The Impact of Fertility Relaxation on Female Labor Market Outcomes[1]

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Abstract

We explore a 2013 policy shock that relaxed the One-Child Policy in China: couples could have two children under certain circumstances. We show that after the policy shock the salary of female new hires is reduced by 1.2% relative to the salary of male new hires, equivalent to a 22% increase in the gender wage gap in the data. In addition, employers hire 4.4% fewer female employees, and female employees are 4.3% less likely to leave their current jobs. This leads to approximately 1,950 fewer female employees employed and 1,059 leaving their jobs every month in the sample city.

Keywords: One-Child Policy; gender discrimination; gender wage gap; labor market
JEL Codes: J78; J71; J13

[1]We benefitted from the comments of Yvonne Jie Chen, Ting Chen, Yi Chen, Hanming Fang, Jie Gong, Zheng (Michael) Song, and seminar participants at Fudan University, National University of Singapore, Renmin University of China, and ShanghaiTech University.

1 Introduction

With increasing concern about the aging population, many countries have implemented policies to boost birth rates, such as fertility-relaxation policy and extended parental leave, that may encourage females to have more children. However, such policies may widen the gender earnings gap in the labor market.[2] In addition to the human capital differences and occupational and industry differences across gender, motherhood has been discussed as an important factor in explaining the gender earnings gap. It has been documented in recent literature that even though the U.S. gender earnings gap is closing, childbearing responsivities account for a growing share of the remaining gender wage gap (Juhn & McCue, 2017). In addition to the reduced labor supply associated with childbearing responsibilities, another contribution to the "motherhood penalty" is likely to be discrimination if employers perceive mothers to be less productive than non-mothers. This discrimination should be regulated by policymakers; however, as explicit discrimination is illegal in many countries, most discriminatory actions are hidden. These circumstances make identifying discriminatory actions, as well as quantitatively measuring the effect on the gender wage gap, challenging.

In this paper, we study the impact of the relaxation of China's One-Child Policy (OCP, hereafter) on female labor market outcomes. In particular, we have access to an employer-employee matched administrative data that encompasses all of the 5.4 million employees in a major city in China. It allows us to examine the various dimensions of labor market outcomes of each employee at high frequency (monthly or quarterly), including the hiring and leaving of each employee, and, most importantly, the salary of new hires, something that is not easily observed in lab and field experiments testing for labor market discrimination. 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 are also able to have two children if only one parent was an only child. This policy change may signal employers about an anticipated increase in the childbearing burdens of females, and impact labor market outcomes for females, especially for women at reproductive ages.

[2] The gender earnings gap is a well-established fact in the labor economics literature, suggesting that females earn less than males on average (see a literature review in Blau and Kahn (2017)).

We employ a difference-in-differences design to investigate the causal effect of the relaxation of OCP on female labor market outcomes. We use the salary of new hires as the main outcome variable, which is not easily observed in experimental settings.[3] In addition to salary, we also observe two other labor market outcomes in the data: the number of new hires and the number of job-leavers for each employer in each month. We aggregate the employee-month data to the employer-quarter level for regression analysis. Therefore, the outcome variables include the average salary of new hires for each employer-quarter, by gender, and the number of new hires and job-leavers for each employer in each quarter (normalized by employer size), by gender. Our identification variation comes from the difference in each of the three outcomes between female and male new hires, before and after the policy change, *within* each employer. In the most complete setting, we also include employer-by-year fixed effects, employer-by-quarter fixed effects, and female-by-quarter fixed effects to control for employer-specific hiring trends and hiring seasonality, and for gender-specific seasonal trends at the city level.

We show that trends on the three 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 is reduced by 1.2% relative to male new hires, which is equivalent to a 22% increase in the gender wage gap compared with the pre-policy period. As a robustness check, we show that the results are neither driven by male new-hire salary increase after the policy, nor driven by a labor quality or labor effort decline of female employees. We also verify the results using an alternative identification strategy by comparing females who are more likely to have a second child with females who are less likely to have a second child, before and after the policy.

By examining outcomes related to the numbers of new hires and job-leavers, we find that employers hire 4.4% fewer female employees relative to male after the relaxation of the fertility restriction. This decrease is not likely to be driven exclusively by the reduction of female labor supply; otherwise, we should see an increase in female salaries after the policy change. Moreover, we observe that female employees are 4.3% less likely to leave their current jobs in the post-policy period, possibly due to females recognizing increasing labor market discrimination after the policy change and becoming less willing to quit their current jobs.

[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 limitation. We will elaborate on this point in Section 3.

We also document rich heterogeneity on the impact of fertility relaxation on labor market outcomes by employee and employer characteristics. By employees' age cohorts, we find that the effect is the largest for females aged 31-35, those most likely to have a second child in the short term after the policy change. By employer size, we show that large employers and small employers respond to the policy change on different margins: small employers reduce the headcounts of female new hires, while large employers reduce the headcounts of female new hires (but less than small employers) and the salary of female new hires. By employer sector, we show that the effect is the largest in state-owned enterprises (SOEs) compared to private-owned enterprises (POEs) and joint ventures (JVs). Compared to POEs and JVs, SOEs are more likely to be in monopolistic industries with entry barriers, and offering higher salaries than POEs and JVs, as calculated in our sample. This result is consistent with Becker (1971) that discrimination should be stronger in less competitive sectors because competitive forces should reduce or eliminate employer discrimination in the long term. By industry, we show that the effect is primarily from industries that are more "brain" oriented (such as all of the service industries) than "brawn" oriented (such as the manufacturing and construction industries). This result is consistent with the conjecture that discrimination against females is stronger if males can be substituted for females, because "brawn-"oriented industries may have lower substitutability across genders while "brain-"oriented industries may have higher gender substitutability (Olivetti & Petrongolo, 2014; Rendall, 2017).

To the best of our knowledge, this study provides the first causal analysis of the relationship between fertility policies and female labor market outcomes. Our study confirms that while policies designed to boost birth rates may succeed in that goal, they may unintentionally encourage discrimination against female employees of childbearing age. For instance, although maternity leave policies have a positive effect on female labor market outcomes after women give birth (see Rossin-Slater (2017) for a review), long-leave policies have negative or zero effects on female labor market outcomes in the long term (Lalive & Zweimüller, 2009; Lequien, 2012; Schönberg & Ludsteck, 2014). We demonstrate that the gender wage gap is widened by 22% after the relaxation of fertility restrictions, likely due to employer discrimination. Moreover, this effect appears immediately after the policy change, which may precede the birth of a second child for the females in our sample. Therefore, policymakers must devote more effort toward a non-discriminative labor market when implementing pro-fertility policies.

Our study also speaks directly to the literature on gender discrimination in the labor market. Extensive literature uses correspondence studies to identify discrimination. By sending out fictitious resumes with variations on gender but keeping other characteristics the same, callback-rate differences by gender suggest the existence of discrimination.[4] A number of studies use observational data to document gender discrimination in the hiring process in different contexts (Bagues & Esteve-Volart, 2010; Goldin & Rouse, 2000; Helleseter, Kuhn, & Shen, 2018; Kuhn & Shen, 2013; Neumark, Bank, & Van Nort, 1996; among others). Based on administrative datasets, several studies analyze the dynamic impact of childbirth on the gender wage gap (Chen, Zhang, & Zhou, 2018) and on gender inequality in labor market outcomes (Kleven, Landais, & Søgaard, 2019). Our study contributes to the literature in two ways. First, most of the previous works use data from one firm or one experiment, where external validity has always been questioned. This study provides one of the first attempts to introduce universal administrative records of a large city (another example is Kleven, Landais, & Søgaard (2019)), including 5.4 million employees of approximately 100,000 employers from various industries/sectors. Second, correspondence studies can only follow the callback rate, while other dimensions of hiring outcomes, such as salary, are missing (Neumark, 2018). In contrast, this study focuses primarily on the effect of fertility relaxation on new hire salary as well as that effect on the number of new hires and job-leavers.[5]

Our findings are related to discussions on different types of discriminations, in particular, taste-based discrimination versus statistical discrimination (Bertrand & Mullainathan, 2004; Carlsson & Rooth, 2012; Foster & Rosenzweig, 1996; Mobius & Rosenblat, 2006; Zschirnt & Ruedin, 2016). Our results are more consistent with the statistical discrimination that occurs when employers anticipate a productivity decline of females of reproductive age due to the relaxation of fertility restrictions, and, thus, offer lower salaries to female new hires after the policy shock.[6] The advantage of our setting is that the exogenous policy shock gives a (noisy) productivity signal to a specific group of employees. However, we acknowledge that our results cannot rule out taste-based discrimination.

[4] Existing literature using correspondence studies has documented discrimination against young women (Duguet & Petit, 2005; Petit, 2007) and women with childbearing responsibilities (Correll, Benard, & Paik, 2007).

[5] This study is also related to Black and Strahan (2001), who exploit policy shock (banking deregulation) to identify gender discrimination in salary.

[6] Similarly, Agan and Starr (2018) and Doleac and Hansen (2020) document that the "ban the box" policy, which prevents employers from asking about job applicants' criminal records during job applications, would encourage statistical racial discrimination unintentionally.

The rest of this paper is organized as follows. Section 2 discusses the background of fertility policies in China; Section 3 introduces the data source and the data-cleaning procedures; Section 4 outlines the identification strategy; Section 5 presents the main findings; Section 6 provides robustness checks and rules out alternative explanations; Section 7 discusses implications on the gender wage gap and shows heterogeneity analysis; Section 8 concludes.

2 Policy Background

A One-Child Policy in China

After an eight-year voluntary family-planning campaign that begin 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 (Gu, Wang, Guo, & Zhang, 2007; Hesketh, Lu, & 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 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 high monetary penalty, or even by 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 controls reduced 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 allowed to have two or more children. Finally, the literature also points out that governments and SOEs typically imposed stricter OCP requirements on their employees, as compared with private or foreign firms (Cheng, Ma, & Xu, 2016; Gu, Wang, Guo, & Zhang, 2007).

OCP has significantly reduced fertility in China (see Wang (2014) and Zhang (2017) for a review). It also generates profound impacts on other measures. For example, sex ratio has been substantially skewed during the past decades due to sex-selective abortion after OCP emerged as a policy (Chen, Li, & Meng, 2013; Ebenstein, 2010; Tuljapurkar, Li, & Feldman, 1995), which may increase household savings rate (Wei & Zhang, 2011), aggregate savings rate (Bhaskar & Hopkins, 2016; Du & Wei, 2010), housing prices (Wei, Zhang, & Liu, 2017), and even crime rates (Edlund, Li, Yi, & Zhang, 2013) in China through the marriage-market channel.

B Relaxation of One-Child Policy

OCP has effectively reduced the fertility rate and, thus, contributed to a controlled population size in China. The central government's official estimate states that OCP has prevented 400 million births since its introduction in late 1970s.[7] 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 (TFR) in China had dropped to 1.18, far below the replacement level.

In response to the declining fertility rate, between 1991 and 2011 the 31 provinces in mainland China successively revised their local family-planning regulations and allowed families to have two children if *both* parents were an only child.[8] However, this partial relaxation did not significantly change the declining trend in the fertility rate. Therefore, 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. 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

[7] This estimate was first reported in the media conference held by Mr. Bin Li, then the Director of National Population and Family Planning Commission, China. It then appeared repeatedly in various official documents and reports from the central government.
[8] This policy adjustment was implemented in our sample city during the 1990s and should not affect analysis in our sample period.

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 the policy relaxation, in November of 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 "Law on Population and Family Planning" became effective.

C The Effect of Relaxation of One-Child Policy on Fertility Rate

As far as we know, there is no academic research that evaluates the impact of the current relaxation of OCP on fertility rate. By contrast, a few studies examine the impact of a unique policy experiment implemented in Yicheng, Shanxi province 30 years ago. Since 1985, Yicheng, a rural county in the south of Shanxi, was granted an exception to OCP. The county was designated as an experiment locality for TCP, where almost all couples had the option to have two children. This unique experiment provides an excellent opportunity for scholars to infer the potential consequences of TCP from historical data. By comparing the demographics in Yicheng before and after the experiment, Wu (2014) and Wei and Zhang (2014) conclude that replacing OCP with TCP had little impact on the crude fertility rate. Qin and Wang (2017) adopt a synthetic control approach to conduct a rigorous counterfactual analysis on the impact of TCP in Yicheng and fail to find any impact of TCP on the crude fertility rate in Yicheng in the short run. In the long run, their estimation suggests that replacing OCP with TCP may add approximately 3 million newborns to China every year, a number that is significantly lower than the official prediction.

Even though there is no research on the effectiveness of the current relaxation of OCP, the available statistics also suggest a dampened effect of fertility relaxation on fertility rates. According to the official statistics of our sample city, fewer than 30,000 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. This number was lower than what the government predicted before announcing the relaxation of OCP.

3 Data

In this study, we introduce a proprietary employer-employee matched dataset from an anonymous major Chinese city[9] from 2012 to 2014, 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 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 compulsory required to join in the HPF system. Each employee and his/her employer must contribute a designated percentage of base salary to the employee's HPF account every month; the employee can then withdraw the savings in his/her HPF account at the time of a home purchase. The ratio of contribution to base salary is 12% for both employee and employer in our sample city between 2012 and 2014; that is, the monthly contribution amount equals 24% of the base salary of each employee. In our sample city, the base salary for an employee is calculated as his/her average monthly salary in the last calendar year and is annually adjusted every July. However, for a new employee, the HPF system will directly adopt the monthly salary as the base when he/she is hired by the employer.

For our sample city, we have access to complete HPF data of more than 138 million monthly contribution records between January 2012 and December 2014, encompassing about 5.4 million employees from more than 100,000 employers. Theoretically, this dataset covers the labor force 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 labor force, new hires and job-leavers in particular, and to impute the salary for

[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.
[10] See Tang and Coulson (2017) and Chen, Li, Wang, and Wu (2019) for more institutional details on the HPF system.

each employee working for the employer. For each employee, we also have information on age and gender. For employers, we know key characteristics, such as sector (government or firm, as well as ownership type of firms) and industry.

Raw data are cleaned via the following procedures. First, we drop employees with salaries beyond the reasonable range. 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. Second, we only include employees who are no older than 50 years of age during the sample period, because in China some female employees may retire when they are 50 years old. Third, we exclude employers with fewer than five employees, which is a standard exercise in research working with employee-employer data (Bonhomme, Lamadon, & Manresa, 2019; Heyman, Sjöholm, & Tingvall, 2007; Bayard, Hellerstein, Neumark, & Troske, 2003).[11] Fourth, we exclude employers with an abnormal number of new hires or job-leavers. Specifically, an employer is excluded from empirical analysis if in any month during the sample period: 1) the ratio of the number of new employees by gender and the employer size (i.e., number of total employees) in the current year is larger than 1.0; 2) the number of new employees by gender exceeds 100; or 3) the ratio of job-leavers by gender and the number of total employees in the current year exceeds 0.5. Fifth, we retain only employers with hiring records for consecutive years. Last, we require all employers to have HPF contribution records for consecutive years. Appendix Table A1 reports the sample size after each of our sample-processing procedures. After the data-cleaning process, approximately 72 million employee-month level observations for nearly 40,000 employers remain. We aggregate the employee-month data to the employer-quarter level in the following empirical analysis.

It is worth noting that we cannot study the impact of fertility relaxation policy on the salaries of existing employees because we cannot infer the salaries of existing employees in the post-policy period. Given that our HPF contribution data is only available before December 2014, we can only infer the salary of existing employees back to the year 2013 (the policy was announced in November 2013). However, we can infer the salary of new hires who are employed in the post-

[11] We relax the restriction in a separate analysis and the results are consistent with our primary results. See Section 6 for more information.

policy period because the contribution base of new hires in their first employment year is determined by their contracted salary, without any time lags.

4 Identification Strategy

In this study, we adopt a difference-in-differences (DID) design to analyze the impact of fertility relaxation on female labor-market outcomes based on the relaxation of China's OCP in November 2013. As described in Section 2.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.[12] We aggregate the employee-month dataset to the employer-quarter level by gender and define females as the treatment group and males as the control group. As discussed in Section 2.B, the relaxation of OCP was announced in November 2013 and implemented in our sample city in February 2014. Therefore, we define the quarters after the fourth quarter in 2013 as the post period. Following the identification strategy, we have:

$$Y_{ijt} = \beta \times FEMALE_i \times POST_t + \alpha_j + \delta_t + \eta_{it} + \gamma_{jt} + \epsilon_{ijt} \qquad (1)$$

where Y_{ijt} refers to the outcomes for gender i (1=female and 0=male) in employer j at quarter t; our main outcome variable is the salary of newly hired females (i.e., the average salary of female new hires for employer j at quarter t). $FEMALE_i$ is a dummy variable for the treatment group; $POST_t$ is a dummy variable for the post period (2013 Q4 to 2014 Q4); α_j is employer fixed effects; δ_t refers to year-quarter fixed effects; η_{it} is female-quarter fixed effects; γ_{jt} represents employer-year and employer-quarter fixed effects to flexibly controlling for the hiring trend and seasonality of each employer; ϵ_{ijt} is the error term. The standard errors are clustered at employer level. We would expect β to be negative if employers discriminate against females after the policy change by paying female new hires lower salaries.

[12] 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 3, our sample only covers 2012-2014. More importantly, because the policy change in November 2013 was officially interpreted as a first step of OCP reform, it is reasonable to expect that employers should start to form anticipations of the OCP abolishment since then. Because we focus on the *immediate* effect of OCP relaxation on female labor market outcomes, it is more appropriate to focus on the first signal associated with the policy change in November 2013.

In addition to salary, we can observe two other labor market outcomes in the data: the number of new hires and the number of job-leavers of each employer in each quarter. These two outcomes may be influenced by both supply-side and demand-side factors. For example, if the number of female new hires drops after the relaxation of OCP, it is possible that the drop is due to employers making fewer offers to females after the policy change (demand side) or to females being less likely to accept an offer after the policy change (supply side). Similarly, if the number of female job-leavers drops after the policy change, it is possible that employers fire fewer females after the policy change (demand side) or that females are more likely to stay with their current employers instead of hunting for new jobs (supply side). We acknowledge this data limitation, that we cannot precisely disentangle the supply-side factors from the demand-side factors (demand-side factors are the major interest of this study). We continue to study these two outcomes and show that the estimated effect is not likely to be driven solely by supply-side factors.

To verify the parallel trend assumption of the DID specification, we conduct an event study to analyze the dynamic effect of the policy change. Using the year 2012 as the baseline year, we have:

$$Y_{ijt} = \sum \beta_k \times FEMALE_i \times 1\{t = k\} + \alpha_j + \delta_t + \eta_{it} + \gamma_{jt} + \epsilon_{ijt} \quad (2)$$

where the variables are the same as Eq. (1); k ranges from 2013 Q1 to 2014 Q4. The parameter of interest is β_k, which refers to the dynamic impact of fertility relaxation on labor market outcomes.

5 Main Results

Table 1 presents the summary statistics of the three outcome variables for both genders, before and after the policy change. For female new hires, the average salary increased from 3,695 *yuan* per month in the pre-policy period to 4,212 *yuan* in the post-policy period, i.e., an increase of 517 *yuan* per month (about 82 USD). For male new hires, the average salary increased from 3,992 *yuan* to 4,701 *yuan* during the same period, i.e., an increase of 709 *yuan* per month (about 113 USD). On average, the number of female new hires was reduced by 0.4% of the employer size (0.052-0.048) after the policy, while the number of male new hires was reduced by 0.2% during the same period (0.057-0.055). On average, the number of female job-leavers increased by 0.5% of the employer size (0.035-0.030) after the policy, while the number of male job-leavers increased by 0.6% of the employer size (0.037-0.031).

Figure 1 shows the raw trend of the three outcome variables by quarter: the salary (in natural log) of new hires by gender, the number of new hires by gender (normalized by employer size), and the number of job-leavers by gender (normalized by employer size). From Figure 1A, we determine that new hire salaries exhibit an upward trend over time. It is worth noting that in our results male new hires have higher salaries than female new hires, on average, which is consistent with the gender wage gap literature conclusion that males earn higher salaries than females on average. It is reassuring that the salary time series for females and males exhibit parallel trends before the policy change (shown as a dotted vertical line) without any regression adjustment. However, female salaries seem to trend down relative to male in the post-policy period, which leads to a widening gender wage gap.

Figure 1B and Figure 1C present the time series for the number of new hires and job-leavers by gender, series that exhibit strong seasonality. In particular, the number of new hires peaks in the third quarter of each year, corresponding to the traditional starting time of new graduates. The overall pattern suggests that employers hire more males than females on average. The number of job-leavers seems to peak in the second quarter of each year, and the number of male job-leavers is larger than the number of female job-leavers on average.

Table 2 shows the primary DID results on the three outcome variables. As introduced in Section 4, analysis is conducted at the employer-quarter level. For each outcome, we have two specifications. The first specification (columns 1, 3, and 5) controls for employer fixed effects and year-quarter fixed effects. The second specification (columns 2, 4, and 6) further controls for flexible year trend and quarter trend for each employer (employer-year fixed effects and employer-quarter fixed effects) in order to capture employers' strategic differences in recruitment across years and seasonality in hiring and firing. We also control for gender-specific seasonal trends (female-quarter fixed effects) to capture potentially different seasonal trends by gender.

The first two columns of Table 2 present the impact of fertility relaxation on the salary of female new hires. The DID coefficient is significantly negative, which suggests that the salary of female new hires is reduced by approximately 1.2% (column 2) after the relaxation of OCP, relative to the salary of male new hires. This result suggests that employers may anticipate an increase in fertility among females after the policy change and discriminate against female new hires by offering them lower salaries.

Columns 3 and 4 document the impact of the policy change on the number of new hires. The DID coefficient is also significantly negative, suggesting that the number of female new hires declines significantly (relative to male new hires) after the policy. Using the benchmark value of the outcome variable (0.052, the average quarterly number of female new hires normalized by employer size before the policy), the estimation suggests that the relaxation of OCP causes a 4.4% reduction in the number of female new hires. There are two possible explanations for such a negative effect. First, employers may increase discrimination against female job candidates after the policy change, thereby reducing the number of female new hires. Second, females may anticipate their increase in fertility and reduce their labor force participation. Unfortunately, we cannot disentangle these two possibilities using our data because we do not have information on the job-hiring process and on potential candidates. However, we have shown that the salary of female new hires is lower than male new hires after the policy shock, suggesting that the reduction of new hires is not likely to be driven exclusively by the reduction of female labor-force participation; otherwise, female salaries should rise in response to a fall in labor supply.

In the last two columns, we present the impact of fertility relaxation on the female probability of leaving a job. Again, if females are more likely to reduce labor force participation voluntarily in preparation for having more children, we should see an increase in the number of females quitting the labor market after the policy change relative to the number of males. However, as shown in columns 5 and 6, the DID coefficient is significantly negative. An alternative explanation is that employers fire fewer females after the policy change. However, given that the salary of female new hires drops after the policy change, employers should be more willing to fire existing female employees and replace them with new hires in the market, which contradicts the findings in columns 5 and 6. Compared with the benchmark value of the dependent variable, column 6 suggests that female job-leavers are reduced by 4.3% relative to males after the policy change. One possible and credible explanation is that females recognize the increasing labor market discrimination after the policy change and are thus less willing to quit their jobs.

Figure 2 presents the event study (Equation (2)) of the three outcome variables where year 2012 is the baseline year. For the new hire salary, the coefficients become negative and significant when the policy was announced and remain negative and significant after the policy change. Importantly, the impact appears immediately after the policy announcement, suggesting that the effect is not likely to be driven by female labor supply changes, which would not show up so quickly.

For the number of new hires outcome, the coefficients prior to the policy shock (2013 Q4) are not significantly different from zero. After the policy shock, the coefficients are negative in the first two quarters, but insignificant at the 5% level. The coefficients drop further and remain statistically significant after 2014 Q2, which might be explained by the time lag between the recruitment process and the formal start time of a job. For existing employees' departures, the coefficients are insignificantly different from zero before the policy change, negative and marginally significant in the first three quarters after the policy change, and further decline in the last quarter in our sample. These three graphs verify the parallel trend assumption of the DID research design.

6 Robustness Checks

In this section, we conduct a series of robustness checks to ensure the validity of our results. First, we observe a decline in the salaries of female new hires *relative to* male new hires after the policy change. It is possible that our results are driven by an increase in the salaries of male new hires after the policy change. We rule out this alternative explanation using the event study by gender, controlling for very flexible time trends before and after the policy shock. The regression equation is as follows:

$$Y_{jt} = \lambda \times POST_t + f(d_{jt}) + \alpha_j + \epsilon_{jt} \tag{3}$$

where d_{jt} represents the employer j at quarter t relative to the policy shock; $f(\cdot)$ refers to the smooth function. The other variables are the same as in Equation (1). The standard errors are clustered at employer level. The parameter of interest is λ, which refers to the impact of fertility relaxation on labor market outcomes by gender. We choose three or four quarters (at most) as bandwidth, which means that the sample period ranges from the first quarter in 2013 (the fourth quarter in 2012) to the third quarter in 2014 (the fourth quarter in 2014). Following Zimmerman (2019), we conduct a 500-fold cross-validation estimate by bandwidth and polynomial degree, and calculate the average mean squared errors (AMSEs) to select the optimal polynomial degree. As shown in Appendix Table A2, polynomials degree three is the best fit in three of four bandwidths by gender when the outcome is the salary of new hires or the number of job-leavers, while polynomials degree one is the best fit when the dependent variable is the number of new hires. Table 3 shows the regression results with the optimal polynomial degree. Almost all of the coefficient estimates on λ are significantly negative for all of the three outcome variables. The coefficients for females are larger in magnitude than those for males, suggesting that our results are not driven by an increase in the control group.

Second, we address the concern that the negative impact of the policy change on female new hire salary is driven by the change in female labor supply instead of by employer discrimination. There are two types of labor supply changes which may flaw our interpretation. On the one hand, if female new hires in the post-policy period have lower abilities than the female new hires in the pre-policy period, the lower salary in the post-policy period may reflect a quality change in the female labor supply instead of discrimination from the demand side (employers' side). Unfortunately, we do not have information on employee education background in the dataset. However, for those new hires who are job switchers, we can use salary from the previous employer as a proxy for their quality or abilities. We first conduct individual level analysis (instead of employer-quarter level analysis) and show that the negative impact of the policy change on the salary of female new hires is driven primarily by job switchers (column 2, Table 4) instead of fresh graduates (column 1, Table 4). We then use the job-switchers sample and show in column 3 that, on average, female new hires in the post-policy period have lower salaries from previous employers relative to male new hires, suggesting that the quality of the female labor supply is lower in the post-policy period relative to males. However, even if we control for the employee's salary from the previous employer and re-run the analysis in column 2 (column 4, Table 4), the coefficient on the DID term is still significantly negative. The magnitude of the coefficient is reduced by approximately 27% though (comparing column 4 to column 2), which is likely attributed to the change of labor quality. However, it is still possible that employer discrimination may lead to the labor supply change in the post-policy period. For example, perhaps low-quality female candidates are more likely to get fired by their previous employers after the policy change and, thus, are more likely to be on the market. In an alternative approach, we extract the full salary records for each employee in the sample (i.e., not only the current salary as a new hire, but also past salary from previous employers) and construct a panel at the employee-year level.[13] Controlling for employee fixed effects, we study the effect of policy shock using the full sample in column 5 and using the job switcher sample in column 6. The results suggest that females experience a salary decline after the policy change relative to males, a decline that is free from labor quality changes.

On the other hand, it is also possible that the results are driven by the female labor effort decline in the post-policy period, which is another type of labor supply change. Although we have no information on labor effort in this dataset (e.g. working hours), we analyze the impact of fertility relaxation on working hours using a panel dataset from China Family Panel Studies (CFPS) for 2010, 2012, and 2014 to rule out this possibility.[14] These data have rich information about individual working hours at the national representative level. We compare daily working hours of females (treatment group) relative to males (control group) before and after the relaxation of OCP. Following the identification strategy, we have:

$$Y_{it} = \beta_1 \times FEMALE_i \times POST_t + \beta_2 \times X_{it} + \alpha_i + \delta_t + \epsilon_{it} \tag{4}$$

where Y_{it} refers to the daily working hours on individual i in year t[15]; $FEMALE_i$ is a dummy variable for the treatment group; $POST_t$ is a dummy variable for the post period; X_{it} represents the demographic characteristics including marriage, children, and age; we also control for individual fixed effects, α_i, and year fixed effects, δ_t; ϵ_{it} is the error term. The standard errors are clustered at individual level. We require that the individuals work in the urban areas and aged from 23 to 35 years old in 2013, while the self-employed individuals are excluded. We also require that the individual has no more than one child in 2012. The daily working hours are winsorized at 1 and

[13] Note that the employees who did not change their jobs in 2014 would have no record in 2014 due to data limitation. See Section 3 for more details.

[14] The CFPS survey is an annual longitudinal survey lunched 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 attrition rate was 21.4% from 2010 to 2012, and 15.6% from 2012 to 2014.

[15] 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?*". 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 day). Note that the transformation does not vary across gender, and thus would not bias our coefficient of DID term.

99 percentage level. In the end, the balanced panel dataset contains 902 individuals in three waves, i.e., two waves before the policy shock and one wave after the policy change. Table A3 provides the summary statistics. On average, 48% of individuals are female, and the individuals work 5.68 hours on average every day.

Table 5 presents the regression results.[16] The respondents have either one child or no child in 2012 in Panel A, and one child in 2012 in Panel B. The outcome variables are daily working hours in columns 1 and 2, and daily working hours (in natural log) in columns 3 and 4. For each specification, we control for year fixed effects and individual fixed effects. We further control for age, dummy variable for marriage, and dummy variable for children in columns 2 and 4. Almost all the coefficients of DID term are insignificantly positive, except that the coefficient in column 2 of Panel A is negative (-0.0017) but also statistically insignificant. The results consistently suggest that the relaxation of OCP would not significantly reduce labor effort, and thus drive our main results in Table 2.

Third, we provide evidence that lower salary of female new hires after the policy change is related to concern about the increasing fertility of females. Based on the micro-level dataset of the 2010 population census[17], we calculate the probability of having a second child in 2010 conditional on having one child in 2009 by age and industry in the four municipalities and 22 provincial capitals[18]. Panel A of Figure A1 plots the distribution of the probability of having a second child by age and industry. Using this variation, we investigate whether female new hires in an age-industry group who have a higher probability of having a second child are more likely to have a lower salary (than female new hires in an age-industry group who have relatively lower probability of having a second child) after the policy change. The specification is as follows:

$$Y_{ijt} = \beta \times EXPOSURE_{it} \times POST_t + \alpha_j + \delta_t + \eta_{it} + \gamma_{jt} + \theta \times X_{it} + \epsilon_{ijt} \qquad (5)$$

where Y_{ijt} is the salary of new hire *i* in employer *j* at quarter *t*; $EXPOSURE_{it}$ refers to the probability of having a second child in 2010, conditional on having one child in 2009, by age and industry for

[16] As a robustness check, we require that the respondents are between ages 23-30 in 2013, and repeat the same empirical exercise. The results are available in Table A4, which are similar.
[17] We thank Ting Chen for kindly sharing the dataset.
[18] We exclude the provincial capitals in Guangxi, Inner Mongolia, Ningxia, Tibet, and Xinjiang because these are Minority Autonomous Regions that are exempted from OCP (as discussed in Section 2.A).

individual i at quarter t; X_{it} represents demographic characteristics, including age and birthplace; η_{it} is age-quarter fixed effects. The other variables are the same as in Equation (1). The standard errors are clustered at employer level.

As shown in Table 6, the interaction term is significantly negative, suggesting that the salary reduction is stronger for females who are more likely to have a second child. In addition, Panel B of Figure A1 verifies that the effect is insignificantly different from zero in the pre-policy period (using year 2012 as the benchmark period).[19]

Fourth, we use alternative sets of fixed effects to check the robustness of the results. Instead of using the full sets of fixed effects, we exclude employer-quarter fixed effects or employer-year fixed effects. The results are reported in Table A6, and are similar to the main results in Table 2 in terms of magnitude and significance. Fifth, because each of our observations is aggregated at the employer-quarter level, we use weighted OLS regression by gender-specific employer size as a robustness check. Table A7 reports the results, which are consistent with the main findings. Sixth, we change the aggregation frequency from quarterly to monthly and semi-yearly. One concern with monthly aggregation is that the hiring outcomes may have too many zeros because employers may not have new hires every month. In contrast, semi-yearly aggregation may give us too few observations per employer, especially post-policy period. Appendix Table A8 reports the regression results, which are largely consistent with the primary results in Table 2. Seventh, we exclude employers with fewer than three employees instead of the minimum of five employees (as discussed in Section 3). Table A9 shows that these regression results are consistent with the main results. Finally, some may concern that our results are driven by the fourth quarter every year instead of policy shock. We exclude the observations in the fourth quarter and repeat the main regression. Appendix Table A10 presents the regression results, which are consistent the primary results.

[19] We also provide evidence that married female employees are more likely to be affected based on individual-level subsamples. Specifically, we identify married employees from a subsample of 44,112 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 individuals. In order to minimize the probability of counting married individuals as unmarried in the rest of the sample, we restrict the individuals in both subsamples to those younger than 30 years old. As shown in Table A5, the salary reduction of married new hires is larger than that of unmarried new hires, which is consistent with our main results. The coefficient in column 1 is insignificant, however, and is likely due to small sample size.

7 Discussions

In this section, we first discuss the implications of our results on gender wage gap and then explore the rich heterogeneity of the impact of fertility relaxation on female labor market outcomes by employer and employee characteristics.

In our primary results, we show that the salary of female new hires is reduced by 1.2% after the policy change relative to male new hires. How much does this magnitude contribute to the gender wage gap in our sample? We answer this question by calculating the gender wage gap of all the new hires in our sample before the policy shock.[20] As shown in column 1 of Table 7, female new hires have approximately 6.1 log points lower salary than male new hires, and the coefficient is reduced to 5.4 log points after controlling for employer fixed effects in column 2. The coefficient remains similar (5.5 log points in column 3) if we employ the same fixed effects used in the main results (Table 2). One problem with these regressions is that we do not have education information for the new hires, an important control variable in the salary equation. However, we have a small subsample of employees who are mortgage applicants and, thus, have access to these employees' education background information. Columns 4 and 5 compare the estimation on salary equation using this subsample without and with education background as a control variable, both of which produce similar results on gender wage gap. Therefore, we believe that our estimation of the gender wage gap of new hires should be credible even without controlling for education. Given that the relaxation of OCP reduces the salary of female new hires by 1.2%, we calculate that such an effect represents an approximately 22% (1.2%/5.5%) increase in the gender wage gap in our data, which is significant in terms of its economic magnitude.

Next, we explore the heterogeneity of the impact of the policy change. 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 females in the sample into six age cohorts. In Table 8, we

[20] We regress the salary in natural log on the dummy on female, controlling for year fixed effects, employer fixed effects, age and age polynomials of the employee, and a dummy variable indicating whether the employee is a native resident of that city or not. The regression equation can be specified as $Y_{ijt} = \beta \times MALE_i + X_{it} + \alpha_j + \delta_t + \epsilon_{ijt}$, where the variables are the same as Equation (6); X_{it} represents a quartic function in age, education (i.e., a set of dummy variables indicating the highest level of education), and a categorical variable indicating the ranks of an employee's professional title.

show that the salary reduction of female new hires is most significant for age cohorts 31-35 (1.4%), 26-30 (1.1%), and 22-25 (0.8%), but insignificant for the rest of the groups. This result is consistent with the explanation that after the relaxation of OCP employers discriminate against females in fertile age. Similarly, the reduction of female new hires is largest for females aged 31-35 (7.8%), followed by 22-25 (6.3%), and 26-30 (3.1%). The coefficient for females aged 41-45 is negative (4.3%) but insignificant. As a validity check, the impact on those aged 46-50, women who are unlikely to give birth, is positive and insignificant. Finally, we show that females in age cohorts 41-45 (16.7%), 36-40 (10.5%), 31-35 (8.8%), and 22-25 (5.1%) are less likely to leave their jobs after the policy shock. To summarize, the reduction in female salaries is largest for younger female cohorts, while the reduction in female headcount exists for all reproductive age cohorts.

We also verify heterogeneity by age by plotting the gender wage gap of new hires at each age (from 23 to 50 years old) in our sample, before and after the policy change. The age-specific estimation on the gender wage gap follows the equation below:

$$Y_{ijt} = \beta \times MALE_i + X_{it} + \alpha_j + \delta_t + \epsilon_{ijt} \quad (6)$$

where Y_{ijt} refers to the salary for new hire i in employer j at quarter t; $MALE_i$ is a dummy variable for gender; X_{it} represents the birthplace dummy; α_j is employer fixed effects; δ_t refers to the year-quarter fixed effects; ϵ_{ijt} is the error term. Robust standard errors are clustered at the employer level. We run the regression shown in Equation (6) for each age before and after the policy shock. Therefore, the coefficient β captures the salary premium of males relative to females at each specific age. Figure 3 plots the coefficients and 95% confidence intervals of β at different ages, before and after the policy change. This graph is consistent with our primary results and the heterogeneity analysis by age cohort described above, showing that the male salary premium rises significantly after the policy change, especially for the reproductive age cohorts (25–35 years old).

Second, we investigate whether effects are driven by new firms established after the policy change or by existing firms. Table A11 reports the composition change before and after the policy. Nearly 6,000 employers (or 15%) appear in the data only after the policy change, and 354 (or 1%) employers disappear from the data before the policy shift. Interestingly, the employers appearing only after the policy change (very likely established after the policy) are mainly private-owned enterprises (POEs) with lower female to male employee ratios. Figure 4 plots the distribution of the gender composition of employees of the three types of employers even further. Figure 4 shows that the female to male ratio shifts leftward for employers established after the policy change relative to employers that survived throughout the policy shock. Table 9 shows the primary results using the balanced panel (i.e., employers that survive throughout our sample period, which are relatively larger businesses). The findings on salary and the number of job-leavers remain similar to the main results in Table 2. However, in the balanced panel we do not find a reduction of female new hires after the policy change, as suggested in columns 3 and 4. One possible explanation is that large employers and small employers play on different margins. Specifically, large employers may respond to the policy change by cutting the salary of female new hires, while small employers may respond by reducing the headcount of female new hires.

To verify this conjecture, we conduct a heterogeneity analysis on employer size. We use the median employer size of each year in our sample as the cutoff points: 29.5 in 2012, 26 in 2013, and 22 in 2014. As shown in Table 10, the salary reduction of female new hires is insignificant for small employers; the salary reduction is driven mainly by large employers (1.5%). The reduction in the number of female new hires is larger (5.3%) for small employers than it is for large employers (3.1%); the reduction in the number of female job-leavers is driven mainly by large employers (6.7%). The overall message is consistent with our conjecture that small employers respond to the relaxation of OCP by reducing female headcounts, while large employers respond to the policy change both by reducing female headcounts (but less than small employers) and cutting the salary of female new hires. Moreover, females employed by large employers are less likely to leave their jobs than females in small employers, which might be due to the stability of working for large employers as well as the associated plausibly better non-pecuniary benefits.

Third, we explore heterogeneity by different employer sectors, namely, public sector, state-owned enterprises (SOEs), private-owned enterprises (POEs), and joint ventures (JVs). As suggested in Table 11, we find that the salary reduction of female new hires is not significant for the public sector, which is reasonable because public sector salary is based on the rank of the position and is, thus, less flexible. In contrast, the increase in the gender wage gap after the policy change is driven by SOEs (3.0%), followed by JVs (1.4%), and POEs (0.8%). The reduction of female new hires is driven primarily by the public sector (9.1%) and SOEs (6.7%).[21] The finding that increased discrimination is stronger for SOEs than POEs and JVs is consistent with Becker's theory that firms in less competitive sectors are likely to have stronger discrimination. Otherwise, new firms drive away discriminating firms by hiring the discriminated employees at lower cost and making higher profits in a competitive sector (Becker, 1971). In addition, we show that females in SOEs and POEs are significantly less likely to leave their jobs after the policy shock, while females in other sectors do not show a significant change in terms of job leaving.

Last, we examine the heterogeneous effect by industry characteristics. In particular, we categorize the first-digit industries into "brawn" versus "brain." "Brawn" industries refer to most of the primary and secondary industries that specialize in manual labor work, which is arguably less substitutable across gender (Olivetti & Petrongolo, 2014; Rendall, 2017), while "brain" industries refer primarily to tertiary industries that require less physical labor and, thus, are more substitutable across gender.[22] Our hypothesis is that discrimination against female employees is likely to be stronger in "brain" industries, because employers can employ more males to substitute for females after the policy change. Table 12 presents the heterogeneity analysis by "brawn" and "brain" industries. Columns 1 and 2 indicate that the negative effect on salary pertains only to the "brain" industries, consistent with our hypothesis. Interestingly, the effect on the number of new hires is positive and significant for the "brawn" industries while it is insignificant for the "brain" industries. This effect may result from two possible reasons: 1) employers that are matched to industry characteristics are more likely to be large employers; we have shown in Table 10 that large employers play on the intensive margin by reducing the salary of female new hires instead of reducing female headcounts; 2) the "brawn" industries may substitute females in reproductive age groups with females in other age cohorts. Appendix Table A12 reports the coefficients on the interaction of treatment effect (i.e. Female × Post in main results) with industry characteristics and age cohorts. These results suggest that the "brawn" industries hire more females in age cohorts 22-25 and 46-50, both categories of women who are less likely to have a second child, while the

[21] There is rich anecdotal evidence that the recruitment of civil servants in China discriminates against females. See http://paper.cnwomen.com.cn/content/2018-03/10/047089.html, for example.

[22] Following Olivetti and Petrongolo (2014) and Rendall (2017), we divide employers into "brawn" (containing agriculture, forestry, animal husbandry and fishing, mining, manufacturing, electricity, heat, gas and water production and supply, and construction) and "brain" (including all service industries and the public sectors).

"brain" industries hire fewer females in almost all age cohorts except ages 46-50 (statistically significant in ages 22-25).

8 Conclusion

In this study, we investigate the consequences of the relaxation of OCP on gender discrimination in the labor market. Using an employer-employee matched administrative dataset from a major city in China, we show that after the policy shock employers reduce the salary of female new hires by 1.2% relative to the salary of male new hires, which leads to a 22% gender wage gap increase in our sample. We verify that this result is driven by the decline of female salaries instead of an increase in male salaries, and that it is not driven by a change in labor quality or labor effort. We also provide evidence that this salary reduction is related to concerns about increasing female fertility. Moreover, effects are heterogeneous by employee and employer attributes. The effect is the largest for females aged 31-35, who might be the most likely to have a second child after the policy change. In addition, large and small employers respond to the policy change on different margins: small employers reduce the headcounts of female new hires, while large employers reduce the number of female new hires (less than small employers) and reduce the salary of female new hires. We also show that the salary effect is largest in state-owned enterprises (SOEs) as compared to private-owned enterprises (POEs) and joint ventures (JVs), and in industries that have arguably higher gender substitutability.

In addition to results about salary, we also find that employers hire 4.4% fewer female employees relative to male after the relaxation of fertility restriction, and that female employees are 4.3% less likely to leave their current jobs after the policy. These estimates can be translated into approximately 1,950 fewer female employees employed every month and 1,059 fewer female employees leaving their jobs every month in our sample city.[23] The reduction of female labor-force participation may lead to negative impacts in the long term, such as enlarging the educational gender gap in the next generation (Fan, Fang, & Markussen, 2015) and salary inequality (Greenwood, Guner, Kocharkov, & Santos, 2014).

[23] The average employer size is 64.351 and the number of employers is 39,525. According to the coefficients in Table 2, in the sample city there are 1,950 (0.0023×64.351×39,525/3=1950) fewer female employees being hired, and 1,059 (0.0013×64.351×39,525/3=1059) fewer female employees leaving their current jobs every month after the policy shock.

Even though our analysis is based on one major city in China, we believe that the results can be generalized to the entire country because the sample city is likely the most developed city in terms of enforcement of laws and legislation. Therefore, labor market discrimination should be less in this city compared to in an average city in China. Moreover, we believe that the complete relaxation of OCP announced at the end of 2015 may further exacerbate labor market discrimination, something that could not be tested in this study because our sample does not cover that post-policy period. At the national level, the unemployment rate of females relative to males has increased after the complete relaxation of OCP in 2015, consistent with our finding that fertility relaxation policy exacerbates disadvantages to females in the labor market.[24]

[24] Data source: International Labour Organization. See https://www.ilo.org/shinyapps/bulkexplorer1/?lang=en&segment=ref_area&id=CHN_A for more details.

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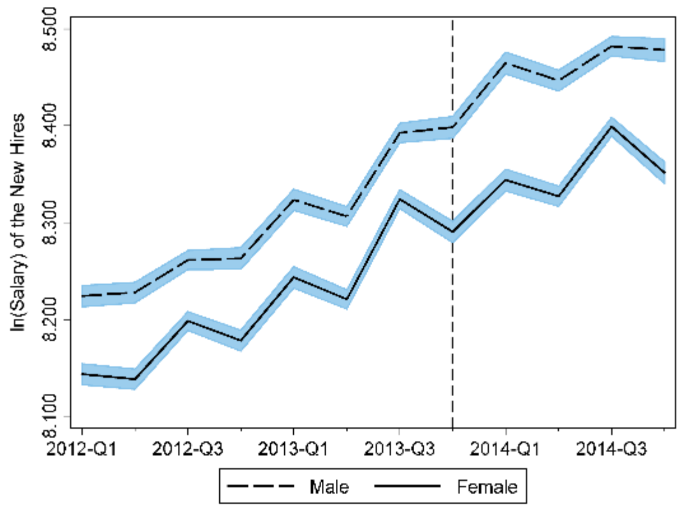
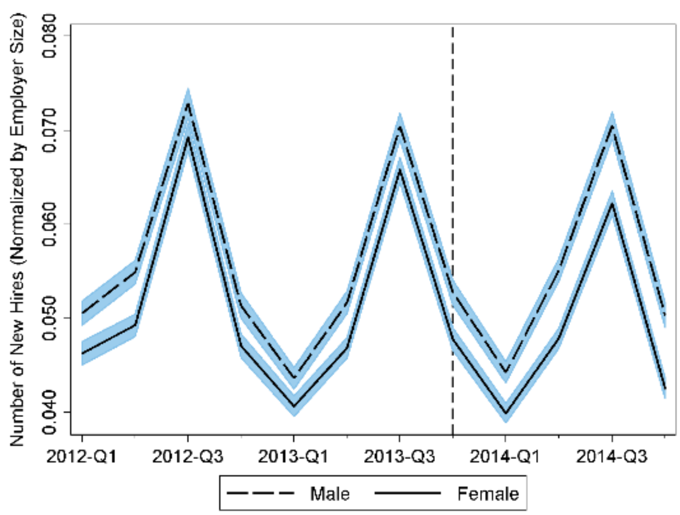
Figures

Figure 1. Raw Trend of Labor Market Outcomes

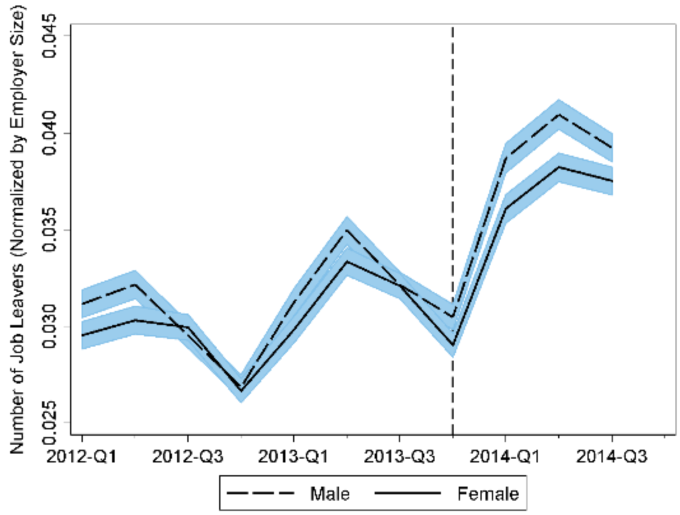
Note: This figure shows the raw trend of three outcome variables by gender, namely, the salary (in natural log) of the new hires by gender, the number of new hires by gender (normalized by employer size), and the number of job leavers by gender (normalized by employer size). The shadow area refers to the 95 percent confidence intervals.



A. Salary (in Natural Log) of the New Hires



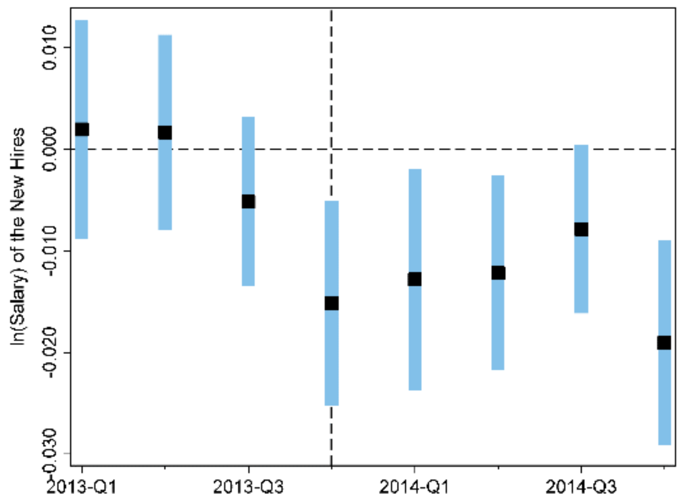
B. Number of New Hires (Normalized by Employer Size)



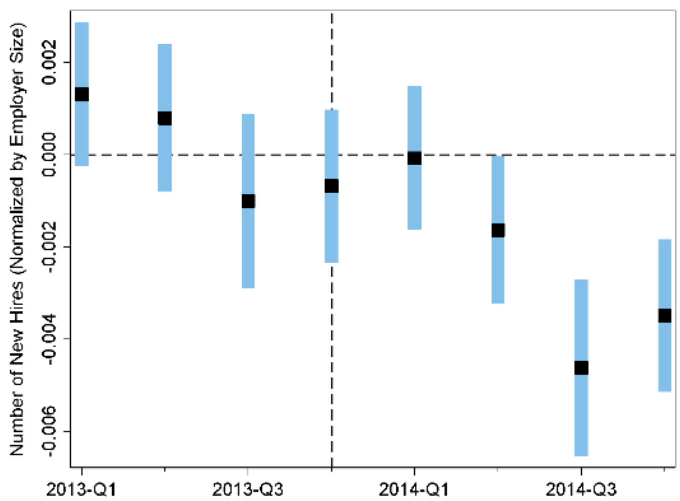
C. Number of Job Leavers (Normalized by Employer Size)

Figure 2. Event Study: Main Results

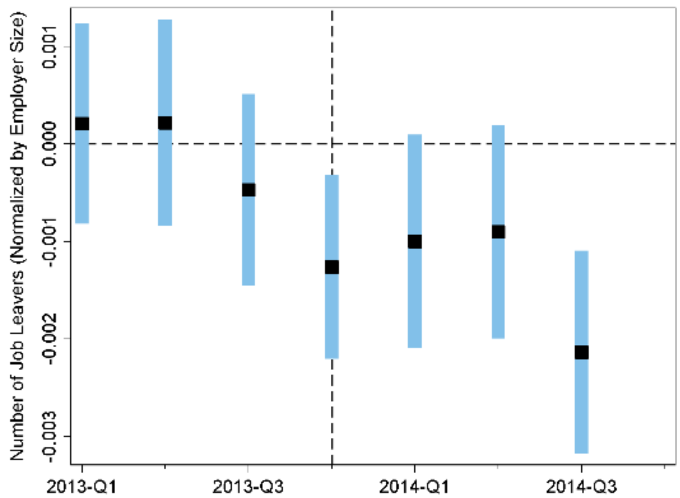
Note: This figure explores the dynamic effect of the relaxation of OCP on female labor market outcomes. The sample period is from 2012 to 2014, and year 2012 is taken as the baseline period. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in Panel A; the number of new hires (normalized by employer size) in Panel B; and the number of job leavers (normalized by employer size) in Panel C. For each specification, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects. Robust standard errors are clustered at employer level. The bars refer to the 95 percent confidence intervals.



A. Salary (in Natural Log) of the New Hires



B. Number of New Hires (Normalized by Employer Size)



C. Number of Job Leavers (Normalized by Employer Size)

Figure 3. Gender Wage Gap of New Hires by Age

Note: This figure plots the gender wage gap of new hires by age before and after the policy shock. Each point represents the gender wage gap estimate for a specific age before or after the policy, with the 95% confidence intervals in the shadow area. For each specification, we control for year-quarter fixed effects, employer fixed effects and the birth place dummy. Robust standard errors are clustered at employer level.

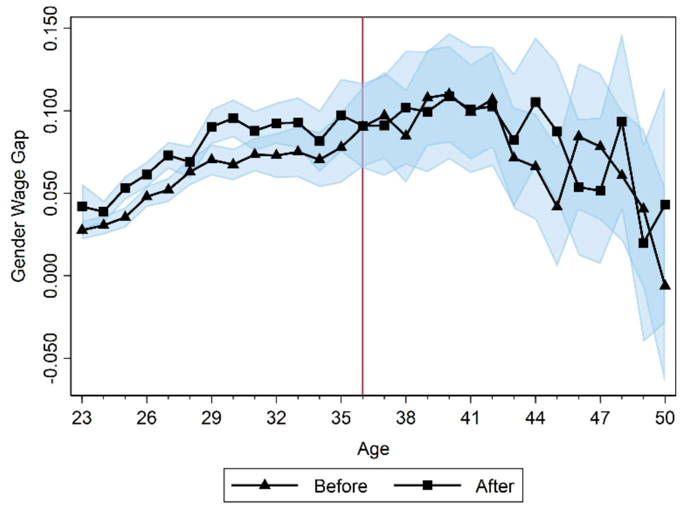
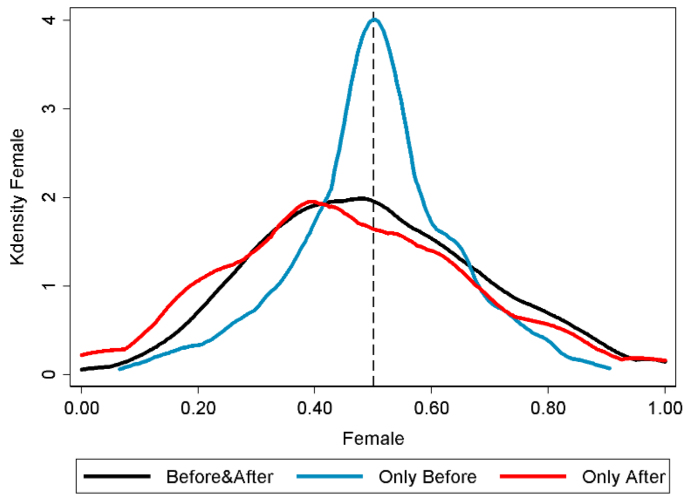


Figure 4. Distribution of Sex Ratio by Employer Status

Note: This figure plots the distribution of the percentage of female employees of the three types of employers, including the employers existing before and after the policy, only existing before the policy, and only existing after the policy. The vertical dashed line refers to female employee share=0.5.



Tables

Table 1. Summary Statistics: Administrative Data around the Policy Shock

Note: This table reports the summary statistics of the administrative datasets for new hires and job leavers around the policy shock. The sample period is from 2012 to 2014. The dataset is aggregated to the employer-quarter level by gender.

Panel A. Full Sample		Before			After		
	Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
New Hires	Salary	201,205	3844.880	2486.069	156,596	4460.136	3128.736
	# of employer	31,134			33,609		
	New Hire	426,602	0.054	0.116	364,792	0.051	0.121
	# of employer	33,590			39,171		
Job Leavers	Leave	426,602	0.031	0.061	287,066	0.036	0.070
	# of employer	33,590			38,182		

Panel B. Female		Before			After		
	Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
New Hires	Salary	99,929	3695.472	2359.391	77,211	4212.260	2875.571
	# of employer	29,241			30,202		
	New Hire	213,301	0.052	0.112	182,396	0.048	0.113
	# of employer	33,590			39,171		
Job Leavers	Leave	213,301	0.030	0.060	143,533	0.035	0.069
	# of employer	33,590			38,182		

Panel C. Male		Before			After		
	Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
New Hires	Salary	101,276	3992.300	2596.630	79,385	4701.225	3339.114
	# of employer	28,991			30,108		
	New Hire	213,301	0.057	0.119	182,396	0.055	0.128
	# of employer	33,590			39,171		
Job Leavers	Leave	213,301	0.031	0.062	143,533	0.037	0.071
	# of employer	33,590			38,182		

Table 2. Main Results

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in columns 1 and 2; the number of new hires (normalized by employer size) in columns 3 and 4; and the number of job leavers (normalized by employer size) in columns 5 and 6. We control for year-quarter fixed effects and employer fixed effects in columns 1, 3 and 5, while we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects in columns 2, 4 and 6. 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.

Variables	(1) ln(Salary)	(2) ln(Salary)	(3) New Hire	(4) New Hire	(5) Leave	(6) Leave
Female × Post	-0.0137***	-0.0117***	-0.0023***	-0.0023***	-0.0012***	-0.0013***
	(0.0021)	(0.0022)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Observations	356,057	318,533	791,394	791,394	713,668	713,668
R-squared	0.7225	0.8554	0.2948	0.6221	0.2570	0.5240
Benchmark	3695.472	3695.472	0.0523	0.0523	0.0303	0.0303
Relative Effect	-0.0136	-0.0116	-0.0440	-0.0440	-0.0396	-0.0429
Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer FE	YES	NO	YES	NO	YES	NO
Employer-year FE	NO	YES	NO	YES	NO	YES
Employer-quarter FE	NO	YES	NO	YES	NO	YES
Female-quarter FE	NO	YES	NO	YES	NO	YES

Table 3. Robustness Check: Gender Substitution

Note: This table reports the robustness check on gender substitution. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in Panel A; the number of new hires (normalized by employer size) in Panel B; and the number of job leavers (normalized by employer size) in Panel C. We conduct an event study by gender using bandwidth varying from 3 to 4. For each specification, we control for employer fixed effects and flexible time trend. The optimal polynomial degrees are selected based on 500-fold cross-validation estimates. 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.

	(1)	(2)	(3)	(4)
Panel A	ln(Salary)	ln(Salary)	ln(Salary)	ln(Salary)
Post	-0.0189***	-0.0250***	-0.0108*	-0.0070
	(0.0057)	(0.0049)	(0.0058)	(0.0051)
Observations	100,168	128,314	102,642	131,439
R-squared	0.7797	0.7659	0.7824	0.7707
Bandwidth	3	4	3	4
Polynomial Degree	3	3	3	3
Gender	Female	Female	Male	Male
Panel B	New Hire	New Hire	New Hire	New Hire
Post	-0.0180***	-0.0070***	-0.0178***	-0.0055***
	(0.0008)	(0.0007)	(0.0009)	(0.0008)
Observations	238,513	308,072	238,513	308,072
R-squared	0.3735	0.3504	0.3793	0.3596
Bandwidth	3	4	3	4
Polynomial Degree	1	1	1	1
Gender	Female	Female	Male	Male
Panel C	Leave	Leave	Leave	Leave
Post	-0.0083***	-0.0066***	-0.0075***	-0.0051***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Observations	238,513	268,597	238,513	268,597
R-squared	0.3646	0.3485	0.3683	0.3517
Bandwidth	3	4	3	4
Polynomial Degree	3	3	3	3
Gender	Female	Female	Male	Male
Employer FE	YES	YES	YES	YES

Table 4. Robustness Check: Labor Quality Change

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes based on individual level data. The sample period is from 2012 to 2014. The dataset is aggregated to individual-employer-quarter level in columns 1-4 and individual-employer-year level in columns 5 and 6. The outcome is the salary (in natural log) of the new hires in columns 1, 2 and 4; the salary (in natural log) of the new hires in last employers in column 3; and the salary (in natural log) of employees in columns 5 and 6. For each specification in columns 1-4, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects, female-quarter fixed effects and demographic characteristics, including age and birth place. We also control for year (salary in the last employees) fixed effects in columns 3 and 4, and the salary (in natural log) of the new hires in last employers in column 4. For columns 5 and 6, we control for demographic characteristics, individual fixed effects and employer-year fixed effects. 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.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ln(Salary)	ln(Salary)	ln(Past Salary)	ln(Salary)	ln(Salary)	ln(Salary)
Female × Post	-0.0043*	-0.0190***	-0.0156***	-0.0139***	-0.0152***	-0.0146***
	(0.0026)	(0.0027)	(0.0030)	(0.0024)	(0.0020)	(0.0021)
Observations	552,650	795,886	795,886	795,886	6,557,783	1,207,195
R-squared	0.7871	0.7209	0.4742	0.7752	0.9562	0.9247
Demographic Attributes	YES	YES	YES	YES	YES	YES
Year-quarter FE	YES	YES	YES	YES	NO	NO
Employer-year FE	YES	YES	YES	YES	YES	YES
Employer-quarter FE	YES	YES	YES	YES	NO	NO
Female-quarter FE	YES	YES	YES	YES	NO	NO
ln(Past Salary)	NO	NO	NO	YES	NO	NO
Year FE (Past Salary)	NO	NO	YES	YES	NO	NO
Individual FE	NO	NO	NO	NO	YES	YES
Sample	Fresh Grad	Job Switcher	Job Switcher	Job Switcher	Full Sample	Job Switcher

Table 5. Robustness Check: Labor Effort Change

Note: This table explores the effect of the relaxation of OCP on female's labor effort using the CFPS survey dataset. The individual-year level balanced panel data involve three waves in 2010, 2012 and 2014. We require that the individuals work in the urban areas and aged from 23 to 35 in 2013, while the self-employed individuals are excluded. The respondents have either one child or no child in 2012 in Panel A, and one child in 2012 in Panel B. The outcomes are daily working hours in columns 1 and 2, and daily working hours (in natural log) in columns 3 and 4. The daily working hours are winsorized at 1 and 99 percentage level. For each specification, we control for year fixed effects and individual fixed effects. We further control for age, dummy variable for marriage, and dummy variable for children in columns 2 and 4. 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.

	(1)	(2)	(3)	(4)
Panel A	Working Hours	Working Hours	ln(Working Hours)	ln(Working Hours)
Female × Post	0.0315	-0.0017	0.0238	0.0170
	(0.1843)	(0.1852)	(0.0399)	(0.0402)
Observations	2,706	2,706	2,706	2,706
R-squared	0.4680	0.4698	0.4572	0.4591
# of Child in 2012	0/1	0/1	0/1	0/1
Panel B	Working Hours	Working Hours	ln(Working Hours)	ln(Working Hours)
Female × Post	0.1972	0.1849	0.0631	0.0610
	(0.2632)	(0.2649)	(0.0543)	(0.0546)
Observations	1,542	1,542	1,542	1,542
R-squared	0.4368	0.4379	0.4291	0.4303
# of Child in 2012	1	1	1	1
Demographic	NO	YES	NO	YES
Individual FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Age in 2013	23-35	23-35	23-35	23-35
Sample Period	2010-2014	2010-2014	2010-2014	2010-2014

Table 6. Robustness Check: Exposure to the Policy Shock

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes, using the probability of having a second child in 2010 conditional on having one kid in 2009 by age and industry. We calculate the probability based on the 2010 census dataset; see the text for more details on how we calculate the probability. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires. For each specification, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and demographic characteristics, including age and birth place. We also control for age-quarter fixed effects in column 2. 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.

VARIABLES	(1) ln(Salary)	(2) ln(Salary)
Exposure × Post	-0.0790**	-0.0661**
	(0.0333)	(0.0329)
Observations	259,516	259,516
R-squared	0.7325	0.7391
Demographic Attributes	YES	YES
Year-quarter FE	YES	YES
Employer-year FE	YES	YES
Employer-quarter FE	YES	YES
Age-quarter FE	NO	YES

Table 7. Gender Wage Gap in Our Sample

Note: This table reports the gender wage gap in our sample. In columns 1 and 3, the sample period is from 2012Q1 to 2013Q3, while the dataset is aggregated to employer-quarter level. In columns 4 and 5, we adopt the micro-level mortgage sample between 2006 and 2013. The outcome is the salary (in natural log). For each specification, we control for year-quarter fixed effects and individual characteristics, including a quartic function in age and birth place dummy. We further control for employer fixed effects in columns 2, 4 and 5, and employer-year fixed effects and employer-quarter fixed effects in column 3. We also control for a sets of dummy variables indicating education and professional title 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.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	ln(Salary)	ln(Salary)	ln(Salary)	ln(Salary)	ln(Salary)
Female	-0.0610***	-0.0541***	-0.0551***	-0.1138***	-0.1160***
	(0.0041)	(0.0018)	(0.0018)	(0.0104)	(0.0105)
Observations	1,008,818	1,007,221	986,208	230,977	230,977
R-squared	0.0660	0.6521	0.7128	0.5374	0.5481
Year-quarter FE	YES	YES	YES	YES	YES
Age	YES	YES	YES	YES	YES
Local	YES	YES	YES	YES	YES
Employer FE	NO	YES	NO	YES	YES
Employer-year FE	NO	NO	YES	NO	NO
Employer-quarter FE	NO	NO	YES	NO	NO
Education Level	NO	NO	NO	NO	YES
Professional Title	NO	NO	NO	NO	YES
Sample	New Hire	New Hire	New Hire	Mortgage	Mortgage
Sample Period	2012Q1-2013Q3	2012Q1-2013Q3	2012Q1-2013Q3	2006-2013	2006-2013

Table 8. Heterogeneous Effect: by Employees' Age Cohorts

Note: This table explores the effect of the relaxation of OCP on new female's labor market outcomes by employees' age cohorts. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender and age cohorts. The outcome is the salary (in natural log) of the new hires in Panel A; the number of new hires (normalized by employer size) in Panel B; and the number of job leavers (normalized by employer size) in Panel C. For each specification, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects. 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.

	(1)	(2)	(3)	(4)	(5)	(6)
	22-25	26-30	31-35	36-40	41-45	46-50
Panel A	ln(Salary)	ln(Salary)	ln(Salary)	ln(Salary)	ln(Salary)	ln(Salary)
Female × Post	-0.0085***	-0.0108***	-0.0141***	0.0115	-0.0086	-0.0272
	(0.0027)	(0.0029)	(0.0050)	(0.0096)	(0.0120)	(0.0178)
Observations	137,987	176,243	97,264	33,200	19,730	8,551
R-squared	0.8850	0.8761	0.8628	0.8486	0.8502	0.8718
Benchmark	3034.361	3758.437	4279.297	4194.435	4029.017	4158.5
Relative Effect	-0.0085	-0.0107	-0.0140	0.0116	-0.0086	-0.0268
Panel B	New Hire	New Hire	New Hire	New Hire	New Hire	New Hire
Female × Post	-0.0011***	-0.0006***	-0.0007***	0.0000	-0.0001	0.0001**
	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	791,394	791,394	791,394	791,394	791,394	791,394
R-squared	0.5037	0.5530	0.5151	0.4498	0.4397	0.4359
Benchmark	0.0174	0.0192	0.0090	0.0034	0.0023	0.0010
Relative Effect	-0.0632	-0.0313	-0.0778	0.0000	-0.0435	0.1000
Panel C	Leave	Leave	Leave	Leave	Leave	Leave
Female × Post	-0.0004***	-0.0001	-0.0005***	-0.0002***	-0.0002***	-0.0000
	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0000)
Observations	713,668	713,668	713,668	713,668	713,668	713,668
R-squared	0.4362	0.4550	0.4087	0.3651	0.3553	0.3577
Benchmark	0.0079	0.0118	0.0057	0.0019	0.0012	0.0018
Relative Effect	-0.0506	-0.0085	-0.0877	-0.1053	-0.1667	0.0000

Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer-year FE	YES	YES	YES	YES	YES	YES
Employer-quarter FE	YES	YES	YES	YES	YES	YES
Female-quarter FE	YES	YES	YES	YES	YES	YES

Table 9. Heterogeneous Effect: Balanced Panel

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes based on a balanced panel dataset. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in columns 1 and 2; the number of new hires (normalized by employer size) in columns 3 and 4; and the number of job leavers (normalized by employer size) in columns 5 and 6. We control for year-quarter fixed effects and employer fixed effects in columns 1, 3 and 5, while we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects in columns 2, 4 and 6. 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.

VARIABLES	(1) ln(Salary)	(2) ln(Salary)	(3) New Hire	(4) New Hire	(5) Leave	(6) Leave
Female × Post	-0.0168***	-0.0145***	0.0005*	0.0004	-0.0009***	-0.0010***
	(0.0024)	(0.0026)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Observations	282,946	254,389	659,928	659,928	604,934	604,934
R-squared	0.7097	0.8440	0.2104	0.4612	0.2369	0.4625
Benchmark	3730.274	3730.274	0.0390	0.0390	0.0286	0.0286
Relative Effect	-0.0167	-0.0144	0.0128	0.0103	-0.0315	-0.0350
Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer FE	YES	NO	YES	NO	YES	NO
Employer-year FE	NO	YES	NO	YES	NO	YES
Employer-quarter FE	NO	YES	NO	YES	NO	YES
Female-quarter FE	NO	YES	NO	YES	NO	YES

Table 10. Heterogeneous Effect: by Employer Size

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes by employer size. We calculate the number of the employees in the employers in the specific years and use the median (29.5 in 2012; 26 in 2013; 22 in 2014) as the cutoff point for large/small employers. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in columns 1 and 2; the number of new hires (normalized by employer size) in columns 3 and 4; and the number of job leavers (normalized by employer size) in columns 5 and 6. For each specification, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects. 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.

VARIABLES	(1) Small ln(Salary)	(2) Large ln(Salary)	(3) Small New Hire	(4) Large New Hire	(5) Small Leave	(6) Large Leave
Female × Post	0.0002	-0.0154***	-0.0033***	-0.0013***	-0.0007	-0.0019***
	(0.0045)	(0.0025)	(0.0007)	(0.0004)	(0.0005)	(0.0002)
Observations	90,127	222,687	392,016	399,378	352,580	361,088
R-squared	0.8758	0.8509	0.6293	0.6634	0.5084	0.6128
Benchmark	3537.49	3777.574	0.0628	0.0421	0.0324	0.0283
Relative Effect	0.0002	-0.0153	-0.0525	-0.0309	-0.0216	-0.0671
Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer-year FE	YES	YES	YES	YES	YES	YES
Employer-quarter FE	YES	YES	YES	YES	YES	YES
Female-quarter FE	YES	YES	YES	YES	YES	YES

Table 11. Heterogeneous Effect: by Employer Sector

Note: This table explores the effect of the relaxation of OCP change on new female's labor market outcomes by employer sector. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in Panel A; the number of new hires (normalized by employer size) in Panel B; and the number of job leavers (normalized by employer size) in Panel C. For each specification, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects. 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.

	(1)	(2)	(3)	(4)
	Public Sector	SOE	POE	Joint Venture
Panel A	ln(Salary)	ln(Salary)	ln(Salary)	ln(Salary)
Female × Post	-0.0056	-0.0299***	-0.0080***	-0.0140**
	(0.0073)	(0.0091)	(0.0029)	(0.0063)
Observations	29,496	21,016	168,095	45,449
R-squared	0.8529	0.8250	0.8452	0.8438
Benchmark	5403.929	3661.256	3047.600	4521.209
Relative Effect	-0.0056	-0.0295	-0.0080	-0.0139
Panel B	New Hire	New Hire	New Hire	New Hire
Female × Post	-0.0019***	-0.0020**	-0.0023***	-0.0018*
	(0.0005)	(0.0010)	(0.0007)	(0.0009)
Observations	141,266	59,614	365,964	90,978
R-squared	0.5565	0.5931	0.6194	0.5556
Benchmark	0.0210	0.0297	0.0750	0.0495
Relative Effect	-0.0905	-0.0673	-0.0307	-0.0364
Panel C	Leave	Leave	Leave	Leave
Female × Post	-0.0004	-0.0014**	-0.0016***	-0.0006
	(0.0003)	(0.0006)	(0.0005)	(0.0007)
Observations	129,068	54,296	326,156	82,918
R-squared	0.4764	0.4804	0.5071	0.4905
Benchmark	0.0099	0.0176	0.0412	0.0351
Relative Effect	-0.0404	-0.0795	-0.0388	-0.0171
Year-quarter FE	YES	YES	YES	YES
Employer-year FE	YES	YES	YES	YES
Employer-quarter FE	YES	YES	YES	YES
Female-quarter FE	YES	YES	YES	YES

Table 12. Heterogeneous Effect: by Industry

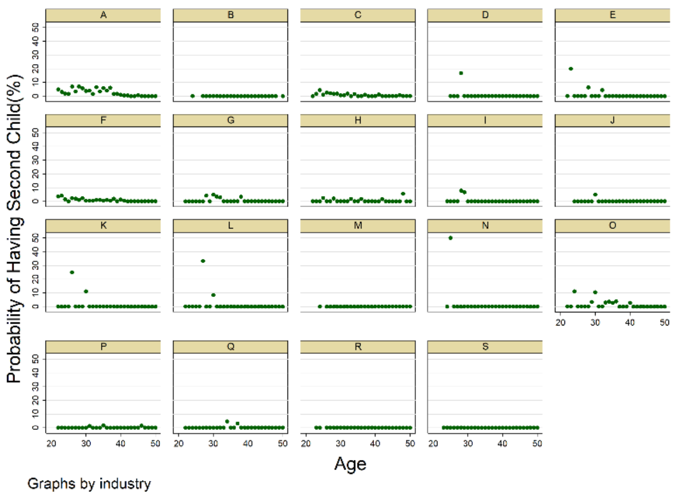
Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes by industry. Following Olivetti and Petrongolo (2014) and Rendall (2017), according to their industries, we divide the employers into "brawn" (containing agriculture, forestry, animal husbandry and fishing, mining, manufacturing, electricity, heat, gas and water production and supply, and construction) ,and "brain" (including all service industries and the public sectors) industries. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in columns 1 and 2; the number of new hires (normalized by employer size) in columns 3 and 4; and the number of job leavers (normalized by employer size) in columns 5 and 6. For each specification, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects. 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.

VARIABLES	(1) Brawn ln(Salary)	(2) Brain ln(Salary)	(3) Brawn New Hire	(4) Brain New Hire	(5) Brawn Leave	(6) Brain Leave
Female × Post	-0.0094	-0.0145***	0.0039***	-0.0003	-0.0010	-0.0011***
	(0.0113)	(0.0040)	(0.0014)	(0.0005)	(0.0010)	(0.0003)
Observations	13,219	101,390	30,012	289,566	27,388	264,268
R-squared	0.8080	0.8528	0.5703	0.5766	0.4687	0.5218
Benchmark	3318.255	4202.820	0.0337	0.0380	0.0206	0.0219
Relative Effect	-0.0094	-0.0144	0.1157	-0.0079	-0.0485	-0.0502
Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer-year FE	YES	YES	YES	YES	YES	YES
Employer-quarter FE	YES	YES	YES	YES	YES	YES
Female-quarter FE	YES	YES	YES	YES	YES	YES

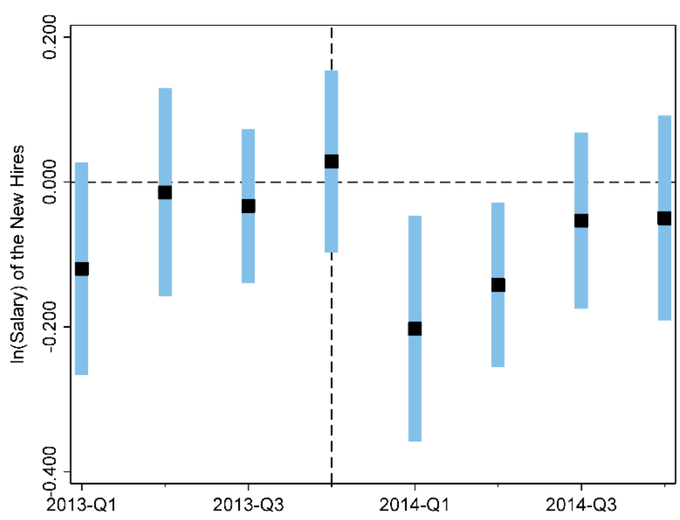
Appendix

Figure A1. Distribution of Exposure and Event Study

Note: Panel A of this figure shows the distribution of the probability of having a second child in 2010 conditional on having one child in 2009 by age and industry. The probability is calculated based on the 2010 census by the authors. Panel B of the figure plots the event study of the diff-in-diffs using the probability as a proxy for treatment intensity of the policy change. We control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and age-quarter fixed effects. Robust standard errors are clustered at employer level. The bars refer to the 95 percent confidence intervals.



A. Distribution of the Probability of Having a Second Child



B. Event Study

Table A1. Effect of Sample Restrictions on Sample Size

Note: This table reports the impact of each of our sample processing procedures on the monthly raw dataset. The data includes all the deposit records in the local Housing Provident Fund system from 2012 to 2014.

Processing Procedure	Sample Size			# of
	Full Sample	New Hire	Job Leaver	Employers
Raw dataset	138,532,302	3,487,133	2,806,164	104,029
1. Drop employees with salary beyond the reasonable range	110,274,396	2,950,286	2,254,287	98,141
2. Drop employees more than 50 years old	100,880,117	2,904,699	2,134,971	97,534
3. Drop employers with less than 5 employees	94,990,177	2,597,581	1,897,563	46,295
4. Require that the ratio between the number of new hires by gender and the employer size (i.e., number of total employees) is no more than 1.0 every month; the number of new hires by gender is no more than 100 every month	76,732,790	1,866,117	1,452,907	45,146
5. Require that the ratio between job leavers by gender and the number of total employees is no more than 0.5 every month	73,512,543	1,737,274	1,217,647	41023
6. Require that all employers have hiring records in consecutive years	72,373,962	1,723,991	1,206,145	39,529
7. Require that all employers have consecutive employment records in consecutive years	72,373,543	1,723,952	1,206,113	39,525
Working dataset	72,373,543	1,723,952	1,206,113	39,525

Table A2. Polynomial Choice by AMSE

Note: Following Zimmerman (2019), this table reports the sample average mean squared errors from 500-fold cross-validation estimates by bandwidth and polynomial degree.

Panel A. Salary of New Hires		Polynomial degree		
Gender	Bandwidth	Degree 1	Degree 2	Degree 3
Female	3	0.317424	0.317457	**0.317300**
	4	0.339790	0.339741	**0.339445**
Male	3	0.391498	0.391478	**0.391206**
	4	**0.388161**	0.388442	0.388382

Panel B. New Hires				
Gender	Bandwidth	Degree 1	Degree 2	Degree 3
Female	3	**0.012826**	0.012834	0.012830
	4	**0.012667**	0.012669	0.012718
Male	3	**0.015721**	0.015761	0.015732
	4	**0.012464**	0.012516	0.012479

Panel C. Job Quitters				
Gender	Bandwidth	Degree 1	Degree 2	Degree 3
Female	3	0.004388	0.004389	**0.004385**
	4	0.004812	0.004812	**0.004812**
Male	3	0.004998	0.004999	**0.004996**
	4	**0.004937**	0.004938	0.004939

Table A3. Summary Statistics: Survey Data

Note: This table reports the summary statistics of the CFPS survey dataset. The individual-year level balanced panel data involve three waves in 2010, 2012 and 2014. We require that the individuals work in the urban areas and aged from 23 to 35 in 2013, while the self-employed individuals are excluded. The respondents have either one child or no child in 2012 in Panel A, and one child in 2012 in Panel B. The daily working hours are winsorized at 1 and 99 percentage level.

Panel A	Obs	Mean	Std. Dev.	Min	Max
Working Hours	2,706	5.6761	2.7085	1.1429	12.2301
Female	2,706	0.4834	0.4998	0	1
Married	2,706	0.6707	0.4700	0	1
Children	2,706	0.5628	0.4961	0	1
Age	2,706	28.2764	4.0390	20	36
Panel B	Obs	Mean	Std. Dev.	Min	Max
Working Hours	1,542	5.9241	2.7725	1.1429	13.8082
Female	1,542	0.5370	0.4988	0	1
Married	1,542	0.9514	0.2152	0	1
Children	1,542	0.9222	0.2680	0	1
Age	1,542	30.1005	3.3988	20	36

Table A4. Robustness Check: Labor Effort Change using Different Age Cohort

Note: This table explores the effect of the relaxation of OCP on female's labor effort using the survey dataset. The individual-year level balanced panel data involve three waves in 2010, 2012 and 2014. We require that the individuals work in the urban areas and aged from 23 to 30 in 2013, while the self-employed individuals are excluded. The respondents have either one child or no child in 2012 in Panel A, and one child in 2012 in Panel B. The outcomes are daily working hours in columns 1 and 2, and daily working hours (in natural log) in columns 3 and 4. The daily working hours are winsorized at 1 and 99 percentage level. For each specification, we control for year fixed effects and individual fixed effects. We further control for age, dummy variable for marriage, and dummy variable for children in columns 2 and 4. 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.

	(1)	(2)	(3)	(4)
Panel A	Working Hours	Working Hours	ln(Working Hours)	ln(Working Hours)
Female × Post	0.0499	-0.0160	0.0332	0.0183
	(0.2446)	(0.2455)	(0.0539)	(0.0543)
Observations	1,581	1,581	1,581	1,581
R-squared	0.4683	0.4732	0.4603	0.4653
# of Child in 2012	0/1	0/1	0/1	0/1
Panel B	Working Hours	Working Hours	ln(Working Hours)	ln(Working Hours)
Female × Post	0.1991	0.1637	0.0963	0.0913
	(0.4877)	(0.4911)	(0.1027)	(0.1033)
Observations	588	588	588	588
R-squared	0.4197	0.4209	0.4121	0.4129
# of Child in 2012	1	1	1	1
Demographic	NO	YES	NO	YES
Individual FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Age in 2013	23-30	23-30	23-30	23-30
Sample Period	2010-2014	2010-2014	2010-2014	2010-2014

Table A5. Robustness Check: Married and Unmarried Sample

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes by employee's marriage status. The sample period is from 2012 to 2014. The dataset is aggregated to individual-employer-quarter level. In order to ensure the married and unmarried sample comparable, we restrict the sample to age 22-30. The outcome is the salary (in natural log) of the new hires. For each specification, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects, female-quarter fixed effects and demographic characteristics, including age and birth place. 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.

	(1)	(2)
	Married	Unmarried
VARIABLES	ln(Salary)	ln(Salary)
Female × Post	-0.0467	-0.0179***
	(0.0320)	(0.0023)
Observations	6,933	1,140,495
R-squared	0.8384	0.7462
Demographic Attributes	YES	YES
Year-quarter FE	YES	YES
Employer-year FE	YES	YES
Employer-quarter FE	YES	YES
Female-quarter FE	YES	YES
Age	22-30	22-30

Table A6. Robustness Check: Using Different Fixed Effects

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes using different fixed effects. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in columns 1 and 2; the number of new hires (normalized by employer size) in columns 3 and 4; and the number of job leavers (normalized by employer size) in columns 5 and 6. For each specification, we control for year-quarter fixed effects and female-quarter fixed effects. We also control for employer-year fixed effects in columns 1, 3 and 5, and employer-quarter fixed effects in columns 2, 4 and 6. 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.

VARIABLES	(1) ln(Salary)	(2) ln(Salary)	(3) New Hire	(4) New Hire	(5) Leave	(6) Leave
Female × Post	-0.0134***	-0.0128***	-0.0023***	-0.0023***	-0.0013***	-0.0013***
	(0.0022)	(0.0022)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Observations	344,606	332,390	791,394	791,394	713,668	713,668
R-squared	0.7857	0.7959	0.4266	0.5091	0.3630	0.4247
Benchmark	3695.472	3695.472	0.0523	0.0523	0.0303	0.0303
Relative Effect	-0.0133	-0.0127	-0.0440	-0.0440	-0.0429	-0.0429
Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer-year FE	YES	NO	YES	NO	YES	NO
Employer-quarter FE	NO	YES	NO	YES	NO	YES
Female-quarter FE	YES	YES	YES	YES	YES	YES

Table A7. Robustness Check: Weighted by Employer Size by Gender

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes using weighted OLS regression by gender-specific employer size. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in columns 1 and 2; the number of new hires (normalized by employer size) in columns 3 and 4; and the number of job leavers (normalized by employer size) in columns 5 and 6. We control for year-quarter fixed effects and employer fixed effects in columns 1, 3 and 5, while we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects in columns 2, 4 and 6. 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.

VARIABLES	(1) ln(Salary)	(2) ln(Salary)	(3) New Hire	(4) New Hire	(5) Leave	(6) Leave
Female × Post	-0.0231***	-0.0185***	0.0006	-0.0008***	-0.0017***	-0.0019***
	(0.0041)	(0.0038)	(0.0003)	(0.0002)	(0.0003)	(0.0002)
Observations	356,057	318,533	785,947	785,412	708,840	708,351
R-squared	0.7521	0.8680	0.3626	0.7082	0.4011	0.6631
Benchmark	3695.472	3695.472	0.0523	0.0523	0.0303	0.0303
Relative Effect	-0.0228	-0.0183	0.0115	-0.0153	-0.0561	-0.0627
Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer FE	YES	NO	YES	NO	YES	NO
Employer-year FE	NO	YES	NO	YES	NO	YES
Employer-quarter FE	NO	YES	NO	YES	NO	YES
Female-quarter FE	NO	YES	NO	YES	NO	YES

Table A8. Robustness Check: Using Different Frequency of Aggregation

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes based on the datasets aggregated to different frequency. The sample period is from 2012 to 2014. The dataset is aggregated to employer-month level by gender in columns 1, 2 and 3, and aggregated to employer-semi year level by gender in columns 4, 5 and 6. The outcome is the salary (in natural log) of the new hires in columns 1 and 4; the number of new hires (normalized by employer size) in columns 2 and 5; and the number of job leavers (normalized by employer size) in columns 3 and 6. For each specification, we control for year-period fixed effects, employer-year fixed effects, employer-period fixed effects and female-period fixed effects. 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.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ln(Salary)	New Hire	Leave	ln(Salary)	New Hire	Leave
Female × Post	-0.0102***	-0.0008***	-0.0005***	-0.0111***	-0.0039***	-0.0019***
	(0.0021)	(0.0001)	(0.0001)	(0.0023)	(0.0008)	(0.0005)
Observations	466,761	2,335,202	2,116,714	221,798	401,402	323,472
R-squared	0.8341	0.5368	0.4033	0.8789	0.6889	0.6622
Benchmark	3712.258	0.0175	0.0102	3507.076	0.0983	0.0592
Relative Effect	-0.0101	-0.0457	-0.0490	-0.0110	-0.0397	-0.0321
Year-period FE	YES	YES	YES	YES	YES	YES
Employer-year FE	YES	YES	YES	YES	YES	YES
Employer-period FE	YES	YES	YES	YES	YES	YES
Female-period FE	YES	YES	YES	YES	YES	YES
Period	Month	Month	Month	Half Year	Half Year	Half Year

Table A9. Robustness Check: Using Different Employer Size Cutoff

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes based on the dataset excluding the employers with less than 3 employees. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in columns 1 and 2; the number of new hires (normalized by employer size) in columns 3 and 4; and the number of job leavers (normalized by employer size) in columns 5 and 6. We control for year-quarter fixed effects and employer fixed effects in columns 1, 3 and 5, while we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects in columns 2, 4 and 6. 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.

VARIABLES	(1) ln(Salary)	(2) ln(Salary)	(3) New Hire	(4) New Hire	(5) Leave	(6) Leave
Female × Post	-0.0129***	-0.0106***	-0.0023***	-0.0022***	-0.0010***	-0.0010***
	(0.0020)	(0.0021)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Observations	387,244	339,677	966,450	966,450	868,676	868,676
R-squared	0.7222	0.8569	0.2800	0.6025	0.2279	0.5014
Benchmark	3692.540	3695.472	0.0548	0.0548	0.0306	0.0306
Relative Effect	-0.0128	-0.0105	-0.0420	-0.0401	-0.0327	-0.0327
Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer FE	YES	NO	YES	NO	YES	NO
Employer-year FE	NO	YES	NO	YES	NO	YES
Employer-quarter FE	NO	YES	NO	YES	NO	YES
Female-quarter FE	NO	YES	NO	YES	NO	YES

Table A10. Robustness Check: Excluding the Fourth Quarter

Note: This table explores the effect of the relaxation of OCP on female's labor market outcomes based on the dataset excluding the observations in the fourth quarter. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender. The outcome is the salary (in natural log) of the new hires in columns 1 and 2; the number of new hires (normalized by employer size) in columns 3 and 4; and the number of job leavers (normalized by employer size) in columns 5 and 6. We control for year-quarter fixed effects and employer fixed effects in columns 1, 3 and 5, while we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects and female-quarter fixed effects in columns 2, 4 and 6. 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.

VARIABLES	(1) ln(Salary)	(2) ln(Salary)	(3) New Hire	(4) New Hire	(5) Leave	(6) Leave
Female × Post	-0.0122***	-0.0099***	-0.0023***	-0.0023***	-0.0013***	-0.0013***
	(0.0024)	(0.0025)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Observations	270,245	239,370	585,500	585,500	585,500	585,500
R-squared	0.7290	0.8622	0.3241	0.6377	0.2742	0.5312
Benchmark	3715.432	3715.432	0.0532	0.0532	0.0309	0.0309
Relative Effect	-0.0121	-0.0099	-0.0432	-0.0432	-0.0421	-0.0421
Year-quarter FE	YES	YES	YES	YES	YES	YES
Employer FE	YES	NO	YES	NO	YES	NO
Employer-year FE	NO	YES	NO	YES	NO	YES
Employer-quarter FE	NO	YES	NO	YES	NO	YES
Female-quarter FE	NO	YES	NO	YES	NO	YES

Table A11. Composition Change

Note: This figure reports the composition change before and after the policy. There are three types of employers, including the employers exist before and after the policy, only exist before the policy and only exist after the policy.

		Before & After	Only Before	Only After
Total Number		33,236	354	5,935
Female %		0.4958	0.5012	0.4583
Employer's Sector	Public	5,915	13	191
	POE	15,440	243	4,684
	SOE	2,493	11	175
	Joint Venture	3,821	45	242

Table A12. Interaction of Treatment Effect with Industry Characteristics and Age Cohorts

Note: This table reports the coefficients of the interaction of treatment effect (i.e. Female × Post in main results) with industry characteristics and age cohorts. The sample period is from 2012 to 2014. The dataset is aggregated to employer-quarter level by gender and age cohorts. The outcome is the number of new hires (normalized by employer size). All the results come from the same regression. In this specification, we control for year-quarter fixed effects, employer-year fixed effects, employer-quarter fixed effects, female-quarter fixed effects, female-brain-age cohorts fixed effects and post-brain-age cohorts fixed effects. 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.

	Dependent Variable: New Hire					
	22-25	26-30	31-35	36-40	41-45	46-50
Substitution: Brain	-0.0027***	-0.0002	-0.0001	-0.0001	-0.0004	0.0017
	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0010)	(0.0018)
Substitution: Brawn	0.0040**	0.0017	-0.0010	0.0001	0.0005	0.0075**
	(0.0017)	(0.0016)	(0.0016)	(0.0020)	(0.0022)	(0.0034)
Observations	240,716					
R-squared	0.6617					
Controls	Year-quarter FE Employer-year FE Employer-quarter FE Female-quarter FE Female-brain-age cohorts FE Post-brain-age cohorts FE					