Discussion Paper Series

IZA DP No. 18732

June 2026

China's One-Child Exemptions and the Spousal Age Gap

Jiani Gao
Independent Researcher

Min Sun Park
Binghamton University

Solomon W. Polachek
Binghamton University
and IZA@LISER

IZA@LISER NETWORK
ISSN: 2365-9793

IZA DP No. 18732 June 2026

China’s One-Child Exemptions and the Spousal Age Gap*

Abstract

This paper examines the impact of exemptions to China’s 1979 One- Child Policy on fertility and the spousal age gap. The exemptions, implemented at different times across provinces, created exogenous variation that we exploit using a staggered Difference-in-Differences design. Our analysis uses the 2010 China Family Panel Studies and focuses on couples with rural hukou status, who were most affected by these exemptions during the study period. We find that the exemptions led to a 32% increase in fertility, consistent with the policy’s pro-natalist intent. We also observe a 25% increase in the spousal age gap, likely driven by the increased demand for fertility following the policy change. These effects run counter to the broad international trend over this period, in which fertility rates have generally declined and spousal age gaps have narrowed. We additionally find suggestive evidence that the exemptions are associated with a more traditional household division of labor, with possible implications for the gender wage gap.

JEL classification

J12, J13, J16, O53

Keywords

China, one-child policy exemptions, fertility, spousal age gap, household division of labor

Corresponding author

Solomon W. Polachek
polachek@binghamton.edu

* *Funding*: The authors declare that no funds, grants, or other support were received during the preparation of this manuscript.
Competing Interests: The authors have no relevant financial or non-financial interests to disclose.
Author Contributions: Jiani Gao, Min Sun Park, and Solomon W. Polachek each contributed equally to the conceptualization, analysis, writing, and editing of the manuscript. Additionally, Jiani Gao translated and compiled data from the Family Planning Commission, originally written in Chinese, into English.
Acknowledgments: We are grateful to Mei-Hsieu Chen, David Slichter, and Sulagna Mookerjee, as well as to the editor and three anonymous referees for their thoughtful comments, which substantially improved the paper. The paper also benefited substantially from feedback obtained at various conferences and workshops. All remaining errors are our own.

1 Introduction

This paper examines the causal impact of exemptions to China's One-Child Policy (OCP) on fertility and spousal age gaps, focusing particular attention on resulting changes in the age difference between husbands and wives. Although the OCP was officially enacted in 1979, exemptions began to be introduced in 1982 and gradually expanded until the 2015 nationwide reform, which allowed all couples to have two children if they preferred.

Global data indicate that husbands are typically older than their wives, with age gaps decreasing from over 8 years in the lowest-income countries to approximately 2 years in the highest-income countries (United Nations, 2000; Zhang, 2014; Feng and Ren, 2022). Several marriage market models attempt to explain this pattern (Vella and Collins, 1990; Siow, 1998; Giolito, 2004; Hamilton and Siow, 2007; Díaz-Giménez and Giolito, 2013). They argue that fecundity constraints, namely the shorter window during which women can conceive compared to men, shape partner preferences. Men tend to prefer younger women to maximize the chances of having children, while women prefer older men who are likely to have the financial resources to better support a family. As a result, both genders develop preferences that reinforce an age gap between the spouses. The greater the demand for children, the more the incentive for men to marry younger women, and for women to marry older men.

Based on this reasoning, we hypothesize that the age differential between newly married husbands and wives increases as the demand for children rises. Cross-country evidence supports this hypothesis. The spousal age gap is positively associated with the demand for children, which tends to be higher in lower-income countries where children play a greater role in economic activities (Zhang, 2014). Consistent with this, Feng and Ren (2022) show that the spousal age gap decreases with GDP per capita even after controlling for polygynous unions. Among monogamous countries, the gap ranges from approximately 2 years in high-income countries such as the United Kingdom (Office for National Statistics, 2021) to over 4 years in low-income countries such as Nepal (Feng and Ren, 2022).

Relatedly, Low (2024) conceptualizes fecundity as "reproductive capital" arguing that women who delay marriage and childbirth to invest in their careers reduce their reproductive capital over time. In an online experiment, Low (2024) finds that men who are aware of the age-fertility trade-off have a strong preference for younger women. Consistent with this finding, Gershoni and Low (2021) show that a 1994 Israeli policy offering free in vitro fertilization treatments enabled women to delay childbearing, thereby increasing the marriage prospects of older women and narrowing the spousal age gap.

In line with this literature, Li and Shi (2025) show that the OCP lowered the probability of having a second child. Lin et al. (2025) show that growing up under the OCP, and thereby being exposed to a small-family culture, led Chinese immigrants in the U.S. to have fewer children as adults. Zhang (2023) further shows that under the OCP, families with first-born daughters had lower fertility when a parent worked in the public sector, where enforcement tended to be stricter. Public-sector workers who violated the policy faced salary reductions, disciplinary sanctions, demotion, or even dismissal, making the costs of non-compliance considerably higher than for their private-sector counterparts. This stricter enforcement was also linked to higher maternal labor supply, particularly when husbands worked in the public sector. Several other studies also found that stricter fertility restrictions improved female empowerment by raising women's educational attainment, labor market participation, and delaying marriage (Huang et al., 2021; Fang, 2023). Additional research explored the broader consequences of the OCP, including its impact on the sex ratio imbalance (Li et al., 2011), household savings rates (Wei and Zhang, 2011), crime rates (Edlund et al., 2013), and the "missing women" phenomenon (Bulte et al., 2011).

Huang et al. (2025) examine how the OCP distorted marriage market outcomes and estimate the associated welfare loss, exploiting exemptions granted to ethnic minorities as a source of variation. While their study also exploits OCP exemptions, they focus on marriage formation across ethnic groups rather than on spousal age differentials within marriages. Among the studies examining spousal age gaps, the most relevant to our work are Polachek et al. (2015) and Han and Zhao (2022). However, they both examine the impact of the OCP itself, rather than on exemptions from the policy. Polachek et al. (2015) employs a Difference-in-Differences strategy to show that the OCP reduced demand for children, which led to a narrowing of the spousal age and educational gaps. In contrast, our results show that higher fertility, permitted under the exemptions, is associated with a wider spousal age gap.

On the other hand, Han and Zhao (2022) find that the OCP increased the spousal age gap. They attribute this to the sex ratio imbalance resulting from the interaction between the OCP and son preference, which created a surplus of males and intensified competition in the marriage market. Using a Difference-in-Differences strategy that exploits the differential application of the OCP between Han Chinese and ethnic minorities, they show that the age differential increased for male-headed families. While both their study and ours find an increase in the spousal age gap, the mechanisms differ. Han and Zhao (2022) identify a channel in which a shortage of women drives male competition, whereas our study identifies a channel in which increased fertility demand under the exemptions raises the value of younger wives in the marriage market. Our study also differs in sample. Han and Zhao (2022) focus on cohorts born after 1978, who were directly exposed to the policy-induced sex ratio imbalance, whereas the majority of our sample comprises individuals born prior to the OCP, minimizing this influence.

We exploit the staggered rollout of exemptions to the OCP as a natural experiment, leveraging the geographic and temporal variation in exemption implementations across provinces using a staggered Difference-in-Differences (DiD) framework. This policy change is particularly noteworthy because, even as fertility rates declined and spousal age gaps narrowed worldwide, the exemptions produced significant effects in the opposite direction among affected couples. Our DiD estimates capture the extent to which treated couples diverged from the common trend. The results suggest that relaxing birth restrictions partially offset the ongoing decline in fertility and the convergence in spousal ages within the treated population, with broader implications for gender roles and the division of labor within the household.

We find that exemptions increased fertility by 32%. Additionally, the spousal age gap increased by 0.42 years, a 25% increase. Decomposing this effect, we find that the widening age gap is driven largely by a decline in wives' age at marriage, while husbands' age at marriage remains largely unchanged. This asymmetric pattern supports the hypothesis that increased fertility demand raises the relative value of younger wives.

We address identification concerns of our main analysis through a Goodman-Bacon (2021) decomposition, providing evidence that our estimates are not driven by problematic comparisons (Section 6). We further implement a Sun and Abraham (2021) heterogeneity-robust estimator for all analyses, including the spousal age gap, its decomposition into husbands' and wives' age at marriage, fertility, and household division of labor. A heterogeneity analysis by joint age at marriage further shows that the response to the exemption is driven by the broad majority of couples. Those who married with an older husband and a younger wife, a group that already had the widest spousal age gap and the highest fertility before the policy, respond little to the exemption, and excluding them yields a more precisely estimated effect on the spousal age gap, consistent with the fertility-demand mechanism. Our results contrast with trends in many countries where spouses are becoming more similar in age and education, and suggest that increased fertility widens the spousal age gap with broader implications for the gender wage gap and household division of labor.

The implications for household structure and gender dynamics are both significant and policy-relevant. Being older at the time of marriage often means having accumulated more human capital and earnings power. Older husbands typically enter marriage with higher wages, which translates to more bargaining power within the household and reinforces the traditional division of labor. This dynamic is closely tied to the well-documented disruptions in women's labor force activities during childbearing and child-rearing periods (Mincer and Polachek, 1974; Polachek, 1981; Blau and Kahn, 2013, 2017; Harkness and Waldfogel, 2003; Pal and Waldfogel, 2016; Juhn and McCue, 2017; Hamermesh, 2022). Within such division of labor, men tend to work more consistently over the life cycle compared to wives (Grossbard, 2014). Consequently, men tend to invest relatively more in human capital, often by taking jobs requiring more on-the-job training, less flexibility, longer commutes, and greater travel (Goldin, 2014). These investments raise a husband's wage compared to his wife's and widen the husband-wife age gap over the course of a traditional marriage. We provide suggestive evidence consistent with this prediction in Section 7.

To our knowledge, this is the first rigorous empirical study to examine the marital impacts of China's OCP exemptions. Previous research primarily focused on the effects of the nationwide Two-Child Policy in 2015. These studies explored outcomes such as fertility (Zeng and Hesketh, 2016), household savings (Tan and Liu, 2020), labor market impacts (He et al., 2023), economic growth (Zheng, 2016), and population aging (Wang et al., 2017). Zeng and Hesketh (2016) predict that the 2015 Two-Child Policy will reduce abortions, eliminate the problem of unregistered children, and contribute to a more normal sex ratio. Tan and Liu (2020) argue that having a second child is associated with reduced household saving rates. He et al. (2023) examine how the policy raised employers' expectations that women of childbearing age would leave the workforce due to childbearing and childcare responsibilities, contributing to greater labor market discrimination against women in hiring. Zheng (2016) argues that the OCP was a driver of population ageing, and that the universal Two-Child Policy is significant for relieving the subsequent socio-economic pressure and promoting economic growth. Finally, Wang et al. (2017) use simulations to show that the aging population issue may persist, but the Two-Child Policy helped normalize the previously abnormal sex ratio at birth. However, most of these studies rely on the 2015 nationwide Two-Child Policy and overlook the role of earlier exemptions, which already allowed many couples, particularly in rural areas, to have a second child. As a result, analyses that treat the 2015 nationwide policy as the sole intervention may suffer from bias since a large portion of the population had already been treated under the earlier exemptions.

The rest of the paper is organized as follows: Section 2 provides background on China's fertility policies. Section 3 describes the data. Section 4 outlines the empirical strategy. Section 5 presents the results. Section 6 provides robustness checks. Section 7 provides suggestive evidence on the policy's implications for household division of labor. Finally, Section 8 concludes.

2 Fertility Policy in China

China is unique among nations for its pro- and anti-natalist policies. While fertility rates have generally declined worldwide, China's fertility increased during the 1950s and 1960s under Chairman Mao's pro-natalist policies. However, China's fertility rate declined sharply based on anti-natalist policies from the 1970s through the 1980s. This anti-natalist program culminated in 1979 with China's OCP, which was enforced using a "second-child penalty" at the provincial level. This policy was strictly applied across all provinces, with the exception of the minority autonomous regions, which we exclude from our analysis.

In 1982, China gradually began to relax the OCP. According to the *Law of the People's Republic of China on Population and Family Planning*, these exemptions were administered at the provincial level, rather than at smaller local prefecture levels. These provinces designed their own fertility rules and granted permits for second children based on specific criteria. For example, rural couples whose first child was a girl were often allowed to have a second child.

The number of provinces establishing exemptions for the OCP increased over time. The exemption process involved three common criteria applied to Han majority couples: (1) couples with rural hukou whose first child was a girl; (2) couples with rural hukou in which one spouse was an only child; and (3) couples where both spouses were only children. As we discuss in Section 3, our main analysis restricts attention to Policy 1, which accounts for the vast majority of treated observations in our sample.

Table 1: Years One-Child Exemptions by Provinces Initiated

Province	Policy 1	Policy 2	Policy 3
Anhui	1988	1984	1984
Beijing		1984	1984 (rural 1983)
Chongqing			1984
Fujian	1991	2002	1984
Gansu	1997		2002
Guangdong	1982		1984
Guangxi	1985		1985
Guizhou	1998		1984
Hainan	1982		1984
Hebei	1989		1984 (rural 1982)
Heilongjiang	1990		1984 (rural 1983)
Henan	1990		
Hubei	1987		2002
Hunan	1987		1984
Jiangsu		1985	1984
Jiangxi	1990		1990 (rural 1983)
Jilin	1988	2002	1984
Liaoning	1985	1984	1984
Neimeng	1988		2002
Ningxia			1986
Qinghai			1986
Shandong	1986		1984 (rural 1982)
Shanghai		1990	1984
Shanxi	1989		1986 (rural 1982)
Shaanxi	1991		1986
Sichuan			1984
Tianjin		1984	1984 (rural 1983)
Xinjiang			1992
Yunnan			1990
Zhejiang	1985		1985 (rural 1984)

Notes: The table shows the three most common one-child exemptions applied to the Han majority. Policy 1: rural couples with only one girl; Policy 2: rural couples with one spouse being a single child; Policy 3: both spouses are single children.

Eventually, most provinces implemented some or all of these criteria through the OCP exemption process. By 1990, two provinces had all three exemptions, 14 provinces had two, and 12 provinces had adopted one. By 2000, all 30 provinces had at least one exemption type. There was also considerable timing variation in the two child permits even within the provinces.

One concern is that couples may have migrated across provinces to access more favorable exemptions. We address this in Section 4 using CFPS information on respondents' hukou and birth provinces. Second, couples could, in principle, change their hukou type within a province. However, since the exemptions in our study apply exclusively to rural hukou holders, exploiting them would require converting from an urban to a rural hukou. Under the hukou system, conversion is overwhelmingly from rural to urban, as urban hukou provides substantially greater access to public services (Afridi et al., 2015). Voluntary conversion to rural status is therefore unlikely.

We obtain second child permit data that outline birth policies from original Family Planning Commission documents written in Chinese, which are more complete and less ambiguous than English translations. Our classification is based on the three criteria applied to the Han majority previously explained. Table 1 summarizes the timing of each type of exemption in each province between 1980 and 2010. Our analysis focuses on Policy 1, the firstborn daughter exemption, which was adopted at different times across provinces. For example, Shaanxi passed this exemption in 1991, while Jiangxi adopted it in 2002. The staggered timing of adoption across provinces provides the identifying variation for our empirical strategy.

3 Data

To exploit this variation empirically, we draw on demographic data from the 2010 CFPS. The CFPS is a nationally representative survey of Chinese communities, families, and individuals put together by the Institute of Social Science Survey (ISSS) of Peking University. Information covers characteristics such as economic activities, educational outcomes, family relationships, migration, and health. CFPS has advantages over other datasets due to its national representativeness (e.g., the China Health and Nutrition Survey only covers 15 provinces) and has information on both spouses' siblings. Neither the China Health and Nutrition Survey nor the Chinese population censuses have such necessary information to determine eligibility for the OCP exemptions. To do so, we incorporate individual-level demographic information from the 2010 CFPS baseline survey, including age, education, ethnicity, household registry (hukou) status, number of siblings, year of marriage, spousal characteristics, and number of children.

First, because the OCP exemptions did not apply to the national minorities, we limit the sample to Han-majority couples married between 1980 and 2010. Second, we restrict the sample to ever-treated couples, since every province in our data eventually implemented the exemptions. This means we have no untreated provinces to serve as controls. Couples married in provinces that adopted the exemption in later years serve as the comparison group for each treatment cohort. This method ensures that both treatment and comparison groups are comparable, as all couples were eventually eligible for the policy under one of the three exemption categories. Additionally, we restrict the sample to couples in their first marriage and exclude interprovincial couples and couples with mixed hukou status (e.g., one rural and one urban), as these groups were governed by different fertility policies.

Li et al. (2011) observe that the impact of the OCP on sex ratios tends to level off in the long run, possibly due to the increasing influence of the OCP exemptions prevalent. This suggests the OCP may still affect our outcomes, potentially biasing our estimates. To reduce this confounding effect, especially around the time just before the exemptions were introduced, we exclude observations from provinces that adopted the exemption in 1985 or earlier. Additionally, because the gender of a firstborn child is unknown prior to birth, we treat all rural couples residing in provinces exposed to the "first-girl exemption" as treated, regardless of the actual gender of their first child. Finally, we exclude couples with an urban hukou because of the significant demographic and socioeconomic differences between rural and urban hukou holders, as well as the small number of urban hukou holders in the sample. This results in 2,150 rural Han-majority couples.

Upon examining our sample composition after the restrictions, we find that of the 2,150 ever-treated couples, 2,126 are affected by Policy 1 (rural couples with a first-born girl), 17 are affected by Policy 2 (rural couples with one spouse being an only child), and 7 are affected by Policy 3 (both spouses being only children). The low prevalence of Policy 2 and Policy 3 in our sample is primarily due to the restrictive nature of their eligibility criteria. Policy 2 requires at least one spouse to be a single child, and Policy 3 requires both spouses to be single children. Given that the average number of siblings in our sample is approximately three for both husbands and wives (Table 2), very few couples meet these criteria. The 1985 cutoff further reduces the number of eligible observations, as many provinces that adopted Policy 2 or Policy 3 did so by 1985 or earlier, and observations eligible under these early-adopted policies are excluded from our analysis.[1]

Furthermore, our treatment assignment accounts for the sequential adoption of multiple exemptions within a province. When a province adopted more than one exemption type at different times, couples are assigned to the policy under which they first become eligible. For example, if a province introduced Policy 2 before Policy 1, couples meeting the Policy 2 criteria are classified as treated under Policy 2 at the time of its introduction, while couples who do not meet the Policy 2 criteria but later become eligible under Policy 1 are classified as treated under Policy 1 at the time of its adoption. This sequential assignment ensures that each couple is assigned to a single, distinct policy treatment and that the estimated effects of each policy are not confounded by overlap with other exemption types.

Additionally, since the OCP exemptions are legally determined by hukou status rather than physical residence, we define treatment eligibility based on rural hukou registration. While some rural hukou holders may reside in urban areas, their birth planning policies remain covered by their registered hukou province. Table C5 provides further support, showing no statistically significant difference in treatment effects between agricultural and non-agricultural

[1] Even without the 1985 restriction, over 93% of ever-treated observations fall under Policy 1, confirming that the dominance of Policy 1 reflects the demographic composition of the sample rather than the choice of cutoff year.

households, suggesting that residential location does not meaningfully alter the policy's impact.

Our final analysis sample consists of 2,126 rural Han-majority couples across 14 provinces. These are the provinces where the first-girl exemption was introduced after 1985. The remaining provinces either did not adopt Policy 1, adopted the exemption in 1985 or earlier, or did not have observations that meet the exemption criteria.

For the analysis on spousal age gaps, we use year of marriage as the time variable, which allows us to treat the data as cross-sectional, drawn from the 2010 wave of the CFPS. In contrast, for fertility outcomes, we convert the data into a panel format, as explained in the next section for the same sample used for analyzing spousal age gaps.

Table 2 presents summary statistics for the 14 provinces included in our main analysis. The average number of children per couple as of 2010 was 1.81, while the average number of children prior to the implementation was 0.68. The average year of marriage is approximately 1993. Husbands and wives had similar family sizes, with an average of 3.12 and 3.23 siblings, respectively. The average age at first marriage is 23.3 for men and 21.8 for women, yielding an average spousal age gap of 1.5 years. In terms of education, 45% of husbands completed junior high school, 27% had primary school or below, 16% had no formal education, and 13% attained senior high school or above. Among wives, 30% completed junior high school, 30% had primary school or below, 34% had no formal education, and 6% attained senior high school or above. Additionally, Table 2 reports the share of households in which only the husband

Table 2: Summary Statistics

	Mean	SD	Min	Max	N
Number of Children 2010	1.81	0.81	0.00	5.00	2126
Number of Children Before Law	0.68	0.99	0.00	5.00	2126
Marriage Year	1992.65	8.45	1980.00	2010.00	2126
Husband's Number of Siblings	3.12	1.72	0.00	10.00	2126
Wife's Number of Siblings	3.23	1.68	0.00	10.00	2126
Husband's Age at Marriage	23.30	2.84	18.00	44.00	2126
Wife's Age at Marriage	21.78	2.43	18.00	39.00	2126
Husband-Wife Age Gap (in years)	1.53	2.73	-10.00	20.00	2126
Husband's Education (%)					
No formal education	0.16	0.36	0.00	1.00	2126
Primary school	0.27	0.44	0.00	1.00	2126
Junior high school	0.45	0.50	0.00	1.00	2126
Senior high or above	0.13	0.33	0.00	1.00	2126
Wife's Education (%)					
No formal education	0.34	0.47	0.00	1.00	2126
Primary school	0.30	0.46	0.00	1.00	2126
Junior high school	0.30	0.46	0.00	1.00	2126
Senior high or above	0.06	0.23	0.00	1.00	2126
Rural Hukou	1.00	0.00	1.00	1.00	2126
Only Husband Works (%)	0.19	0.40	0.00	1.00	2126

Notes: Data are from the 2010 China Family Panel Studies (CFPS). The sample consists of 2,126 rural Han-majority couples across 14 provinces affected by Policy 1 (first-born girl exemption).

reports positive wage income (19%), which we use as a measure of household division of labor in Section 7.

4 Empirical Estimation

4.1 The Effect of the OCP Exemptions on Marital Outcomes

To identify the effect of the OCP exemptions on spousal age gaps, we use the following staggered DiD model:

$$Y_{cpm} = \kappa_0 + \kappa_1 Post\_Event_{cpm} + \boldsymbol{X'_{cm}}\boldsymbol{\psi} + \delta_p + \gamma_m + \varepsilon_{cpm} \tag{1}$$

where some specifications additionally include $\delta_p \times m$. Y_{cpm} represents the age gap (husband's age minus the wife's age) for couple c in province p with marriage year m. $Post\_Event_{cpm}$ is a dummy variable equal to one if the couple married in or after the year their province adopted the exemption. We include province fixed effects δ_p and marriage year fixed effects γ_m. We additionally estimate specifications that include province-specific linear time trends, $\delta_p \times m$, to account for potential differences in how provinces enforced the OCP exemptions across marriage cohorts, and wild cluster bootstrap inference (Cameron et al., 2008; Roodman et al., 2019)

A potential concern is that the strength of OCP enforcement varied across regions and over time, which could confound our estimates if these changes coincided with exemption adoption. Our specification addresses this in several ways. Province fixed effects and year fixed effects absorb cross-sectional differences and common temporal changes in enforcement intensity, while province-specific time trends capture gradual, province-level shifts. Moreover, the enforcement variation documented by Ebenstein (2010) operates primarily across urban and rural hukou regimes, with urban residents facing considerably stricter financial penalties for unauthorized births. Since our sample is restricted to rural Han-majority couples, the largest source of enforcement variation does not affect our estimates. To the extent that enforcement was gradually relaxing over our sample period, this would work against finding a positive effect of exemptions, making our estimates conservative.

In addition, another potential concern with our identification strategy is that couples may have migrated across provinces after marriage, in which case the observed province of residence in 2010 may not reflect the policy environment at the time of marriage. Our baseline sample already excludes interprovincial couples, where spouses' hukou registrations are in different provinces. As an additional restriction, we further restrict the sample to individuals whose hukou remains registered in their province of birth, ensuring that province assignment accurately reflects the policy environment at the time of marriage. While individuals may physically reside in a province different from their hukou registration, this additional restriction limits the sample to those whose hukou remains in their birth province. This makes it more likely that the couple resided in the province whose policy timing we use for identification. This restriction reduces the sample from 2,126 to 2,010 couples.

Calendar years are not the relevant time reference here, as a couple's marriage year and age gap remain fixed over time. Instead, the timing of the OCP exemption relative to the couple's marriage year is the appropriate time reference. We restrict our sample to couples who were at least 18 years old at the time of marriage to ensure most individuals had completed their education.

The control vector $\boldsymbol{X_{cm}}$ includes the husband's and wife's number of siblings and indicators for the education category. These variables are largely predetermined. The number of siblings reflects the fertility decisions of each spouse's parents, determined before the respondent's own marriage. In addition, education is typically completed before marriage. In our sample, educational attainment in rural areas is generally low, with fewer than 1% of both husbands and wives having a college education or above, further limiting the scope for policy-induced changes in these covariates.

In addition, we estimate an event study model to decompose the dynamic effect of the OCP exemptions and test the parallel trend assumption:

$$\begin{aligned} Y_{cpm} = &\kappa + \sum_{\nu=-5}^{-2} 1[m - T_p^* = \nu]\varphi_\nu + \sum_{\nu=0}^{5} 1[m - T_p^* = \nu]\varphi_\nu \\ &+ \boldsymbol{X'_{cm}}\boldsymbol{\psi} + \gamma_m + \delta_p + \delta_p \times m + \varepsilon_{cpm} \end{aligned} \tag{2}$$

where φ_ν are the event-time coefficients and $1[m - T_p^* = \nu]$ is equal to 1 if the marriage year is ν years away from the treatment year. In addition, to avoid problems of multicollinearity between the full set of event-time indicators and the fixed effects, we omit the year prior to policy implementation ($\nu = -1$), which serves as the baseline period. All else is the same as above.

4.2 Addressing Limitations of Two-Way Fixed Effects Models and Suggested Corrections

Our baseline regressions rely on the traditional two-way fixed effects (TWFE) framework. However, this approach imposes strong assumptions of homogeneous treatment effects across cohorts treated at different times and can produce biased estimates in presence of treatment effect heterogeneity (Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2024; Borusyak et al., 2024; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). To address these concerns, we replicate our main results for the spousal age gap using an alternative estimator that is robust to treatment effect heterogeneity. Specifically, we implement the methods proposed by Sun and Abraham (2021) as our primary heterogeneity-robust estimator. Because no province remains never-treated in our sample, the Sun and Abraham (2021) estimator uses the last-treated cohort as the comparison group. We prefer this interaction-weighted approach because, with only a limited number of treatment cohorts, not-yet-treated comparison sets become thin in later periods, making fully nonparametric alternatives such as Callaway and Sant'Anna (2021) less precise. Our results remain consistent with those obtained from the baseline TWFE models.

5 Results

5.1 The Effect of the OCP Exemptions on Spousal Age Gaps

5.1.1 Baseline Results

Table 3 presents estimates of κ_1 in equation (1) on the spousal age gap and shows that the introduction of OCP exemptions had a positive impact on the age gap. Column (1) in the table shows results for our simplest specification, which includes only province fixed effects and married year fixed effects. Column (2) adds husband and wife level controls. Columns (3) and (4) further include province-specific linear time trends to account for potential differences in enforcement trajectories across provinces. Columns (5) through (8) restrict the sample to couples whose hukou remains registered in their province of birth. Within this restricted sample, columns (5) and (6) replicate the baseline specifications, while columns (7) and (8) add province-specific linear time trends. The estimated effect of the exemptions on the spousal age gap is stable across all eight specifications, ranging from 0.40 to 0.47 years.

The effect size on the age gap from our simplest specification corresponds to an increase in the husband-wife age gap of 0.42 years, statistically significant at the 10 percent level. When controls are included, results indicate an increase in the age gap of 0.43 years, significant at the 10 percent level. This represents a 25% increase, from a pre-policy mean of 1.71 years to 2.13 years. The 0.42-year (approximately 5-month) increase in the spousal age gap is com-

Table 3: The Impact of OCP Exemptions on Spouses' Age Gap

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0.420*	0.429*	0.452*	0.467**	0.396*	0.418*	0.425*	0.447*
	(0.223)	(0.224)	(0.233)	(0.234)	(0.235)	(0.239)	(0.242)	(0.245)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean	1.71	1.71	1.71	1.71	1.72	1.72	1.72	1.72
Wild Cluster Bootstrap P-value	0.068	0.065	0.058	0.053	0.105	0.092	0.090	0.077
Observations	2,126	2,126	2,126	2,126	2,010	2,010	2,010	2,010

Notes: This table reports estimates from a staggered Differences-in-Differences model using the 2010 China Family Panel Studies (CFPS). The dependent variable is the spousal age gap (husband's age minus wife's age at marriage). The sample consists of 2,126 rural Han-majority couples across 14 provinces affected by Policy 1 (first-born girl exemption). Columns (1)-(4) use the full sample; columns (5)-(8) further restrict to individuals who have not changed their hukou province since marriage (N = 2,010). Controls include the husband's and wife's number of siblings and education category indicators. Standard errors clustered at the county level are reported in parentheses. Wild cluster bootstrap p-values (9,999 replications) are reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

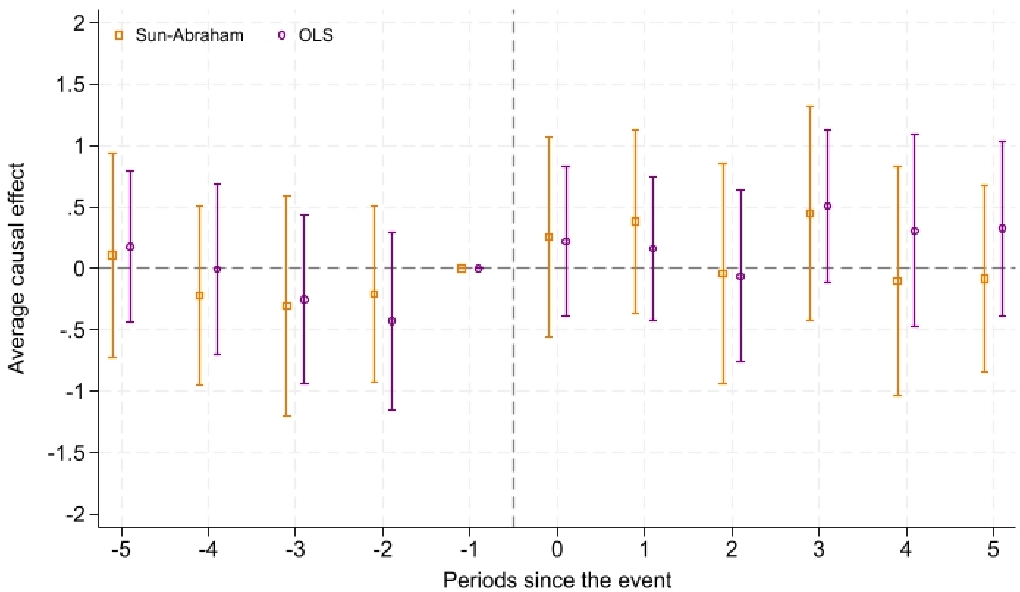
parable in magnitude to Gershoni and Low (2021), who find that Israel's policy offering free in vitro fertilization treatments narrowed the spousal age gap by approximately 0.25 years. Notably, their result operates through an opposing channel. By relaxing fecundity constraints, the Israeli policy reduced men's preference for younger wives, whereas the OCP exemptions increased fertility demand and thereby strengthened this preference. These results are consistent with our hypothesis that the policy-driven increase in fertility demand led men to marry younger women with fewer fecundity constraints.

This pooled effect, however, averages over meaningful heterogeneity across couples. As we show in Section 5.1.4, a single subgroup of couples, those with older husbands and younger wives, shows little response to the exemption. These couples already had the highest fertility and the widest spousal age gap before the policy change, and for this reason, likely had little room to respond, so their inclusion reduces the precision of the pooled estimate. Excluding this subgroup yields a more precisely estimated effect on the spousal age gap.

Appendix A shows that the exemptions are associated with a 32% increase in fertility, consistent with the policy's pro-natalist intent. Using the same ever-treated Policy 1 sample, we estimate that the exemptions increased the cumulative number of children by 0.114, significant across all eight specifications (Table A1). The post-treatment increase in fertility followed by a gradual decline (Figure A1) is consistent with a policy-driven response, as couples respond to the newly available option and then reach their desired family size. This fertility response supports the mechanism outlined in Section 1.

5.1.2 Event Study Figures

To assess whether this pooled estimate reflects a genuine post-treatment shift rather than pre-existing trends, we turn to event-study estimates. Figure 1 presents estimates using equation (2), as well as adjusted event study estimators that are robust to heterogeneous treatment effects over time and across groups (Sun and Abraham, 2021). The parallel trends assumption appears to hold: coefficients in pre-policy periods are generally statistically insignificant and show no discernible trends. A joint F-test fails to reject the null that pre-period coefficients are jointly equal to zero ($F(4, 86) = 0.613, p = 0.654$), supporting the parallel trends assumption. The robust estimator yields estimates broadly consistent with the TWFE event-study results. Post-policy coefficients from the TWFE are generally positive across post-treatment periods. While the Sun and Abraham estimates are less precisely estimated, the overall trajectory is consistent with the pooled estimates in Table 3.



Notes: Each coefficient represents the estimated effect of the OCP exemptions on the spousal age gap at event-time period ν relative to the year of policy adoption, normalized to zero at $\nu = -1$. The purple circle markers show TWFE estimates; the orange square markers show heterogeneity-robust estimates following Sun and Abraham (2021). Both specifications include province and marriage-year fixed effects and province-specific linear time trends. Controls include the husband's and wife's number of siblings and education category indicators. Confidence intervals are at the 95% level. The sample consists of 2,126 rural couples from the 2010 CFPS.

Figure 1: The Impact of the OCP Exemptions on the Age Gap of Married Couples

5.1.3 Decomposition: Husband's and Wife's Age at Marriage

To understand which side of the marriage market drives the widening of the spousal age gap, we estimate the baseline specification separately using the wife's age at marriage and the husband's age at marriage as dependent variables. Tables B1 and B2 report the results, and Figures B1 and B2 present the corresponding event studies. Joint F-tests indicate that pre-period coefficients are jointly insignificant for both wife's age at marriage ($F(4, 86) = 1.438, p = 0.228$) and husband's age at marriage ($F(4, 86) = 0.049, p = 0.995$), supporting the parallel trends assumption.

The estimates indicate that the widening of the age gap is driven primarily by a decline in wives' age at marriage. In the baseline specification without individual-level controls, the policy reduces wives' marriage age by 0.34 to 0.36 years (significant at the 10% level), relative to a pre-policy mean of 21.4 years. When individual-level controls are included, point estimates remain similar in magnitude (-0.31 to -0.33) but become statistically insignificant, suggesting that the estimated effect is not precisely measured once additional covariates are included. In contrast, the effect on husbands' age at marriage is small, positive (0.05 to 0.15 years), and statistically insignificant across all specifications, indicating that the widening of the age gap is driven predominantly by changes on the wife's side. This pattern is consistent with the fertility-demand mechanism described above.

5.1.4 Heterogeneity by Joint Age at Marriage

We next examine whether the effect of the OCP exemptions on spousal age gaps varies with the joint distribution of husbands' and wives' ages at marriage. We classify couples into four subgroups by splitting each side of the marriage at the sex-specific median age at marriage, which is 23 for husbands and 22 for wives. The four subgroups are (Old H, Old W), (Old H, Young W), (Young H, Old W), and (Young H, Young W), where "Young" indicates marrying at or below the sex-specific median.

Inspecting the pre-policy distribution by subgroup shows that one of the four stands apart from the rest. Among (Old H, Young W) couples, the average pre-policy spousal age gap is 5.4 years and the average pre-policy number of children is 1.65,[2] compared with 1.5 years and 1.47 for the rest of the sample.[3] We therefore estimate an interaction specification in which the

[2]These pre-policy descriptives are couple-level averages computed over couples married before their province's adoption year (the pre-policy sample). The number of children counts births occurring before the adoption year and is therefore distinct from the panel-based pre-policy mean of 0.36 reported in Appendix A, which averages the cumulative child count across all pre-treatment couple-year observations, including early marriage years in which couples had not yet had children.

[3]Spousal age gaps in this comparison are measured in absolute value, so that the average reflects the typical age difference between spouses regardless of which spouse is older. The pattern holds across all four subgroups. The average pre-policy absolute spousal age gap is 1.82 years for (Old H, Old W), 5.44 years for (Old H, Young W), 1.53 years for (Young H, Old W), and 1.35 years for (Young H, Young W). The corresponding pre-policy numbers of children are 1.49, 1.65, 1.41, and 1.48. The (Old H, Young W) subgroup stands apart on both margins.

(Old H, Young W) subgroup is contrasted with the rest of the sample:[4]

$$
\begin{aligned}
Y_{cpm} = {} & \alpha_0 + \alpha_1\, OHYW_{cpm} + \alpha_2\, Post\_Event_{cpm} + \alpha_3\, (OHYW_{cpm} \times Post\_Event_{cpm}) \\
& + \boldsymbol{X'_{cm}}\boldsymbol{\psi} + \gamma_m + \delta_p + \varepsilon_{cpm},
\end{aligned}
\tag{3}
$$

where $OHYW_{cpm}$ is an indicator equal to one if the couple is in the (Old H, Young W) subgroup and zero otherwise. The coefficient α_1 captures the pre-policy difference in the outcome between the (Old H, Young W) subgroup and the rest of the sample, α_2 captures the policy effect for the rest of the sample, and α_3 captures the additional policy effect specific to the (Old H, Young W) subgroup, so that the total policy effect for that subgroup is $\alpha_2 + \alpha_3$.

Table C1 reports the results for the spousal age gap. The pre-policy difference α_1 is positive and statistically significant at the 1% level, with point estimates between 4.476 and 4.560 years, indicating that the (Old H, Young W) subgroup had a substantially larger spousal age gap than the rest of the sample even before the policy change. The policy effect for the rest of the sample, α_2, is positive and statistically significant in every column (0.326 to 0.383 years). The differential effect α_3 is negative and significant at the 1% level in all eight specifications (-0.799 to -0.848 years). The implied total post-policy change for the (Old H, Young W) subgroup, $\alpha_2 + \alpha_3$, is therefore negative across all eight columns. In other words, the spousal age gap did not widen in this subgroup in response to the exemption, in contrast to the widening observed in

[4] We estimate the interaction separately for each of the four subgroups, contrasting it with the rest of the sample, rather than estimating a single fully interacted four-way specification. The latter would substantially reduce statistical power given the limited number of couples per subgroup.

the rest of the sample.[5]

Table C2 reports the analogous results for the number of children. The pre-policy difference α_1 is positive and statistically significant, with point estimates between 0.046 and 0.088 additional children per couple. The (Old H, Young W) subgroup is also the only subgroup for which the analogous pre-policy fertility differential α_1 against the rest of the sample is statistically significant at conventional levels, so it stands out on the fertility margin in the same way as on the age-gap margin. The policy effect for the rest of the sample, α_2, is positive and significant in every column (0.065 to 0.127 children). The differential effect α_3 is negative in every column (-0.029 to -0.063), with magnitudes that roughly offset the rest-of-sample response, although the differential is not individually statistically significant. The sign and magnitude are consistent with the (Old H, Young W) subgroup being unresponsive on the fertility margin as well.

Taken together, the two interactions, namely the differential effect α_3 in Tables C1 and C2, point to a single reading. The (Old H, Young W) subgroup had both the largest pre-policy spousal age gap and the highest pre-policy fertility, and a sizeable share of couples in this subgroup were already violating the One-Child Policy and paying the associated fines. The exemption to the

[5] Estimating the analogous interaction for each of the other three subgroups against the rest of the sample yields a positive differential response in every case. The (Old H, Young W) subgroup is the only one with a positive and statistically significant pre-policy differential α_1 on the spousal age gap, and is the only one in which the spousal age gap fails to widen in response to the exemption. The (Young H, Young W) subgroup shows the largest positive differential (α_3 between 0.856 and 0.873 years, $p < 0.01$), the (Young H, Old W) subgroup a smaller positive differential (α_3 between 0.370 and 0.605 years, significant at the 10% level or better in most specifications), and the (Old H, Old W) subgroup a positive but smaller differential (α_3 between 0.398 and 0.467 years, significant at the 10% level in some specifications).

One-Child Policy was therefore unlikely to be a binding constraint on the (Old H, Young W) subgroup's subsequent behavior, which is consistent with the absence of any post-policy widening on the age-gap margin and with the offsetting differential on the fertility margin.

To make this implication explicit, we re-estimate the main specification on the sample that excludes the (Old H, Young W) subgroup, which leaves 1,702 of the 2,126 couples in the full sample. Table C3 reports the spousal age-gap results. The post-policy coefficient is positive in every column, ranging from 0.305 to 0.370 years, with wild cluster bootstrap p-values between 0.026 and 0.051. Relative to the full-sample estimates reported in Table 3 (0.396 to 0.467, p_{wild} between 0.053 and 0.105), the point estimates are slightly smaller in magnitude but more precisely estimated, so the conclusion that the policy widens the spousal age gap is unchanged. Figure C1 shows the corresponding event study. The pre-period exhibits a downward pre-trend in the spousal age gap that is more pronounced than in the full-sample event study, against which the post-policy rise is correspondingly sharper. Ignoring this pre-trend therefore understates the size of the post-policy adjustment, and the difference-in-differences estimate in Table C3 is, if anything, a lower bound on the true effect for the responding population.

Because the OCP exemptions were assigned at the province level, we also assess inference when standard errors are clustered at that level rather than at the county level (Abadie et al., 2023). With only 14 provinces, the wild cluster bootstrap that we already use for inference is the appropriate tool, though it is known to be conservative when the number of clusters is small. Under province-level clustering, the full-sample effect on the spousal age gap remains positive but is no longer statistically significant at conventional levels ($p \approx 0.172$ in the baseline specification, and $p > 0.10$ in all eight columns), whereas the effect estimated on the sample that excludes the (Old H, Young W) subgroup remains significant ($p \approx 0.007$, and $p < 0.01$ in all eight columns). This contrast is consistent with the reading developed above. The non-responsive (Old H, Young W) subgroup dilutes the full-sample estimate, and once it is set aside the policy effect on the spousal age gap is significant even under the more demanding province-level clustering.

We also re-estimate the fertility specification on the same restricted sample. Table C4 shows that the post-policy effect is positive in every column, ranging from 0.060 to 0.126 additional children per couple, with wild cluster bootstrap p-values between 0.001 and 0.048. The magnitudes are essentially unchanged relative to the full-sample analogues and the precision is slightly improved, which rules out the alternative reading that the headline fertility result is mechanically driven by the high-baseline-fertility subgroup. Unlike the spousal age gap, the fertility effect remains statistically significant under province-level clustering in both the full sample ($p \approx 0.026$) and the sample excluding the (Old H, Young W) subgroup ($p \approx 0.020$), reflecting that the fertility response is not concentrated in the (Old H, Young W) subgroup. Figure C2 shows the corresponding event study for this restricted sample.

5.1.5 Heterogeneity by Household Employment Sector

We also examine whether the policy effect varies by household employment sector. Table C5 presents results from interacting the treatment indicator with an agricultural household indicator. The interaction coefficient is small in magnitude (0.003-0.041) and statistically insignificant across all specifications, indicating no differential effect between agricultural and non-agricultural households. This suggests that the policy's impact on the spousal age gap operates broadly across employment types among rural hukou holders rather than being concentrated in a particular sector.

6 Robustness

We now assess the robustness of our spousal age gap estimates through a Goodman-Bacon (2021) decomposition.

6.1 Goodman-Bacon Decomposition

Goodman-Bacon (2021) shows that the TWFE estimator in a staggered DiD setting can be expressed as a weighted average of all possible 2×2 DiD estimates, comparing each pair of treatment groups using their relative timing. This decomposition is useful because some of these 2×2 comparisons use already-treated units as controls, which can produce biased estimates if treatment effects evolve over time. By examining the individual 2×2 estimates and their weights, we can assess whether such problematic comparisons drive our overall result.

Figure D1 presents the Goodman-Bacon (2021) decomposition of our TWFE estimate. This decomposition is diagnostic of the TWFE weighting structure rather than a replication of our preferred specification.[6] All seven 2×2 comparisons yield positive estimates, with 82% of the total weight concentrated in earlier-treated versus later-control comparisons, which represent the cleanest source of identifying variation. The remaining weight comes from later-treated versus earlier-control comparisons, which also yield positive estimates, indicating that negative weighting is unlikely to drive our results.

7 Implications for Household Division of Labor

The preceding analysis establishes that OCP exemptions increased both fertility and the spousal age gap. A natural question is whether these changes in marital sorting carry downstream consequences for the division of labor within households. Larger age gaps are associated with greater differences in human capital and earnings power between spouses, which can reinforce traditional household arrangements in which the husband specializes in market work and the wife in home production (Mincer and Polachek, 1974; Polachek, 1975; Grossbard, 2014; Goldin, 2014). In this section, we provide suggestive

[6] The decomposition is estimated on province-by-marriage-year collapsed data without individual-level controls or province-specific linear time trends, as the standard Goodman-Bacon (2021) decomposition applies to parsimonious two-way fixed effects specifications. The overall DiD estimate ($\hat{\beta} = 1.15$) reflects this simplified specification and should not be compared directly to the main TWFE estimate in Table 3. The key finding, that all seven 2×2 comparisons yield positive estimates, holds regardless of specification.

evidence on this channel.

We examine whether the OCP exemptions affected the probability that only the husband participates in the labor market. Specifically, we construct a binary indicator equal to one if the husband reports positive wage income while the wife does not. We replace the dependent variable Y_{cpm} in equation (1) and (2) with this indicator and estimate the same specification, including province and marriage-year fixed effects, province-specific linear time trends, and wild cluster bootstrap inference (Cameron et al., 2008; Roodman et al., 2019).

Table E1 presents the results. The point estimates are consistently positive across all eight specifications, ranging from 0.015 to 0.021 (approximately 8 to 11 percent of the pre-policy mean), a direction consistent with the exemptions shifting affected households toward a more traditional single-earner arrangement. We emphasize, however, that these estimates are not statistically significant at conventional levels in the pooled specification, which likely reflects the limited proportion of couples in our rural sample with formal wage income, reducing statistical power when averaging across all post-treatment periods. We therefore read this outcome as no more than suggestive.

The event study in Figure E1 reveals a more heterogeneous pattern over time. Pre-treatment coefficients are close to zero and show no discernible trend, supporting the parallel trends assumption for this outcome. A joint F-test indicates that the pre-period coefficients are jointly insignificant ($F(4, 86) = 1.191, p = 0.321$). In the post-treatment period, the Sun-Abraham interaction-weighted estimates are statistically significant at event-time periods 2 and 3, with the TWFE estimate also reaching significance at period 3. Significance thus appears only in isolated post-treatment periods rather than throughout, so we interpret this pattern cautiously as broadly consistent with, rather than as establishing, a shift toward more traditional household arrangements following the policy-induced increase in fertility and spousal age gaps.

We also estimate the (Old H, Young W) vs. rest interaction specification for the share of couples in which only the husband works. Table E2 reports the results. None of the three coefficients (α_1, α_2, α_3) is individually statistically significant, which is consistent with the small pooled estimates reported above and with the (Old H, Young W) subgroup attenuating the average response. We therefore re-estimate the regression for the share of couples in which only the husband works on the sample that excludes the (Old H, Young W) subgroup. Table E3 shows that the post-policy coefficient roughly triples in magnitude, with point estimates between 0.042 and 0.048, and wild cluster bootstrap p-values fall by an order of magnitude into the 0.086 to 0.146 range.[7] Figure E2 shows the corresponding event study, which displays no pre-trend and a level shift toward more traditional specialization in the post-period. The post-period coefficients are visibly larger in magnitude than in the full-sample event study, in line with the larger difference-in-differences point estimate in Table E3. Removing this subgroup therefore points to a response, marginally significant at best, that is concentrated among the responding majority and was previously offset by the non-response of the (Old H, Young W) subgroup. We nonetheless regard the household division of labor evidence as suggestive

[7] In the full sample, the corresponding coefficients are 0.015 to 0.021 with wild cluster bootstrap p-values between 0.44 and 0.54, as reported in Table E1.

given the imprecision of these estimates.

These findings are also consistent with recent evidence on the link between marital sorting and intra-household allocation. Francesconi et al. (2026) show that sorting on gender role attitudes strongly predicts subsequent division of housework, childcare, earnings, and paid labor within marriages, supporting the interpretation that the policy-induced widening of spousal age gaps may have meaningful downstream consequences for household division of labor.

8 Conclusion

The spousal age gap between men and women is frequently attributed to the demand for children. Because women face more stringent fecundity restraints, men tend to prefer younger partners for childbearing. At the same time, the demand for children creates preferences that reinforce spousal age gaps. In this paper, we provide empirical evidence from rural China by exploiting the staggered rollout of China's OCP exemptions across provinces. Using a DiD framework, we find that exposure to the policy increases fertility by 32% and widens the husband-wife age gap by approximately 0.42 years, a 25% increase. These results demonstrate that exogenous changes in fertility restrictions, which reflect an increased demand for children, can influence patterns of marital matching by widening spousal age gaps. This widening is driven by the broad majority of couples rather than by those who married with an older husband and a younger wife. The latter had already reached the widest spousal age gap and the highest pre-policy fertility, which suggests that the exemption was less likely to be a binding constraint for them, so that excluding this group yields a larger and more precisely estimated effect.

More broadly, pro-natalist policies can influence not only fertility outcomes but also the composition of marriages through spousal age gaps. These changes in marital sorting may, in turn, have implications for household division of labor and gender wage inequality, as tentatively suggested by the positive but largely imprecise estimates on single-earner household formation (which reach statistical significance only in isolated post-treatment periods of the event study) and by the broader literature linking marital sorting to intra-household allocation (Francesconi et al., 2026).

The findings highlight the role of fertility demand in shaping marital sorting patterns. While many countries are experiencing declining fertility rates and narrowing spousal age gaps, we show that an exogenous increase in the demand for children can partially offset these trends, widening both the fertility rate and the spousal age gap among the affected population. Although the analysis is based on rural China, the results are likely relevant for understanding the underlying drivers of the gender wage gap prevalent worldwide, particularly in market-oriented societies. With China's transition to a market-oriented economy in the 1980s, patterns of spousal matching and gender-based divisions of labor have become increasingly important in shaping household income and gender inequality (Grossbard, 2014; Blau and Kahn, 2017). In particular, the evidence highlights how demographic policies that exogenously raise fertility can unintentionally shape marital patterns and may contribute to gender inequality. This underscores the importance of integrating potential unintended consequences into evaluating and designing family-related policies.

References

Abadie, Alberto, Susan Athey, Guido W Imbens, and Jeffrey M Wooldridge (2023) "When Should You Adjust Standard Errors for Clustering?" *The Quarterly Journal of Economics*, 138 (1), 1–35.

Afridi, Farzana, Sherry Xin Li, and Yufei Ren (2015) "Social identity and inequality: The impact of China's hukou system," *Journal of public economics*, 123, 17–29.

Blau, Francine D and Lawrence M Kahn (2013) "Female labor supply: Why is the United States falling behind?" *American Economic Review*, 103 (3), 251–256.

——— (2017) "The gender wage gap: Extent, trends, and explanations," *Journal of Economic Literature*, 55 (3), 789–865.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess (2024) "Revisiting event-study designs: robust and efficient estimation," *Review of Economic Studies*, 91 (6), 3253–3285.

Bulte, Erwin, Nico Heerink, and Xiaobo Zhang (2011) "China's one-child policy and 'the mystery of missing women': ethnic minorities and male-biased sex ratios," *Oxford Bulletin of Economics and Statistics*, 73 (1), 21–39.

Callaway, Brantly and Pedro HC Sant'Anna (2021) "Difference-in-differences with multiple time periods," *Journal of Econometrics*, 225 (2), 200–230.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller (2008) "Bootstrap-based improvements for inference with clustered errors," *The review of economics and statistics*, 90 (3), 414–427.

Cohen, Alma, Rajeev Dehejia, and Dmitri Romanov (2013) "Financial incentives and fertility," *Review of Economics and Statistics*, 95 (1), 1–20.

De Chaisemartin, Clément and Xavier d'Haultfoeuille (2024) "Difference-in-differences estimators of intertemporal treatment effects," *Review of Economics and Statistics*, 1–45.

Díaz-Giménez, Javier and Eugenio Giolito (2013) "Accounting for the timing of first marriage," *International Economic Review*, 54 (1), 135–158.

Ebenstein, Avraham (2010) "The "missing girls" of China and the unintended consequences of the one child policy," *Journal of Human resources*, 45 (1), 87–115.

Edlund, Lena, Hongbin Li, Junjian Yi, and Junsen Zhang (2013) "Sex ratios and crime: Evidence from China," *Review of Economics and Statistics*, 95 (5), 1520–1534.

Fang, Liyuan (2023) "The one-child policy and gender bias in China: A review of recent literature," *Journal of Humanities, Arts and Social Science*, 7 (6).

Feng, Ying and Jie Ren (2022) "Within marriage age gap across countries," *Economics Letters*, 210, 110190.

Francesconi, Marco, Cheti Nicoletti, and Khushboo Surana (2026) "Gender Role Attitudes and Marital Sorting: Implications for Household Inequality," CESifo Working Paper 12538, CESifo.

Gershoni, Naomi and Corinne Low (2021) "Older yet fairer: How extended reproductive time horizons reshaped marriage patterns in Israel," *American Economic Journal: Applied Economics*, 13 (1), 198–234.

Giolito, Eugenio P (2004) "A search model of marriage with differential fecundity," IZA Discussion Papers No. 1082.

Goldin, Claudia (2014) "A grand gender convergence: Its last chapter," *American Economic Review*, 104 (4), 1091–1119.

Goodman-Bacon, Andrew (2021) "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 225 (2), 254–277.

Grossbard, Shoshana (2014) *The marriage motive: a price theory of marriage: how marriage markets affect employment, consumption, and savings*: Springer.

Hamermesh, Daniel S (2022) "Moms' time—Married or not," in *Mothers in the Labor Market*, 1–27: Springer.

Hamilton, Gillian and Aloysius Siow (2007) "Class, gender and marriage," *Review of Economic Dynamics*, 10 (4), 549–575.

Han, Jun and Zhong Zhao (2022) "One-child policy and marriage market in China," *Review of Development Economics*, 26 (1), 57–84.

Harkness, Susan and Jane Waldfogel (2003) "The family gap in pay: Evidence from seven industrialized countries," in *Worker Well-being and Public Policy*, 369–413: Emerald Group Publishing Limited.

He, Haoran, Sherry Xin Li, and Yuling Han (2023) "Labor market discrimination against family responsibilities: A correspondence study with policy change in China," *Journal of Labor Economics*, 41 (2), 361–387.

Huang, Wei, Xiaoyan Lei, and Ang Sun (2021) "Fertility restrictions and life cycle outcomes: Evidence from the One-Child Policy in China," *Review of Economics and Statistics*, 103 (4), 694–710.

Huang, Wei, Yinghao Pan, and Yi Zhou (2025) "One-child policy, marriage distortion, and welfare loss," *Review of Economics and Statistics*, 107 (5), 1341–1355.

Juhn, Chinhui and Kristin McCue (2017) "Specialization then and now: Marriage, children, and the gender earnings gap across cohorts," *Journal of Economic Perspectives*, 31 (1), 183–204.

Li, Hongbin and Xinzheng Shi (2025) "The effect of the one-child policy on fertility in China: identification based on difference-in-differences," *Journal of Population Economics*, 38 (1), 2.

Li, Hongbin, Junjian Yi, and Junsen Zhang (2011) "Estimating the effect of the one-child policy on the sex ratio imbalance in China: Identification based on the difference-in-differences," *Demography*, 48, 1535–1557.

Lin, Siyuan, Laura Argys, and Susan Averett (2025) "Exposure to the one-child policy and fertility among Chinese immigrants to the US," *Review of Economics of the Household*, 1–34.

Low, Corinne (2024) "Pricing the biological clock: The marriage market costs of aging to women," *Journal of Labor Economics*, 42 (2), 395–426.

Milligan, Kevin (2005) "Subsidizing the stork: New evidence on tax incentives and fertility," *Review of Economics and statistics*, 87 (3), 539–555.

Mincer, Jacob and Solomon William Polachek (1974) "Family investments in human capital: Earnings of women," *Journal of Political Economy*, 82 (2, Part 2), S76–S108.

Office for National Statistics (2021) "Marriage data for the United Kingdom," `https://www.ons.gov.uk/aboutus/transparencyandgovernance/freedomofinformationfoi/marriagedatafortheunitedkingdom`.

Pal, Ipshita and Jane Waldfogel (2016) "The family gap in pay: New evidence for 1967 to 2013," *RSF: The Russell Sage Foundation Journal of the Social Sciences*, 2 (4), 104–127.

Polachek, Solomon William (1975) "Potential biases in measuring male-female discrimination," *Journal of Human Resources*, 205–229.

——— (1981) "Occupational self-selection: A human capital approach to sex differences in occupational structure," *The Review of Economics and Statistics*, 60–69.

Polachek, Solomon William, Xu Zhang, and Xing Zhou (2015) "A Biological Basis for the Gender Wage Gap: Fecundity and Age and Educational Hypogamy," in *Gender convergence in the labor market*, 35–88: Emerald Group Publishing Limited.

Roodman, David, Morten Ørregaard Nielsen, James G MacKinnon, and Matthew D Webb (2019) "Fast and wild: Bootstrap inference in Stata using boottest," *The Stata Journal*, 19 (1), 4–60.

Siow, Aloysius (1998) "Differential fecundity, markets, and gender roles," *Journal of Political Economy*, 106 (2), 334–354.

Sun, Liyang and Sarah Abraham (2021) "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 225 (2), 175–199.

Tan, Jing and Kui Liu (2020) "The impact of two-child policy on household savings in China and mechanisms," *Applied Economics Letters*, 27 (20), 1672–1676.

United Nations (2000) "Difference in Mean Age of Marriage Between Men and Women Still High, Population Commission Told," https://press.un.org/en/2000/20000615.pop771.doc.html.

Vella, Frank and Sean Collins (1990) "The value of youth: equalizing age differentials in marriages," *Applied Economics*, 22 (3), 359–373.

Wang, Fei, Liqiu Zhao, and Zhong Zhao (2017) "China's family planning policies and their labor market consequences," *Journal of Population Economics*, 30, 31–68.

Wei, Shang-Jin and Xiaobo Zhang (2011) "The competitive saving motive: Evidence from rising sex ratios and savings rates in China," *Journal of Political Economy*, 119 (3), 511–564.

Zeng, Yi and Therese Hesketh (2016) "The effects of China's universal two-child policy," *The Lancet*, 388 (10054), 1930–1938.

Zhang, Lin (2023) "One-child policy, economic sector, and female labor supply: evidence from urban China," *Applied Economics Letters*, 30 (7), 944–949.

Zhang, Xu (2014) "Fecundity and husband-wife age gap at first marriage—cross-country analysis," *Theoretical Economics Letters*, 4 (7), 598–607.

Zheng, Bingwen (2016) "Population ageing and the impacts of the universal two-child policy on China's socio-economy," *Economic and Political Studies*, 4 (4), 434–453.

Appendices

A Effects on Fertility

A.1 Empirical Strategy

To test the intended effects of the OCP exemptions (whether the law induced greater fertility), we estimate the policy's effect on fertility using the same ever-treated rural Policy 1 sample as the main age gap analysis. Using children's birth year and couples' year of marriage, we construct a panel dataset to track time dynamics of how many children each couple had in each observation year, instead of observing the total number of children in the year 2010. The underlying individuals are identical to those in the age gap analysis. The panel structure simply provides multiple observations per couple over time. We estimate the following staggered DiD model:

$$NumChildren_{cptm} = \beta_0 + \beta_1 Post\_Event_{cpt} + \boldsymbol{X'_{cm}}\boldsymbol{\psi} + \omega_t + \delta_p + \varepsilon_{cptm} \qquad (4)$$

where some specifications additionally include $\delta_p \times t$. The dependent variable $NumChildren_{cptm}$ is the number of children for couple c in province p in year t married in year m. $Post\_Event_{cpt}$ is a binary indicator equal to 1 if the couple resides in a province that had implemented the exemption by year t. $\boldsymbol{X'_{cm}}$ is a vector that includes controls for husband and wife characteristics. Year fixed effects ω_t and province fixed effects δ_p are included. We additionally estimate specifications that include province-specific linear time trends to account for the possibility that provinces implementing the exemptions in different years may have experienced different enforcement trajectories over time. ε_{cptm} is the error term.

To examine treatment dynamics and test the parallel trends assumption, we also estimate an event-study version of the model:

$$\begin{aligned} NumChildren_{cptm} =& \beta + \sum_{\nu=-5}^{-2} 1[t - T_p^* = \nu]\varphi_\nu + \sum_{\nu=0}^{5} 1[t - T_p^* = \nu]\varphi_\nu \\ & + \boldsymbol{X'_{cm}}\boldsymbol{\psi} + \omega_t + \delta_p + \delta_p \times t + \varepsilon_{cptm} \end{aligned} \tag{5}$$

where φ_ν are the event-time coefficients and $1[t - T_p^* = \nu]$ is equal to one if the observation year is ν years away from the treatment year T_p^*. The omitted category is $\nu = -1$, the year prior to treatment. We include the same fixed effects and the same controls as in equation (4).

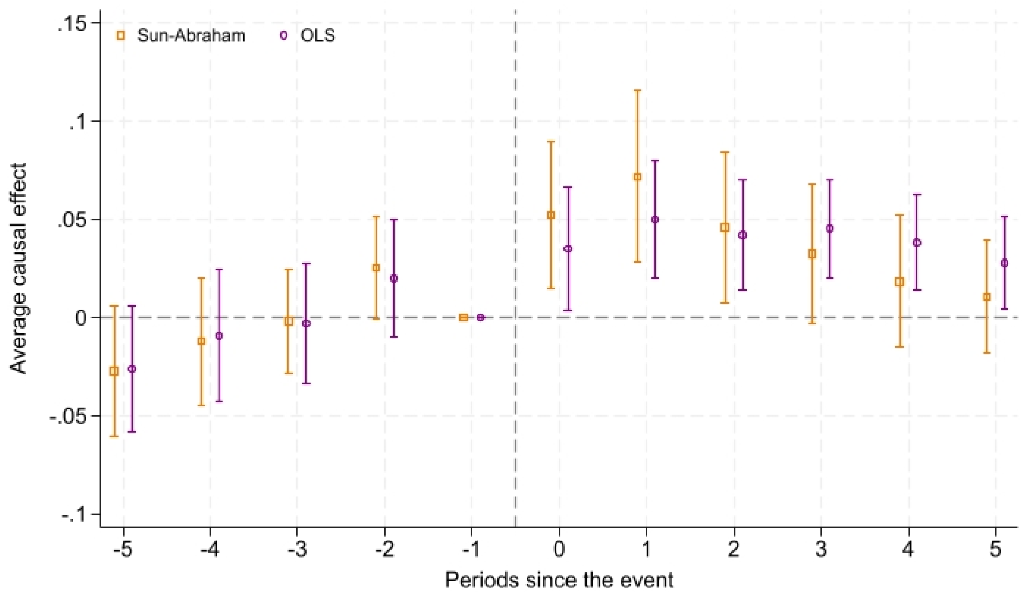
A.2 Results

Table A1 reports the main results on fertility. The policy is associated with an increase of 0.114 children per couple, statistically significant at the 1% level, both with and without controls. Given the pre-policy sample mean of 0.36[8], this corresponds to approximately a 32% increase in fertility. This is on the higher end of pro-natalist policy effects documented in the literature. To

[8]The pre-policy mean of 0.36 reflects the average cumulative number of children across all couple-year observations in the pre-treatment period, which is lower than the couple-level average of 0.68 reported in Table 2 because early panel years contribute observations in which couples have not yet had children.

put this magnitude in perspective, for comparison, Milligan (2005) finds that Quebec's Allowance for Newborn Children increased fertility by approximately 17%, while Cohen et al. (2013) estimate a 7.8% increase from Israeli child subsidies. The comparatively larger response we document is consistent with the nature of the intervention. The OCP exemptions removed a binding legal prohibition on second births, which is likely to generate a stronger fertility response than marginal financial incentives. These findings suggest that the OCP exemptions successfully induced higher fertility among eligible couples.

Event study estimates are shown in Figure A1. A joint F-test rejects the null that the pre-period coefficients are jointly zero ($F(4, 86) = 4.145, p = 0.004$), indicating some pre-existing upward movement in fertility prior to the policy. However, the coefficients increase notably after the policy and gradually decline within five years, a pattern consistent with a policy-driven response rather than a simple continuation of pre-existing trends. This suggests that the policy's impact on the rate of childbearing is concentrated in the years immediately following the exemption, consistent with couples responding to the newly available option and then reaching their desired family size.



Notes: Each coefficient represents the estimated effect of the OCP exemptions on the cumulative number of children per couple at event-time period ν relative to the year of policy adoption, normalized to zero at $\nu = -1$. The dependent variable is constructed as a panel from the same ever-treated rural Policy 1 sample used in the age gap analysis. The purple circle markers show TWFE estimates; the orange square markers show heterogeneity-robust estimates following Sun and Abraham (2021). Both specifications include province and year fixed effects and province-specific linear time trends. Controls include the husband's and wife's number of siblings and education category indicators. Standard errors are clustered at the county level using the wild cluster bootstrap (9,999 replications). Confidence intervals are at the 95% level. The sample consists of 64,039 couple-year observations, constructed as a panel from the 2,126 ever-treated rural Policy 1 couples in the 2010 CFPS.

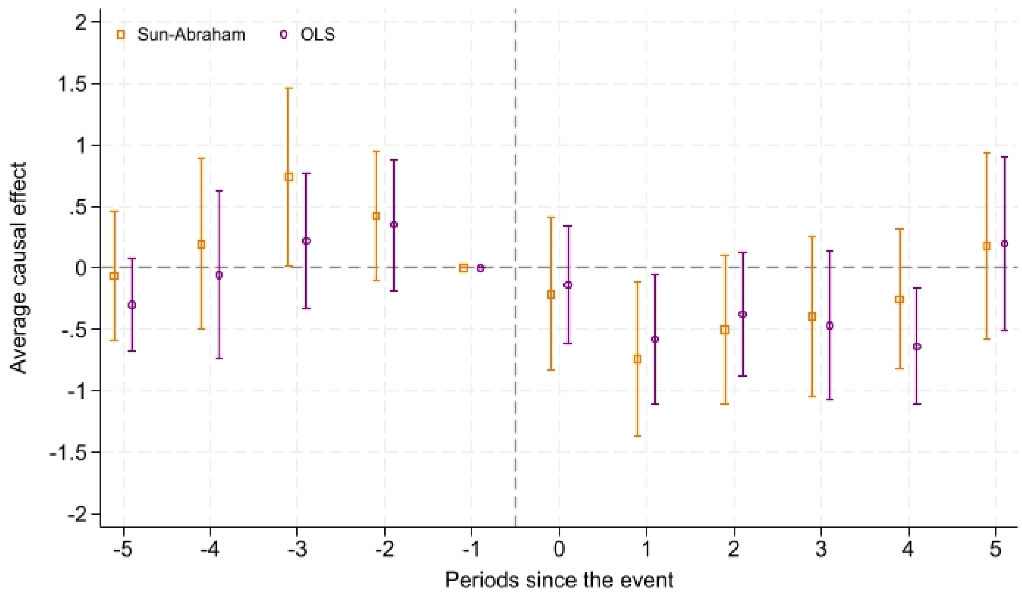
Figure A1: The Impact of OCP Exemptions on Fertility

Table A1: The Impact of OCP Exemptions on Fertility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0.114***	0.115***	0.054*	0.053*	0.119***	0.120***	0.060**	0.059**
	(0.030)	(0.031)	(0.028)	(0.028)	(0.030)	(0.030)	(0.028)	(0.028)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36
Wild Cluster Bootstrap P-value	0.001	0.001	0.069	0.076	0.001	0.001	0.045	0.054
Observations	64,039	64,039	64,039	64,039	60,518	60,518	60,518	60,518

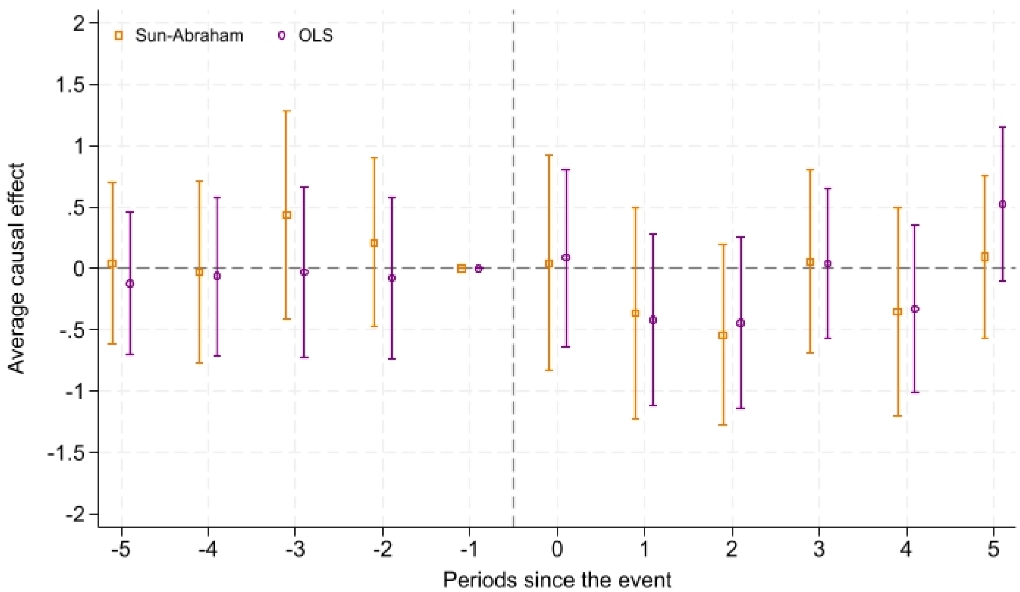
Notes: This table reports estimates from a staggered Differences-in-Differences model using the 2010 CFPS. The dependent variable is the cumulative number of children per couple in each observation year, constructed as a panel from the same ever-treated rural Policy 1 sample used in the age gap analysis. Controls include the husband's and wife's number of siblings and education category indicators. Province and year fixed effects are included in all specifications. Standard errors clustered at the county level are in parentheses. Wild cluster bootstrap p-values (9,999 replications) are reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Decomposition: Husband's and Wife's Age at Marriage



Notes: Each coefficient represents the estimated effect of the OCP exemptions on wife's age at marriage at event-time period ν relative to the year of policy adoption, normalized to zero at $\nu = -1$. The purple circle markers show TWFE estimates; the orange square markers show heterogeneity-robust estimates following Sun and Abraham (2021). Both specifications include province and marriage-year fixed effects and province-specific linear time trends. Controls include the husband's and wife's number of siblings and education category indicators. Confidence intervals are at the 95% level. The sample consists of 2,126 rural couples from the 2010 CFPS.

Figure B1: The Impact of the OCP Exemptions on the Age at Marriage of Wives



Notes: Each coefficient represents the estimated effect of the OCP exemptions on husband's age at marriage at event-time period ν relative to the year of policy adoption, normalized to zero at $\nu = -1$. The purple circle markers show TWFE estimates; the orange square markers show heterogeneity-robust estimates following Sun and Abraham (2021). Both specifications include province and marriage-year fixed effects and province-specific linear time trends. Controls include the husband's and wife's number of siblings and education category indicators. Confidence intervals are at the 95% level. The sample consists of 2,126 rural couples from the 2010 CFPS.

Figure B2: The Impact of the OCP Exemptions on the Age at Marriage of Husbands

Table B1: The Impact of OCP Exemptions on Wives' Age at Marriage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.340*	-0.307	-0.351*	-0.316	-0.344*	-0.320	-0.362*	-0.332
	(0.197)	(0.200)	(0.197)	(0.203)	(0.201)	(0.206)	(0.203)	(0.210)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean	21.41	21.41	21.41	21.41	21.37	21.37	21.37	21.37
Wild Cluster Bootstrap P-value	0.090	0.134	0.082	0.133	0.095	0.129	0.086	0.127
Observations	2,126	2,126	2,126	2,126	2,010	2,010	2,010	2,010

Notes: This table reports estimates from a staggered Differences-in-Differences model using the 2010 CFPS. The dependent variable is the wife's age at first marriage. The sample and specifications are identical to Table 3. Standard errors are clustered at the county level and are in parentheses. Wild cluster bootstrap p-values (9,999 replications) are reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: The Impact of OCP Exemptions on Husbands' Age at Marriage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0.080	0.122	0.100	0.151	0.052	0.098	0.063	0.115
	(0.233)	(0.235)	(0.235)	(0.236)	(0.239)	(0.243)	(0.242)	(0.243)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean	23.13	23.13	23.13	23.13	23.10	23.10	23.10	23.10
Wild Cluster Bootstrap P-value	0.727	0.608	0.666	0.527	0.826	0.690	0.790	0.643
Observations	2,126	2,126	2,126	2,126	2,010	2,010	2,010	2,010

Notes: This table reports estimates from a staggered Differences-in-Differences model using the 2010 CFPS. The dependent variable is the husband's age at first marriage. The sample and specifications are identical to Table 3. Standard errors are clustered at the county level and are in parentheses. Wild cluster bootstrap p-values (9,999 replications) are reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Heterogeneity Analysis

Table C1: Spousal Age Gap: (Old Husband, Young Wife) Subgroup versus the Rest of the Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.375***	0.383***	0.358**	0.372**	0.341**	0.358**	0.326**	0.344**
	(0.137)	(0.140)	(0.144)	(0.147)	(0.139)	(0.142)	(0.145)	(0.148)
Old H, Young W	4.560***	4.504***	4.527***	4.476***	4.552***	4.502***	4.525***	4.477***
	(0.193)	(0.192)	(0.200)	(0.198)	(0.202)	(0.198)	(0.207)	(0.203)
Post × Old H, Young W	-0.839***	-0.848***	-0.799***	-0.811***	-0.827***	-0.830***	-0.804***	-0.808***
	(0.261)	(0.258)	(0.273)	(0.269)	(0.272)	(0.266)	(0.282)	(0.276)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean (Old H, Young W)	5.441	5.441	5.441	5.441	5.447	5.447	5.447	5.447
Wild Cluster Bootstrap P-value	0.003	0.002	0.005	0.004	0.005	0.004	0.007	0.006
Observations	2,126	2,126	2,126	2,126	2,010	2,010	2,010	2,010

Notes: The dependent variable is the spousal age gap (husband's age at marriage minus wife's age at marriage). The specification estimates equation (3), where *Old H, Young W* indicates couples in which the husband married above the median age (23) and the wife at or below the median age (22). Columns (1)–(4) use the full sample of 2,126 ever-treated rural Policy 1 couples; columns (5)–(8) use the migration-restricted subsample of 2,010 couples. Controls include the husband's and wife's number of siblings and education category indicators, and all specifications include province and marriage-year fixed effects. Standard errors clustered at the county level are in parentheses; wild cluster bootstrap p-values from 9,999 replications (for the *Post* × *Old H, Young W* coefficient) are shown in the second-to-last row. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Number of Children: (Old Husband, Young Wife) Subgroup versus the Rest of the Sample

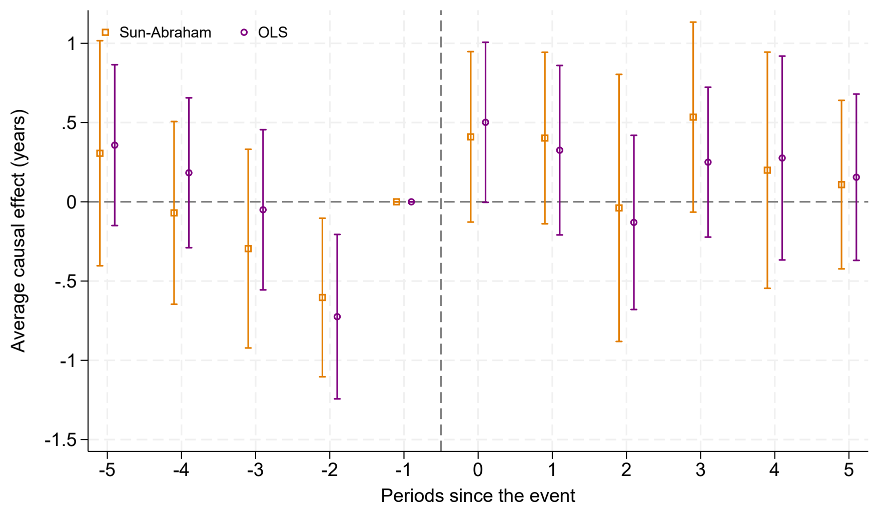
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.126***	0.126***	0.067**	0.065**	0.127***	0.127***	0.070**	0.067**
	(0.030)	(0.030)	(0.028)	(0.028)	(0.030)	(0.030)	(0.028)	(0.028)
Old H, Young W	0.076**	0.046	0.082**	0.053	0.079**	0.052	0.088**	0.060*
	(0.037)	(0.031)	(0.038)	(0.032)	(0.040)	(0.033)	(0.041)	(0.034)
Post × Old H, Young W	-0.053	-0.046	-0.063	-0.056	-0.034	-0.029	-0.047	-0.042
	(0.047)	(0.041)	(0.049)	(0.043)	(0.049)	(0.043)	(0.051)	(0.045)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean (Old H, Young W)	0.478	0.478	0.478	0.478	0.487	0.487	0.487	0.487
Wild Cluster Bootstrap P-value	0.268	0.277	0.210	0.204	0.490	0.502	0.372	0.365
Observations	64,039	64,039	64,039	64,039	60,518	60,518	60,518	60,518

Notes: The dependent variable is the cumulative number of children per couple by observation year, constructed as a panel from the ever-treated rural Policy 1 sample. The specification estimates the analogue of equation (3) on the panel, where *Old H, Young W* indicates couples in which the husband married above the median age (23) and the wife at or below the median age (22). Columns (1)–(4) use the full panel of 2,126 couples; columns (5)–(8) use the migration-restricted panel of 2,010 couples. Controls include the husband's and wife's number of siblings and education category indicators, and all specifications include province and year fixed effects. Standard errors clustered at the county level are in parentheses; wild cluster bootstrap p-values from 9,999 replications (for the *Post × Old H, Young W* coefficient) are shown in the second-to-last row. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: Spousal Age Gap: Sample Excluding the (Old Husband, Young Wife) Subgroup

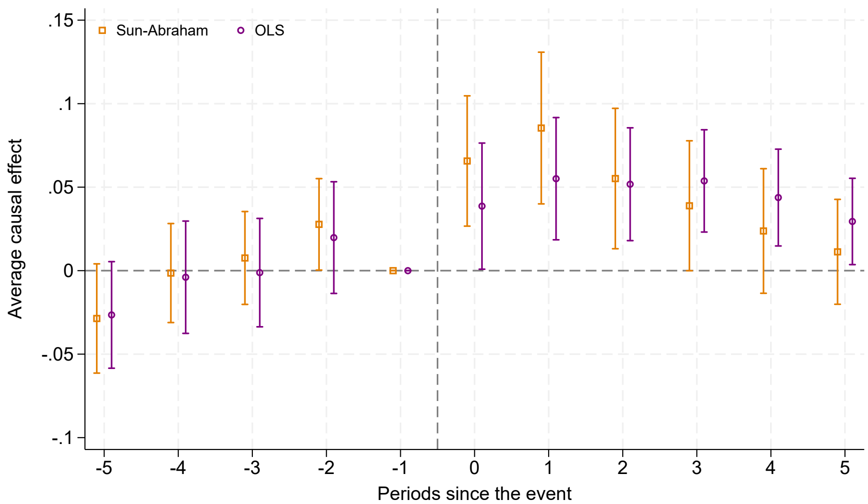
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.343**	0.347**	0.366**	0.370**	0.305*	0.317**	0.332**	0.341**
	(0.152)	(0.157)	(0.159)	(0.164)	(0.154)	(0.158)	(0.160)	(0.164)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean	0.646	0.646	0.646	0.646	0.661	0.661	0.661	0.661
Wild Cluster Bootstrap P-value	0.029	0.034	0.026	0.028	0.051	0.050	0.039	0.039
Observations	1,702	1,702	1,702	1,702	1,617	1,617	1,617	1,617

Notes: The dependent variable is the spousal age gap (husband's age at marriage minus wife's age at marriage). The specification estimates equation (1) on the sample that excludes the (Old H, Young W) subgroup (husbands marrying above the median age of 23 and wives at or below the median age of 22). Columns (1)–(4) use the full sample of 1,702 couples; columns (5)–(8) use the migration-restricted subsample of 1,617 couples. Controls include the husband's and wife's number of siblings and education category indicators, and all specifications include province and marriage-year fixed effects. Standard errors clustered at the county level are in parentheses; wild cluster bootstrap p-values from 9,999 replications are shown in the second-to-last row. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



Notes: Each coefficient is the estimated effect of the OCP exemptions on the spousal age gap at event-time ν relative to the year of policy adoption, normalized to zero at $\nu = -1$, estimated on the sample that excludes the (Old H, Young W) subgroup. Purple circles show TWFE estimates and orange squares show heterogeneity-robust estimates following Sun and Abraham (2021). Both specifications include province and marriage-year fixed effects and province-specific linear time trends, with the husband's and wife's number of siblings and education category indicators as controls. Confidence intervals are at the 95% level. The sample consists of 1,702 rural Policy 1 couples from the 2010 CFPS.

Figure C1: Event Study of the Spousal Age Gap, Sample Excluding the (Old Husband, Young Wife) Subgroup



Notes: Each coefficient is the estimated effect of the OCP exemptions on the cumulative number of children per couple at event-time ν relative to the year of policy adoption, normalized to zero at $\nu = -1$, estimated on the sample that excludes the (Old H, Young W) subgroup. Purple circles show TWFE estimates and orange squares show heterogeneity-robust estimates following Sun and Abraham (2021). Both specifications include province and year fixed effects and province-specific linear time trends, with the husband's and wife's number of siblings and education category indicators as controls. Confidence intervals are at the 95% level. The sample consists of 51,355 couple-year observations from the 1,702 sample couples in the 2010 CFPS.

Figure C2: Event Study of the Number of Children, Sample Excluding the (Old Husband, Young Wife) Subgroup

Table C4: Number of Children: Sample Excluding the (Old Husband, Young Wife) Subgroup

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.122***	0.123***	0.060**	0.060**	0.124***	0.126***	0.063**	0.062**
	(0.031)	(0.031)	(0.028)	(0.028)	(0.031)	(0.031)	(0.028)	(0.028)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean	0.330	0.330	0.330	0.330	0.329	0.329	0.329	0.329
Wild Cluster Bootstrap P-value	0.001	0.001	0.048	0.047	0.001	0.000	0.039	0.037
Observations	51,355	51,355	51,355	51,355	48,767	48,767	48,767	48,767

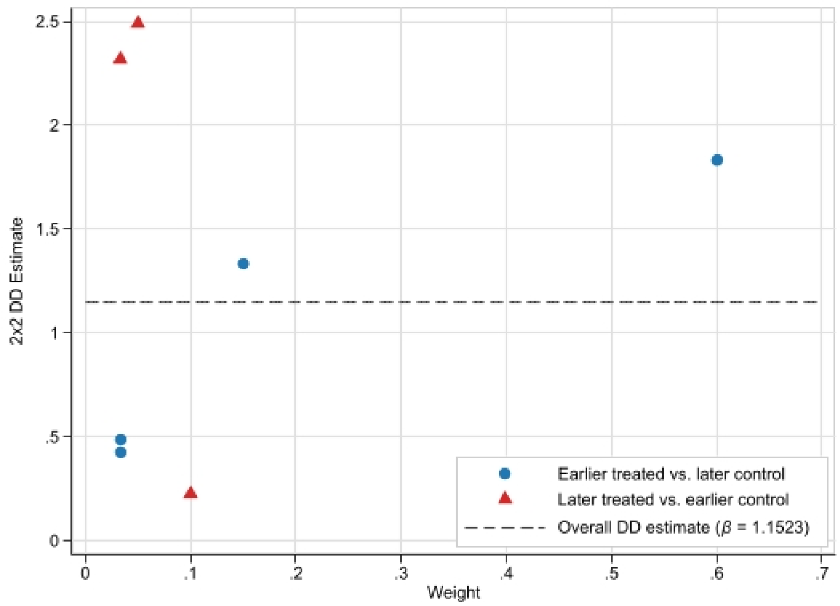
Notes: The dependent variable is the cumulative number of children per couple by observation year, constructed as a panel from the ever-treated rural Policy 1 sample. The specification estimates equation (4) on the sample that excludes the (Old H, Young W) subgroup (husbands marrying above the median age of 23 and wives at or below the median age of 22). Columns (1)–(4) use the full panel of 51,355 couple-year observations from 1,702 couples; columns (5)–(8) use the migration-restricted panel of 48,767 couple-year observations from 1,617 couples. Controls include the husband's and wife's number of siblings and education category indicators, and all specifications include province and year fixed effects. Standard errors clustered at the county level are in parentheses; wild cluster bootstrap p-values from 9,999 replications are shown in the second-to-last row. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C5: Heterogeneous Effects on Spousal Age Gap by Household Employment Sector

	(1)	(2)	(3)	(4)
Post	0.409*	0.421*	0.453*	0.467*
	(0.246)	(0.244)	(0.248)	(0.246)
Post × Agricultural Household	0.041	0.023	0.007	0.003
	(0.234)	(0.235)	(0.236)	(0.236)
Controls	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y
Pre-Policy Mean	1.71	1.71	1.71	1.71
Wild Cluster Bootstrap P-value	0.863	0.920	0.979	0.987
Observations	2,121	2,121	2,121	2,121

Notes: This table reports estimates from an interaction specification where the treatment indicator is interacted with a binary indicator for whether both spouses report agricultural employment. The dependent variable is the spousal age gap. The sample excludes couples whose employment sector is classified as unknown, reducing the sample from 2,126 to 2,121 in the full sample and from 2,010 to 2,005 in the migration-restricted sample. The base specification is otherwise identical to Table 3. Standard errors clustered at the county level are in parentheses. Wild cluster bootstrap p-values (9,999 replications) are reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

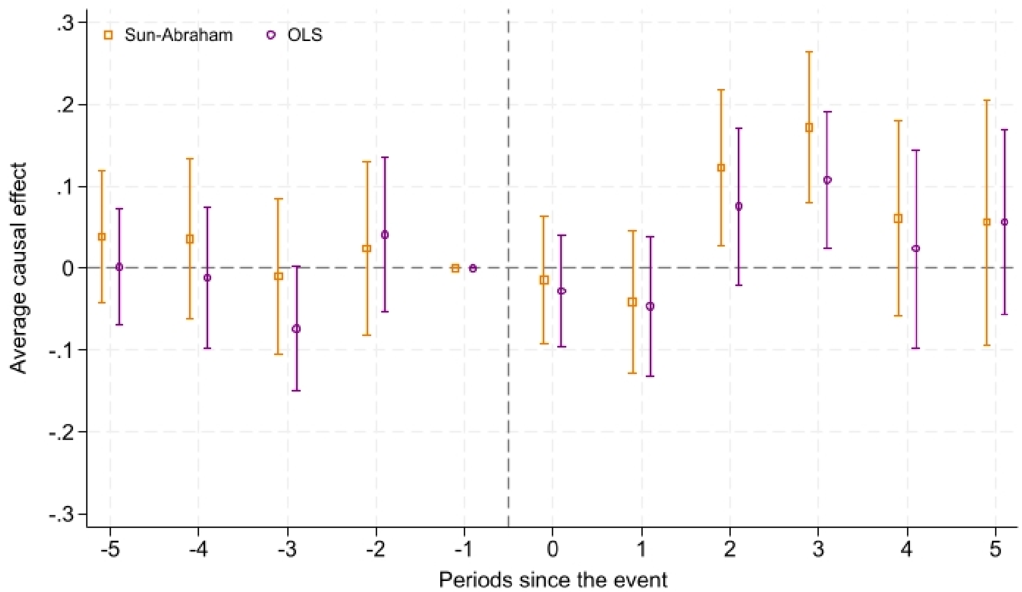
D Robustness



Notes: The figure presents the Goodman-Bacon (2021) decomposition of the TWFE estimate into its constituent 2×2 Differences-in-Differences comparisons. Each point represents one comparison, with the horizontal axis indicating its weight in the pooled TWFE estimate and the vertical axis indicating the corresponding point estimate.

Figure D1: Goodman-Bacon Decomposition for the Spousal Age Gap

E Implications for Household Division of Labor



Notes: Each coefficient represents the estimated effect of the OCP exemptions on the probability that only the husband participates in the labor market at event-time period ν relative to the year of policy adoption, normalized to zero at $\nu = -1$. The purple circle markers show TWFE estimates; the orange square markers show heterogeneity-robust estimates following Sun and Abraham (2021). Both specifications include province and marriage-year fixed effects and province-specific linear time trends. Controls include the husband's and wife's number of siblings and education category indicators. Confidence intervals are at the 95% level. The sample consists of 2,126 rural couples from the 2010 CFPS.

Figure E1: The Impact of the OCP Exemptions on the Household Division of Labor

Table E1: The Impact of OCP Exemptions on Household Division of Labor

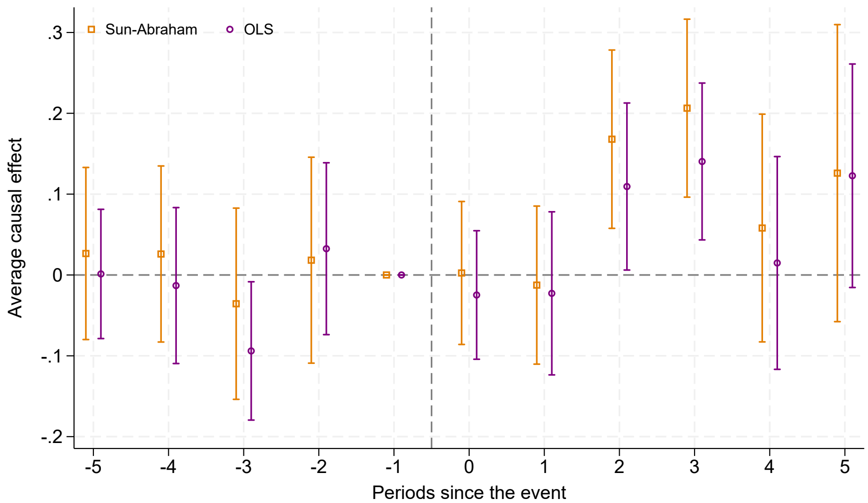
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0.015	0.016	0.017	0.016	0.021	0.020	0.019	0.018
	(0.026)	(0.026)	(0.025)	(0.025)	(0.027)	(0.027)	(0.026)	(0.026)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
Wild Cluster Bootstrap P-value	0.537	0.534	0.493	0.504	0.440	0.451	0.447	0.463
Observations	2,126	2,126	2,126	2,126	2,010	2,010	2,010	2,010

Notes: This table reports estimates from a staggered Differences-in-Differences model using the 2010 CFPS. The dependent variable is a binary indicator equal to one if only the husband participates in the labor market (reports positive wage income while the wife does not). The sample and base specification are identical to Table 3. Standard errors clustered at the county level are in parentheses. Wild cluster bootstrap p-values (9,999 replications) are reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E2: Share of Couples in Which Only the Husband Works: (Old Husband, Young Wife) Subgroup versus the Rest of the Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.021	0.022	0.021	0.021	0.027	0.027	0.025	0.024
	(0.026)	(0.026)	(0.026)	(0.026)	(0.028)	(0.028)	(0.027)	(0.027)
Old H, Young W	0.009	0.017	0.006	0.013	0.006	0.013	0.005	0.012
	(0.021)	(0.021)	(0.023)	(0.022)	(0.022)	(0.021)	(0.023)	(0.023)
Post $\times$ Old H, Young W	-0.031	-0.036	-0.026	-0.030	-0.035	-0.040	-0.031	-0.035
	(0.040)	(0.039)	(0.039)	(0.039)	(0.040)	(0.040)	(0.040)	(0.040)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean (Old H, Young W)	0.118	0.118	0.118	0.118	0.116	0.116	0.116	0.116
Wild Cluster Bootstrap P-value	0.454	0.376	0.528	0.457	0.398	0.330	0.451	0.396
Observations	2,126	2,126	2,126	2,126	2,010	2,010	2,010	2,010

Notes: The dependent variable is a binary indicator equal to one if the husband reports positive wage income while the wife does not. The specification estimates equation (3), where *Old H, Young W* indicates couples in which the husband married above the median age (23) and the wife at or below the median age (22). Columns (1)–(4) use the full sample of 2,126 ever-treated rural Policy 1 couples; columns (5)–(8) use the migration-restricted subsample of 2,010 couples. Controls include the husband's and wife's number of siblings and education category indicators, and all specifications include province and marriage-year fixed effects. Standard errors clustered at the county level are in parentheses; wild cluster bootstrap p-values from 9,999 replications (for the *Post* $\times$ *Old H, Young W* coefficient) are shown in the second-to-last row. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



Notes: Each coefficient is the estimated effect of the OCP exemptions on the probability that only the husband participates in the labor market at event-time ν relative to the year of policy adoption, normalized to zero at $\nu = -1$, estimated on the sample that excludes the (Old H, Young W) subgroup. Purple circles show TWFE estimates and orange squares show heterogeneity-robust estimates following Sun and Abraham (2021). Both specifications include province and marriage-year fixed effects and province-specific linear time trends, with the husband's and wife's number of siblings and education category indicators as controls. Confidence intervals are at the 95% level. The sample consists of 1,702 rural Policy 1 couples from the 2010 CFPS.

Figure E2: Event Study of the Share of Couples in Which Only the Husband Works, Sample Excluding the (Old Husband, Young Wife) Subgroup

Table E3: Share of Couples in Which Only the Husband Works: Sample Excluding the (Old Husband, Young Wife) Subgroup

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.048*	0.046	0.046	0.044	0.048	0.046	0.045	0.042
	(0.028)	(0.028)	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)
Controls	N	Y	N	Y	N	Y	N	Y
Province-Specific Linear Time	N	N	Y	Y	N	N	Y	Y
Migration Accounted	N	N	N	N	Y	Y	Y	Y
Pre-Policy Mean	0.134	0.134	0.134	0.134	0.135	0.135	0.135	0.135
Wild Cluster Bootstrap P-value	0.086	0.100	0.097	0.116	0.107	0.121	0.127	0.146
Observations	1,702	1,702	1,702	1,702	1,617	1,617	1,617	1,617

Notes: The dependent variable is a binary indicator equal to one if the husband reports positive wage income while the wife does not. The specification estimates equation (1) on the sample that excludes the (Old H, Young W) subgroup (husbands marrying above the median age of 23 and wives at or below the median age of 22). Columns (1)–(4) use the full sample of 1,702 couples; columns (5)–(8) use the migration-restricted subsample of 1,617 couples. Controls include the husband's and wife's number of siblings and education category indicators, and all specifications include province and marriage-year fixed effects. Standard errors clustered at the county level are in parentheses; wild cluster bootstrap p-values from 9,999 replications are shown in the second-to-last row. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.