinguished by the prolonged enforcement of its stringent one-child policy and the vast population affected. The repeal of the one-child policy in China provides

[1]For example, Rosenzweig and Wolpin (1980) use the birth of twins as a shock to child amount, Angrist and Evans (1998) consider the sex composition of the first two offspring as an exogenous influence on the likelihood of a third child, Lundborg et al. (2017) employ in vitro fertilization (IVF) treatment outcomes to study variations in first childbirths, and Markussen and Strøm (2022) use miscarriage events to analyze the impacts of the first, second, and third childbirths.

[2]For instance, India from 1976 to 1977 and Peru from 1996 to 2000, targeting the indigenous population, are notable examples.

a natural experiment, allowing for an exploration of not only the immediate impacts on childbirth but also the direct and indirect impacts on labor market outcomes. The direct impacts are the mechanical impacts of the one-child policy relaxation on childbirth and the decline of women's labor supply due to maternity leave, as well as the further trajectory of earning gaps caused by unequal responsibilities in child caring, illustrated by Kleven and Landais (2017); Kleven et al. (2022). There are also indirect impacts of the one-child policy change on women's labor market outcomes not caused by the rise in fertility but due to mistreatment by employers and recruiters. By exploring the variation of eligibility for having a second child, I can show both the direct and indirect impacts of the lift of the one-child policy and provide suggestive evidence that the indirect impacts contribute more to the overall impacts on women's labor market outcomes.

The widely discussed and controversial one-child policy in China was first introduced in late 1979, and after 1984, several exceptions were included. In 2013, a relaxation was made, allowing couples to have a second child if one of the partners was an only child. By 2015, this policy evolved into a universal two-child policy, granting all couples the right to have two children. In response to concerns about persistently low fertility rates and an aging population, a further shift to a three-child policy happened in 2021. This paper focuses on the policy transitions of 2013 and 2015. The phased nature of these policy changes provides an opportunity to identify the policy impacts causally on fertility and women's labor market outcomes using a staggered Difference in Differences (DID) design.

I employ the China Family Panel Survey (CFPS), a nationally representative panel survey dataset, for this study. This survey interviewed members from over 14,000 families in 2010 and has since conducted biennial follow-up surveys. My analysis encompasses six survey waves from 2010 to 2020, with two waves preceding the 2013 policy change, one in the interim between the two policy shifts, and three after the 2015 universal two-child policy announcement.

Regression results show that after the policy relaxation, there was a 5.4 percentage point increase in fertility rates among eligible childbearing-age married women between survey waves. Concurrently, there was a 3.2 percentage points (4.51%) decline in working status around the survey time, with average weekly work hours decreasing by 2.654 hours (8.22%). Further analysis implies that the drop in working status is a consequence of both fewer new hires and more forced job displacement. Moreover, heterogeneous analysis indicates that women across three age cohorts—16 to 25, 26 to 35, and 36 to 45 in 2010—all exhibited increased childbirth rates in response to the policy relaxation, but the decline in working status, weekly work time, and finding new jobs, as well as the increase in involuntary displacement only appeared among the older age groups (both 26 to 35 and 36 to 45 in 2010). Further heterogeneity analysis by the number of children and gender of the first child in 2010 reveals that even though women with only one child before the policy change showed a lower increase in new births, they were the only group who received penalties in labor outcomes. Due to son preferences, these penalties were even more pronounced among mothers of an only daughter than mothers of an only son.

These discrepancies between women employees' reaction to the fertility restriction lift and the penalties they received after the policy change was announced show suggestive evidence that employers overestimate the overall increase in fertility among eligible women employees and also disproportionally punish women with a higher anticipated probability of having one more child, particularly mothers of only daughters. Hence, the indirect impacts of the one-child policy relaxation through employers' reactions are the main contributors to the negative impacts on newly eligible women's labor market outcomes. Dynamic estimates show that the increase in new births did not persist, and affected women returned to work two years after the policy change, but with lasting work time and earnings penalties. The larger impacts on working status, work time, and promotions among mothers of an only child, particularly a daughter, are concentrated on better-educated, urban, and non-agricultural women, even though these attributes are not associated with a higher rise in fertility. Dynamic analysis shows that the rise in fertility did not persist, and women who lost their jobs also returned to work soon, but there are some pieces of evidence that the impacts on work time and wage rates are persistent.

Recent methodological literature highlights potential biases in two-way fixed effects (TWFE) estimates within a DID design, mainly when treatment effect heterogeneity exists across different groups treated at varying times, as reviewed by Roth et al. (2022) and de Chaisemartin and D'haultfoeuille (2022). This issue, termed the "forbidden comparison" by Goodman-Bacon (2021), is particularly prevalent in staggered adoption designs. To check the robustness of my findings, I first assess potential negative weights using the test method proposed by de Chaisemartin and D'Haultfœuille (2020). Subsequently, I employ the heterogeneity robust estimation methods suggested by Callaway and Sant'Anna (2021) and by Wooldridge (2021) with reduced observation sizes and only the first policy change in 2013. The results, albeit less precise, align directionally with my primary findings. Additional robustness checks reveal that the policy changes had negligible effects on women from ethnic minorities, women aged above 45 in 2010, and fertile-aged men on labor market outcomes, proving that no other parallel policy shocks were causing these impacts on women's labor market outcomes.

This paper contributes to three strands of literature. The first strand concerns the fertility penalty and gender inequality in the labor market. Numerous studies, such as those by Altonji and Blank (1999), Bertrand (2011), Olivetti and Petrongolo (2016), Blau and Kahn (2017), have explored this topic. To causally identify the effects of childbirth on women's labor supply and earnings, research has generally taken two directions. One approach uses exogenous variations in childbirth or child quantity, making comparisons within specific cohorts, as seen in works by Rosenzweig and Wolpin (1980), Angrist and Evans (1998), Lundborg et al. (2017), and Markussen and Strøm (2022). Another adopts event study methodologies to trace the trajectories of within-couple earning disparities post-childbirth, as demonstrated by Angelov et al. (2016), Andresen and Nix (2022) and Kleven et al. (2019). In this context, the present paper exploits the unique backdrop of China's one-child policy relaxation, offering insights into both the direct effects of childbirth and broader societal consequences, especially for women with heightened fertility expectations.

The second strand delves into the impacts of government family policies. While many governments, especially in high-income countries, have implemented policies to bolster women's employment and encourage fertility, empirical assessments of these policies' actual effects remain limited[3]. For instance, studies by Kleven et al. (2021) and Ginja et al. (2023) have found mixed results regarding gender gap convergence following policy reforms in Austria and Sweden, respectively. This paper delves deeper into China's sweeping fertility policy changes, elucidating varied family responses and broader societal impacts.

The third strand focuses on the empirical analysis of China's evolving fertility policy. As reviewed by Zhang (2017), China's one-child policy has long been an attractive research topic. Empirical research on the one-child policy has delved into areas such as fertility (Li et al. (2005) and Cai (2010)), child educational attainment (Li et al. (2008), Rosenzweig and Zhang (2009), Li and Zhang (2017), and Huang et al. (2021)), family distortion (Huang et al. (2016), Huang and Zhou (2015)), parental labor supply (Wang et al. (2017)), and the sex ratio (Ebenstein (2010), Li et al. (2011), and García (2022)). The transition in 2015 to a universal two-child policy marked a significant policy shift with potentially wide-reaching implications. Some researchers, like Qin and Wang (2017), have used two-child policy experimentation in one county from 1985 to gain insights into the potential effects of a universal two-child policy. The closest research to this paper is a

[3] United Nations Department of Economic and Social Affairs: Population Division (2021) notes that between 2015 and 2019, 28% of global governments advocated for increased fertility, with significant emphasis in Europe, North America, and parts of Asia.

fictitious resume experiment conducted by He et al. (2023), which offers insights into potential labor market biases following the policy change. This paper's strength lies in its use of CFPS, a comprehensive family survey dataset, enabling a comprehensive examination of family responses and labor market outcomes in the wake of the policy shift. This work represents the most exhaustive evaluation of China's universal two-child policy concerning fertility decisions and women's labor market outcomes.

The subsequent sections of this paper are organized as follows: the second section provides an overview of the policy background; the third section outlines data sources and sample selection; the fourth section introduces empirical strategy; the fifth section results and robustness checks; and the final section concludes the paper.

2 Background

China's population policies underwent significant changes in the two decades following 1949. Initially, Mao Zedong, the country's supreme leader, advocated for population growth to support social reform and economic development, leading to a surge in population in the early 1950s. However, after the 1953 census came out and especially the population rebounded after the Great Famine (1959-1961), informal family planning efforts emerged but were disrupted by the Great Leap Forward in 1958 and the Cultural Revolution in 1966. A significant shift occurred in 1971, following a population surge to over 800 million in 1969. The government's new policy, encapsulated in the slogans 'One child is not too few, two are just fine, and three are too many' and later 'Later, Longer, and Fewer,' advocated for delayed marriage (minimum ages of 23 for women and 25 for men) and spacing of up to two children by at least three years. This strategy effectively halved China's fertility rate from 1971 to 1978 (Zhang (2017)).

In 1979, a year after Deng Xiaoping's rise to leadership, China implemented a more stringent population control measure: the compulsory one-child policy, which was applied across most provinces, including both rural and urban areas, with few exceptions in rural regions of Qinghai, Ningxia, Xinjiang, and Yunnan. However, starting in 1984, the policy was moderately relaxed for particular couples in response to significant resistance, particularly from rural families with an only daughter. This relaxation remained stable until 2013 (Wang et al. (2017)).

Before 2013, the policy was rigorously enforced for almost all urban couples and most rural couples. The distinction between urban and rural was typically based on the household registration system (hukou), comprising agricultural (rural) and non-agricultural (urban) statuses. Exemptions permitting a second child without penalties included the following scenarios:

- Couples where both partners are only children.
- Rural couples from less populated and underdeveloped provinces or autonomous regions, specifically Hainan, Yunnan, Qinghai, Ningxia, and Xinjiang.
- Rural couples with a first-born daughter in all provinces except Shanghai, known as the "one-and-a-half-child policy"[4].
- In Tianjin, Liaoning, Jilin, Shanghai, Jiangsu, Fujian, and Anhui, rural couples where the husband or wife is an only child.
- Couples where both partners are from minority ethnic groups.

In addition to these exemptions, more lenient policies in certain less populated and underdeveloped areas allowed three children or had no limit, especially for minority nomadic couples. In Xizang, urban couples were generally allowed two children, while rural residents faced no family planning restrictions.

Besides policy variations, the enforcement of the one-child policy significantly differed across provinces, cities, and between rural and urban areas, as well as among Han and minority populations. In urban areas, particularly for employees in state-owned enterprises, violations of the one-child policy could lead to severe consequences, including job and welfare loss (Zhang (2017)). In contrast, rural families typically faced monetary fines for unauthorized additional children[5]. Minority couples generally were exempt from the one-child policy, even in urban settings[6].In this analysis, I will initially apply a uniform rule for minority couples, assuming that all couples where both partners belong to minority groups are permitted to have a second child. Subsequently, I will delve deeper into the specific impacts of birth control policy changes on these minority couples.

[4]This policy also applied in Beijing, Tianjin, Chongqing, Chengdu, and Jiangsu, but only to rural couples in mountain areas. Wang et al. (2017) categorizes these regions under the same policy, with Shanghai as the only exception.

[5]The fine could be a significant amount in many provinces, but varied a lot (Ebenstein (2010); Huang et al. (2021))

[6]As noted by Wang et al. (2017), rural minority couples in all provinces, except for Jiangsu, were allowed a second child. Urban minority couples outside Fujian, Guangdong, Hunan, Henan, Hubei, Jiangsu, Liaoning, and Yunnan were also permitted a second child. A special case is Han-minority couples, of which the regulations are different in different provinces (Huang and Zhou (2015))

There were also specific exemptions varying by province, such as for remarried couples or individuals in particular occupations or with unique family circumstances. However, these were limited in scope and are not the focus of this paper.

Since the early 1990s, China's overall fertility rate steadily declined, falling below the replacement level to 1.22 in the 2000 census and further to 1.18 in the 2010 census [7]. Concerned about the low fertility rate and an aging population, the Chinese government cautiously amended the one-child policy in 2013. This amendment allowed couples, where either the husband or wife was an only child, to have a second child. Following the announcement, all provinces updated their family planning regulations by mid-2014. This policy change primarily impacted urban couples and some rural couples. However, the fertility rate did not increase as much as anticipated. New births increased marginally from 16.4 million in 2013 to 16.87 million in 2014 and then decreased to 16.55 million in 2015, prompting a broader policy relaxation.

In 2015 the government introduced a universal two-child policy, allowing all couples to have two children without restrictions. Incentives for single-child families were removed, pre-birth approval procedures were canceled, and regions were encouraged to provide subsidies for second children. Post-implementation, the total number of births increased to 17.86 million in 2016, the highest since 2010. However, birth numbers declined in subsequent years, reaching 10.62 million by 2021. This trend suggests that while the one-child policy's relaxation released some unmet fertility demand, it was insufficient to reverse the declining birth rates. In 2021, a three-child policy was announced, and even though it is still too early to assess its impact, the number of newborns in 2022 was not promising.

The evolving fertility policies in China, including the diverse regulations across rural and urban areas and among Han and minority ethnic groups in various provinces, provide an opportunity to causally identify the effects of the two-step relaxation of the one-child policy. This analysis mainly focuses on the childbirth decisions of newly eligible couples under these policy changes and the further impacts on labor market outcomes, particularly on married women.

[7] As for the reasons for these declines, Zhang (2017) summarizes that a reconciliation of empirical research is that the birth control policy was the main contributor of the decline in the 1970s and early 1980s, but socioeconomic development played a crucial role in fertility decline in the long term.

3 Data Source

This paper's analysis mainly uses data from the China Family Panel Survey (CFPS), conducted by Peking University's Institute of Social Science Survey (ISSS) across 25 provincial regions in China [8]. The CFPS, initiated in 2010, successfully interviewed 14,960 families, including all family members, termed 'core members'. Follow-up surveys were conducted biannually, tracking these individuals, new family members, and their offspring. The dataset used in this study encompasses data from 2010 to 2020, covering the first six waves of the survey.

The CFPS is particularly suitable for this research for several reasons. Firstly, it encompasses 25 provinces[9], providing a representative sample of China's population while conveniently excluding regions with unique family planning policies. Secondly, the survey waves from 2010, 2012, 2014, 2016, 2018, and 2020 span crucial periods: two waves precede the 2013 policy change, one coincides with the interim period, and three follow the implementation of the universal two-child policy in 2015. Lastly, the CFPS offers comprehensive demographic and socioeconomic data. It allows for identifying couples eligible for a second child and examining how changes in this eligibility correlate with their labor market outcomes.

3.1 Sample Selection

In my empirical analysis, the sample is restricted to women of fertile age (aged 16 to 45 in 2010) who were married in 2010, the baseline year. This selection is because family planning policies primarily target married couples. Until 2015, a marriage certificate was required for a birth permit, without which a child could not obtain household registration (hukou) and access to elementary education and social welfare. Moreover, in my analysis, the treatment status is the eligibility to have a second child under the policy, and a qualified unmarried woman can be eligible to have a second child after getting married under the one-child policy. Then, the impacts of a new marriage on a newly married woman's fertility and labor market outcomes will also be part of the average treatment effects, which is not favorable. However, the relaxation of the one-child policy can affect fertility and labor market outcomes by influencing marriage or divorce decisions in whichever direction. To consider the impacts of the birth control policy change on marriage and divorce outcomes, I limit the samples to already married couples in 2010, before the policy change, to

[8] The website of CFPS is http://www.isss.pku.edu.cn/cfps/en/index.htm

[9] Excluding Xinjiang, Xizang, Qinghai, Inner Mongolia, Ningxia, and Hainan.

include the impacts of the policy change on divorce but not on a new marriage, and I will also show regression results on a new marriage and new divorce. Table A1 shows that the critical criteria of the eligibility of having a second child had no impact on the probability of getting married. The policy change caused affected married couples to be less likely to get divorced, possibly due to the potential of having a second child strengthening marital bonds. By restricting the sample to women married at the baseline year, the analysis captures the policy change's impact on divorce decisions and subsequent effects on fertility and labor market outcomes. It also ignores effects on marriage decisions, for which there is no substantial evidence of impact. In addition to the primary analysis on fertile-aged women, I will also present regression results for fertile-aged men and older women (aged 46 to 60 in 2010) as robustness checks.

3.2 Main Outcomes and Summary Statistics

The main outcomes in my empirical part include new birth, working status, weekly work hours, promotion, finding new jobs, voluntary job displacement, involuntary job displacement, and wage rates. The new birth equals one of an individual in a specific year if the birth year of her child is that specific year or the previous year. For example, the new birth variable equals one in the survey wave 2020 if the respondent has a child born in 2020 or 2019. Thus, the indicator of new births in 2014 and 2016 can also capture the impact of the policy changes in 2015 and 2013. Working status is defined as whether an individual has worked at least one hour in the past week when she was surveyed or could return to a job within six months or in the slack season of agricultural work or business. Weekly working hours measure the average work time in one week, including overtime work in the past 12 months in the primary job position, and it includes working hours as employed workers, self-employed, doing agricultural work, or ruining business. Promotion is an indicator of whether an individual getting an executive promotion, a technical title promotion, or both within the past 12 months, and this question was not asked in the survey year 2012. The variable Finding new jobs is defined to be one if an individual's primary job or part-time jobs were started in the past two years or after the last time when this individual was surveyed. IHS wage rate is the inverse hyperbolic sine transformation of the average wage rates in the past twelve months before the survey was conducted. The wage rate is calculated by dividing total employed income by annual work hours, which is measured by multiplying average weekly work hours by four (weeks a month) and twelve (months a year). Employed earnings are the sum of net income from the primary job and all other part-time jobs, excluding agricultural and business revenues. Involuntarily and voluntarily leaving are based on the answer to why you left your previous job. Involuntarily leaving equals one if the answer is unit bankruptcy, shutdown or dissolution, layoffs or job cuts, being fired or dismissed, contract expiration, and end of seasonal or temporary work. Voluntarily leaving includes Leaving for childbirth or family issues, looking for another job, accepting a new job offer, returning to school or training, retiring or leaving office, or other specific reasons not listed.

Table 1 shows summary statistics of women's primary demographic and work-related information, as well as their spouses' information. On average, 46.7% of the women live in urban communities, while only half of them have urban Hukou (household registration); 10.7% of them are from minority ethnicities, and only 5.5% of them are only children. On average, they have 1.605 children. 75% of these women are working, and 17% get a new job in the past two years. 1% and 9.4% of women left their last jobs due to involuntary and voluntary reasons, respectively. On average, they work 34.233 hours per week, and 3% get a promotion in the past year. The average number of education years they received is 7.587 years, and 54% and 9.2% of these women finished junior high school (9 years of educational years) and college (15 years of educational years). In the baseline year of 2010, 25.2% of the fertile-aged married women did agricultural work as their main job, and 26.2% were doing employed work, most of whom were hired by private firms (24.3%). On average, 45.9%, 10.9%, and 43.3% of these women had one child, no child, or more than one child. With respect to their husbands, 9.4% are only children, and 90.6% are currently working, with an average of 46.614 hours a week and much higher wage rates. 4.7% of these men got a promotion in the past year, and 20.7% changed or found a new job in the past year. On average, they received 8.653 years of educational years, and 63.9% and 10.6% have finished junior high school and college, respectively. In 2010, the same proportion of their husbands were doing agricultural work, while more were employed (37.5%), and mostly by private firms.

4 Empirical Strategy

This section outlines the methodology employed to investigate the causal effects of China's one-child policy relaxation on women's labor market outcomes. A generalized Difference in Differences (DID) approach with a staggered adoption design is used, tailored to the context where different couples became eligible for a second child at varying times and remained treated after that. Additionally, an event study model is incorporated to examine the dynamic impacts of this policy change.

4.1 Static DID

I employ a Two-Way Fixed Effects (TWFE) model to estimate the average treatment effects for specific years in the static framework. The model is formalized as follows:

$$Y_{it} = \alpha_i + \theta_t + D_{it}\beta_{treated} + \epsilon_{it} \tag{1}$$

Here, Y_{it} represents the outcome for individual i in year t. The variable α_i denotes individual fixed effects, capturing systematic differences in outcomes among individuals. θ_t is the year fixed effects, accounting for overall changes affecting all units equally, such as macroeconomic trends or national policy shifts. D_{it} indicates the treatment status of individual i in year t, specifically the eligibility to have a second child without penalties. The coefficient of interest, $\beta_{treated}$, measures the impact of this treatment, while ϵ_{it} is the error term clustered at the individual level.

In this study, married women are categorized into three groups based on their treatment status: the always-treated, the early-treated, and the late-treated. The always-treated group includes those eligible to have a second child before the 2013 policy change. The early-treated group became eligible following the 2013 relaxation, and the late-treated group became eligible after the 2015 policy change. It is important to note that the TWFE model uses the always-treated group as a control to estimate the average treatment effects on the early and late-treated groups and also uses both the always-treated group and the early-treated group as controls to estimate the group-time-specific treatment effects on the late-treated group after 2014. This could cause the so-called "Forbidden Comparison" (Goodman-Bacon (2021)) problem if there are significant treatment effects heterogeneity among different groups at different times. Further details and discussions on this estimation method will be presented in subsequent subsections.

4.2 Dynamic DID

The dynamic aspect involves a generalized TWFE model to estimate treatment effects concerning specific years before or after treatment:

$$Y_{it} = \alpha_i + \theta_t + \sum_r \mathbb{1}[R_{it} = r]\beta_r + \epsilon_{it} \tag{2}$$

In equation (2), R_{it} represents the relative years to treatment for individual i at year

t. The effects at $r = -2$ are normalized to zero, allowing the interpretation of coefficients β_r as relative treatment effects compared to the survey year preceding the treatment. For this study, early-treated individuals have a relative time sequence of $r = -4, -2, 0, 2, 4, 6$, while late-treated individuals follow $r = -6, -4, -2, 0, 2, 4$.

Primary regression results and heterogeneity analysis rely mainly on the model (1). The event study model will provide insights into how the labor market outcomes evolved in response to the policy shock over time.

4.3 Identification Assumptions

To causally identify the treatment effects using either model (1) or model (2), two main assumptions are necessary, the no anticipation effects assumption and the parallel trend assumption (Sun and Abraham (2021); Borusyak et al. (2022); Roth et al. (2022)).

The no anticipation effects assumption means that before the treatment happens, individuals were not behaving differently in anticipation of this policy change. In the context here, it means that neither the early-treated group couples nor the late-treated couples had an out-of-plan second child facing a penalty or had preparation on the labor market for an upcoming second child, acting as if they could anticipate a policy change. This is a rational assumption because, first, the one-child policy in China was a rather severe policy restriction with a large amount of penalty, especially for urban residents, and the conduct of this policy of local officials was severe too because it was in high priority for local officials' assessment for promotion. The second support comes from the characteristics of policy experimentation in China summarized by Wang and Yang (2022). The central government dominates national policy experimentation decisions, and it is unreasonable to believe couples would strategically anticipate this policy change and start childbearing in advance. In the robustness check part, I use the conventional event study method and the recently proposed treatment effects heterogeneity robust estimation method to test whether anticipatory behavior existed before the policy changed.

Another main assumption is the parallel trend assumption, which requires that the outcomes of individuals in different groups will be on the same trend if there are no treatment status changes. A natural extension of the parallel trend assumption in a simple 2X2 comparison to the parallel trend assumption in a staggered adoption setting requires that the parallel trend of potential outcomes meet for all 2X2 comparisons between any two groups and periods. This is a very strong assumption and, in the context of this paper, can hardly be satisfied. By definition, during all periods, I have an always-treated group, an early-treated group, and a late-treated group, separated by their hukou category and province and the number of siblings of the wife and husband in each couple. Couples and individuals in these three groups can differ largely in family background and socioeconomic characteristics, so their potential outcomes could have different trends. In some recent methodological literature, researchers have considered variants of the parallel trend assumption. Callaway and Sant'Anna (2021) use a weaker version conditional on covariates; Sun and Abraham (2021) consider a parallel trend assumption using only groups that are eventually treated, and not groups that never get treated; Borusyak et al. (2022) propose a stronger version of parallel trends assumption to get a more efficient estimator. In the robustness check part, I use the estimation method proposed by Callaway and Sant'Anna (2021) and the estimation method proposed by Sun and Abraham (2021) to conduct joint tests of all pre-trends. It shows that using that method, which excludes the always-treated group individuals and only uses not-yet-treated group women or last-to-be-treated women as controls, can satisfy the parallel trend assumption in a joint test.

4.4 Issues On Staggered TWFE Model

As shown in several recent econometric papers, the TWFE model, either static or dynamic, will be biased by treatment heterogeneity among groups and periods (Callaway and Sant'Anna (2021); Goodman-Bacon (2021); de Chaisemartin and D'Haultfœuille (2020); Sun and Abraham (2021); Borusyak et al. (2022); also see reviews by Roth et al. (2022), and de Chaisemartin and D'haultfoeuille (2022)). This contamination happens because, in a staggered adoption design model, in both static and dynamic settings, the TWFE estimator is a weighted average of all 2X2 comparisons between any pair of groups and periods. However, these weights, without any economic interpretation, can be negative because of "Forbidden Comparisons" (Goodman-Bacon (2021)). Some comparisons are called "forbidden" because the control group in these comparisons, or the group of individuals with invariant treatment status between two time periods, can be early treated groups.

I include several practices as robustness checks to show that these treatment effects heterogeneity does not significantly bias my regression results. First, I will check how serious is the "Forbidden comparison" contamination by testing for possible negative weights using the method proposed by de Chaisemartin and D'Haultfœuille (2020). Then, I will employ the new estimation methods proposed by Callaway and Sant'Anna (2021) (CS2021) and by Wooldridge (2021) (JW2021) at alternative specifications to estimate both the average treatment effects and to reach event study graphs to show that similar but less significant results hold, due to restricted samples.

5 Regression Results

In this section, I show regression results on the impacts of the one-child policy relaxation on women's labor market outcomes. To better interpret the results, I will first present the impact of the policy change on new births. Then, using the impacts on fertility as references, I will show the impacts of the policy change on women's labor market outcomes, including working status, reasons for leaving previous jobs, weekly working hours, promotion, and wage rates. I will also show the heterogeneous effects of the policy change on women of different ages, of different employers, with different amounts of existing children, and the sex of their first child. To illustrate why different groups of women were influenced by the policy shock differently, I also use triple difference estimation to show that the different impacts are connected with education attainment, hukou category, occupation, and family average income. Event study results through different methods will also be presented to show the evolving impacts of the policy change, followed by multiple robustness checks.

5.1 Impacts of Two-Child Policy on New Birth

I start the regression part by exploring how effective was this birth control policy change on the actual new births. Like all the following regressions, the regression I show in this subsection is restricted to married women aged 16 to 45 in 2010.

In Table 2, the first column shows that after the policy shift, an average of 5.4 percentage points (84.3% relative to the control mean), more eligible women have a new birth in a two-year interval. Columns 2 to 4 show results on new births among three subgroups of different ages, including aged 16 to 25 in 2010, 26 to 35 in 2010, and 36 to 45 in 2010. Eligible married women have more new births at three age groups reacting to the restriction shift, with 20.4 p.p., 4.8 p.p., and 0.5 p.p. increases, respectively, and an 88.7%, 50%, and 50% increase, respectively. This implies that even though this policy change did not alter the long-term trends of declining fertility rates in China, in the short term, affected married women in different age groups within fertile age all responded to the policy shift by having more new births while the younger group's response on fertility was highest.

5.2 Impacts of Two-child Policy on Women's Labor Market Outcomes

Childbearing and childcare responsibilities have a significant impact on women's labor participation and gender wage gaps. The results in Table 3 show that when the one-child policy was lifted, married women who became eligible for a second child decreased their working status by 3.1 percentage points, which was a 4.36% decline compared to the control group's mean. On average, their weekly working hours dropped by 2.625 hours, and promotion rates experienced a drop of 0.6 percentage points (16.28%). Additionally, there was a 3.4 percentage point drop in the probability of finding new jobs and an average of an 8.8% decline in wage rates.

The scale of the labor supply decline is smaller than the increase in new births, but it is based on the respondent's answers about their labor supply within one month before the survey time. Hence, the average estimated decline in labor supply among affected women was comparable to the increase in fertility. The drop in working status can be attributed either to the supply or demand side shock, and the analysis of the reasons for leaving the previous job can provide some suggestive evidence on why the policy change leads to lower working status.

Columns (6) and (7) in Table 3 present the results of the regression analysis on the reasons for leaving the previous job. This question was only asked after the survey wave of 2014, so the estimation only covers the average treatment effects of the second policy change that happened in 2016. Column (6) shows that after the relaxation of the one-child policy, 0.9 percentage points more women who were affected by the policy change had to leave their jobs involuntarily. This includes being fired, laid off, or their contracts ending. This was a 90% increase relative to the control group. However, column (7) shows that the impact of leaving the previous job voluntarily due to reasons such as childbirth, family issues, returning to school, receiving training, accepting a new job offer, or finding a new job was negligible. These results indicate that the decline in the working status of newly eligible women was due to both more forced job displacement and higher obstacles to finding new jobs. These two consequences of the policy change were indirect impacts of the one-child policy lift, resulting from mistreatment at the workplace and biased hiring.

5.3 Mechanisms

5.3.1 Heterogeneous Impacts of by Age Groups

I present the various effects of the policy change on the labor market outcomes of women of different ages while also analyzing the policy's impact on fertility rates. The entire sample of women is divided into three age groups, and the results of the impact of the two-child policy for each group are presented in Table Table 4. Even though all three age groups of newly eligible women raised their fertility rates, the results indicate that only women between the ages of 26 to 35 experienced a significant decline in their working status, while all women experienced a decrease in work hours. Women between the ages of 35 and 45 received fewer promotions due to the policy change, and both women aged 26 to 35 and 36 to 45 received fewer job offers as a result. The second panel of the table depicts that women of all ages between 16 to 45 in the year 2010 had lower wage rates, and the youngest group was affected the most. Furthermore, the last two regressions show that women above the age of 36 in 2010 had higher rates of leaving their previous jobs involuntarily, and women between the ages of 26 and 35 in 2010 also experienced slightly higher rates. However, the youngest group was not affected. There were no impacts on the rates of leaving previous jobs for either family reasons or another job for either group.

The age group with the largest rise in childbirth was not the same group facing the most substantial work-related impacts. This discrepancy suggests that the observed reduction in working status among treated women was more closely tied to employers' perceptions of increased fertility and the associated opportunity costs of maternity leave rather than the actual rise in childbirth. This is especially relevant for women aged 26 to 35 and above 36 in 2010, who usually have more senior roles with higher earnings. Therefore, the indirect effects of this relaxation of the one-child policy could be more critical to the negative impacts on work-related outcomes.

5.3.2 Heterogeneous Impacts by Child Amount and Gender

To further explore what caused the decline in working status and the role of employers' mistreatment, I present a heterogeneous analysis of the impacts of the one-child policy relaxation on childbirth and labor market outcomes by child amount and gender of the first child among women with only one child in 2010. During the time of the one-child policy, most married couples had to comply with the strict fertility regulations, limiting their choices to either one or two children. However, following the relaxation of the policy in 2013 and 2015, couples with only one child were no longer restricted in terms of fertility. As a result, it is reasonable to expect that women of reproductive age with only one child would experience the greatest increase in fertility and be more likely to face mistreatment during the job search or at the workplace.

Table 5 displays the varied effects of the two-child policy on both fertility rates and labor market outcomes of married women who are impacted by the policy based on the number of children they had in 2010. The first column highlights that eligible women who already had two or more children in 2010 are still more likely to have another child after the policy change. Although the two-child policy did not end the family planning regime, the 2015 policy change significantly relaxed population control measures, removed subsidies for single-child parents, and eliminated pre-birth approval requirements. Additionally, some provinces implemented measures to encourage fertility, which resulted in a rise in births among families with two or more children before the policy change. Women who did not have any children before the policy change responded even more to the policy, meaning they advanced the birth of their first child compared to women who did not have any children in the control groups. Interestingly, newly eligible women with one child responded less to the policy shift on fertility rates.

However, the following regression results show that women with only one child are the only subgroup that experienced a significant drop in working status, weekly work time, and promotion. There are slight declines in wage rates and increases in involuntarily leaving. Women with no child in 2010 are reacting to the birth control lift the most while still experiencing no drop in working status, work time, wage rates, or promotion, and no rise in job displacement either. Both women with one child and no child in 2010 got significantly fewer new job opportunities due to the policy change. Women with two or more children already in 2010 did not have a lower labor supply or probability of finding new jobs but had lower wage rates and higher job displacement rates due to both voluntary and involuntary reasons. The discrepancy between the increases in fertility and the impacts on work-related outcomes also suggests that the declines in work-related outcomes are caused by indirect impacts of the policy change, which came from the demand-side factors, not direct impacts due to childbirth. The indirect impacts are associated with employers' perceptions of their women employees' fertility increase, and this estimation turns out to be biased. Moreover, the different impacts on women with only one child or more children suggest that women with more children accepted lower-paid positions while women with only one child, who were better educated, chose to stay unemployed for a longer period until finding jobs with closer wages.

Another evidence of the role of indirect impacts of the one-child policy change comes from the heterogeneous analysis among women with just one child by gender of their children. Due to son preferences in China, it is reasonable to anticipate a higher response in childbearing following the policy change. Table 6 shows regression results of heterogeneous impacts analysis by gender of the first child. The first column reveals that following the policy change, mothers with a single daughter and mothers with a single son were both more likely to have a second child. However, the policy reform notably had a more substantial effect on encouraging mothers with only daughters to have additional children, highlighting the continuing influence of son preference in household fertility decisions. The subsequent results show a much more pronounced decline in working status and weekly work hours for mothers with only daughters than those with only sons. Additionally, mothers with a single daughter were significantly less likely to receive promotions and new jobs post-policy change, a trend not observed among mothers with a single son, while only affected mothers of a single son had lower wage rates. The magnitude of these differences in working status and work time cannot be solely attributed to the 2 p.p. (21.7%) difference in fertility response between the two groups. The different impacts on working status, work time, promotions, wage rates, and finding new jobs imply that compared with mothers of single sons, women with a single daughter before the policy change were more likely to lose their jobs and find it harder to find a new job. The job loss among mothers of a single son was lighter, but they experienced a larger wage decline. This suggests that employers disproportionately anticipated a higher willingness among mothers with daughters to have more children following the policy changes, leading to biased treatment of female employees at the workplace and fewer new hires.

5.3.3 Hetergeneous Impacts by Employer Type

Furthermore, I introduce the heterogeneous impacts of the one-child policy change on women's labor market outcomes by employer type among married women who were employed in 2010 and at the ages 16 to 45. If there are no indirect impacts of the policy change on women's labor market outcomes, I shall not find different impacts among women employed by private firms or the public sector, which includes the government, public institutes, and state-owned enterprises, as long as the female employees's rise in fertility are the same. If anything different, there could be more decline in labor supply and work time among public employees due to higher job security, flexibility, and a more women-friendly environment. Table 7 shows heterogeneous impacts by employer types. Results show that both women employed by public sectors and private firms had the same rise in fertility. However, women who worked at public institutes experienced no significant impacts on working status, work time, promotions, wage rates, and job displacement. The only detectable impacts are more new job opportunities. Women employed by private firms before the policy change, on the contrary, had lower working status, fewer weekly work hours, lower wage rates, more involuntary displacement, and slightly fewer new job offers. This provides a stronger piece of evidence that the indirect impacts of the policy change, due to employers' reaction, contribute more to the policy impacts on women's labor market outcomes. Compared with jobs in the public sector, private employment has lower job security and flexibility, and this leads to more job loss, particularly due to involuntary reasons, fewer new job opportunities, and lower wages.

5.3.4 More Heterogeneous Analysis

What caused the disproportionate punishment against women in older age groups, with only one child and especially with only one daughter? In this part, I focus on the differential impacts on women with different amounts of children. It is noticeable that mothers with a single child in 2010 were better educated, more concentrated in urban areas, and more likely to do non-agricultural work. These attributes are connected with a higher opportunity cost of having a new birth, both to their families and employers. By showing the negative impacts on their labor market outcomes are more associated with these pretreatment covariates, not childbearing, I can show that the indirect impacts of the one-child policy relaxation contribute more to the negative shocks newly eligible women experienced. I use the following triple-difference specification to show that the negative impacts on work-related outcomes are correlated with the attributes mentioned above.

$$
\begin{aligned}
Y_{it} = \alpha_i + \theta_t + \beta_1 D_{it} + \beta_2 D_{it} \times ChildAmount_{it} + \beta_3 D_{it} X_{it} \\
+ \beta_4 D_{it} \times ChildAmount_{it} \times X_{it} + \epsilon_{it}
\end{aligned}
\tag{3}
$$

In this model, X_{it} are individual's attributes, including education attainment in 2010, whether living in an urban community or not in 2010, whether having an urban hukou or not in 2010, whether doing a non-agricultural job in 2010, and whether from families with average income above the median in 2010. To avoid the endogenous choice of the above characteristics, I only use that information in 2010, before any relaxation of the one-child policy. Estimates of β_4 can indicate differential impacts on women with one child or no child by different attributes.

Table A2 shows that among newly eligible single-child mothers, the better-educated women were slightly more likely to have another child while having a slightly higher decline in working status, work time, and wage rates compared with their counterparts who did not receive a junior high school degree. They were also more likely to leave their last job voluntarily and received significantly fewer promotions and fewer new jobs. Tests indicate that better-educated mothers who had one child before the policy change significantly had more new births but were less likely to work, worked fewer hours per week, were promoted less, and got fewer new jobs. The impacts on wage rates and displacement are insignificant. Table 5 implies that women with only one child under the one-child policy chose to work less and did not accept lower wage rates, facing mistreatment at the workplace and during the hiring process. On the contrary, women with more than one child before the policy relaxed chose to receive lower wage rates for less job loss. Furthermore, Table A2 indicates that these different reactions among mothers of a single child were concentrated among better-educated women.

China's urban and rural divide was a key factor under the one-child policy regulation since the policy varied essentially by households' Hukou status. Moreover, The economic activities and family structure are also different in urban and rural areas, as well as average fertility willingness. In particular, families living in cities with rural Hukou differ from their urban cohort families with urban Hukou and their rural cohorts with rural Hukou. Table A3 shows the results of heterogeneous impacts by the community category an individual lives in or her hukou category in 2010. The table indicates significant disparities among women in rural or urban areas and with or without urban Hukou. The first two columns show that compared with their rural cohorts, the rise of the fertility of women in urban communities or with an urban Hukou with no child before the policy change was significantly less. The postponing of childbearing among younger married women due to higher future fertility expectations is consistent with previous findings in the US (Amuedo-Dorantes and Kimmel (2005); Caucutt et al. (2002)), in Europe (Sobotka (2004)), and in both developing countries (Bongaarts (1999)) and in developed countries (Beaujouan (2023)) as a common phenomenon. Reasons for fertility delay include economic growth (Dioikitopoulos and Varvarigos (2023)), wage inequality (Caucutt et al. (2002)) and signaling in the labor market (Ng and Wang (2020)). In samples of this paper, married women with no child in 2010, especially with urban Hukou, are much better educated (53.1% of these women had college degrees, and only 10.4% of married women with rural Hukou and no child had college degrees in 2010) and also more career-oriented. However, the following results show that even with a lower childbearing rise, urban women with no child in 2010 still got punished in working status, work time, and promotions. These impacts were associated with fewer new hires since no impacts were detected on job displacement.

As for urban married women with one child in 2010, their responses to the birth control policy relaxation were significantly higher than their rural counterparts. This is because of the stricter regulation in cities and on women with urban Hukou. These higher rises in birth rate translate into a higher decline in working status, work hours, and promotions, but not in wage rates and job displacement. Thus, similar to women with better educational backgrounds, women in cities and with urban Hukou also chose to work less instead of receiving lower wage rates.

Table A4 shows similar and highly correlated results on heterogeneous impacts among single-child mothers with different occupations. Affected single-child mothers who were doing non-agricultural work had a slightly lower rise in fertility than those who had agricultural work as their main job but experienced significantly larger declines in working status and promotion rates. Positive and significant impacts on involuntary job loss and no impacts on wage rates also imply these mothers of a single child chose to work less due to both involuntary displacement and fewer hires and not to receive lower wages.

These further heterogeneous results show that the direct impacts of the policy change through childbearing are comparable or even smaller among women with better socioeconomic status, while the indirect impacts contribute to the different punishments for women more.

5.4 Dynamic Effects

Previous results show the discrepancy between the relative increase in fertility and the impact on work-related outcomes. It suggests that employers overestimated their female employees' fertility responses to the restriction lift, and better-educated urban women chose to work less instead of receiving lower wages. In this part, I will examine the dynamic impacts of the policy change to test how persistent the overestimation is and whether the affected women return to work.

First, I use model (2) to estimate the treatment effects concerning relative years before and after the treatment year and then draw an event study graph to show the dynamics of these impacts. Figures in Figure 1 and Figure 2 are the even study results of the policy impacts on new birth, working status, weekly work hours, and wage rates. First, I can use these figures to show that all of these four specifications pass the placebo test, and there are no clear different pre-trends among the treated and control groups in the DID setup.

The top figure in Figure 1 indicates that there was a quick boost in fertility the same year when the one-child policy relaxation was announced, while the increase in fertility could not last after two years. The second figure in Figure 1 shows a large and significant decline in labor participation of newly eligible women in the same year when the policy changed and bounced quickly after two years. This shows that even though some affected women lost their jobs after the policy change, they quickly returned to work two years later. Similarly, both figures in Figure 2 show that after a drop in work hours and wage rates, after the policy change was announced, the work time and wage rate quickly return back to the initial level at least after two years, and there are no persistent impacts on women's labor market outcomes.

5.4.1 Dynamic Effects Using Treatment Heterogeneity Robust Methods

As mentioned above, the estimation of dynamic treatment effects under this staggered adoption design could be biased due to treatment effects heterogeneity by both time and treatment groups. Another concern in the model specification (2) is that it considers the always-treated group of women as being treated in 2010. It also uses early and late-treated groups as controls to calculate group and time-specific average treatment effects. A better way to estimate the dynamic effects is to ignore all always-treated women.

I have employed alternative methods for estimating dynamic impacts in response to these concerns. The first approach used is the heterogeneity robust estimation method proposed by Callaway and Sant'Anna (2021). This method addresses the "negative weights" problem by excluding the always-treated groups and relying on either the never-treated or not-yet-treated groups as controls. This allows for estimating time-group-specific average treatment effects across all relevant pairs of comparisons. The results are then aggregated to derive either equally weighted or group size weighted overall treatment effects and event study style treatment effects.

In particular, I chose to use not-yet-treated groups of women as controls in my setup since there are no never-treated individuals since everyone has been eligible to have a second child since 2016. A1a of A1 shows the estimates of dynamic impacts of the one-child policy lift on new births by relative years concerning the eligibility. It shows that there were no pre-trends before the policy changed, while the same year when the policy changed, there was a large and insignificant increase in fertility, and also two years after the policy changed. The boosting effects on new births did not persist after more than four years. A1b of A1 shows that there were no impacts of the policy shift on working status the same year as the policy change happened and two years after, but there were positive impacts on employment four years and six years after. Moreover, A2a of A2 shows large and more persistent impacts on weekly work hours even after six years. There was a slight drop in salary earnings in the first two years but not persistent, shown by A2b of A2. With fewer observations and focusing on only the first policy change in 2013, the alternative method by Callaway and Sant'Anna (2021) still shows a similar overall trend. There was a significant increase in fertility, which was not persistent, and there was also suggestive evidence that the impacts on work-related outcomes, mainly working hours, persisted.

Another alternative method I used to estimate the dynamic impacts of the policy change and draw even study graphs was the method proposed by Wooldridge (2021) (also see Borusyak et al. (2022), Gardner (2021), and Liu et al. (2022) for similar specifications). The method he called "the two-way Mundlak Regression" fits a TWFE model $y_{it}(\inf) = \alpha_i + \theta_t + \epsilon_{it}$ using only not-yet-treated observations at specific periods. Then, he uses the fitted model to predict never-treated outcomes for each unit and get unit-specific treatment effects as $\hat{y_{it}}(\inf) - y_{it}$, and average treatment effects further. As reviewed by Roth et al. (2022), this method can potentially improve efficiency by using more periods than other methods, but it requires stronger parallel trend assumptions.

Figure A3 and Figure A4 show estimates of the dynamic impacts of the policy change on all primary outcomes. Figure A3a shows an insignificant increase in fertility after the policy change was announced and persisted even after 6 years. Figure A3b shows no significant impacts on working status after the policy changed and slightly positive trends, if any. Figure A4a shows that the one-child policy relaxation decreased the weekly working hours of affected women the same year, but this impact was not persistent either. Lastly, Figure A4b shows that, on average, there were slight but persistent drops in annual salary income among affected women, even after six years.

In summary, dynamic estimates using the two alternative methods can provide more free-of-treatment heterogeneity bias results and ignore always-treated individual groups. Estimates using both methods provide some common findings. First, the relaxation of the one-child policy led to a short-term rise in fertility, but the impacts were not persistent. Second, there were no detectable impacts on labor participation but persistent negative impacts on weekly working hours. Third, even though not significant, there was some evidence of lasting negative impacts on the salary income of newly eligible women after the two-child policy was announced. The dynamic analysis implies that affected women who lost their jobs after the policy change returned to work quickly, but there is some evidence showing that the negative impacts on work hours and wage rates are persistent, even six years after the policy change was announced.

5.5 Robustness Check

I conducted a series of checks to show my results were robust to alternative specifications, and the population policy change caused the changes in labor market outcomes, not other parallel policy changes.

5.5.1 Robustness Checks: Impacts on Women of Minority Ethnicities

As mentioned above in the Section 2 part, the regulations of fertility on couples from minority ethnicities were different from those of Han ethnicity. Whether the relaxed policy fits couples with both partners from minority ethnicities or just one also differs by province, hukou status, and situations (Wang et al. (2017)). Also, the complementation of the one-child policy was normally less stringent against minority couples (Zhang (2017)). If the one-child policy lift caused a decline in working status, the impacts on women from minority groups after the policy change should be minor. Table A5 indicates that the relaxation of the one-child policy had no impact on fertility among women of ethnic minorities, but they were still less likely to work, worked slightly fewer hours a week, had lower wage rates, and had more job displacement due to both voluntary and involuntary reasons. The fact that they also experienced a significant drop in working status but did not have more childbirths provides another piece of evidence that the indirect impacts of the one-child policy lift through employers' reaction matter more on women's labor market outcomes.

5.5.2 Robustness Checks: Impacts on Males and Older Women

In this part, I introduce the impacts of the one-child policy relaxation on married men aged 16 to 45 in 2010 to show how the relaxation of the birth control policy exacerbated gender disparities in the labor market. Moreover, the impacts on men and married women aged 46 to 60 can also indicate whether there are parallel policy shocks causing negative impacts on women's labor market outcomes.

Table A6 shows regression results of the impacts of the policy change on married males aged 16 to 45 in 2010 and married women above 45 and below 60 based on their eligibility status change. Affected males were having more new births, which was a mechanical result, and they also experienced a smaller drop in working status and neglectable impacts on work time and earnings. They also had a lower rate of getting promotions and significantly more voluntary job displacement. The voluntary job change can be caused either by family reasons or by searching for new jobs. However, the relatively smaller drop in working status implies that newly eligible men react to the policy change and higher family fertility by changing jobs, even though the results did not find evidence of a wage rise. Another specification is focused on married women aged 45 to 60 before the policy change. There should not be any detectable impacts on their childbearing, and if there are any impacts found on their labor market outcomes, they can only be indirect impacts. Table A6 shows that the policy change on these women had no impact on their fertility, which was basically zero, neither no working status nor weekly work time. However, they received fewer promotions, got fewer new jobs, had lower wage rates, and were less likely to leave their previous jobs voluntarily. The results indicate that there were no other policy or economic shocks affecting women's labor supply, and the mistreatment of female employees is not limited to fertile aged women. Working women above 45 years old also received fewer promotions, earned less, and chose to stay with previous employers, seemingly due to fewer new hires of women. These findings are consistent with the finding in He et al. (2023), in which they proved newly second-child-eligible women received fewer calls after the universal two-child policy was announced.

5.5.3 Results Using Alternative Estimation Methods

To echo the recent concern about potential bias caused by treatment effects heterogeneity while estimating the average treatment effects in a difference in differences design, especially in a staggered adoption design, I also used alternative methods to estimate static average treatment effects, using limited samples.

First, to test how serious is the potential negative weights problem in my design, I use the method proposed by de Chaisemartin and D'Haultfœuille (2020)[10] to calculate the proportion of comparisons with negative weights and the scale of the sum of negative weights. The tests show that under the common trends assumption, TWFE estimates the average treatment effects of 20 local average treatment effects (LATE) for new birth, working status, weekly working hours, and salary earnings, and 18 for promotion. Among the weights assigned to these specific LATEs, 6 weights in all weights of the estimates of new birth, working status, and salary earnings were negative, 7 in the estimate of weekly working hours, and 5 in the estimate of promotion were negative. The sum of negative weights

[10] Using Stata code "*twowayfeweights*" provided by de Chaisemartin and D'Haultfœuille (2020)

among these estimates ranges from 0.148 to o.588, so the negative weights problem exists in my setup. While the TWFE estimates of the outcome of voluntarily leaving previous jobs and involuntarily leaving both have 5 negative weights among 15 total LATEs, the sum of negative weights was only 0.064. It implies that the treatment heterogeneity issue is doubtful to lead to biased results for the ATE estimates of these two main outcomes.

Since there were potential concerns about the bias in the estimation of average treatment effects, I also used alternative methods, the treatment heterogeneity robust methods proposed by Callaway and Sant'Anna (2021) and by Wooldridge (2021) to generate ATE as a robustness check. In the dynamic estimate part, I used both of these two methods to reach the event study results. These two methods can also naturally calculate ATE using either an equally weighted sum or a group-size weighted sum of LATEs.

The CS2021 estimation method shows positive impacts of the one-child policy relaxation on new birth, working status, and involuntary displacement, all insignificant though. It also generates negative but insignificant results on the impacts of the policy change on weekly working hours, probability of getting promotions, salary earnings, and voluntarily leaving previous jobs, and results are insignificant either.

The JW2021 estimation method generates positive and insignificant results of the impacts on new birth, working status, and involuntary displacement, and negative and insignificant results of the impacts on weekly working hours, salary earnings, and voluntarily leaving previous jobs. It also generates significantly negative results on the impacts of the policy change on getting promotions.

In general, the JW2021 method is more efficient than other alternative estimation methods, including the CS2021 method, but it still uses fewer observations than the TWFE method. All regression results generated by both the JW2021 and CS2021 methods are still of the same sign as the TWFE estimates except for the result on working status, while the impacts on the event of getting promotions are still significantly negative using the CS2021 method.

6 Conclusion

After over 35 years, China changed its famous and highly controversial one-child policy to a universal two-child policy in 2015, with a clear intention to reverse the trends of declining fertility and population aging. However, the responses of different families to this policy change, as well as the impacts of this policy change on labor market outcomes, particularly on women, are still unclear.

In this paper, I use a nationally representative household panel survey in China from 2010 to 2020, the CFPS survey dataset, and a staggered Difference in Differences research design based on the staggered adoption nature of the policy shift processes to casually identify the impacts of this one-child policy relaxation. Variations come from detailed regulations on couples with different family compositions, ethnicities, and hukou statuses from different provinces and stages of the policy change.

Regression results show that, on average, eligible women are having more new births and are also less likely to work, working fewer hours per week, getting fewer promotions, earning less, and are forced to leave previous jobs more often. The negative impacts on work-related outcomes are most pronounced among women at older ages (26 to 35 and 36 to 45 in 2010), and women with only one child, particularly with only one daughter, even though their responses on fertility were relatively smaller. These discrepancies indicate that the negative impacts the newly eligible women experienced mainly indirect impacts of the policy change, particularly mistreatment against them at the workplace and overestimation of their fertility willingness from employers. Dynamic estimation also provides suggestive evidence of the overestimation by showing that these policy-affected women quickly return to work, using different methods, including the traditional event study method, the heterogeneity robust methods by Callaway and Sant'Anna (2021) and Wooldridge (2021). Lasting negative impacts on work time and salary earnings showed that this negative shock has persistent impacts. Multiple robustness checks were practiced to show that this one-child policy change caused the impacts found through the regression. All results were robust using different specifications and alternative estimation methods.

The findings in this paper provide a comprehensive analysis of the individual response to a more generous fertility policy and further impacts on labor market outcomes, particularly for women. Due to overestimation and misconceptions about fertility rise, women could be hurt in the labor market and hinder efforts to gender disparities reduction. This misconception could also be one of the reasons that the population policy change in China was ineffective in reversing the downward fertility rate trend and could not encourage more childbearing despite multiple incentives provided by the local government. More stringent women's employment protection policies, low-cost social childcare facilities, and anti-discrimination in recruitment are necessary to make the population-encouraging policy work.

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Tables

Table 1: Summary Statistics

	Mean	SD	N
Own Information			
Urban	0.467	0.499	41663
Hukou	0.236	0.425	41411
Minority	0.107	0.309	38644
Single Child	0.055	0.228	31795
Child Amount	1.605	0.853	43080
Working Status	0.750	0.433	41632
Finding New Jobs	0.170	0.376	40780
Non-voluntarily Leaving	0.010	0.098	25985
Voluntarily Leaving	0.094	0.292	25985
Weekly Work Hours	34.233	28.135	34643
Getting Promotion	0.030	0.169	33345
IHS Wage Rate	0.968	1.474	38801
Doing Agricultural Work in 2010	0.252	0.434	42905
Being Employed in 2010	0.262	0.440	42905
Working in Private Sectors in 2010	0.243	0.429	41699
Education Years	7.587	4.545	40975
Junior High School	0.540	0.498	42447
College Graduates	0.092	0.289	42447
Having One Child in 2010	0.459	0.498	43080
Having No Child in 2010	0.109	0.311	43080
Having More Than One Child in 20	0.433	0.495	43080
Spouse's Information			
Single Child	0.094	0.291	26582
Working Status	0.906	0.292	35239
Finding New Jobs	0.207	0.405	34104
Weekly Work Hours	46.614	25.464	28255
Getting Promotion	0.047	0.211	27792
IHS Wage Rate	1.624	1.739	31309
Education Years	8.653	3.976	34650
Junior High School	0.639	0.480	34154
College or Higher	0.106	0.308	34154
Doing Agricultural Work in 2010	0.250	0.433	34200
Being Employed in 2010	0.375	0.484	34200
Working in Private Sectors in 20	0.362	0.481	32148

Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. Observations were restricted among married women aged 16 to 45 in 2010.

Table 2: Regression Results on New Birth of All Fertile Aged Women and by Three Age Groups

	(1) Age 16 to 45	(2) Age 16 to 25	(3) Age 26 to 35	(4) Age 36 to 45
treatment	0.054***	0.204***	0.048***	0.005*
	(0.005)	(0.022)	(0.010)	(0.002)
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	38,188	6,209	13,271	18,708
Control Means	0.064	0.230	0.096	0.010

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All data are from the China Family Panel Survey (CFPS) from 2010 to 2020. The outcome variable new birth is defined to be one if, at this year, a woman had a new birth at that year or the year before. Regressions were restricted among married women in 2010, and results of four groups are shown: 16 to 45, 16 to 25, 26 to 35, and 36 to 45 in 2010. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Table 3: Regression Results on Labor Market Outcomes

	(1) Working Status	(2) Weekly Work Hours	(3) Promotion	(4) Finding New Jobs
treatment	-0.032***	-2.654***	-0.006*	-0.034***
	(0.008)	(0.623)	(0.004)	(0.007)
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	36,873	29,733	28,811	36,085
Control Means	0.710	32.295	0.042	0.155

	(5) IHS Wage Rate	(6) Involuntarily Leaving	(7) Voluntarily Leaving
treatment	-0.088***	0.009***	0.008
	(0.025)	(0.003)	(0.008)
Year FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	33,961	22,120	22,120
Control Means	0.938	0.010	0.058

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All data are from the China Family Panel Survey (CFPS) from 2010 to 2020. Among married women aged 16 to 45 in 2010, regression results of six outcome variables are reported: working status, weekly work hours, promotion, inverse hyperbolic sine transformation (IHS) of wage rates, involuntarily and voluntarily leaving previous jobs. Working status equals one if the individual worked at least one hour in the past week before the survey, can return to work within six months, or is during business off-season. Weekly work hours are a variable reflecting the total work time in a normal week of their main job in the past 12 months at the time of the survey. Promotion is based on whether the respondent got either a technical promotion or an executive promotion in the past 12 months. IHS wage rates were calculated using annual employed income and annual work hours. Involuntarily leaving contains bankruptcy, shutdown or dissolution, layoffs or job cuts, to be fired or dismissed, contract expiration, and end of seasonal or temporary work. Voluntarily leaving contains leaving for childbirth or other family issues, looking for another job, accepting a new job offer, returning to school or skill training, retiring or leaving office, or other specific reasons not listed. In 2020, there was a new option: leaving because of COVID-19, which could be voluntary, involuntary, or both. The analysis of COVID-19 on working status is beyond the scope of this paper, so I exclude this answer. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Table 4: Regression Results on Women's Labor Market Outcomes by Age Groups

	Working Status			Weekly Work Hours			Promotion			Finding New Jobs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
treatment	-0.019	-0.040***	-0.017	-3.242*	-3.537***	-1.598*	-0.010	-0.004	-0.007*	-0.022	-0.054***	-0.017*
	(0.025)	(0.014)	(0.011)	(1.668)	(1.024)	(0.888)	(0.012)	(0.008)	(0.004)	(0.022)	(0.014)	(0.010)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,905	12,764	18,204	5,038	10,365	14,330	4,539	9,991	14,281	5,662	12,421	18,002
Control Means	0.512	0.701	0.752	23.212	32.175	34.092	0.045	0.054	0.036	0.227	0.182	0.126

	IHS Wage Rate			Non-voluntarily Leaving			Voluntarily Leaving		
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
treatment	-0.171**	-0.057	-0.053	-0.010	0.011	0.011***	-0.003	0.001	0.012
	(0.068)	(0.046)	(0.032)	(0.011)	(0.008)	(0.004)	(0.034)	(0.014)	(0.010)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,415	11,603	16,943	3,751	7,777	10,592	3,751	7,777	10,592
Control Means	0.703	1.129	0.877	0.009	0.014	0.008	0.111	0.051	0.056

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All data are from the China Family Panel Survey (CFPS) from 2010 to 2020. Regressions were restricted among married women aged 16 to 45 in 2010. Each group of regression results with the same outcome contains three columns, separated by three age groups: 16 to 25 in 2010, 26 to 35 in 2010, and 35 to 45 in 2010. Dependent variables include working status, weekly work hours, promotion, and inverse hyperbolic sine transformation (IHS) of yearly salary earnings, involuntarily leaving the last job, and voluntarily leaving the last job. Details of each outcome can be found in table notes of Table 3. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Table 5: **Heterogeneous Impacts by Child Amount in 2010**

	(1) New Birth	(2) Working Status	(3) Weekly Work Hours	(4) Promotion
treatment	0.047***	-0.016	-1.257	0.006
	(0.005)	(0.011)	(0.868)	(0.004)
treatment × No Child	0.114***	0.028	2.301	-0.012
	(0.015)	(0.024)	(1.631)	(0.013)
treatment × One Child	-0.010	-0.036***	-3.215***	-0.023***
	(0.006)	(0.012)	(1.016)	(0.006)
Test:$\beta\_1 + \beta\_2 = 0$	0.000	0.587	0.481	0.652
Test:$\beta\_1 + \beta\_3 = 0$	0.000	0.000	0.000	0.002
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	38,188	36,870	29,730	28,808
Control Means	0.064	0.710	32.292	0.042

	(5) Finding New Jobs	(6) IHS Wage Rate	(7) Involuntarily Leaving	(8) Voluntarily Leaving
treatment	0.004	-0.164***	0.014***	0.017*
	(0.010)	(0.031)	(0.004)	(0.010)
treatment × No Child	-0.074***	0.114	-0.021*	-0.040
	(0.021)	(0.071)	(0.013)	(0.034)
treatment × One Child	-0.061***	0.130***	-0.008	-0.011
	(0.012)	(0.040)	(0.005)	(0.012)
$Test : \beta\_1 + \beta\_2 = 0$	0.001	0.453	0.566	0.493
$Test : \beta\_1 + \beta\_3 = 0$	0.000	0.315	0.180	0.579
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	36,085	33,964	22,123	22,123
Control Means	0.155	0.939	0.010	0.058

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. All regressions were restricted among married women aged 16 to 45 in 2010 with one and only one child. Dependent variables include new birth, working status, weekly work hours, promotion, and inverse hyperbolic sine transformation (IHS) of yearly salary earnings, involuntarily leaving the last job, and voluntarily leaving the last job. Details of each outcome can be found in table notes of Table 2 and Table 3. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Table 6: Heterogeneous Impacts by Gender of the First Child in 2010

	(1) New Birth	(2) Working Status	(3) Weekly Work Hours	(4) Promotion
treatment	0.092***	-0.115***	-8.551***	-0.036***
	(0.012)	(0.019)	(1.449)	(0.012)
treatment × First Child as a Son	-0.020*	0.065***	4.426***	0.037***
	(0.011)	(0.019)	(1.509)	(0.013)
$Test: \beta\_1 + \beta\_2 = 0$	0.000	0.000	0.000	0.866
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	17,169	16,651	13,607	13,023
Control Means	0.074	0.722	34.139	0.056

	(5) Find New Jobs	(6) IHS Wage Rate	(7) Involuntarily Leaving	(8) Voluntarily Leaving
treatment	-0.072***	-0.063	0.010	0.002
	(0.018)	(0.067)	(0.009)	(0.022)
treatment × First Child as a Son	0.060***	-0.110	-0.002	-0.005
	(0.018)	(0.070)	(0.010)	(0.022)
$Test: \beta\_1 + \beta\_2 = 0$	0.393	0.000	0.138	0.838
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	16,295	15,046	10,047	10,047
Control Means	0.167	1.248	0.011	0.066

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. All regressions were restricted among married women aged 16 to 45 in 2010 with one and only one child. Dependent variables include new birth, working status, weekly work hours, promotion, and inverse hyperbolic sine transformation (IHS) of yearly salary earnings, involuntarily leaving the last job, and voluntarily leaving the last job. Details of each outcome can be found in table notes of Table 2 and Table 3. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Table 7: Regression Results on Women's Labor Market Outcomes by Employer Type

	(1) New Birth	(2) Working Status	(3) Weekly Work Hours	(4) Promotion
treatment	0.059***	-0.022	-0.175	0.011
	(0.014)	(0.018)	(1.323)	(0.022)
treatment × private	-0.004	-0.023	-3.498**	0.013
	(0.012)	(0.019)	(1.392)	(0.021)
$Test: \beta\_1 + \beta\_2 = 0$	0.000	0.004	0.004	0.127
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	9,042	8,840	7,221	6,976
Control Means	0.045	0.954	48.880	0.119

	(5) Find New Jobs	(6) IHS Wage Rate	(7) Involuntarily Leaving	(8) Voluntarily Leaving
treatment	0.079***	0.064	0.015	0.012
	(0.022)	(0.093)	(0.010)	(0.023)
treatment × private	-0.096***	-0.291***	0.012	0.025
	(0.022)	(0.096)	(0.012)	(0.024)
$Test: \beta\_1 + \beta\_2 = 0$	0.423	0.002	0.003	0.092
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	8,673	7,587	5,572	5,572
Control Means	0.281	2.401	0.007	0.054

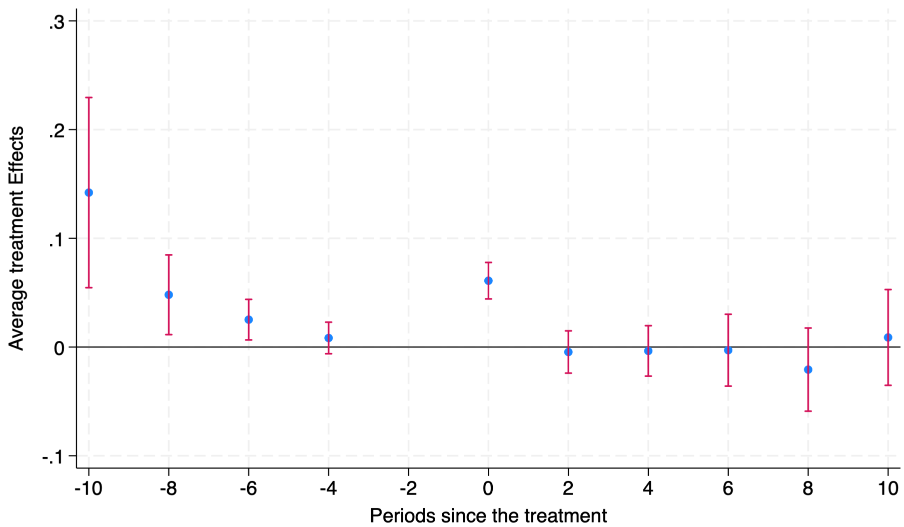
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

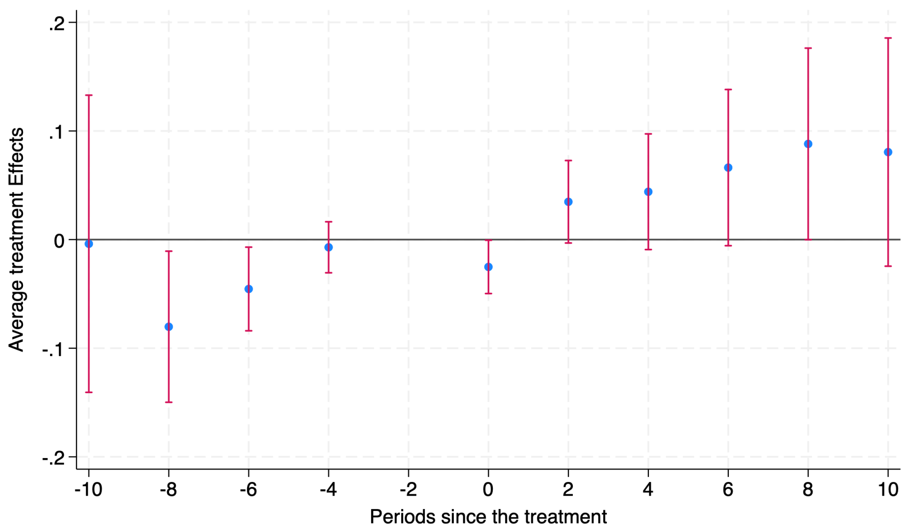
Notes: All data are from the China Family Panel Survey (CFPS) from 2010 to 2020. Regressions were restricted among married women aged 16 to 45 in 2010. the covariate private Dependent variables include working status, weekly work hours, promotion, and inverse hyperbolic sine transformation (IHS) of yearly salary earnings, involuntarily leaving the last job, and voluntarily leaving the last job. Details of each outcome can be found in table notes of Table 3. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Figures

Figure 1: Dynamic Impacts on New Birth and Working Status



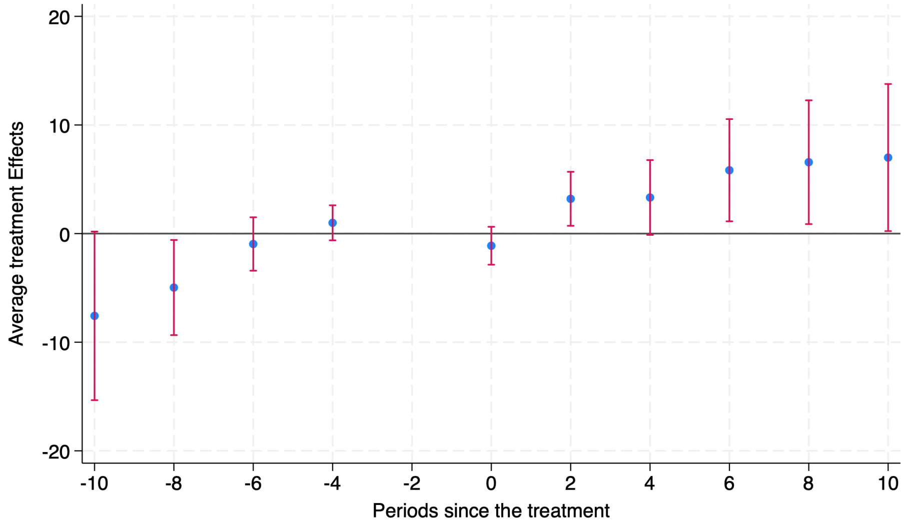
(a) Dynamic Impacts on New Birth



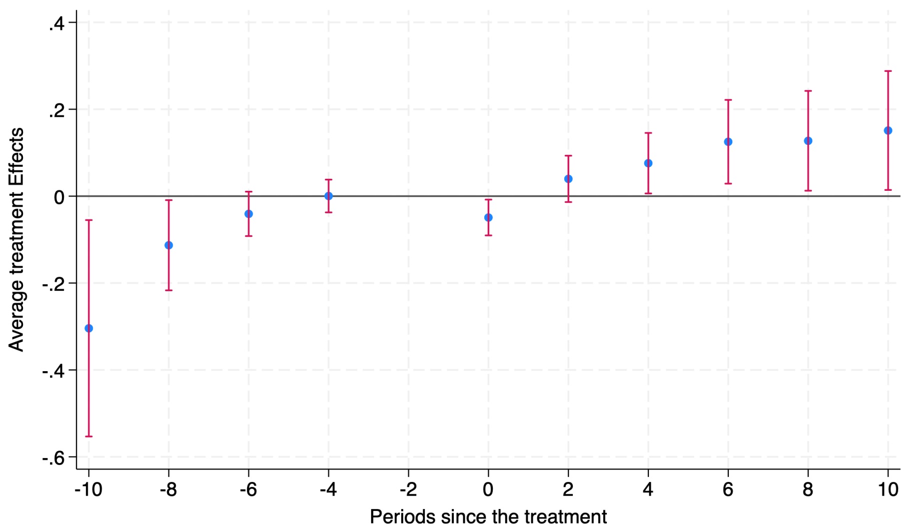
(b) Dynamic Impacts on Working Status

Notes: All data are from the China Family Panel Survey (CFPS) from 2010 to 2020. Regressions were restricted among married women aged 16 to 45 in 2010. Two outcomes are new birth and working status. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level. These figures show dynamic estimates using the Stata package "csdid" proposed by Callaway and Sant'Anna (2021), and the figures were drawn using Stata package "event_plot" proposed by Borusyak et al. (2022).

Figure 2: Dynamic Impacts on Weekly Work Hours and Wage Rates



(a) Dynamic Impacts on Weekly Work Hours



(b) Dynamic Impacts on Wage Rates

Notes: All data are from the China Family Panel Survey (CFPS), the year 2010 to 2020. Regressions were restricted among married women aged 16 to 45 in 2010. Two outcomes are weekly working hours and yearly salary earnings. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level. The figures were drawn using the Stata package "event_plot" proposed by Borusyak et al. (2022).

A Appendix

Tables

Table A1: Regression Results on New Marriage and Divorce

	Newly Married		Newly Divorced
	(1)	(2)	(3)
Hukou $\times$ single_child $\times$ post_2014	0.033		
	(0.029)		
Hukou $\times$ single_child $\times$ post_2016		0.005	
		(0.025)	
treatment			-0.032***
			(0.004)
Year FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	27,846	27,846	28,297
Control Means	0.052	0.052	0.028

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All data are from the China Family Panel Survey (CFPS) from 2010 to 2020. The dependent variable in the first two columns is Newly Married, and it equals one if the marriage status changes from unmarried, cohabitation, divorced, or widowed to married in a specific year compared with the previous survey year. The main regression results are the cross effects of Hukou(one if having non-agricultural hukou), being a single child, and time indicators of post-2014 or 2016, representing after the policy change in 2014 or 2016, and it is restricted to all fertile-aged women. The third column shows regression results on new divorce among married women at the same age interval, and the outcome is new divorce, which equals one if the marriage status changed from being married, cohabitation, unmarried, or widowed to divorced. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Table A2: Differential Impacts by Education Level in 2010

	(1) New Birth	(2) Working Status	(3) Weekly Work Hours	(4) Promotion
treatment	0.056***	-0.011	-1.004	0.005
	(0.005)	(0.012)	(1.038)	(0.004)
treatment × Having No Child	0.014	-0.079	-0.145	-0.028
× Junior High School or Higher	(0.033)	(0.053)	(3.540)	(0.024)
treatment × Having One Child	0.012	-0.034	-3.147	-0.038***
× Junior High School or Higher	(0.013)	(0.026)	(2.179)	(0.011)
$Test1 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000	0.675	0.616	0.541
$Test2 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000	0.000	0.000	0.000
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	37,720	36,482	29,408	28,485
Control Means	0.064	0.710	32.292	0.042

	(5) Finding New Jobs	(6) IHS Wage Rate	(7) Involuntarily Leaving	(8) Voluntarily Leaving
treatment	0.006	-0.208***	0.015***	0.011
	(0.012)	(0.033)	(0.005)	(0.010)
treatment × Having No Child	-0.087*	-0.027	-0.007	0.092
× Junior High School Graduates or Higher	(0.048)	(0.148)	(0.018)	(0.080)
treatment × Having One Child	-0.050*	-0.019	0.002	0.020
× Junior High School Graduates or Higher	(0.025)	(0.085)	(0.011)	(0.026)
$Test1 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000	0.853	0.553	0.899
$Test2 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000	0.906	0.353	0.122
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	35,752	33,628	21,802	21,802
Control Means	0.155	0.939	0.010	0.058

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level. Variables no child and one child are based on an individual's child amount in 2010, and the variable junior or higher was defined as one if an individual finished at least junior school by 2010.

Table A3: Differential Impacts by Community or Hukou Category in 2010

	New Birth		Working Status		Weekly Work Hours		Promotion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment	0.049***	0.048***	-0.013	-0.009	-2.214**	-1.340	0.009**	0.007*
	(0.006)	(0.005)	(0.011)	(0.011)	(0.965)	(0.900)	(0.004)	(0.004)
treatment × Having No Child	-0.021		-0.088*		-10.017***		-0.062**	
× Living in Urban Area	(0.032)		(0.049)		(3.356)		(0.027)	
treatment × Having One Child	0.049***		-0.097***		-10.051***		-0.008	
× Living in Urban Area	(0.014)		(0.028)		(2.236)		(0.012)	
treatment × Having No Child		-0.117***		-0.061		-10.473**		-0.075**
× Having Urban Hukou		(0.037)		(0.060)		(4.104)		(0.031)
treatment × Having One Child		0.041***		-0.060		-9.180***		-0.020
× Having Urban Hukou		(0.015)		(0.037)		(2.879)		(0.016)
$Test1 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000		0.185		0.161		0.021	
$Test2 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000		0.000		0.000		0.002	
$Test3 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$		0.019		0.068		0.020		0.011
$Test4 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$		0.000		0.000		0.000		0.001
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37,951	37,864	36,678	36,615	29,572	29,538	28,660	28,618
Control Means	0.064	0.064	0.710	0.710	32.292	32.292	0.042	0.042

	Find New Jobs		IHS Wage Rate		Involuntarily Leaving		Voluntarily Leaving	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
treatment	0.016	0.010	-0.172***	-0.166***	0.010**	0.013***	-0.005	0.007
	(0.011)	(0.011)	(0.034)	(0.032)	(0.005)	(0.005)	(0.010)	(0.010)
treatment × Having No Child	-0.058		0.032		-0.026		-0.026	
× Living in Urban Area	(0.044)		(0.150)		(0.027)		(0.071)	
treatment × Having One Child	-0.044*		-0.046		-0.005		-0.037	
× Living in Urban Area	(0.025)		(0.087)		(0.012)		(0.028)	
treatment × Having No Child		-0.005		-0.184		0.020		0.020
× Having Urban Hukou		(0.051)		(0.198)		(0.023)		(0.073)
treatment × Having One Child		-0.026		0.001		-0.004		-0.031
× Having Urban Hukou		(0.030)		(0.117)		(0.013)		(0.038)
$Test1 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000		0.838		0.549		0.930	
$Test2 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000		0.472		0.110		0.096	
$Test3 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$		0.002		0.308		0.270		0.385
$Test4 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$		0.000		0.864		0.271		0.014
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,911	35,867	33,809	33,751	21,985	21,979	21,985	21,979
Control Means	0.155	0.155	0.939	0.939	0.010	0.010	0.058	0.058

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level. Variables No Child and One Child are based on each individual's child number in 2010. Urban is defined as one in which an individual lives in a community categorized as urban in 2010, and hukou was defined as one in which an individual owned a non-agricultural hukou in 2010.

Table A4: Differential Impacts by Occupation in 2010

	(1) New Birth	(2) Working Status	(3) Weekly Work Hours	(4) Promotion
treatment	0.069***	-0.138***	-6.702***	0.014***
	(0.006)	(0.012)	(1.166)	(0.003)
treatment × Having No Child × non_agri	0.049	0.086	-1.292	-0.026
	(0.042)	(0.054)	(4.277)	(0.018)
treatment × Having One Child × non_agri	-0.008	-0.052**	-1.225	-0.028***
	(0.014)	(0.024)	(2.247)	(0.009)
$Test1 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000	0.029	0.194	0.448
$Test2 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000	0.010	0.000	0.000
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	38,188	36,870	29,730	28,808
Control Means	0.064	0.710	32.292	0.042

	(5) Finding New Jobs	(6) IHS Wage Rate	(7) Involuntarily Leaving	(8) Voluntarily Leaving
treatment	0.030**	-0.206***	0.004	-0.007
	(0.015)	(0.040)	(0.006)	(0.013)
treatment × Having No Child × non_agri	-0.044	-0.270	-0.026	-0.065
	(0.065)	(0.186)	(0.016)	(0.047)
treatment × Having One Child × non_agri	-0.034	-0.026	-0.002	0.021
	(0.029)	(0.082)	(0.012)	(0.029)
$Test1 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000	0.288	0.557	0.498
$Test2 : \beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$	0.000	0.485	0.079	0.143
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	36085	33964	22123	22123
Control Means	0.155	0.939	0.010	0.058

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level. Variables no child one child was based on their child amount in 2010. Non-agricultural was defined as one if an individual's main job was not farming on her land, including employed farming, in 2010.

Table A5: Treatment Effects on Minority Women

	(1) New Birth	(2) Working Status	(3) Weekly Work Hours	(4) Promotion
treatment	0.051***	-0.033***	-2.770***	-0.006
	(0.005)	(0.009)	(0.701)	(0.004)
treatment × Minority	-0.028	-0.045*	-0.380	0.003
	(0.017)	(0.025)	(2.021)	(0.011)
$Test : \beta\_1 + \beta\_2 = 0$	0.170	0.002	0.120	0.760
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	33,168	32,160	25,456	24,193
Control Means	0.064	0.710	32.292	0.042

	(5) Find New Jobs	(6) IHS Wage Rate	(7) Involuntarily Leaving	(8) Voluntarily Leaving
treatment	-0.031***	-0.071***	0.006*	0.010
	(0.008)	(0.027)	(0.004)	(0.009)
treatment × Minority	0.009	-0.172**	0.017	0.055*
	(0.021)	(0.084)	(0.016)	(0.029)
$Test : \beta\_1 + \beta\_2 = 0$	0.303	0.003	0.120	0.021
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	31,543	29,550	18,539	18,539
Control Means	0.155	0.939	0.010	0.058

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. All regressions were restricted among married women aged 16 to 45 in 2010. Dependent variables include new birth, working status, weekly work hours, promotion, and inverse hyperbolic sine transformation (IHS) of yearly salary earnings, involuntarily leaving the last job, and voluntarily leaving the last job. Details of each outcome can be found in table notes of Table 2 and Table 3. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Table A6: Treatment Effects on Males and Older Women

	New Birth		Working Status		Weekly Work Hours		Promotion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment	0.053***	0.001	-0.022***	-0.005	-0.697	-0.439	-0.017***	-0.005***
	(0.005)	(0.000)	(0.006)	(0.010)	(0.625)	(0.661)	(0.005)	(0.002)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Males	Yes		Yes		Yes		Yes	
Age above 45 in 2010		Yes		Yes		Yes		Yes
Observations	32,490	24,494	31,428	23,493	24,561	19,374	24,077	18,417
Control Means	0.066	0.000	0.852	0.552	42.876	21.668	0.076	0.015
	Find New Jobs		IHS Wage Rate		Involuntarily Leaving		Voluntarily Leaving	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
treatment	-0.018**	-0.027***	-0.037	-0.068***	0.004	-0.002	0.018***	-0.022**
	(0.009)	(0.006)	(0.034)	(0.019)	(0.003)	(0.004)	(0.005)	(0.010)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Males	Yes		Yes		Yes		Yes	
Age above 45 in 2010		Yes		Yes		Yes		Yes
Observations	30,166	23,363	27,483	22,748	18,521	13,552	18,521	13,552
Control Means	0.186	0.055	1.582	0.363	0.008	0.013	0.021	0.096

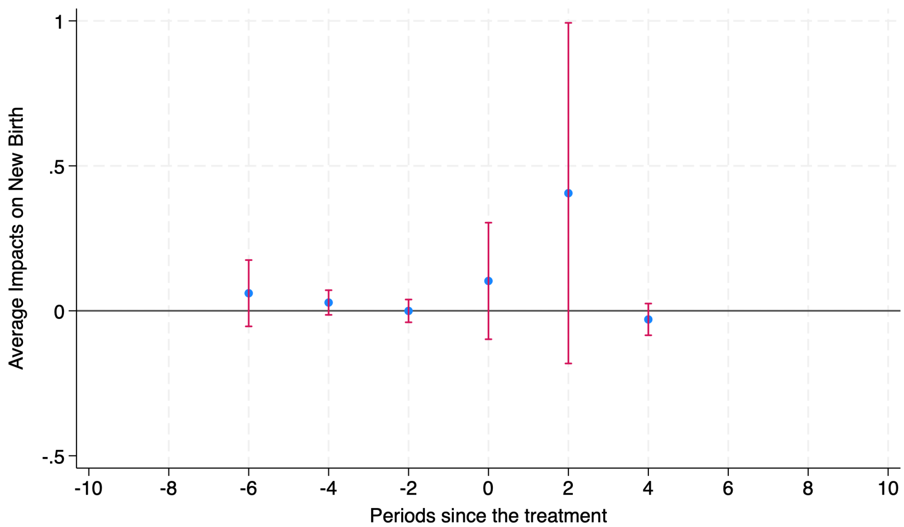
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

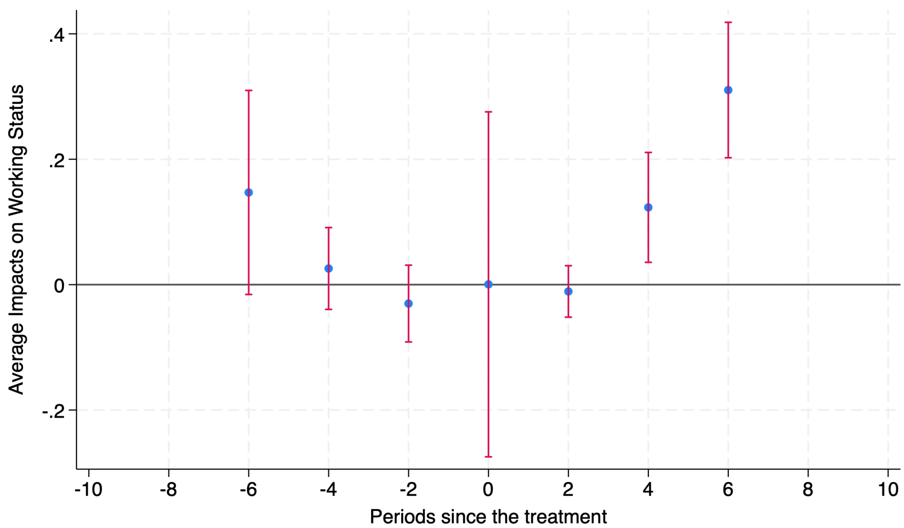
Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. Regressions are restricted among married men aged 16 to 45 in 2010 or married women aged 46 to 60 in 2010. Dependent variables include new birth, working status, weekly work hours, promotion, inverse hyperbolic sine transformation (IHS) of yearly salary earnings, involuntarily leaving the last job, and voluntarily leaving the last job. Details of each outcome can be found in table notes of Table 2 and Table 3. All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Figures

Figure A1: Dynamic Impacts Using CS2021



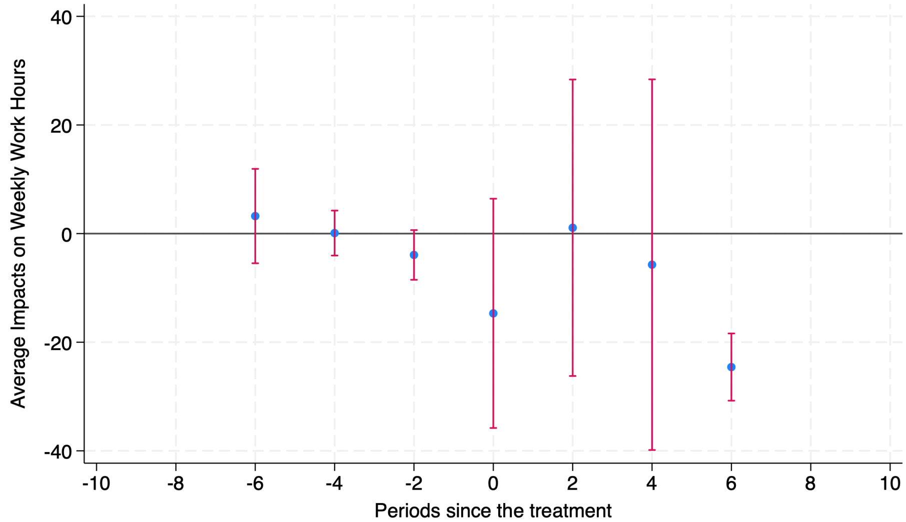
(a) Dynamic Impacts on New Birth Using CS2021



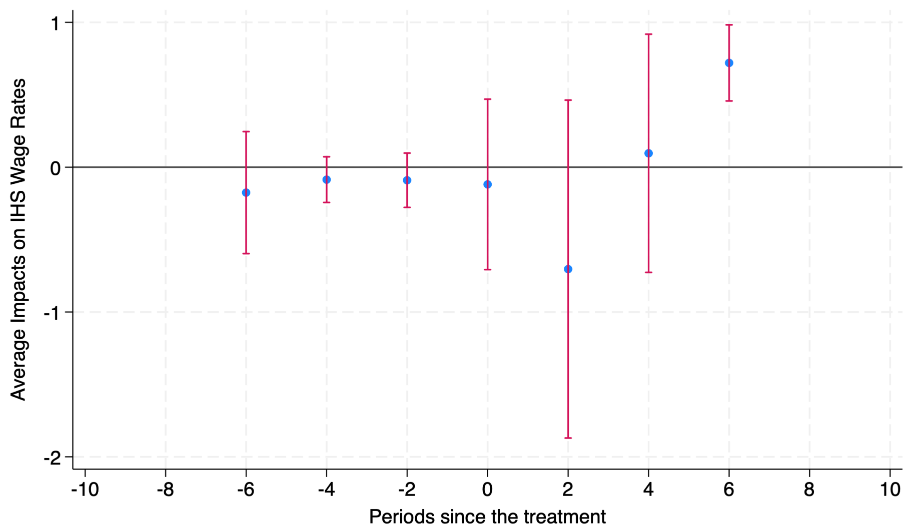
(b) Dynamic Impacts on Working Status Using CS2021

Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. All regressions were restricted among married women aged 16 to 45 in 2010 with one and only one child. Estimation results were achieved using the Stata package ”csdid”, based on the estimation method proposed by Callaway and Sant'Anna (2021). The event-study graph was drawn using the Stata package ”event-plot” by Kirill Borusyak (Borusyak et al. (2022)). All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Figure A2: Dynamic Impacts Using CS2021 (Continued)



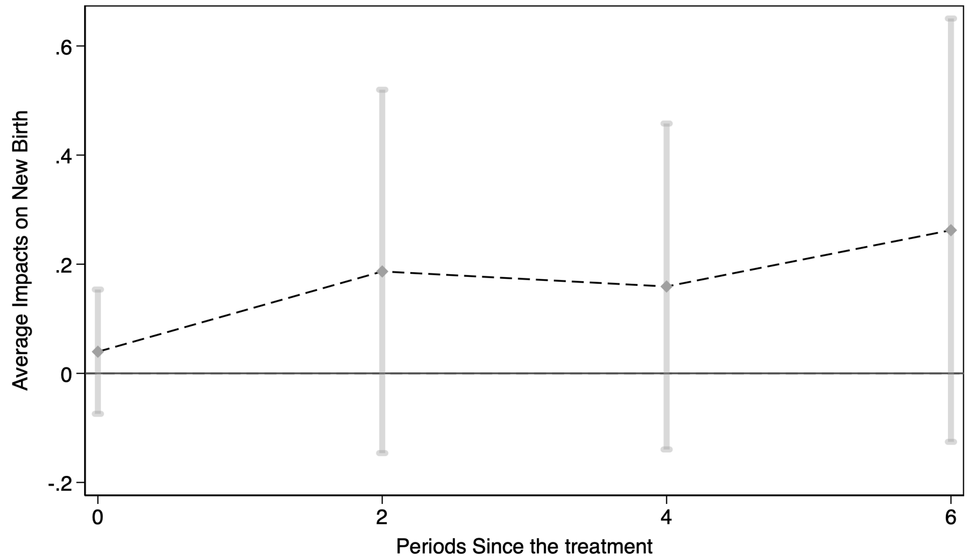
(a) Dynamic Impacts on Weekly Working Hours Using CS2021



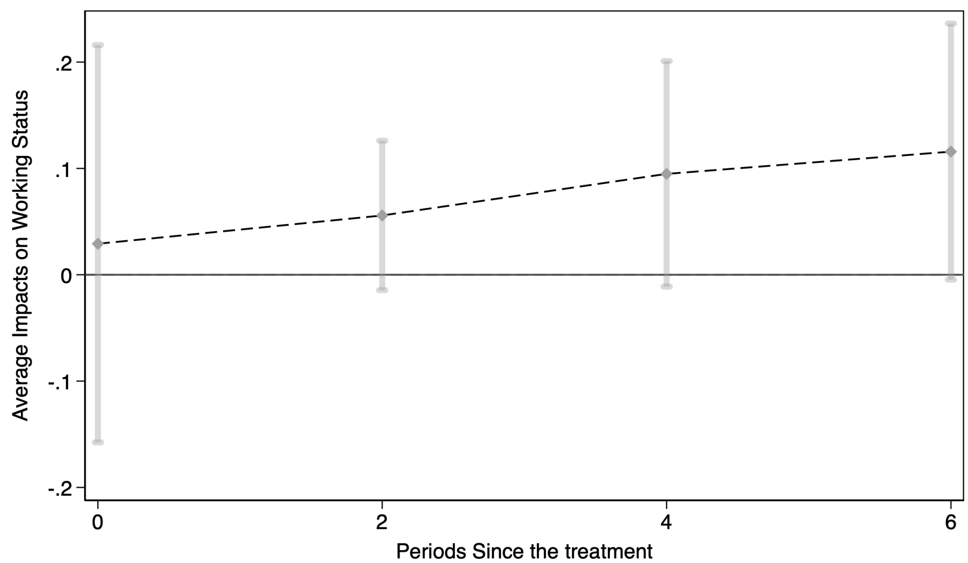
(b) Dynamic Impacts on Wage Rates Using CS2021

Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. All regressions were restricted among married women aged 16 to 45 in 2010 with one and only one child. Estimation results were achieved using the Stata package "csdid", based on the estimation method proposed by Callaway and Sant'Anna (2021). The event-study graph was drawn using the Stata package "event-plot" by Kirill Borusyak (Borusyak et al. (2022)). All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Figure A3: Dynamic Impacts Using JW2021



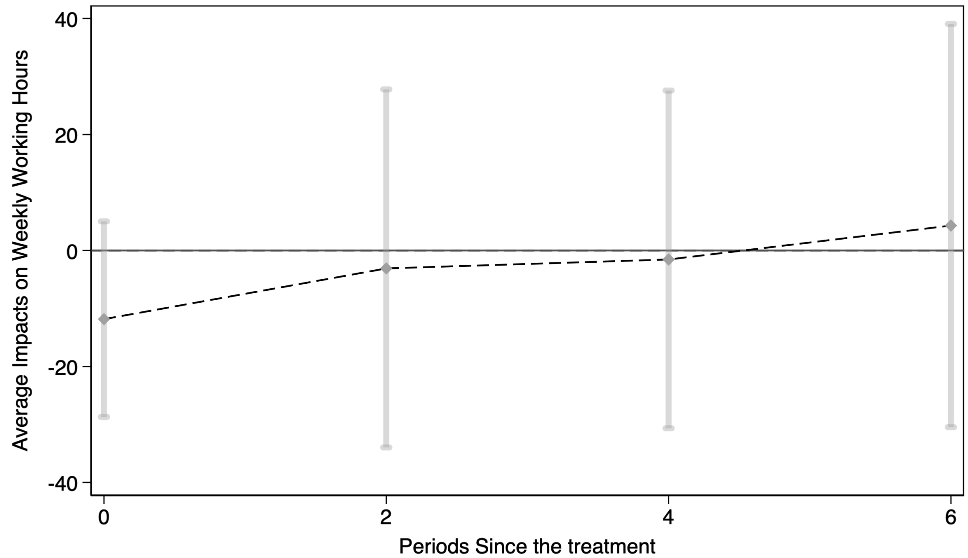
(a) Dynamic Impacts on New Birth Using JW2021



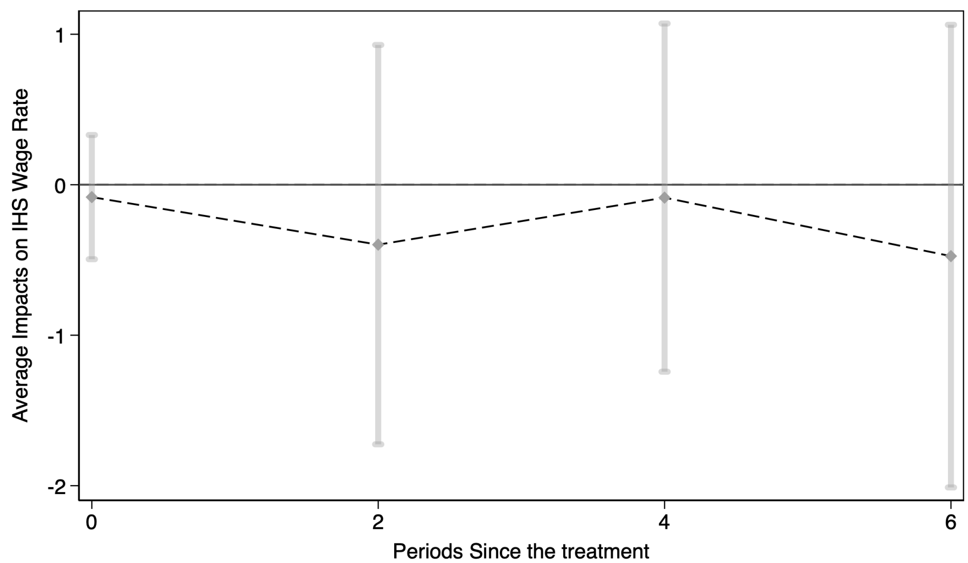
(b) Dynamic Impacts on Working Status Using JW2021

Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. All regressions were restricted among married women aged 16 to 45 in 2010 with one and only one child. Estimation results were achieved using the Stata package "jwdid" by Fernando Rios-Avila, based on the estimation method proposed by Wooldridge (2021). All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.

Figure A4: Dynamic Impacts Using JW2021 (Continued)



(a) Dynamic Impacts on Weekly Working Hours Using JW2021



(b) Dynamic Impacts on Wage Rates Using JW2021

Notes: All data are from the China Family Panel Survey (CFPS), 2010 to 2020. All regressions were restricted among married women aged 16 to 45 in 2010 with one and only one child. Estimation results were achieved using the Stata package "jwdid" by Fernando Rios-Avila, based on the estimation method proposed by Wooldridge (2021). All specifications include individual fixed effects and year fixed effects. Standard errors are robust and clustered at the individual level.