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For Better or for Babies: Fertility Constraints and Marriage in China

Lucie Giorgi
Eva Raiber

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Lucie Giorgi[1] and Eva Raiber[1]

[1] *Aix Marseille Univ, CNRS, AMSE, Marseille, France*

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Abstract

We examine how the 2015 relaxation of China's one-child policy affected marriage outcomes. Before the reform, some groups were already permitted to have two children. In China, where the sex ratio is heavily skewed toward men, being exempt from the one-child constraint may have been a desirable characteristic for marriage, increasing men's marriage odds. Using detailed policy data on exemptions and individual data from 2010-2018, we find that after the relaxation, men previously allowed a second child are less likely to marry compared to those not allowed. There is no effect for women. The results suggest that differential fertility constraints distorted who got married by advantaging certain men when there was a demand for a second child and strong marriage competition. Furthermore, suggestive evidence shows that the relaxation increased matching by education when exemptions were moderately widespread, indicating that fertility constraints also shaped who married whom.

Keywords: fertility, family planning, marriage, China
JEL codes: J12, J13, J18, O53

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1 Introduction

Marriage is a formalized union between two people that has important socio-economic consequences. It is one of the most crucial lifetime decisions with effects on labour market participation, consumption, mental health, and physical well-being.[1] In countries with skewed sex ratios, such as China and India, many men remain unmarried, which entails lower life satisfaction, poorer health outcomes, elevated risks of early mortality, and higher involvement in criminal behaviour (Edlund et al., 2013; Zhou and Hesketh, 2017; Chang et al., 2024). Understanding what drives marriage formation is therefore crucial not only for individuals and families, who adapt pre-marital investments and long-term planning to marital prospects,[2] but also for governments, which must adjust family, health, education, and retirement policies accordingly.

In many cultural settings, marriage is considered a necessary step before having children. Wanting children can thus motivate marriage, and people might sort into matches depending on their fertility plans. Fertility plans can in turn be shaped by family planning policies.[3] Family planning policies can favour some groups while constraining others by imposing a higher cost for having and raising children. Most countries nowadays have family planning schemes, increasingly designed to encourage childbearing by relaxing financial constraints.[4] We investigate whether policy-related fertility constraints can affect who gets married.

This paper studies the end of China's One-Child Policy (OCP), one of the largest and most restrictive family planning policies of all time. In a country where children are important culturally and financially, the policy imposed a strong burden on households. Yet, several groups were exempted from the strict one-child constraint and allowed to have two children without having to pay a monetary fine. These conditions were dependent on specific socio-economic characteristics of either one or both spouses, giving some people a "second-child advantage". Through this policy

[1]See for example Becker (1974); Chiappori et al. (2018); Calvo et al. (2024) for economic effects and Gove et al. (1990); Horwitz et al. (1996); Simon (2002); Marcussen (2005); Horn et al. (2013); Wang et al. (2020); Huntington et al. (2022) on the relationship with mental and physical well-being.

[2]For example, Wei and Zhang (2011) link the unbiased sex ratio to household saving patterns and Wei et al. (2017) to home ownership. Han and Shi (2019) show how pre-marital investment changes with expected marriage competition.

[3]Examples are Bailey (2010); Joshi and Schultz (2013); Bailey (2012); González (2013); Zhang (2017); Raute (2019).

[4]In 2019, only 27% of countries did not have any official policy. The share of countries having family planning policies aiming at raising fertility levels increased from 9% in 1976 to 28% in 2019 (United Nations Department of Economic and Social Affairs, Population Division, 2021).

relaxation, those previously exempted from the strict one-child constraint and eligible to have two children lost their relative second-child advantage. We investigate if losing this advantage affects getting married.

Why should relaxing fertility constraints affect marriage rates? First, as postulated in Huang et al. (2023), if a larger family size is preferred, relaxing fertility constraints can increase the number of marriages. Allowing couples to have more than one child raises the utility of marriage, thereby increasing incentives to marry among those previously restricted. As the number of marriages increases, we should observe a change in marriage rates for both men and women. This mechanism relies on the assumption that there are men and women who do not want to get married when they are allowed only one child, but want to get married when they can have two children.

We propose a second mechanism which relates to competition for spouses when there is an unbalanced sex ratio. China is a culture in which sons are important (Gupta et al., 2002). Over time, exacerbated by the fertility constraints, this has led to a significant sex ratio imbalance (Ebenstein, 2010; Li et al., 2011; Eklund et al., 2017). Nowadays, there are many more marriageable men than women[5] and unmarried rates among men have been rising (Jiang et al., 2014; Eklund et al., 2017; Han and Zhao, 2022). Before the relaxation, those exempt from the one-child constraint could have had a marriage-market advantage. When there is a strong sex ratio imbalance, those of the abundant sex, men in this case, compete for spouses. Being allowed to have two children can increase their chances of being chosen as a spouse if having a second child is a valued outcome of marriage. Everything else constant, exempt men could thus have higher chances of getting married relative to those subject to the one-child constraint. With the relaxation, they lose this relative advantage: the likelihood of getting married for previously exempt men decreases while the likelihood of previously non-exempt increases. We expect this effect only for men, while the likelihood of getting married stays constant for women.

Using detailed province-level policy data for family planning regulations, we identify who is allowed to have a second child based on observable quasi-exogenous characteristics. Regulations were set on the provincial level and varied between different socio-economic groups. Most exemptions targeted poorer groups, allowing rural and minority households a second child under certain

[5] The sex ratio at birth, i.e. the number of males per 100 females, increased from 108.5 in 1982 to 117.96 in 2010 (National Bureau of Statistics of China, 2012).

conditions. We link policy data to bi-annual individual data from the China Family Panel Study (CFPS), 2010–2018. For each wave, we define the cohort of marriageable men and women. We use a difference-in-differences approach to estimate how the 2013–2015 policy relaxation affected marriage odds, depending on whether individuals were allowed to have a second child.

We find that the policy relaxation did not affect women's marriage rates. However, there is a significant effect for men: Compared to those previously not allowed a second child, those previously allowed are less likely to get married after the policy relaxation. Being allowed to have a second child thus seems to have been an advantage in the marriage market, and losing this advantage decreases marriage odds for previously exempt men while increasing them for those previously not exempt. We find that the effect is concentrated in areas where fertility rates are high. This confirms that being allowed a second child is only a valuable characteristic in the marriage market when people want to have two children. Furthermore, the effect is driven by provinces with strongly male-biased sex ratios, suggesting that competition matters. When competition for a spouse among men is strong, being allowed to have a second child can increase marriage odds. It implies that women who value children as part of the marital outcome, on the margin, selected those with whom they could have two children.

The results suggest that previously advantaged men experienced a drop in their marriage rates due to the policy change. In a society where marriage is an important outcome, they are losing out on the relaxation policy. The results are robust to several sensitivity analyses, such as omitting one socio-economic group, changing the age threshold of the "marriageable age", extending the time frame to include the smaller waves from 2020 and 2022, or focusing on the balanced panel. Importantly, we generally do not find significant pre-trends in the years before the policy change regarding marriage rates.

Using the same difference-in-differences approach, we find that the relaxation increased the likelihood to have a second child among those previously not allowed a second child compared to those who were previously allowed. However, we observe a significant pre-trend in this outcome concentrated in low fertility regions where we did not find a marriage effect, suggesting that couples converged towards having one child, independent of the policy settings, and being allowed to have a second child was not a marriage-market advantage. In high fertility regions, where we find an effect on marriage rates, the policy relaxation increased second births among previously restricted couples, with no evidence of pre-trends.

Finally, we investigate how the relaxation might have influenced not just who gets married but also who marries whom. We postulate that in the absence of policy-related fertility constraints, matches would be on match quality measured by the level of education, a key characteristic for marriage in China (Raiber et al., 2023; Rossi and Xiao, 2024; Yu et al., 2025). We find that in provinces with a medium share of the population previously exempted, marriage matches become more associated with education after the relaxation. The coefficients are significant at 10% only and should be taken with caution due to data limitations restricting the number of province-cohorts included in the analysis. Yet, the results are in line with previous literature (Huang et al., 2023) that the policy-related fertility constraints not only affected who got married but also who married whom.

This study contributes to several strands of the literature. First, it highlights another unintended effect of the OCP and fertility restrictions in general. Most closely related, Huang et al. (2023) investigate the effect of the OCP on marriages and inter-ethnic marriages. Looking at marriages in 1980 and 1990, where the sex ratio imbalance was less of an issue, they find that the OCP restrictions decrease marriage rates. They also find that the exemption for minorities applying to one spouse or both spouses affects inter-ethnic marriages. The results from this paper are in line with these findings. We document that these marriage distortions continued until the relaxation of the OCP and that it later on predominantly affected men due to the rising sex ratio imbalance. Relatedly, fertility restrictions in China have also been found to have affected ethnic identity (Jia and Persson, 2021), education (Huang et al., 2021; Raiber, 2022), and mental health in retirement (Chen and Fang, 2021).

Second, the paper adds to the literature on spousal preferences. The biological and anthropological literature highlights that male preferences are often shaped by the preference for a highly fertile spouse, where fertility is defined as the probability of present reproduction. Women, on the other hand, value characteristics related to men's biological quality and investment potential, such as their financial situation.[6] Women's preference for men's financial resources is generally explained by their association with greater investment in children, which improves children's outcomes, and with the potential to support multiple children. This paper provides direct evidence that women

[6] See, among many, Buss (1989); Jones (1996); Bovet et al. (2018); Walter et al. (2020).

select spouses based on the man's reproductive capacity - the expected number of future children. At the margin, some women preferred a husband with whom they had the option to have two children.

Third, we complement the literature on the consequences of a skewed sex ratio on marriage outcomes. In his seminal work, Becker theorizes bargaining over marriage and stresses the importance of the sex ratio for relative marriage outcomes (Becker (1973); Becker (1981)). In general, the scarce sex secures better marriage outcomes and greater gains from marriage, while more individuals of the abundant sex remain unmarried. Indeed, a female-biased sex ratio has been found to increase men's likelihood to get married, improving their marriage outcomes (Abramitzky et al., 2011), and decreasing women's bargaining power within the household (Brainerd, 2017; La Mattina, 2017). Conversely, a male-biased sex ratio increases women's marriage rates (Angrist, 2002; Francis, 2011; Grosjean and Khattar, 2019) and in China is associated with behavioral changes such as reduced alcohol and tobacco consumption among men where women are scarce (Porter, 2016). Yet, Wei and Zhang (2019) highlight the difficulties faced by high-income women in China, whose marriage prospects worsen under competitive pressures despite a male-biased sex ratio. Finding a marriage effect for men in areas with a heavily male-biased sex ratio points to intense spousal competition among men, with women in a stronger position to choose among potential partners. It also suggests that the relaxation of fertility constraints will raise unmarried rates among previously exempt men, thereby likely reinforcing existing disadvantages for rural men and ethnic minorities.

Finally, we contribute to the analysis of marriage matching by education. The literature finds that positive assortative matching - matching of spouses with the same level of education - has been on the rise for multiple decades in most countries (Mare, 1991; Chiappori et al., 2012; Song et al., 2017; Hirschl et al., 2024). Several reasons for this trend have been advanced, such as changes in the educational distributions of men and women, increases in female labor market participation rates, an increase in the skill premium, or geographic sorting (Fernandez et al., 2005; Greenwood et al., 2016; Permanyer et al., 2019; Leesch and Skopek, 2023; Mao and Wen, 2025). Assortative mating is linked to increases in income inequality (Greenwood et al., 2014; Eika et al., 2019). Our suggestive results indicate that the fertility constraints distorted matching along education levels, and relaxing the fertility constraints increases it with relevant consequences for income inequality, which is expected to increase.

2 Fertility Policies, Marriage, and Sex Ratio Imbalances in China

2.1 From the One-Child Policy to the Two-Child Policy

China introduced measures to decrease population growth as early as 1962 (Chen and Fang, 2021). During the "later, longer, fewer"-policy, launched in 1971, families were encouraged to have fewer children, starting later, and with longer birth intervals (Attane, 2002; Zhang, 2017; Wang et al., 2017). Birth quotas were set and enforced on the provincial level with the promotion of contraceptive methods, but also pressure to abort or get sterilized. In 1979, the central government announced the One-child policy (OPC) that had at its core the goal for each couple to have one child only. The policy was enforced with monetary fines but also forced abortion and sterilizations, or mandatory IUD insertion (Banister, 1991; Li et al., 2011; Feng et al., 2016). Again, the policy was implemented on the provincial level.

Yet, the policy was not implemented equally for everyone. In the 1980s, during the implementation phase, different exemptions were introduced. While married couples always had to apply for a "child permit" (unmarried couples or singles were not allowed to have children), some could apply for a "second child permit" under different eligibility rules. While these were first informal, responding to the resistance to the policy, particularly in rural areas, they were formalized in family planning guidelines in the 1990s (Scharping, 2013). Importantly, they varied between provinces and between different socio-economic groups within provinces (Gu et al., 2007; Raiber, 2022; Han and Zhao, 2022).

The two most important groups targeted by the early exemptions were ethnic minorities and the rural population. In the rural areas, where fertility rates were high, the policy implementation met resistance (Scharping, 2013). Agricultural households relied on (male) children to provide labour and support during old age. As a response to this, most provinces allowed couples with a rural household registration status whose first child was a girl to apply for a second child permit. While over time the number of households active in agriculture decreased, exemptions stayed coupled with the rural household registration status. The household registration status, *hukou*, gets assigned at birth (until the mid-90s by the mother's status) and was extremely difficult to change. In some provinces, rural couples were allowed to have two children, independent of the sex of the firstborn and the status of their spouse (Gu et al., 2007; Raiber, 2022).

Ethnic minorities in most provinces were allowed to have two children, and in some autonomous regions, even more. This often only applied to ethnic minorities that were below a population size threshold. Yet, this was not the case in all provinces, and there was also variation in whether both spouses had to be from the ethnic minority or only one of them. In some cases, only rural ethnic minorities were allowed to have two children. Ethnic minority groups are generally among the poorest groups in China (Wang, 2007; Gustafsson and Sai, 2009), and concessions toward their fertility options are often motivated by anti-poverty goals (Scharping, 2013).

The provinces also had other criteria for remarried couples, couples that adopted or had a child overseas. Several also allowed specific groups whose work was a great hardship to have two children: These often included fishermen, miners and army veterans. A vague condition of "economic hardship" often complemented the criteria under which married couples could apply for a second child permit. This underlines that, at least at the beginning of the OCP, the exemptions had a pro-poor focus. Children, especially sons, were not only culturally important to carry on the family name, but they were also important sources of financial assistance during old age.[7]

After the first introductory years, the punishment for having too many children became mainly monetary: Married couples who had a second child without being allowed to were subject to monetary fines that were often a multiple of their annual income. Furthermore, they risked losing their job, especially those working for a government agency, and other career-related punishments. They would have less access to government infrastructure, for example, formal childcare slots (Scharping, 2013). This meant that wealthy couples who could afford it could "pay" to have a second child.

The "one child per family"-goal was announced to apply to one generation. In the 1990s, provinces thus introduced an exemption for couples in which both spouses were only children. While at the beginning this exemption had little bite, as most people had at least one sibling, it became more important in the 2000s for those who were already born under the OCP (Raiber, 2022).

The OCP was lifted in several steps over three years (Feng et al., 2016). The process began in March 2013, when China established the National Health and Family Planning Commission

[7] Recent literature from the "later, longer, fewer"-campaign suggests that having fewer children did not decrease financial resources in old age. Yet, they find that they have fewer visits and are mentally more vulnerable (Huang et al., 2021). The effect of the OCP on this still needs to be evaluated.

by merging two institutions associated with family planning and health. In November 2013, the government announced that only one spouse had to be an only child for the couple to be allowed to have a second child in all provinces. This policy was implemented in the following year. In 2014, discussions about a general two-child policy were ongoing (Guangzong, 2014; CNBC, 2014). Finally, in October 2015, the central government declared that all couples would be allowed to have two children starting in 2016. Couples who voluntarily had one child during the OCP would continue to be rewarded and subsidised if they chose to have another child now (Wang et al., 2017). Yet, of the estimated 11 million couples who became eligible with the 2013 change, only 1.69 million had applied to have their second child by August 2015 (Feng et al., 2016). This led to concerns that after the relaxation of the OCP, families would not take the opportunity to have a second child, especially as raising a child is considered costly by many in China. Already during the OCP, some families chose to have only one child when they were allowed to have two (Zhenzhen et al., 2009). However, some studies showed an increase in fertility with the two-child policy (TCP) (Zhang and Zheng, 2021; Wu, 2022).

2.2 Marriage Pattern and Sex Ratio

China is traditionally a patriarchal and patrilineal society and starting starting a family is an important goal (Yeung and Hu, 2013; Hu and Scott, 2016; Ma et al., 2019). Sons are particularly valued as they contribute economically to the household, perpetuate the family line and provide social security for their parents in their old age (Li and Cooney, 1993; Moore, 1998; Das Gupta et al., 2003; Banister, 2004; Murphy et al., 2011). Although attitudes towards marriage and family life have become more liberal (Hu and Scott, 2016), marriage has been, until lately a prerequisite for being allowed to have a child (Scharping, 2013; Wu, 2022). Even if cohabitation outside marriage has increased in recent years, it remains very rare and, in most cases, it is only a step before marriage (Yeung and Hu, 2016). Thus, even if China has moved closer to Western norms in many regards, marriage remains very traditional, valued, and widespread (Jones and Yeung, 2014). At this point, China contrasts with other East Asian countries such as Japan and South Korea that have seen substantial drops in marriage rates (Raymo et al., 2015). Still, there has been a downward trend in marriage rates in the past 10 years (Rossi and Xiao, 2024; National Bureau of Statistics of China, 2025).

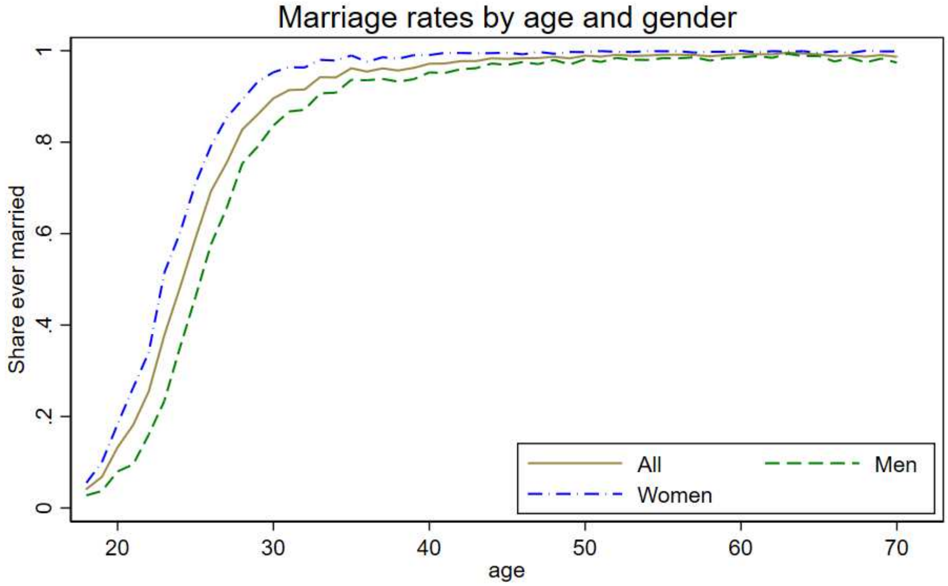


Figure 1: Marriage rates by age and gender (respondents from the CFPS interviewed between 2014 and 2018).

Men and women still tend to get married relatively early (Yeung and Hu, 2016; Raymo et al., 2015). Marriage is legal at 22 years for men and 20 years for women. The age of first marriage increased from 24 in 1980 to 26 in 2010 for men and from 23 in 1980 to 23.9 in 2010 for women (Raymo et al., 2015).[8] Yet, due to the imbalance in the sex ratio, there is a surplus of men who struggle to find a partner, especially those who are less educated and from poor rural areas (Jiang et al., 2014; Han and Zhao, 2022). This gap can also be seen in Figure 1, which illustrates the marriage rates by age in recent years from the China Family Panel Study (CFPS). While the initial gap between men's and women's marriage rates can be explained by men marrying later, the gap never closes until age 50. The generation that is strongly affected by the sex ratio imbalance was up to 40 years old in 2018. Looking at those between 35 and 40, nearly all women are married (over 95%), yet only around 90% of men are married. This disparity is expected to increase with age.

[8] In comparison, in 2010, the average age at first marriage for men was around 31 and for women around 29 in Korea, Japan and Taiwan (Raymo et al., 2015)

3 Data and Estimation Strategy

3.1 Individual Data

We use data from the China Family Panel Survey (CFPS), a survey launched in 2010 and conducted every two years by the Institute of Social Science Survey (ISSS) of Peking University. The CFPS provides information on the economic and non-economic aspects of the Chinese population. The survey covers information on economic activities, educational achievements, family relationships, migration, and health. In the baseline survey, 15,000 families and nearly 30,000 individuals within these families were interviewed. We use the waves from 2010 to 2018. The available sample is constructed to be nationally representative. Some autonomous regions with specific statuses were not included in the CFPS (Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia and Hainan).

We construct our sample as all individuals within the age range when they are allowed to marry, of childbearing age and expected to be married at that time. We choose as the lower threshold 20 for women and 22 for men, the minimum legal age for marriage. We set 35 as the upper threshold. Jones and Yeung (2014) and Yeung and Hu (2016) find that almost all women are married by the age of 30. Even though the legal age of marriage is 20 and 22, some people reported being married before that. We vary the upper and lower levels of our sample cut-offs as robustness checks.

We define an individual as either *married* or *never married*. *Married* includes those who were married at least once in their life. Within our age group, divorced and widowed individuals are rare. We consider those cohabiting as *never married*. Only married couples are allowed to have children, and cohabitation in this context is usually an intermediate step before marriage.

The survey is collected every two years, and a large share of respondents are present in all waves. However, over the years, to account for attrition and increased coverage, new respondents were added. Specifically, respondents added in 2012 are different from respondents in 2010 on certain dimensions (see section 3.3 Descriptive Statistics), presumably to increase the representativeness of the survey.

We restrict the sample to those between 20 (women)/22 (men) to 35 years in a given year. We thus treat our sample as a repeated cross-section. We use the information on those interviewed in 2010 to create a cross-sectional panel for 2008. However, in the follow-up questionnaires, not all questions included in the 2010 baseline were asked. For one, while we know if people are married or not, we lack information on the exact year of marriage for a significant part of the sample. Furthermore, individuals were not asked about their siblings. For an important share of respondents, we could recover this information from the parents' questionnaire. Yet, for nearly 20% of the sample, this was not possible. We exclude them from our analysis.

3.2 Policy Data

We collected all relevant articles from the provincial Family Planning Regulations valid between 2008 and 2014.[9] These specify the conditions under which married couples can apply to have a second child. We use only criteria that are quasi-exogenous and observable: rural household status, having siblings/brothers, and minority status. The household status is generally inherited from the parents, as is the ethnic minority status. We also assume that at the time of marriageable age, their parents will not have another child making the "only child"-status unlikely to change at that point.

We disregard exemptions that are based on the job type, disability of the first child, or living in a specific remote or border area as these are either endogenous (job type), not yet realized (e.g., disability of first child), or unobservable (e.g., living in a remote area).[10] We also do not include special rules for remarried couples.[11] Appendix Table A1 summarizes the relevant provincial policies in 2013.

We categorize individuals into different exemption groups.

- Group 1 includes those allowed two children regardless of their spouse's characteristics (e.g., only one partner needs to be an ethnic minority).
- Group 2 consists of those allowed two children only if their spouse shares the same characteristic (e.g., both partners must be ethnic minorities).

[9] We located the relevant provincial Family Planning Regulation documents online from different sources.: mainly the "Legal and Regulatory Database", a Chinese national large-scale full-text retrieval database of laws and regulations, based at https://falvdb.com/ and the Law Database of the Pekin University (https://www.pkulaw.com/). The translated documents, including the specific source, are available upon request.

[10] The county within the CFPS is anonymous. However, specific areas such as remote or border areas are less likely to be covered.

[11] Even if divorce is increasing, remarriage is still stigmatised and rare (Ma et al., 2018; Hu and To, 2018; Hu and Qian, 2019).

- Group 2b includes those whose eligibility depends on both their spouse's characteristics and the sex of their firstborn—specifically, rural couples who are allowed a second child only if their firstborn is a girl.

- Group 3 comprises individuals who were not allowed to have two children based on their characteristics.

For our empirical analysis, we group those in groups 1 and 2 as "2-child eligible pre-TCP". This has two reasons: First, in 2014, only children moved from group 2 to group 1 based on the government announcement that all couples with one spouse being an only child were allowed to have two children in 2013. Thus, if they were in different treatment groups, they would change their treatment status within the time of implementation, making the interpretation of the results difficult. Second, how strong the effect is might depend on being in group 1 and group 2, but also the distributions of the group characteristics within the relevant marriage set, and on in-group preferences, which vary with the different groups. Lacking empirical information on this, and without a clear prediction from the theoretical model (developed in Appendix B, summarised in section 4), we decided to use the most transparent method. We include those in group 2b as half-"2-child eligible pre-TCP". We presume that at the time of marriage, couples expect the first child to be a girl with a likelihood of around 50%, and then be able to have a second child.

As illustrated in Table 1, around 20 to 25% of the population are allowed to have a second child (the variable "2-child eligible pre-TCP" equal to 1) and around 50% are allowed to have a second child if their firstborn is a girl (the variable "2-child eligible pre-TCP" equal to 0.5). Between 20 and 25% have no advantage. Appendix Figure A1 illustrate the variation of the three relevant socio-economic groups.

3.3 Descriptive Statistics

The final sample for our main specification of men and women in their marriageable age (men: 22 - 35; women: 20 - 35) consists of 40,891 observations from 2010 to 2018, with around 6400-8300 observations per year. As summarized in Table 1, Han individuals - the ethnical majority - represent around 90% of our sample, which is close to the proportion within the total population of China (National Bureau of Statistics of China, 2020). We have a higher share of women than men in our sample in 2008 and 2010 as we have a lower threshold for women to be included (20 years old for women and 22 for men), but this majority disappears with the added respondents in 2012. The majority of the sample has a rural residence status. Changes between the 2010 survey and the following years are due to more respondents with a rural household status being added to the survey. The marriage rates vary between 74% and 59%. The decrease in the share of married individuals could be due to the resampling between the years, explained by higher attrition among those who get married over the year, or indicative of a decreasing trend in marriage rates.

Table 1: Summary statistics

	(1) 2008	(2) 2010	(3) 2012	(4) 2014	(5) 2016	(6) 2018
Female	0.58	0.57	0.48	0.47	0.49	0.49
Age	28.24	28.00	27.56	27.66	27.80	28.03
Han	0.91	0.91	0.92	0.92	0.92	0.91
Rural residence status	0.65	0.66	0.71	0.73	0.75	0.75
Only child	0.16	0.19	0.20	0.20	0.20	0.20
Group 1	0.04	0.05	0.05	0.22	0.23	0.23
Group 2	0.17	0.19	0.21	0.04	0.03	0.03
Group 2b	0.46	0.46	0.51	0.52	0.53	0.52
Group 3	0.33	0.30	0.24	0.22	0.22	0.21
2-child eligible pre-TCP	0.44	0.47	0.50	0.52	0.52	0.53
Married	0.74	0.74	0.64	0.62	0.63	0.60
Observations	6391	6210	6672	6629	7608	8266

Note: Based on the China Family Panel Study 2010 - 2018. Each year, men and women of marriageable age (men: 22 - 35; women 20 - 35) are included in the survey. *Exception: 2008 is based on the data from 2010, using information on the marriage year.

3.4 Empirical Specification

For the empirical specification, we use a Difference-in-Differences estimation where those being "2-child eligible pre-TCP" are defined as our treatment group. The policy change came in several steps between 2013 and 2015. The years 2012 and prior thus constitute the "pre-period" and those after 2015 is the "post-period". In specification 1, we use a post indicator for the years 2016 and 2018 and use the years 2008 - 2012 as the pre-groups. We exclude the year 2014 as it is partially treated. In specification 2, we use indicators for the different survey years. As such, we can investigate pre-trends comparing 2008 and 2010 to 2012, as well as the dynamic treatment effects of the relaxation. We estimate the specifications separately for men and women.

Specification 1 is the following:

$$married_{ipt} = \alpha\ 2child\_eligible\_preTCP_{ip} \times post_t + \beta\ X_i + \gamma_t' \rho_p + \phi_c + \epsilon_{ipt} \tag{1}$$

where $married_{ipt}$ corresponds to 1 if individual i in province p has ever been married in year t (including those currently married, divorced or widowed), and 0 otherwise. $2child\_eligible\_preTCP_{ip}$ is defined according to the definition in the policy data subsection. We include the difference-in-differences indicators $post_t$ equal to 1 for 2016 and 2018 (2014 is excluded from this specification). The coefficient of interest is thus α. The regression includes baseline levels for our treatment variable $2child\_eligible\_preTCP_{ip}$.

We include year times province fixed effects $\gamma_t' \rho_p$, allowing for differential province trends. We also include a vector of individual-level controls X_i with indicators for the relevant socio-economic groups (hukou status, minority status, only child), an indicator for the exemption groups (spouse-dependent or independent advantage, or half-advantage - see previous subsection), and relevant individual characteristics (age, age squared and level of education). We also add county-level fixed effects which are below the province level (ϕ_c). The standard errors based on the unobserved error term ϵ_{ipt} are clustered on treatment level (groups within the provinces).

We further estimate specification 2:

$$married_{ipt} = \alpha\ 2child\_eligible\_preTCP_{ip} \times \gamma_t + \beta\ X_i + \gamma_t' \rho_p + \phi_c + \epsilon_{ipt} \tag{2}$$

where include year indicators γ_t for each CFPS wave with 2012 as the baseline year. The coefficients of interest are thus α: The coefficients for the years 2008 and 2010 measure the pre-trends. The coefficients for 2014, 2016 and 2018 measure the treatment effect.

4 Potential Marriage Effects of the Policy Relaxation

Why should policy-related fertility constraints have an effect on marriage? Such constraints vary the cost of having an additional child, depending on the characteristics of the couple. In Appendix

B, we formally derive two mechanisms through which policy relaxation may influence marriage decisions; here, we briefly summarise them.

Mechanism 1: The policy relaxation increases the number of marriages among those previously not allowed to have two children.

The first mechanism, proposed by Huang et al. (2015) and formalized in Appendix B.1 and B.2 (Propositions 1 and 2), builds on the idea that children are a central outcome of marriage. Allowing couples to have a second child raises the marriage surplus, defined as the additional utility generated by forming a marriage compared to remaining single. A higher surplus increases the incentive to marry, so marriage rates are expected to rise when fertility constraints are relaxed, among couples who desire more than one child.

If this mechanism holds, we expect that individuals who were previously not exempt from the one-child limit become more likely to marry following the policy change, whereas the marriage likelihood of already exempt groups remains unaffected. Comparing exempt and non-exempt groups, therefore, provides a test of whether marriage rates converge after the reform. Since marriages involve both men and women, the effect should be symmetric across genders. The mechanism relies on two important conditions. First, there must be couples who prefer two children. Second, there must be "marginal couples": men and women who would remain unmarried under a one-child constraint but choose to marry once permitted to have two children. It is debatable if there are couples fulfilling both conditions at the same time – preferring two children, but not wanting to get married if they are allowed to have only one child.

Mechanism 2: The policy relaxation decreases marriage rates of previously exempt men relative to non-exempt men.

The second mechanism builds on the imbalanced sex ratio, and is formalized in Section B.3 (Proposition 3). When one side of the marriage market is more abundant – in this case, men – they must compete for spouses, while the scarcer side – women – can be more selective. If at least some women prefer to have two children, they will favor men who are eligible for a second child, giving these men a "second-child advantage" in the marriage market. This increases their likelihood of marrying. Importantly, men eligible for a second child, whether eligibility depends on their spouse's characteristics or not, enjoy an advantage over those who are never eligible.

If this mechanism holds, men who were previously exempt from the one-child limit lose their "second-child advantage" once the policy is relaxed, placing them on equal footing with non-exempt men. As a result, their likelihood of marriage declines, while marriage rates for previously non-exempt men rise. Because of the skewed sex ratio, we expect this effect to be asymmetric, affecting only men. The mechanism requires that a substantial share of women value the option of having more than one child, and that the sex ratio is significantly biased toward men.

5 Results: Effect of the Two-Child Policy on Who Gets Married

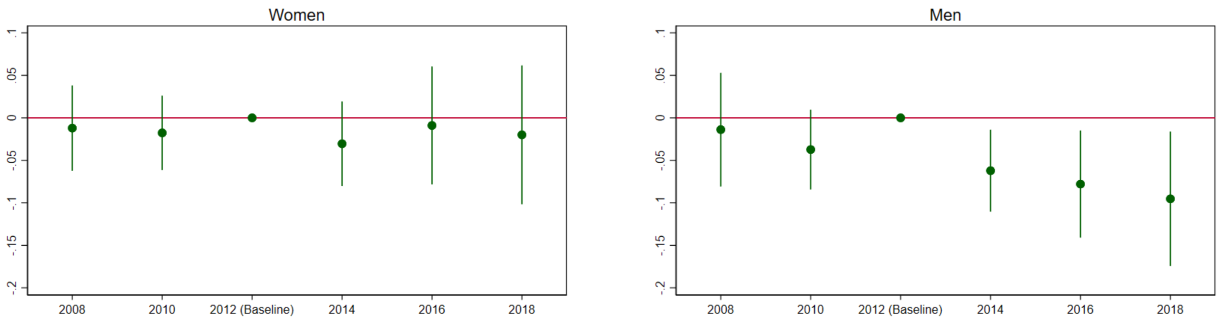
5.1 Main Results on Men and Women

Table 2 summarises the results based on specification 1, which includes a post-indicator for the years 2016 and 2018, and excludes the intermediate period of 2014. We find that there is no effect for women, with the coefficient being close to 0 (columns 1 - 3). However, we find that there is a significant negative effect on men (columns 4-6). Compared to those previously not eligible for a second child, eligible men are less likely to get married after the policy relaxation. The result holds with county fixed effects and controlling for the local fertility rate.

Table 2: Effect of the Two-Child Policy on Marriages: Specification 1

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.013	-0.002	0.006	-0.054**	-0.070**	-0.064**
	(0.604)	(0.959)	(0.833)	(0.039)	(0.013)	(0.020)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	18133	16096	15519	17013	15037	14338

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, Han, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.



(a) Effect of the TCP on marriages for women

(b) Effect of the TCP on marriages for men

Figure 2: Effect of the Two-Child Policy on Marriages by Gender

Note: See Table 3, columns 2 and 5. Linear regression with 2012 as the baseline and marriage as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table 3 shows the results for specification 2, where we include interactions for each survey year and use 2012 as the baseline year. The results are illustrated in Figure 2. For women, none of the coefficients are significant: There are no significant differences between the baseline year 2012 and the years before. There is also no significant difference between 2012 and the years following the relaxation. For men, however, we observe negative coefficients after the policy relaxation. The difference between the baseline year 2012 and the year 2018 is significant in all specifications. The year 2016 is significant in two out of the three specifications, and weakly significant in one. The year 2014 is significant in two out of three specifications. There is no significant difference between the baseline year 2012 and the previous years.

No effect for women suggests that there is no change in the overall number of marriages. The asymmetric effects between men and women indicate evidence for mechanism 2, that the policy relaxation decreases marriage rates of previously exempt men relative to non-exempt men. As such, the negative coefficient for men adds up the effect of both groups: marriage rates for previously exempt men go down, being replaced by previously not exempt men who experience an increase in marriage rate. It is noteworthy to stress that the general condition of Stable Unit Treatment Value Assumption (SUTVA) does not apply, making the coefficient difficult to interpret in quantitative terms.

Table 3: Effect of the Two-Child Policy on Marriages: Specification 2

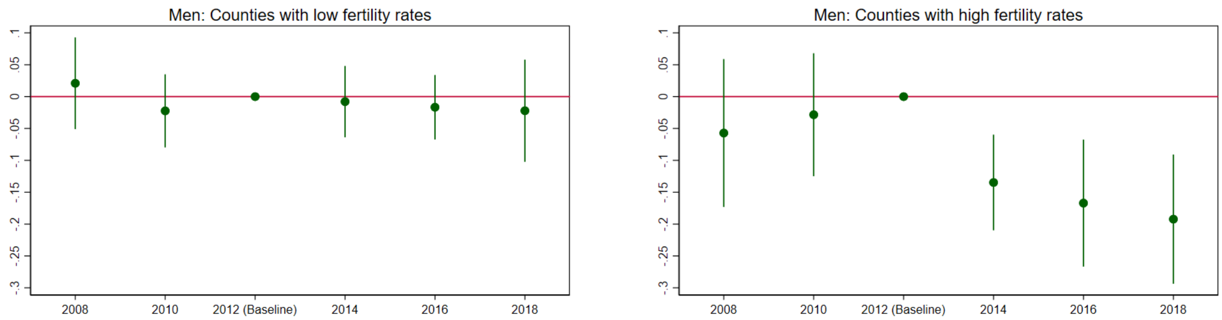
	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Pre:						
2-child eligible pre-TCP $\times$ 2008	-0.015	-0.012	-0.010	-0.009	-0.014	-0.016
	(0.555)	(0.634)	(0.689)	(0.790)	(0.682)	(0.612)
2-child eligible pre-TCP $\times$ 2010	-0.023	-0.018	-0.016	-0.033	-0.037	-0.038
	(0.299)	(0.424)	(0.461)	(0.157)	(0.117)	(0.107)
Post:						
2-child eligible pre-TCP $\times$ 2014	-0.026	-0.030	-0.024	-0.044*	-0.062**	-0.059**
	(0.330)	(0.227)	(0.370)	(0.100)	(0.012)	(0.015)
2-child eligible pre-TCP $\times$ 2016	-0.011	-0.009	0.003	-0.057*	-0.078**	-0.074**
	(0.748)	(0.798)	(0.943)	(0.077)	(0.016)	(0.015)
2-child eligible pre-TCP $\times$ 2018	-0.034	-0.020	-0.003	-0.075**	-0.095**	-0.086**
	(0.268)	(0.628)	(0.949)	(0.033)	(0.019)	(0.032)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	21259	19003	18195	20516	18173	17150

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

5.2 Heterogeneity

Fertility Preferences: We now turn to what factors are driving the results and if we find supporting evidence for mechanism 2. We only expect being allowed to have a second child to affect marriages if people actually want to have two children. As we do not have data on individual fertility preferences, we use the local fertility rates (calculated as the average number of children for couples aged 35 to 40 within the same county) as a proxy for local fertility preferences. We first split the sample into two groups with a median split to look separately at counties with a lower and a higher fertility rate and then pool the sample and include an interaction term. Figure 3 illustrates the effect for men (the corresponding figures for women can be found in the Appendix Figure A2). The results are striking: There is no effect on men's marriage rates in counties with low fertility rates, but there is a large negative effect on marriage rates in counties with high fertility rates.

Appendix Table A2 summarizes the results for both men and women for both specifications 1 and 2. For women, the "post"-coefficient is not significant either in high or low fertility counties (columns 1-4). For men, the coefficient in low fertility counties is close to zero and not significant (columns 7 and 8). The coefficient in high fertility counties is negative and significant (columns 9 and 10). The specification with annual indicators underlines the results. For women, neither of the coefficients is significant (one pre-coefficient is significant at 10%). For men, the coefficients for 2014, 2016 and 2018 are negative and significant only in high fertility counties. The coefficient size also increases over time, highlighting the role of 2014 as the intermediate year and the effect becoming more important over time. Again, none of the pre-coefficients is significantly different from our baseline year. Looking at the interaction term for men (columns 11 and 12), we find that the coefficient is significantly lower in high-fertility areas than in low-fertility areas.



(a) Effect of the TCP on marriages in low-fertility provinces

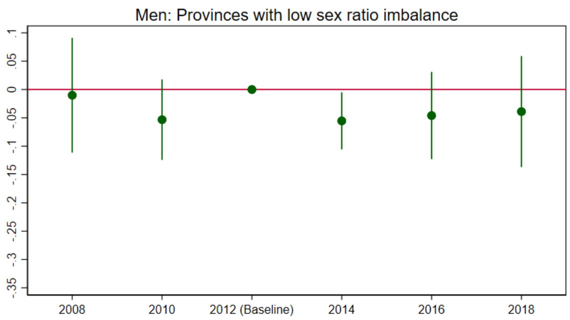
(b) Effect of the TCP on marriages in high-fertility provinces

Figure 3: Men: Effect of the Two-Child Policy on Marriages by Fertility Level

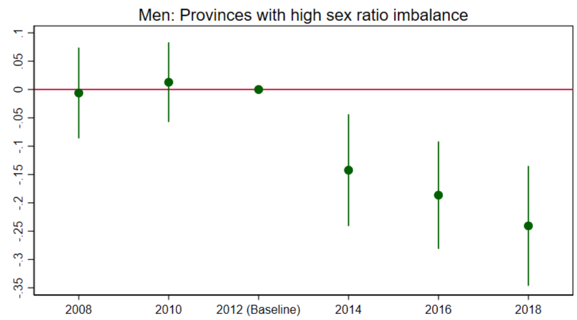
Note: See Table A2, Panel B, columns 8 and 10. Linear regression with 2012 as the baseline and marriage as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Sex ratio: Next, we also investigate the effect of the sex ratio. Only if there is a sufficiently strong sex imbalance, we expect an effect for the abundant sex (mechanism 2). The sex ratio is biased towards more men than women in all provinces in China. Yet, there are provinces with a stronger sex imbalance than others. We use the 1990 census data for the sex ratio at birth to split the sample into provinces with a high sex imbalance and a low sex imbalance. Again, we first run our regressions separately for both groups and then pool the sample and include an interaction term. The results are illustrated for men in Figure 4 (and for women in Appendix Figure A3). Again, the results are very visual: There is no consistent effect for men living in provinces with a rather balanced sex ratio. Yet, there is a strong effect for men living in provinces with a high sex ratio imbalance.

The regression results are displayed in Appendix Table A3. Again, we find no consistent effect for women (columns 1-4; with one significant coefficient in strongly imbalanced provinces). For men, the post-indicator is significant in provinces with a highly skewed sex ratio (columns 9 and 10), but not significant and close to zero in provinces with a more balanced sex ratio (columns 7 and 8). Looking at the annual indicators, all post coefficient are significant for provinces with a highly skewed sex ratio. Only the year of 2014 is significant in provinces with a more balanced sex ratio, while the years 2016 and 2018 are not, and the coefficients are much smaller, suggesting either a



(a) Effect of the TCP on marriages in provinces with low sex imbalance



(b) Effect of the TCP on marriages in provinces with high sex imbalance

Figure 4: Men: Effect of the Two-Child Policy on Marriages by Provincial Sex Ratio

Note: See Table A3, columns 3 and 5. Linear regression with 2012 as the baseline and marriage as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

weaker effect or no effect. Looking at the interaction confirms this. There might be a weak effect also in more balanced provinces, but the interaction term for the high sex ratio is negative and significant for the post indicator and negative, though not necessarily significant, for the annual post coefficients.

5.3 Robustness

Including respondents who participated in all waves between 2010 and 2018 ("balanced sample"): As the sampling of the CFPS changed between 2010 and 2012, we run our two specifications on the subsample of respondents who participated in all waves. As such, we exclude those where we recovered the information from the parents' questionnaire. The results are displayed in Appendix Table A4. The results remain robust to this sample restriction. All the coefficients for women are insignificant as before. The post-indicator stays negative and significant in all three specifications for men. For the annual indicators, the coefficients for the years 2016 and 2018 are significant for all specifications (though only at 10% for 2018 in two specifications). The comparison between 2010 and 2012, which is close to being significant in the overall sample, is now very close to zero.

Excluding ethnic minorities: As minorities have a specific status in China, and this might depend on which minority (and which province), they could have different marriage trends than the rest of the population. This trend might not be picked up by the provincial-time fixed effects. We thus run our regression excluding all minorities, keeping only those of the Han majority. The results, robust to this sample restriction, are displayed in Appendix Table A5. If anything, the results become clearer with this exclusion, suggesting that minorities are not the group that is driving the overall results. Appendix Table A6 illustrates the results with the balanced sample (including respondents who were interviewed between 2010 and 2018).

Excluding only children: As ethnic minorities, only children might have different marriage trends than their peers with siblings. For one, they might have different fertility preferences as they grew up being an only child, and thus they might not want to have multiple children (or any), decreasing their motivation to get married. In Appendix Table A7, we find that the results are robust to this sample restriction. Again, we find that coefficients are slighly larger for men. We also find that some coefficients are now (weakly) significant for women in some specifications. We do not want to over-interpret these potentially spurious results. Appendix Table A8 illustrates the results with the balanced sample (including respondents who were interviewed between 2010 and 2018). With the balanced sample, the coefficient for 2010, weakly different from the 2012 baseline when including the whole sample, is not significant and close to zero. The results suggest that only children are not driving the overall result. The results might rather be driven by the rural population that previously benefited from their "half-advantage" rule as they were often allowed to have a second child when the firstborn was a girl. Losing this advantage seems to have a strong impact on the marriage odds of rural men.

Age Thresholds: We also verify if our results hinge on our selection of the age thresholds 20/22 - 35. The lower threshold was chosen as these are the legal marriage limits. However, some get married before this age. The second was chosen to cover the main age range where people get married and have at least their first child.

We first relax the lower limit and include all individuals aged 18 or older. The results are displayed in Appendix Table A9. The results stay essentially the same, with the coefficient for men being significant in all specifications. Yet, some pre-coefficients are weakly significant in some specifications. However, when focusing on the balanced sample (Appendix Table A10), the significance of the pre-coefficients disappears. For women, on the other hand, some post-coefficients are weakly significant.

We then relax the upper limit and include men and women up to 40 years old in Appendix Table A11. Again, some coefficients are significant for women in this sample, yet the overall picture is too noisy to determine if there is also a weak effect among women. For men, the coefficients stay negative and strongly significant, with the pre-period coefficient in 2010 being significantly different from the 2012 baseline. Yet again, focusing on the balanced sub-sample in Appendix Table A12, the difference between 2010 and 2012 disappears.

Imputing odd years: Based on the information on in which year marriages took place, we can construct the cohorts for the missing odd years. When this information is missing, we assume marriage occurred in the corresponding even year. To fill the gaps for the other variables we use the value from the following even year. The results for the whole sample are displayed in Appendix Table A13 and for the balanced sample in Appendix Table A14. The results are also illustrated in Appendix Figure A4. The general results hold, with a significant effect for men and not for women. The imputed sample suggests that the effect comes in in 2016, the year after the TCP was officially announced, and not in 2015.

Extending the time frame with the 2020 and 2022 waves: We complete our dataset with the 2020 and 2022 waves. 2020 was conducted during Covid and had a much smaller sample. The 2022 wave equally contains fewer observations than the follow-up waves from 2012 to 2018, and just recently became available. Appendix Table A15 displays the summary statistics, adding the years 2020 and 2022. We observe a much lower number of observations (n=2118) in 2020, less than one-third of the baseline year of 2012 (n=6576). 2022 has more observations than 2020 (n=5403), but still roughly 20% less than 2012.

The main results hold and are displayed in Appendix Table A16. There is no significant effect on women. For men, the post-indicator is significant for two out of the three specifications. Looking at the annual indicators, the coefficients for 2020 and 2022 are negative but not significant. Yet, the coefficient size for 2022 is similar to those for 2014 and 2016, only with larger standard errors. Thus, because of the lower sample size for these two waves, we cannot make a conclusive statement about the marriage effect being short-lived or continuing up to 2022. This is also illustrated in Appendix Figure A5.

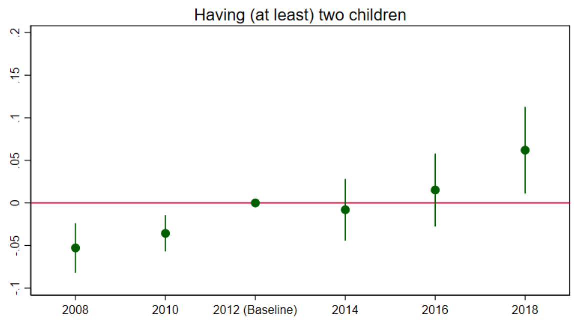
5.4 Effect of the Two-Child Policy on Fertility Outcomes

The same empirical strategy can be adapted to test whether the relaxation had an effect on fertility outcomes. Ideally, we would use expected or planned fertility before individuals get married. This captures best their intentions, which influence their search and selection of a marriage partner. Once married, planned fertility is endogenous to the marriage outcome and the marital bargaining process (Cleland et al., 2020). Later, when couples have their first child, their planned fertility is updated, taking into account the actual cost and effort of having a child (Kodzi et al., 2012; Preis et al., 2020). Unfortunately, only the 2022 CFPS wave collects data on planned fertility. Contrary to common perception based on current fertility rates, most men and women (75%) in their 20s want to have at least two children, with the average expected number of children being 1.88.

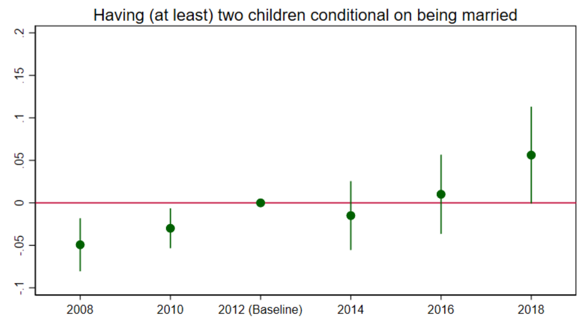
Further, we investigate the effect of the relaxation on actual fertility rates. Even if planned fertility might be revised with marriage and the first child, we expect at least some effect on fertility outcomes to explain our marriage results. For this, we use the same empirical specification with some modifications. First, we switch our "treatment indicator". While before it was an indicator for those allowed two children before the relaxation (*2-child eligible pre-TCP*), we now use an indicator for those who were allowed only one child (*1-child eligible pre-TCP*). This is only for exposition purposes: Those who were allowed to have only one child are "treated" by the relaxation process as they are now allowed to have a second child, while those already eligible should not experience any change. Second, we change the age threshold to account for the age span when people generally have a second child. We include women aged 23-40 and men aged 25-40. We also group men and women, and in another specification condition on them being married to not capture an effect through differential marriage rates.

Figure 5 Panel a and b illustrate the results, displayed in Appendix Table A17. While we see that those previously allowed only one child are more likely to have a second child compared to those who were already allowed to have a second child before the relaxation, we also observe significant pre-trends. The pre-trends can be explained by fewer and fewer couples who are allowed to have a second child actually having a second child.

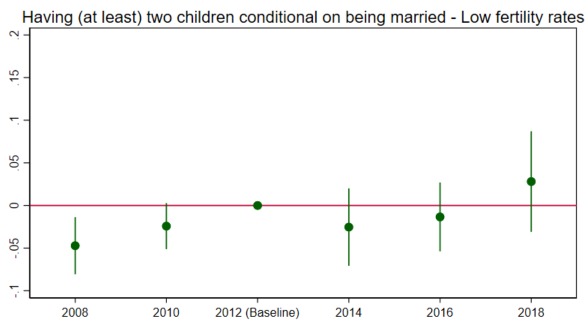
We again split the sample into counties with high and low fertility rates to proxy for local fertility preferences. We find that the pre-trends are driven by counties with low fertility rates



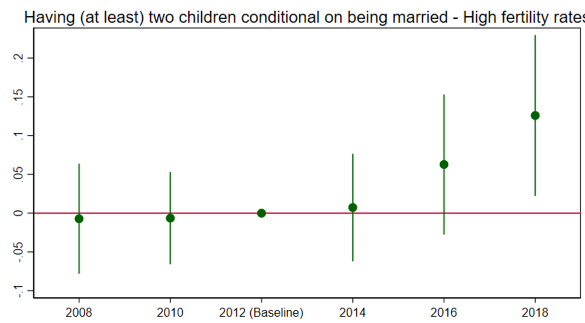
(a) Effect of the TCP on having (at least) two children



(b) Effect of the TCP on having (at least) two children - Married only



(c) Effect of the TCP on having (at least) two children in low fertility counties - Married only



(d) Effect of the TCP on having (at least) two children in high fertility counties - Married only

Figure 5: Effect of the Two-Child Policy on Fertility Outcomes

Note: See Table A17, Panel B, columns 2 and 5; Table A18, Panel B, columns 2 and 4. Linear regression with 2012 as the baseline and marriage as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

(Panel c). In these counties, we previously found no effect on male marriage rates, suggesting that being allowed to have a second child was not an advantage in the marriage market. The results on fertility outcomes underline this result that even those allowed to are less and less likely to have a second child, and there was no effect of the relaxation. However, in counties with high fertility rates (Panel d), where we previously found evidence for a "second child advantage in the marriage market", we do not find any significant pre-trends (with estimates being close to zero) and a significant increase in the likelihood of having a second child in 2018. The detailed results are displayed in Appendix Table A18.

6 Distortions on Who Marries Whom

The policy change might have affected not only who got married, but also who married whom. We do not observe an effect on marriage rates of women who were previously allowed to have a second child, yet their match quality can be affected. Education is often used to proxy spouse quality (for example, in Huang et al. (2023)). Do previously eligible women now lose out on marrying highly educated men? In this section, we address this question empirically. Yet, our approach has several constraints stemming mostly from data limitations. Ideally, it will be verified with the micro-level population census data upon availability.

6.1 Methodology

Analyzing matches on the marriage market level: Investigating individual-level match quality is restricted due to multiple reasons, leading us to investigate matches on the marriage market level. The first one comes from the dataset. In the follow-up waves, the survey only asks minimal questions about the new spouses. We have no information on their household status, ethnicity, or number of siblings. We have information on their age and educational level, though there is a lot of missing data.

The other issue arises due to the structure of spousal preferences and the fertility constraints. For one, education and group membership correlate: People in rural areas have lower educational attainments, while the Han majority and only children, on average, have higher educational levels. At the same time, group membership might itself be a positively or negatively valued characteristic in the marriage market. For example, having an urban hukou can be valuable, and the Han majority might display a dislike against ethnic minorities (Raiber et al., 2023; Hu, 2024). It is difficult to gauge the value of these characteristics with respect to education. For example, a rural woman eligible to have two children might have been able to marry a moderately educated urban man before the reform. After the reform, she might not be able to marry an urban husband, but will marry a rural husband who has a higher education.

Another issue is that spouse-dependent and spouse-independent criteria for a second child can have different effects. If the rural woman needs to marry a rural husband to have two children, this can restrict her choice, and she might marry a rural husband who is, nevertheless, well educated.

After the reform, she is now unconstrained and can marry an urban husband who is potentially less educated. However, if rural men are generally less educated, she might be able to find a more educated urban husband, while before she was constrained to the rural men. Yet, this constraint does not exist for women who fulfil a spouse-independent criterion, for example, some ethnic minorities.

Defining treated marriage markets: In all cases, differential fertility constraints can be seen as potentially distorting marriage matches that otherwise would be dependent on education. Yet, the distortion is non-linear. Few eligible people presumably means little distortion. If a significant share of the population within one marriage market is eligible for a second child, they are either highly valued partners (if their eligibility is not spouse dependent) or constrained in their partner choice (if their eligibility is spouse independent) – and thus potentially distort matches. However, if nearly everyone within a marriage market is eligible, eligibility is neither an advantage nor a real constraint, and we expect little distortion in matches.

We geographically define a marriage market as one province, as there are not enough individuals in the dataset to evaluate it at a finer-grained level. We evaluate marriages within one cohort, which preferably encompasses a period of two years. Yet, as the follow-up waves have less and less information on spouses, and we can only use couples where we have complete information on both marriage partners, we need to group the post-years from 2016 to 2020 within one cohort. For the pre-cohorts, we have two of them: Those who married in 2009 and 2010, and those who married in 2011 and 2012. We do not include those who married between 2013 and 2015. Figure 6 illustrates the share of individuals fulfilling any eligibility criteria based on the policy pre-2015 within one province-cohort. We observe a high level of heterogeneity, with some province-cohorts having few eligible and some with a share over 70%. We use a quartile split to define the treated marriage markets:[12] Those in the lowest quartile and highest quartile are expected to have little distortion on marriages, and those in between the highest distortion. Those in the middle, with a "medium eligibility level", are thus defined as our treated marriage markets.

Measuring distortions in matching along education: We assume that in the absence of any policy-related distortions, marriages would be along educational levels. This assumption relies

[12] We use a quartile split to avoid arbitrarily fixing the thresholds and to have the same number of observations in our treatment and control group.

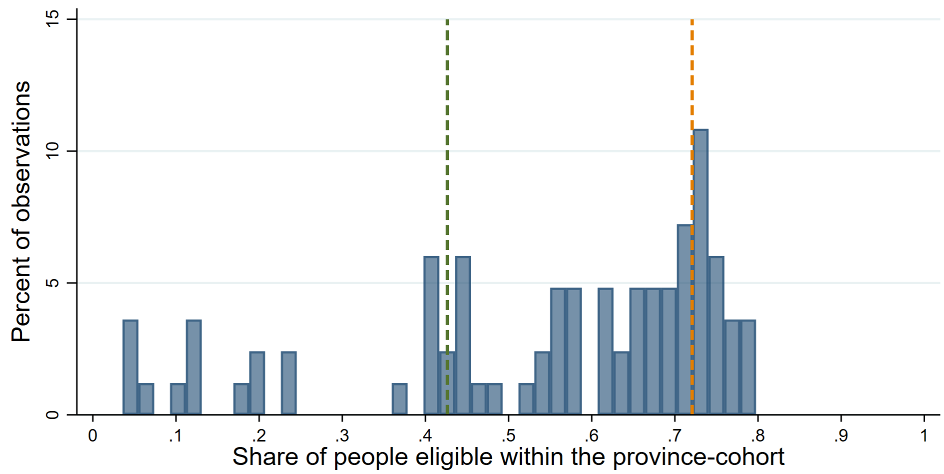


Figure 6: Distribution of the share eligible for a second child within the province-cohort. Dashed lines indicate the 25th percentile (green) and the 75th percentile (orange). The 50 percent between the lines is defined as the treated province-cohorts.

on recent literature measuring individual preferences showing that education is valued on both sides, with women in particular displaying a strong preference for marrying a man with a high educational level (Raiber et al., 2023; Yu et al., 2025). This differs from homogamic preferences for a spouse with the same educational level. Yet, increasing preferences for spousal education and homogamic preferences both lead to positive assortative matching, i.e. spouses having the same level of education, when the educational distributions of men and women are similar.

To evaluate marriage distortions, we first look at the correlation between spouses' educational levels for each province-cohort. Similar correlation methods are often used to measure matching by educational levels (for example, Fernández and Rogerson, 2001; Greenwood et al., 2003,0; Dong and Xie, 2023).[13] Second, we compare actual matches within a province-cohort to how matches would look if they were based on increasing preferences for education. For each province-cohort, we rank men and women according to their educational level and then calculate matches with increasing preferences: the two individuals with the highest educational level get married, those with the second-highest educational level get married, and so forth. For each province-cohort, this gives us a perfect increasing-preferences matching matrix that we can compare to the actual matching matrix.

[13]However, correlation is sensitive to shifts in the marginal distributions of education and therefore does not cleanly isolate positive assortative matching when educational distributions differ across men and women or change over time (Mare, 1991; Torche, 2010). Still, when the marginal distributions remain relatively stable, correlation provides a reasonable measure of changes in matching along educational levels.

We measure the deviation from perfect increasing-preferences matching as the difference between the two matrices, normalised by the number of couples. This gives us a distortion score between 0 and 1, with 0 implying that the actual marriages are the same as perfect increasing-preferences marriages and 1 implying that all actual marriages deviate from perfect increasing-preferences ones.

This measure relates to the minimum distance measure that uses perfect assortative matches as one benchmark (see review in Chiappori et al. (2022), used by Fernández and Rogerson (2001) and Abbott et al. (2019)).[14] As a comparison, we also calculate the deviation from perfect positive assortative matches. We find that these measures are highly correlated with a correlation coefficient of 0.8.

For some smaller provinces, we have very few couples in our dataset. This problem is exacerbated in the follow-up waves and especially in the post-cohort, even when grouping several years. We thus exclude province-cohorts with few couples. We face the trade-off that setting a low threshold implies more province-cohorts, but with an imprecise deviation score measure, and a high threshold implies more precise deviation score measures but fewer province-cohort observations. For our main specification, we set the threshold at more than 12 couples and verify the results with different thresholds (between more than 10 and 18). We find that the average deviation score is 0.35 with little variation depending on the threshold.

Empirical specification: We run the following specifications:

$$deviation\_score_{pc} = \alpha \; medium\_elibigility\_level_{pc} \times \; post_c + \beta X_{pc} + \mu_p + \epsilon_{ipc} \tag{3}$$

where $deviation\_score_{pt}$ is the deviation score in province p for cohort c, $medium\_elibigility\_level_{pc}$ is an indicator for those with a medium share of people being eligible for a second child based on the policy pre-2015 in province p in cohort c, X_{pc} are time-varying province controls, and μ_p province fixed effects. α, the coefficient of the interaction term, is our coefficient of interest, and the regression includes baseline levels for $medium\_elibigility\_level_{pc}$ and the $post_c$ indicator. Standard errors are clustered on the province level.

With X_{pc}, we control for the gender ratio (overall share of women within the cohort). We also use the share of the different socio-economic groups (Han, rural household, and only children). As

[14] They generally use random matching as another benchmark. We refrain from this as we are not interested in the degree of assortativeness compared to random matches, but only in the deviation from the "undistorted outcome".

mentioned before, they can be valuable characteristics in the marriage market independent from the policy-related constraints and thus by themselves distort matching on education.

6.2 Results

Table 4 displays the results. We can see in Panel A that the correlation between spouses' educational levels increases in treated provinces - those with a medium share of population being eligible for a second child - after the relaxation. The coefficients are weakly significant at 10%. The relaxation has no effect on non-treated province. In columns (3) and (4), we check for a pre-trend by comparing the two pre-periods, but find no effect. In Panel B, we find the same for the deviation score. The deviation from perfect increasing-preferences matching decreases after the relaxation for treated provinces only. Again, coefficients are significant at 10% in most of the specifications and significant at 5% in the last. The difference between the two pre-periods is again insignificant, with coefficients close to 0. Appedix Table A19 illustrates the results for the deviation from perfect assortative matching. Coefficients are similar but only significant in one specification.

Appendix Table A20 shows the results for different thresholds on how many couples need to be in a province-cohort for it to be included. This has an effect on how many post-cohorts are in the regression. The coefficients qualitatively stay the same, with most of the estimates being significant on at least 10%.

The results suggest that policy-related fertility distortions not only distorted who got married but also who married whom. Without the fertility constraints, matches are expected to become more associated with education. However, this analysis, due to the data constraints, remains highly suggestive.

Table 4: Effect of the Two-Child Policy on Marriages Matching by Education

	(1)	(2)	(3)	(4)
Panel A:	**Dependent variable: Correlation coefficient**			
Reference group:	*Married between 2009 and 2012*		*Married in 2011 or 2012*	
Middle quartiles of previously eligible	0.221*	0.253*	0.216*	0.243*
× married post 2015	(0.066)	(0.051)	(0.072)	(0.093)
Married post 2015	-0.056	-0.016	-0.075	0.020
	(0.497)	(0.852)	(0.395)	(0.854)
Middle quantiles of previously eligible			-0.010	-0.061
× married 2009 or 2010			(0.926)	(0.606)
Panel B:	**Dependent variable: Deviation Score**			
Reference group:	*Married between 2009 and 2012*		*Married in 2011 or 2012*	
Middle quartiles of previously eligible ×	-0.101*	-0.156*	-0.111*	-0.174**
× married post 2015	(0.058)	(0.063)	(0.081)	(0.033)
Married post 2015	-0.024	-0.003	-0.013	0.016
	(0.563)	(0.967)	(0.787)	(0.813)
Middle quantiles of previously eligible			-0.020	0.009
× married 2009 or 2010			(0.822)	(0.914)
Province FE	yes	yes	yes	yes
Controls	no	yes	no	yes
Observations	62	62	62	62
Post Observations	16	16	16	16

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Linear regression where each observation is a province-cohort with more than 12 couples having married in that time frame and with the respondent being between 20 (women)/22 (men) and 35 years old. The dependent variable is the correlation coefficient (Panel A) and the deviation score measuring deviation for perfect increasing-preferences matching (Panel B). Includes two pre-cohorts (couples married in 2009 or 2010 and married in 2011 or 2012) and one post-cohort (couples married between 2016 and 2020). "Married post 2015" is an indicator for the post-cohort. "Middle quartiles of previously eligible" is an indicator for province-cohorts where the population share of those eligible for a second child based on the policy before 2015 is between the 25th and the 75th quartile. Controls include the population share of individuals being Han, having a rural hukou and being an only child within the province-cohort. Standard errors are clustered on the province level. Data from the CFPS 2010-2022.

7 Conclusion

In this paper, we investigate the effect of the end of the OCP on the marriage outcomes for individuals of childbearing age. To do so, we use heterogeneity in the implementation of the OCP and its effect on marriages using a difference-in-difference framework. Results suggest that the relaxation of the OCP led to a decrease in marriages for men who were previously exempt from the one-child constraint and eligible for a second child, compared to non-eligible men. The heterogeneity analysis suggests that in areas where there is demand for a second child and a male-biased sex ratio, being allowed a second child before the policy relaxation was an advantage for men in the marriage market and increased their marriage odds. Losing this advantage decreases their marriage chances and increases those previously without this advantage.

Exemptions from the one-child limit were mostly given to disadvantaged groups; here, we focus on rural households and ethnic minorities. Unmarried rates among rural men were already on the rise before the relaxation (Jiang et al., 2014; Eklund et al., 2017), and losing their second-child relative advantage is expected to disadvantage them further. The policy relaxation, therefore, was not beneficial for everyone and, on the contrary, reinforces preexisting marriage gaps and differences in socio-economic outcomes.

We find no effect on the marriage rate of women, suggesting less competition among women for a spouse. Yet, we cannot make a statement about competition among women for a high-quality spouse, which has been which is discussed in recent literature (Wei and Zhang, 2019; Ong et al., 2020). Our results suggest that women in areas with high fertility rates and a male-biased sex ratio, on the margin, selected a husband with whom they could have a second child. This is direct evidence that reproductive capacity matters in spousal choice and highlights how a biased sex ratio can be favourable for women.

Furthermore, we find a modest effect of the relaxation of the OCP on fertility outcomes. While the expected number of children is still at least two for most people in their 20s, fertility rates in China have plummeted. Our results suggest that in areas with low fertility rates, fewer and fewer people had a second child before the relaxation, even if they were allowed to. Thus, the two-child policy and the following relaxations are expected to have little bite. In areas with high fertility rates, we observe an increase in fertility outcomes with the relaxation. However, fertility rates are presumably high because many were already allowed to have a second child, and thus, there are few in these areas that actually now gain eligibility - again suggesting a small overall fertility effect of the relaxation in line with other studies (Wang et al., 2017; Zhang and Zheng, 2021; Wu, 2022).

Finally, we investigate marriage outcomes as a function of how widespread exemptions were before the relaxation. We find suggestive evidence that the differential fertility constraints distorted marriage by education. This suggests that education will become more important after the relaxation. Marriage by education, either the most highly educated men and women marrying or men and women with the same education marrying, is associated with increases in household income inequality. This has already been on the rise (Song et al., 2017), and, based on our results, the policy relaxation is predicted to increase it even further.

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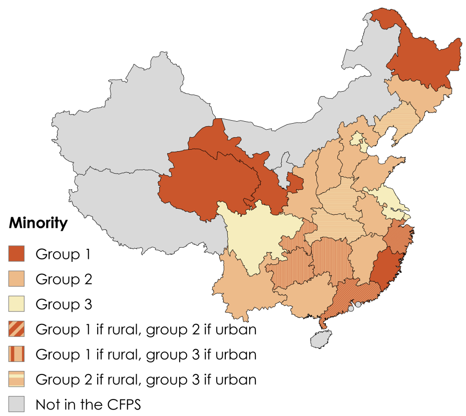
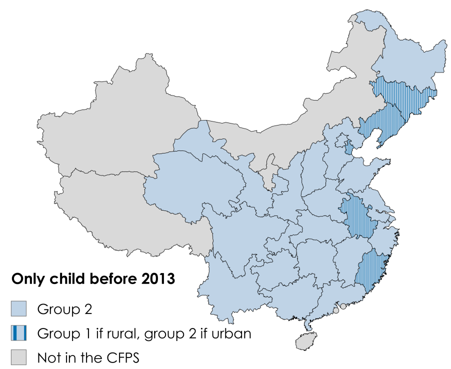
A Additional Figures and Tables

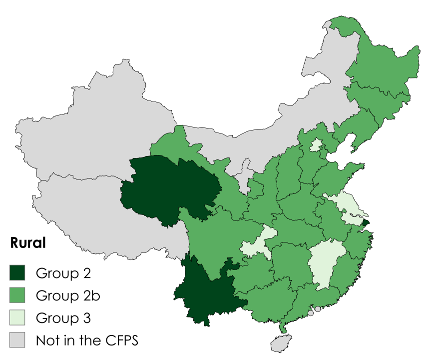
A.1 Additional Figures



(a) Ethnicity-related exemptions

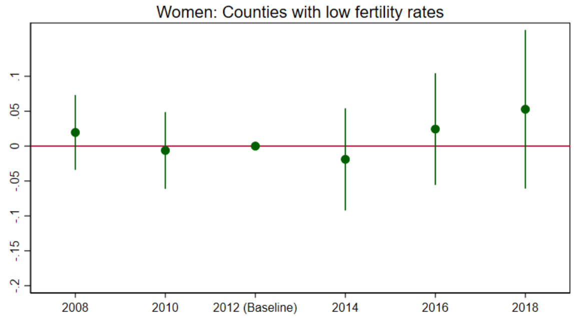


(b) Only-child-related exemptions

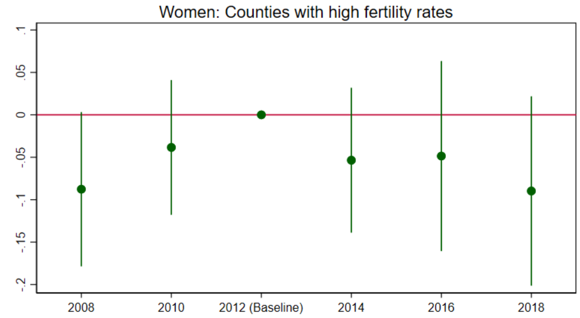


(c) Rural-household-related exemptions

Figure A1: Distribution of different exemption groups



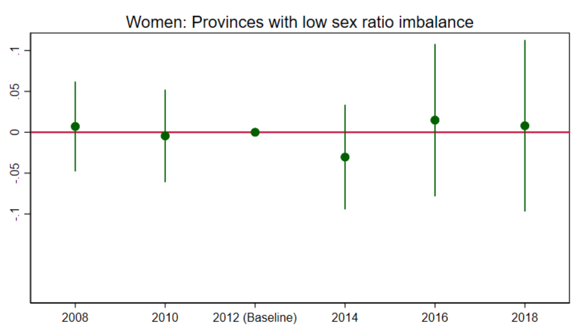
(a) Effect of the TCP on marriages in low-fertility provinces



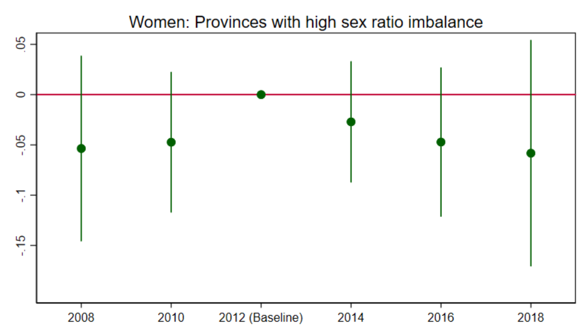
(b) Effect of the TCP on marriages in high-fertility provinces

Figure A2: Women: Effect of the Two-Child Policy on Marriages by Fertility Level

Note: See Table A2, Panel B, columns 7 and 9. Linear regression with 2012 as the baseline and marriage as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.



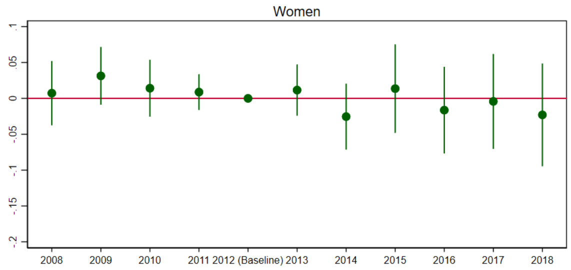
(a) Effect of the TCP on marriages in provinces with low sex ratio imbalance



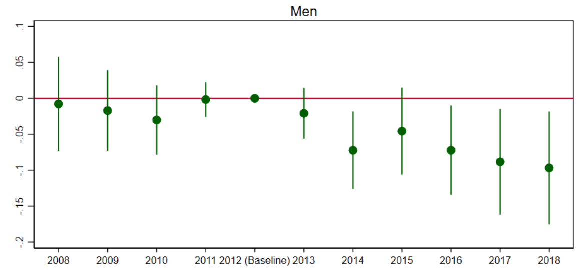
(b) Effect of the TCP on marriages in provinces with high sex ratio imbalance

Figure A3: Women: Effect of the Two-Child Policy on Marriages by Provincial Sex Ratio

Note: See Table A3, Panel B, columns 7 and 9. Linear regression with 2012 as the baseline and marriage as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

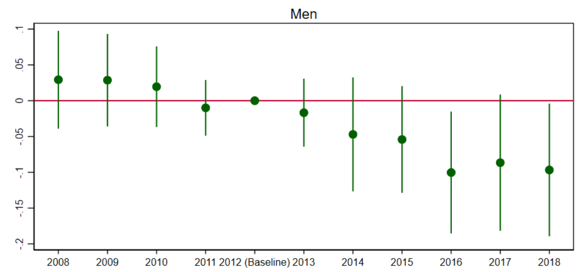


(a) Effect of the TCP on marriages for women



(b) Effect of the TCP on marriages for men

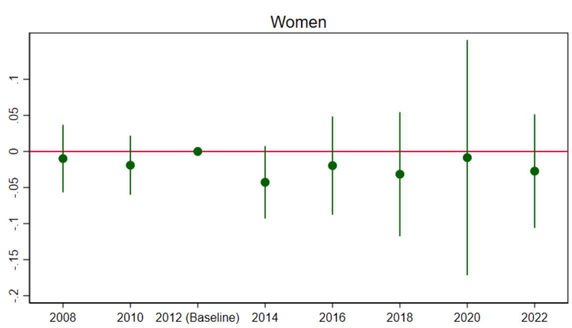
(c) Effect of the TCP on marriages for women - Balanced sample



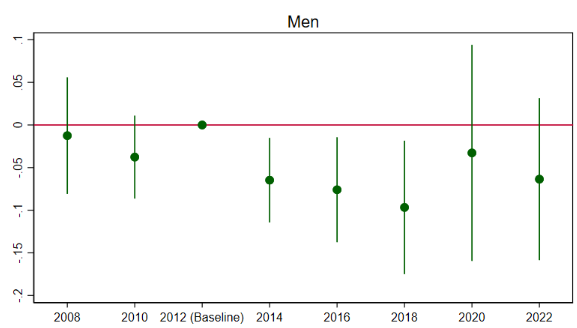
(d) Effect of the TCP on marriages for men - Balanced sample

Figure A4: Effect of the Two-Child Policy on Marriages by Gender

Note: See Table A13 and A14, Panel B, columns 2 and 5. Linear regression with 2012 as the baseline and marriage as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.



(a) Effect of the TCP on marriages for women



(b) Effect of the TCP on marriages for men

Figure A5: Effect of the Two-Child Policy on Marriages: Including the 2020 and 2022 waves

Note: See Table A16, Panel B, columns 2 and 5. Linear regression with 2012 as the baseline and marriage as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators, county fixed effects, and individual characteristics (female, age, Han, rural residence status, only child, education). Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2022.

A.2 Additional Tables

Table A1: Provincial eligibility criteria for a second-child permit in 2013: Exemption categories used for the analysis

Province	**Only Child**	**Ethnic Minority**	**Rural Population**
Beijing	Both spouses	/	/
Tianjin	Both spouses; one spouse for agricultural population	/	/
Hebei	Both spouses	both spouses for ethnicity less than 10 million	First-born a girl
Shanxi	Both spouses	Both spouses	First-born a girl
Liaoning	Both spouses; one spouse for the agricultural population	One spouse living in a rural/minority area	First-born a girl
Jilin	Both spouses; one spouse for the agricultural population	Both spouses for ethnicity less than 10 million	First-born a girl
Heilongjiang	Both spouses	Both spouses for ethnicity less than 10 million	First-born a girl
Shanghai	Both spouses; one spouse for agricultural population	/	/
Jiangsu	Both spouses; one spouse for the agricultural population	/	/
Zhejiang	Both spouses	Both spouses; one spouse living in rural/minority area	First-born a girl
Anhui	Both spouses; one spouse for agricultural population	Both spouses	First-born a girl
Fujian	Both spouses; one spouse for the agricultural population	Both spouses; one spouse living in a rural/minority area, except Zhuang	First-born a girl
Jiangxi	Both spouses	Both spouses	/
Shandong	Both spouses	Both spouses	First-born a girl (woman needs rural *hukou*)
Henan	Both spouses	Both spouses living in rural/minority area	First-born a girl
Hubei	Both spouses	Both spouses living in rural/minority area	First-born a girl
Hunan	Both spouses	One spouse living in rural/minority area	First-born a girl
Guangdong	Both spouses	Both spouses; one spouse living in rural/minority area	First-born a girl
Guangxi	Both spouses	Both spouses for ethnicity less than 10 million	First-born a girl (woman needs rural *hukou*)
Chongqing	Both spouses	One spouse living in rural/minority area	/
Sichuan	Both spouses	/	/
Guizhou	Both spouses	One spouse living in rural/minority area	First-born a girl
Yunnan	Both spouses	/ (Ethnicity-specific for third child)	All rural
Shaanxi	Both spouses	Both spouses for ethnicity less than 10 million	First-born a girl

Gansu	Both spouses	One spouse living in rural/minority area	First-born a girl
Qinghai	Both spouses	One spouse	All rural

Note: "/" indicates no exemption for this group. Does not include exemptions that are based on experiencing "real difficulties" (except for the exemption for all rural in Yunnan, where the literature agrees that all couples are eligible) or living in a specific area (except living in rural or minority area). Based on official documentation of the relevant Family Planning Regulations. Documents for each province, including the link to the data source and the Google translation, are available upon request. The table only includes the eligibility criteria for these three groups - there are other groups which are not included here. Inner Mongolia, Hainan, Tibet, Ningxia and Xinjiang are not included as they are not covered by the CFPS 2010.

Table A2: Effect of the Two-Child Policy on Marriages by Fertility Level

	Women						Men					
	Fertility Low		Fertility High		All		Fertility Low		Fertility High		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2-child eligible pre-TCP × post 2015	-0.002	0.023	-0.020	-0.025	0.005	0.030	-0.006	-0.026	-0.141**	-0.155***	-0.006	-0.038
	(0.960)	(0.593)	(0.626)	(0.559)	(0.868)	(0.453)	(0.782)	(0.245)	(0.011)	(0.005)	(0.796)	(0.133)
2-child eligible pre-TCP × post 2015 x High Fertility Rate					-0.042	-0.062					-0.098*	-0.072
					(0.386)	(0.270)					(0.067)	(0.127)
Province x Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
County FE	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	9205	7421	8099	8098	17304	15519	8301	6587	7753	7749	16056	14338
Pre:												
2-child eligible pre-TCP × 2008	0.014	0.020	-0.089*	-0.088*	0.015	0.016	0.028	0.021	-0.052	-0.057	0.023	0.017
	(0.599)	(0.469)	(0.059)	(0.059)	(0.579)	(0.541)	(0.405)	(0.564)	(0.352)	(0.328)	(0.495)	(0.635)
2-child eligible pre-TCP × 2010	-0.013	-0.006	-0.037	-0.038	-0.009	-0.005	-0.019	-0.022	-0.023	-0.028	-0.015	-0.019
	(0.621)	(0.821)	(0.362)	(0.336)	(0.724)	(0.849)	(0.484)	(0.440)	(0.608)	(0.557)	(0.593)	(0.511)
Post:												
2-child eligible pre-TCP × 2014	-0.028	-0.019	-0.049	-0.053	-0.004	-0.010	0.003	-0.008	-0.122***	-0.135***	0.008	-0.026
	(0.395)	(0.606)	(0.266)	(0.215)	(0.893)	(0.761)	(0.932)	(0.780)	(0.007)	(0.001)	(0.793)	(0.380)
2-child eligible pre-TCP × 2016	0.011	0.024	-0.044	-0.049	0.014	0.027	-0.003	-0.017	-0.143***	-0.167***	-0.005	-0.034
	(0.776)	(0.545)	(0.448)	(0.389)	(0.704)	(0.449)	(0.900)	(0.513)	(0.008)	(0.001)	(0.859)	(0.251)
2-child eligible pre-TCP × 2018	-0.006	0.053	-0.088	-0.090	0.003	0.049	-0.002	-0.022	-0.181***	-0.192***	0.001	-0.041
	(0.882)	(0.357)	(0.152)	(0.113)	(0.933)	(0.353)	(0.948)	(0.583)	(0.001)	(0.000)	(0.983)	(0.332)
Pre:												
2-child eligible pre-TCP × 2008 × High Fertility Rate					-0.098**	-0.095*					-0.083	-0.086
					(0.049)	(0.054)					(0.194)	(0.197)
2-child eligible pre-TCP × 2010 × High Fertility Rate					-0.036	-0.036					-0.019	-0.024
					(0.411)	(0.410)					(0.707)	(0.646)
Post:												
2-child eligible pre-TCP × 2014 × High Fertility Rate					-0.054	-0.047					-0.115**	-0.073*
					(0.248)	(0.324)					(0.016)	(0.096)
2-child eligible pre-TCP × 2016 × High Fertility Rate					-0.067	-0.077					-0.110**	-0.099**
					(0.187)	(0.160)					(0.033)	(0.040)
2-child eligible pre-TCP × 2018 × High Fertility Rate					-0.114*	-0.146**					-0.151***	-0.107**
					(0.073)	(0.046)					(0.004)	(0.043)
Province x Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
County FE	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	10617	8667	9528	9525	20147	18195	9858	7816	9333	9331	19193	17150

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, county fixed effects, group indicators and individual characteristics (female, age, Han, rural residence status, only child, education). Columns 2, 4, 6, 8, and 10 include county fixed effects. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A3: Effect of the Two-Child Policy on Marriages by Provincial Sex Ratio

	Women						Men					
	Sex imbalance Low		Sex imbalance High		All		Sex imbalance Low		Sex imbalance High		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2-child eligible pre-TCP × post 2015	0.014	0.013	-0.064**	-0.027	0.007	0.010	-0.010	-0.028	-0.171***	-0.211***	-0.033	-0.046*
	(0.687)	(0.735)	(0.048)	(0.432)	(0.848)	(0.813)	(0.682)	(0.310)	(0.000)	(0.000)	(0.154)	(0.082)
2-child eligible pre-TCP × post 2015 x High Sex Ratio					-0.054	-0.032					-0.096***	-0.108***
					(0.275)	(0.578)					(0.007)	(0.008)
Province x Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
County FE	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Observations	9489	8508	8644	7587	18133	16096	8957	8057	8056	6978	17013	15037
Pre:												
2-child eligible pre-TCP × 2008	0.003	0.003	-0.054	-0.054	0.005	0.008	-0.010	-0.010	0.008	-0.006	-0.015	-0.012
	(0.919)	(0.919)	(0.246)	(0.246)	(0.866)	(0.762)	(0.838)	(0.842)	(0.842)	(0.878)	(0.774)	(0.818)
2-child eligible pre-TCP × 2010	-0.012	-0.012	-0.047	-0.047	-0.012	-0.003	-0.052	-0.053	0.020	0.013	-0.058	-0.056
	(0.662)	(0.662)	(0.178)	(0.178)	(0.679)	(0.910)	(0.141)	(0.139)	(0.557)	(0.711)	(0.109)	(0.127)
Post:												
2-child eligible pre-TCP × 2014	-0.026	-0.026	-0.027	-0.027	-0.029	-0.034	-0.033	-0.055**	-0.094**	-0.142***	-0.059**	-0.073***
	(0.442)	(0.442)	(0.368)	(0.368)	(0.374)	(0.258)	(0.244)	(0.032)	(0.034)	(0.006)	(0.034)	(0.004)
2-child eligible pre-TCP × 2016	0.021	0.021	-0.047	-0.047	0.015	0.012	-0.026	-0.046	-0.138***	-0.186***	-0.053	-0.071*
	(0.642)	(0.642)	(0.205)	(0.205)	(0.726)	(0.804)	(0.521)	(0.236)	(0.003)	(0.000)	(0.158)	(0.061)
2-child eligible pre-TCP × 2018	0.005	0.005	-0.058	-0.058	-0.002	0.004	-0.029	-0.039	-0.177***	-0.241***	-0.055	-0.060
	(0.902)	(0.902)	(0.301)	(0.301)	(0.959)	(0.944)	(0.472)	(0.428)	(0.001)	(0.000)	(0.178)	(0.214)
Pre:												
2-child eligible pre-TCP × 2008 × High Sex Ratio					-0.056	-0.060					0.040	0.019
					(0.292)	(0.259)					(0.531)	(0.767)
2-child eligible pre-TCP × 2010 × High Sex Ratio					-0.028	-0.040					0.096*	0.081
					(0.505)	(0.351)					(0.053)	(0.106)
Post:												
2-child eligible pre-TCP × 2014 × High Sex Ratio					0.001	0.003					0.016	0.003
					(0.976)	(0.929)					(0.668)	(0.919)
2-child eligible pre-TCP × 2016 × High Sex Ratio					-0.072	-0.057					-0.032	-0.041
					(0.247)	(0.336)					(0.501)	(0.402)
2-child eligible pre-TCP × 2018 × High Sex Ratio					-0.083	-0.064					-0.068	-0.106*
					(0.172)	(0.443)					(0.175)	(0.062)
Province x Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
County FE	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Observations	11087	11087	9007	9007	21259	19003	10778	9656	9738	8455	20516	18173

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, county fixed effects, group indicators and individual characteristics (female, age, Han, rural residence status, only child, education). Columns 2, 4, 6, 8, and 10 include county fixed effects. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018. 2010-2018.

Table A4: Effect of the Two-Child Policy on Marriages: Balanced Sample from 2010 to 2018

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	0.011	0.031	0.031	-0.095**	-0.097**	-0.095**
	(0.726)	(0.391)	(0.393)	(0.016)	(0.017)	(0.016)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	12820	11563	11285	9779	8878	8598
Pre:						
2-child eligible pre-TCP × 2008	-0.005	0.001	0.003	0.011	0.005	0.003
	(0.838)	(0.964)	(0.902)	(0.760)	(0.881)	(0.921)
2-child eligible pre-TCP × 2010	-0.024	-0.018	-0.017	0.005	-0.003	-0.002
	(0.308)	(0.442)	(0.456)	(0.857)	(0.925)	(0.929)
Post:						
2-child eligible pre-TCP × 2014	-0.023	-0.011	-0.008	-0.028	-0.029	-0.031
	(0.429)	(0.712)	(0.794)	(0.462)	(0.466)	(0.377)
2-child eligible pre-TCP × 2016	0.010	0.020	0.024	-0.086*	-0.099**	-0.094**
	(0.778)	(0.579)	(0.504)	(0.062)	(0.033)	(0.031)
2-child eligible pre-TCP × 2018	-0.009	0.009	0.014	-0.091**	-0.095*	-0.095*
	(0.790)	(0.839)	(0.752)	(0.046)	(0.073)	(0.068)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	14846	13501	13119	11400	10396	10020

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, Han, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A5: Effect of the Two-Child Policy on Marriages: Excluding ethnic minorities

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.023	-0.004	0.008	-0.083***	-0.092***	-0.089***
	(0.405)	(0.911)	(0.794)	(0.005)	(0.003)	(0.002)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	16510	14657	14147	15566	13759	13101
Pre:						
2-child eligible pre-TCP × 2008	-0.005	-0.002	0.000	-0.017	-0.021	-0.023
	(0.867)	(0.932)	(0.995)	(0.638)	(0.583)	(0.516)
2-child eligible pre-TCP × 2010	-0.019	-0.014	-0.012	-0.036	-0.039	-0.039
	(0.421)	(0.569)	(0.623)	(0.159)	(0.133)	(0.124)
Post:						
2-child eligible pre-TCP × 2014	-0.023	-0.016	-0.004	-0.086***	-0.087***	-0.080***
	(0.428)	(0.559)	(0.879)	(0.005)	(0.003)	(0.005)
2-child eligible pre-TCP × 2016	-0.017	-0.009	0.007	-0.096***	-0.107***	-0.102***
	(0.660)	(0.820)	(0.856)	(0.006)	(0.003)	(0.002)
2-child eligible pre-TCP × 2018	-0.026	0.001	0.023	-0.107***	-0.119**	-0.114**
	(0.435)	(0.973)	(0.607)	(0.009)	(0.010)	(0.011)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	19374	17326	16607	18779	16642	15681

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A6: Effect of the Two-Child Policy on Marriages: Excluding ethnic minorities - Balanced Sample from 2010 to 2018

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.012	0.022	0.025	-0.130***	-0.121**	-0.118**
	(0.739)	(0.590)	(0.547)	(0.006)	(0.014)	(0.011)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	11673	10543	10299	8963	8130	7862
Pre:						
2-child eligible pre-TCP × 2008	0.008	0.015	0.018	0.004	-0.002	-0.004
	(0.774)	(0.627)	(0.566)	(0.916)	(0.969)	(0.924)
2-child eligible pre-TCP × 2010	-0.027	-0.020	-0.019	0.009	0.000	0.001
	(0.280)	(0.423)	(0.436)	(0.774)	(0.994)	(0.987)
Post:						
2-child eligible pre-TCP × 2014	-0.032	-0.005	-0.002	-0.064	-0.050	-0.053
	(0.346)	(0.873)	(0.952)	(0.171)	(0.295)	(0.211)
2-child eligible pre-TCP × 2016	-0.007	0.017	0.023	-0.129**	-0.126**	-0.121**
	(0.855)	(0.667)	(0.565)	(0.019)	(0.026)	(0.023)
2-child eligible pre-TCP × 2018	-0.013	0.018	0.025	-0.127**	-0.119*	-0.121**
	(0.740)	(0.716)	(0.604)	(0.019)	(0.055)	(0.043)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	13532	12324	11986	10456	9535	9173

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A7: Effect of the Two-Child Policy on Marriages: Excluding only children

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.041	-0.037	-0.028	-0.089***	-0.111***	-0.093**
	(0.136)	(0.164)	(0.275)	(0.002)	(0.004)	(0.014)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Observations	no	no	yes	no	no	yes
N	15327	13557	13094	13069	11484	11010
Pre:						
2-child eligible pre-TCP × 2008	-0.023	-0.022	-0.018	-0.046	-0.046	-0.045
	(0.443)	(0.477)	(0.559)	(0.138)	(0.162)	(0.168)
2-child eligible pre-TCP × 2010	-0.022	-0.024	-0.020	-0.057*	-0.060*	-0.059*
	(0.453)	(0.408)	(0.486)	(0.078)	(0.063)	(0.068)
Post:						
2-child eligible pre-TCP × 2014	-0.009	-0.015	-0.001	-0.052*	-0.078***	-0.076***
	(0.740)	(0.509)	(0.961)	(0.055)	(0.006)	(0.007)
2-child eligible pre-TCP × 2016	-0.038	-0.052	-0.040	-0.116***	-0.131***	-0.111***
	(0.296)	(0.142)	(0.249)	(0.006)	(0.004)	(0.008)
2-child eligible pre-TCP × 2018	-0.074**	-0.054	-0.039	-0.127***	-0.170***	-0.142***
	(0.022)	(0.148)	(0.329)	(0.000)	(0.000)	(0.002)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	17924	15960	15297	15755	13831	13117

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A8: Effect of the Two-Child Policy on Marriages: Excluding only children - Balanced Sample from 2010 to 2018

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.025	-0.012	-0.018	-0.134**	-0.156***	-0.145***
	(0.431)	(0.712)	(0.597)	(0.012)	(0.004)	(0.007)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	10980	9912	9695	7525	6823	6653
Pre:						
2-child eligible pre-TCP × 2008	-0.023	-0.022	-0.018	-0.046	-0.046	-0.045
	(0.443)	(0.477)	(0.559)	(0.138)	(0.162)	(0.168)
2-child eligible pre-TCP × 2010	-0.022	-0.024	-0.020	-0.057*	-0.060*	-0.059*
	(0.453)	(0.408)	(0.486)	(0.078)	(0.063)	(0.068)
Post:						
2-child eligible pre-TCP × 2014	-0.009	-0.015	-0.001	-0.052*	-0.078***	-0.076***
	(0.740)	(0.509)	(0.961)	(0.055)	(0.006)	(0.007)
2-child eligible pre-TCP × 2016	-0.038	-0.052	-0.040	-0.116***	-0.131***	-0.111***
	(0.296)	(0.142)	(0.249)	(0.006)	(0.004)	(0.008)
2-child eligible pre-TCP × 2018	-0.074**	-0.054	-0.039	-0.127***	-0.170***	-0.142***
	(0.022)	(0.148)	(0.329)	(0.000)	(0.000)	(0.002)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Oberservations	17924	15960	15297	15755	13831	13117

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A9: Effect of the Two-Child Policy on Marriages: Age range from 18 to 35

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.025	-0.009	-0.002	-0.073**	-0.085***	-0.079***
	(0.282)	(0.722)	(0.947)	(0.030)	(0.000)	(0.000)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	22269	19773	19095	24452	21805	20837
Pre:						
2-child eligible pre-TCP × 2008	-0.018	-0.014	-0.012	-0.014	-0.010	-0.012
	(0.390)	(0.508)	(0.555)	(0.562)	(0.700)	(0.636)
2-child eligible pre-TCP × 2010	-0.024	-0.019	-0.017	-0.033*	-0.031	-0.032*
	(0.192)	(0.301)	(0.342)	(0.085)	(0.106)	(0.100)
Post:						
2-child eligible pre-TCP × 2014	-0.032	-0.033	-0.027	-0.068***	-0.087***	-0.081***
	(0.194)	(0.140)	(0.222)	(0.002)	(0.000)	(0.000)
2-child eligible pre-TCP × 2016	-0.028	-0.017	-0.007	-0.080***	-0.089***	-0.085***
	(0.375)	(0.573)	(0.824)	(0.002)	(0.000)	(0.000)
2-child eligible pre-TCP × 2018	-0.042	-0.022	-0.008	-0.087***	-0.104***	-0.094***
	(0.130)	(0.518)	(0.827)	(0.002)	(0.001)	(0.002)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	26156	23397	22427	29426	26294	24866

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 18-35 and women aged 18-35 in the respective year. Data from the CFPS 2010-2018.

Table A10: Effect of the Two-Child Policy on Marriages: Age range from 18 to 35 - Balanced Sample from 2010 to 2018

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	0.005	0.024	0.024	-0.104**	-0.109***	-0.106***
	(0.853)	(0.416)	(0.419)	(0.021)	(0.000)	(0.000)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	16103	14515	14160	15150	13802	13361
Pre:						
2-child eligible pre-TCP × 2008	-0.013	-0.009	-0.006	0.002	-0.001	-0.002
	(0.568)	(0.718)	(0.808)	(0.947)	(0.960)	(0.947)
2-child eligible pre-TCP × 2010	-0.027	-0.023	-0.020	-0.001	-0.007	-0.006
	(0.225)	(0.292)	(0.333)	(0.966)	(0.772)	(0.789)
Post:						
2-child eligible pre-TCP × 2014	-0.023	-0.014	-0.010	-0.054*	-0.065**	-0.065**
	(0.364)	(0.589)	(0.694)	(0.050)	(0.020)	(0.011)
2-child eligible pre-TCP × 2016	0.001	0.011	0.015	-0.096***	-0.105***	-0.100***
	(0.978)	(0.728)	(0.607)	(0.004)	(0.001)	(0.001)
2-child eligible pre-TCP × 2018	-0.013	0.006	0.012	-0.095***	-0.109***	-0.107***
	(0.648)	(0.867)	(0.742)	(0.006)	(0.004)	(0.005)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	18731	17009	16525	17749	16231	15620

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 18-35 and women aged 18-35 in the respective year. Data from the CFPS 2010-2018.

Table A11: Effect of the Two-Child Policy on Marriages: Age range from 20 (women)/22 (men) to 40

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.025	-0.024	-0.021	-0.052**	-0.057***	-0.055***
	(0.168)	(0.252)	(0.310)	(0.010)	(0.005)	(0.007)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	24942	22340	21689	23782	21019	20173

Pre:						
2-child eligible pre-TCP × 2008	0.004	0.007	0.008	-0.000	-0.012	-0.014
	(0.814)	(0.707)	(0.655)	(0.987)	(0.633)	(0.552)
2-child eligible pre-TCP × 2010	-0.010	-0.006	-0.005	-0.030	-0.039**	-0.039**
	(0.589)	(0.730)	(0.771)	(0.110)	(0.037)	(0.031)
Post:						
2-child eligible pre-TCP × 2014	-0.034*	-0.039**	-0.034*	-0.040*	-0.055***	-0.051***
	(0.089)	(0.034)	(0.071)	(0.067)	(0.004)	(0.006)
2-child eligible pre-TCP × 2016	-0.018	-0.024	-0.018	-0.051**	-0.066***	-0.066***
	(0.469)	(0.325)	(0.447)	(0.042)	(0.005)	(0.003)
2-child eligible pre-TCP × 2018	-0.030	-0.020	-0.013	-0.070***	-0.082***	-0.076***
	(0.176)	(0.473)	(0.640)	(0.008)	(0.003)	(0.005)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	29073	26235	25334	28349	25170	23950

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-40 and women aged 20-40 in the respective year. Data from the CFPS 2010-2018.

Table A12: Effect of the Two-Child Policy on Marriages: Age range from 20 (women)/22 (men) to 40 - Balanced Sample from 2010 to 2018

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.020	-0.011	-0.013	-0.095***	-0.087***	-0.088***
	(0.396)	(0.663)	(0.625)	(0.000)	(0.000)	(0.000)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	18504	16864	16531	14404	13161	12837
Pre:						
2-child eligible pre-TCP × 2008	0.009	0.012	0.013	0.017	0.008	0.007
	(0.688)	(0.603)	(0.540)	(0.515)	(0.757)	(0.772)
2-child eligible pre-TCP × 2010	-0.012	-0.008	-0.008	-0.004	-0.013	-0.013
	(0.559)	(0.675)	(0.699)	(0.838)	(0.459)	(0.449)
Post:						
2-child eligible pre-TCP × 2014	-0.040**	-0.033*	-0.029	-0.037	-0.037	-0.038*
	(0.043)	(0.098)	(0.131)	(0.118)	(0.107)	(0.073)
2-child eligible pre-TCP × 2016	-0.018	-0.018	-0.015	-0.084***	-0.091***	-0.091***
	(0.467)	(0.489)	(0.544)	(0.005)	(0.002)	(0.001)
2-child eligible pre-TCP × 2018	-0.023	-0.009	-0.009	-0.095***	-0.092***	-0.093***
	(0.347)	(0.760)	(0.756)	(0.001)	(0.004)	(0.003)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	21401	19654	19211	16702	15348	14909

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-40 and women aged 20-40 in the respective year. Data from the CFPS 2010-2018.

Table A13: Effect of the Two-Child Policy on Marriages: Imputing odd years

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.027	-0.020	-0.016	-0.054**	-0.062**	-0.058**
	(0.233)	(0.424)	(0.542)	(0.025)	(0.017)	(0.023)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	35574	31782	30135	34691	30829	28892
Pre:						
2-child eligible pre-TCP × 2008	0.007	0.007	0.006	-0.005	-0.008	-0.007
	(0.775)	(0.749)	(0.799)	(0.880)	(0.814)	(0.829)
2-child eligible pre-TCP × 2009	0.031	0.031	0.030	-0.015	-0.017	-0.016
	(0.111)	(0.125)	(0.165)	(0.586)	(0.549)	(0.572)
2-child eligible pre-TCP × 2010	0.012	0.014	0.013	-0.029	-0.030	-0.029
	(0.553)	(0.480)	(0.525)	(0.245)	(0.217)	(0.236)
2-child eligible pre-TCP × 2011	0.008	0.009	0.009	-0.003	-0.002	-0.002
	(0.541)	(0.488)	(0.513)	(0.822)	(0.891)	(0.905)
Post:						
2-child eligible pre-TCP × 2013	0.021	0.012	0.009	-0.014	-0.021	-0.024
	(0.268)	(0.522)	(0.616)	(0.441)	(0.245)	(0.195)
2-child eligible pre-TCP × 2014	-0.021	-0.025	-0.028	-0.060**	-0.072***	-0.072***
	(0.408)	(0.275)	(0.243)	(0.022)	(0.009)	(0.009)
2-child eligible pre-TCP × 2015	0.011	0.014	0.013	-0.036	-0.046	-0.041
	(0.711)	(0.662)	(0.673)	(0.205)	(0.138)	(0.168)
2-child eligible pre-TCP × 2016	-0.013	-0.016	-0.018	-0.059**	-0.072**	-0.072**
	(0.652)	(0.590)	(0.547)	(0.044)	(0.024)	(0.017)
2-child eligible pre-TCP × 2017	-0.012	-0.004	0.006	-0.076**	-0.088**	-0.082**
	(0.649)	(0.899)	(0.852)	(0.020)	(0.019)	(0.030)
2-child eligible pre-TCP × 2018	-0.030	-0.023	-0.010	-0.080**	-0.097**	-0.091**
	(0.296)	(0.525)	(0.787)	(0.021)	(0.016)	(0.030)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	38700	34816	32827	38194	34090	31726

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A14: Effect of the Two-Child Policy on Marriages: Imputing odd years - Balanced Sample from 2010 to 2018

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.009	0.004	0.005	-0.094***	-0.093***	-0.089***
	(0.743)	(0.903)	(0.874)	(0.007)	(0.008)	(0.010)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	24759	22552	21678	18938	17358	16573
Pre:						
2-child eligible pre-TCP × 2008	0.025	0.026	0.024	0.034	0.029	0.028
	(0.241)	(0.245)	(0.273)	(0.329)	(0.396)	(0.398)
2-child eligible pre-TCP × 2009	0.049**	0.047**	0.046*	0.036	0.029	0.028
	(0.029)	(0.048)	(0.062)	(0.262)	(0.380)	(0.373)
2-child eligible pre-TCP × 2010	0.020	0.020	0.019	0.026	0.020	0.019
	(0.313)	(0.345)	(0.378)	(0.341)	(0.492)	(0.487)
2-child eligible pre-TCP × 2011	0.011	0.010	0.016	-0.009	-0.010	-0.011
	(0.480)	(0.523)	(0.332)	(0.600)	(0.612)	(0.613)
Post:						
2-child eligible pre-TCP × 2013	0.018	0.017	0.014	-0.031	-0.017	-0.019
	(0.375)	(0.349)	(0.440)	(0.141)	(0.484)	(0.370)
2-child eligible pre-TCP × 2014	-0.008	-0.003	-0.003	-0.047	-0.047	-0.053
	(0.787)	(0.928)	(0.932)	(0.177)	(0.243)	(0.142)
2-child eligible pre-TCP × 2015	0.018	0.026	0.023	-0.051	-0.054	-0.047
	(0.567)	(0.429)	(0.467)	(0.179)	(0.151)	(0.179)
2-child eligible pre-TCP × 2016	0.015	0.022	0.020	-0.094**	-0.100**	-0.097**
	(0.661)	(0.534)	(0.560)	(0.025)	(0.021)	(0.019)
2-child eligible pre-TCP × 2017	0.019	0.029	0.032	-0.089**	-0.087*	-0.087*
	(0.535)	(0.428)	(0.391)	(0.038)	(0.074)	(0.060)
2-child eligible pre-TCP × 2018	-0.000	0.010	0.017	-0.091**	-0.097**	-0.095**
	(0.997)	(0.801)	(0.671)	(0.029)	(0.041)	(0.044)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	26785	24542	23515	20559	18929	18006

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A15: Summary statistics including waves 2020 and 2022

	(1) 2008 mean	(2) 2010 mean	(3) 2012 mean	(4) 2014 mean	(5) 2016 mean	(6) 2018 mean	(7) 2020 mean	(8) 2022 mean
Female	0.58	0.57	0.48	0.48	0.49	0.49	0.47	0.49
Age	28.24	27.99	27.58	27.67	27.78	28.01	29.11	28.59
Han	0.91	0.91	0.92	0.92	0.92	0.91	0.91	0.91
Rural residence status	0.65	0.65	0.71	0.73	0.75	0.75	0.85	0.82
Only child	0.16	0.19	0.18	0.19	0.20	0.20	0.17	0.20
Group 1	0.04	0.05	0.05	0.21	0.22	0.23	0.20	0.23
Group 2	0.17	0.19	0.19	0.04	0.03	0.03	0.05	0.04
Group 2b	0.46	0.46	0.51	0.53	0.53	0.52	0.59	0.55
Group 3	0.33	0.30	0.25	0.23	0.22	0.22	0.16	0.18
2-child eligible pre-TCP	0.44	0.47	0.50	0.51	0.51	0.52	0.54	0.54
Married	0.74	0.74	0.64	0.63	0.63	0.61	0.66	0.54
Observations	6340	6165	6576	6510	7516	8203	2118	5403

Note: Based on the China Family Panel Study 2010 - 2022. Each year, men and women of marriageable age (men: 22 - 35; women 20 - 35) are included in the survey. *Exception: 2008 is based on the data from 2010, using information on the marriage year.

Table A16: Effect of the Two-Child Policy on Marriages: Including the 2020 and 2022 waves

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.018	-0.015	-0.011	-0.040	-0.065**	-0.062**
	(0.447)	(0.568)	(0.696)	(0.182)	(0.024)	(0.026)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	21678	18265	17403	20641	17084	16123
Pre:						
2-child eligible pre-TCP × 2008	-0.013	-0.010	-0.008	-0.007	-0.012	-0.014
	(0.593)	(0.675)	(0.745)	(0.827)	(0.717)	(0.665)
2-child eligible pre-TCP × 2010	-0.024	-0.019	-0.017	-0.033	-0.038	-0.038
	(0.244)	(0.358)	(0.398)	(0.170)	(0.128)	(0.121)
Post:						
2-child eligible pre-TCP × 2014	-0.034	-0.043*	-0.035	-0.045*	-0.065**	-0.062***
	(0.195)	(0.095)	(0.173)	(0.083)	(0.011)	(0.009)
2-child eligible pre-TCP × 2016	-0.015	-0.020	-0.012	-0.048	-0.076**	-0.074**
	(0.654)	(0.567)	(0.705)	(0.135)	(0.016)	(0.012)
2-child eligible pre-TCP × 2018	-0.036	-0.032	-0.019	-0.066*	-0.097**	-0.090**
	(0.261)	(0.468)	(0.679)	(0.057)	(0.016)	(0.023)
2-child eligible pre-TCP × 2020	-0.019	-0.009	-0.015	-0.031	-0.033	-0.016
	(0.770)	(0.917)	(0.876)	(0.492)	(0.609)	(0.815)
2-child eligible pre-TCP × 2022	-0.045	-0.027	-0.023	-0.035	-0.064	-0.060
	(0.191)	(0.494)	(0.615)	(0.476)	(0.187)	(0.229)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	24775	21172	20061	24054	20152	18888

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016, 2018, 2020 and 2022. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, Han, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2022.

Table A17: Effect of the Two-Child Policy on Fertility Outcomes: Having at Least Two Children

	All			Married only		
	(1)	(2)	(3)	(4)	(5)	(6)
2-child eligible pre-TCP × post 2015	-0.103***	-0.072**	-0.065**	-0.094***	-0.063**	-0.057*
	(0.002)	(0.013)	(0.022)	(0.006)	(0.041)	(0.058)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	55080	49845	48376	48069	43759	42744
Pre:						
2-child eligible pre-TCP × 2008	0.061***	0.053***	0.048***	0.053***	0.049***	0.044***
	(0.001)	(0.001)	(0.001)	(0.005)	(0.002)	(0.005)
2-child eligible pre-TCP × 2010	0.042***	0.036***	0.033***	0.031**	0.030**	0.026**
	(0.001)	(0.001)	(0.003)	(0.025)	(0.014)	(0.028)
Post:						
2-child eligible pre-TCP × 2014	-0.001	0.008	0.020	0.007	0.015	0.024
	(0.978)	(0.661)	(0.274)	(0.746)	(0.464)	(0.236)
2-child eligible pre-TCP × 2016	-0.035	-0.015	-0.013	-0.030	-0.010	-0.007
	(0.126)	(0.484)	(0.565)	(0.238)	(0.668)	(0.770)
2-child eligible pre-TCP × 2018	-0.086***	-0.062**	-0.055**	-0.085***	-0.056*	-0.051*
	(0.004)	(0.017)	(0.041)	(0.009)	(0.053)	(0.085)
Province x Year FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
County FE	no	yes	yes	no	yes	yes
Local Fertility Rate	no	no	yes	no	no	yes
Observations	64931	59189	57192	56446	51808	50423

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Includes province*year fixed effects, group indicators and individual characteristics (female, age, Han, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A18: Effect of the Two-Child Policy on Fertility Outcomes: Having at Least Two Children by Fertility Level

	Married only			
	Low fertility rate		High fertility rate	
	(1)	(2)	(3)	(4)
2-child eligible pre-TCP $\times$ post 2015	-0.053**	-0.033	-0.110*	-0.099*
	(0.015)	(0.197)	(0.060)	(0.078)
Province x Year FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
County FE	no	yes	no	yes
Observations	24891	20869	21874	21874
Pre:				
2-child eligible pre-TCP $\times$ 2008	0.044***	0.047***	0.018	0.007
	(0.009)	(0.007)	(0.616)	(0.839)
2-child eligible pre-TCP $\times$ 2010	0.022	0.024*	0.016	0.006
	(0.103)	(0.081)	(0.599)	(0.829)
Post:				
2-child eligible pre-TCP $\times$ 2014	0.025	0.025	0.001	-0.005
	(0.261)	(0.278)	(0.978)	(0.876)
2-child eligible pre-TCP $\times$ 2016	0.000	0.013	-0.062	-0.062
	(0.995)	(0.513)	(0.187)	(0.173)
2-child eligible pre-TCP $\times$ 2018	-0.051**	-0.028	-0.136**	-0.125**
	(0.036)	(0.346)	(0.012)	(0.020)
Province x Year FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
County FE	no	yes	no	yes
Observations	28848	24543	25878	25878

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Linear regression with a post indicator equal to 1 for cohorts 2016 and 2018, and an indicator for ever married as the dependent variable. Pre-cohorts include 2008, 2010 and 2012. The cohort 2014 is not included. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the post indicator. Panel B: Linear regression with 2012 as the baseline and an indicator for ever married as the dependent variable. Shows coefficient estimates of being eligible for a second child before the Two-Child Policy interacted with the year indicators. Both Panels include province*year fixed effects, group indicators and individual characteristics (female, age, Han, rural residence status, only child, education). Columns 2, 3, and 5, 6 include county fixed effects. Columns 3 and 6 include the average county fertility rate as a control. Standard errors clustered on the treatment level (group*province). Cohorts include all men aged 22-35 and women aged 20-35 in the respective year. Data from the CFPS 2010-2018.

Table A19: Effect of the Two-Child Policy on Marriage Assortativity Measured by the Distance Between Perfect Assortative Matching and Actual Matching

	(1)	(2)	(3)	(4)
Reference group:	*Married between 2009 and 2012*		*Married in 2011 or 2012*	
Middle quartiles of previously eligible ×	-0.091	-0.141	-0.120	-0.170**
married post 2015	(0.287)	(0.157)	(0.157)	(0.049)
Middle quantiles of previously eligible ×			-0.058	-0.014
married 2009 or 2010			(0.464)	(0.847)
Province FE	yes	yes	yes	yes
Controls	no	yes	no	yes
Observations	62	62	62	62
Post Observations	16	16	16	16

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Linear regression where each observation is a province-cohort with more than 12 couples having married in that time frame and with the respondent being between 20 (women)/22 (men) and 35 years old. The dependent variable is the deviation from perfect assortative matching. Includes two pre-cohorts (couples married in 2009 or 2010 and married in 2011 or 2012) and one post-cohort (couples married between 2016 and 2020). "Married post 2015" is an indicator for the post-cohort. "Middle quartiles of previously eligible" is an indicator for province-cohorts where the population share of those eligible for a second child based on the policy before 2015 is between the 25th and the 75th quartile. Controls include the population share of individuals being Han, having a rural hukou and being an only child within the province-cohort. Standard errors are clustered on the province level. Data from the CFPS 2010-2022.

Table A20: Effect of the Two-Child Policy on Marriage Matching by Education: Different cut-offs

	(1)	(2)	(3)	(4)	(5)	(5)	(7)	(8)
Panel A:	**Dependent variable: Correlation coefficient**							
Middle quartiles of previously	0.227**	0.259**	0.221*	0.253*	0.148	0.193	0.323*	0.358**
eligible × married post-2015	(0.036)	(0.031)	(0.066)	(0.055)	(0.242)	(0.226)	(0.058)	(0.010)
Panel B:	**Dependent variable: Deviation Score**							
Middle quartiles of previously	-0.084	-0.124*	-0.101*	-0.156*	-0.111	-0.165	-0.200***	-0.365***
eligible × married post-2015	(0.109)	(0.097)	(0.058)	(0.066)	(0.107)	(0.143)	(0.001)	(0.001)
Province FE	yes	yes	yes	yes	yes	yes	yes	yes
Distribution Control	no	yes	no	yes	no	yes	no	yes
Group Shares	no	yes	no	yes	no	yes	no	yes
Observations	64	64	62	62	60	60	51	51
Post Observations	18	18	16	16	14	14	9	9

Note: P-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Linear regression where each observation is a province-cohort. Varies different cut-offs for the number of couples having married in that time frame and with the respondent being between 20 (women)/22 (men) and 35 years old within one province-cohort: more than 10 (columns (1) and (2)), 12 (columns (3) and (4)), 14 or 16 (columns (5) and (6)), 18 (columns (7) and (8)). The dependent variable is the correlation coefficient (Panel A) and the deviation score measuring deviation for perfect increasing-preferences matching (Panel B). Includes two pre-cohorts (couples married in 2009 or 2010 and married in 2011 or 2012) and one post-cohort (couples married between 2016 and 2020). "Married post 2015" is an indicator for the post-cohort. "Middle quartiles of previously eligible" is an indicator for province-cohorts where the population share of those eligible for a second child based on the policy before 2015 is between the 25th and the 75th quartile. Controls include the population share of individuals being Han, having a rural hukou and being an only child within the province-cohort. Standard errors are clustered on the province level. Data from the CFPS 2010-2022.

B Theoretical Framework

B.1 Marriage Rates with Fertility Constraints

In this section, we sketch the theoretical framework and derive the predictions to be tested in the empirical section. We consider the decision of men and women to get married (we consider only heterosexual marriages), based on Huang et al. (2015). After marriage, couples decide how many children they will have.

As a first step, we consider a representative couple with woman i and man j. When married, they decide how many children they will have, denoted n_{ij}, from whom they derive strictly increasing and concave utility $\alpha u(n_{ij})$ with α measuring their fertility preferences. They earn $y_m > 0$ and incur the cost $C > 0$ for raising each child.

Denote $\bar{n}_{ij}$ the fertility constraints or assigned birth quota that the couple is subject to due to the policy setting: a couple is allowed one or two children: $\bar{n}_{ij} \in \{1, 2\}$. For each child they have more than what they are allowed to have, they pay a fine $f \geq 0$. These could be the official fines collected by the government, potential career consequences, and other disadvantages, but also the cost of getting around the assigned birth quota by for example temporal emigration.

A married couple thus gets the following utility:

$$U_{ij}^m(\bar{n}_{ij}) = \alpha u(n_{ij}) + y_m - Cn_ij - \delta_{n_{ij} \geq \bar{n}_{ij}}(n_{ij} - \bar{n}_{ij})f \tag{A1}$$

with $\delta_{n_{ij} \geq \bar{n}_{ij}}$ being an indicator function equal to one if the couples has more children than they are allowed to. Denote $n_{ij}^* \in \{1, 2\}$ the number of children the couple wants to have that maximizes equation A1.

If they stay single, they do not have children, men receive income $y_{s,m}$, and women receive income $y_{s,w}$. We define the marriage surplus as the utility of getting married minus the utility of staying single. The marriage surplus is thus defined as:

$$V_{ij}(\bar{n}_{ij}) = U_{ij}^m(\bar{n}_{ij}) - y_{s,m} - y_{s,w} \tag{A2}$$

We assume transferable utility: The marriage surplus can be freely divided between the two spouses, as in Choo and Siow (2006) or Chiappori et al. (2018). As a result, the couple decides to get married if the marriage surplus $V_{ij}(\bar{n}_{ij})$ is positive.

Proposition 1 *Increasing the birth quota from $\bar{n}_{ij} = 1$ to $\bar{n}_{ij} = 2$ increases marriage rates for a positive fine level f when $\alpha > \underline{\alpha}$ and $\underset{\sim}{\alpha} < \alpha < \tilde{\alpha}(f)$, with*

- $\underline{\alpha} = \frac{C}{u(2)-u(1)}$
- $\underset{\sim}{\alpha} = \frac{2C+y_{s,m}+y_{s,w}-y_m}{u(2)}$

- $\tilde{\alpha}(f) = \begin{cases} \frac{C+y_{s,m}+y_{s,w}-y_m}{u(1)}, & \textit{if } f > \alpha(u(2)-u(1)) - C \\ \\ \frac{2C+f+y_{s,m}+y_{s,w}-y_m}{u(2)}, & \textit{if } f \leq \alpha(u(2)-u(1)) - C \end{cases}$

We first show that increasing the birth quota increases the marriage surplus when couples want to have two children ($\alpha > \underline{\alpha}$). We then show that for marriage rates to increase, the marriage surplus needs to be negative before the increase and positive after the increase ($\underset{\sim}{\alpha} < \alpha < \tilde{\alpha}(f)$).

1. **Marriage surplus increases when the constraint is relaxed if $\alpha > \underline{\alpha}$.**

   The couple wants to have 2 children (i.e., $n^* = 2$) without a fine when $\alpha > \underline{\alpha}$:

$$\alpha u(2) - 2C > \alpha u(1) - C \quad \Rightarrow \quad \alpha > \underline{\alpha} = \frac{C}{u(2) - u(1)}.$$

   We compare the surplus when they are constraint ($\bar{n}_{ij} = 1$) and not constraint ($\bar{n}_{ij} = 2$). When $\bar{n}_{ij} = 2$, the couple can have 2 children without a fine:

$$U^m_{ij}(\bar{n}_{ij} = 2, n_{ij} = 2) = \alpha u(2) + y_m - 2C.$$

   When $\bar{n}_{ij} = 1$, the couple is constrained, and if they choose $n_{ij} = 2$, they must pay a fine. They can also choose to only have one child, $n_{ij} = 1$, which gives a lower marriage surplus as $n_{ij} = 1$ is not the optimal number of children.

   When the constrained couples chooses $n_{ij} = 2$, they must pay a fine, such that:

$$U^m_{ij}(\bar{n}_{ij} = 1, n_{ij} = 2) = \alpha u(2) + y_m - 2C - f.$$

   So the difference in marriage surplus is:

$$\Delta V_{ij} = V_{ij}(\bar{n}_{ij} = 2) - V_{ij}(\bar{n}_{ij} = 1) = f.$$

   This is strictly positive for $f > 0$.

   The decision for a constrained couple to have one or two children depends on the fine. They choose to have only one child if:

$$U^m_{ij}(\bar{n}_{ij} = 1, n_{ij} = 1) > U^m_{ij}(\bar{n}_{ij} = 1, n_{ij} = 2)$$

$$\alpha u(1) + y_m - C > \alpha u(2) + y_m - 2C - f$$
$$f > \alpha(u(2) - u(1)) - C$$

2. **Marriage rates only increase if $V_{ij}(\bar{n}_{ij} = 1) < 0 < V_{ij}(\bar{n}_{ij} = 2)$.**

While the surplus increases, this only leads to a change in marriage behavior if the couple was previously not marrying due to a negative surplus under the constraint, but would now marry when the constraint is removed and the surplus becomes positive.

- **Define the threshold $\tilde{\alpha}(f)$ such that $V_{ij}(\bar{n}_{ij} = 1) = 0$.**

  We have the marriage surplus for $V_{ij}(\bar{n}_{ij} = 1)$ depending on the number of children the couple has as a function of the fine level:

$$V_{ij}(\bar{n}_{ij} = 1) = \begin{cases} V_{ij}(\bar{n}_{ij} = 1, n_{ij} = 1), & \text{if } f > \alpha(u(2) - u(1)) - C \\ V_{ij}(\bar{n}_{ij} = 1, n_{ij} = 2), & \text{if } f \leq \alpha(u(2) - u(1)) - C \end{cases}$$

  Solving $V_{ij}(\bar{n}_{ij} = 1) = 0$ for α gives:

$$\tilde{\alpha}(f) = \begin{cases} \frac{C + y_{s,m} + y_{s,w} - y_m}{u(1)}, & \text{if } f > \alpha(u(2) - u(1)) - C \\ \\ \frac{2C + f + y_{s,m} + y_{s,w} - y_m}{u(2)}, & \text{if } f \leq \alpha(u(2) - u(1)) - C \end{cases}$$

  Therefore:

  - If $\alpha < \tilde{\alpha}(f)$, then $V_{ij}(\bar{n}_{ij} = 1) < 0$: the couple does not marry under the constraint.
  - If $\alpha > \tilde{\alpha}(f)$, then $V_{ij}(\bar{n}_{ij} = 1) > 0$: the couple marries even under the constraint.

- **Define the threshold $\underset{\sim}{\alpha}$ such that $V_{ij}(\bar{n}_{ij} = 2) = 0$.**

  Solving $V_{ij}(\bar{n}_{ij} = 2) = 0$ for α gives:

$$\Rightarrow \underset{\sim}{\alpha} = \frac{2C + y_{s,m} + y_{s,w} - y_m}{u(2)}.$$

  Therefore:

  - If $\alpha < \underset{\sim}{\alpha}$, then $V_{ij}(\bar{n}_{ij} = 2) < 0$: no marriage even after relaxing the constraint.
  - If $\alpha > \underset{\sim}{\alpha}$, then $V_{ij}(\bar{n}_{ij} = 2) > 0$: the couple marries under the relaxed policy.

Hence, the policy of relaxing the fertility constraint increases marriage rates only for couples with $\alpha > \underline{\alpha}$ and $\underset{\sim}{\alpha} < \alpha < \tilde{\alpha}(f)$: Couples who were previously not marrying when constrained but now do so when the constraint is lifted. In words, there need to be couples that want two children when unconstraint ($\alpha > \underline{\alpha}$), do not want to get married when they are only allowed to have a child ($\alpha < \tilde{\alpha}(f)$), but want to get married when allowed a second child ($\alpha > \underset{\sim}{\alpha}$). While this is feasible, it is also possible (depending on fines f) that this set is empty.

B.2 Including differential fertility constraints

We suppose that women are defined by their socio-economic group x_i and men by x_j, which determines their assigned birth quota. There are three different groups: $x_i \in \{1, 2, 3\}$ and $x_j \in \{1, 2, 3\}$. The birth quota for women i are:

- $x_i = 1$ (Group 1): Allowed to have two children, independent on the characteristic of their spouse:

$$V_{ij}(x_i = 1, x_j) = V_{ij}(\bar{n}_{ij} = 2) \; \forall \; x_j$$

- $x_i = 2$ (Group 2): Allowed to have two children if they marry someone from the same group, or from group 1.

$$V_{ij}(x_i = 2, x_j) = \begin{cases} V_{ij}(\bar{n}_{ij} = 2), & \text{if } x_j = 2 \text{ or } x_j = 1 \\ V_{ij}(\bar{n}_{ij} = 1), & \text{otherwise} \end{cases}$$

- $x_i = 3$ (Group 3): Never allowed to children, except if they marry someone from group 1.

$$V_{ij}(x_i = 3, x_j) = \begin{cases} V_{ij}(\bar{n}_{ij} = 2), & \text{if } x_j = 1 \\ V_{ij}(\bar{n}_{ij} = 1), & \text{otherwise} \end{cases}$$

The birth quota rules for man x_j are symmetric. For $\alpha > \underline{\alpha}$ (people prefer two children), the marriage surpluses can thus be ranked:

$$V_{ij}(x_i = 1, \forall x_j) = V_{ij}(\forall x_i, x_j = 1) = V_{ij}(x_i = 2, x_j = 2) > V_{ij}(x_i = 3, x_j \neq 1) = V_{ij}(x_i \neq 1, x_j = 3)$$

For now, we assume that there is a balanced marriage market, that is, the same number of women and men, with $I = J$. We also assume that there is the same number of men and women within each socio-economic group ($I_1 = J_1$, $I_2 = J_2$, $I_3 = J_3$). A property of transferable utility for marriage is that the final matches are those that maximize the sum of all marriage surpluses within a marriage market (Roth and Sotomayor, 1992; Choo and Siow, 2006).

Intuitively, consider a marriage market with one woman and one man from group 1, and one woman and one man from group 3. The group-3 woman is willing to offer a larger share of the marriage surplus to the group-1 man than the group-1 woman is, because her alternative of marrying the group-3 man yields a lower surplus:

$$V_{ij}(x_i = 1, x_j = 3) + V_{ij}(x_i = 3, x_j = 1) > V_{ij}(x_i = 1, x_j = 1) + V_{ij}(x_i = 3, x_j = 3)$$

As a result, all women from group 1 will match with men from group 3, all men from group 1 will match with women in group 3, and men and women in group 2 will intermarry. Yet, group 1 might be rare, and thus, not all group-3 people will manage to find a group-1 spouse. This is the case if $I_1 < \frac{I_3}{2}$. In this case, group 3 individuals, on average, have a lower birth quota than those from group 1 or group 2. We can stipulate:

Proposition 2 *Increasing the birth quota from $\bar{n}_{ij} = 1$ to $\bar{n}_{ij} = 2$ for all couples increases marriage rates among those not allowed a second child (group 3) for a positive fine level f when $\alpha > \underline{\alpha}$ and $\underset{\sim}{\alpha} < \alpha < \tilde{\alpha}(f)$, with*

- $\underline{\alpha} = \frac{C}{u(2)-u(1)}$
- $\underset{\sim}{\alpha} = \frac{2C+y_{s,m}+y_{s,w}-y_m}{u(2)}$
- $\tilde{\alpha}(f) = \begin{cases} \frac{C+y_{s,m}+y_{s,w}-y_m}{u(1)}, & \textit{if } f > \alpha(u(2)-u(1))-C \\ \frac{2C+f+y_{s,m}+y_{s,w}-y_m}{u(2)}, & \textit{if } f \leq \alpha(u(2)-u(1))-C \end{cases}$

and when those allowed to have a second child independent of their spouses' characteristics (group 1) are rare ($I_1 < \frac{I_3}{2}$).

One notes that we do not include any in-group preferences. In-group preferences would lead to more homogamic, within-group matches and would relax the condition that group 1 individuals need to be rare to a certain extent.

B.3 Including an unbalanced sex ratio

We now assume that within the marriage market, there are more men than women ($J > I$), and group-1 individuals are relatively rare ($I_1 < \frac{I_3}{2}$). The share of people within each group stays the same (e.g., $\frac{I_1}{I} = \frac{J_1}{J}$). Now, some men stay unmarried even if their marriage surplus with a birth quota of 1 would be positive (while all women with a positive marriage surplus get married). Transferable utility matching implies that men who create the lowest marriage surplus are those who stay unmarried. Under the condition that group 1 people are rare, this implies group-3 men. This also touches on the surplus men in group 2 ($J_2 - I_2$), who now compete with group-3 men. Assuming that if the matches give the same marriage surplus one is chosen at random, we can calculate the likelihood of marriage for men in each group:

- Group 1: All men get married (to group-3 women): $L_1 = 1$
- Group 2: Either they get married to a group-2 women, which happens with likelihood $\frac{I_2}{J_2}$, or compete with group-3 men for group-1 women and group-3 women who do not marry a group-1 man (which happens with likelihood $\frac{J_2-I_2}{J_2}$):
  $L_2 = \frac{I_2}{J_2} + \frac{I_1+(I_3-J_1)}{J_3+(J_2-I_2)} . \frac{J_2-I_2}{J_2}$
- Group 3: They compete for group-1 women and group-3 women who do not marry a group-1 man (together with group-2 men who are not marrying group-2 women)
  $L_3 = \frac{I_1+(I_3-J_1)}{J_3+(J_2-I_2)}$

We thus have $L_1 > L_2 > L_3$. Now, after the policy relaxation, which assigns a birth quota of 2 to everyone, every match creates the same marriage surplus, thus marriages are random. The post-relaxation likelihood L_p for all men is $L_p = \frac{I}{J}$, which is lower than L_1 and L_2 and higher than L_3. Thus, we can postulate:

Proposition 3 *Under the assumption of a skewed sex ratio with more men than women ($J > I$), a rare group 1 ($I_1 < \frac{I_3}{2}$), high fertility preferences ($\alpha > \underline{\alpha}$) and high preferences for marriage ($\alpha > \tilde{\alpha}(f)$), assigning a birth quota of 2 to everyone increases the likelihood of getting married for group-3 men and decreases the likelihood of getting married for group-2 and group-1 men.*

Note that if there are high preferences for marriage ($\alpha > \tilde{\alpha}(f)$) (that is, people want to get married even if they are allowed to have only one child) then we only observe the change in relative marriage rates postulated by Proposition 3 and not an increase in the number of marriage as postulated by Proposition 2. Furthermore, Proposition 2 implies a symmetric effect for both men and women, while Proposition 3 implies an effect only for men. Given heterogeneous fertility preferences, we could also observe both effects.