The Impact of Fertility Relaxation on the Gender Wage Gap[1]

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Abstract

In this paper, we study the immediate impact of fertility relaxation on the gender wage gap. We explore a 2013 policy shock that relaxed the One-Child Policy in China. Using an employer-employee matched administrative data, we show that after the policy shock, the salary of female new hires is reduced by 2.7% relative to the salary of male new hires, equivalent to a 47% increase in the gender wage gap in the data. The effect is more pronounced for female new hires aged below 35, implying the salary reduction is related to concerns about the increasing fertility of females.

Keywords: One-Child Policy; Gender Wage Gap; Labor Market
JEL Codes: J13; J18; J20

Data Availability Statement

The data used in this paper are confidential, so we cannot make them public. However, we allow anybody to come to Tsinghua University to replicate our results. All the code and other material are available upon request to the corresponding author.

Disclosure Statement

The authors have nothing to disclose.

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I. Introduction

With increasing concern about the aging population, many countries have implemented policies to boost birth rates, such as parental leave, maternity leave, and childcare, because these policies could reduce the labor market cost of children for women (Baker and Milligan 2008; Lefebvre and Merrigan 2008; Schönberg and Ludsteck 2014; Baker, Gruber, and Milligan 2008; Bauernschuster and Schlotter 2015; Havnes and Mogstad 2011; Dahl et al. 2016) and thus make having children more attractive. However, fertility policies may encourage females to have more children and increase the employers' labor costs, which would adversely affect female labor market outcomes and widen gender inequality in the labor market, even before childbirth (Das and Polachek 2015). To the best of our knowledge, only a few studies focus on the "anticipation effect" of family policies for females prior to childbirth (Baum 2003; Das and Polachek 2015; Kleven et al. 2020), and they find mixed results. In this paper, we study the gender wage gap induced by the fertility policy even before childbirth by exploring the immediate impact of the relaxation of China's One-Child Policy (OCP, hereafter) on the gender wage gap, a crucial indicator of gender inequality.[2]

To estimate the immediate impact of fertility relaxation on the gender wage gap, we explore employer-employee matched administrative data that encompass all of the 5.9 million employees in a major city in China. The data allow us to examine the salary of new hires at a high frequency (monthly).[3] The relaxation of OCP in China was announced in November 2013. Before the policy change, only families with both parents as the only children were allowed to have two children; the new policy stated that families could also have two children if only one parent was an only

[2] The gender wage gap is a well-established fact in labor economics literature, suggesting females earn less than males on average (for a literature review, see Blau and Kahn (2017)).
[3] We cannot study the impact of the policy change on the salary of existing employees, because we cannot infer their salaries in the post-policy period due to data limitations. We elaborate on this point in Section III.A.

child. This policy change may signal an anticipated increase in the childbearing burdens of females and impact new-hire salaries for females, especially for women of reproductive ages. We employ a difference-in-differences design to investigate the causal effect of the relaxation of OCP on female new-hire salaries. Our identification variation comes from the difference in the outcome between female and male new hires, before and after the policy change, *within* each employer. In the most complete setting, we also include employer-year-month fixed effects to control for employer-specific hiring trends and hiring seasonality.

Before estimating the impact of fertility relaxation on the gender wage gap, we first study the policy impact on new hires and job leavers to provide a more thorough understanding of the labor market effect of fertility relaxation. The findings demonstrate fertility relaxation would decrease both female new hires and job leavers compared with males.

We then show that trends in the outcomes for male and female employees were parallel before the policy shock. However, *immediately* after the relaxation of OCP in November 2013, the salary of female new hires was reduced by 2.7% relative to male new hires, equivalent to a 47% increase in the gender wage gap compared with the pre-policy period. Next, we document heterogeneity on the impact of fertility relaxation on the gender wage gap by employees' age cohorts. We find the reduction in salary for female new hires only occurs in the age cohorts below 35, the reproductive ages for women, whereas the fertility shock has little impact on female new hires above 35. Hereby, we conduct a difference-in-difference-in-differences (DDD) design by utilizing older men and women (i.e., ages 46–50) as an additional control group and reach a similar conclusion. We also provide suggestive evidence that the salary reduction for married new hires, who are more likely to give birth, is larger than that for unmarried new hires. The findings suggest the negative policy effect on female labor outcomes is related to the concerns about increasing fertility due to fertility

relaxation. Moreover, we conduct a series of robustness checks to ensure the internal validity of our results. Specifically, we address the concerns about composition change, linear pre-trends, and seasonality effects.

Last, we discuss the mechanisms for the salary reduction of female new hires from the perspectives of demand-side factors (employer) and supply-side factors (employee). First, we have shown employers hire fewer female employees than males after the relaxation of the fertility restriction, suggesting a decrease in female labor demand. Second, we analyze the roles of labor-supply change in both extensive and intensive margins. Using the China Family Panel Studies (CFPS, hereafter) data,[4] we find female labor-force participation remains largely unchanged, and their daily working hours increase compared with the working hours for males after the policy shock. Overall, the main results are likely due to the change in both labor and labor supply.

This study contributes to three strands of literature. First and foremost, this study sheds light on the literature on the labor market effect of family policies. Existing studies have examined the impact of family policies, such as parental leave (Kleven et al. 2020; Lalive and Zweimüller 2009; Lalive et al. 2014), maternity leave (Baker and Milligan 2008; Schönberg and Ludsteck 2014; Dahl et al. 2016), and childcare (Baker, Gruber, and Milligan 2008; Bauernschuster and Schlotter 2015; Havnes and Mogstad 2011; Lefebvre and Merrigan 2008; Kleven et al. 2020). In Appendix B, we summarize the previous studies that examine the impact of those family policies on female labor market outcomes. The literature on parental leave, maternity leave, and childcare policies suggests the extension of parental leave/maternal leave/childcare, in general, has a non-negative effect on

[4] The CFPS survey is an annual longitudinal survey launched by the Institute of Social Science Survey of Peking University. The survey contains a series of questionnaires about households and individuals from 118 cities in 25 provinces in China.

the labor market outcome of mothers after childbirth. Although some studies show the policy impact on mothers right before childbirth, only a few studies focus on the "anticipation effect" of fertility policies for females prior to childbirth (Baum 2003; Das and Polachek 2015; Kleven et al. 2020). Baum (2003) finds parental leave and childcare have little impact on wage and employment; Das and Polachek (2015) show parental leave would increase both the labor-force participation rate and unemployment rate; Kleven et al. (2020) conclude parental leave and childcare would not affect the gender wage gap before childbirth. We differentiate from these three studies in two ways. First, existing literature primarily studies parental leave in developed countries, whereas we analyze a less studied fertility-relaxation policy in China, the largest developing country in the world. Second, our results complement the literature by documenting an immediate negative impact of fertility relaxation on the wages of female employees as well as other labor market outcomes, which is likely to be explained by the perceived increase in female childcaring burdens and labor costs from the demand (employer's) side.

Second, our study speaks directly to the literature on gender inequality in the labor market (see Marianne (2011) and Olivetti and Petrongolo (2016) for a review). Specifically, our study is closely linked to the studies explaining the gender wage gap (see Blau and Kahn (2017) for a recent review). Recent studies argue the child penalty is the most important factor in explaining the remaining gender wage gap (Juhn and McCue 2017; Kleven, Landais, and Søgaard 2019) in developed countries. Our findings show the gender wage gap widened by 47% immediately after the fertility policy, indicating the (perceived) increasing childbirth could contribute much to the gender wage gap as well.

Third, this study also advances the literature on the policy impact of OCP in China. Although OCP has significantly reduced fertility in China (Huang, Lei, and Sun 2021; McElroy and Yang 2000;

Zhang 2017), it distorted the sex ratio with the assistance of sex-selective abortion during the past decades (Chen, Li, and Meng 2013; Ebenstein 2010; Tuljapurkar, Li, and Feldman 1995).[5] In addition, Rosenzweig and Zhang (2009) discuss the trade-off between the quantity and quality of children by exploring the impact of OCP and find the OCP could induce more human-capital investment. Recently, a growing literature has explored the effect of the relaxation of OCP, such as on human capital investment (Liu 2014), gender discrimination (He, Li, and Han 2023), and fertility and maternal labor supply (Wu 2022). We add to this strand of research by documenting the negative impact of the relaxation of OCP on the gender wage gap.

The rest of this paper is organized as follows. Section II discusses the background of fertility policies in China and examines the impact of the relaxation of OCP policy on fertility; Section III introduces the data source and identification strategy in detail; Section IV presents the empirical findings; and Section V concludes.

II. Policy Background

A. One-Child Policy in China

After an eight-year voluntary family-planning campaign that began in 1971, the Chinese central government embarked on an ambitious family-planning policy program in 1979 that included restrictions on the number of children that a couple could have and encouraged late marriage and childbearing (Hesketh, Lu, and Xing 2005; Zhang 2017). The primary part of the policy package specified that, generally, a married couple could have at most one child (therefore, we refer to the

[5] The skewed sex ratio may increase the household savings rate (Wei and Zhang 2011), aggregate savings rate (Bhaskar and Hopkins 2016; Du and Wei 2013), housing prices (Wei, Zhang, and Liu 2017), and even crime rates (Edlund et al. 2013) in China through the marriage-market channel.

family-planning policy program as the One-Child Policy). Couples exceeding the birth quota would be severely penalized by being prohibited from applying for local household registration (*hukou*) for newborn children, being subject to a high monetary penalty, or even losing jobs. The national-level policy enforcement of OCP experienced some adjustments, or even wavering, in the mid-1980s, but had been stabilized since the early 1990s until its recent relaxation.

OCP policy enforcement varied significantly within the country. First, OCP enforcement was generally looser in rural areas because the policy significantly reduced the household labor force for agricultural production. In addition, the preference for a son was more prevalent in rural areas, and birth control reduced the chances of having a son. Observing the realities of OCP implementation in rural areas, several provinces allowed a married couple in rural areas to have a second child if the first child was a daughter, conditional on a sufficiently long spacing between the two births. Second, women of ethnic minorities were typically exempted from OCP and were allowed to have two or more children.

OCP has effectively reduced the fertility rate and thus contributed to the controlled population size in China (Ebenstein 2010; Zhang 2017). The central government's official estimate states that OCP has prevented 400 million births since its introduction in the late 1970s.[6] However, OCP may have been too restrictive on China's population growth from its implementation and for more than 30 years. According to the 2010 population census, the total fertility rate in China had dropped to 1.18, far below the replacement level (2.1). In response to the declining fertility rate, between 1991 and 2011, the 31 provinces in mainland China successively revised their local family-planning

[6] This estimate was first reported at the media conference held by Mr. Bin Li, then the Director of the National Population and Family Planning Commission in China. It then appeared repeatedly in various official documents and reports from the central government.

regulations and allowed families to have two children if *both* parents were only children. This policy adjustment was implemented in our sample city during the 1990s and should not affect analysis in our sample period.

On November 12, 2013, as an important part of a package of policies with the purpose of "comprehensively deepening the reform [in China,]" the Third Plenum of the 18th Central Committee of the Chinese Communist Party (CCP) announced the decision to relax OCP.[7] Under the adjusted policy, families could have two children if even *one* parent was an only child, rather than the more stringent requirement that both parents be only children. Following this announcement, on December 28, 2013, the Standing Committee of the National People's Congress (NPCSC) formally guided the local People's Congresses to amend local family-planning regulations accordingly. Even more importantly, the above two official announcements of the CCP Central Committee and NPCSC both explicitly interpreted this policy change as the beginning of "a continuous and gradual reform of the birth policy." The policy adjustment was then formally implemented in all provinces in the first quarter of 2014 (February in our sample city) when local family-planning regulations were revised. In the following empirical analysis, we utilize this policy adjustment for our identification strategy.

As a next step of policy relaxation, the central government further relaxed the fertility restriction in 2015 and 2021. In November 2015, the Fifth Plenum of the 18th Central Committee of the CCP announced the decision to replace OCP with an unconditional Two-Child Policy (TCP), which allows *all* families to have two children. TCP was formally implemented nationwide beginning January 1, 2016, when the Amendment of the "Law on Population and Family Planning" became

[7] To the best of our knowledge, the package contains no other items related to female labor except for relaxing the OCP.

effective. Furthermore, the central government allowed all families to have *three* children at present. The Three-Child Policy was announced in May 2021 and implemented in July 2021 (Central Committee of the Communist Party of China and State Council 2021).

B. Impact of Fertility Relaxation on Fertility

In this subsection, we provide several pieces of suggestive and direct evidence on the effectiveness of the current relaxation of OCP on fertility both at the national level and in our sample city. At the national level, we show the share of households affected by the conditional two-child policy based on a dataset from CFPS in 2010. Specifically, we keep the households where the females were in the reproductive-age cohorts (younger than 35 years old) and categorize the households into three groups: both parents were an only child, one parent was an only child, and neither parent was an only child. We also show the number of households by household type. As shown in Table A1, the conditional two-child policy would affect around 17% (564/3,327=0.17) of the households who are eligible for a second child after the policy shock at the national level.

More importantly, based on the China Household Finance Survey (CHFS) panel data, Wu (2022) shows that after a household is eligible to have a second child, the number of children would increase by 0.122 (about 10%) on average. The effect is pronounced three years after being eligible. Meanwhile, as stated by Jizhe Ning (the Commissioner of the National Bureau of Statistics of China), the share of the second child increased from 30% in 2013 to 50% in 2017, and the relaxation of OCP induced 10 million newborns between 2014 and 2017 (State Council Information Office 2021).

In our sample city, according to the official statistics of the sample city, about 30,000 eligible households applied for the Birth Approval Certificate for a second child from January to December

2014, accounting for approximately 7% of households of childbearing age and eligible for a second child after the policy change.[8] This figure further increased to 53,000 by September 2015. Given that the number of newborn babies was around 210,000 every year from 2011–2013 in our sample city, the fertility of a second child by eligible couples cannot be neglected. Collectively, the above suggestive evidence demonstrates fertility relaxation increases the fertility of the second child and thus boosts the fertility level.

In addition to the suggestive evidence, we formally conduct an empirical analysis to study the causal impact of fertility relaxation on the fertility rate. Specifically, we manually collect the city-year fertility-rate information from China's Statistical Yearbook. The panel data cover 35 major cities from 2006–2015, one year before the unconditional Two-Child Policy. The research design relies on Ebenstein (2010), who provides the strictness of OCP and fine rates by province. On the one hand, according to OCP enforcement, as discussed in Section II.A, Ebenstein (2010) divides the 31 provinces into three cohorts: OCP, "1.5-Child" Policy, and Two-Child Policy. On the other hand, Ebenstein (2010) also calculates the fine rate when the parents were subject to fines due to additional children. Note Ebenstein (2010) finds fertility is lower in the provinces with stricter fertility control and higher fine rates. Therefore, we can hypothesize that the increase in the fertility rate would be higher in the provinces with stricter fertility control and higher fine rates after the relaxation of OCP. Following the identification strategy, we conduct a DDD design:

$$(1)\quad Y_{cpy} = \beta_1 \times \text{"1.5-Child" Policy}_p \times FINE_p \times POST_y + \beta_2 \times \text{Two-Child Policy}_p \times FINE_p \times POST_y + \beta_3 \times \text{"1.5-Child" Policy}_p \times FINE_p + \beta_4 \times \text{Two-Child Policy}_p \times FINE_p + \beta_5 \times FINE_p \times POST_y + \tau_c + \delta_y + \epsilon_{cy},$$

[8] Note that no eligible households applied for the Birth Approval Certificate for a second child before 2014.

where Y_{cpy} is the fertility rate of city *c* in province *p* in year *y*; *"1.5-Child" Policy*$_p$ and *Two-Child Policy*$_p$ are dummy variables indicating whether the city is located in the provinces belonging to OCP and "1.5-Child" Policy regions, respectively; $FINE_p$ is the fine rate in 2000, the latest data in Ebenstein (2010); $POST_y$ is a dummy variable for the post period (i.e., 2013–2015); τ_c and δ_y refer to the city fixed effects and year fixed effects, respectively; ϵ_{cy} is the error term. The standard errors are clustered at the city level. B_1 and β_2 are parameters of interest, implying the impact of the relaxation of OCP on the fertility rate in the regions with stricter fertility control and higher fine rates, compared with those with more lenient fertility control or lower fine rates.

Table 1 reports the regression results. In various specifications, we show the relaxation of OCP would boost fertility in the regions with stricter fertility control (i.e., OCP) and higher fine rates relative to those with more lenient fertility control (i.e., Two-Child Policy) or lower fine rates. Given that the anonymous sample city in this study belongs to the OCP region and has one of the highest fine rates (Ebenstein 2010), we could conclude the relaxation of OCP would, to some extent, boost fertility in our sample city.

III. Data and Identification Strategy

A. Data

In this study, we introduce a proprietary employer-employee matched dataset from an anonymous major Chinese city from 2010–2014,[9] based on the administrative records from the local Housing Provident Fund (HPF) system. China's HPF system is currently the largest compulsory housing-saving system in the world; it has been legally implemented in all cities in mainland China since

[9] We have to hide the name of the city as required by the data provider. It is one of the largest and most developed cities in China. Both the OCP (and its relaxation) and the HPF scheme in this city are consistent with most other cities.

1994.[10] As required by the "Regulations on the Administration of Housing Provident Funds" (State Council Document No. 1999-262), all full-time employees in urban China are compulsorily required to join the HPF system. Each employee and their employer must contribute a designated percentage of their base salary to the employee's HPF account every month. The employee can then withdraw the savings from their HPF account at the time of a home purchase.

In our sample city, the ratio of contribution to base salary is 12% for both employee and employer between 2010 and 2014; that is, the monthly contribution amount equals 24% of each employee's base salary. The base salary for an existing employee is calculated as his/her average monthly salary in the last calendar year and is annually adjusted every July.[11] For instance, the base salary for an existing employee is constant from July 2014–June 2015, which equals the average monthly salary in the year 2013. Hence, we cannot study the impact of the fertility-relaxation policy on the salaries of existing employees, because we cannot infer the salaries of existing employees in the post-policy period. Because our HPF contribution data are only available before December 2014, we must infer the salary of existing employees back to the year 2013 (the policy was announced in November 2013). However, for a new employee, the HPF system will directly adopt the monthly salary as the base when the employer hires him/her. Hence, we can infer the salary of new hires employed in the post-policy period.[12]

For our sample city, we have access to complete HPF data of more than 215.3 million monthly contribution records between January 2010 and December 2014, encompassing about 5.9 million employees from more than 111,000 employers. Theoretically, this dataset covers the labor-force

[10] See Tang and Coulson (2017) for more institutional details on the HPF system.
[11] Theoretically, the monthly salary covers all payments from the employer to the employee, including base pay, overtime pay, subsidy, and bonus.
[12] As discussed above, we can infer the salary of new hires as the monthly contribution amount divided by 24%.

conditions of all employers in the city, including both the public sector and firms from various industries and ownership types. The detailed contribution records allow us to trace the change in each employer's new hires and job leavers, in particular, and to impute the salary for each employee working for the employer.[13] We also have information on each employee's age, birthplace, and gender, as well as the employer's size.

Raw data are cleaned via the following procedures. First, we drop employees with salaries beyond the reasonable range so we could obtain a reliable inferred salary. Specifically, according to the rules of the HPF system, the base salary should be no more than three times the average salary in the city and no less than the minimum wage standard set by the local government. We drop approximately 21.5% (16.8%) of employee-month-level observations (new hires) because of the unreasonable salary range. Second, we only include employees who are no older than 50 years of age during the sample period, because some female employees may retire when they are 50 years old in China. Appendix Table A2 reports the sample size after each of our sample-processing procedures. After the data-cleaning process, approximately 150.7 million (or 70% of) employee-month-level observations (4.4 million new hires) for nearly 100,000 employers remain.[14]

To comprehensively examine the impact of fertility relaxation on female labor market outcomes, we construct three samples based on the administrative dataset. First, in the New Hire Sample, we aggregate the employee-month dataset to the employer-month level by gender[15] and calculate the

[13] Based on the HPF data of monthly contribution records, we define the observation as a new hire if she joins a new employer and as a job leaver if she resigns from her current employer. Because we have access to the information from January 2009–December 2014, we can infer the new hires between January 2010 and December 2014 and the job leavers between January 2010 and November 2014. Note we cannot identify the job leavers in December 2014, because we have no information on the employment of the employees in 2015.

[14] The ratio of the final sample size to the original sample size might be relatively low. For instance, the number of workers is reduced by 63%, and the number of establishments is reduced by 84% in Bayard et al. (2003), who also explore an employer-employee administrative matched dataset.

[15] We also verify the findings based on a quarter-level or half year-level dataset. The results are available upon request.

number of new hires (normalized by employer size) for each employer. Second, in the Job Leaver Sample, similar to the New Hire Sample, we compute the number of job leavers (normalized by employer size) in each employer-month bin and conduct panel data at the employer-month level by gender. Lastly, we identify the new hires in the administrative dataset and define those records as the Salary Sample. Each observation in this pooled sample represents a new hire.

Table 2 provides the summary statistics of the three samples. On average, the new hires and job leavers account for 6.4% and 4.7% of the employer size each month, respectively. Among those new hires, the average salary is 3,780 yuan per month, which is lower than the average salary of all employees (ranging from 4,201 to 6,463 yuan per month) between 2010 and 2014, calculated by the local Human Resources and Social Security Bureau. As for the employee attributes, 46.7% of new hires are female, the average age is 29.0 years old, and 34.0% of new hires are local residents. The new hires are employed by employers who have 1,192 employees on average.

B. Identification Strategy

In this study, we adopt a difference-in-differences (DID) design to analyze the impact of fertility relaxation on the salary of female new hires based on the relaxation of China's OCP in November 2013.[16] As described in Section II.B, this policy change not only extends the two-children permission from couples with both parents as only children to those with only one parent as an only child, but also was publicly perceived as an official signal of further relaxation of OCP.[17] One

[16] To the best of our knowledge, no confounding event occured may have affected males and females differently in November 2013.

[17] We do not choose to adopt the overall abolishment of OCP in November 2015 as the policy shock for two reasons. First, as described in Section III.A, our sample only covers 2010–2014. More importantly, because the policy change in November 2013 was officially interpreted as a first step of OCP reform, we can reasonably expect that employers should have been anticipating the OCP abolishment since then. Because we focus on the *immediate* effect of OCP relaxation on female labor market outcomes, focusing on the first signal associated with the policy change in November 2013 is more appropriate.

may be concerned that fertility relaxation had been discussed in China before the policy announcement; therefore, the policy is not an unexpected exogenous shock. To address this concern, we obtain the Baidu Index, which provides information on the search volume of a keyword via Baidu. The Baidu Index, similar to Google Trends, has been widely used in the economics literature, such as Fisman et al. (2021) and Qin and Zhu (2018). Figure 1 demonstrates the weekly average search volume of "two-child" (*er hai* in Chinese) via Baidu, where the vertical line denotes the policy announcement. As shown in Figure 1, the search volume remained low and stable before the policy announcement, implying the relaxation of OCP in November 2013 is likely to be an unexpected exogenous shock. In addition, the search volume rises rapidly after the policy announcement. This finding suggests the policy attracted enough attention from the public as well as the employers, and thus, the employers can respond to the policy quickly.

To explore the policy impact, we define females as the treatment group and males as the control group. As discussed in Section 2.2, the relaxation of OCP was announced in November 2013 and implemented in our sample city in February 2014. Therefore, we define the months after November 2013 as the post-period. Following the identification strategy, we have

$$(2) \quad Y_{ijym} = \beta \times FEMALE_i \times POST_{ym} + \gamma_{jym} + \theta \times X_{iym} + \epsilon_{ijym},$$

where Y_{ijym} refers to the salary of new hire i in employer j in month m in year y; $FEMALE_i$ is a dummy variable for the treatment group; $POST_{ym}$ is a dummy variable for the post-period (November 2013 to December 2014); γ_{jym} represents employer-year-month fixed effects to flexibly control for the hiring trend and seasonality of each employer; X_{iym} is demographic characteristics, including gender, age, and birthplace at the county level; ϵ_{ijym} is the error term. The standard errors are clustered at the employer level. We would expect β to be negative if the fertility-relaxation

policy would widen the gender wage gap after the policy change by lowering female new-hire salaries.

To verify the parallel-trends assumption of the DID specification, we conduct an event study to analyze the dynamic effect of the policy change. Using the first quarter of 2010 as the baseline, we have

$$(3) \quad Y_{ijym} = \sum \beta_k \times FEMALE_i \times 1\{y,m \in k\} + \gamma_{jym} + \theta \times X_{iym} + \epsilon_{ijym},$$

where the variables are the same as Equation 2; k ranges from 2010 Q2–2014 Q4. The parameter of interest is β_k, which refers to the dynamic impact of fertility relaxation on labor market outcomes.

In addition, we also study the impact on two other labor market outcomes in the data: the number of new hires and the number of job leavers of each employer each month. Note we aggregate the employee-month dataset to the employer-month level by gender. We can adopt a specification similar to Equation 2. That is, we compare the number of female new hires and job leavers (treatment group) relative to the number of male new hires and job leavers (control group) before and after the relaxation of OCP. We can also examine the parallel-trends assumption using a specification similar to Equation 3.

IV. Results

A. Policy Impact on New Hires and Job Leavers

Before estimating the impact of fertility relaxation on the gender wage gap, we first study the policy impact on new hires and job leavers to provide a more thorough understanding of the labor market effect of fertility relaxation.

Table 3 shows the primary DID results for new hires and job leavers based on the New Hire Sample and Job Leaver Sample, respectively. As introduced in Section III, analysis is conducted at the employer-month level. For each outcome, we have four specifications. In Column 1, we include no control variable. Starting from Column 2, we add employer fixed effects, year-month fixed effects, and employer-year-month fixed effects one at a time. The specification is most complete in Column 4, presented in Equation 2. Panel A documents the impact of the policy change on the number of new hires. The coefficients of the DID term are significantly negative at the 1% level in all columns, and the magnitudes remain unchanged from Column 1 to Column 4. The results imply the number of female new hires declined significantly (relative to male new hires) after the policy. Using the pre-policy average of the outcome variable (0.065, the average monthly number of female new hires normalized by employer size before the policy), the estimation suggests the relaxation of OCP causes a 4.8% reduction in the number of female new hires. In Panel B, we present the impact of fertility relaxation on the probability of females leaving a job. Again, the DID coefficients are significantly negative in all columns and remain stable. Compared with the pre-policy average of the dependent variable, Panel B suggests female job leavers are reduced by 2.6% relative to males after the policy change.

To verify the parallel-trends assumption of the DID specification, we conduct an event study to analyze the dynamic effect on female labor market outcomes using the specification in Equation 3. Figure 2 presents the event study of the two outcome variables, where the first quarter of 2010 is the baseline period. For both outcome variables, most of the coefficients prior to the policy shock (2013 Q4) are not significantly different from zero. However, the coefficients for new hires (Panel A) and job leavers (Panel B) become significantly negative after the policy change. Overall, these graphs verify the parallel-trends assumption of the DID research design.

B. Policy Impact on the Salary of New Hire

We then examine the policy impact on the gender wage gap using the same DID design. Before moving to the regression analysis, Figure 3 presents the raw trend of the salary of new hires by gender and quarter. From this figure, we determine that new-hire salaries exhibit an upward trend over time. Note that in our results, male new hires have higher salaries than female new hires, on average, which is consistent with the conclusion in the gender-wage-gap literature that males earn higher salaries than females on average. Reassuringly, the salary time series for females and males exhibit parallel trends before the policy change (shown as a dashed vertical line) without any regression adjustment. However, female salaries seem to trend down relative to males in the post-policy period, which leads to a widening gender wage gap.

Now, we move on to the DID regression results reported in Table 4 using different specifications. It presents the impact of fertility relaxation on the salary of female new hires. In Column 1, we include no control variable. Starting from Column 2, we add employer fixed effects, year-month fixed effects, employer-year-month fixed effects, and demographic attributes, including age and birthplace, one at a time. The specification is most complete in Column 5, which is presented in Equation 2. As shown in Table 4, the coefficients of the DID term are significantly negative at the 1% level in all columns, and the magnitudes remain largely unchanged from Column 2 to Column 5. With the most complete specification in Column 5, the estimate shows the salary of female new hires reduces by approximately 2.7% after the relaxation of OCP, relative to the salary of male new hires. How much does this magnitude contribute to the gender wage gap in our sample? Because the gender wage gap in our data was around 5.8% (the *Female* coefficient in Column 5) before the policy shock, we calculate that such an effect represents an approximately 47%

(2.7%/5.8%) increase in the gender wage gap, which is significant in terms of its economic magnitude.

Similar to Figure 2, we also examine the dynamic effect of fertility relaxation on female new-hire salaries, using the specification in Equation 3 to verify the parallel-trends assumption. Figure 4 presents the results where the first quarter of 2010 is the baseline period. Again, almost all of the coefficients prior to the policy shock (2013 Q4) are not significantly different from zero. However, the coefficients become negative and significant immediately when the policy was announced and remain negative and significant after the policy change. This observation supports our identification strategy.

Furthermore, we provide evidence that the lower salary of female new hires after the policy change is related to concern about the increasing fertility of females. To do so, we explore the heterogeneity of the impact of fertility relaxation on the salary of female new hires by age cohort. The relaxation of fertility restrictions targets the increasing fertility of moving from one child to two children. Therefore, age cohorts that have the potential for the birth of a second child should be more affected by the policy. We divide the new hires in the sample into six age cohorts. In Columns 1–6 of Table 5, we show the salary reduction of female new hires is most significant for age cohorts below 35. As a validity check and placebo test, the impact on women ages 46–50, who are unlikely to give birth, is positive and insignificant.[18] Moreover, we formally conduct a DDD design by utilizing the older men and women (i.e., ages 46–50) as an additional control group. As shown in Column 7 of Table 5, the DDD term is significantly negative, while the DID term is

[18] Figure A1 plots the event-study results corresponding to Columns 4–6 of Table 5 using the specification in Equation 3. Overall, the differences between the males and females remain small and stable before and after the fertility relaxation, which strengthens the validity of our story.

economically and statistically insignificant, consistent with the heterogeneity by age cohort. To summarize, the reduction in female salaries is the largest for younger female cohorts.

In addition, we show married female employees are more likely to be affected based on a subsample. Specifically, we identify married employees from a subsample of 153,488 HPF mortgage loan applicants between 2006 and 2014 (where we have access to their marriage information) and regard others without any marriage information as unmarried employees. Note the observations identified in the "unmarried" sample here may actually contain some married employees. To minimize the probability of counting married employees as unmarried in the rest of the sample, we restrict the employees in both subsamples to those younger than 30 years old. As shown in Table 6, the salary reduction of married new hires (12.01%) is larger than that of unmarried new hires (2.95%). Overall, the results suggest the main findings are related to the concern about the increasing fertility of females.

C. Robustness Checks

In this section, we conduct a series of robustness checks to ensure the internal validity of our results. First and most importantly, one may be concerned that the males are not a perfect control group, because fertility relaxation may affect them as well. On the one hand, similar to the females, children may affect male employees because the males need to take care of the kids as well, although perhaps to a lesser extent than females. On the other hand, males and females are substitutable in the labor market. Suppose the labor supply for female employees is lower after the policy change. In that case, employers can employ more males to substitute for females. In other words, we observe a decline in the salaries of female new hires *relative to* male new hires after the policy change. Our results may be driven by an increase in the salaries of male new hires after the

policy change. All in all, the males may also be affected by the policy shock and thus violate the assumption of DID design. As discussed in Section IV.B, the salary reduction primarily occurs in the younger female cohorts, and the policy has little impact on female new hires above 35, who are unlikely to give birth. To some extent, the heterogeneous effect by age cohort and the DDD analysis could serve as a placebo test and mitigate this concern.

Second, another major concern is that the new hires are not comparable before and after the fertility relaxation. For instance, if female new hires in the post-policy period have lower abilities than those in the pre-policy period, the lower salary in the post-policy period may reflect a quality change in the female labor supply (i.e., composition change). Note 48.9% of new-hire records are repeated individuals in the sample. To address this concern, we can control for the labor-quality change by exploring the variations within the employee.[19] Adding employee fixed effects in Column 5 of Table 4, we study the effect of a policy shock in Column 1 of Table 7. We also verify the results by restricting the sample to the repeated new hires in Columns 2 and 3. Specifically, we replicate the main result in Column 2 and further add the employee fixed effects in Column 3. All of the coefficients of the DID term remain significantly negative,[20] suggesting females experience a salary decline after the policy change relative to males, a decline free from composition change.

Third, we explore the potential impact of differential linear pre-trends between females and males based on the procedure proposed by Roth (2022). The results are shown in Table A3. Taking the new hire's salary as an example, we first compute the slope against which pre-tests have 50% power and find we could detect a positive linear trend of a magnitude of 0.0004 in the event-study

[19] Some may be concerned the employee fixed effects may change from learning by doing the last job, which may bias the estimation. However, in that case, our estimates would be lower bound because the learning by doing would increase rather than decrease the salary of new hires.
[20] The magnitudes are not comparable to the estimates in the main results, because of sample selection.

analysis. If the pre-trend exists, it will create a bias of at most -0.0033 percentage points in the last quarter. Our actual estimate in the last quarter is -0.0148, which is substantially larger than the potential bias. Similar to the new-hire salary, the potential biases are also significantly lower than the actual estimates for the number of new hires and job leavers. Therefore, the analysis suggests our main results are not likely to be driven by pre-trends.

Finally, some may be concerned our results are driven by the fourth quarter every year instead of the policy shock. We exclude the observations in the fourth quarter of each year and repeat the main regression. Appendix Table A4 presents the regression results, which are consistent with the primary results.

D. Mechanism

In sum, the relaxation of OCP would reduce the salary of female new hires and widen the gender wage gap. Meanwhile, fertility relaxation would also decrease both female new hires and job leavers compared with males. A natural question is how to understand the above empirical results. In theory, the relaxation of OCP would affect the labor market from the perspectives of both demand-side factors (employer) and supply-side factors (employee).

On the one hand, given that the maternity leave was 98 days and the paternity leave was 15 days[21] during the sample period, the (perceived) increasing childbirth would increase the labor cost and thus have a negative impact on the employers of female employees, compared with the employers of male employees. The increasing childbirth could finally result in a decreasing demand for female labor in the labor market. We have shown the number of female new hires and their salaries

[21] The birth allowance and medical treatment for childbirth are paid by birth insurance for the insured and by employers for the uninsured (State Council 2012).

are lower than those of male new hires after the policy shock. The findings are consistent with the story that the demand for female labor is reduced after the fertility-relaxation policy. Additionally, one possible and credible explanation for the negative impact on job leavers is that females recognize the decreasing labor market demand after the policy change and are thus less willing to quit their jobs.

On the other hand, fertility relaxation may also reduce the female labor supply in preparation for having more children (Wu, 2022).[22] Following Barham, Kuhn, and Turner (2023), Guo et al. (2018), Li et al. (2020), and Isaac (2023), we measure the labor supply in both extensive (i.e., labor-force participation) and intensive margins (i.e., working hours). Although we have no information on labor-force participation and working hours in this dataset, we analyze the impact of fertility relaxation on labor supply using a panel dataset from CFPS for 2010, 2012, and 2014 to test this hypothesis.[23] These data have rich information about employment and working hours at the national representative level. We compare the labor supply of females (treatment group) relative to males (control group) before and after the relaxation of OCP. Following the identification strategy, we have

$$(4) \quad Y_{iy} = \beta_1 \times FEMALE_i \times POST_y + \beta_2 \times X_{iy} + \alpha_i + \delta_y + \epsilon_{iy},$$

where Y_{iy} refers to the labor-force participation and daily working hours of individual i in year y, respectively;[24] $FEMALE_i$ is a dummy variable for the treatment group; $POST_y$ is a dummy variable

[22] Theoretically, females may reduce labor supply both quantitatively (labor-force participation and working hours) and qualitatively (e.g., effort). The reduction in working effort may also lead to lower salaries; however, to the best of our knowledge, we have no information on the working effort in the HPF dataset and CFPS data. Therefore, empirically studying the impact of fertility relaxation on working effort is challenging and awaits future research. We thank the reviewer for pointing out this issue.

[23] The attrition rate was 21.4% from 2010–2012 and 15.6% from 2012–2014.

[24] The questions about working hours are different across waves. In 2010 and 2012, the questions are "*How many days did you work each month during the past year?*" and "*How many hours did you work each day during the past year?*"

for the post period; X_{iy} represents the demographic characteristics including marriage, children, and age. We also control for individual fixed effects, α_i, and year fixed effects, δ_y, and ϵ_{iy} is the error term. The standard errors are clustered at the individual level. We require individuals working in urban areas to be ages 23–35 in 2013. The income and daily working hours are winsorized at the 1% and 99% levels. In the end, the balanced panel dataset contains 1,397 individuals in three waves, that is, two waves before the policy shock and one wave after the policy change. Table A5 provides the summary statistics. On average, 54% of individuals are female; the yearly income is about 27,826 yuan; 77% of the individuals are employed; and the employees work 5.39 hours on average every day.

Table 8 presents the regression results. We first replicate the main findings using the CFPS dataset in Columns 1 and 2 and then show the results on labor supply in Columns 3–6. The outcome variables are salary in natural log in Columns 1 and 2, a dummy variable indicating whether the individual is employed in Columns 3 and 4, and daily working hours in natural log in Columns 5 and 6. For each specification, we control for the year fixed effects and individual fixed effects. We further control for age, a dummy variable for marriage, and a dummy variable for children in Columns 2, 4, and 6. The results show the salary of females is reduced by about 5.52% compared with males, consistent with our main results. Note the effects are economically significant but statistically insignificant, most likely because of the small sample size. More importantly, as shown in Columns 3–6, the labor-force participation remains unchanged after the policy shock, while the daily working hours increase by 3.28%. Again, the effect on daily working hours is economically significant but statistically insignificant due to the small sample size; therefore, we

In 2014, the question is "*How many hours did you work each week during the past year?*" Accordingly, we calculate the daily working hours each day (regardless of holidays or normal working days). Note the transformation does not vary across gender, and thus would not bias our coefficient of DID term.

cannot rule out the possibility that the relaxation of OCP would not significantly affect labor supply. Overall, the empirical findings may result from the decrease in the labor demand of females as well as the change in labor supply.

V. Conclusion

In this study, we investigate the immediate consequences of the relaxation of OCP on the gender wage gap. Using an employer-employee matched administrative dataset from a major city in China, we show that after the policy shock, the salary of female new hires is reduced by 2.7% relative to the salary of male new hires, which leads to a 47% gender wage gap increase in our sample. The effect is significant for females under the age of 35, who might be the most likely to have a second child after the policy change, suggesting this salary reduction is related to concerns about increasing female fertility. We verify the results using an alternative identification strategy and address the concerns about composition change. Moreover, we provide evidence that both labor demand and supply may contribute to the salary reduction of female new hires.

Whereas our dataset covers only the formal economy, some may be concerned that the existence of the informal economy would bias our results, considering the sizable informal employment in China (Huang 2009; ILO 2012). First, some low-quality females may switch from the formal economy to the informal economy after the policy shock, which is another form of the unintended and negative consequence of the relaxation of fertility restriction. Second, the informal economy is less likely to be regulated by the government and protected by the law (Portes and Haller 2010). As such, the impact of the relaxation of fertility restriction on female new-hire salaries would be larger in the informal economy.

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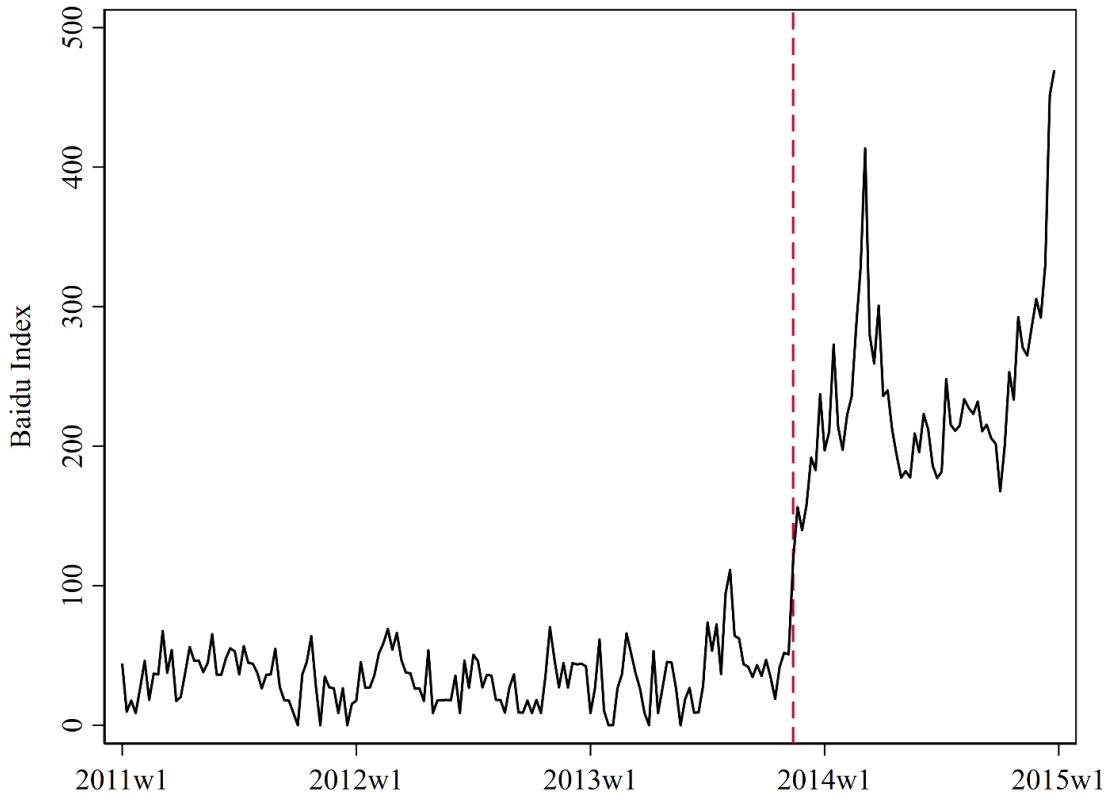
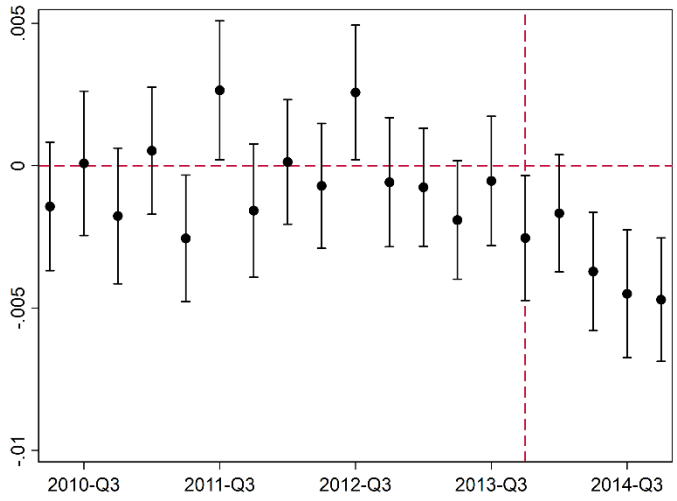
Figures and Tables

Figure 1. The Baidu Index of "Two Child"

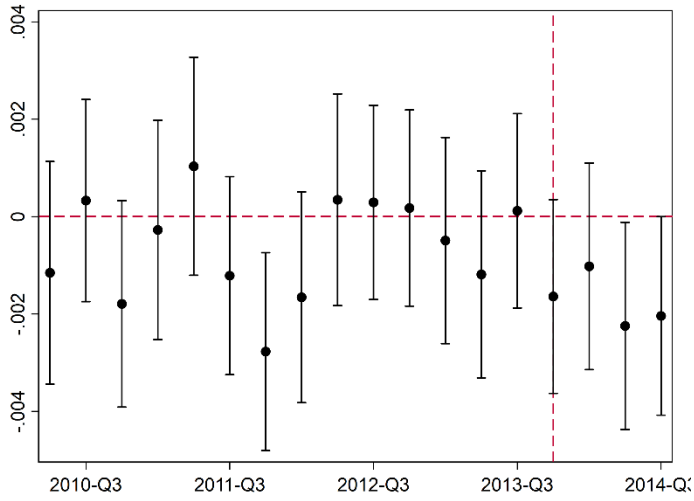


Note: This figure shows the weekly average search volume of "two-child" (*er hai* in Chinese) via Baidu. The vertical line denotes the announcement of the policy.

Figure 2. Event Study: Impact of fertility relaxation on female labor market outcomes



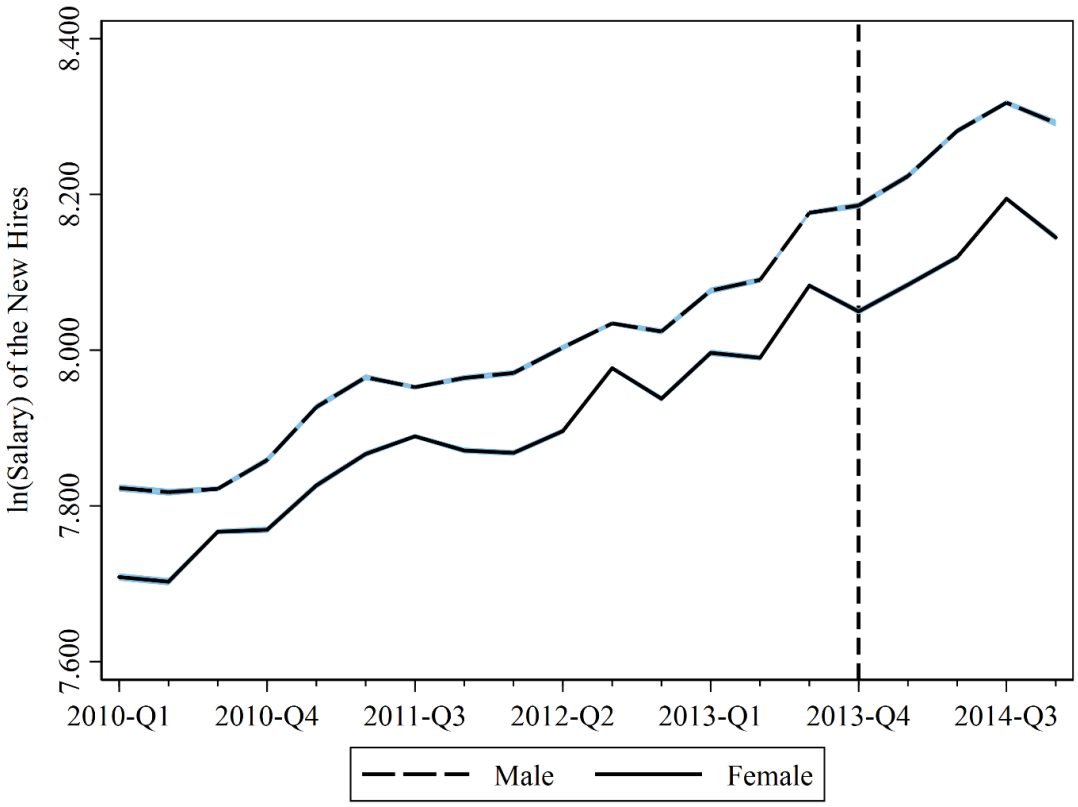
Panel A. New Hire



Panel B. Job Leaver

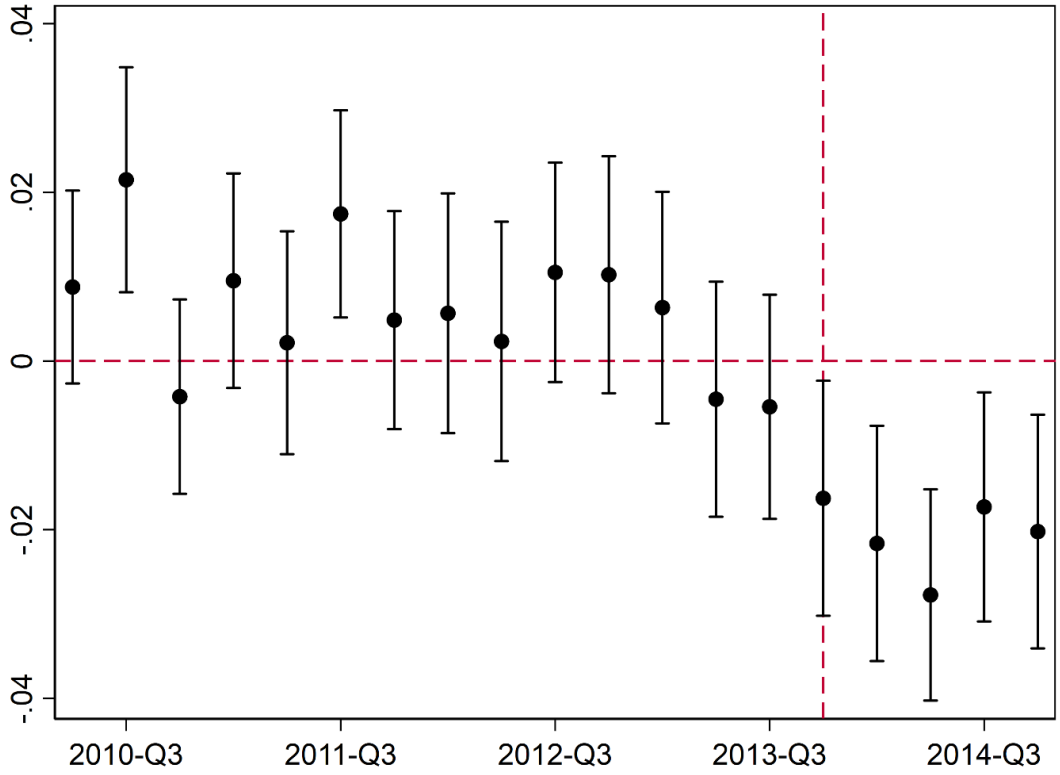
Note: This figure explores the dynamic effect of the relaxation of OCP on the number of female new hires and job leavers. The sample period is from 2010–2014, and the first quarter of 2010 is taken as the baseline period. In each panel, we control for employer-year-month fixed effects and gender. Robust standard errors are clustered at the employer level. The bars refer to the 95% confidence intervals.

Figure 3. Distribution of New Hire Salary: By gender and by different periods



Note: This figure shows the quarterly average salary of the new hires with a 95% confidence interval by gender. The vertical line denotes the policy announcement, and the shadow area refers to the 95% confidence interval.

Figure 4. Event Study: Impact of fertility relaxation on the salary of female new hires



Note: This figure explores the dynamic effect of the relaxation of OCP on the salary of female new hires. The sample period is from 2010–2014, and the first quarter of 2010 is taken as the baseline period. The outcome is the salary (in the natural log) of the new hires. We control for employer-year-month fixed effects and demographic characteristics, including gender, age, and birthplace. Robust standard errors are clustered at the employer level. The bars refer to the 95% confidence intervals.

Table 1. Impact of Fertility Relaxation on Fertility Rate

	(1)	(2)	(3)
		Fertility Rate	
"1.5-Child" Policy	-0.0006		
	(0.0008)		
One-Child Policy	-0.0018		
	(0.0012)		
Fine	-0.0013**		
	(0.0005)		
"1.5-Child" Policy × Fine	0.0014**		
	(0.0007)		
One-Child Policy × Fine	0.0016**		
	(0.0007)		
Post	0.0016**	0.0016**	
	(0.0007)	(0.0007)	
"1.5-Child" Policy × Post	-0.0004	-0.0005	-0.0005
	(0.0009)	(0.0009)	(0.0009)
One-Child Policy × Post	-0.0007	-0.0007	-0.0007
	(0.0010)	(0.0010)	(0.0010)
Fine × Post	-0.0006	-0.0006	-0.0006
	(0.0004)	(0.0004)	(0.0004)
"1.5-Child" Policy × Fine × Post	0.0003	0.0003	0.0003
	(0.0006)	(0.0006)	(0.0006)
One-Child Policy × Fine × Post	0.0011**	0.0011**	0.0011**
	(0.0004)	(0.0004)	(0.0004)
Observations	349	349	349
R-squared	0.0850	0.6663	0.7083
City FE	NO	YES	YES
Year FE	NO	NO	YES

Note: This table explores the effect of the relaxation of OCP on the fertility rate. The sample covers 35 major Chinese cities from 2006–2015. We control for city fixed effects in Column 2, and city fixed effects and year fixed effects in Column 3. Robust standard errors are clustered at the city level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table 2. Summary Statistics: Administrative dataset

	Variables	Obs	Mean	Std. Dev.	Min	Max
New Hire	New Hire	2,535,988	0.0638	0.1724	0	1
Sample	# of employer	104,942				
Job Leaver	Job Leaver	2,372,694	0.0470	0.1458	0	1
Sample	# of employer	101,484				
	Salary	4,427,392	3,780.3740	2,946.7370	800	17,375
	Female	4,427,392	0.4665	0.4989	0	1
Salary	Age	4,427,392	29.0440	6.0444	22	50
Sample	Local	4,427,392	0.3395	0.4736	0	1
	Employer Size	4,427,392	1,191.9680	3,177.8400	1	24,672
	# of employer	95,576				

Note: This table reports the summary statistics of the administrative datasets. The sample period is from 2010–2014.

Table 3. Impact of Fertility Relaxation on New Hires and Job Leavers

	(1)	(2)	(3)	(4)
Panel A		New Hire		
Female	0.0006**	0.0006**	0.0006**	0.0006**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Post	-0.0013***	-0.0455***		
	(0.0004)	(0.0003)		
Female × Post	-0.0031***	-0.0031***	-0.0031***	-0.0031***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Observations	2,535,988	2,535,988	2,535,988	2,535,988
R-squared	0.0001	0.1742	0.1833	0.6579
Pre-policy Average	0.0650	0.0650	0.0650	0.0650
Relative Effect	-0.0477	-0.0477	-0.0477	-0.0477
Panel B		Job Leaver		
Female	0.0031***	0.0031***	0.0031***	0.0031***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Post	0.0074***	0.0144***		
	(0.0003)	(0.0003)		
Female × Post	-0.0012***	-0.0012***	-0.0012***	-0.0012***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Observations	2,372,694	2,372,694	2,372,694	2,372,694
R-squared	0.0005	0.1528	0.1551	0.6322
Pre-policy Average	0.0468	0.0468	0.0468	0.0468
Relative Effect	-0.0256	-0.0256	-0.0256	-0.0256
Employer FE	NO	YES	YES	NO
Year-month FE	NO	NO	YES	NO
Employer-year-month FE	NO	NO	NO	YES

Note: This table explores the effect of the relaxation of OCP on the number of female new hires and job leavers. The sample period is from 2010–2014. The dataset is aggregated to the employer-month level by gender. The outcome is the number of new hires (normalized by employer size) in Panel A and the number of job leavers (normalized by employer size) in Panel B. We control for employer fixed effects in Column 2, employer fixed effects and year-month fixed effects in Column 3, and employer-year-month fixed effects in Column 4. Robust standard errors are clustered at the employer level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table 4. Impact of Fertility Relaxation on the Salary of New Hires

	(1)	(2)	(3)	(4)	(5)
			ln(Salary)		
Female	-0.0870***	-0.0729***	-0.0725***	-0.0708***	-0.0578***
	(0.0056)	(0.0027)	(0.0027)	(0.0028)	(0.0026)
Post	0.2865***	0.2399***			
	(0.0086)	(0.0071)			
Female × Post	-0.0548***	-0.0258***	-0.0257***	-0.0254***	-0.0274***
	(0.0056)	(0.0029)	(0.0029)	(0.0030)	(0.0030)
Observations	4,427,392	4,427,392	4,427,392	4,427,392	4,427,392
R-squared	0.0374	0.5368	0.5539	0.7187	0.7500
Employer FE	NO	YES	YES	NO	NO
Year-month FE	NO	NO	YES	NO	NO
Employer-year-month FE	NO	NO	NO	YES	YES
Demographic attributes	NO	NO	NO	NO	YES

Note: This table explores the effect of the relaxation of OCP on the salary of female new hires. The sample period is from 2010–2014. The outcome is the salary (in the natural log) of the new hires. We control for employer fixed effects in Column 2, employer fixed effects and year-month fixed effects in Column 3, employer-year-month fixed effects in Column 4, and employer-year-month fixed effects and demographic characteristics, including age and birthplace, in Column 5. Robust standard errors are clustered at the employer level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table 5. Heterogeneous Effect by Age Cohort

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age Cohort	22-25	26-30	31-35	36-40	41-45	46-50	All
				ln(Salary)			
Female	-0.0236***	-0.0646***	-0.0899***	-0.0988***	-0.0843***	-0.0580***	-0.0576***
	(0.0025)	(0.0035)	(0.0062)	(0.0080)	(0.0077)	(0.0075)	(0.0065)
Female × Post	-0.0261***	-0.0248***	-0.0169***	-0.0107	-0.0087	0.0114	-0.0086
	(0.0039)	(0.0040)	(0.0047)	(0.0074)	(0.0089)	(0.0113)	(0.0085)
Female × Young							-0.0003
							(0.0061)
Young × Post							0.0021
							(0.0065)
Female × Young× Post							-0.0191**
							(0.0082)
Observations	1,475,124	1,606,412	666,060	285,668	193,399	117,397	4,427,392
R-squared	0.7928	0.7742	0.8123	0.8624	0.8886	0.9013	0.7500
Employer-year-month FE	YES	YES	YES	YES	YES	YES	YES
Demographic attributes	YES	YES	YES	YES	YES	YES	YES

Note: This table explores the effect of the relaxation of OCP on the salary of female new hires by age cohort. The sample period is from 2010–2014. The outcome is the salary (in the natural log) of the new hires. For each specification, we control for employer-year-month fixed effects and demographic characteristics, including age and birthplace. Robust standard errors are clustered at the employer level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table 6. Heterogeneous Effect by Marital Status

Marital	(1) Married	(2) Unmarried
	ln(Salary)	
Female	-0.1054***	-0.0442***
	(0.0085)	(0.0025)
Female × Post	-0.1201***	-0.0295***
	(0.0325)	(0.0033)
Observations	77,846	3,050,408
R-squared	0.9145	0.7652
Employer-year-month FE	YES	YES
Demographic attributes	YES	YES

Note: This table explores the effect of the relaxation of OCP on the salary of female new hires by employee's marital status. The sample period is from 2010–2014. To ensure the married and unmarried sample is comparable, we restrict the sample to ages 22–30. The outcome is the salary (in the natural log) of the new hires. For each specification, we control for employer-year-month fixed effects and demographic characteristics, including age and birthplace. Robust standard errors are clustered at the employer level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table 7. Robustness Check: Labor-quality change

	(1)	(2)	(3)
Sample	All	Repeated New Hires	
	ln(Salary)		
Female		-0.0670***	
		(0.0031)	
Female × Post	-0.0325***	-0.0352***	-0.0325***
	(0.0033)	(0.0037)	(0.0023)
Observations	4,427,392	2,163,184	2,163,184
R-squared	0.9707	0.7556	0.9432
Employee FE	YES	NO	YES
Employer-year-month FE	YES	YES	YES
Demographic attributes	YES	YES	YES

Note: This table explores the effect of the relaxation of OCP on the salary of female new hires when controlling for the labor-quality change. The sample period is from 2010–2014. The sample is restricted to the repeated new hires in Columns 2 and 3. The outcome is the salary (in the natural log) of the new hires. For each specification, we control for employer-year-month fixed effects and demographic characteristics, including age and birthplace. We further control for employee fixed effects in Columns 1 and 3. Robust standard errors are clustered at the employer level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table 8. Impact of Fertility Relaxation on Labor Supply

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Income)		Employed		ln(Working Hours)	
Female × Post	-0.0533	-0.0552	0.0076	0.0017	0.0432	0.0328
	(0.0631)	(0.0632)	(0.0211)	(0.0211)	(0.0397)	(0.0397)
Observations	2,847	2,847	4,191	4,191	3,857	3,857
R-squared	0.7688	0.7690	0.5325	0.5358	0.5254	0.5275
Demographic	NO	YES	NO	YES	NO	YES
Individual FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Note: This table uses the CFPS survey dataset to explore the effect of the relaxation of OCP on females' labor supply. The individual-year-level balanced panel data involve three waves: 2010, 2012, and 2014. We require that the individuals work in urban areas and are ages 23–35 in 2013. The outcome variables are salary in natural log in Columns 1 and 2, a dummy variable indicating whether the individual is employed in Columns 3 and 4, and daily working hours in natural log in Columns 5 and 6. The income and daily working hours are winsorized at the 1% and 99% levels. For each specification, we control for year fixed effects and individual fixed effects. We further control for age, a dummy variable for marriage, and a dummy variable for children in Columns 2, 4, and 6. Robust standard errors are clustered at the household level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

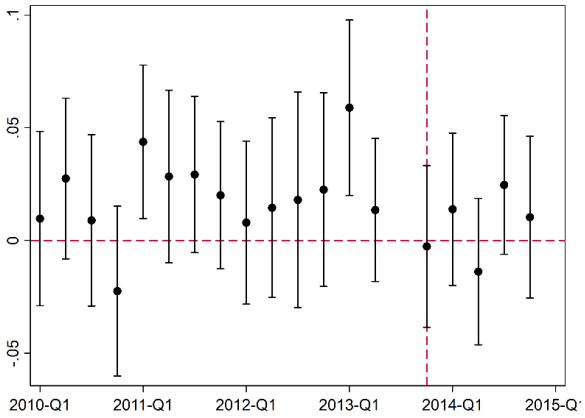
The Impact of Fertility Relaxation on the Gender Wage Gap

Sumit Agarwal, Keyang Li, Yu Qin, Jing Wu

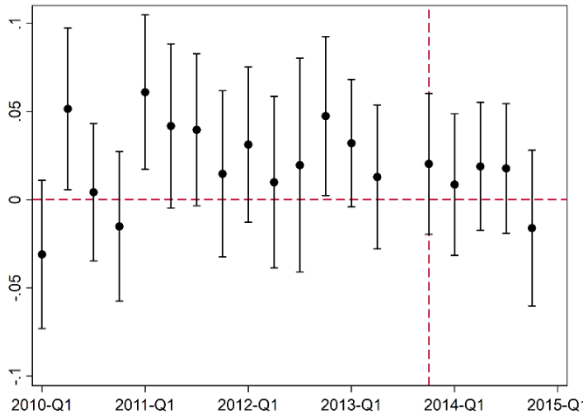
Online Appendix

Appendix A: Additional Figures and Tables

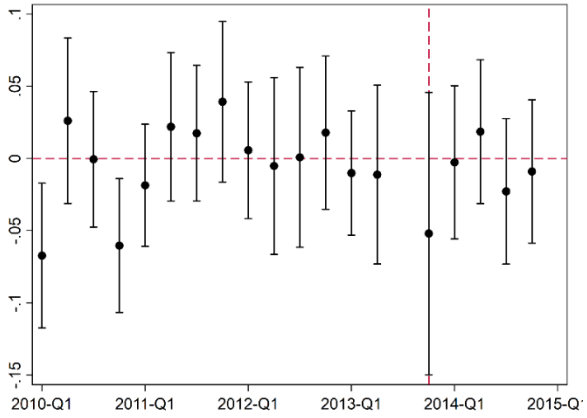
Figure A1. Event Study: Impact of fertility relaxation on the salary of female new hires by age cohort



Panel A. 36–40



Panel B. 41–45



Panel C. 46–50

Note: This figure explores the dynamic effect of the relaxation of OCP on the salary of female new hires by age cohort. The sample period is from 2010–2014, and the third quarter of 2013 is taken as the baseline period. The outcome is the salary (in the natural log) of the new hires. We control for employer-year-month fixed effects and demographic characteristics, including gender, age, and birthplace. Robust standard errors are clustered at the employer level. The bars refer to the 95% confidence intervals.

Table A1. Number of Children by Household Types

	Number of Children			Total
Household Type	0	1	2+	
Both parents were not an only child	310	1,070	680	2,060
One parent was an only child	148	564	223	935
Both parents were an only child	66	222	44	332
Total	524	1,856	947	3,327

Note: This table reports the number of households by household type and number of children. The data come from the CFPS in 2010. We keep the households where the females were younger than 35 years old.

Table A2. Effect of Sample Restrictions on Sample Size

Processing Procedure	Sample Size			# of
	Full Sample	New Hire	Job Leaver	Employers
Raw dataset	215,316,418	5,617,924	5,377,910	111,515
1. Drop employees with a salary beyond the reasonable range	168,994,134	4,673,863	4,264,990	105,478
2. Drop employees more than 50 years old	150,661,883	4,427,392	3,875,619	104,942

Note: This table reports the impact of each sample processing procedure on the monthly raw dataset. The data include all the deposit records in the local Housing Provident Fund system from 2010–2014.

Table A3. Robustness Check: Roth test on the potential effects of pre-trend

Outcome	Slope	Potential Bias in the Last Quarter	Actual Estimate in the Last Quarter
New Hire	0.0001	-0.0006	-0.0042
Job Leaver	0.0001	-0.0005	-0.0022
Salary	0.0004	-0.0033	-0.0148

Note: This table reports the results of the Roth test on the potential effects of the pre-trend. For each outcome variable, this table presents the slope against which pre-tests have 50% power and the potential bias in the last quarter if the pre-trend exists. This table also shows the actual estimate for the last quarter based on the event-study design.

Table A4. Robustness Check: Excluding the fourth quarter

	(1)	(2)	(3)	(4)	(5)
			ln(Salary)		
Female	-0.0858***	-0.0724***	-0.0720***	-0.0701***	-0.0570***
	(0.0057)	(0.0028)	(0.0028)	(0.0029)	(0.0026)
Post	0.2953***	0.2496***			
	(0.0098)	(0.0077)			
Female × Post	-0.0538***	-0.0268***	-0.0267***	-0.0265***	-0.0284***
	(0.0065)	(0.0033)	(0.0033)	(0.0033)	(0.0032)
Observations	3,469,089	3,469,089	3,469,089	3,469,089	3,469,089
R-squared	0.0370	0.5374	0.5549	0.7160	0.7477
Employer FE	NO	YES	YES	NO	NO
Year-month FE	NO	NO	YES	NO	NO
Employer-year-month FE	NO	NO	NO	YES	YES
Demographic attributes	NO	NO	NO	NO	YES

Note: This table explores the effect of the relaxation of OCP on the salary of female new hires based on the dataset, excluding the observations in the fourth quarter of each year. The sample period is from 2010–2014. The outcome is the salary (in the natural log) of the new hires. We control for employer fixed effects in Column 2, employer fixed effects and year-month fixed effects in Column 3, employer-year-month fixed effects in Column 4, and employer-year-month fixed effects and demographic characteristics, including age and birthplace, in Column 5. Robust standard errors are clustered at the employer level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table A5. Summary Statistics: Survey data

	Obs	Mean	Std. Dev.	Min	Max
Income	2,847	27826.2000	25175.3400	300	150000
Employed	4,191	0.7712	0.4201	0	1
Working Hours	3,857	5.3907	2.8644	1	12.1429
Female	4,191	0.5347	0.4989	0	1
Married	4,191	0.7616	0.4261	0	1
Children	4,191	0.6717	0.4697	0	1
Age	4,191	28.8879	3.9350	20	36

Note: This table reports the summary statistics of the CFPS survey dataset. The individual-year-level balanced panel data involve three waves: 2010, 2012, and 2014. We require that the individuals work in urban areas and are ages 23–35 in 2013. The daily working hours are winsorized at the 1% and 99% levels.

Appendix B: Literature Review

Table B1. Literature Summary

Policy	Literature	Targeted Group	Finding
Parental Leave	Asai (2015)	Mothers with children aged 0-1	Cash benefit has little effect on mothers' job continuity.
	Baker and Milligan (2008)	Mothers surrounding the childbirth / Mothers with a child aged 0-1	Job-protected leaves have a positive effect on mothers' job continuity.
	Baum (2003)	Mothers with children aged 0-1 / Women of childbearing age	The state maternity leave legislation has little impact on employment and wages.
	Baum and Ruhm (2016)	Mothers surrounding the childbirth	Maternal leave has a positive effect on mothers' employment, job continuity, and hours and weeks of work.
	Bana, Bedard, and Rossin-Slater (2020)	Mothers after childbirth	Higher benefit during leave has little impact on maternal labor market outcomes. It would increase job continuity.
	Bergemann and Riphahn (2022)	Mothers for up to 42 months after birth	The parental leave reform has no impact on long-run labor force participation but speeds up mothers' labor market return.
	Berger and Waldfogel (2004)	Mothers after childbirth	The mothers with maternity leave return to work more quickly after the maternity leave.
	Bičáková and Kalíšková (2019)	Mothers after childbirth	The job protection period has a positive effect on post-leave unemployment.
	Burgess et al. (2008)	Mothers after childbirth	The parental leave leads to a shorter return time for the mothers after childbirth.
	Dahl et al. (2016)	Mothers after childbirth	The increase in maternal leave has little impact on parental earnings and labor force participation.
	Das and Polachek (2015)	Young California women	The California paid family leave would increase the labor force participation rate, the unemployment rate, and the duration of unemployment for young women.
	Del Rey, Kyriacou, and Silva (2020)	Women aged 15-64	The duration of maternity leave has an inverted U-shaped

		relationship with female labor force participation.
Del Rey, Racionero, and Silva (2021)	Women aged 25-54	The decrease in parental leave entitlements leads to lower gender wage gaps but higher gender employment rate gaps.
Ekberg, Eriksson, and Friebel (2013)	Mothers after childbirth	The increased parental leave incentives for fathers have a positive long-term effect on female earnings and a negative effect on employment rates.
Geyer, Haan, and Wrohlich (2015)	Mothers with young children	Parental leave has a negative effect on female employment in the first year after childbirth and a modest positive impact on labor supply in the second year after childbirth.
Han, Ruhn, and Waldfogel (2009)	Mothers surrounding the childbirth	Parental leave has little impact on maternal employment.
Hanel (2013)	Mothers after childbirth	Maternity leave would delay mothers' return to work and does not affect wages in the long run.
Hanratty and Trzcinski (2009)	Mothers with children aged 0-1	The expansion in paid family leave and transfers would not reduce employment.
Kleven et al. (2020)	Mothers surrounding the childbirth	Parental leave has a negative and small effect on female labor outcomes after childbirth in the short run, while it has little impact in the long run. Parental leave does not affect female labor outcomes before childbirth.
Lalive et al. (2014)	Mothers of newborn children	Longer cash benefits would delay the return to work, while prolonged parental leave has little impact on mothers' labor market outcomes.
Lalive and Zweimüller (2009)	Mothers of newborn children	The increase in the duration of parental leave leads to a decrease in employment and earnings in the short run but has little impact in the long run.
Mullerova (2017)	Mothers with children up to 3 years old	The increase in payment of universal parental benefits has a negative effect on mothers' labor force participation.
Puhani and Sonderhof (2009)	Young women	The parental leave extension would reduce job-related

			training, even if they have no child.
	Rossin-Slater, Ruhm, and Waldfogel (2013)	Mothers with children aged 1-3	The parental leave extension would increase the working hours and wage incomes for employed mothers.
	Schönberg and Ludsteck (2014)	Mothers of newborn children	The expansions in maternity leave coverage lead to lower employment for mothers after childbirth in the short run. It also has a small effect on employment, job continuity, and income in the long run.
Childcare	Andresen and Havnes (2019)	Mothers and fathers with two-year-olds residing	Childcare has a positive effect on mothers' labor supply and has no impact for fathers.
	Baker, Gruber, and Milligan (2008)	Mothers with only children aged 0-4	The employment of women increases because of the universal childcare subsidies.
	Bauernschuster and Schlotter (2015)	Mothers with the youngest child born between 1992 and 2000 / mothers with the youngest child aged 3-4	Public childcare increases maternal employment.
	Berlinski and Galiani (2007)	Mothers with at least one child aged 3-5	The childcare subsidy increases maternal employment.
	Bettendorf, Jongen, and Muller (2015)	Mothers with the youngest child up to 12 years old	The childcare subsidy has a modest and positive effect on maternal employment.
	Blau and Tekin (2007)	Single mothers with at least one child under the age of 13	Childcare subsidy has a positive impact on single mothers' employment.
	Cascio (2009)	Mothers with five-year-old children	Childcare subsidy has a positive impact on single mothers' employment but has no impact on married mothers.
	Connelly (1992)	Married mothers with children under the age of 13	Childcare costs lead to lower women's labor force participation.
	Dang, Hiraga, and Viet Nguyen (2022)	Mothers with at least one child under the age of six	Childcare has no impact on women's labor force participation. However, childcare can increase the total annual wage and the probability of having a wage-earning job.

	Fitzpatrick (2010)	Mothers who live with their own children who were born around the cutoff date for kindergarten eligibility	Universal prekindergarten availability has little impact on maternal labor supply.
	Fitzpatrick (2012)	Mothers with singleton children aged 5	Public school enrollment increases single mothers' labor supply while other mothers' labor supply remains unchanged.
	Gelbach (2002)	Mothers with the youngest child up to five years old	Childcare subsidies increase maternal labor supply.
	Givord and Marbot (2015)	Mothers with the youngest child up to three years old	Childcare subsidies have a modest impact on mothers' labor force participation.
	Goux and Maurin (2010)	Mothers with children under age 3	Early school availability increases mothers' employment.
	Haeck, Lefebvre, and Merrigan (2015)	Mothers with children aged 1-4	Universal preschool policies have positive effects on mothers' labor supply.
	Havnes and Mogstad (2011)	Mothers with the youngest child aged 3-10	Childcare has little effect on maternal employment.
	Herbst (2017)	Mothers with the youngest child aged 0-12	The subsidized and universal childcare program has a positive effect on maternal employment.
	Hojman and Lopez Boo (2022)	Mothers with children aged 0-4	Public childcare can increase mothers' work.
	Kimmel (1998)	Mothers or guardians of children under the age of 13	High childcare prices would lower mothers' employment.
	Kleven et al. (2020)	Mothers surrounding the childbirth	Childcare has no effect on the employment and earnings of females before or after childbirth.
	Lefebvre and Merrigan (2008)	Mothers with at least one child aged 1-5	Childcare subsidies can increase mothers' labor force participation.
	Lefebvre, Merrigan, and Verstraete (2009)	Mothers with at least one child aged 6-11 and no children less than 6	Childcare subsidies have a positive effect on mothers' labor supply.
	Lundin, Mörk, and Öckert (2008)	Married mothers with at least one child aged 1-9	Reduced childcare prices have little effect on the female labor supply.

	Müller and Wrohlich (2020)	Mothers with at least one child aged 1-3	Subsidized childcare can increase mothers' labor market participation.
	Nollenberger and Rodríguez-Planas (2015)	Mothers of 3-year-olds	Full-time public children can increase mothers' employment.
	Österbacka and Räsänen (2021)	Mothers with one or two children	The home care allowance would delay the return to employment.
	Ribar (1995)	Married mothers with at least one child under the age of 15	Childcare prices have a small and negative impact on the labor supply of married mothers.
	Tekin (2006)	Single mothers with at least one child under the age of 13	Childcare subsidies can increase the probability of working at a standard job for single mothers.
	Tekin (2007)	Single mothers with at least one child under the age of 13	Lower childcare prices can increase single mothers' employment.
	Yamaguchi, Asai, and Kambayashi (2018)	Mothers with children under the age of 3.5	Childcare has a positive effect on market participation, working hours, and earnings for most mothers.

Note: This table summarizes the previous studies that examine the impact of fertility policies (parental leave or childcare) on female labor market outcomes.

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