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When the Last Big Arrow Was Loosed: How the One-Child Policy relaxations reshaped fertility trends in China [1]

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Abstract
In 2016, China's Universal Two-Child Policy ended the decades-long One-Child Policy. Fertility rose through 2017 and then fell, fueling claims that the reform's effects were transitory. Using the China Family Panel Studies and province-year exemption histories since the 1980s, we reconstruct couple-year second-child eligibility and estimate its causal effect. Eligibility raises the second-birth probability by 7.1 percentage points, with effects persisting for at least a decade. Counterfactual simulations imply that relaxations lifted the TFR level but left its secular downward slope largely intact, highlighting the distinction between a temporary spike, an upward level shift, and a genuine reversal of decline.

Keywords: One-Child Policy; Universal Two-Child Policy; total fertility rate; parity progression; difference-in-differences; event study; China
JEL classification: J11, J13, J18

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I. Introduction

In 2016, China ended its decades-long One-Child Policy (OCP), allowing all couples to have a second child in an effort to reverse the persistent fertility decline. In the wake of the reform, the total fertility rate (TFR) rose from 1.67 in 2015 to 1.82 in 2017.[1] The rebound, however, was short-lived. The TFR then declined sharply and fell below pre-reform levels, reaching 1.18 by 2022 (see Figure 1). This post-2016 pattern sparked a debate over whether the 2016 Universal Two-Child Policy (UTCP) produced only a temporary boost and, more broadly, whether removing parity restrictions can meaningfully reshape fertility trajectories once fertility preferences have shifted. Numerous media outlets, commentators, and public opinion agencies in China reported that the 2016 UTCP produced only short-lived effects (China News, 2020; Economic Observer, 2023; Ren, 2024). Academic studies likewise found largely temporary impacts (Fang et al., 2024; Feng et al., 2016; Guo et al., 2024; Qin and Wang, 2017; Zeng and Hesketh, 2016).

We argue that the 2016 UTCP and its preceding relaxations produced a significant and *persistent* increase in fertility. Figure 2 illustrates two competing theories. The first posits a temporary policy effect: a short-lived spike followed by reversion to the pre-existing downward trend (Panel A). Such a pattern could arise if desired fertility is already low but the long-standing restriction created a backlog of constrained couples who, once eligibility is granted, act quickly to avoid age-related fecundity constraints. The second theory predicts a persistent effect that manifests as a parallel upward shift in the fertility trend (Panel B), implying a lasting increase in the annual second-birth rate.

Yet the aggregate series in Figure 1 does not display a simple parallel shift. Interpreting post-2016 fertility therefore requires taking into account the gradual dismantling of the OCP over several decades. After a series of small-scale relaxations beginning in the 1990s, two major changes followed: the 2014 Selective Two-Child Policy, which permitted a second birth if either spouse was an only child (henceforth "STCP (either)"), and the 2016 UTCP, which allowed all couples to have a second child. Extending the Panel B framework, a sequence of relaxations may sustain fertility at an elevated level for some time (Panel C) or even generate incremental increases if relaxations intensify

[1] Total fertility rate is defined as the average number of children a woman has during her childbearing years, typically between ages 15 and 49.

in pace or scale (Panel D), even if the underlying trend continues to drift downward.

We provide quasi-experimental evidence on how successive OCP relaxations affected second-birth fertility and reshaped China's fertility trend. Using the China Family Panel Studies (2010–2022), we construct a couple-year panel with retrospective fertility histories covering 1980–2021. Our identification exploits staggered expansions of second-child eligibility arising from both the nationwide 2014 and 2016 reforms and earlier province- and individual-specific exemptions and regulations. We estimate difference-in-differences models with two-way fixed effects (TWFE) for couples with one child and implement event-study specifications to trace dynamics over the subsequent decade. We also examine heterogeneity across subgroups and use the causal estimates in a counterfactual micro-simulation to quantify how the relaxations altered recent fertility paths.

We make three contributions. First (identification), we leverage a substantially richer set of second-child-eligibility variation than prior work, which typically focuses on the 2014/2016 nationwide relaxations and proxies exposure with spouses' only-child status (Ge and Shi, 2024; Wu, 2022). Because many couples were already eligible under earlier exemptions, such designs misclassify treatment and attenuate effects. We reconstruct couple-year eligibility using province-by-year rule histories spanning 1982–2012, including the early Selective Two-Child Policy for couples in which both spouses were only children ("STCP (both)") and the One-and-a-Half-Child Policy for rural couples with a firstborn daughter, together with contemporaneous regulations on minimum birth intervals (often four years) and maternal age (typically 24–30) (Dong, 2020; Zhang and Liu, 2016).

The staggered rollout across provinces yields multiple, plausibly exogenous sources of variation. Eligibility is determined by ethnicity, only-child status, *hukou* registration (rural or urban),[2] sex of the firstborn, years since first birth, and mother's age, which we encode in TWFE models to estimate the effect of eligibility on annual second-birth fertility. While Li (2024) and Fang et al. (2024) use several exemption criteria (e.g., ethnicity, first child's sex, and hukou registration), our incorporation of binding birth-interval and maternal-age rules, integral parts of the policy regime, distinguishes our

[2]Hukou is China's household registration system that classifies individuals as rural (agricultural) or urban (non-agricultural) and registers them to a specific locality, usually their birthplace. Hukou registration shapes access to education, healthcare, housing, employment, and social security (Guo et al., 2025; Song, 2014). Urban hukou holders enjoy better access to these resources, leading to higher education, income, and occupational prestige (Koh et al., 2025).

design.[3]

Second (dynamics), we study longer-run responses to second-birth eligibility than the literature, which largely examines short windows after the 2014/2016 relaxations (Fang et al., 2024; Ge and Shi, 2024; Jin et al., 2024; Li, 2024).[4] We construct a retrospective couple-year panel covering 1980–2021, precisely embed the evolving exemption rules, and track eligibility at the couple-year level. Event-study estimates trace dynamics for a decade, allowing us to distinguish short-lived timing responses from persistent level shifts in fertility.

Third (macro implications), we translate micro-level estimates to the aggregate by simulating a counterfactual fertility path in the absence of post-1990 relaxations. This provides the first quantitative assessment of how successive OCP relaxations reshaped China's national fertility trajectory.

We also relate to research on policy responses to low fertility. Governments have deployed *price-based* instruments, including tax incentives (Azmat and González, 2010; Feyrer et al., 2008; Hart and Galloway, 2023), baby bonuses, and cash transfers (González and Trommlerová, 2022; Kim, 2024), and *quantity-based* restrictions adopted in various forms by over 70 percent of countries (De Silva and Tenreyro, 2017), including Vietnam's "one or two-child" policy (Ngo, 2020), South Korea's "stop at two," India's "two-child norm" (Mandal and Wenjun, 2023; Rao, 2022), and China's OCP. The number of countries enforcing restrictions has fallen in recent decades (Guo et al., 2024),[5] yet evidence on the consequences of removing such restrictions remains scarce. By evaluating China's relaxations, we provide guidance for policymakers contemplating transitions away from restrictive regimes.

Our main findings are as follows. First, OCP relaxations raise a couple's annual probability of a second birth by 7.1 percentage points; event-study estimates indicate that the effect persists for at least a decade with relatively stable magnitude. Second, responses are larger among rural and lower-socioeconomic-status households and, most strikingly, among mothers whose first child is a daughter. Stronger responses are also

[3] This study also contributes to the literature on China's family-planning policies (FPPs). Prior work has examined the OCP's enforcement and penalties using cross-sectional and temporal variation in fines (García, 2024; Huang et al., 2021, 2025; McElroy and Yang, 2000; Zhang, 2017), ethnic minorities as comparison groups (Li and Zhang, 2007), and cross-prefecture or cross-cohort exposure (Rossi and Xiao, 2024). By contrast, causal evidence on the effects of OCP *relaxations* remains limited; we fill this gap by providing new causal estimates.

[4] Jin et al. (2024) examines STCP (either) within one year; Ge and Shi (2024) and Fang et al. (2024) analyze two–three years post-UTCP; Li (2024) considers up to four years.

[5] E.g., South Korea repealed its limits in 1996, China eased controls in 2016, and Vietnam lifted its policy in 2025

found in regions characterized by higher sex ratios, wider gender wage gaps, and more traditional gender norms. Third, counterfactual simulations indicate that second-birth fertility drives most medium-run variation in overall fertility: the baseline and counterfactual TFR series pull apart sharply after 2014, implying a meaningful upward shift caused by the nationwide relaxations. Yet fertility returns to a steep decline after 2017, indicating that relaxations mainly acted as a buffer—lifting the level of the fertility path without altering its secular downward slope. These results clarify the post-2016 debate and underscore the importance of distinguishing between a temporary spike, an upward level shift, and a genuine reversal of the declining fertility trend.

II. Background

A. Evolution of China's Family-Planning Policies

One-Child Policy. — Fertility in China rose sharply in the 1960s, with the total fertility rate (TFR) near six births per woman, and the population surpassed 800 million by 1969. The central government launched the voluntary "Later, Longer, Fewer" (LLF) campaign in the early 1970s, promoting later marriage, longer birth intervals, and smaller families. In 1979, LLF gave way to the OCP, the most stringent family-planning regime in China's history. The OCP generally mandated a single birth per couple, with limited exceptions (e.g., ethnic minorities and designated rural areas; see column 1 in Table 1).

Early exemptions. — The OCP initially met substantial resistance, particularly in rural areas (Zhang, 2017). Provinces gradually relaxed enforcement via three exemption forms. First, beginning in 1982, provinces adopted the STCP ("both") for couples in which both spouses were only children, which spread nationwide by 2012 (columns 2–3 in Table 1). Second, starting in 1985, many provinces introduced the One-and-a-Half-Child Policy, allowing rural couples with a firstborn daughter to have a second child (column 4). Third, prior to the nationwide relaxation in 2014, fourteen provinces piloted a rural version of the STCP (either), permitting a second child if either spouse was an only child (see columns 5–6). Operational definitions of a "rural couple" varied: some provinces required the wife to hold a rural hukou, others accepted either spouse, and nearly half required both spouses (column 8).

Unauthorized second births—those not meeting exemption rules—were subject to financial and employment sanctions. Urban employees in state-owned enterprises and government agencies risked dismissal from their work units (*danwei*); rural households faced sizable one-time fines (Zhang, 2017). Because many provinces did not issue detailed family-planning guidelines until 1990 (Table A1), early enforcement was relatively lenient (Yang, 2003). Couples sometimes circumvented the rules—for example, via inter-ethnic marriages (Huang et al., 2025), claims of twins (Huang et al., 2016), or temporary relocation to other provinces.[6] Consistent with lenient early enforcement, second births remained common throughout the 1980s (Figure A1).

Birth-interval and maternal-age regulations. — To prevent early exemptions from undermining the OCP's objectives, provinces layered on rules governing minimum birth intervals and maternal age. As summarized in Table 2, provinces adopted one of three frameworks: (i) a minimum interval (typically four years) between the first and second births; (ii) a minimum maternal age for the second birth (usually 24–30); or (iii) both requirements—most commonly a four-year interval and a minimum maternal age of 28. By 1998, every province had adopted at least one framework; these rules remained in force until their gradual termination by 2016.

Recent relaxations. — Despite exemptions, fertility control remained stringent through the early 2010s, and many couples still lacked second-child eligibility (Figure B1). During this period, the TFR fell from 2.97 in 1982 to 1.80 in 2012.[7] The central government then initiated three major relaxations. First, the STCP (either) was announced in December 2013 and rolled out from 2014 (column 6 in Table 1). Second, the UTCP, announced in October 2015 and implemented on January 1, 2016, permitted all couples to have a second child, formally ending the OCP (column 7). Third, in 2021 the Three-Child Policy allowed up to three children per couple.

B. Fertility Trends in China

Figure 3 reports TFR series from three sources: the World Bank, the *China Population and Employment Statistics Yearbook*, and the CFPS.[8] From 2011 to 2015, the Yearbook and World Bank series diverged, likely reflecting birth underreporting while the OCP remained in force. Rising internal migration further reduced data accuracy (Wang and Ge, 2013); the 2010 Census recorded underreporting of 15.06%, up from

[6] A widely viewed 1992 Spring Festival Gala skit, "Guerrilla Fighters of Excess Births," depicted a couple evading the OCP restrictions. *Source*: `https://xiqu.cctv.com/2012/12/06/VIDE1354796043534220.shtml`, accessed August 6, 2025.

[7] World Bank data.

[8] The World Bank reports national TFR for women aged 15–49. The Yearbook provides annual age-specific fertility rates derived from Censuses and the Annual National Population Change Sample Survey (approximately 1‰ sample in non-Census years). The CFPS provides birth histories for women in 25 provinces; we construct a retrospective panel to compute annual TFR for 1970–2022.

10.61% in 2000 and 5.81% in 1990 (Cui et al., 2013). Underreporting diminished after the 2016 UTCP as restrictions eased.

Despite these data frictions, the three series align on a three-phase trajectory tied to policy evolution. First, from the early 1970s to the mid-1990s, the TFR fell from roughly 6.0 to 1.5, driven by the LLF and OCP enforcement. Second, from 1995 to 2013, the TFR hovered around 1.5 amid small-scale relaxations. Third, following the 2016 UTCP, the TFR rose from 1.67 in 2015 to 1.82 in 2017 before declining again to 1.18 in 2022.

III. Data

Our main data source is the China Family Panel Studies (CFPS), a nationally representative biennial household longitudinal survey conducted by Peking University since 2010. The CFPS covers 25 provinces and has collected seven waves to date (2010–2022). Each wave surveys all household members and records rich information on socioeconomic status, marital and fertility histories, current residence, and hukou registration.

A. Constructing a Retrospective Couple-Year Panel

We begin by identifying wife–husband pairs in all CFPS waves. Using women's reported birth year, marriage year, and complete fertility histories, we construct a retrospective couple-year panel with annual flow measures of fertility by calendar year. The panel spans 1980–2021; we drop 2022 because the 2022 wave does not cover the full calendar year.

A couple enters the panel in the year of marriage and remains in the panel until the last year observed in the survey.[9] For each couple-year, we observe the stock of children at the start of the year and whether an additional child was born within the year.

Our fertility-flow outcomes are indicators for the nth birth ($n = 1, 2, 3, \ldots$), conditional on having $n - 1$ children at the beginning of year t. The indicator equals one if the nth birth occurs during year t and zero otherwise.[10]

Most existing studies use the stock of children at the survey date as the main outcome (Fang et al., 2024; Guo et al., 2024; Wu, 2022). We instead work with an annualized retrospective panel, which offers several advantages. First, it allows us to isolate births that occur *after* policy changes; counting all children at the survey date conflates births

[9] Divorce and remarriage are extremely rare among the cohort in our sample.

[10] We cannot exclude adopted children born after 2010 because the CFPS does not report adoption status after the 2010 wave. Adopted children account for only 0.42% of all children in our sample in 2010, so their inclusion is unlikely to affect our estimates.

that predate and postdate a policy change. Second, the flow measure provides a common outcome definition for women born in different years, irrespective of whether they have completed childbearing at the survey date. Using the stock of children for women under age 50 mixes younger cohorts, whose fertility is still in progress, with older ones and mechanically understates fertility for younger women. Third, the couple-year structure allows us to embed the complex evolution of OCP relaxations precisely and to track second-child eligibility at the couple–year level. Finally, it enables us to study both short- and longer-run responses to policy changes in a unified framework.

B. Sample Restriction

We impose several sample restrictions before constructing the regression sample.

Ethnicity, return migration, and remarriage. — We exclude ethnic minorities. Under the OCP, couples in which one or both spouses belonged to an ethnic minority were typically allowed to have two children. Minority couples therefore were frequently used as a comparison group in studies of the OCP (Zhang, 2017). Their fertility behaviors, however, likely differ substantially from those of non-minorities. Hence, using them as controls in our setting raises concerns about parallel trends (Guo et al., 2024), especially for analyses of OCP relaxations. We define minority couples as those in which either spouse reports minority status. In our data, 7.8 percent of couples are classified as minorities.[11]

We also drop couples in which either spouse has returned to mainland China from Hong Kong or Taiwan, as such couples were always exempt from the OCP, and remarried couples, who were subject to distinct eligibility rules (García, 2024). These groups are very small in our data (see Table B1).

Regression sample. — Our main analysis focuses on the margin most directly affected by relaxations: the second birth. The regression sample therefore consists of couples with exactly one child.[12] We follow each couple from the year of their first birth until they have a second child or reach the end of the fertility window. The dependent variable is an indicator for having a second birth in calendar year $t + 1$, the year following the eligibility assignment in year t.

We restrict the regression sample to couple-years in which the wife is aged 22–45 and

[11] Formally, the exemption applied only to minority groups with fewer than ten million members. Our broader definition is unlikely to materially affect the eligibility measure. The three largest minorities are Miao, Yi, and Manchu; only the Manchu population is close to ten million, and it accounts for only 1.39% of couples.

[12] We exclude couples whose firstborns are twins.

the husband 22–60 at the start of year t. The lower bound of 22 reflects that, during our study period, individuals typically completed formal education by this age, allowing us to treat education as predetermined. This choice also makes the effect heterogeneity analysis by education easier to interpret.

Our final regression sample contains 8,985 unique couples and 91,268 couple-year observations between 1980 and 2021. Given that OCP enforcement was relatively lax before the mid-1990s, we split the period into 1980–1994 and 1995–2021 and place greater emphasis on the latter. The 1980–1994 period contains 4,832 couples and 27,666 couple-years; the 1995–2021 period contains 6,557 couples and 63,602 couple-years, with some couples appearing in both subperiods.

C. Second-Birth Fertility Rates

Among couples in the regression sample, the average second-birth probability is 0.070 for eligible couple-years and 0.050 for ineligible couple-years (columns (1)–(2) of Table 3). Figure 4 plots the trends in per-year second-birth rates for the two groups. Eligible couples consistently exhibit higher second-birth rates throughout the sample period. For both groups, second-birth fertility declines sharply before 1995, remains relatively stable between 1995 and 2013, then spikes in 2016–2017 following the 2016 UTCP before a downward trajectory.

D. Second-Child Eligibility

As described in Section II.A, OCP relaxations unfolded at different times across provinces and demographic groups, generating multiple sources of quasi-experimental variation in second-child eligibility. Our treatment variable captures this variation through an indicator for couple i's second-child eligibility in year t:

$$Eligible_{it} = \mathbf{\Lambda}_{pt}(Z_{it}), \tag{1}$$

where $\mathbf{\Lambda}_{pt}(\cdot)$ is a policy function mapping couple characteristics Z_{it} into eligibility status according to the family-planning regulations in province p in year t. We take July 1 of year t as the reference date to determine $\mathbf{\Lambda}_{pt}(\cdot)$.

The vector Z_{it} consists of the five key characteristics that jointly determine eligibility: the couple's only-child status, hukou registration (rural vs. urban), the sex of the first child, the number of years since the first birth, and the wife's age. All of these variables can be constructed from the CFPS, as detailed in Appendix C.

García (2024) document 17 criteria under which couples could obtain second-child eligibility without incurring a fine in the early years of the OCP (Table B1). We focus on seven practically relevant criteria: remarriage, return migration from Hong Kong or Taiwan, minority status, only-child status, rural status, sex of the first child, and years since the first birth. We exclude couples that meet any of the first three criteria from the regression sample and use the remaining four, together with the wife's age (a criterion not considered by García (2024)), to construct $Eligible_{it}$. The other ten criteria are extremely rare and, as shown in Table B1, have negligible prevalence in our data: omitting them is unlikely to affect our results.

Figure 5 illustrates the staggered timing of eligibility expansions for rural couples in Zhejiang, Hebei, and Sichuan. We partition couples into six mutually exclusive groups defined by their only-child status (both spouses only children, one spouse an only child, neither spouse an only child) and the sex of the first child (daughter vs. son). The timelines reveal substantial variation in the timing of second-child eligibility both across provinces and across groups. Similar expansions occurred nationwide (Figure B1), culminating in universal eligibility in 2016 under the UTCP. These patterns confirm the staggered nature of OCP relaxations and underlie our identification strategy.

E. Other Explanatory Variables

Our regression models include three sets of control variables. The first comprises eligibility-related characteristics: each couple's only-child status, hukou registration, sex of the first child, years since the first birth, and the wife's age. Although these variables enter the policy function $\mathbf{\Lambda}_{pt}(\cdot)$, we also condition on them directly in the regressions. Hukou registration, years since the first birth, and the wife's age vary over time; the remaining variables are time-invariant.

We determine only-child status using information on the reported number of siblings and the number of children of each spouse's parents. We code only-child status to one if each spouse has no siblings and zero otherwise. We construct an indicator for rural hukou by tracking each spouse's hukou registration across waves, starting from their first appearance in the CFPS. For years prior to the first CFPS interview, we impute hukou using childhood registration records (hukou at birth, age three, and age twelve); when childhood and current registrations coincide, we carry this value back. Because conversion from urban to rural hukou is extremely rare, we treat individuals with urban hukou in childhood as urban throughout. Hukou registration is highly stable after marriage: in our regression sample, no couple changes hukou status over time.

We group years since the first birth into three categories (0–4 years, reference group; 5–10 years; and 11+ years) and the wife's age into two-year bins. The sex of the first child is coded as an indicator equal to one if the first child is a boy and zero otherwise. Finally, although it does not enter the eligibility rule, we include an indicator $1stBirthThisYear_{it}$ for whether the first birth occurs in year t. This accounts for the infrequent but non-negligible incidence of consecutive-year births.

The second set of controls captures socioeconomic status. For each spouse, we include education indicators for college or above, high school, middle school, and primary school or below (reference group). These are treated as time-invariant, as no woman in our sample acquires additional schooling after marriage. We also control for the education of the child's grandparents, constructing separate measures for grandmothers and grandfathers. For each couple, we compute the average years of schooling of the maternal and paternal sides. If grandparental education is missing for both sides, we set this average to zero and include a separate indicator that equals one if grandparental education is missing and zero otherwise. Finally, we include a time-varying indicator for the wife's current urban residence. For years prior to the first CFPS interview, we impute urban residence based on the respondent's reported childhood location and current hukou registration.

The third set contains demographic characteristics that may influence fertility. We include the husband's age (in five-year bins) and the ages of grandmothers and grandfathers, defined as the average of the maternal and paternal sides and grouped into under 60 (reference group), 60–69, and 70+, with separate missing indicators. In addition, we use the CFPS interviewer's assessments of the wife's appearance and intelligence on 1–7 scales as proxies for opportunity costs and intra-household bargaining power. We dichotomize appearance into "normal (reference)" and "exceptionally beautiful" and intelligence into "below average (reference)" and "above average," and include indicators for missing ratings. See Table C1 for complete variable definitions.

Table 3 reports descriptive statistics for the dependent and explanatory variables at the couple-year level. Columns (1)–(2) present statistics by eligibility status: 8,525 ineligible couples (75,820 couple-years) and 2,679 eligible couples (15,448 couple-years). Eligible couples are more likely to hold rural hukou, to be only children, to have a firstborn daughter, to be older, and to have spent more years since their first birth; they are also more likely to reside in rural areas. The distributions of wives' and husbands' education are broadly similar across the two groups: about 70% of individuals in both groups have at most middle-school education. Grandparents of eligible couples are more likely to have above-average schooling (47.3% of grandmothers and 44.5% of grandfathers) than those of ineligible couples (31.8% and 33.5%), likely reflecting selection induced by earlier TCPs. Grandparental ages are similar across groups. Wives in eligible couples are more likely to be rated as exceptionally beautiful (38.2% vs. 31.2%) and to have above-average intelligence (37.3% vs. 30.3%).

Columns (3)–(4) of Table 3 summarize characteristics by second-birth status: 86,431 couple-years without a second birth and 4,837 couple-years in which a second birth occurs. Among second-birth observations, 4.8% correspond to couples whose first child is born in the same calendar year, confirming that closely spaced births, although uncommon, do occur. Couples who have a second child are more likely to hold rural hukou, to have siblings, to have a firstborn daughter, to be younger, and to reside in rural areas. Educational attainment is lower among couples with a second child: 58.2% of wives and 41.2% of husbands have primary education or less, compared with 38.7% and 30.3% among those who do not.[13] Their grandparents also tend to be less educated (64.3% vs. 59.9% of grandmothers and 68.0% vs. 59.8% of grandfathers have below-average education) and younger on average (56.2 vs. 60.9 years for grandmothers and 58.9 vs. 63.6 years for grandfathers). Finally, wives perceived as exceptionally beautiful or above-average in intelligence are less likely to have a second child: among couples with a second birth, 23.1% of wives are rated beautiful and 21.5% above-average in intelligence, compared with 32.9% and 32.0% among those without a second birth.

IV. Empirical Strategy

Our first goal is to estimate how OCP relaxations affected the annual probability of having a second birth. For expositional clarity, we begin with a stylized two-way fixed effects (TWFE) model that exploits only cross-provincial variation in OCP relaxations:

$$2ndBirth_{i,p,t+1} = \alpha + \beta_1 Eligible_{pt} + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \phi_p + \lambda_t + \omega_p t + \epsilon_{ipt}, \tag{2}$$

where $2ndBirth_{i,p,t+1}$ is an indicator for whether couple i in registered province p has a second child in year $t+1$. The variable $Eligible_{pt}$ is a province-year indicator equal to one if couples in province p are allowed a second child in year t under the prevailing family-planning regulations. The vector $\boldsymbol{X}_{it}$ collects control variables, ϕ_p and λ_t denote

[13] Figure C1 displays the full distribution of education by second-birth status.

province and year fixed effects, respectively, and $\omega_p t$ is a province-specific linear time trend. The error term is ϵ_{ipt}, and α, β, and γ are parameters to be estimated, with β being the coefficient of interest. Standard errors would typically be clustered at the province level.

This specification is standard in settings where policy changes roll out across provinces at different times. In our context, however, OCP relaxations determine eligibility at the couple level through detailed exemption rules that depend on individual circumstances, such as the sex of the first child and the wife's age, and these rules differ across provinces and over time. As a result, $Eligible_{pt}$ is a coarse proxy for the couple-level treatment status, $Eligible_{ipt}$, and introduces non-classical measurement error in the treatment variable. This can attenuate β toward zero and, if the extent of measurement changes over time, may also undermine the province-level parallel-trends assumption.

A. From Province-Level to Cell-Level Treatment Variation

To address this, we exploit the fact that couple i's second-child eligibility in year t is deterministically given by the policy rule and a low-dimensional set of characteristics:

$$Eligible_{ipt} = \mathbf{\Lambda}_{pt}(Z_{it}),$$

where $\mathbf{\Lambda}_{pt}(\cdot)$ denotes the exemption rules in province p as of July 1 of year t, and Z_{it} is the vector of couple characteristics that enter those rules.

We discretize Z_{it} into cells defined on a grid:

$$\{p \times d \times b \times c\},$$

where p indexes province, b is the first child's birth year, and c is the wife's birth cohort. The index d runs over 24 mutually exclusive demographic groups defined by the interaction of three characteristics: (i) spouses' only-child status (both spouses only children, either spouse an only child, neither), (ii) hukou status (both rural, wife only, husband only, neither), and (iii) the sex of the first child (daughter vs. son). The first child's birth year, b, and the wife's birth cohort, c, are needed because many provinces imposed minimum birth-interval and/or maternal-age requirements, so eligibility in year t depends on the ages of the first child and the mother.

Given the province-year rule $\mathbf{\Lambda}_{pt}(\cdot)$, the cell $Z_{pdbc,t}$ contains (approximately) all information relevant for determining the couple-level treatment status, $Eligible_{ipt}$. Our extended TWFE model can therefore be written as:

$$\begin{aligned} 2ndBirth_{i,pdbc,t+1} &= \alpha + \beta_1 Eligible_{ipt} + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \phi_{pdbc} + \lambda_t + \omega_p t + \epsilon_{i,pdbc,t} \\ &= \alpha + \beta_1 \boldsymbol{\Lambda}_{pt}(Z_{pdbc,t}) + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \phi_{pdbc} + \lambda_t + \omega_p t + \epsilon_{i,pdbc,t}, \end{aligned} \tag{3}$$

where ϕ_{pdbc} denotes fixed effects for the (p, d, b, c) cell, replacing the province fixed effects in Equation (2), and standard errors are clustered at the $p \times d \times b \times c$ level. In this formulation, the treatment variable is measured precisely at the couple level, and the rich set of cell fixed effects absorbs composition shifts across groups, making the parallel-trends assumption more plausible.

In practice, the full $\{p \times d \times b \times c\}$ grid generates an extremely large number of cells, many of which contain few or no observations, rendering estimation inefficient or infeasible. To implement Equation (3), we therefore construct a parsimonious version of the grid that retains information relevant for eligibility while economizing on degrees of freedom. In brief, we (i) collapse demographic groups d with identical eligibility patterns across all provinces and years (Table D1), (ii) collapse first-birth years b in provinces without binding birth-interval rules, (iii) collapse subsets of b after birth-interval rules are abolished, and (iv) analogously collapse subsets of wife's birth cohorts c in provinces without maternal-age rules or for cohorts no longer subject to such rules. Appendix Section D.D1 provides full details.

B. Accounting for Closely Spaced Births

We further refine Equation (3) to account for the low probability of having a second birth in the calendar year immediately following the first birth. Although two births in consecutive years are uncommon, they are not extremely rare in our data, and ignoring this feature could bias the estimated treatment effect if the policy response differs for couples who very recently had their first birth. To capture this, we add an indicator $1stBirthThisYear_{it}$ for whether couple i has its first birth in year t, along with its interaction with eligibility. Our baseline specification is:

$$\begin{aligned} 2ndBirth_{i,pdbc,t+1} = \alpha &+ \beta_1 \boldsymbol{\Lambda}_{pt}(Z_{pdbc,t}) + \beta_2 1stBirthThisYear_{it} \\ &+ \beta_3 \left[\boldsymbol{\Lambda}_{pt}(Z_{pdbc,t}) \times 1stBirthThisYear_{it}\right] \\ &+ \boldsymbol{X}_{it}\boldsymbol{\gamma} + \phi_{pdbc} + \lambda_t + \omega_p t + \epsilon_{i,pdbc,t}, \end{aligned} \tag{4}$$

where β_2 captures how the second-child probability of couples who have just had their first child differs from that of other couples, while β_3 allows the eligibility effect to vary for these couples. The treatment effect for couples whose first birth did not occur in year t is given by β_1, and for couples with a first birth in year t by $\beta_1 + \beta_3$.

C. Event-Study Specification

To test the parallel-trends assumption and study the dynamics of treatment effect, we estimate an event-study version of the TWFE model. Let $D_{k(it)}$ be an indicator equal to one if couple i in year t is k years away from its first year of second-child eligibility, where $k \in \{-10, -9, \ldots, 10\}$, and zero otherwise. We define $k = 0$ as the first year in which the couple becomes eligible. The event-study specification is:

$$2ndBirth_{i,pdbc,t+1} = \alpha + \sum_{k=-10}^{10} \beta_k D_{k(it)} + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \phi_{pdbc} + \lambda_t + \epsilon_{i,pdbc,t}, \tag{5}$$

where we omit $k = -1$ so that $\beta_{-1} = 0$ serves as the reference period (the year just before eligibility is first granted). We bin all event times $k < -10$ into $k = -10$ and all $k > 10$ into $k = 10$ to ensure sufficient observations for each coefficient. Couples whose eligibility status later reverts to ineligible due to changes in exemption rules (i.e., in Gansu, Henan, and Hubei; see Table 1) are excluded from the event-study analysis.

We plot the estimated β_k coefficients to examine whether pre-treatment trends are flat (supporting the parallel-trends assumption) and whether post-treatment effects are short-lived or persistent over the subsequent decade.

D. Exogeneity of Policy Rollout

Our identification strategy assumes that, conditional on observables and fixed effects, the timing of OCP relaxations is not driven by unobserved factors that are also correlated with fertility trends. One concern is that provinces may have adjusted family-planning rules in response to local demographic or economic conditions.

To address this, we compile a balanced provincial panel for 1980–2012 and regress indicators for the adoption of two early relaxations—the STCP (both) for couples in which both spouses are only children, and the One-and-a-Half-Child Policy for rural couples with a firstborn daughter—on lagged province covariates, including GDP per capita, the shares of primary and tertiary sectors in GDP, fiscal expenditure per capita, crude birth and death rates, total population, the rural population share, and the female population share. The dependent variable is a binary indicator that switches from 0 to 1 in the year a relaxation is introduced and remains one thereafter. As reported in Appendix Table D2, these covariates do not significantly predict adoption, and joint tests fail to reject the null of no association. This evidence supports the view that the timing of relaxations was primarily driven by top-down policy decisions rather than local socioeconomic conditions.

V. Results

A. Baseline Results

Table 4 reports estimates of Equation (4) for the full 1980–2021 sample (columns 1–3) and for the two subsamples 1980–1994 (columns 4–5) and 1995–2021 (columns 6–7). The sample consists of couples with exactly one child in which the wife is aged 22–45 and the husband 22–60 at the start of year t.

Column 1 controls for the wife's age, *pdbc* fixed effects (province $\times$ demographic group $\times$ first-birth year $\times$ mother's birth cohort), and year fixed effects. Column 2 adds the full set of covariates described in Section III.E, and column 3 further includes province-specific linear time trends. The subperiod specifications follow the same structure, with and without province-specific trends.

Across all specifications and periods, the coefficient on *Eligible* is positive and statistically significant at the 1% level. In our preferred specification with the full set of controls and province-specific trends (column 3), second-child eligibility raises the annual second-birth probability by 7.2 percentage points. Given a sample mean of 5.3%, this represents a 1.4-fold increase. The effect is larger in the earlier period—9.1 percentage points in 1980–1994 (column 5)—than in the later period, where it is 7.1 percentage points in 1995–2021 (column 7). This pattern is consistent with higher baseline fertility and stronger responses to policy relaxations in the earlier decades.

The coefficient on $1stBirthThisYear$ is negative and precisely estimated in all columns: couples who had their first child in year t are much less likely to have a second birth in year $t+1$. The interaction between *Eligible* and $1stBirthThisYear$ is close to zero for 1980-1994 but becomes negative and statistically significant in the later period, indicating that recently delivered women respond less to eligibility expansions than women whose first birth occurred earlier. Evaluated at the means, the effective treatment effect for couples with a very recent first birth, $\beta_1 + \beta_3$, remains positive but is smaller than β_1 for other couples.

To gauge the importance of using detailed eligibility rules and staggered variation, Appendix Section D.D3 contrasts our baseline estimates with two simpler designs frequently in the literature: (i) before–after OLS comparisons around the 2014 and 2016 nationwide relaxations, and (ii) a difference-in-differences (DID) strategy that defines treatment solely by spouses' only-child status.[14] Both approaches yield weaker and unstable results that are highly sensitive to the sample window, consistent with substantial treatment misclassification when eligibility is not constructed from the full policy rules. We therefore focus on the cell-based TWFE estimates in Table 4 as our preferred estimates.

B. Event-Study Evidence

We next examine the dynamics of fertility responses using the event-study specification in Equation (5). Figure 6 plots the estimated coefficients β_k for 1980–1994 (Panel A) and 1995–2021 (Panel B). Event time k is defined relative to the first year in which a couple becomes eligible for a second child, either because of an OCP relaxation or because the couple satisfies binding birth-interval or maternal-age requirements. We normalize the year immediately prior to eligibility ($k = -1$) to zero, so all coefficients are interpreted relative to that reference period.

Two main patterns emerge. First, pre-treatment coefficients are all statistically indistinguishable from zero for both periods, supporting the parallel-trends assumption. Second, once eligibility is granted, second-birth probabilities rise and remain elevated for at least a decade. In both panels, the effect stabilizes at around 7–9 percentage points, closely matching the average effects in Table 4. There is no sharp, short-lived spike confined to the first one or two years after eligibility. Instead, the response builds and then persists, indicating that the relaxations induced sustained additional second births.

C. Robustness

Appendix Section E reports a wide range of robustness checks; here we summarize the main findings.

First, we compare our cell-based TWFE estimates with stylized models that use alternative sources of variation. A province-level specification that exploits only cross-provincial differences in relaxations, as in Equation 2, and an individual (couple) fixed-effects specification that relies solely on within-couple variation, both produce qualitatively similar but smaller effects than our baseline (Appendix Table E1). This pattern is consistent with attenuation bias when treatment is misclassified, highlighting the value

[14] See Appendix Tables D3 and D4 for full results.

of our accurately defined *pdbc* cells.

Second, we show that the results are robust to alternative ways of defining eligibility and to different sample restrictions. Using implementation rather than announcement dates to assign eligibility yields estimates that are virtually identical to the baseline ones (Table E3). Restricting the sample to the post-2000 period—when OCP enforcement and migrant monitoring were tightened—leads to somewhat smaller but still sizable effects, with no evidence of differential pre-trends (Table E4 and Figure E1). Varying the age range of wives, excluding couples with cross-province marriages, excluding couples whose hukou changed, and dropping inter-provincial migrants all leave the main estimates largely unchanged (Appendix Table E5).

Third, we address concerns that standard TWFE estimators can be biased when treatment effects are heterogeneous across groups or over time. We re-estimate event-study specifications using the alternative TWFE estimators proposed by De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), and Borusyak et al. (2024). Despite differences in how these estimators construct counterfactuals and in their feasible event-time support, the resulting dynamic patterns closely track our TWFE estimates (Figure E2). This suggests that our main conclusions are not driven by bias from heterogeneous treatment effects.

Finally, as discussed earlier, Appendix Section D.D3 shows that when we mimic simpler before–after OLS and sibling-based DID designs used in prior work, we obtain much weaker and sometimes insignificant effects. Together with the robustness checks above, this reinforces the importance of reconstructing eligibility at the couple-year level using the full set of policy rules and exploiting the rich staggered variation in relaxations.

VI. Heterogeneous Responses to Eligibility Expansions

This section examines which couples and which local environments drive the fertility response to OCP relaxations. We study heterogeneity along three dimensions: (i) the type of eligibility expansion, distinguishing early exemptions from later nationwide reforms; (ii) couples' demographic characteristics that proxy for preferences and opportunity costs; and (iii) regional economic and cultural conditions that shape the returns to children and the constraints faced by women. Throughout, we estimate variants of Equation (4) that allow the eligibility effect to differ across groups, holding fixed the baseline controls, *pdbc* fixed effects, and year fixed effects (and, where indicated, province-specific linear time trends). Standard errors are clustered at the $p \times d \times b \times c$ level. Full regression results are reported in Appendix Tables E6, E7, and E8.

A. Heterogeneity by Relaxation Type

We begin by asking whether the fertility response differs between early, targeted exemptions and later, nationwide relaxations. Table 5 decomposes *Eligible* into three mutually exclusive sources of second-child eligibility: (i) early rural exemptions (most prominently the One-and-a-Half-Child Policy for rural couples with a firstborn daughter); (ii) early non-rural exemptions (such as the STCP (both) for urban couples in which both spouses were only children); and (iii) recent relaxations that pool eligibility granted under the 2014 STCP (either) and the 2016 UTCP.

Two patterns stand out. First, early rural exemptions generate the largest fertility response. These exemptions applied precisely to groups with both a high latent demand for a second child and relatively low marginal costs of childbearing—rural households, often facing weaker formal old-age insurance and stronger son preference, for whom a second birth was a salient margin even under tight policy constraints. When these constraints were relaxed, the policy released substantial pent-up demand, resulting in comparatively large increases in second births.

Second, the estimated effects for the recent nationwide relaxations are materially smaller. This attenuation is consistent with a shift in the binding constraint over time. By the 2010s, many couples—especially urban, higher-SES households—appear to be constrained less by legal eligibility and more by economic and career-related costs of childbearing. In that environment, removing a quantity restriction can still raise fertility, but the marginal impact is mechanically limited when desired fertility has already fallen and when the opportunity costs of additional childbearing are high.

Appendix Table E6 provides a complementary decomposition based on sibling-oriented eligibility rules: STCP (both), the One-and-a-Half-Child rural exemption (firstborn daughter), STCP (either), and the UTCP. The same conclusion emerges: the aggregate response is disproportionately driven by the rural exemption for couples with a firstborn daughter, whereas the marginal effects for the later, broader reforms are more modest. Taken together, these results suggest that the headline debate that focuses narrowly on the 2016 reform misses an important fact: the fertility impact of relaxing the OCP is largest precisely in the settings where the restriction most tightly bound historically and where underlying preferences for a second child remained strong.

B. Heterogeneity by Demographic Characteristics

We next examine heterogeneity across couples. We allow the eligibility effect to vary by seven characteristics: the wife's and husband's education (four categories), the wife's hukou registration (rural vs. urban), the wife's age (22–29, 30–34, 35–45), interviewer-rated appearance and intelligence, and the sex of the first child. We estimate models separately for 1980–1994 and 1995–2021 to allow both the baseline second-birth probability and the policy environment to differ across periods.

Panel A of Figure 7 presents the results; Appendix Table E7 reports the corresponding estimates. Several robust patterns are worth emphasizing.

First, the eligibility effect is larger among lower-SES couples—those with less schooling and those holding rural hukou. This pattern aligns with a standard opportunity-cost interpretation: when market returns to women's time are lower and when formal insurance is weaker, the marginal utility of an additional child is higher and the shadow price of childbearing is lower. The rural–urban gradient is also consistent with institutional differences embedded in China's hukou system, which shape access to jobs, benefits, and childcare resources (Guo et al., 2025; Song, 2014). While rural households typically face tighter resource constraints, they may also face stronger incentives to rely on children as old-age security and to conform to traditional family norms, amplifying the fertility response once eligibility constraints are relaxed.

Second, the eligibility effect is stronger for wives aged 30–34. This age profile is consistent with two forces operating simultaneously: biological fecundity and policy-imposed spacing/age requirements. Younger wives are less likely to realize an immediate second birth even when eligible, particularly in periods and provinces where minimum birth-interval rules bind; at older ages, declines in fecundity and heightened career/family trade-offs may compress the feasible response window. The peak response at 30–34, therefore, suggests that eligibility expansions primarily accelerate or enable second births among couples who are close to, but not beyond, the biologically and institutionally relevant margin.

Third, the results for interviewer-rated appearance and intelligence are suggestive of heterogeneity in opportunity costs and bargaining power. Wives rated as exceptionally beautiful or above average in intelligence exhibit smaller fertility responses to eligibility expansions. These measures are imperfect proxies, but they plausibly correlate with labor-market prospects and the returns to career investment, which raise the cost of additional childbearing (Kanazawa, 2014; Zhang et al., 2023). In this sense, the heterogeneity pattern is consistent with the broader education gradient: eligibility expansions have their largest effects where the policy constraint is most likely to be binding relative to economic constraints.

Finally, and most strikingly, the response is substantially stronger when the first child is a girl. This pattern points to persistent son preference in China (Basu and De Jong, 2010; Ebenstein, 2010; García, 2024). Eligibility expansions relax a key constraint for couples seeking a son, increasing the probability that they continue childbearing after a firstborn daughter. The fact that this gradient remains pronounced even in the later period underscores that son preference and related norms remain an important driver of second-birth behavior, even as overall desired fertility falls.

C. Heterogeneity by Regional Characteristics

We then turn to the local environment. We study seven regional dimensions: current rural versus urban residence, the sex ratio at birth, female labor force participation (FLFP), the gender wage gap, gender-role attitudes about the division of labor within the household, male housework share, and historical OCP stringency. These measures are constructed at the city level (or at the province level for fines), as explained in detail in Appendix Section E.E7. We classify each measure into terciles.

Panel B of Figure 7 summarizes the results, with full estimates reported in Appendix Table E8. The regional patterns sharpen the interpretation of the couple-level gradients.

First, fertility responses are stronger in areas with higher sex ratios at birth. This finding reinforces the role of son preference: where male-biased fertility behavior is stronger, eligibility expansions are more likely to translate into additional second births as couples continue childbearing after a firstborn daughter.

Second, responses are systematically larger in less egalitarian environments—regions with low FLFP, wider gender wage gaps, and more traditional gender-role attitudes. These patterns are consistent with a mechanism in which eligibility expansions raise fertility most where women face stronger norms to specialize in home production and where the private cost of childbearing (in foregone earnings) is lower or less salient. Conversely, in more gender-equal settings, the same eligibility expansion may bind less tightly because the primary constraint is not policy eligibility but the high opportunity cost of additional childbearing and the difficulty of reconciling work and family (Arpino et al., 2015; Blau et al., 2020; Myong et al., 2021). In this sense, regional heterogeneity mirrors the education and beauty/intelligence gradients: the policy is most effective where the legal constraint is more important than the economic constraint.

Finally, we find smaller fertility responses in provinces with historically higher OCP fines. One interpretation is that stringent OCP enforcement reshaped preferences and norms over time, inducing persistent low-fertility behavior even after formal constraints were relaxed (Guo et al., 2024; Rossi and Xiao, 2024). This persistence is important for understanding why the post-2016 fertility decline can coexist with positive causal effects of relaxations: eligibility expansions do raise second births, but their aggregate impact is muted in places where enforcement was strongest and where low-fertility norms have become entrenched.

These patterns suggest that the fertility gains from relaxing parity restrictions are state-dependent: they are large when legal constraints bind against still-high desired fertility, but muted once desired fertility has fallen and constraints shift to the work–family margin. Consistent with this view, eligibility expansions have their largest effects among groups and regions where demand for a second child remains strong—notably after a firstborn daughter—and where the opportunity costs of childbearing are lower, while effects are smaller in urban, high-opportunity-cost environments. This heterogeneity helps reconcile a persistent positive micro effect with the renewed post-2016 decline in aggregate fertility.

VII. How OCP Relaxations Reshaped Fertility Trends

We now translate the micro-level eligibility effect into aggregate fertility dynamics. Using Census-based population counts and CFPS-based parity-specific birth probabilities, we simulate annual births and total fertility rates (TFR) under (i) the observed sequence of relaxations and (ii) a counterfactual in which no relaxations occur after 1990.

A. Simulation Design

The key challenge is that relaxations affect fertility primarily at the second-birth margin, while also altering the parity composition of women in subsequent years. Standard TFR calculations require only age-specific fertility rates in each year and do not track parity stocks. Our counterfactual therefore simulates, for each age–province–hukou cell, the number of women at each parity and the births generated at each parity.

The simulation proceeds in three steps (Appendix F provides full details). First, we construct annual counts of women aged 15–45 by age, province, and hukou status for 1982–2021 from Census and Mini-Census microdata, and we recover the parity distribution in 1982. Second, we estimate parity-specific birth probabilities using the CFPS. For second births, we use our preferred TWFE specification for one-child couples (Equation 4) to recover the causal effect of second-child eligibility; for the no-relaxation counterfactual, we set post-1990 eligibility expansions to zero when forming predicted second-birth rates. For first births and third-or-higher births, we estimate simpler models because eligibility is irrelevant at these parities (Table F1).

Third, we forward-simulate parity stocks year by year, using births to transition women from parity k in year t to parity $k+1$ in year $t+1$, while scaling the implied parity distribution to match the Census-based totals in each age–province–hukou cell. We compute parity-specific fertility rates and aggregate them into a TFR series.

Figure F3 shows that the simulated baseline TFR closely matches both the CFPS-based and World Bank series, supporting the reliability of the simulation.

B. Counterfactual Fertility Trends

Figure 8 compares the baseline simulated fertility path (consistent with the observed sequence of OCP relaxations since 1990) with a counterfactual in which post-1990 eligibility expansions are shut down.

Parity-specific dynamics. — Panel A plots parity-specific fertility rates. First-birth fertility declines persistently over 1990–2021, while third-and-higher-order fertility remains low throughout, consistent with binding constraints and low demand at higher parities. Against this backdrop, second-birth fertility accounts for most medium-run variation in overall fertility: the baseline second-birth rate falls sharply in the 1990s, rises gradually after 2000, and increases markedly around the mid-2010s before declining again after 2017. The counterfactual series makes clear that a substantial share of the post-2000 increase—and virtually all of the mid-2010s surge—reflects expanded eligibility rather than a broad-based recovery in desired family size. Importantly, this aggregate pattern need not mirror the time profile of the marginal treatment effect in Table 4. The TWFE coefficient captures an intensive-margin behavioral response among those affected, whereas the aggregate series also reflects extensive-margin coverage: following the 2014 reform, a large cohort of previously ineligible one-child couples became newly eligible (Appendix Figure F4), mechanically amplifying the aggregate impact even if the marginal effect on individual behavior is smaller.

Implications for TFR. — Panel B translates these parity dynamics into total fertility. The baseline and counterfactual TFR series are close in the 1990s, but the gap widens over time and becomes especially pronounced after 2014. By 2017, the baseline TFR reaches its highest level since the mid-1990s, indicating that the nationwide relaxations generated a meaningful upward *level shift* in fertility. Yet the baseline path resumes a sharp decline immediately after 2017. The key implication is that the relaxations acted as a quantitatively important buffer, raising the level of the fertility path, without overturning the underlying downward trajectory.

Urban–rural patterns. — Panels C and D show differences in these aggregate effects between urban and rural. Early relaxations primarily loosened constraints in rural areas, producing a clear rural baseline–counterfactual gap while leaving the urban fertility trend nearly unchanged through the mid-2000s. After the nationwide relaxations, both groups diverge, with a particularly pronounced urban gap. This pattern is consistent with the idea that the final, nationwide relaxations released a substantial amount of previously constrained second-birth demand among urban couples, yet the post-2017 decline underscores how quickly aggregate fertility can resume falling when opportunity costs and preferences continue to shift.

Panel D formalizes the compositional shift in where the aggregate effect comes from. Before 2014, rural couples account for the overwhelming majority of the baseline–counterfactual TFR gap. After the 2014 relaxation, the urban contribution dominates and peaks during 2015–2017, consistent with a sequential release of constrained fertility: earlier exemptions first affected rural couples, whereas the final nationwide relaxations made urban couples the primary drivers of the temporary boost. The subsequent decline in the urban share by 2021 highlights the central tension of the post-2016 period: eligibility expansions can retain a positive micro-level effect for those still at the margin, while the macro trajectory continues to fall as the pool of responsive households shrinks and desired fertility declines.

Births implied by the TFR gap. — Finally, the simulations imply sizable demographic effects in levels: comparing baseline and counterfactual births indicates that post-1990 relaxations added roughly 2.3–5.3 million births per year in the post-2014 period (Appendix Figure F5). Taken together, Figure 8 clarifies why a positive causal effect of eligibility expansions can coexist with a renewed macro-level fertility decline: relaxations raise second-birth fertility and shift the level of the fertility path upward, but they do not reverse the deeper forces driving the downward trend.

VIII. Conclusion

This paper explores how the gradual dismantling of China's One-Child Policy (OCP) reshaped fertility behavior and aggregate fertility trends. Motivated by the post-2016 debate—a brief rebound followed by a renewed decline—we argue that the relevant object is not whether fertility remained high after the Universal Two-Child Policy (UTCP), but whether the sequence of relaxations generated a persistent increase in second-birth fertility relative to a counterfactual path in which eligibility had not expanded.

Our empirical approach combines nationally representative microdata from the China Family Panel Studies with detailed province-by-year histories of exemption rules and related eligibility constraints. By reconstructing couple-year second-child eligibility based on only-child status, hukou registration, the sex of the first child, binding birth-spacing rules, and maternal-age requirements, we exploit staggered expansions in eligibility and estimate cell-based two-way fixed effects and event-study specifications in a retrospective couple-year panel spanning 1980–2021. This design addresses a central limitation of recent work, which often proxies exposure using only the 2014/2016 nationwide reforms and spouses' only-child status and therefore misclassifies treatment for couples already eligible under earlier exemptions.

Three conclusions emerge. First, OCP relaxations materially increase second births, and the effect is persistent. In our preferred specification, second-child eligibility raises a couple's annual probability of a second birth by 7.1 percentage points, with effects that persist for at least a decade. Event-study estimates show no evidence of a sharp, short-lived spike confined to the first year or two after eligibility is granted. Instead, second-birth probabilities rise and stay higher, consistent with a sustained increase in completed parity-two fertility rather than a purely transitory timing response.

Second, the effects are heterogeneous in ways that illuminate when parity restrictions bind. Responses are larger among rural and lower-socioeconomic-status households and, most strikingly, among couples whose first child is a daughter. Regionally, eligibility expansions generate larger fertility responses in environments characterized by higher sex ratios at birth, wider gender wage gaps, and more traditional gender norms. Taken together, these patterns suggest that parity restrictions interact with underlying preferences and gender norms—including persistent son preference—and that the effectiveness of relaxing such restrictions diminishes as the binding constraint shifts from legal eligibility to the work–family margin and the opportunity costs of additional childbearing.

Third, the aggregate implications are economically meaningful but do not amount to a reversal of China's long-run fertility decline. Using Census-based population counts and CFPS-based parity-specific birth probabilities, we simulate fertility dynamics under the observed sequence of relaxations and compare them to a counterfactual path in which post-1990 eligibility expansions are shut down. The simulations show that second-birth fertility accounts for most medium-run variation in overall fertility and that a substantial share of the post-2000 increase—and virtually all of the mid-2010s surge—is attributable to expanded eligibility. The baseline and counterfactual TFR series diverge sharply after 2014, indicating a meaningful upward level shift generated by the nationwide relaxations. At the same time, fertility resumes a steep downward trajectory after 2017, underscoring that the relaxations acted as a buffer that raised the level of the fertility path without overturning its downward slope. In levels, the implied number of additional births attributable to post-1990 relaxations is sizable in the post-2014 period, on the order of several million births per year.

These findings help reconcile a central empirical tension in the post-2016 debate. A policy can generate a persistent causal effect and a persistent upward shift in the fertility path, yet the observed aggregate series can continue to fall when deeper determinants of fertility—urbanization, rising education and earnings opportunities for women, and constraints in the work–family balance—continue to move against childbearing. More broadly, our results underscore the importance of distinguishing between (i) a temporary post-reform spike, (ii) a persistent upward level shift, and (iii) a genuine reversal of the underlying downward trend. Evaluations that focus narrowly on a short post-2016 window risk conflating these distinct objects and understating the cumulative role of earlier, staggered relaxations.

For policy, the Chinese experience suggests that removing parity restrictions can raise fertility where such restrictions remain binding, but its aggregate capacity is limited once desired fertility has fallen and opportunity costs become the dominant constraint. This logic cautions against viewing the Three-Child Policy as a sufficient instrument for restoring fertility to replacement levels. More generally, countries contemplating transitions away from restrictive fertility regimes should expect heterogeneous and potentially short-lived aggregate gains unless complementary policies relax structural constraints—

particularly those governing childcare availability, the compatibility of work and family, housing costs, and gender inequality in both labor markets and household production.

Our analysis has limitations that point to directions for future research. First, while we document persistence over a decade, policy changes may shape fertility norms and preferences over longer horizons (Guo et al., 2024); identifying such cultural and intergenerational channels remains an important task. Second, our design focuses on the second-birth margin, the principal channel through which OCP relaxations operated, but broader general-equilibrium responses—including marriage timing, migration, and labor-market adjustments—may also contribute to long-run demographic outcomes. Finally, understanding how quantity-based policies interact with emerging price-based pronatalist policies and with reforms that directly target gendered opportunity costs is a key frontier for evaluating the effectiveness of contemporary fertility policy.

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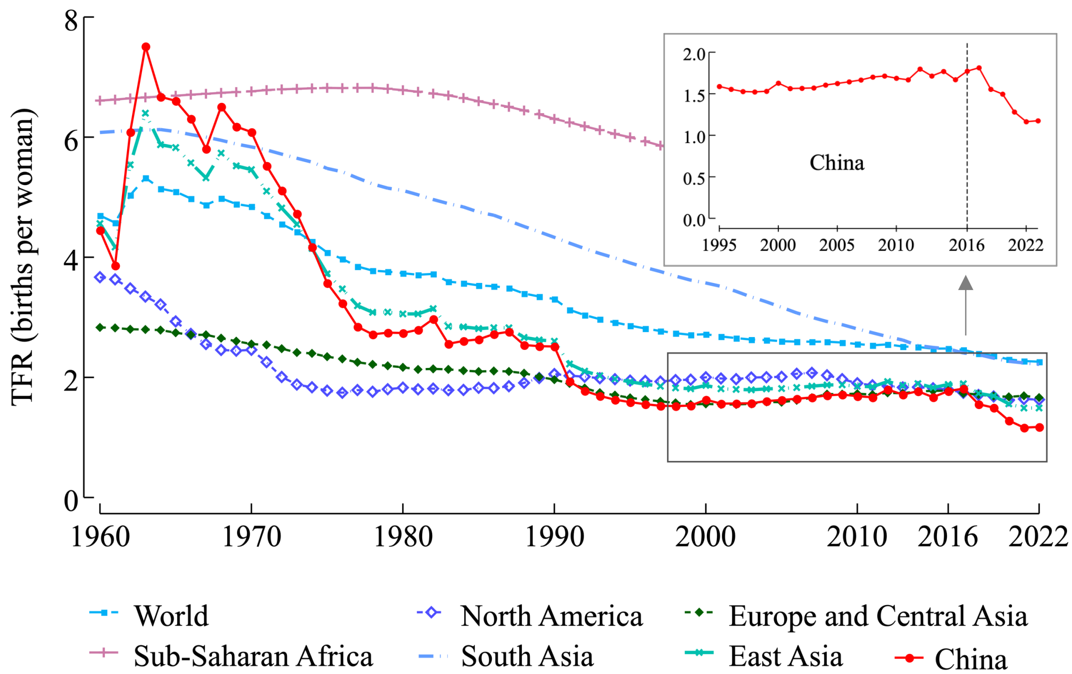
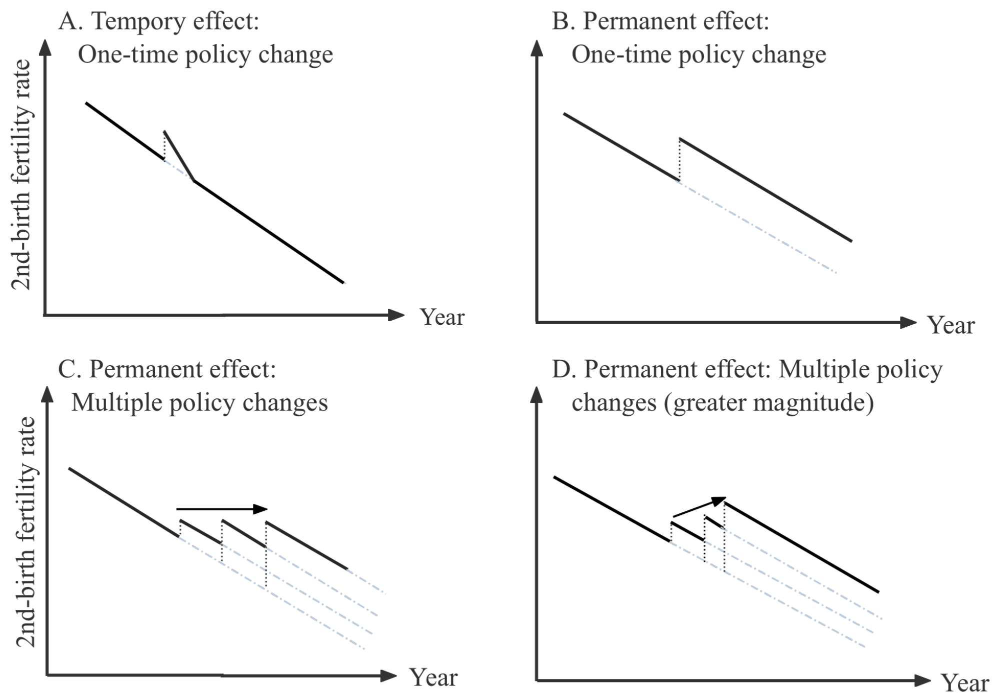
Figures and Tables

Figure 1 : Global Total Fertility Rate, 1960–2022



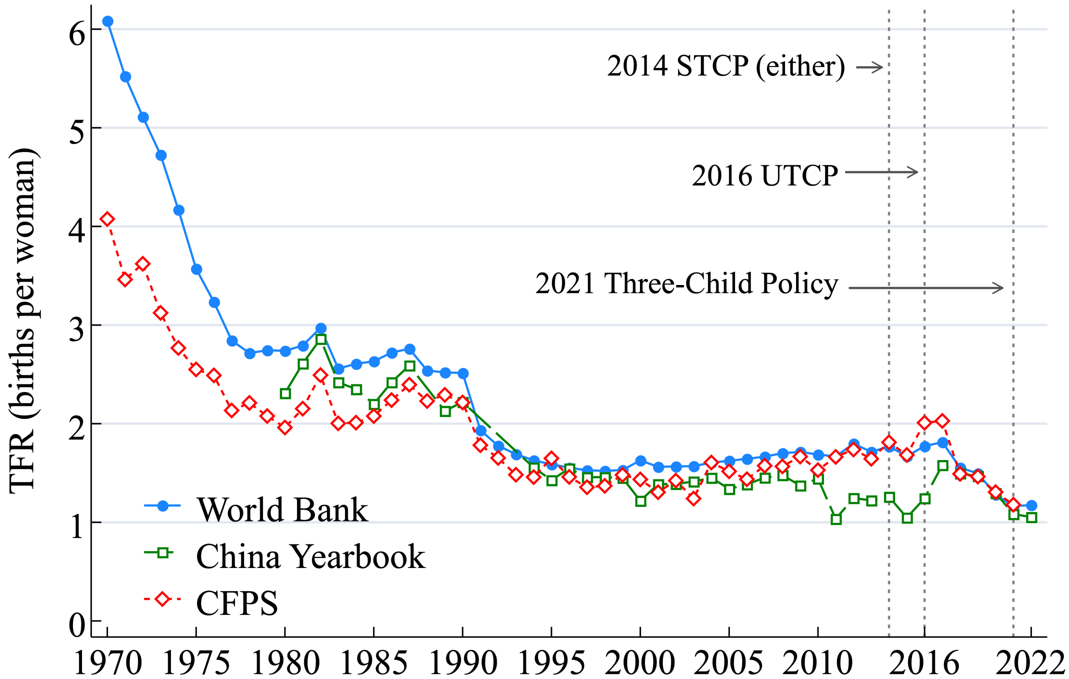
Note: The figure shows total fertility rate (TFR) trends from global and regional perspectives (births per woman aged 15–49), with China highlighted by a solid red line. Regional series are distinguished by unique line patterns and markers.
Data sources: World Development Indicators (https://data.worldbank.org/indicator/SP.DYN.TFRT.IN); authors' compilation.

Figure 2 : Stylized Second-Birth Responses to OCP Relaxations



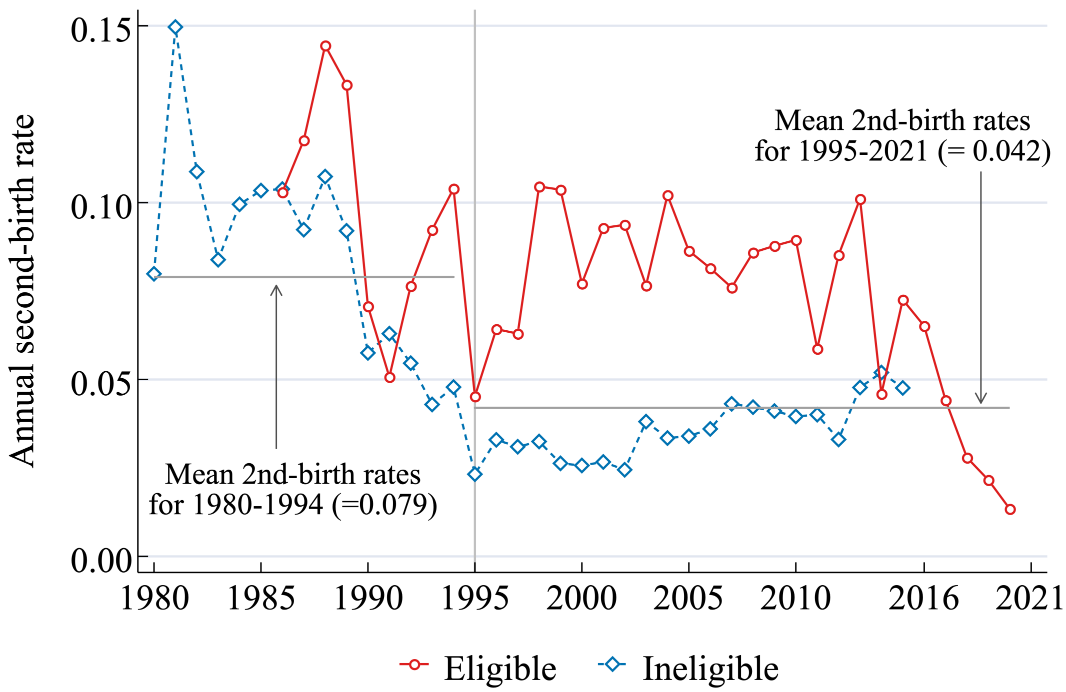
Note: Panel A shows a temporary spike with reversion to the pre-policy trend. Panel B shows a persistent parallel upward shift. Panel C shows temporary stabilization under successive relaxations. Panel D shows incremental growth when relaxations intensify over time. Stylized illustration; not to scale.

Figure 3 : China's Total Fertility Rate, 1970–2022



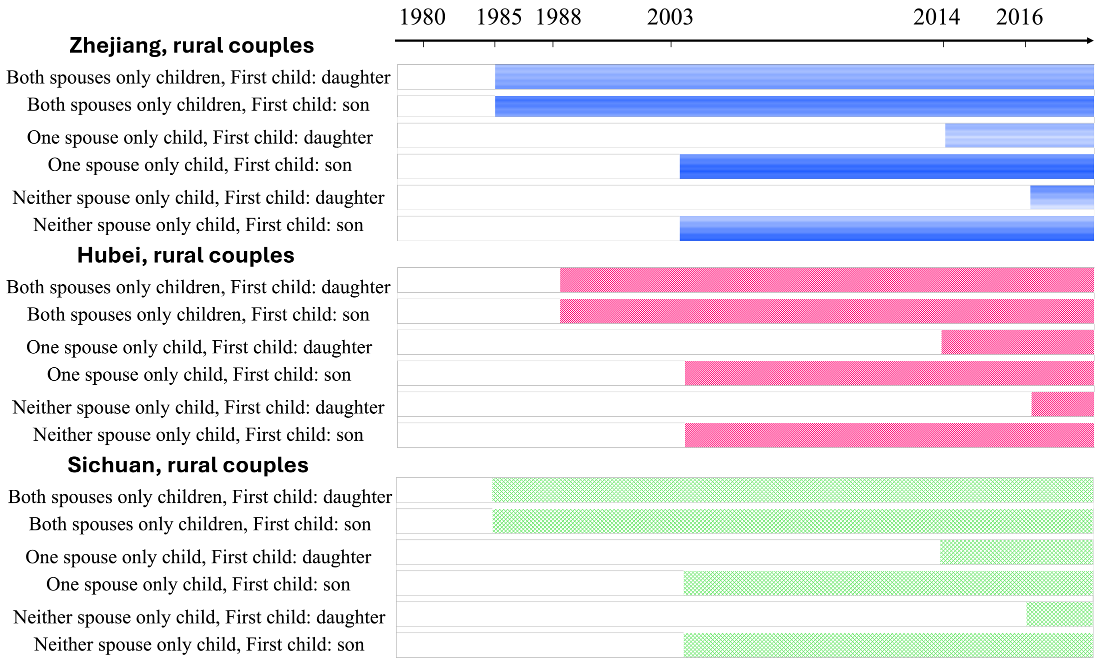
Note: The figure reports TFR from three sources: World Bank (internationally comparable), *China Population and Employment Statistics Yearbook* (official age-specific fertility rates), and CFPS (retrospective birth histories in 25 provinces). Vertical lines mark 2014 STCP (either), 2016 UTCP, and the 2021 Three-Child Policy. Divergence in 2011–2015 likely reflects underreporting during strict OCP enforcement and rising internal migration. *Data sources*: World Bank, *China Statistical Yearbook*, CFPS, and authors' calculations.

Figure 4 : Second-Birth Rates by Eligibility Status, 1980–2021



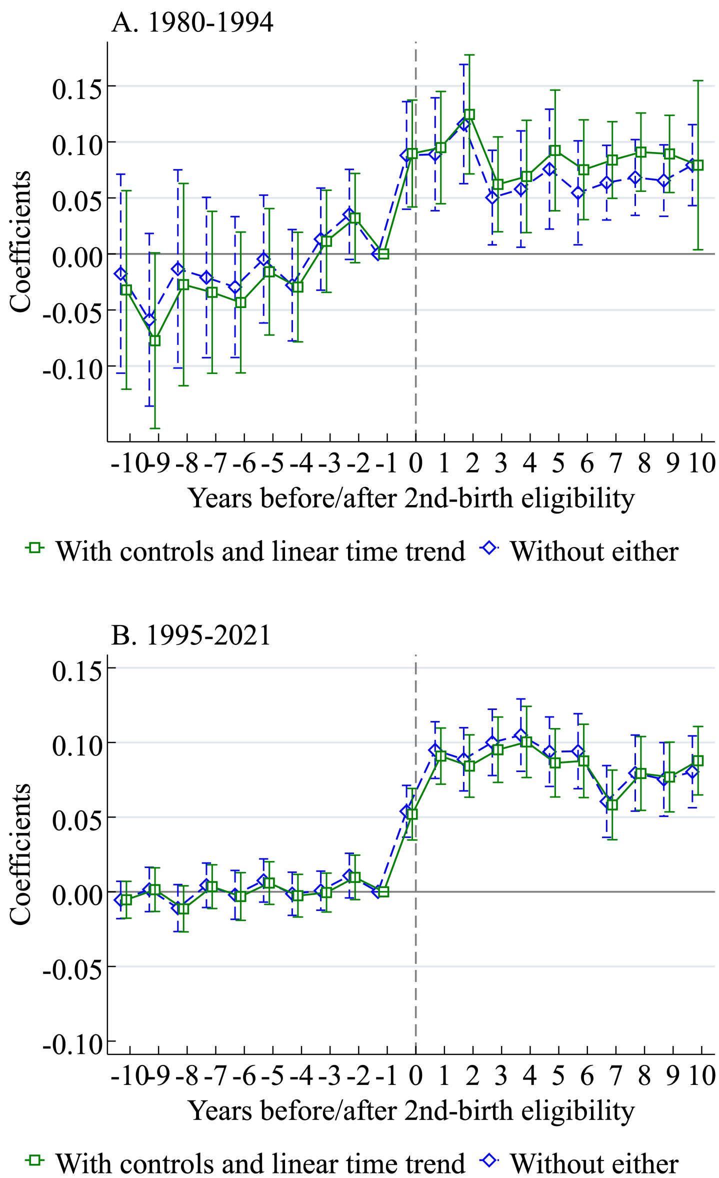
Note: Annual second-birth rates are plotted for eligible (solid red) and ineligible (dashed blue) couples with one child. We omit periods with sparse data (eligible couples before 1986; ineligible couples after 2016). Horizontal lines indicate mean rates for 1980–1994 and 1995–2021.
Sources: CFPS and authors' calculations.

Figure 5 : Illustrative Timing of Second-Child Eligibility (Rural Couples in Three Provinces)



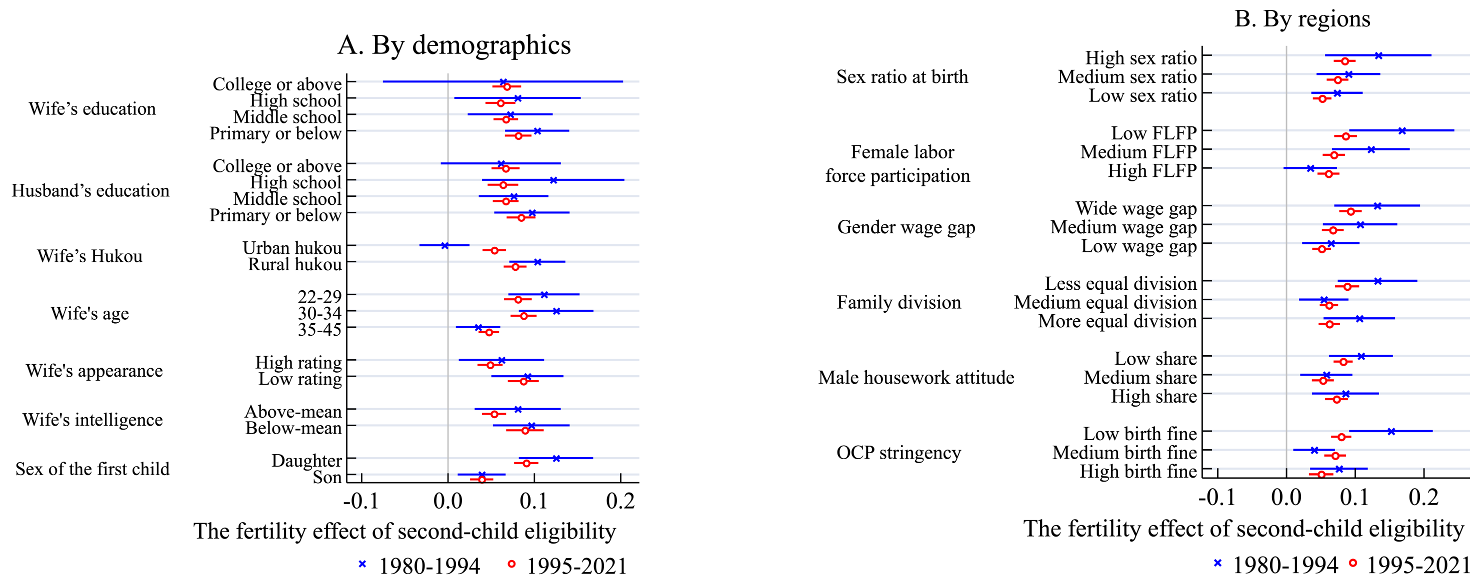
Note: The figure illustrates the timing of second-child eligibility for rural couples in Zhejiang (blue), Hubei (pink), and Sichuan (green). Within each province, rural couples are divided into six groups based on spouses' only-child status (three combinations) and the first child's gender (two categories). Display is illustrative.
Data sources: Authors' coding is based on published provincial regulations.

Figure 6 : Event-Study Estimates of Second-Child Eligibility Effects



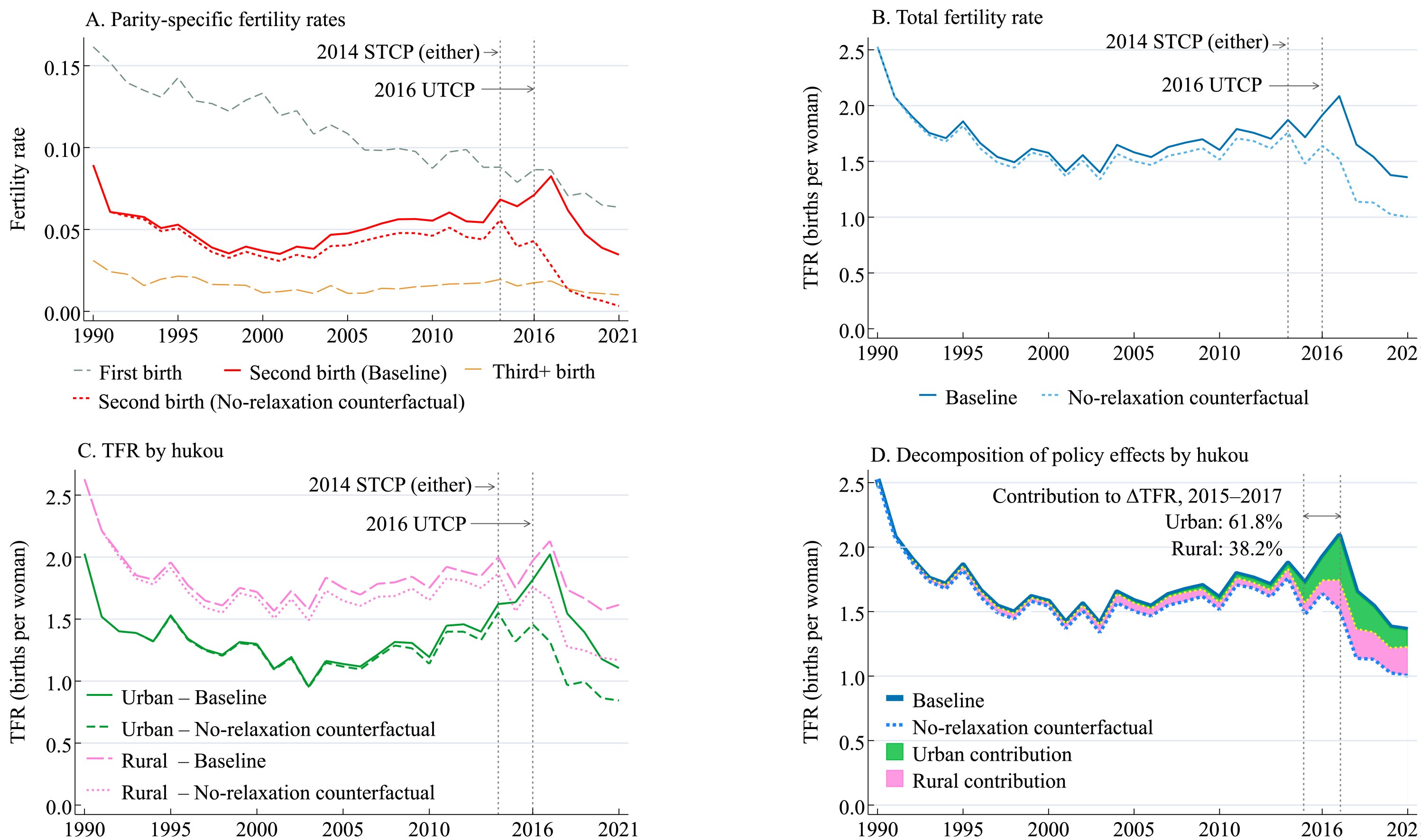
Note: Panels A (1980–1994) and B (1995–2021) plot coefficients from Equation (5) relative to $t = -1$ (the year immediately before a couple first becomes eligible for a second child). Solid green lines show estimates with the full set of controls and province-specific linear time trends; dashed blue lines show estimates without these adjustments. We exclude couples that ever lose second-child eligibility. Error bars indicate 95% confidence intervals. Standard errors are clustered at the province × demographic group × first child's birth year × mother's birth year level.

Figure 7 : Heterogeneous Effects by Demographics and Regions



Note: This figure reports subgroup-specific estimates of the effect of second-child eligibility on the probability of having a second birth in year $t+1$. Estimates are obtained from variants of Equation (4) in which $Eligible_{it}$ is interacted with mutually exclusive subgroup indicators. All specifications include the baseline controls, *pdbc* fixed effects, and year fixed effects. City-level measures are constructed from CFPS waves and cities are grouped into terciles (high/medium/low) within each measure; province-level OCP stringency is proxied by historical fines for unauthorized births. The underlying coefficients are reported in Appendix Tables E7 and E8. Error bars indicate 95% confidence intervals, with standard errors clustered at the $p \times d \times b \times c$ level.

Figure 8 : Simulated Fertility Trends and Births: Baseline vs. No-Relaxation Counterfactual



Note: The figure compares simulated outcomes under the observed sequence of OCP relaxations (baseline; solid) with a counterfactual in which no relaxations occur after 1990 (dotted). Panel A reports parity-specific annual birth probabilities. Panels B and C report total fertility rates (TFR) computed by aggregating simulated age-specific fertility rates for women aged 15–45; Panel C shows TFR separately by hukou status. Panel D decomposes the baseline–counterfactual TFR gap into urban and rural contributions (green and pink stacked areas, respectively), computed as the population-weighted components of the overall gap (see Appendix F for details).

Table 1: Announcement Timeline for OCP Relaxations by Province

	2nd-birth always allowed	Early relaxation of the OCP			Recent relaxation of the OCP			Definition of "rural couples"
		STCP (both)		One-and-a-Half-Child Policy	STCP (either)		UTCP	
Province	Rural	Rural	Entire province	Rural	Rural	National	National	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ningxia	1980	1980	1987	1980	1980	2014	2016	Wife
Tibet	1980	1980	1992	1980	1980	2014	2016	Either
Xinjiang	1980	1980	1992	1980	1980	2014	2016	Wife
Hebei	NA	1982	1985	1995	NA	2014	2016	Wife
Shanxi	NA	1982	1987	1990	1987–2000, 2014	2014	2016	Wife
Tianjin	NA	1983	1985	NA	1983	2014	2016	Wife
Shandong	NA	1983	1984	1986	NA	2014	2016	Wife
Beijing	NA	1983	1985	NA	1985	2014	2016	Wife
Heilongjiang	NA	1983	1985	2000	NA	1985–1990, 2014	2016	Both
Jiangxi	NA	1983	1990	1986	NA	2014	2016	Both
Guangdong	NA	NA	1984	1986	1986–1999, 2014	2014	2017	Both
Jiangsu	NA	NA	1984	NA	1991	2014	2016	Wife
Guizhou	NA	NA	1984	1999	NA	2014	2016	Both
Gansu	NA	1984–1990, 2003	2003	1998	NA	2014	2016	Both
Henan	NA	1984–1990, 2012	2012	2012	NA	2014	2016	Both
Chongqing	NA	1985	1985	NA	NA	2014	2016	Both
Sichuan	NA	1985	1985	NA	NA	2014	2016	Both
Hunan	NA	NA	1985	1987	NA	2014	2016	Both
Liaoning	NA	NA	1985	1988	1985	2014	2016	Wife
Anhui	NA	NA	1985	1989	1985	2014	2016	Both
Guangxi	NA	NA	1985	1989	NA	1985–1989, 2014	2016	Wife
Shanghai	NA	NA	1985	NA	1991	2014	2016	Wife
Jilin	NA	NA	1985	1994	2003	2014	2016	Both
Zhejiang	NA	1985	1985	2003	NA	2014	2016	Both
Hubei	NA	1985–1988, 2003	2003	1988	NA	2014	2016	Wife
Fujian	NA	NA	1985	2003	2003	2014	2016	Both
Qinghai	NA	NA	1986	NA	NA	2014	2016	Wife
Hainan	NA	NA	1986	1986	1986	2014	2016	Wife
Shaanxi	NA	NA	1986	2003	NA	2014	2016	Both
Yunnan	NA	NA	1991	NA	NA	2014	2016	Wife
Inner Mongolia	NA	NA	2003	1989	NA	2014	2016	Both

Note: Provinces are ordered by the first introduction of any exemption. Policy years follow a July-1 attribution rule: announcements made before July 1 are assigned to the current year; otherwise to the following year. STCP (both) permits a second child when *both* spouses are only children; the One-and-a-Half-Child Policy applies to rural couples with a firstborn daughter; STCP (either) permits a second child when *either* spouse is an only child; the UTCP (2016) allows all couples a second child. The last column reports each province's definition of a "rural couples" (wife's hukou only / either spouse's / both spouses'). Chongqing became a municipality in 1997; before then its regulations followed Sichuan's.

Table 2: Provincial Regulations on Birth Intervals and Minimum Maternal Age

Province	Content of additional regulations	Birth interval	Maternal age
Jiangxi	1. Birth interval at least 4 years and maternal age at least 25 at the second birth; 2. Birth interval at least 2 years and maternal age at least 28 at the second birth.	1980–2008	1980–2008
Guangdong	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1980–2008	2002–2008
Zhejiang	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1982–2006	2002–2006
Qinghai	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 26 at the second birth; 3. No regulation on pastoral area.	1982–2013	1982–2013
Ningxia	Birth interval at least 4 years.	1982–2013	NA
Shaanxi	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1986–2008	2002–2008
Hubei	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1987–2008	2003–2008
Sichuan	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 30 at the second birth.	1987–2015	2002–2015
Xinjiang	Birth interval at least 3 years except pastoral area.	1988–2005	NA
Guizhou	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 30 at the second birth.	1988–2008	1998–2008
Anhui	Apply for the second birth after the older child turns 3 or after the mother turns 26.	1988–2010	2002–2010
Fujian	1. Birth interval at least 4 years and maternal age at least 25 at the second birth; 2. No regulation on interval if maternal age at least 30 at the second birth.	1988–2011	2000–2011
Hainan	Birth interval at least 4 years.	1989–2002	NA
Gansu	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1989–2005	2002–2005
Guangxi	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1989–2011	2002–2011
Tianjin	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1989–2015	2003–2015
Hebei	1. Birth interval at least 4 years and maternal age at least 26 at the second birth; 2. No regulation on interval if maternal age at least 28 at the second birth.	1989–2015	1989–2015
Shanghai	Birth interval at least 4 years.	1990–2003	NA
Inner Mongolia	1. Birth interval at least 4 years for Han, and 3 years for ethnic minorities; 2. Birth interval at least 2 years and maternal age at least 24 at the first birth.	1990–2007	1990–2007
Heilongjiang	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 23 at the first birth.	1990–2013	2000–2013
Henan	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1990–2013	2003–2013
Yunnan	Birth interval at least 4 years for Han, and 3 years for ethnic minorities.	1990–2014	NA
Hunan	1. Birth interval at least 4 years and maternal age at least 25 at the second birth; 2. Birth interval at least 2 years and maternal age at least 24 at the first birth; 3. No regulation on interval if maternal age at least 28 at the second birth.	1991–2006	1999–2006
Beijing	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 28 at the second birth.	1991–2015	2003–2015
Tibet	1. Birth interval at least 4 years; 2. No regulation on interval if maternal age at least 35 at the second birth.	1993–2016	1993–2016
Chongqing	Apply for the second birth after the older child turns 3 or after the mother turns 28.	1998–2015	2002–2015
Shanxi	Maternal age at least 28 at the second birth.	NA	1982–2008
Jiangsu	Maternal age at least 24 at the second birth.	NA	1985–2010
Jilin	Maternal age at least 30 at the second birth.	NA	1987–2001
Shandong	Maternal age at least 30 at the second birth.	NA	1988–2012
Liaoning	Maternal age at least 26 at the second birth.	NA	1988–2013

Note: Each row summarizes province-specific requirements for a second birth, including the minimum birth interval and minimum maternal age. Columns 3 and 4 show the period during which each requirement was in force. "NA" indicates no explicit provincial requirement.
Data sources: Dong (2020) and Zhang and Liu (2016).

Table 3: Summary Statistics for the Dependent Variable and Key Regressors by Eligibility and Second-Birth Status (Couple-Year Level)

	(1) Eligible=0 N=75,820, #Couple=8,525		(2) Eligible=1 N=15,448, #Couple=2,679		(3) 2ndBirth=0 N=86,431, #Couple=8,456		(4) 2ndBirth=1 N=4,837, #Couple=4,837	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
2ndBirth	0.050	(0.217)	0.070	(0.255)	–	–	–	–
1stBirthThisYear	0.075	(0.263)	0.021	(0.145)	0.067	(0.250)	0.048	(0.215)
Explanatory variables: key eligibility determinants								
RuralHukouW	0.551	(0.497)	0.788	(0.408)	0.575	(0.494)	0.871	(0.335)
RuralHukouH	0.539	(0.498)	0.737	(0.440)	0.557	(0.497)	0.848	(0.359)
SiblingW_0 (reference group)	0.068	(0.251)	0.312	(0.463)	0.113	(0.317)	0.031	(0.173)
SiblingW_1+	0.932	(0.251)	0.688	(0.463)	0.887	(0.317)	0.969	(0.173)
SiblingH_0 (reference group)	0.087	(0.282)	0.363	(0.481)	0.139	(0.346)	0.040	(0.196)
SiblingH_1+	0.913	(0.282)	0.637	(0.481)	0.861	(0.346)	0.960	(0.196)
Firstborn_son	0.639	(0.480)	0.391	(0.488)	0.605	(0.489)	0.459	(0.498)
YrSinceFirstBirth	7.475	(5.856)	9.668	(5.779)	8.047	(5.946)	4.249	(3.418)
YrSinceFirstBirth_0_4 (reference group)	0.401	(0.490)	0.217	(0.412)	0.356	(0.479)	0.623	(0.485)
YrSinceFirstBirth_5_10	0.300	(0.458)	0.373	(0.484)	0.312	(0.463)	0.316	(0.465)
YrSinceFirstBirth_11+	0.299	(0.458)	0.410	(0.492)	0.332	(0.471)	0.061	(0.239)
AgeW	31.962	(6.182)	33.992	(5.809)	32.547	(6.173)	27.992	(4.135)
Explanatory variables: socioeconomic status proxies								
EduW_college+	0.081	(0.273)	0.098	(0.298)	0.087	(0.282)	0.030	(0.169)
EduW_high_school	0.180	(0.384)	0.146	(0.353)	0.179	(0.383)	0.089	(0.284)
EduW_middle_school	0.341	(0.474)	0.365	(0.482)	0.348	(0.476)	0.300	(0.458)
EduW_primary_or_below (reference group)	0.398	(0.490)	0.390	(0.488)	0.387	(0.487)	0.582	(0.493)
EduH_college+	0.106	(0.308)	0.112	(0.315)	0.111	(0.314)	0.042	(0.200)
EduH_high_school	0.201	(0.401)	0.160	(0.366)	0.196	(0.397)	0.155	(0.362)
EduH_middle_school	0.385	(0.487)	0.416	(0.493)	0.390	(0.488)	0.391	(0.488)
EduH_primary_or_below (reference group)	0.308	(0.462)	0.312	(0.463)	0.303	(0.459)	0.412	(0.492)
EduGrandmother_high	0.335	(0.472)	0.473	(0.499)	0.362	(0.481)	0.293	(0.455)
EduGrandmother_low (reference group)	0.624	(0.484)	0.500	(0.500)	0.599	(0.490)	0.680	(0.467)
EduGrandmother_missing	0.041	(0.199)	0.027	(0.162)	0.040	(0.195)	0.027	(0.161)
EduGrandfather_high	0.318	(0.466)	0.445	(0.497)	0.341	(0.474)	0.310	(0.462)
EduGrandfather_low (reference group)	0.616	(0.486)	0.525	(0.499)	0.598	(0.490)	0.643	(0.479)
EduGrandfather_missing	0.067	(0.249)	0.030	(0.169)	0.061	(0.239)	0.048	(0.213)
UrbanCrrntW	0.621	(0.485)	0.552	(0.497)	0.624	(0.485)	0.349	(0.477)
Explanatory variables: other demographic characteristics								
AgeH	33.713	(6.678)	35.950	(6.399)	34.346	(6.691)	29.553	(4.616)
AgeGrandmother	60.403	(8.285)	61.718	(7.927)	60.879	(8.242)	56.191	(6.809)
AgeGrandfather	63.164	(8.788)	64.188	(8.336)	63.594	(8.722)	58.885	(7.385)
AppearanceW_HighRating	0.312	(0.463)	0.382	(0.486)	0.329	(0.470)	0.231	(0.421)
AppearanceW_LowRating (reference group)	0.674	(0.469)	0.594	(0.491)	0.656	(0.475)	0.750	(0.433)
AppearanceW_missing	0.014	(0.117)	0.024	(0.153)	0.015	(0.123)	0.019	(0.136)
IntelligenceW_AboveMean	0.303	(0.460)	0.373	(0.484)	0.320	(0.467)	0.215	(0.411)
IntelligenceW_BelowMean (reference group)	0.691	(0.462)	0.618	(0.486)	0.673	(0.469)	0.778	(0.415)
IntelligenceW_missing	0.006	(0.076)	0.009	(0.093)	0.006	(0.079)	0.007	(0.081)

Note: Columns (1) and (2) report means and standard deviations for ineligible versus eligible couples; columns (3) and (4) report the same statistics for couples without versus with a second birth. All statistics are at the couple-year level. The "reference group" indicates the omitted category when dummies are used in the following analysis. See Appendix for full summary statistics by eligibility status (Table C2).

Table 4: Effect of Second-Child Eligibility on Second-Birth Fertility

	Per-year second-birth probability						
	1980–2021			1980–1994		1995–2021	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Eligible	0.074***	0.073***	0.072***	0.091***	0.091***	0.072***	0.071***
	(0.006)	(0.006)	(0.005)	(0.015)	(0.015)	(0.006)	(0.006)
1stBirthThisYear	–0.092***	–0.092***	–0.088***	–0.137***	–0.132***	–0.047***	–0.042***
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.005)	(0.005)
Eligible×1stBirthThisYear	–0.010	–0.008	–0.012	–0.004	–0.005	–0.042**	–0.049***
	(0.014)	(0.014)	(0.014)	(0.054)	(0.054)	(0.017)	(0.017)
Age group dummies	✓	✓	✓	✓	✓	✓	✓
Detailed controls		✓	✓	✓	✓	✓	✓
pdbc fixed effects	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Province-specific linear time trends			✓		✓		✓
$\overline{Y}$	0.053	0.053	0.053	0.079	0.079	0.042	0.042
R^2	0.227	0.228	0.231	0.299	0.306	0.191	0.195
N (Couple-years)	91,268	91,268	91,268	27,666	27,666	63,602	63,602
N (Unique couples)	8,985	8,985	8,985	4,832	4,832	6,557	6,557
N (Clusters)	5,065	5,065	5,065	2,614	2,614	3,821	3,821

Note: The dependent variable is an indicator for having a second birth in year $t+1$. The sample includes couples with one child in which the wife is aged 22–45 and the husband 22–60 at the date of survey in year t. *pdbc* fixed effects control for province × demographic group × first-birth year × mother's birth cohort. $\overline{Y}$ denotes the sample mean of the dependent variable. N (Couple-years) reports couple–year observations; N (Unique couples) reports distinct couples; N (Clusters) reports the number of *pdbc* cells. Standard errors, in parentheses, are clustered at the province × demographic group × first child's birth year × mother's birth year level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Effect Heterogeneity by Relaxation Type

	Per-year second-birth probability					
	1980–2021		1980–1994		1995–2021	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible_early rural exemptions	0.096***	0.097***	0.106***	0.105***	0.103***	0.104***
	(0.008)	(0.008)	(0.018)	(0.018)	(0.009)	(0.009)
Eligible_early non-rural exemptions	0.064***	0.060***	0.033*	0.034**	0.078***	0.075***
	(0.011)	(0.011)	(0.018)	(0.016)	(0.016)	(0.015)
Eligible_recent relaxations	0.026***	0.024***			0.031***	0.028***
	(0.006)	(0.006)			(0.007)	(0.007)
1stBirthThisYear	–0.092***	–0.089***	–0.137***	–0.132***	–0.048***	–0.043***
	(0.004)	(0.004)	(0.008)	(0.008)	(0.005)	(0.005)
Eligible×1stBirthThisYear	–0.003	–0.006	–0.008	–0.009	–0.037**	–0.045***
	(0.014)	(0.014)	(0.054)	(0.054)	(0.017)	(0.017)
Age group dummies	✓	✓	✓	✓	✓	✓
Detailed controls	✓	✓	✓	✓	✓	✓
pdbc fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Province-specific linear time trends		✓		✓		✓
$\overline{Y}$	0.053	0.053	0.079	0.079	0.042	0.042
R^2	0.229	0.232	0.299	0.306	0.192	0.196
N (Couple-years)	91,268	91,268	27,666	27,666	63,602	63,602
N (Unique couples)	8,985	8,985	4,832	4,832	6,557	6,557
N (Clusters)	5,065	5,065	2,614	2,614	3,821	3,821

Note: The three eligibility variables classify OCP relaxations into mutually exclusive categories: (i) *Eligible_early rural exemptions* covers early exemptions for rural couples (e.g., the One-and-a-Half-Child Policy); (ii) *Eligible_early non-rural exemptions* covers early non-rural exemptions (e.g., urban couples in which both spouses were only children); and (iii) *Eligible_recent relaxations* pools eligibility granted under the 2014 *STCP (either)* and the 2016 *UTCP*. The sample and control variables follow Table 4. Standard errors, in parentheses, are clustered at the province × demographic group × first child's birth year × mother's birth year level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendices

Online Appendix for: When the Last Big Arrow Was Loosed: How the One-Child Policy Relaxation Reshaped Fertility Trends in China

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This online appendix provides supplementary material for the paper "When the Last Big Arrow Was Loosed: How the One-Child Policy Relaxation Reshaped Fertility Trends in China". Section A elaborates on the enforcement and subsequent relaxation of the One-Child Policy (OCP). Section B details the criteria for second-child eligibility. Section C presents variable definitions and descriptive statistics. Section D addresses identification concerns and reports supporting validity tests. Section E provides additional figures and tables for robustness checks as well as supplementary tables with detailed estimates for heterogeneity analysis. Section F describes the procedure for the counterfactual simulation of the total fertility rate (TFR) trend.

A. Enforcement and Relaxation of the OCP

This section provides additional details on the implementation of the OCP and its subsequent relaxations.

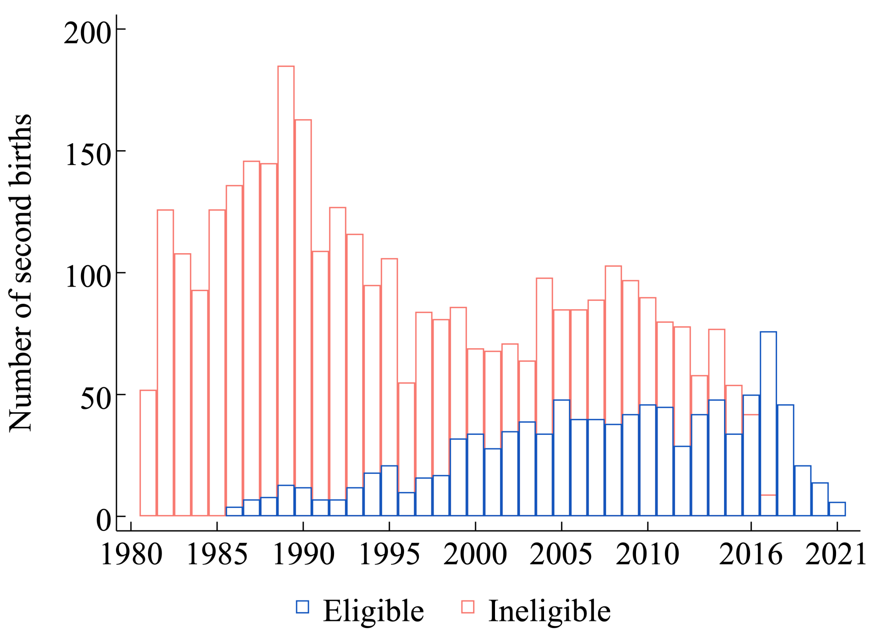
We first document that enforcement of the OCP was relatively lenient in its early years. As shown in Table A1, most provinces did not enact detailed family-planning regulations until the mid-1990s. In the absence of clear, enforceable guidelines, local authorities often lacked both explicit directives and strong incentives to implement the policy strictly. Consistent with this institutional environment, Figure A1 shows that second births remained relatively common throughout the 1980s, supporting the view that OCP enforcement was weak during this initial phase.

Table A1: Initial Enactment Years of Provincial Family-Planning Regulations

Year	Province
1980	Guangdong
1985	Zhejiang
1986	Qinghai, Ningxia, Shaanxi
1987	Sichuan, Hubei
1988	Tianjin, Liaoning, Jilin, Anhui, Fujian, Shandong, Guangxi, Guizhou
1989	Hebei, Shanxi, Heilongjiang, Hunan, Gansu, Hainan
1990	Inner Mongolia, Shanghai, Jiangsu, Jiangxi, Henan, Yunnan
1991	Beijing
1992	Xinjiang, Tibet

Note: This table reports the year in which each province first enacted its formal family-planning regulations.

Figure A1 : Second Births Before and After Eligibility



Note: Bars show counts of second births to couples in periods before (red) and after (blue) they become eligible for a second child. The main text focuses on rate-based comparisons; we report counts here for completeness. *Data sources*: CFPS and authors' calculations.

B. Criteria for Second-Child Eligibility

García (2024) documents 17 criteria under which couples could obtain second-child eligibility without incurring a fine in the early years of the OCP, as listed in column (1) of Table B1. In our analysis, we focus on seven of these criteria based on their empirical relevance: remarriage, return migration from Hong Kong or Taiwan, ethnic-minority status, the couple's only-child status, rural status, the sex of the first child, and the number of years since the first birth. We exclude couples who satisfy any of the first three conditions (remarriage, Hong Kong/Taiwan returnees, or minority status) and use the remaining four criteria—only-child status, rural status, the sex of the first child, and birth spacing—together with the wife's age (a key component of provincial rules not explicitly considered by García (2024) to construct the eligibility indicator $Eligible_{it}$.

The other ten criteria listed by García (2024) are rare and therefore unlikely to materially affect our eligibility measure. To assess this, we combine information from several datasets to approximate the prevalence of each excluded criterion. Specifically, for each condition, we compute the share of married women aged 22–45 with at most one child who meet the relevant criterion. We use the CFPS and the China Health and Retirement Longitudinal Study (CHARLS), two nationally representative household surveys that provide detailed demographic, socioeconomic, and health information. The resulting prevalence rates, reported in columns (2)–(4) of Table B1, show that couples satisfying any of these less common conditions constitute only a small fraction of the population.

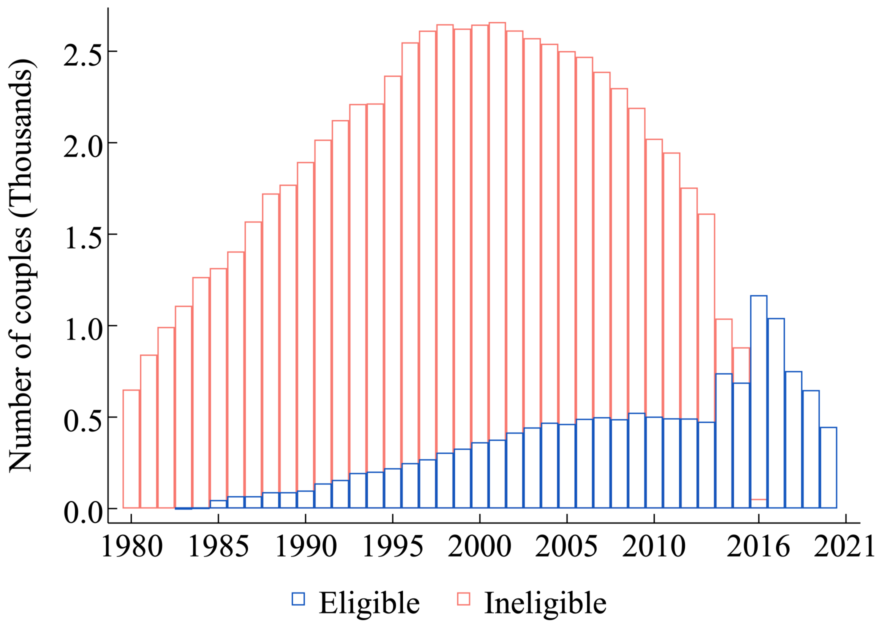
Figure B1 further demonstrates the gradual and sustained expansion of second-child eligibility over time, culminating in universal eligibility in 2016 under the Universal Two-Child Policy (UTCP). Together, these patterns confirm the staggered nature of the OCP relaxations and provide quasi-experimental variation in second-child eligibility, which we exploit for causal identification.

Table B1: Criteria for Second-Child Eligibility and Prevalence of Eligible Couples

Criteria	% of couples meeting the criterion among married women aged 22–45 with at most one child	Measurements of couples meeting the criterion	Data source
(1) First child is dead	0.04%	Couples whose first child is deceased	CFPS 2010–2022
(2) Prolonged childlessness followed by the adoption of a child	0.00%	Couples whose first child is adopted	CHARLS (2014, 2018)
(3) In a **remarriage**, one spouse is childless and the other spouse already has one or two children	1.08%	Couples in which at least one spouse is remarried	CFPS 2010–2022
(4) **One or both spouses returned to China from Hong Kong or Taiwan**	0.02%	Couples in which at least one spouse holds non-mainland Chinese hukou	CFPS 2010–2022
(5) **One or both spouses belong to an ethnic minority with fewer than ten million members**	4.29%	Couples in which at least one spouse belongs to a minority group with population below 10 million	CFPS 2010–2022
(6) One spouse is disabled and cannot work	2.34%	Couples in which the husband or the wife is disabled and unable to work	CHARLS 2018
(7) A rural couple living in sparsely populated mountain, reclamation, or border areas	1.54%	Couples in which both spouses are Han farmers	CFPS 2014
(8) One spouse is a deep-sea fisherman	0.55%	Couples in which either spouse works in the fishery industry	CFPS 2010–2018
(9) One spouse has been continuously working in underground mining for more than five years	1.28%	Couples in which the husband has worked continuously in underground mining for more than five years	CFPS 2010–2018
(10) **One or both spouses are only children**	19.44%	Couples in which at least one spouse is an only child	CFPS 2010–2022
(11) Only one child or one son has been born in the family over two generations	0.56%	Couples in which both the husband and his father are only children	CFPS 2010–2022
(12) Among brothers (typically on the husband's side), only one is able to produce children	0.50%	Couples in which the husband's brothers have no children	CHARLS (2015)
(13) The husband settles in his wife's family, which has daughters but no sons	1.27%	Couples living with the wife's parents	CFPS 2010–2022
(14) One spouse is the only child of a revolutionary martyr	0.03%	Couples whose family receives martyr subsidies	CFPS 2010
(15) Couple has real economic difficulties or claims other peculiar reasons	2.10%	Couples receiving Dibao assistance (targeted cash transfers to low-income households)	CFPS (2010, 2012, 2014)
(16) **First child is a girl**	22.01%	Couples whose first child is a daughter	CFPS 2010–2022
(17) **Birth interval between the first two children (typically four years in most provinces)**	43.57%	Couples whose first birth occurred at least four years earlier	CFPS 2010–2022

Note: This table summarizes the main criteria for second-child eligibility and their prevalence among couples. Percentages are computed as the share of married women aged 22–45 with at most one child who satisfy each criterion. Criteria shown in bold are incorporated in our analysis; the remaining criteria are extremely rare and are therefore excluded from the construction of our eligibility measure. *Data sources*: García (2024); CFPS; CHARLS; authors' calculations.

Figure B1 : Counts of Eligible and Ineligible Couples by Year



Note: Annual counts of couples with one child, by second-child eligibility status: eligible (blue) and ineligible (red).
Data sources: CFPS, provincial rule histories, and authors' calculations.

C. Variables

Table C1 summarizes the definitions of all variables used in our analysis. We group them into three sets: (i) eligibility-related characteristics, (ii) proxies for socioeconomic status, and (iii) other demographic characteristics. This appendix provides additional detail on their construction, complementing Section III.E.

Eligibility-related characteristics. — The first set of controls covers each couple's only-child status, hukou registration, the sex of the first child, years since the first birth, and the wife's age. Among these, hukou registration, years since the first birth, and the wife's age are time-varying; the remaining variables are time-invariant.

We determine only-child status using information on the reported number of siblings and the number of children of each spouse's parents. Sibling information is collected in the 2010 wave. For individuals with missing sibling data, we impute the number of siblings using the "relationship to household head" variable to infer the number of children each set of parents has, successfully imputing 18.7% of missing values for wives and 31.4% for husbands. We code only-child status as an indicator equal to one if each spouse has no siblings and zero otherwise.

We construct an indicator for rural hukou by tracking each spouse's hukou registration across CFPS waves, beginning with their first survey entry. For years prior to the first CFPS interview, we impute hukou from childhood records. The CFPS records hukou at birth, age three, and age twelve. We compare childhood hukou with current status and, when they coincide, carry that value back. For individuals with urban hukou in childhood, we assume urban hukou throughout their life, as conversion from urban to rural hukou is rare. This imputation strategy is well-founded: hukou registration is highly stable after marriage, and no couple in our regression sample changes hukou status over the observation period.

The remaining eligibility-related variables are coded as follows. Years since the first birth are categorized into three groups: 0–4 years (reference group), 5–10 years, and 11+ years. The wife's age is grouped into two-year intervals. The sex of the first child is coded as one for sons and zero for daughters. Finally, although it does not affect eligibility directly, we control for whether a woman had her first birth in the current year, to capture the fact that having two births in consecutive years is uncommon but does occur.

Socioeconomic status proxies. — The second set of controls captures couples' socioeconomic status. For each spouse, we include education dummies: college or above, high school, middle school, and primary school or below (reference group). These variables are treated as time-invariant, as no woman in our data pursues additional schooling after marriage.

We also control for the education of the child's grandparents, constructing separate measures for grandmothers and grandfathers. For each couple, we compute the average years of schooling of the maternal and paternal sides. If grandparental education is missing for both sides, we set this average to zero and include a separate indicator that equals one if grandparental education is missing and zero otherwise. From this mean, we define indicators for below-average and above-average education separately for grandmothers and grandfathers, as listed in Table C1.

Finally, we include a time-varying indicator for the wife's current urban residence. For years prior to the first CFPS interview, we impute urban residence based on the respondent's reported childhood location and current hukou registration.

Other demographic characteristics. — The third set contains demographic characteristics that may influence fertility. We incorporate the husband's age, categorized into five-year bins, and the ages of grandmothers and grandfathers. Grandparental ages are constructed as an average of both spouses' sides and are grouped into three categories: under 60 (reference group), 60–69, and 70+, with an additional indicator for missing values. Missing data for grandparental age are handled analogously to the grandparental education.

We also use the CFPS interviewer's ratings of the wife's appearance and intelligence (on a 1–7 scale) as proxies for opportunity cost and intra-household bargaining power. For analysis, we dichotomize appearance into "normal" and "exceptionally beautiful" and intelligence into "below average" and "above average," and include indicators for missing ratings.

Table C2 presents summary statistics of all variables by eligibility status at the couple-year level for the regression sample. Figure C1 shows the distribution of education levels for wives and husbands by second-birth status and indicates that couples in which both spouses have at most primary schooling are more likely to have a second child than couples without a second child.

Table C1: Definitions of Dependent and Explanatory Variables

Variables	Definition
Dependent variable	
$2ndBirth_{i,t+1}$	equals 1 if couple i has a second child in year $t+1$, and 0 otherwise.
Explanatory variables: eligibility status	
$Eligible_{it}$	equals 1 if couple i is eligible to have a second child in year t.
$1stBirthThisYear_{it}$	equals 1 if couple i has a first child in year t, and 0 otherwise.
Explanatory variables: key eligibility determinants	
Hukou:	
$RuralHukouW_{it}$	equals 1 if the wife in couple i has a rural hukou in year t.
$RuralHukouH_{it}$	equals 1 if the husband in couple i has a rural hukou in year t.
Only-Child status:	
$SiblingW\_0_i, SiblingW\_1{+}_i, SiblingH\_0_i, SiblingH\_1{+}_i$	equals 1 if the wife/husband in couple i has no siblings (reference group)/at least one sibling. Only-child status is determined by the reported number of siblings and the number of children born to their parents.
First child's gender:	
$Firstborn\_son_i$	equals 1 if couple i's first child is a son.
Birth spacing and maternal age:	
$YrSinceFirstBirth\_0\_4_{it}, YrSinceFirstBirth\_5\_10_{it}, YrSinceFirstBirth\_11{+}_{it}$	equals 1 if the years since the first child's birth for couple i in year t is: 0–4 (reference group), 5–10, or 11+ years.
$AgeW\_22\_23_{it}, AgeW\_24\_25_{it}, AgeW\_26\_27_{it}, AgeW\_28\_29_{it}, AgeW\_30\_31_{it}, AgeW\_32\_33_{it}, AgeW\_34\_35_{it}, AgeW\_36\_37_{it}, AgeW\_38\_39_{it}, AgeW\_40\_41_{it}, AgeW\_42\_43_{it}, AgeW\_44\_45_{it}$	equals 1 if the wife in couple i belongs to age group: 22–23 (reference group), 24–25, 26–27, 28–29, 30–31, 32–33, 34–35, 36–37, 38–39, 40–41, 42–43, or 44–45 in year t.
$CohortW\_1930\_1959_i, CohortW\_1960\_1969_i, CohortW\_1970\_1979_i, CohortW\_1980\_2000_i$	equals 1 if the wife in couple i belongs to birth cohort group: 1930–1959 (reference group), 1960–1969, 1970–1979, 1980–2000.
Explanatory variables: socioeconomic status proxies	
Education:	
$EduW\_college{+}_i, EduW\_high\_school_i, EduW\_middle\_school_i, EduW\_primary\_or\_below_i$	equals 1 if the wife's education level in couple i is: college or above, high school, middle school, and primary school or below (reference group).
$EduH\_college{+}_i, EduH\_high\_school_i, EduH\_middle\_school_i, EduH\_primary\_or\_below_i,$	equals 1 if the husband's education level in couple i is: college or above, high school, middle school, and primary school or below (reference group).
$EduGrandmother\_high_i, EduGrandmother\_low_i, EduGrandmother\_missing_i, EduGrandfather\_high_i, EduGrandfather\_low_i, EduGrandfather\_missing_i$	equals 1 if the average education level of couple i's mother/father and mother-in-law/father-in-law falls into one of the following categories: above the mean, below the mean (reference group), or missing.
Urban location:	
$UrbanCrrntW_{it}$	equals 1 if the wife in couple i lives in an urban area in year t.
Explanatory variables: Other demographic characteristics	
Age:	
$AgeH\_22\_25_{it}, AgeH\_26\_30_{it}, AgeH\_31\_35_{it}, AgeH\_36\_40_{it}, AgeH\_41\_45_{it}, AgeH\_46\_50_{it}, AgeH\_51\_60_{it}$	equals 1 if the husband in couple i belongs to age group: 22–25 (reference group), 26–30, 31–35, 36–40, 41–45, 46–50, 51–60 in year t.
$AgeGrandmother\_under\_60_i, AgeGrandmother\_60\_69_i, AgeGrandmother\_70{+}_i, AgeGrandmother\_missing_i, AgeGrandfather\_under\_60_i, AgeGrandfather\_60\_69_i, AgeGrandfather\_70{+}_i, AgeGrandfather\_missing_i$	equals 1 if the average age of couple i mother/father and mother-in-law/father-in-law falls in age group: under 60 (reference group), 60–69, 70+, or missing.
Appearance and Intelligence:	
$AppearanceW\_HighRating_i, AppearanceW\_LowRating_i, AppearanceW\_missing_i$	equals 1 if the interviewer-rated appearance of the wife in couple i is: above the mean, below the mean (reference group), or missing.
$IntelligenceW\_AboveMean_i, IntelligenceW\_BelowMean_i, IntelligenceW\_missing_i$	equals 1 if the interviewer-rated intelligence of the wife in couple i is: above the mean, below the mean (reference group), or missing.

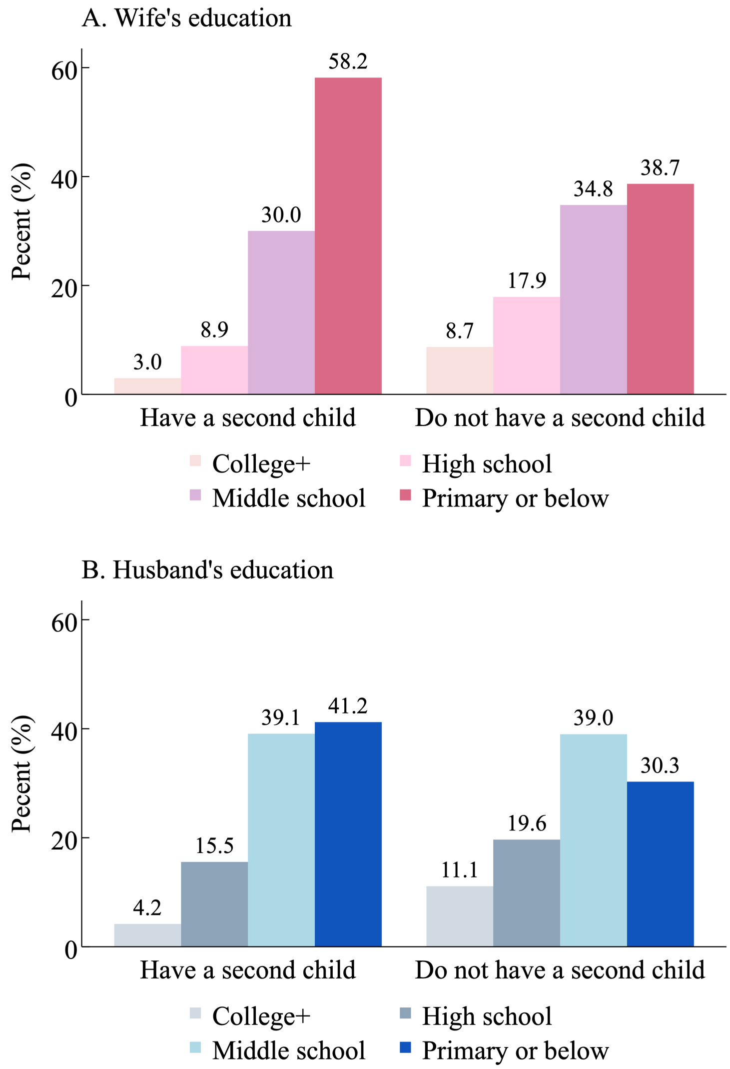
Note: This table reports the definitions of all variables used in the analysis. Time-invariant indicators capture couples' only-child status, grandparental age and education, the sex of the first child, and the wife's appearance and intelligence. All other variables are time-varying.

Table C2: Summary Statistics for the Dependent Variable and All Regressors by Eligibility Status (Couple-Year Level)

	(1) Eligible=0		(2) Eligible=1			(3) Eligible=0		(4) Eligible=1	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
2ndBirth	0.050	(0.217)	0.070	(0.255)	EduH_college+	0.106	(0.308)	0.112	(0.315)
1stBirthThisYear	0.075	(0.263)	0.021	(0.145)	EduH_high_school	0.201	(0.401)	0.160	(0.366)
Explanatory variables: key eligibility determinants					EduH_middle_school	0.385	(0.487)	0.416	(0.493)
RuralHukouW	0.551	(0.497)	0.788	(0.408)	EduH_primary_or_below (reference group)	0.308	(0.462)	0.312	(0.463)
RuralHukouH	0.539	(0.498)	0.737	(0.440)	EduGrandmother_high	0.335	(0.472)	0.473	(0.499)
SiblingW_0 (reference group)	0.068	(0.251)	0.312	(0.463)	EduGrandmother_low (reference group)	0.624	(0.484)	0.500	(0.500)
SiblingW_1+	0.932	(0.251)	0.688	(0.463)	EduGrandmother_missing	0.041	(0.199)	0.027	(0.162)
SiblingH_0 (reference group)	0.087	(0.282)	0.363	(0.481)	EduGrandfather_high	0.318	(0.466)	0.445	(0.497)
SiblingH_1+	0.913	(0.282)	0.637	(0.481)	EduGrandfather_low (reference group)	0.616	(0.486)	0.525	(0.499)
Firstborn_son	0.639	(0.480)	0.391	(0.488)	EduGrandfather_missing	0.067	(0.249)	0.030	(0.169)
YrSinceFirstBirth	7.475	(5.856)	9.668	(5.779)	UrbanCrrntW	0.621	(0.485)	0.552	(0.497)
YrSinceFirstBirth_0_4 (reference group)	0.401	(0.490)	0.217	(0.412)	***Explanatory variables: other demographic characteristics***				
YrSinceFirstBirth_5_10	0.300	(0.458)	0.373	(0.484)	AgeH	33.713	(6.678)	35.950	(6.399)
YrSinceFirstBirth_11+	0.299	(0.458)	0.410	(0.492)	AgeH_22_25 (reference group)	0.098	(0.298)	0.026	(0.160)
BirthyearW	1966.438	(9.022)	1974.113	(9.024)	AgeH_26_30	0.268	(0.443)	0.209	(0.407)
CohortW_1935_1959 (reference group)	0.246	(0.430)	0.069	(0.254)	AgeH_31_35	0.244	(0.430)	0.265	(0.441)
CohortW_1960_1969	0.369	(0.483)	0.218	(0.413)	AgeH_36_40	0.198	(0.399)	0.229	(0.420)
CohortW_1970_1979	0.306	(0.461)	0.408	(0.491)	AgeH_41_45	0.144	(0.351)	0.199	(0.399)
CohortW_1980_2000	0.079	(0.270)	0.304	(0.460)	AgeH_46_50	0.036	(0.186)	0.063	(0.242)
AgeW	31.962	(6.182)	33.992	(5.809)	AgeH_51_60	0.006	(0.075)	0.009	(0.095)
AgeW_22_23(reference group)	0.068	(0.252)	0.017	(0.129)	AgeGrandmother	60.403	(8.285)	61.718	(7.927)
AgeW_24_25	0.107	(0.310)	0.038	(0.192)	AgeGrandmother_under60 (reference group)	0.468	(0.499)	0.417	(0.493)
AgeW_26_27	0.120	(0.325)	0.092	(0.290)	AgeGrandmother_60_69	0.353	(0.478)	0.393	(0.488)
AgeW_28_29	0.116	(0.320)	0.116	(0.320))	AgeGrandmother_70+	0.134	(0.340)	0.162	(0.368)
AgeW_30_31	0.102	(0.303)	0.129	(0.335)	AgeGrandmother_missing	0.045	(0.208)	0.028	(0.165)
AgeW_32_33	0.094	(0.291)	0.108	(0.310)	AgeGrandfather	63.164	(8.788)	64.188	(8.336)
AgeW_34_35	0.086	(0.280)	0.098	(0.297)	AgeGrandfather_under60 (reference group)	0.354	(0.478)	0.311	(0.463)
AgeW_36_37	0.079	(0.270)	0.088	(0.283)	AgeGrandfather_60_69	0.376	(0.484)	0.413	(0.492)
AgeW_38_39	0.073	(0.260)	0.087	(0.282)	AgeGrandfather_70+	0.216	(0.412)	0.250	(0.433
AgeW_40_41	0.067	(0.250)	0.087	(0.282)	AgeGrandfather_missing	0.054	(0.226)	0.027	(0.161)
AgeW_42_43	0.060	(0.238)	0.093	(0.290)	AppearanceW_HighRating	0.312	(0.463)	0.382	(0.486)
AgeW_44_45	0.028	(0.164)	0.047	(0.212)	AppearanceW_LowRating (reference group)	0.674	(0.469)	0.594	(0.491)
Explanatory variables: socioeconomic status proxies					AppearanceW_missing	0.014	(0.117)	0.024	(0.153)
EduW_college+	0.081	(0.273)	0.098	(0.298)	IntelligenceW_AboveMean	0.303	(0.460)	0.373	(0.484)
EduW_high_school	0.180	(0.384)	0.146	(0.353)	IntelligenceW_BelowMean (reference group)	0.691	(0.462)	0.618	(0.486)
EduW_middle_school	0.341	(0.474)	0.365	(0.482)	IntelligenceW_missing	0.006	(0.076)	0.009	(0.093)
EduW_primary_or_below (reference group)	0.398	(0.490)	0.390	(0.488)	*N*	75,820		15,448	

Note: This table reports summary statistics of all variables by eligibility status at the couple-year level. Columns (1) and (3) correspond to ineligible couple-years (*Eligible* = 0), and columns (2) and (4) correspond to eligible couple-years (*Eligible* = 1). The full sample comprises 75,820 ineligible couple-year observations and 15,448 eligible couple-year observations.

Figure C1 : Education of Wives and Husbands by Second-Birth Status



Note: Histograms report the distribution of education for wives (Panel A) and husbands (Panel B) separately by second-birth status (whether a second birth occurs during the couple-year).
Data sources: CFPS and authors' calculations.

D. Identification Strategy: Implementation and Validation

D1. Construction of pdbc Cells

In the extended TWFE model (4), we use cells defined on the

$$\{p \times d \times b \times c\}$$

grid to capture nearly all information relevant for determining $Eligible_{it}$. We estimate β_1 in (4) including fixed effects ϕ_{pdbc} and cluster standard errors at the $p \times d \times b \times c$ level.

The index d initially comprises 24 mutually exclusive demographic groups: {both spouses only children, either, neither} $\times$ {both rural hukou, wife only, husband only, neither} $\times$ {firstborn daughter, firstborn son}. The index b is the first child's birth year, and c the wife's birth cohort. As described in Section II.A, provinces imposed different combinations of minimum maternal-age requirements and minimum birth-interval requirements. Accordingly, the grid can effectively reduce to $\{p \times d \times 1 \times c\}$ when only maternal age is regulated, $\{p \times d \times b \times 1\}$ when only birth intervals are regulated, and $\{p \times d \times b \times c\}$ when both rules apply.

Even so, the full $\{p \times d \times b \times c\}$ grid yields an impractically large number of cells, many with very few or no observations. To maintain feasibility while preserving the variation that matters for eligibility, we construct a parsimonious version of the grid through four steps.

Step 1: Collapsing demographic groups (d). — We start from the 24 combinations given by {both spouses only children, either, neither} $\times$ {both rural hukou, wife only, husband only, neither}$\times$ {firstborn daughter, firstborn son}. By inspecting the policy function $\mathbf{\Lambda}_{pt}(\cdot)$ over time, we identify groups that share identical eligibility patterns in all provinces and years. We collapse such groups without loss of information. Specifically, Groups 3–4, 5–6, 13–16, and 20–24 are merged as shown in Table D1. Groups 20–24 exhibit a pattern that differs in Tibet, but since our estimation sample contains no couples from Tibet, merging these groups does not affect any observations. This procedure reduces the number of demographic groups from 24 to 15.

Step 2: Collapsing b and c when rules are absent. — The first child's birth year b is relevant only in provinces with birth-interval regulations, and the wife's birth cohort c is relevant only in provinces with maternal-age regulations. In provinces without birth-interval rules, b has no effect on eligibility; we therefore pool all b values into a single category in those provinces. Similarly, in provinces without maternal-age rules, c has no effect on eligibility. Rather than collapsing c completely, however, we preserve meaningful variation in mothers' birth years by grouping c into ten five-year cohorts based on the distribution of women's birth years (Figure D1): $<$1950, 1950–1954, 1955–1959, 1960–1964, 1965–1969, 1970–1974, 1975–1979, 1980–1984, 1985–1989, and $\geq$1990. In provinces with both birth-interval and maternal-age rules, we retain the full cross-classification of b and c.

Step 3: Collapsing b after birth-interval rules end. — In provinces with birth-interval regulations, b matters for eligibility only if the first birth occurs while the regulation is in force. Once the regulation is abolished, the exact value of b no longer affects eligibility. We therefore collapse all b values corresponding to first births after the termination of the birth-interval rule into a single group. For example, in Shanghai the birth-interval rule ended in 2004 (Table 2); we thus do not distinguish first-birth years for couples whose first child was born in 2004 or later.

Step 4: Collapsing c after maternal-age rules end. — An analogous argument applies to maternal-age regulations. While the rule is in force, mothers below the mandated minimum age are ineligible for a second child, and so their birth cohorts c are relevant. Once the regulation ends, maternal age no longer affects eligibility. For example, Shandong required mothers to be at least 30 years old for a second birth until 2013 (Table 2). Women born after 1984 were all younger than 30 in 2014 and thus no longer subject to this restriction. In such cases, we collapse c for the affected cohorts into the broader cohort bins defined in Step 2.

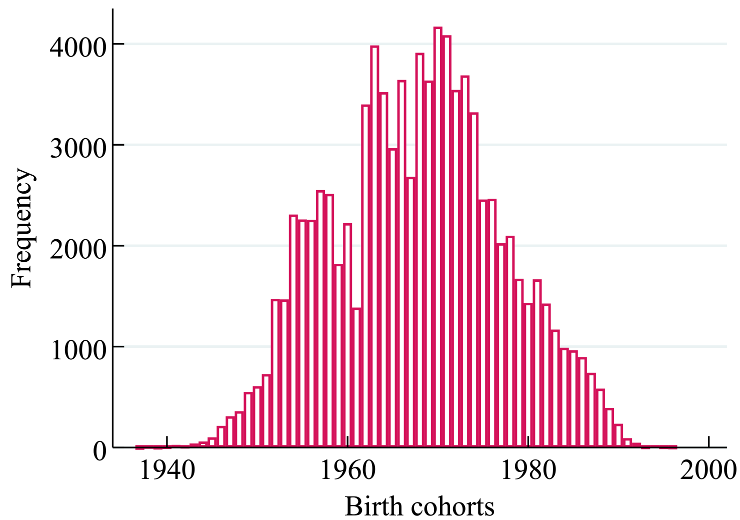
Table D1: Year of Second-Child Eligibility Across Groups and Provinces

Groups	Collapsed group	Couples' only-child status	Couples' rural hukou	1st child's gender	Anhui	Beijing	Chongqing	Fujian	Gansu	Guangdong	Guangxi	Guizhou	Hainan	Hebei	Henan	Heilongjiang	Hubei	Hunan	Jilin	Jiangsu
1	1	both	both	daughter	1985	1983	1985	1985	1984–90, 1998	1984	1985	1984	1984	1982	1984–90, 2012	1983	1985	1985	1985	1984
2	2	both	both	son	1985	1983	1985	1985	1984–90, 2003	1984	1985	1984	1984	1982	1984–90, 2012	1983	1985–88, 2003	1985	1985	1984
3	3	both	only wife	daughter	1985	1983	1985	1985	2003	1984	1985	1984	1984	1985	2012	1983	1985–88, 1988	1985	1985	1984
4	3	both	only wife	son	1985	1983	1985	1985	2003	1984	1985	1984	1984	1985	2012	1983	1985–88, 2003	1985	1985	1984
5	4	both	only husband	daughter	1985	1985	1985	1985	2003	1984	1985	1984	1984	1985	2012	1985	2003	1985	1985	1984
6	4	both	only husband	son	1985	1985	1985	1985	2003	1984	1985	1984	1984	1985	2012	1985	1985–88, 2003	1985	1985	1984
7	5	both	neither	daughter	1985	1985	1985	1985	2003	1984	1985	1984	1984	1985	2012	1985	2003	1985	1985	1984
8	6	both	neither	son	1985	1985	1985	1985	2003	1984	1985	1984	1984	1985	2012	1985	1985–88, 2003	1985	1985	1984
9	7	either	both	daughter	1985	1985–91, 2014	2014	2003	1998	1986	1985	1999	1986–90, 2005	1995	2012	1985–90, 2000	1988	1987	1993	1991
10	8	either	both	son	1985	1985–91, 2014	2014	2003	2014	1986–99, 2014	1985–89, 2014	2014	1986–90, 2005	2014	2014	1985–90, 2014	2014	2014	2003	1991
11	9	either	only wife	daughter	2014	1985–91, 2014	2014	2014	2014	2014	1985	2014	1986–90, 2005	2014	2014	1985–90, 2000	1988	2014	2014	1991
12	10	either	only wife	son	2014	1985–91, 2014	2014	2014	2014	2014	1985–89, 2014	2014	1986–90, 2005	2014	2014	1985–90, 2014	2014	2014	2014	1991
13	11	either	only husband	daughter	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	1985–90, 2014	2014	2014	2014	2014
14	11	either	only husband	son	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	1985–90, 2014	2014	2014	2014	2014
15	11	either	neither	daughter	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	1985–90, 2014	2014	2014	2014	2014
16	11	either	neither	son	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	1985–90, 2014	2014	2014	2014	2014
17	12	neither	both	daughter	1988	2016	2016	2003	1998	1986	1989	1999	1986–90, 2005	1995	2012	2000	1988	1987	1993	2016
18	13	neither	both	son	2016	2016	2016	2016	2016	2017	1989	2016	1986–90, 2005	2016	2016	2000	1988	2016	2016	2016
19	14	neither	only wife	daughter	2016	2016	2016	2016	2016	1986–99, 2017	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016
20	15	neither	only wife	son	2016	2016	2016	2016	2016	2017	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016
21	15	neither	only husband	daughter	2016	2016	2016	2016	2016	2017	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016
22	15	neither	only husband	son	2016	2016	2016	2016	2016	2017	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016
23	15	neither	neither	daughter	2016	2016	2016	2016	2016	2017	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016
24	15	neither	neither	son	2016	2016	2016	2016	2016	2017	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016

Groups	Collapsed group	Couples' only-child status	Couples' rural hukou	1st child's gender	Jiangxi	Liaoning	Inner Mongolia	Ningxia	Qinghai	Shandong	Shanxi	Shaanxi	Shanghai	Sichuan	Tianjin	Tibet	Xinjiang	Yunnan	Zhejiang
1	1	both	both	daughter	1983	1985	1989	1980	1986	1983	1983	1987	1985	1985	1983	1980	1980	1991	1985
2	2	both	both	son	1983	1985	2003	1980	1986	1983	1983	1987	1985	1985	1983	1980	1980	1991	1985
3	3	both	only wife	daughter	1990	1985	2003	1980	1986	1983	1983	1987	1985	1985	1983	1980	1980	1991	1985
4	3	both	only wife	son	1990	1985	2003	1980	1986	1983	1983	1987	1985	1985	1983	1980	1980	1991	1985
5	4	both	only husband	daughter	1990	1985	2003	1980	1986	1984	1987	1987	1985	1985	1985	1980	1992	1991	1985
6	4	both	only husband	son	1990	1985	2003	1980	1986	1984	1987	1987	1985	1985	1985	1980	1992	1991	1985
7	5	both	neither	daughter	1990	1985	2003	1987	1986	1984	1987	1987	1985	1985	1985	1992	1992	1991	1985
8	6	both	neither	son	1990	1985	2003	1987	1986	1984	1987	1987	1985	1985	1985	1992	1992	1991	1985
9	7	either	both	daughter	1985	1985	1989	1980	2014	1986	1987	2003	1991	2014	1983	1980	1980	2014	2003
10	8	either	both	son	2014	1985	2014	1980	2014	2014	1987–2000, 2014	2014	1991	2014	1983	1980	1980	2014	2014
11	9	either	only wife	daughter	2014	1985	2014	1980	2014	1986	1987	2014	1991	2014	1983	1980	1980	2014	2014
12	10	either	only wife	son	2014	1985	2014	1980	2014	2014	1987–2000, 2014	2014	1991	2014	1983	1980	1980	2014	2014
13	11	either	only husband	daughter	2014	2014	2014	1980	2014	2014	2014	2014	2014	2014	2014	1980	2014	2014	2014
14	11	either	only husband	son	2014	2014	2014	1980	2014	2014	2014	2014	2014	2014	2014	1980	2014	2014	2014
15	11	either	neither	daughter	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014
16	11	either	neither	son	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014
17	12	neither	both	daughter	1985	1988	1989	1980	2016	1986	1990	2003	2016	2016	2016	1980	1980	2016	2003
18	13	neither	both	son	2016	1988	2016	1980	2016	1986	1990	2016	2016	2016	2016	1980	1980	2016	2016
19	14	neither	only wife	daughter	2016	2016	2016	1980	2016	2016	2016	2016	2016	2016	2016	1980	1980	2016	2016
20	15	neither	only wife	son	2016	2016	2016	1980	2016	2016	2016	2016	2016	2016	2016	1980	1980	2016	2016
21	15	neither	only husband	daughter	2016	2016	2016	1980	2016	2016	2016	2016	2016	2016	2016	1980	2016	2016	2016
22	15	neither	only husband	son	2016	2016	2016	1980	2016	2016	2016	2016	2016	2016	2016	1980	2016	2016	2016
23	15	neither	neither	daughter	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016
24	15	neither	neither	son	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016

Note: This table reports, for each province, the year in which each group obtained second-child eligibility and the grouping strategies used to reduce the number of groups. Column 1 defines 24 mutually exclusive groups based on couples' only-child status, hukou status, and the gender of the first child: {*both spouses only children, either, neither*} × {*both rural hukou, wife only, husband only, neither*} × {*firstborn daughter, firstborn son*}. Column 2 indicates the collapsed groups to which each original group belongs.

Figure D1 : Distribution of Wives' Birth Cohorts in the Estimation Sample



Note: The regression sample comprises couples with one child and wives aged 22–45.
Data sources: CFPS and authors' calculations.

D2. Exogeneity of Early Relaxation Timing

We next investigate whether the timing of early OCP relaxations was systematically related to provincial characteristics. Using provincial panel data for 1980–2012, we estimate linear probability models in which the dependent variable is a binary indicator that switches from zero to one in the year a province introduces an early relaxation and remains one thereafter. Column (1) of Table D2 uses an indicator for the adoption of the STCP (both); column (2) uses an indicator for the adoption of the One-and-a-Half-Child Policy.

Explanatory variables include GDP per capita, the shares of primary and tertiary sectors in GDP, fiscal expenditure per capita, crude birth and death rates, total population, the rural population share, and the female population share. All specifications include province and year fixed effects.

The estimates show that none of these covariates significantly predicts the timing of early relaxations, either individually or jointly. The F-tests reported at the bottom of Table D2 yield large p-values (0.738 and 0.836), indicating that we cannot reject the null that provincial characteristics are unrelated to the adoption of early relaxations.

This evidence supports the interpretation that early relaxation assignments were largely driven by central policy decisions rather than by time-varying local conditions.

Table D2: Determinants of Early OCP Relaxation Assignments

	Early relaxations	
	STCP (both)	One-and-a-Half-Child Policy
	(1)	(2)
GDP per capita (RMB)	–5.558	3.098
	(3.422)	(3.997)
Primary industry share of GDP (%)	0.069	0.163
	(0.174)	(0.115)
Tertiary industry share of GDP (%)	–0.040	0.113
	(0.105)	(0.115)
Fiscal expenditure per capita (thousand RMB)	–6.960	0.083
	(8.960)	(7.826)
Birth rate (per thousand people)	0.926	1.369
	(2.467)	(2.314)
Death rate (per thousand people)	–0.942	–1.488
	(2.708)	(2.430)
Total population (100 millions)	0.019	–0.056
	(0.068)	(0.041)
Rural population share (%)	–0.002	–0.005
	(0.008)	(0.011)
Female population share (%)	0.020	–0.010
	(0.077)	(0.082)
Province fixed effects	✓	✓
Year fixed effects	✓	✓
R^2	0.235	0.068
N (Province-years)	1,023	1,023
F-test: (p-value)	0.738	0.836

Note: This table examines the determinants of early OCP relaxation assignments using a balanced provincial panel for 1980–2012. The dependent variables are binary indicators that equal 1 from the year of policy introduction onward and 0 otherwise. Column (1) refers to the STCP (both) for couples in which both spouses are only children; column (2) refers to the One-and-a-Half-Child Policy for rural couples with a firstborn daughter. Explanatory variables include GDP per capita (RMB), the share of primary and tertiary sectors in GDP, fiscal expenditure per capita (thousand RMB), crude birth and death rates (per thousand people), total population (100 million), the rural population share, and the female population share.
Data source: Province-level covariates are from the *China Statistical Yearbook*.

D3. Alternative, Simpler Research Designs: Before–After OLS and Sibling-Based DID

For comparison with our baseline cell-based TWFE estimates, we consider two simpler identification strategies that have been used in the literature and show that they yield weaker and less stable results. This exercise underscores the importance of using comprehensive eligibility rules and the full staggered variation.

Before–After OLS comparisons. — We first estimate OLS models that compare second-birth probabilities before and after the 2014 and 2016 nationwide relaxations. Let $Yr2014\_2021_t$ be an indicator equal to one for calendar years 2014–2021 and zero otherwise, and let $Yr2016\_2021_t$ be an indicator equal to one for 2016–2021. We estimate:

$$\begin{aligned} 2ndBirth_{i,pd,t+1} &= \alpha + \beta_1 Yr2014\_2021_t + \beta_2 1stBirthThisYear_{it} + \boldsymbol{X}_{it}\boldsymbol{\gamma} \\ &\quad + \phi_{pd} + \omega_p t + \epsilon_{i,pd,t}, \qquad (1) \\ 2ndBirth_{i,pd,t+1} &= \alpha + \beta_1 Yr2014\_2021_t + \beta_2 1stBirthThisYear_{it} \\ &\quad + \beta_3 Yr2016\_2021_t + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \phi_{pd} + \omega_p t + \epsilon_{i,pd,t}, \qquad (2) \end{aligned}$$

where $2ndBirth_{i,pd,t+1}$ is an indicator for a second birth in year $t+1$, $\boldsymbol{X}_{it}$ contains the same set of controls as in Equation (4), ϕ_{pd} denotes fixed effects for province $\times$ sibling-status group (both spouses only children, one only child, neither), and $\omega_p t$ is a province-specific linear time trend. Standard errors are clustered at the province $\times$ sibling-status group level.

Table D3 reports results for four sample windows: 1980–2021, 1995–2021, 2005–2021, and 2012–2021. In the full sample and the more recent windows, second-birth probabilities are higher after 2014, while the 1995–2021 sample shows no noticeable change. When we separate 2016 using Equation (2), the estimated effect of the 2016 relaxation is close to zero or even negative in the longer windows and becomes positive and significant only in the short 2012–2021 window. The sign and magnitude of the estimates thus depend heavily on the chosen sample period, illustrating the sensitivity and limited interpretability of simple before–after comparisons.

Sibling-based difference-in-differences. — Following Ge and Shi (2024) and Wu (2022), we also estimate DID models that define treatment and control groups using only the spouses' sibling status. Couples in which either spouse is an only child obtained second-child eligibility in 2014, while eligibility expanded in 2016 to couples in which neither spouse is an only child. Let $Eligible_{it}(Sib_{pd})$ be an indicator equal to one if couple i in province p and sibling group d is eligible for a second child in year t under this rule, and zero otherwise. We estimate:

$$\begin{aligned} 2ndBirth_{i,pd,t+1} = \alpha &+ \beta_1 Eligible_{it}(Sib_{pd}) + \beta_2 1stBirthThisYear_{it} \\ &+ \beta_3 Eligible_{it}(Sib_{pd}) \times 1stBirthThisYear_{it} \\ &+ \boldsymbol{X}_{it}\boldsymbol{\gamma} + \phi_{pd} + \omega_p t + \lambda_t + \epsilon_{i,pd,t}, \end{aligned} \tag{3}$$

and a specification that distinguishes couples who become eligible in 2014 from those who become eligible in 2016:

$$\begin{aligned} 2ndBirth_{i,pd,t+1} = \alpha &+ \beta_1 Eligible2014_{it}(Sib_{pd}) + \beta_2 1stBirthThisYear_{it} \\ &+ \beta_3 Eligible2014_{it}(Sib_{pd}) \times 1stBirthThisYear_{it} \\ &+ \beta_4 Eligible2016_{it}(Sib_{pd}) \\ &+ \beta_5 Eligible2016_{it}(Sib_{pd}) \times 1stBirthThisYear_{it} \\ &+ \boldsymbol{X}_{it}\boldsymbol{\gamma} + \phi_{pd} + \omega_p t + \lambda_t + \epsilon_{i,pd,t}, \end{aligned} \tag{4}$$

where $Eligible2014_{it}(Sib_{pd})$ equals one from 2014 onward for couples in which either spouse is an only child, and $Eligible2016_{it}(Sib_{pd})$ equals one from 2016 onward for couples in which neither spouse is an only child. As before, ϕ_{pd} denotes province $\times$ sibling-status fixed effects, λ_t year fixed effects, and $\omega_p t$ province-specific linear time trends; standard errors are clustered at the province $\times$ sibling-status group level.

Table D4 reports the estimates for the 1980–2021 and 1995–2021 samples. Whether we collapse eligibility into a single treatment indicator or separate the 2014 and 2016 relaxations, the estimated effects of eligibility based on sibling status are small and statistically insignificant. Because this design ignores other exemption criteria (e.g., hukou type, sex of the first child, birth interval, maternal age), many couples are misclassified as treated or untreated, generating severe attenuation bias.

Taken together, the before–after OLS and sibling-based DID designs highlight the value of our richer identification strategy, which reconstructs precise couple-year eligibility using the full set of policy rules and exploits the staggered expansion across *pdbc* cells.

Table D3: OLS Before–After Comparisons Around the 2014 and 2016 Relaxations

	Per-year second-birth probability							
	1980–2021		1995–2021		2005–2021		2012-2021	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Average effect of the 2014 and 2016 relaxations								
Yr2014_2021	0.027***	0.026***	–0.003	–0.005	0.014***	0.008	0.016***	0.018***
	(0.007)	(0.006)	(0.007)	(0.007)	(0.005)	(0.006)	(0.006)	(0.007)
1stBirthThisYear	–0.085***	–0.082***	–0.059***	–0.052***	–0.081***	–0.073***	–0.093***	–0.093***
	(0.013)	(0.013)	(0.008)	(0.006)	(0.014)	(0.012)	(0.017)	(0.016)
B. Effects of the 2014 and 2016 relaxations								
Yr2014_2021	0.027***	0.027***	0.001	–0.000	0.014***	0.009*	0.015***	0.022***
	(0.006)	(0.005)	(0.006)	(0.006)	(0.004)	(0.005)	(0.005)	(0.008)
Yr2016_2021	–0.001	–0.002	–0.008	–0.010*	–0.001	–0.004	0.006	0.016*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.009)
1stBirthThisYear	–0.085***	–0.082***	–0.059***	–0.052***	–0.081***	–0.073***	–0.093***	–0.094***
	(0.013)	(0.013)	(0.008)	(0.006)	(0.014)	(0.012)	(0.017)	(0.016)
Age group dummies	✓	✓	✓	✓	✓	✓	✓	✓
Detailed controls		✓		✓		✓		✓
Province $\times$ SibGroup fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Province-specific linear time trends		✓		✓		✓		✓
N (Couple-years)	91,268	91,268	63,602	63,602	32,356	32,356	12,238	12,238

Note: This table presents simple before–after comparisons for the 2014 STCP (either) and the 2016 UTCP. The dependent variable is an indicator for having a second birth in year $t+1$. *Yr2014_2021* equals 1 for years 2014–2021; *Yr2016_2021* equals 1 for years 2016–2021. The sample consists of couples with one child, where the wife is aged 22–45 and the husband 22–60. Columns 1–2 use the full 1980–2021 sample; columns 3–4, 5–6, and 7–8 restrict to 1995–2021, 2005–2021, and 2012–2021, respectively. Panel A reports specifications with a single post-2014 indicator, while Panel B includes separate indicators for 2014–2021 and 2016–2021. $Province \times SibGroup$ fixed effects control for interactions between hukou province and sibling-status groups (both only children, one only child, neither only child). Other controls follow Table 4. Standard errors, in parentheses, are clustered at province $\times$ sibling-status group level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D4: Difference-in-Differences Based on Variation in Sibling Status

	Per-year second-birth probability					
	1980–2021			1995–2021		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Average effect of the 2014 and 2016 relaxations						
Eligible (sib)	0.012	0.009	–0.004	0.006	0.007	–0.002
	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.011)
1stBirthThisYear	–0.088***	–0.084***	–0.084***	–0.059***	–0.052***	–0.052***
	(0.013)	(0.013)	(0.014)	(0.008)	(0.007)	(0.007)
Eligible (sib) × 1stBirthThisYear	0.004	0.018	0.019	–0.016	–0.006	–0.009
	(0.033)	(0.032)	(0.033)	(0.032)	(0.032)	(0.033)
B. Effects of the 2014 and 2016 relaxations						
Eligible (sib) in 2014	0.006	0.004	0.003	0.003	0.002	0.002
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
Eligible (sib) in 2016	0.008	0.012	0.014	0.013	0.012	0.014
	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
1stBirthThisYear	–0.088***	–0.084***	–0.084***	–0.059***	–0.052***	–0.052***
	(0.013)	(0.013)	(0.013)	(0.008)	(0.006)	(0.006)
Eligible (sib) in 2014×1stBirthThisYear	0.000	0.015	0.019	–0.022**	–0.011	–0.009
	(0.011)	(0.015)	(0.014)	(0.008)	(0.012)	(0.011)
Eligible (sib) in 2016×1stBirthThisYear	0.039	0.055	0.061	0.025	0.037	0.033
	(0.120)	(0.115)	(0.116)	(0.118)	(0.115)	(0.115)
Age group dummies	✓	✓	✓	✓	✓	✓
Detailed controls		✓	✓		✓	✓
Province × SibGroup fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Province-specific linear time trends			✓			✓
N (Couple-years)	91,268	91,268	91,268	63,602	63,602	63,602

Note: This table estimates DID models that define treatment and control groups using the spouses' sibling status, with Panel A treating eligibility as a single treatment and Panel B differentiating between eligibility in 2014 and 2016. The dependent variable is an indicator for having a second birth in year $t + 1$. In Panel A, *Eligible(sib)* equals 1 from 2014 onward for couples in which either spouse is an only child and from 2016 onward for couples in which neither spouse is an only child, and 0 otherwise. In Panel B, *Eligible(sib)_2014* equals 1 from 2014 onward for couples in which either spouse is an only child; *Eligible(sib)_2016* equals 1 from 2016 onward for couples in which neither spouse is an only child. The sample consists of couples with one child, where the wife is aged 22–45 and the husband 22–60, for 1980–2021 (columns 1–3) and 1995–2021 (columns 4–6). We include *Province × SibGroup* fixed effects (interactions of hukou province with sibling-status groups: both only children, one only child, neither only child). Other controls follow Table 4. Standard errors, in parentheses, are clustered at the province × sibling-status group level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

E. Additional Robustness Analysis

This section reports additional robustness checks that complement the results in Sections V and VI.

E1. Estimates from Stylized TWFE Models

We first assess the sensitivity of our results to alternative TWFE specifications that exploit only a subset of the available variation. Our baseline model uses finely defined *pdbc* cells to capture nearly all variation in second-child eligibility and leverages cross-cell differences to identify the causal effect. As robustness checks, we consider two simplified specifications.

The first is a province-level TWFE model that uses only cross-provincial variation in OCP relaxations, as in Equation (2), including province fixed effects and province-specific linear time trends, with standard errors clustered at the provincial level. The second replaces province fixed effects with individual (couple) fixed effects and clusters standard errors at the couple level, thereby relying on within-couple changes in eligibility over time.

Appendix Table E1 shows that both specifications produce qualitatively similar but generally smaller estimates than our baseline cell-based model in Table 4. The province-level analysis yields smaller effects in the early period (1980–1994), while the individual fixed-effects analysis yields smaller effects in the later period (1995–2021). This pattern is consistent with attenuation bias arising from using treatment variation that is either too coarse (province level) or too restrictive (couple level), reinforcing the advantage of our cell-based design.

Table E1: Robustness: Province or Individual Fixed Effect

	Per-year second birth probability					
	1980–2021	1980–1994	1995–2021	1980–2021	1980–1994	1995–2021
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible	0.047***	0.051*	0.054***	0.070***	0.092***	0.069***
	(0.012)	(0.026)	(0.013)	(0.005)	(0.014)	(0.006)
1stBirthThisYear	–0.084***	–0.122***	–0.050***	–0.096***	–0.138***	–0.039***
	(0.015)	(0.026)	(0.007)	(0.004)	(0.007)	(0.005)
Eligible×1stBirthThisYear	0.009	0.030	–0.014	–0.053***	0.000	–0.105***
	(0.015)	(0.060)	(0.015)	(0.015)	(0.060)	(0.016)
Baseline controls	✓	✓	✓	✓	✓	✓
Province fixed effects	✓	✓	✓			
Individual fixed effects				✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Province-specific linear time trends	✓	✓	✓	✓	✓	✓
$\overline{Y}$	0.053	0.079	0.042	0.047	0.067	0.039
R^2	0.095	0.133	0.076	0.307	0.393	0.271
N (Couple-years)	91,268	27,666	63,602	90,508	27,150	63,358
N (Unique couples)	8,985	4,832	6,557	8,427	4,316	6,313

Note: This table compares specifications with province fixed effects (columns 1–3) and individual fixed effects (columns 4–6). The dependent variable is an indicator for having a second birth in year $t+1$. The sample sizes in columns (4)–(6) are smaller because singleton observations are excluded when estimating individual fixed effects. Standard errors are clustered at the province level in columns (1)–(3) and at the couple level in columns (4)–(6). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

E2. Alternative Definition of Second-Child Eligibility

We next examine whether our results are sensitive to using policy implementation dates instead of announcement dates when defining eligibility. The 2014 and 2016 nationwide relaxations were introduced without long advance notice (He et al., 2023), and for earlier provincial relaxations, the lag between announcement and implementation was typically short. Table E2 documents these lags for three early policies: the STCP (both) for rural couples and all couples, and the One-and-a-Half-Child Policy. The average gap between announcement and implementation dates is around 41–42 days for the STCP (both) and around 28 days for the One-and-a-Half-Child Policy, far shorter than a standard 280-day gestation period. It is therefore unlikely that couples systematically adjusted the timing of births within this window.

To verify this, we re-define eligibility using implementation dates rather than announcement dates and re-estimate Equation (4). Table E3 reports results that are nearly identical to those in Table 4. We conclude that the short gap between policy announcement and implementation does not materially affect our estimates.

Table E2: Announcement and Implementation Dates of Early Relaxations

	STCP (both)						One-and-a-Half-Child Policy		
	A. Rural couples in which both are only children			B. Couples in which both are only children			C. Firstborn daughter		
Province	Announced	Implemented	ΔDates	Announced	Implemented	ΔDates	Announced	Implemented	ΔDates
Beijing	12/2/1982	1/1/1983	30	9/11/1984	10/1/1984	20			
Tianjin	2/7/1983	2/7/1983	0	7/6/1984	9/1/1984	57			
Hebei	1/1/1982	5/1/1982	120	8/4/1984	8/6/1984	2	9/2/1994	9/2/1994	0
Shanxi	6/29/1982	12/1/1982	155	12/31/1986	1/1/1987	1	9/22/1989	1/1/1990	101
Inner Mongolia				9/27/2002	12/1/2002	65	1988 (exact date unknown)	12/8/1988	
Liaoning				9/24/1984	9/24/1984	0	5/28/1988	5/28/1988	0
Jilin				8/18/1984	8/18/1984	0	10/1/1993	10/1/1993	0
Heilongjiang	1/31/1983	1/31/1983	0	8/2/1984	8/2/1984	0	12/18/1999	2/1/2000	45
Shanghai				10/1/1984	10/1/1984	0			
Jiangsu				2/1/1984	7/1/1984	151			
Zhejiang	7/20/1984	7/20/1984	0	2/5/1985	2/9/1985	4	9/3/2002	9/3/2002	0
Anhui				8/17/1984	12/1/1984	106	10/31/1988	12/1/1988	31
Fujian				7/1/1984	11/1/1984	123	7/26/2002	9/1/2002	37
Jiangxi	1/1/1983	1/18/1983	17	6/16/1990	9/1/1990	77	11/4/1985	11/4/1985	0
Shandong	7/29/1982	7/29/1982	0	5/14/1984	5/10/1984	0	2/13/1986	2/13/1986	0
Henan	5/7/1984	5/7/1984	0	11/25/2011	11/25/2011	0	11/25/2011	11/25/2011	0
Hubei	7/2/1984	7/26/1984	24	12/1/2002	1/1/2003	31	12/19/1987	3/1/1988	73
Hunan				11/27/1984	1/1/1985	35	1/1/1987	6/6/1987	156
Guangdong				2/1/1984	6/12/1984	132	6/1/1986	6/1/1986	0
Guangxi				3/29/1985	4/3/1985	5	9/17/1988	1/1/1989	106
Hainan				2/1/1984	6/12/1984	132	6/1/1986	6/1/1986	0
Chongqing	7/1/1984	7/1/1984	0	7/1/1984	7/11/1984	10			
Sichuan	7/1/1984	7/1/1984	0	7/1/1984	7/11/1984	10			
Guizhou				5/3/1984	7/27/1984	85	7/24/1998	7/24/1998	0
Yunnan				12/22/1990	4/1/1991	100			
Tibet				5/8/1992	5/8/1992	0			
Shaanxi				7/25/1986	7/31/1986	6	9/29/2002	9/29/2002	0
Gansu	3/29/1984	4/22/1984	24	9/27/2002	9/27/2002	0	9/29/1997	9/29/1997	0
Qinghai				4/11/1986	4/17/1986	6			
Ningxia				8/28/1986	8/28/1986	0			
Xinjiang				4/7/1992	7/1/1992	85			
	ΔDates: mean=42, std=64.93			ΔDates: mean=41, std=49.86			ΔDates: mean=28, std=46.90		

Note: This table presents the official announcement date, implementation date, and the difference between them in days (ΔDates = implementation date minus announcement date) for each province and policy. Panel A refers to rural couples in which both spouses are only children; Panel B to all couples in which both spouses are only children; and Panel C to rural couples with a firstborn daughter. The last row reports the national mean and standard deviation of ΔDates for each policy.

Data sources: Provincial *Family Planning Regulations* and policy documents are available on provincial government websites.

Table E3: Robustness: Alternative Definition of Eligibility Based on Implementation Dates

	Per-year second birth probability						
	1980–2021			1980–1994		1995–2021	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Eligible	0.074***	0.073***	0.072***	0.091***	0.091***	0.072***	0.071***
	(0.006)	(0.006)	(0.005)	(0.015)	(0.015)	(0.006)	(0.006)
1stBirthThisYear	–0.092***	–0.092***	–0.088***	–0.137***	–0.132***	–0.047***	–0.042***
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.005)	(0.005)
Eligible×1stBirthThisYear	–0.010	–0.008	–0.012	–0.004	–0.005	–0.042**	–0.049***
	(0.014)	(0.014)	(0.014)	(0.054)	(0.054)	(0.017)	(0.017)
Age group dummies	✓	✓	✓	✓	✓	✓	✓
Detailed controls		✓	✓	✓	✓	✓	✓
pdbc fixed effects	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Province-specific linear time trends			✓		✓		✓
$\overline{Y}$	0.053	0.053	0.053	0.079	0.079	0.042	0.042
R^2	0.227	0.228	0.231	0.299	0.306	0.191	0.195
N (Couple-years)	91,265	91,265	91,265	27,663	27,663	63,602	63,602
N (Unique couples)	8,984	8,984	8,984	4,831	4,831	6,557	6,557
N (Clusters)	5,064	5,064	5,064	2,613	2,613	3,821	3,821

Note: The dependent variable is an indicator for having a second birth in year $t+1$. *Eligible* is constructed using policy implementation dates rather than announcement dates. Sample and controls follow Table 4. Standard errors are clustered at the province × demographic group × first child's birth year × mother's birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

E3. Alternative Cutoff Year

We also consider using the year 2000 as an alternative cutoff to separate earlier from later periods. This choice is motivated by two considerations related to policy compliance. First, as discussed in Section II.A, detailed legal guidelines for OCP enforcement were not issued in many provinces until the mid-1990s, so compliance was weaker in the earlier years. Second, non-compliance through migration was an important concern until the central government issued directives in 1995 and 1999 that strengthened fertility governance for migrants. The 1999 family-planning regulations for migrants required that: (i) migrants' fertility be jointly managed by their hukou and current residence, with the latter taking priority; (ii) migrants obtain a "Marriage and Childbearing Certificate" from their hukou locality and present it at their destination; (iii) migrants without this certificate be denied residence permits, work permits, and business licenses. These provisions made unapproved births increasingly difficult after 1999.

Table E4 reports estimates for three samples: 1980–2021, 1980–1999, and 2000–2021. The results confirm that OCP relaxations increased second births in all periods. The effect is larger in the 1980–1999 period, consistent with higher baseline fertility, while estimates for 2000–2021 are somewhat smaller in magnitude but remain sizable and statistically significant. Figure E1 shows event-study estimates for 1980–1999 and 2000–2021. In both periods, pre-treatment coefficients are flat, and post-treatment effects are persistently positive, mirroring the patterns in Figure 6.

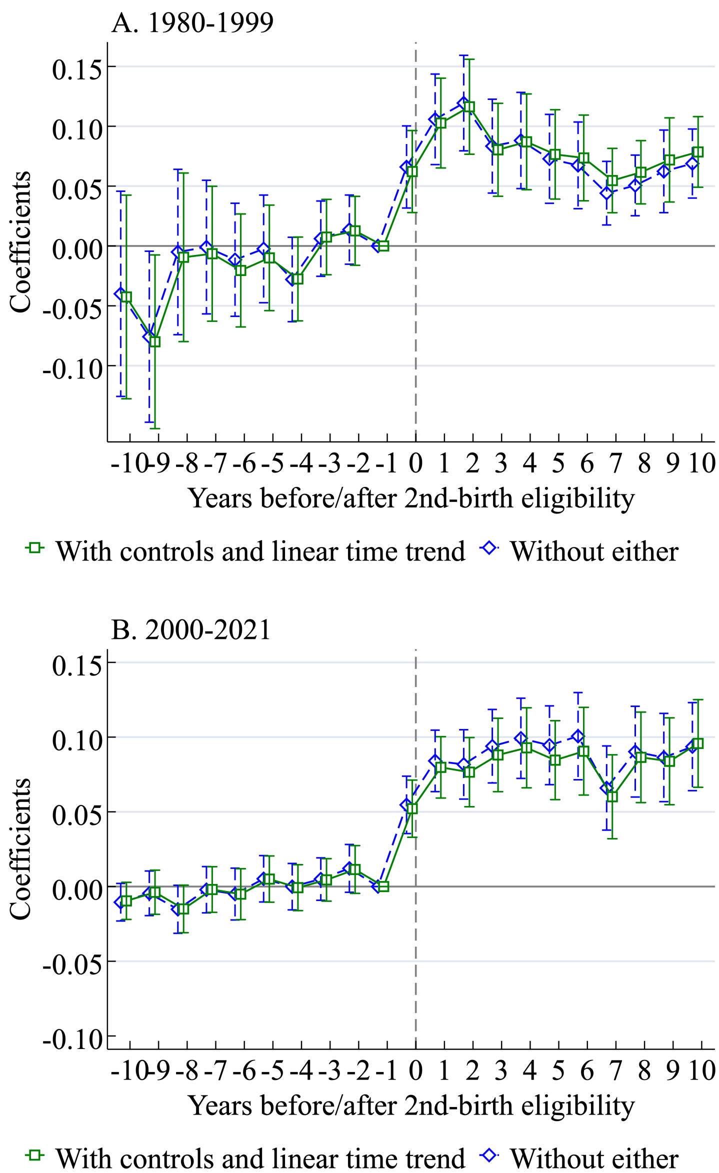
Table E4: Robustness: Using 2000 as the Cutoff Year

	Per-year second birth probability					
	1980–2021		1980–1999		2000–2021	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible	0.071***	0.070***	0.091***	0.087***	0.063***	0.060***
	(0.006)	(0.005)	(0.012)	(0.012)	(0.007)	(0.007)
1stBirthThisYear	–0.092***	–0.088***	–0.112***	–0.110***	–0.046***	–0.042***
	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)
Eligible×1stBirthThisYear	–0.007	–0.011	–0.061	–0.061	–0.030*	–0.036**
	(0.014)	(0.014)	(0.044)	(0.044)	(0.018)	(0.017)
Baseline controls	✓	✓	✓	✓	✓	✓
pdbc fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Province-specific linear time trends		✓		✓		✓
$\overline{Y}$	0.053	0.053	0.063	0.063	0.044	0.044
R^2	0.228	0.231	0.277	0.281	0.185	0.188
N (Couple-years)	91,062	91,062	42,834	42,834	48,228	48,228
N (Unique Couples)	8,975	8,975	6,145	6,145	5,587	5,587
N (Clusters)	5,056	5,056	3,367	3,367	3,038	3,038

Note: This table reports robustness checks using 2000 as the cutoff year to split the sample period. The dependent variable is an indicator for having a second birth in year $t+1$. Columns (1)–(2) use the full 1980–2021 sample; columns (3)–(4) restrict to 1980–1999; columns (5)–(6) restrict to 2000–2021. The sample construction and control variables follow Table 4. The sample size differs from Table 4 due to the exclusion of singleton observations.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure E1 : Event-Study Estimates by Period: 1980–1999 vs. 2000–2021



Note: Panels A (1980–1999) and B (2000–2021) plot coefficients from Equation (5) relative to the year before a couple first becomes eligible for a second child ($t = -1$). Solid green lines show estimates with the full set of controls and province-specific linear time trends; dashed blue lines show estimates without these adjustments. We exclude couples that ever lose second-child eligibility. Error bars indicate 95% confidence intervals. Standard errors are clustered at the province × demographic group × first child's birth year × mother's birth year level.

E4. Alternative Sample Restriction

We then explore the robustness of our estimates to alternative sample restrictions along four dimensions: the age range of wives, inter-province marriage selection, strategic hukou acquisition, and migration.

Panel A of Table E5 expands the fertility window to include wives aged 16–45 instead of 22–45. This adds observations for very young women who are typically still in education and rarely consider childbearing. The estimated effects remain close to our baseline results, though the explanatory power of the model (as measured by R^2) declines slightly.

Panel B addresses potential bias from cross-province marriages, where individuals may strategically marry someone from a province with more generous second-child exemptions. We exclude couples whose spouses are registered in different hukou provinces. The resulting estimates are similar to the baseline ones.

Panel C considers strategic hukou acquisition. Because a wife's hukou often transfers to her husband's province upon marriage, husbands' hukou are more stable and informative about potential strategic behavior. We therefore exclude couples in which the husband's current hukou differs from his childhood hukou. The estimates are again robust to this exclusion.

Panel D excludes inter-provincial migrants whose current province of residence differs from their hukou province, thereby addressing the concern that migrants may move to provinces with more lenient OCP enforcement or lower second-child fines. The estimated effect increases slightly relative to the baseline in the early years from 0.091 to 0.106, while the estimated effect of the recent period remains unchanged at 0.071. This suggests that migration to lenient provinces may modestly understate the effect in the earlier period. The unchanged effect size in recent years is consistent with tighter regulations on migrants' fertility behavior as discussed in Appendix Section A.

Table E5: Robustness: Sample Restriction Adjustments

	Per-year second birth probability					
	1980–2021		1980–1994		1995–2021	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Wife aged 16 through 45						
Eligible	0.072***	0.071***	0.092***	0.092***	0.072***	0.071***
	(0.006)	(0.005)	(0.015)	(0.015)	(0.006)	(0.006)
R^2	0.234	0.237	0.305	0.312	0.190	0.194
N (Couple-years)	93,992	93,992	29,379	29,379	64,613	64,613
B. Exclude couples whose hukou province are not matched						
Eligible	0.074***	0.072***	0.090***	0.090***	0.073***	0.071***
	(0.006)	(0.006)	(0.015)	(0.015)	(0.006)	(0.006)
R^2	0.230	0.233	0.300	0.306	0.191	0.195
N (Couple-years)	89,217	89,217	27,306	27,306	61,911	61,911
C. Exclude couples whose hukou province are changed						
Eligible	0.074***	0.073***	0.090***	0.090***	0.074***	0.073***
	(0.006)	(0.006)	(0.016)	(0.016)	(0.006)	(0.006)
R^2	0.233	0.236	0.302	0.309	0.194	0.198
N (Couple-years)	83,521	83,521	25,644	25,644	57,877	57,877
D. Drop migrants						
Eligible	0.074***	0.074***	0.098***	0.106***	0.072***	0.071***
	(0.006)	(0.006)	(0.018)	(0.018)	(0.006)	(0.006)
R^2	0.221	0.232	0.285	0.309	0.188	0.195
N (Couple-years)	83,441	83,441	24,322	24,322	59,119	59,119
Baseline controls	✓	✓	✓	✓	✓	✓
pdbc fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Province-specific linear time trends		✓		✓		✓

Note: The dependent variable is an indicator for having a second birth in year $t + 1$. Panel A restricts the sample to one-child couples in which the wife is aged 16–45. Panel B excludes couples with mismatched hukou registration provinces to mitigate bias from strategic marriage choice. Panel C drops couples who changed their hukou registration province, addressing potential bias from strategic migration. Panel D removes migrants who migrate from their hukou province to their current province. The control variables follow Table 4. Columns (2), (4), and (6) include province-specific linear time trends. Standard errors are clustered at the province × demographic group × first child's birth year × mother's birth year level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

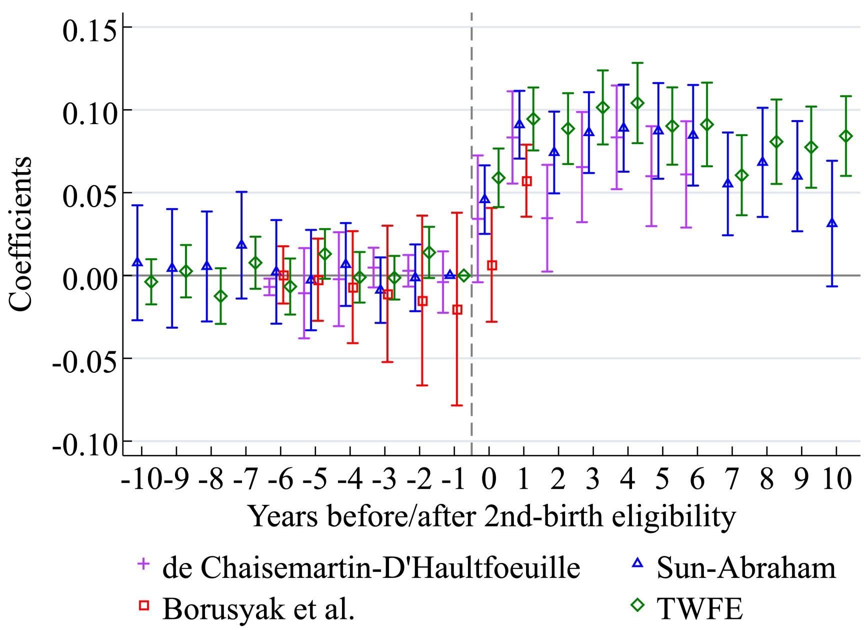
E5. Heterogeneous Treatment Effects and Alternative DID Estimators

As an additional robustness check, we address the concern that standard TWFE estimates may be biased when treatment effects are heterogeneous across groups or over time, because they implicitly compare newly treated units with already treated ones. To mitigate this concern, we replicate the event-study analysis using the alternative TWFE estimators proposed by De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), and Borusyak et al. (2024).

These estimators differ in how they construct comparison groups and in their feasible event-time coverage. For example, the estimator of De Chaisemartin and d'Haultfoeuille (2020) can identify event-time effects up to six post-treatment periods without reusing treated units as controls; Borusyak et al. (2024) allows up to two post-treatment periods; and Sun and Abraham (2021) can accommodate up to ten pre- and post-treatment periods but assumes no treatment reversals (from 1 back to 0). Accordingly, we exclude couples whose second-child eligibility was withdrawn when applying the latter estimator.

Figure E2 compares the resulting event-time coefficients with those from our TWFE specification. All three alternative estimators yield dynamic patterns that closely mirror our TWFE estimates, with flat pre-trends and persistent post-treatment effects of similar magnitude. This suggests that our main conclusions are not driven by biases associated with heterogeneous treatment effects.

Figure E2 : Event-Study Estimates Using Alternative TWFE Estimators, 1995–2021



Note: Event-time coefficients are estimated using four approaches: De Chaisemartin and d'Haultfoeuille (2020) (purple, cross markers), Sun and Abraham (2021) (blue, triangle markers), Borusyak et al. (2024) (red, square markers), and a TWFE specification mirroring Equation 5 (green, diamond markers). TWFE may be inconsistent under treatment-effect heterogeneity; the alternative estimators address this concern but differ in their feasible event-time coverage (up to 6 post-periods for De Chaisemartin and d'Haultfoeuille (2020), up to 2 for Borusyak et al. (2024), and up to 10 pre/post periods for Sun and Abraham (2021)). Couples who were always eligible or experienced an eligibility exit are excluded. Error bars indicate 95% confidence intervals. Standard errors are clustered at the province × demographic group × first child's birth year × mother's birth year level.

E6. Alternative Grouping by Relaxation Type

This subsection complements Table 5 by classifying treated couple-years using a finer set of sibling-oriented eligibility criteria. We partition eligibility into four mutually exclusive groups based on the rule that first grants second-child eligibility: (i) STCP (both), for couples in which both spouses are only children; (ii) the One-and-a-Half-Child Policy, for rural couples with a firstborn daughter; (iii) STCP (either), for couples in which only one spouse is an only child; and (iv) UTCP, for couples in which neither spouse is an only child.

Table E6 reports estimates from specifications that replace the single eligibility indicator with these four indicators.[15] The results reinforce the main message of Section VI. The largest responses are concentrated among rural couples with a firstborn daughter, while the responses associated with the nationwide expansion to couples with neither spouse is an only child are positive but more modest. Eligibility expansions tied purely to only-child-based exemptions exhibit comparatively small and imprecise effects.

[15]To maintain comparability with the baseline, specifications also include $1stBirthThisYear_{it}$ and its interaction with overall eligibility; results are unchanged when we omit this interaction in this decomposition.

Table E6: Effect Heterogeneity by Type of Relaxation

	Per-year second-birth probability					
	1980–2021		1980–1994		1995–2021	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility type (mutually exclusive):						
STCP (both): both spouses only children	0.001	–0.001	0.010	0.013	–0.015	–0.014
	(0.007)	(0.007)	(0.012)	(0.012)	(0.011)	(0.010)
One-and-a-Half-Child: rural, firstborn daughter	0.126***	0.124***	0.157***	0.151***	0.130***	0.129***
	(0.009)	(0.009)	(0.027)	(0.027)	(0.010)	(0.010)
STCP (either): one spouse only child	0.011**	0.012**	0.022*	0.037***	0.010	0.010
	(0.005)	(0.005)	(0.012)	(0.012)	(0.006)	(0.006)
UTCP: neither spouse only child	0.032***	0.032***			0.035***	0.033***
	(0.008)	(0.008)			(0.009)	(0.009)
1stBirthThisYear	–0.092***	–0.089***	–0.136***	–0.132***	–0.048***	–0.043***
	(0.004)	(0.004)	(0.008)	(0.008)	(0.005)	(0.005)
Eligible× 1stBirthThisYear	–0.004	–0.008	–0.020	–0.019	–0.036**	–0.043**
	(0.014)	(0.014)	(0.054)	(0.054)	(0.017)	(0.017)
Baseline controls	✓	✓	✓	✓	✓	✓
pdbc fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Province-specific linear time trends		✓		✓		✓
$\overline{Y}$	0.053	0.053	0.079	0.079	0.042	0.042
R^2	0.231	0.234	0.300	0.307	0.194	0.198
N (Couple-years)	91,268	91,268	27,666	27,666	63,602	63,602
N (Unique couples)	8,985	8,985	4,832	4,832	6,557	6,557
N (Clusters)	5,065	5,065	2,614	2,614	3,821	3,821

Note: The dependent variable is an indicator for having a second birth in year $t + 1$. The four eligibility-type indicators are mutually exclusive by construction and correspond to the exemption rule that first grants second-child eligibility. Specifications follow Table 4. Standard errors, in parentheses, are clustered at the province × demographic group × first child's birth year × mother's birth year level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

E7. Supplementary Tables for Heterogeneity and Construction of Regional Characteristics

Tables E7 and E8 report the regression estimates underlying the heterogeneity patterns summarized in Figures 7. In each case, we interact $Eligible_{it}$ with a mutually exclusive set of subgroup indicators for the dimension of interest and report the implied subgroup-specific treatment effects.

Table E7 presents heterogeneity by couples' demographic characteristics, including each spouse's education, the wife's hukou status and age, interviewer-rated appearance and intelligence, and the sex of the first child.

Table E8 examines heterogeneity by regional characteristics. We construct city-level measures from the CFPS and, for each measure, group cities into terciles based on the city-specific distribution. Specifically, the sex ratios at birth is computed by pooling the 2010–2022 adult and child samples and aggregating by city and birth cohort. Female labor force participation (FLFP) is defined as the employment rate among women aged 16–55. The gender wage gap is defined as the difference between male and female mean wages, normalized by the male mean. Both FLFP and the wage-gap measure are constructed from the 2010–2022 adult waves.

To proxy local gender norms, we use the 2014 and 2020 CFPS waves. Respondents rate agreement on a five-point scale with: (i) "Men should prioritize their careers, while women should prioritize their families," and (ii) "Men should take on half of the household chores." We define city-level gender-inequality measures as the share of respondents who answer 4–5 to statement (i) and 1–2 to statement (ii), and assign cities to terciles accordingly. The city-level male share of housework is constructed analogously from the CFPS and terciled.

For ease of interpretation, terciles are ordered so that "high" corresponds to a higher value of the underlying measure (e.g., a higher sex ratio at birth, higher FLFP, a wider gender wage gap, more traditional attitudes, or a higher male share of housework, depending on the variable). We proxy OCP stringency using historical province-level fines for unauthorized births (Ebenstein, 2010) and group provinces into terciles based on average fines over 1978–1995. Fine-rate data are compiled by (Ebenstein, 2010); available at `https://scholars.huji.ac.il/avrahamebenstein/links/fine-rates-one-child-policy` (last accessed December 17, 2025).

All specifications follow the baseline controls and fixed effects used in Table 4, and we cluster standard errors at the $p \times d \times b \times c$ level.

Table E7: Effect Heterogeneity by Demographic Characteristics

	1980–1994		1995–2021	
	(1)		(2)	
1. Wife's education				
College or above	0.064	(0.071)	0.068***	(0.009)
High school	0.081**	(0.037)	0.061***	(0.009)
Middle school	0.072***	(0.025)	0.067***	(0.007)
Primary or below	0.103***	(0.019)	0.081***	(0.008)
2. Husband's education				
College or above	0.061*	(0.035)	0.067***	(0.008)
High school	0.122***	(0.042)	0.064***	(0.009)
Middle school	0.076***	(0.021)	0.067***	(0.008)
Primary or below	0.097***	(0.022)	0.085***	(0.009)
3. Wife's hukou				
Urban hukou	–0.004	(0.015)	0.054***	(0.007)
Rural hukou	0.103***	(0.017)	0.078***	(0.007)
4. Wife's age				
Age22–29	0.111***	(0.021)	0.081***	(0.008)
Age30–34	0.125***	(0.022)	0.087***	(0.008)
Age35–45	0.035***	(0.013)	0.047***	(0.006)
5. Wife's interviewer-assessed appearance				
High appearance rating	0.062**	(0.025)	0.049***	(0.007)
Lower appearance rating	0.092***	(0.021)	0.087***	(0.009)
6. Wife's interviewer-assessed intelligence				
Above-mean rating	0.081***	(0.025)	0.053***	(0.007)
Below-mean rating	0.096***	(0.023)	0.089***	(0.011)
7. Sex of the first child				
Firstborn daughter	0.125***	(0.022)	0.091***	(0.007)
Firstborn son	0.039***	(0.014)	0.039***	(0.007)
Baseline controls	✓		✓	
pdbc fixed effects	✓		✓	
Year fixed effects	✓		✓	
R^2	0.30		0.19	
N (Couple-years)	27,666		63,602	

Note: This table reports the subgroup-specific treatment effects underlying Panel A of Figure 7. For each characteristic, we estimate a specification that interacts $Eligible_{it}$ with mutually exclusive subgroup indicators and reports the implied treatment effects by subgroup. The dependent variable is an indicator for having a second birth in year $t+1$. The sample and controls follow Table 4. Standard errors, in parentheses, are clustered at the province × demographic group × first child's birth year × mother's birth year level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table E8: Heterogeneity by Regional Characteristics

	1980–1994		1995–2021	
	(1)		(2)	
1. Sex ratio at birth (city terciles)				
High sex ratio	0.133***	(0.039)	0.085***	(0.008)
Medium sex ratio	0.090***	(0.024)	0.074***	(0.008)
Low sex ratio	0.073***	(0.019)	0.052***	(0.007)
2. Female labor force participation (city terciles)				
Low FLFP	0.168***	(0.039)	0.086***	(0.008)
Medium FLFP	0.123***	(0.029)	0.069***	(0.008)
High FLFP	0.035*	(0.020)	0.061***	(0.008)
3. Gender wage gap (city terciles)				
Wide wage gap	0.132***	(0.032)	0.093***	(0.008)
Medium wage gap	0.107***	(0.028)	0.067***	(0.008)
Low wage gap	0.064***	(0.021)	0.051***	(0.007)
4. Gender-role attitudes about the division of labor within the household (city terciles)				
Less equal division	0.132***	(0.030)	0.088***	(0.009)
Medium equal division	0.054***	(0.018)	0.062***	(0.007)
More equal division	0.106***	(0.027)	0.062***	(0.008)
5. Male housework attitudes (city terciles)				
Low share	0.086***	(0.025)	0.073***	(0.009)
Medium share	0.058***	(0.019)	0.053***	(0.008)
High share	0.108***	(0.024)	0.082***	(0.007)
6. OCP Stringency (province terciles based on fines)				
Low birth fine	0.152***	(0.031)	0.080***	(0.007)
Medium birth fine	0.040***	(0.015)	0.071***	(0.008)
High birth fine	0.076***	(0.021)	0.050***	(0.009)
Baseline controls	✓		✓	
pdbc fixed effects	✓		✓	
Year fixed effects	✓		✓	
R^2	0.30		0.19	
N (Couple-years)	27,666		63,602	

Note: This table reports subgroup-specific treatment effects underlying Panel B of Figure 7. City-level measures are constructed from CFPS waves and grouped into terciles separately for each measure; "high" denotes a higher value of the underlying measure. OCP stringency is proxied by historical province-level fines for unauthorized births and grouped into terciles. The dependent variable is an indicator for having a second birth in year $t + 1$. The sample and controls follow Table 4 and dummies for subgroup indicators in each separate regression. Standard errors, parentheses, are clustered at the province × demographic group × first child's birth year × mother's birth year level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

F. Counterfactual Fertility Trends

This appendix describes the counterfactual simulation used to quantify how post-1990 OCP relaxations reshaped fertility trends. The simulation is nontrivial because relaxations operate primarily through second-birth eligibility and, through parity transitions, alter the parity composition of women over time. We therefore simulate parity-specific fertility and the evolution of the parity distribution.

The procedure has three steps. Step 1 constructs annual female population counts by age, province, and hukou status and parity distribution in the initial year (1982). Step 2 estimates parity-specific birth probabilities from the CFPS and maps them to the simulation cell. Step 3 forward-simulates births and parity stocks under the baseline and the no relaxation counterfactual.

Step 1: Female Population Counts and Initial Parity Distribution

We use microdata from the 1982, 1990, 2000, and 2010 Population Censuses and the 2015 Mini-Census, obtained from the Survey Data Center at Jinan University. These microdata provide age, province, and fertility histories. Hukou status is available for all waves except for 1982. Because of this, we impute province-specific rural/urban shares using the 1990 Census and apply them to the 1982 parity distribution.[16]

For each census and mini-census wave, we rescale the microdata to match official population totals by province and hukou status. Specifically, we construct sampling weights as the ratio of the official population count to the corresponding microdata count within each province–hukou cell.[17]

We then generate annual counts of women aged 15–45 by age a, year t, province p, and hukou status $r \in \{\text{urban}, \text{rural}\}$, denoted $NumWomen_{a,t,p,r}$, for 1982–2021. For non-census years between 1982 and 2015, we use cohort-based linear interpolation between adjacent census waves. For years after 2015, we project cohorts forward using aging-based cohort matching and rescale to match official totals; we then interpolate annual values through 2021.

Finally, we recover the initial parity distribution in 1982:

$$NumWomen_{a,1982,k,p,r}, \qquad k \in \{0, 1, 2, 3+\},$$

[16] This approximation is plausible because large-scale rural–urban migration was still limited prior to 1990.

[17] China's administrative geography changed during the study period (e.g., Hainan separated from Guangdong in 1988 and Chongqing became a municipality in 1997). We harmonize provincial boundaries to a consistent set across all years.

which serves as the starting state for the forward simulation.

Step 2: Estimating Parity-Specific Birth Probabilities

We estimate parity-specific annual birth probabilities using the CFPS. The estimation sample includes women aged 15–45 in the 25 CFPS provinces. Because official population statistics count both married and unmarried women, we retain unmarried women in the first- and higher-order birth models.

Second births (parity one). — For women with exactly one child, we use the main-text TWFE specification in Equation 4 to predict second-birth probabilities. To construct the no-relaxation counterfactual, we set post-1990 eligibility expansions to zero when forming predicted second-birth rates.

First births and third-or-higher births (parity zero and parity two+). — First births are universally permitted and, empirically, are not affected by second-child eligibility (Table F1). Third and higher-order births were prohibited under the family-planning regime until 2021, rendering second-child eligibility irrelevant.

Accordingly, for parity zero and parity two+, we estimate:

$$NewBirth_{i,pd,t+1} = \alpha + \boldsymbol{X}_{it}^{w} \times \gamma + \alpha_{pd} + \theta_t + \delta_{i,pd,t}, \tag{1}$$

where $NewBirth_{i,pd,t+1}$ is an indicator for couple i having a birth in year $t+1$, $\boldsymbol{X}_{it}^{w}$ includes age, education, birth-cohort, hukou status, and (current) urban residence, α_{pd} are province-by-demographic-group fixed effects, and θ_t are year fixed effects. We estimate Equation (1) on childless women for first births and on women with at least two children for third-or-higher births.

Mapping to simulation cells. — We aggregated predicted individual birth probabilities to the simulation-cell level (age $\times$ year $\times$ province $\times$ hukou $\times$ parity). Because some fine cells are empty in the CFPS, we impute missing cell rates using coarser bins defined by region, broad age groups, multi-year periods, and hukou status. This yields a complete set of parity-specific fertility rates $FR_{a,t,k,p,r}$ for all cells required in Step 3.

Table F1: Effect of Second-Child Eligibility on First Births

	Per-year first-birth probability						
	1980–2021			1980–1994		1995–2021	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Eligible	0.009	0.011	-0.034	0.032	0.003	-0.015	-0.058
	(0.038)	(0.039)	(0.049)	(0.066)	(0.085)	(0.084)	(0.093)
Age-group dummies	✓	✓	✓	✓	✓	✓	✓
Detailed controls		✓	✓	✓	✓	✓	✓
pd fixed effects	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Province-specific linear time trends			✓		✓		✓
R^2	0.36	0.36	0.36	0.36	0.36	0.35	0.35
N (Couple–years)	3496	3496	3496	1976	1976	1662	1662

Note: The sample consists of childless couples in which the wife is aged 16–45 and the husband is aged 16–60. The sample is restricted to married couples because determining couples' eligibility requires information on the husband's sibling status; single women are therefore excluded from this analysis. The dependent variable equals one if woman i has a first birth in year $t+1$. *Eligible* is an indicator for whether, under the prevailing regulations in year t, couple i would be permitted to have two children based on characteristics defined prior to parity one (e.g., ethnicity, sibling status, hukou status, only-child status, and province); criteria that are undefined prior to the first birth (e.g., sex of the first child) or apply only after the first birth (e.g., birth-interval requirements) are excluded by construction. *pd* denotes province-by-demographic-group fixed effects. Standard errors, in parentheses, are clustered at the *pd* level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

F1. Step 3: Forward Simulation Under Baseline and Counterfactual

We simulate births and parity stocks forward from the initial 1982 state. Let

$$NumBirth_{a,t,k,p,r} = NumWomen_{a,t,k,p,r} \cdot FR_{a,t,k,p,r},$$

denote births generated by women of age a and parity k in year t.

Parity evolves as women age by one year and (if they give birth) move to the next parity. For $k \in \{0, 1, 2\}$, define the pre-rescaling updates:

$$\widetilde{NumWomen}_{a+1,t+1,0,p,r} = NumWomen_{a,t,0,p,r} - NumBirth_{a,t,0,p,r},$$
$$\widetilde{NumWomen}_{a+1,t+1,1,p,r} = NumWomen_{a,t,1,p,r} - NumBirth_{a,t,1,p,r} + NumBirth_{a,t,0,p,r},$$
$$\widetilde{NumWomen}_{a+1,t+1,2,p,r} = NumWomen_{a,t,2,p,r} - NumBirth_{a,t,2,p,r} + NumBirth_{a,t,1,p,r},$$
$$\widetilde{NumWomen}_{a+1,t+1,3+,p,r} = NumWomen_{a,t,3+,p,r} + NumBirth_{a,t,2,p,r}.$$

We then rescale the implied parity distribution to match the Census-based totals in each $(a+1, t+1, p, r)$ cell:

$$NumWomen_{a+1,t+1,k,p,r} = \widetilde{NumWomen}_{a+1,t+1,k,p,r} \cdot \frac{NumWomen_{a+1,t+1,p,r}}{\sum_{k'} \widetilde{NumWomen}_{a+1,t+1,k',p,r}}.$$

We iterate these steps through 2021.

We conduct the simulation under (i) the baseline, using the observed sequence of eligibility expansions when predicting second-birth rates, and (ii) a no-relaxation counterfactual, in which post-1990 eligibility expansions are set to zero. The resulting simulated births and age-specific fertility rates are aggregated into parity-specific fertility series and TFR series, reported in Figure 8.

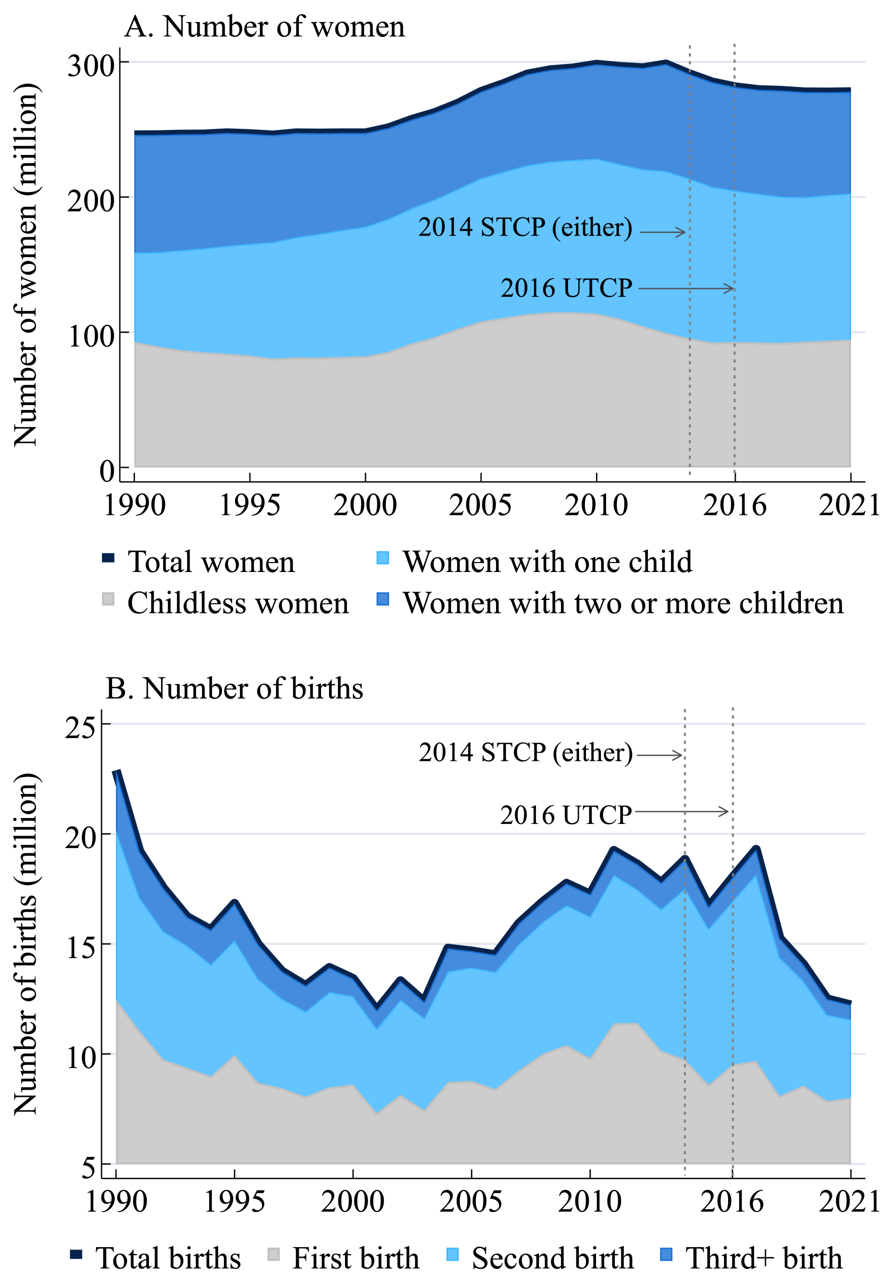
F2. Validation

Figure F3 benchmarks the simulated baseline TFR against two reference series: TFR constructed directly from CFPS birth histories and the World Bank series. The close alignment across the study period supports the reliability of the simulation.

F3. Births Attributable to Relaxations

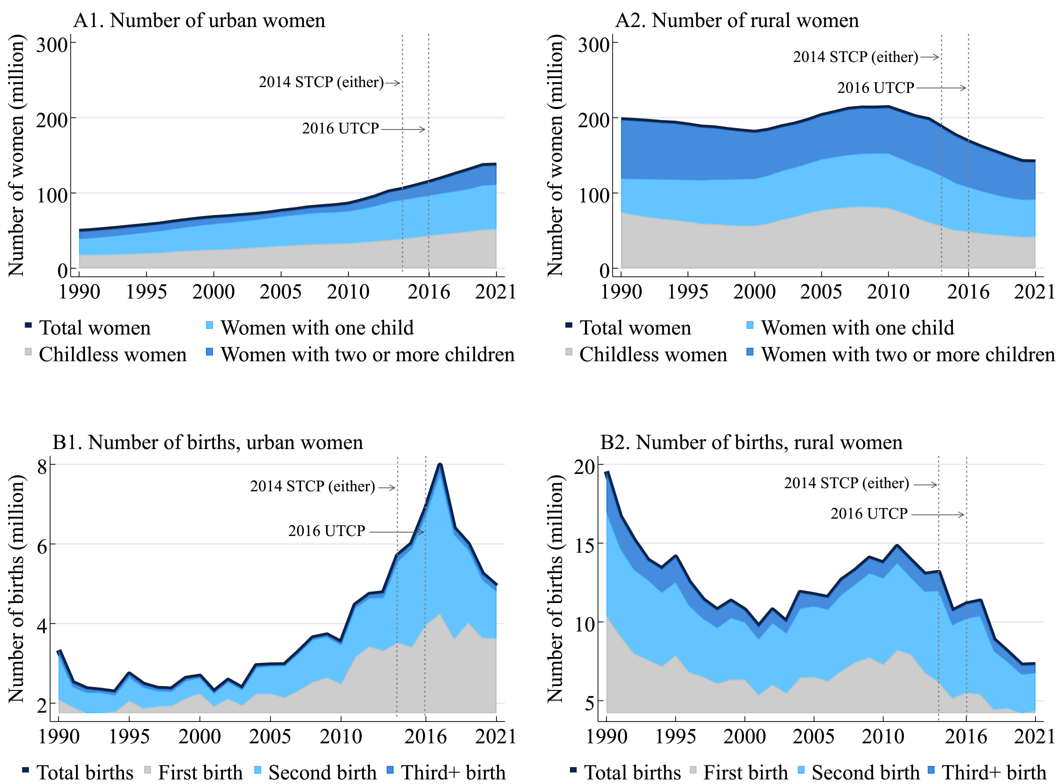
To quantify demographic magnitude, we apply baseline and counterfactual fertility rates to the Census-based female population counts and compute annual births. Figure F5 reports the resulting birth series and their difference.

Figure F1 : Women and Births by Parity, 1990–2021



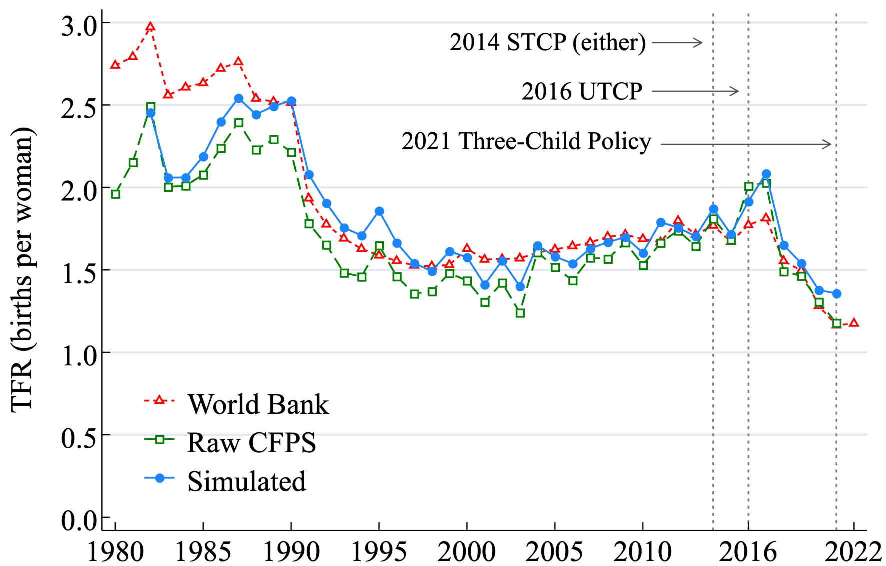
Note: Panel A plots the simulated number of women aged 15–45 by parity (0, 1, and 2+ children). Panel B plots simulated births by birth order (first, second, and third or higher). Counts combine Census-based female population totals with parity-specific birth probabilities estimated from the CFPS. Vertical lines mark major nationwide relaxations (2014 STCP (either), 2016 UTCP, and 2021 Three-Child Policy).

Figure F2 : Women and Births by Parity and Hukou, 1990–2021



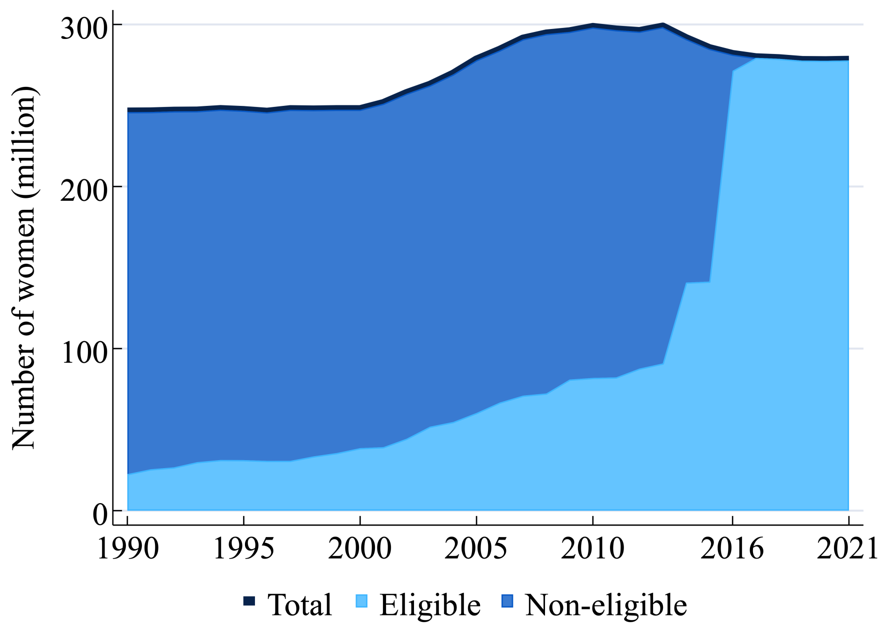
Note: The top row plots the simulated number of women, and the bottom row plots births by parity. Panels on the left (right) restrict to women with urban (rural) hukou. Vertical lines mark major nationwide relaxations (2014 STCP (either), 2016 UTCP, and 2021 Three-Child Policy).

Figure F3 : TFR: World Bank, CFPS Raw, and Simulated Series



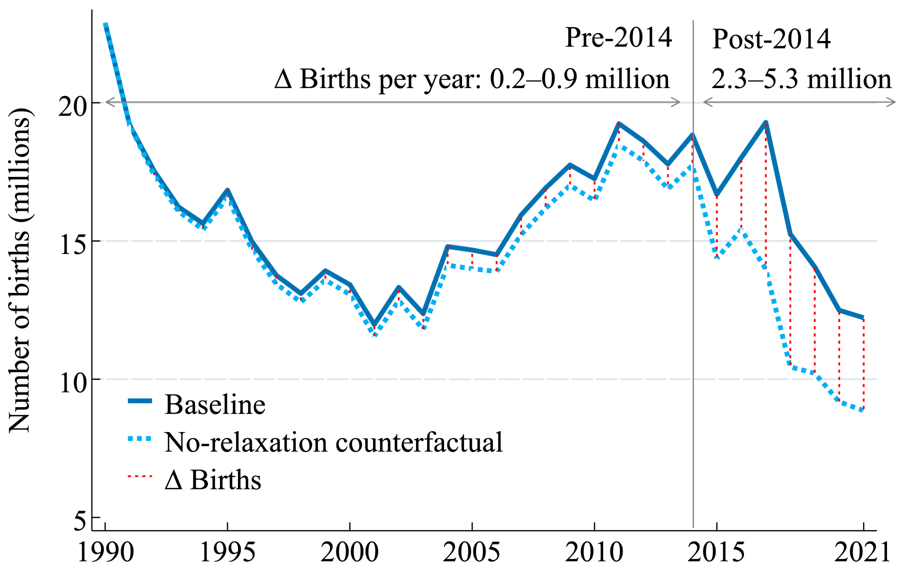
Note: The figure compares total fertility rates (TFR) from three sources: World Bank estimates, TFR constructed directly from CFPS birth histories, and the simulated baseline series combining CFPS-based parity-specific birth probabilities with Census-based population counts. Vertical lines mark major nationwide relaxations (2014 STCP (either), 2016 UTCP, and 2021 Three-Child Policy).

Figure F4 : Women with One Child by Second-Child Eligibility Status



Note: Stacked areas show the annual distribution of women with exactly one child across second-child eligibility status. The solid line plots the total number of one-child women.

Figure F5 : Annual Births under Baseline and No-Relaxation Counterfactual



Note: The figure plots annual births implied by the simulated baseline and no-relaxation counterfactual. The difference between the two lines shows additional births attributable to relaxations. Simulations combine Census-based female population counts with parity-specific birth probabilities estimated from the CFPS and causal estimates for second-birth eligibility from the TWFE model.