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Rufei Guo
Lin Lin
Junjian Yi
Hanyu Zhu

January 30, 2025

The authors declare that they have received no financial support from or held position at any organization that has interests or is relevant to this article.

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Rufei Guo[*] Lin Lin[†] Junjian Yi[‡] Hanyu Zhu[§]

April 19, 2024

Abstract

We study the intergenerational impact of birth-control policies on fertility and examine whether and how these policies persistently reduce fertility even after their abolition. We develop a model that integrates the endogeneous evolution of fertility norms. Birth-control policies, through the reduction of mothers' fertility, establish new norms that shape the fertility decisions of subsequent generations. Consequently, daughters' fertility is lower due to these new norms, even when the policies are no longer in force. To test the model's implications, we leverage a quasi-experimental setting provided by China's One-Child Policy, which was implemented from 1979 in a staggered manner and abolished in 2016. Based on the recently released 2020 population census, our results reveal that the policy has a persistent impact on reducing fertility and affects generations no longer subject to its regulations. This study is the first to provide causal evidence of birth-control policies from an intergenerational perspective, and highlights the role of norms in shaping fertility outcomes.

JEL Classification: D1,J11,J13

Key words: Birth-control Policies; Fertility; Norms; Intergenerational Spillovers

[*]Center for Health Economics and Management, Economics and Management School, Wuhan University. Email: rufei_guo@whu.edu.cn.

[†]School of Labor and Human Resources, Renmin University of China. Email: lin_lin@ruc.edu.cn.

[‡]China Center of Economic Research, National School of Development, Peking University. Email: junjian@nsd.pku.edu.cn.

[§]National School of Development, Peking University. Email: zhuhanyu@stu.pku.edu.cn.

1 Introduction

Fertility rates have decreased across countries and regions at all income levels worldwide over the past half-century, as shown in Fig. 1a. By 2021, the total fertility rate (TFR) for 110 out of 217 countries fell to or below the replacement level of 2.1.[1] This trend of low fertility presents a significant challenge for policymakers globally and has sparked extensive academic interest in exploring the causes of this long-term decline (Doepke et al., 2023). Beyond shifts in social, economic, and cultural conditions, birth-control policies have also played a role in this global fertility decline. Between 1976 and 2013, 160 out of 217 countries implemented such policies in various forms (De Silva and Tenreyro, 2017). Fig. 1b shows the share of countries that implemented such policies by income level over the past half-century. This share has decreased for upper-middle-income countries and increased for low-income countries.[2]

We ask: Have these policies continued to persistently decrease fertility even after their abolition? If so, what are the underlying reasons for this persistence? Understanding the answers to these questions is crucial for countries that had formerly implemented such policies. In Fig. 1c, we categorize upper-middle-income countries into two groups: those that never adopted birth-control policies and those that adopted and later abandoned such policies before 2007. The never-adopted group saw a TFR decrease from 4.4 in 1960 to 1.7 in 2021, while the ever-adopted group experienced a drop from 6.2 in 1960 to 1.3 in 2021. This pattern is consistent with a persistent effect of birth-control policies on fertility.

Our analysis begins with a model that integrates the endogeneous evolution of fertility norms. It examines how birth-control policies for mothers impact the fertility of their daughters and persist even after the policies are no longer in effect for daughters. In our model, a women's fertility is determined not just by the economic costs and benefits of childbearing, but also by social norms for fertility. Following the "conformity" framework (Akerlof, 1997), we assume that a mother's fertility establishes a norm for her daughter. A daughter's fertility level deviates from the "intrinsic" optimal level, which is determined solely by economic costs and benefits, and moves closer to the fertility level of her mother. In an economy with initially high fertility rates, daughters' fertility tends to surpass the

[1] Data sources: World Development Indicator Database. The TFR is defined as the average number of children born to a woman over her lifetime.

[2] We categorized countries into income levels based on the earliest standard released by the World Bank.

"intrinsic" optimal level. However, birth-control policies that reduce mothers' fertility levels can establish new norms of lower fertility, and thereby lead to lower fertility rates in their daughters.

To empirically test our theoretical predictions, we require a one-shot birth control policy that impacts a single generation and is subsequently lifted for following generations. China's One-Child Policy (OCP), and its unexpected abolition, offers an ideal context for such analysis. The OCP's rollout across provinces in the late 1970s and early 1980s offers a quasi-experimental setting by directly imposing a fertility restriction on mothers of those born in the 1970s, and thereby reducing sibling sizes for the 1970s birth cohort. Easing of the OCP began in 2014 with implementation of the "Selective Two-Child Policy," which allowed couples to have a second child if either spouse was an only child. This relaxation culminated on December 27, 2015, with a proposed amendment to the Law of Population and Family Planning. This led to the unexpected introduction of a universal two-child policy on January 1, 2016, and thereby ended the OCP era. Importantly, those born in the 1970s cohort remained within their reproductive years post-abolition.

Our empirical analyses are based on multiple datasets. The recently released 2020 population census provides valuable data on the fertility behavior of the 1970s cohort following termination of the OCP. We also use the 2010 population census, 2005 and 2015 mini-censuses, China Family Panel Studies (CFPS) for 2012 to 2018, and 1987 Fertility In-Depth Survey (FIDS).

We employ a cohort event-study design to identify the causal effect of maternal exposure to the OCP on the fertility of women born in the 1970s, leveraging the rollout of the OCP across provinces in the late 1970s and early 1980s. We carefully measure and validate the timing of the OCP's rollout across provinces using a variety of sources, including government documents, *Population Chronicles*, and the *Encyclopedia of Chinese Family Planning*. In this setup, a mother's exposure to the OCP within a province is determined by her daughter's birth year relative to the year the policy was implemented: Mothers of daughters born post-implementation were more exposed to the OCP, compared with those whose daughters were born prior to the OCP.

We find that maternal exposure to the OCP lowers her daughter's fertility. Conditional on birth-province and birth-year fixed effects, fertility is up to 0.22 lower for daughters born 1 year after OCP implementation, compared with daughters born before the OCP.

This effect is substantial, especially given the notably low TFR of 1.06 in China in 2022.[3] Because the OCP lowers maternal fertility by 0.33, a Wald estimator suggests that a one-unit decrease in maternal fertility leads to a 0.67 (0.22/0.33) reduction in daughters' fertility.

We conduct a battery of robustness checks. First, the event-study design requires the absence of an anticipation effect. We detect no pre-trend in fertility outcomes before the year of OCP implementation. Second, the key identifying assumption of our event-study design is parallel trends in potential fertility outcomes. We control for three contemporaneous events that may correlate with OCP timing and fertility outcomes, and further add interactions between birth-cohort dummies and a series of pre-OCP socioeconomic, cultural, and political conditions. Third, we address concern regarding potential correlation of OCP exposure across generations, since all daughters in our main analytic sample were also subject to the OCP before 2014. We directly examine intergenerational correlation in OCP exposure, and conduct a placebo test by replacing our main dependent variable with daughters' fertility before the OCP relaxation. Fourth, our theory assumes an absence of birth control for the daughters, while in reality the OCP was abolished only after 2016, leaving a 5-year window before we observe fertility in the 2020 census. We carefully examine whether our empirical analysis informs the hypothetical fertility behavior without birth control for the daughters. Last, we use heterogeneity-robust estimators in the recent event-study literature (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). Our empirical results remain robust to all of these identification concerns.

In our theoretical model, intergenerational fertility persistence comes from the evolution of fertility norms. We examine three alternative explanations that attribute intergenerational fertility persistence to changes in daughters' childbearing costs. Specifically, maternal OCP exposure may result in (i) an increase in daughters' human capital via the child quantity-quality trade-off (Becker and Lewis, 1973); (ii) an increase in maternal labor supply, which potentially influences daughters' labor market engagement via the intergenerational transmission of cultural traits (Fernández and Fogli, 2006, 2009); and (iii) an increase in coresidence between daughters and their parents (Rosenzweig and Zhang, 2014). We do not detect significant effects of maternal OCP exposure on daughters' educational attainment, labor-market outcomes, or coresidence behavior.

[3]Data source: Qiao (2023).

Our study provides the first causal evidence on the intergenerational effects of birth-control policies, and complements the macro-level analysis by De Silva and Tenreyro (2020), who evaluate the role of these policies in the global fertility decline. Using microdata, we leverage the phased implementation and subsequent repeal of China's OCP as a quasi-experimental setting. Both our findings and those of De Silva and Tenreyro (2020) show a lasting reduction in fertility due to birth-control policies.

Our research contributes to the burgeoning literature on the diffusion of fertility norms. The literature has explored such diffusion through religious groups (Munshi and Myaux, 2006); migrant networks (Daudin et al., 2019); and cultural lineages (Beach and Hanlon, 2023; Spolaore and Wacziarg, 2022). Notably, Rossi and Xiao (2023) observe a spillover effect of China's "Later, Longer, and Fewer" (LLF) Campaign on fertility among non-targeted minorities. These studies explore variations in fertility norms that result from shifts in culture or policy and analyze how these changes are disseminated within peer groups, with a specific emphasis on the diffusion of fertility norms among peers of the same generation. We add to the literature by studying the intergenerational spillovers in fertility behavior and highlight the role of maternal fertility in shaping fertility norms.

Inspired by this literature, we further incorporate the peer effect on fertility in our model. In this expanded framework, the fertility norm for a daughter is a weighted average of her mother's fertility and the (expected) average fertility of her peer group. This extension yields two novel insights. First, although the peer effect facilitates further diffusion of the low fertility norm among the daughter's generation, it alone cannot generate the intergenerational effects of birth-control policies. Second, the peer effect on its own leads to multiple equilibria, as shown by Akerlof (1997); however, maternal fertility acts as a focal point, and thereby pins down a unique equilibrium for the daughter's generation.

Our main event-study estimates potentially reflect both maternal and peer effects on fertility behavior, if the OCP differentially affected the fertility of mothers of women born in the same province and year. Fortunately, our extended model enables us to derive a lower bound for the component of maternal effect in our main estimates. This is feasible if we can estimate the parameter that represents the peer effect alone. We estimate this parameter by adopting the strategy of Li and Zhang (2009) and Rossi and Xiao (2023). By integrating the model with these estimates, we demonstrate that at least 35% of our estimated effect of maternal OCP exposure on daughters' fertility is due to the maternal

effect alone.

Our study serves as a bridge between two strands of the literature. The first examines the impact of public policies on cultural practices, as exemplified by Bau (2021), who demonstrates that pension policies influence post-marriage residences in societies with matrilocal or patrilocal customs. The second strand explores intergenerational cultural persistence, with studies such as those by Blau et al. (1992); Fernández et al. (2004); Fernández and Fogli (2009); and Giuliano (2007), who demonstrate how source-country traits determine fertility rates, labor force participation, and the living arrangements of second-generation immigrants in the U.S. These two strands of the literature jointly imply that a one-shot policy can have enduring effects through its influence on culture and the persistence of cultural values across generations. This implication is theoretically and empirically explored in our study.

Our study highlights two major policy implications. First, Fig. 1b shows that many less developed countries have recently implemented birth-control policies to reduce fertility (Rossi and Godard, 2022). While initially effective, our research points to their lasting impact on fertility norms by leading to sustained low fertility rates even after policy removal. This necessitates ongoing policy adjustments to prevent irreversible changes in demographic trends. Second, as shown in Fig. 1c, countries that previously employed such policies now face fertility rates below replacement level, which persist even after policy abolition. Traditional measures such as childcare subsidies and extended maternal leave are less effective under these altered norms. Revitalizing fertility rates may thus require new public campaigns to reshape fertility norms.

2 Theory

This section begins by constructing a model in which maternal fertility serves as a social norm that influences the daughter's fertility choice. It then derives the implications of this model for the lasting impact of birth-control policies. Finally, the model is extended to include peer effect on fertility.

2.1 A Model of the Evolution of Fertility Norms

Consider a woman in generation t who chooses her consumption (c_t) and fertility (n_t) to maximize her utility:

$$\begin{aligned} \max_{c_t, n_t} \; U_t = \; & \ln(c_t) + \gamma \ln(n_t) - \sigma |\ln(n_t) - \ln(n_{t-1})|, \\ \text{s.t.} \quad & c_t + p_t n_t = y_t, \end{aligned} \tag{P1}$$

where γ represents the degree of maternal altruism toward children, p_t is childbearing cost, y_t is her income, and n_{t-1} denotes the fertility of her mother or the number of her siblings. The price of consumption is normalized to 1. In this model (P1), the mother's fertility serves as a norm that influences the daughter's fertility choices. The penalty term ($-\sigma|\ln(n_t) - \ln(n_{t-1})|$) captures the utility loss at a rate of σ for every log point deviation of the woman's fertility from her mother's fertility (n_{t-1}).

Our model's formulation is analogous to those of Akerlof (1997); De Silva and Tenreyro (2020); and Spolaore and Wacziarg (2022). In Akerlof (1997) and Spolaore and Wacziarg (2022), the norm is defined as the expected mean fertility for her generation ($\bar{n}_t$), whereas De Silva and Tenreyro (2020) define it as a weighted average of the replacement level of fertility (2.1) and the woman's mother's fertility. By contrast, we define the norm as n_{t-1}, which emphasizes the evolution of fertility norms over generations. We also consider the norm to be the weighted average of n_{t-1} and $\bar{n}_t$ when allowing for the peer effect on fertility in later sections.[4]

In the special case in which $\sigma = 0$, the fertility norm n_{t-1} does not influence the woman's fertility n_t. The optimal fertility n_t^* is only determined by the "intrinsic" economic costs and benefits, given by $n_t^* = \frac{\gamma}{1+\gamma}\frac{y_t}{p_t}$. This intrinsic optimal fertility declines when (i) the childbearing cost p_t rises relative to income y_t (i.e., higher p_t/y_t) or (ii) the preference for children diminishes (i.e., lower γ).

Generally, we derive the optimal fertility (n_t^*) through the following proposition, under the assumption that n_{t-1} is exogenous to the current generation:[5]

[4] De Silva and Tenreyro (2020) include the squared difference between n_t and the norm in their penalty term, while Akerlof (1997) and Spolaore and Wacziarg (2022) use the absolute value of the difference. We follow the latter, as discussed by Akerlof (1997).

[5] We exclude any dynastic interactions except for the evolution of fertility norms over generations. This assumption is justified in our empirical context, given that the birth-control policy exogenously changes the fertility of the woman's mother.

Proposition 1 *When* $\sigma \geq 0$,

$$n_t^* = \begin{cases} n_t^u, & \text{if} \quad n_{t-1} > n_t^u, \\ n_{t-1}, & \text{if} \quad n_t^l \leq n_{t-1} \leq n_t^u, \\ n_t^l, & \text{if} \quad n_{t-1} < n_t^l, \end{cases}$$

where $[n_t^l, n_t^u] = [\frac{\gamma-\sigma}{1+\gamma-\sigma}\frac{y_t}{p_t}, \frac{\gamma+\sigma}{1+\gamma+\sigma}\frac{y_t}{p_t}]$.

Proof. See Appendix Section A.1.

The proposition demonstrates how maternal fertility serves as a norm that influences the daughter's fertility. Specifically, the daughter fully mimics her mother's fertility n_{t-1} when $n_{t-1} \in [n_t^l, n_t^u]$, such that $n_t^* = n_{t-1}$. When $n_{t-1} \notin [n_t^l, n_t^u]$, she partially mimics the norm. If the mother's fertility is sufficiently high (low), such that $n_{t-1} > n_t^u$ ($n_{t-1} < n_t^l$), the daughter's optimal fertility is n_t^u (n_t^l), which is larger (smaller) than the intrinsic optimal fertility.

2.2 Model Implications for the Lasting Effect of Birth-control Policies

Fig. 2 illustrates the lasting effect of birth-control policies. Our model explains this effect. For illustration, consider a sequence of four generations, indexed by 0, 1, 2, and 3, in a society. Based on Proposition 1, we define the fertility interval for each generation as $[n_t^l, n_t^u] = [\frac{\gamma-\sigma}{1+\gamma-\sigma}\frac{y_t}{p_t}, \frac{\gamma+\sigma}{1+\gamma+\sigma}\frac{y_t}{p_t}]$. We make the following assumption:

Assumption 1

(a) All model parameters remain stable except that the price of childbearing, p_t*, consistently increases from generations 0 to 3,*

$$p_0 < p_1 < p_2 < p_3;$$

(b) the fertility of generation 0 starts at the equilibrium level, such that $n_0^* \in [n_0^l, n_0^u]$*; and*

(c) the price change from generation 0 to 1 is large enough, such that $p_1 > \frac{\gamma-\sigma}{1+\gamma-\sigma}\frac{y_1}{n_0^l}$.

The solid line, OABC in Fig. 2, represents the "natural" equilibrium fertility rates without birth-control policies. Point O denotes n_0^* for generation 0. By Assumption 1a, the

fertility interval $[n_t^l, n_t^u]$, where $t = 1, 2, 3$, shifts downward. By Assumption 1c, Proposition 1 states that the equilibrium fertility rates for generations 1, 2, and 3 land on the upper bounds of the fertility intervals, represented by points A, B, and C.

The dashed line, OA'B'C', represents the equilibrium fertility rates with a one-shot birth-control policy on generation 1. Comparing the dashed line with the solid line, we observe a lasting effect of the policy. Because of the policy, we assume that the fertility of generation 1 is constrained to be n^p such that $n^p < n_1^l$ (point A'). When the policy is removed after this generation, the fertility of generation 2 does not bounce back to point B, the natural equilibrium fertility rate. This is because the constrained low fertility of generation 1 establishes a new norm for generation 2. The equilibrium fertility is n_l^2 for both generations 2 and 3. Fig. 2 shows that n_2^l is smaller than both n_2^u and n_3^u.

We summarize the long-lasting impact of the birth-control policies in the following proposition:

Proposition 2 *Under Assumption 1,*

(a) Without a birth-control policy, the equilibrium fertility rates are

$$n_0^* > n_1^* = n_1^u > n_2^* = n_2^u > n_3^* = n_3^u.$$

(b) When a birth-control policy restricts the fertility of generation 1 at $n^p < n_2^l$, but is removed afterward, the equilibrium fertility rates are

$$n_0^* > n_1^* = n^p < n_2^* = n_3^* = n_2^l.$$

Proof. See Appendix Section A.3.

Because $\sigma > 0$, $n_2^l < n_2^u$: The one-shot birth-control policy at generation 1 lowers fertility for generation 2. If we assume $n_2^l < n_3^u$, the policy further lowers fertility for generation 3. Note that the theoretical prediction of Proposition 2 does not hinge on the assumption $n^p < n_2^l$. As long as $n^p < n_2^u$, the policy for generation 1 reduces equilibrium fertility for subsequent generations.

Our model is adaptable to include parental investment in child human capital, and thereby incorporates a mechanism of the child quantity-quality (QQ) trade-off (Becker

and Lewis, 1973).[6] If this mechanism is at work, a birth-control policy could lead to reduced fertility in the first generation, increased investment per child, enhanced human capital in the second generation, and subsequently lower fertility in the second generation. In Section 5.4, we analyze this mechanism within our empirical setting but find no significant effect of the birth-control policy on the human capital of the second generation. This aligns with other studies that investigate the QQ trade-off in the Chinese context (Rosenzweig and Zhang, 2009), which indicates that this mechanism does not drive our empirical findings. For the sake of simplicity, our theoretical model abstracts from child human capital investment.

2.3 Model Extension: Incorporating Peer Effect

We now extend our model to incorporate the peer effect in shaping fertility norms, which has been extensively investigated in the literature (Richerson and Boyd, 2008; Li and Zhang, 2009; Spolaore and Wacziarg, 2022; Beach and Hanlon, 2023; Rossi and Xiao, 2023). Specifically, the utility maximization problem for woman i in generation t becomes

$$\begin{aligned} \max_{c_{i,t},n_{i,t}} \; & U_{i,t} = \ln(c_{i,t}) + \gamma \ln(n_{i,t}) - \sigma |\ln(n_{i,t}) - \ln(N_{i,t})|, \\ \text{s.t.} \quad & c_{i,t} + p_t n_{i,t} = y_t, \end{aligned} \tag{P2}$$

where the fertility norm ($N_{i,t}$) is a weighted average of the mother's fertility ($n_{i,t-1}$) and the (expected) fertility of peers ($\bar{n}_t$), such that

$$N_{i,t} = \alpha n_{i,t-1} + (1-\alpha)\bar{n}_t, \tag{1}$$

where $\alpha \in [0,1]$ represents the strength of maternal relative to the peer effect on the fertility norm. The solution to P2 is identical to that to P1, stated in Proposition 1, when a society starts at the steady state for generation $t-1$. Because of the peer effect, $n_{i,t} = \bar{n}_t$ in the steady state.[7]

We consider a case in which the society does not start at the steady state for generation $t-1$. For simplicity, we assume that a birth-control policy restricts the fertility rate of a proportion $\lambda \in (0,1)$ of women at generation 1 to $n^p < n_2^l$.[8] The policy does not restrict

[6] For a comprehensive review of the child QQ trade-off, see Guo et al. (2022) and Doepke (2015).
[7] See Appendix Sections A.4 and A.5 for the formal proof.
[8] If the birth-control policy restricts every women in generation 1 to n^p, incorporating the peer effect does

the remaining $1-\lambda$ proportion of women.

In this case, two fertility norms emerge for women in generation 2. For those who are daughters of mothers treated by the policy, the norm is $N_{r,2} = \alpha n^p + (1-\alpha)\bar{n}_2$. For the remaining women, it is $N_{ur,2} = \alpha n_1^u + (1-\alpha)\bar{n}_2$. As $n^p < n_1^u$, $N_{r,2} < N_{ur,2}$. Because they have different norms, the optimal fertility differs between these two types of women. The solution to P2 now becomes a Bayesian Nash equilibrium (Blume et al., 2011), in which women in generation 2 have rational expectations that the ex-ante expected average fertility of their peers ($\bar{n}_2$) coincides with the ex-post realized average fertility, such that

$$\bar{n}_2 = \lambda n_{r,2}^* + (1-\lambda) n_{ur,2}^*,$$

and $n_{r,2}^*$ ($n_{ur,2}^*$) is the equilibrium fertility for women whose mothers' fertility was (not) constrained by the policy.

The proposition below characterizes, and Fig. 3 plots, equilibrium fertility for generation 2 in this case.

Proposition 3 *(a) When $\alpha = 0$, the maternal effect does not exist (Fig. 3a). The equilibrium fertility for generation 2, $n_{r,2}^* = n_{ur,2}^* \in [n_t^l, n_t^u]$. Multiple equilibria exist.*

(b) When $\alpha = 1$, the peer effect does not exist (Fig. 3b). $n_{r,2}^* = n_2^l < n_{ur,2}^* = n_2^u$.

(c) When $\alpha \in (0, \alpha_1)$, $n_{r,2}^*$ decreases with λ when $\lambda < \lambda_1$, then stays constant at n_2^l; $n_{ur,2}^*$ stays constant at n_2^u when $\lambda < \lambda_2$, then decreases with λ (Fig. 3c).

(d) When $\alpha \in [\alpha_1, 1)$, $n_{r,2}^*$ decreases with λ when $\lambda < \lambda_3$, then stays constant at n_2^l; $n_{ur,2}^*$ stays constant at n_2^u when $\lambda < \lambda_4$, then decreases with λ (Fig. 3d).

We have $\alpha_1 = \frac{n_2^u - n_2^l}{n_1^u - n^p}$, $\lambda_1 = 1 - \frac{n_2^l - n^p}{(1-\alpha)(n_1^u - n^p)}$, $\lambda_2 = \frac{n_1^u - n_2^u}{(1-\alpha)(n_1^u - n^p)}$, $\lambda_3 = 1 - \frac{\alpha(n_2^l - n^p)}{(1-\alpha)(n_2^u - n_2^l)}$, $\lambda_4 = \frac{\alpha(n_1^u - n_2^u)}{(1-\alpha)(n_2^u - n_2^l)}$, where $\lambda_1 > \lambda_3$, and $\lambda_2 < \lambda_4$.

Proof. See Appendix Sections A.4–A.7.

Based on the proposition, we have five observations. (1) The average fertility rate of generation 2 decreases with a one-shot birth-control policy that constrains fertility for λ proportion of women at generation 1, as long as the maternal effect exists ($\alpha > 0$). This holds true irrespective of the existence of the peer effect. (2) The policy does not affect the average fertility rate of generation 2 when only the peer effect exists ($\alpha = 0$). (3) The

not affect our analysis in the above section.

optimal fertility for those whose mothers are untreated ($n^*_{ur,2}$) decreases with the policy as long as the share of women whose mothers are treated (λ) is sufficiently large. (4) The effect of the one-shot birth-control policy extends naturally beyond generation 2, since the decreased fertility for generation 2 reduces the norms for generation 3 through the maternal effect. (5) Our results for multiple equilibria when the peer effect exists only echo those of Akerlof (1997). However, when $\alpha > 0$, maternal fertility serves as a focal point that pins down a unique equilibrium in our extended model.

In exploring the unique institutional context characterized by the staggered rollout of the OCP among Han Chinese in the late 1970s and early 1980s, followed by its termination in 2016, our main empirical analysis below tests the prediction based on Proposition 2. We also use the differential implementation between Han and ethnic minorities to test some predictions based on Proposition 3. Finally, we use Proposition 3 to quantitatively separate maternal and peer effects in accounting for the long-lasting effect of the OCP on women whose mothers' fertility was directly constrained by the policy, but they themselves are no longer constrained by the policy after 2016.

3 Institutional Background

To test our theoretical prediction, a one-shot birth-control policy is required—one that affects a single generation and is removed for subsequent generations. China's OCP, followed by its sudden abolition in 2016, provides a unique context for such an analysis. This section first describes the rollout of the OCP in the late 1970s and early 1980s, measures the implementation timing of the policy across provinces, and finally describes its nationwide termination in 2016.

3.1 History of the Birth-control Policy in China

China launched its family planning campaign, the "Later, Longer, Fewer" Campaign, in 1971 (Zhang, 2017).[9] In response to fears of an overpopulation crisis, Xiaoping Deng, then the new leader of China, initiated a more stringent family planning program in 1979. This radical initiative, aimed at restricting most families to a single child, was one of the

[9]This slogan advocated for late marriage and childbearing, extended birth spacing (at least 3 years between two births), and limiting the number of children to no more than two. The program began with the State Council's Document 51, "Report on Better Implementing Family Planning Policy," released in 1971.

most extreme birth-control measures in recorded history. It encompassed a comprehensive approach that involved widespread propaganda, regulatory measures, incentives for compliance, and punitive actions, including financial and employment-related penalties. Despite its intent, the program did not fully achieve a universal one-child norm across the country. However, the collective set of these radical population control policies since 1979 has been commonly referred to as the OCP.

The OCP, since its inception, faced significant public resistance. To mitigate this, the government, starting in the 1980s, relaxed the policy for rural residents and ethnic minorities. The central committee's 1984 Document No. 7 allowed rural families, especially those with a first-born daughter, to have a second child, and ethnic minorities were generally permitted more than one child (Scharping, 2013). Despite these relaxations, the policy remained strict for urban populations. Prime Minister Li Peng in 1989 reaffirmed a population target of 1.2 billion by the end of the century, aiming to limit the natural growth rate to below 1.25%. The 2002 Law of Population and Family Planning further legalized the OCP and maintained the stringent one-child rule in urban areas through the 1990s and 2000s.[10]

3.2 Measuring the Rollout Timing of the OCP across Provinces

The phased implementation of the OCP across provinces offers a quasi-experimental setting to examine our theoretical predictions on the evolution of fertility norms. Although the initiative was formally announced in 1979, the central government only enacted general guidance for implementing the policy. The timing of OCP implementation varied significantly across provinces, since it depended on the Communist Party's political decision process and enforcement at the local level.

To measure the timing of OCP rollout across provinces, we first collected numerous government documents, the *Population Chronicles*, and the *Encyclopedia of Chinese Family Planning*. From these, we compiled a timeline for issuing key legal documents—documents, approvals, notices, regulations, and provisions—pertaining to the OCP for each province. The final step is identifying the date each provincial government officially mandated one child per family and imposed heavy penalties on families with excess second-order births. These official issuance dates, along with comprehensive timelines, are presented in Ap-

[10]See Appendix B.1 for more details on the OCP rollout across China.

pendix C Table C1, which reports the start of the OCP in each province. This table shows significant variation in OCP timing across provinces.

We then validate our measure. Appendix C Fig. C1 shows that TFR declined in a province right after our identified OCP implementation year in the event study framework.[11]

We investigate the determinants of implementation timing at provincial level. We regress the timing against a set of socioeconomic and demographic variables measured in the year prior to the implementation, along with the geographic distance between the provincial capital and Beijing. This distance variable serves as a proxy for political proximity to the central government. Appendix C Table C2 reveals that estimates of socioeconomic and demographic variables are minimal and statistically insignificant. However, as expected, the distance from Beijing shows a positive and statistically significant correlation, which suggests that provinces farther from Beijing tend to implement the OCP later. These findings affirm that the timing of the rollout was determined by political decisions of the Communist Party at provincial level.[12] We carefully control for distance to Beijing interacted with birth cohort dummies in our empirical analysis, as discussed in Section 5.4.

3.3 Abolition of the OCP and the Short-lived Fertility Rebound

The central government began to ease the OCP in 2014 with the "Selective Two-child Policy," which allowed couples to have a second child if either spouse was an only child. This policy had limited impact, however, because the targeted population did not seem to respond to the policy. On December 27, 2015, a proposal to amend the Law of Population and Family Planning was submitted to the National People's Congress, and by January 1, 2016, the universal two-child policy was unexpectedly enacted, effectively ending the OCP (He et al., 2023).

This abrupt policy shift in 2016 led to a temporary increase in fertility rates, predominantly due to a rise in second-child births, which particularly benefited the 1970s cohort. Birth rates per 1,000 women aged 15-49 climbed from 25.9 in 2015 to 44.2 in 2017, then gradually fell back to 28.9 by 2021 (Fig. 4a). This fertility spike was driven by second-child births, which surged from 8.9 per 1,000 women in 2015 to 22.1 in 2017, before decreasing to 12.1 in 2021. Meanwhile, the rate of first-child births remained stable, hovering around

[11] See Appendix Section B.2 for details on the event-study estimation.
[12] See Appendix Section B.3 for details on the regression analysis.

16 during 2015-2021.[13]

In 2021, China lifted all remaining fertility restrictions and began encouraging families to have up to three children. Despite these measures, the TFR continued to decline, falling from 1.16 in 2021 to 1.06 in 2022.[14] This indicates that the post-2016 fertility rebound was transient. In response to ongoing low fertility rates, the Chinese government is now formulating and enacting a range of pro-natalist policies, though the effectiveness of these policies remains to be seen.

3.4 Defining Generations

In light of the institutional context, we define women born between 1969 and 1980 as the second generation ($t = 2$ in Fig. 2), their mothers as the first generation, and their children as the third throughout our empirical analysis below. This will align our empirical analysis with the theoretical model. We chose the 1969-1980 range because women born before 1969 were largely unable to give birth after the 2016 OCP abolition, and those born after 1980 saw minimal variation in their mothers' OCP exposure across provinces. Further justification for these thresholds is detailed later.

4 Data Sources

This section outlines our data sources. We draw on the most extensive datasets that contain information on fertility in China: the 2010 and 2020 population censuses, 2005 and 2015 mini-censuses, China Family Panel Studies (CFPS) from 2012 to 2018, and 1987 Fertility In-Depth Survey (FIDS). The 2020 census has particular significance because it marks the first census conducted after the abolition of the OCP, and thus allows for the analysis of fertility behavior in the post-policy era. Together, these datasets provide a unique opportunity to examine both desired and actual fertility across multiple generations, before and after the OCP's termination.

4.1 Population Censuses

[13]Data source: *China Labor and Population Statistical Yearbook*, published by the National Bureau of Statistics: 2007-2021.
[14]Data source: Qiao (2023).

The National Bureau of Statistics (NBS) of China has conducted a population census every 10 years and a 1% population survey (mini-census) midway between each census since 1990. Both the census and mini-census provide nationally representative data that contain rich demographic and socioeconomic information. This includes age, gender, birthplace, ethnicity, fertility (for women aged 15 to 64), education, and labor market outcomes.

Our main dataset draws on the 2020 census. It offers up-to-date, nationally representative data on the actual fertility of women aged 15 to 64 following the OCP's termination. We analyze a 1‰ random sample from this census, accessed through the Peking University Development Research Center portal under NBS authorization.[15] We also use a 20% random sample from the 2005 mini-census, a 0.35% random sample from the 2010 census, and a 20% random sample from the 2015 mini-census for robustness and mechanism analyses. Table 1 reports summary statistics for samples constructed using the census data.

4.2 The Chinese Family Panel Studies

The CFPS is a biennial survey that captures China's societal, economic, educational, and health conditions (Xie, 2012). The baseline was launched in 2010 and includes 33,600 adults and 8,990 children from 14,960 households across 25 provinces. Its probability-proportional-to-size sampling ensures the survey's national representativeness. The CFPS conducted follow-ups in 2012, 2014, 2016, 2018, and 2020. A distinctive feature of the CFPS is its collection of data on desired fertility or fertility intention. In the 2012 and 2018 waves, children aged 10-15 were asked how many children they would like to have in a hypothetical scenario without birth-control restrictions.[16] This question was also posed to adults aged 16 and above in the 2014 and 2018 waves.[17] We use data from these three waves to examine the impact of the OCP on desired fertility for the second and third generations.[18]

4.3 China Fertility In-Depth Survey

[15] Data analysis was conducted in a secure lab at the NBS-Peking University Development Research Center.
[16] Question KA202 in the individual (youth) module.
[17] Question M501 in the individual (adult) module. This question was also designated as 'inquiry regarding the ideal number of children' in the questionnaire. In 2018, the adults were directly asked how many children they would like to have.
[18] The sample, variables, and summary statistics for analysis based on the CFPS are detailed in Appendix Section D.1.

The 1987 FIDS complements the CFPS in our analysis by offering insights into desired fertility for the first generation whose fertility was directly constrained by the OCP, some of whom had died prior to the CFPS baseline survey in 2010. The NBS conducted the FIDS in April 1987 across several provinces. The 1987 FIDS gathered detailed data on female respondents' characteristics, marriage, fertility, breastfeeding history, contraception knowledge and usage, and basic information on their husbands. Notably, it included a question on desired fertility in the absence of the OCP—similar to the CFPS—that targeted all adults.[19]

5 Empirical Analysis

This section outlines our empirical strategy, defines the analytic sample, presents main results, conducts robustness checks, and explores potential mechanisms.

5.1 A Cohort Event-study Design and Analytic Sample

Our main analysis focuses on the effect of OCP exposure of the first generation on fertility of the second generation, as defined in Section 3.4, in the post-policy era. We exploit the staggered rollout of the OCP in an event-study specification:

$$Y_{icp} = \alpha_p + \theta_c + \delta_p \cdot c + \sum_{l \neq -1} \beta_l \cdot D_{cp}^{l} \cdot Early_p + \epsilon_{icp}, \tag{2}$$

where Y_{icp} measures fertility in the 2020 census for a woman i in the second generation, who was born in province p and year (cohort) c; α_p, θ_c, and $\delta_p \cdot c$ capture, respectively, province fixed effects, cohort fixed effects, and province-specific cohort linear trends; D_{cp}^{l} are relative year indicators; $Early_p$ indicates that province p adopted the OCP before 1984; and ϵ_{icp} is the error term. Standard errors are clustered at province level in the estimation of Eq. 2.

We define the relative year indicator, D_{cp}^{l}, to measure the degree of OCP exposure as follows:

$$D_{cp}^{l} = \mathbb{I}[l = c + s - T_p], \tag{3}$$

where l denotes the relative year in our event-study design and represents the OCP exposure

[19] The sample, variables, and summary statistics for analysis based on the 1987 FIDS are detailed in Appendix Section D.2.

level for a woman's mother. Here, T_p is the year a province first implemented the OCP, and s represents the typical spacing between births. In our main analysis, we adopt $s = 4$ years, because over 90% of first-generation mothers experienced birth intervals of up to 4 years.[20] For example, consider women born in a province p in which the OCP was enacted in 1980 ($T_p = 1980$). A woman born in 1976 ($c = 1976$) in this province would be positioned to expect a younger sibling by 1980 ($s = 4$) if her mother planned further childbirths. For such a woman born in 1976, we calculate $c + s - T_p = 1976 + 4 - 1980 = 0$; her relative year equals zero ($l = 0$), making $D^0_{cp} = 1$. Conversely, for women born in years other than 1976 in this scenario, $c + s - T_p \neq 0$; thus, $l \neq 0$ and $D^0_{cp} = 0$. Following this logic, for a woman born in 1977, $D^1_{cp} = 1$, and for one born in 1978, $D^2_{cp} = 1$. This method allows us to assess the varying levels of policy exposure across different cohorts within each province. We designate $l = -1$ as the reference period and bin relative years ≤ -4 to prevent multicollinearity concerns.

Following Bailey et al. (2021) and Goodman-Bacon (2021), we interact $Early_p$ with D^l_{cp} in Eq. 2. This interaction sets D^l_{cp} to 0 for provinces that adopted the policy after 1984, and thus includes these last-treated provinces in the control group. This approach improves the efficiency of our event-study estimators.[21]

To address cross-provincial heterogeneities in life-cycle fecundity patterns, we include province-specific cohort trends. This is crucial, because although the OCP was uniformly terminated in 2016 across all provinces, women of varying ages, whose mothers experienced different degrees of OCP exposure, may have exhibited distinct fertility behaviors between 2016 and 2020. While cohort fixed effects capture common life-cycle fecundity patterns, our addition of province-specific linear trends accounts for any heterogeneities across provinces.[22]

The estimation sample includes urban Han Chinese women born in 1969-1980 in the 2020 census. First, as discussed in Section 3, the restriction to one child per family is largely exempted for ethnic minorities and rural residents. Second, based on the fertility history between 2015 and 2020 by province and birth cohort, our statistical analysis shows that

[20] For a detailed justification of setting $s = 4$ and sensitivity analyses for alternative s values, refer to Appendix Section E.1.

[21] Appendix Section E.2 shows that our estimation remains similar but less precise without interacting D^l_{cp} with $Early_p$.

[22] Appendix Section E.3 presents more discussion and robustness analysis of the specification of the province-specific cohort linear trend.

there is a structural break in cohort 1969:[23] Women born before 1969 seldom experienced childbirth after 2016. Third, we use 1980 as the cutoff year for our sample to account for the policy's latest implementation in 1984 across provinces and align with the 4-year birth spacing ($s = 4$).

Table 1 Panel A reports summary statistics for the sample constructed using the 202 census, which comprises 75,438 women. The average number of children by 2020 for these women is 1.46.

5.2 Identifying Assumptions

We are interested in the estimates of $\beta_l's$ ($l \geq 0$) in Eq. 2, which are used to measure the effects of OCP exposure of the women's mothers (first generation) on the fertility of the women themselves (second generation). To assess the mothers' exposure to the OCP, we leverage variations in both the birth years of the women and the timing of OCP implementation across provinces. These two factors jointly influence the likelihood of these women having younger siblings, and thereby capture the fertility constraint imposed by the OCP on their mothers. Mothers of women born after implementation of the OCP were more exposed, whereas mothers of those before the OCP were less exposed, depending on the years between the OCP timing and the woman's birth year.

Consistent estimation of β_l in our event-study framework requires two key assumptions, as outlined by Sun and Abraham (2021) and Miller (2023). The first is the absence of anticipation effects, which means that mothers should not have altered their fertility prior to the OCP's implementation in anticipation of the policy. The second assumption is parallel trends in potential fertility outcomes. This assumes that in the absence of the OCP, fertility changes across birth cohorts should be the same between provinces that implemented the policy early and those that implemented it later. The second assumption is subtle in our study context, because we estimate the effect of OCP exposure of the mothers' generations on the fertility of the women's generation. This requires that the timing of OCP implementation does not correlate with factors that vary by province and birth cohort and that could influence fertility rates across both generations, conditional on covariates. These two assumptions will be thoroughly examined in Section 5.4.

[23] Appendix C Fig. C2 shows that births in 2016-2020 are nearly 0 for cohorts born before 1969, but they surge for later cohorts born after 1969. We further conduct a formal test on structural breaks following Bailey et al. (2021), and details are presented in Appendix E.4. We also conduct sensitivity analysis of an alternative range of birth cohorts in Appendix E.4.

A particularly important concern in our context is the intergenerational correlation in OCP exposure. In particular, provinces that implemented the OCP earlier may tend to enforce the policy more strictly in subsequent years. This could trigger two challenges. First, if the timing of the OCP rollout correlates with subsequent over-time changes in OCP enforcement within the same province, the common trend hypothesis is violated. Second, if the timing of the OCP rollout correlates with cross-province variations in OCP enforcement, the treatment effect of OCP rollout on second-generation fertility could be heterogeneous across provinces, and thus introduces a misspecification in Eq. 2. In Section 5.4, we directly examine intergenerational correlation in OCP exposure, conduct a placebo test using second-generation fertility before OCP relaxation, and implement heterogeneity-robust estimators in the recent event-study literature (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021).

5.3 Main Results

Before presenting our main results for the fertility of women in the second generation, we first estimate the OCP's impact on their mothers' fertility. This impact corresponds to the effect of birth-control policies on the first generation ($t = 1$ shown in Fig. 5) in our model. Using urban Han Chinese women born in 1975-1980 from the mini-census 2005, we estimate Eq. 2 with the number of siblings as the dependent variable to proxy for the mothers' fertility.[24] Fig. 5 reports that estimates of β_l are near 0 for $l < -1$, which indicates no significant pre-trend. The β_l estimate decreases from -0.07 for $l = 1$ to -0.33 for $l = 5$, consistent with the literature (Qin et al., 2017). This suggests that the OCP significantly reduced fertility in the first generation. Table 2 reports estimates and the corresponding standard errors.

We now present our main results. Based on our main analytic sample of urban Han Chinese women born during 1969-1980 in the 2020 census, Fig. 6 plots our estimates of β_l's. The figure shows no pre-trend, since the estimates of β_l's are nearly zero when $l < -1$. The β_l estimate decreases from -0.05 for $l = 1$ to -0.22 for $l = 5$, which are both significant at the 10% level. Women whose mothers were exposed to the OCP had between 0.05 and 0.22 fewer children compared to those whose mothers were not exposed. The result

[24] Table 1 Panel B provides summary statistics for this sample. We include women born after 1974, since the mini-census 2005 requires only those aged 30 or below to report their sibling size. Mini-census 2005 is the only census wave that requires individuals to report their sibling size.

is consistent with our prediction by Proposition 2. The estimated effect is substantial, especially considering a TFR of 1.06 in 2022.

By combining the findings from Figs. 5 and 6, we calculate the intergenerational persistence in fertility, as implied by Proposition 1. Within a local average treatment effect framework (Imbens and Angrist, 1994), the estimates from Fig. 5 serve as first-stage results that show the OCP's impact on mothers' fertility. Fig. 6 provides reduced-form estimates of the OCP's influence on women's fertility. The ratio of these estimates offers a causal interpretation of how changes in mothers' fertility affect their daughters' fertility (Angrist and Krueger, 1992). Consequently, we infer that for a long-term horizon ($l = 5$), a one-unit decrease in mothers' fertility reduces daughters' fertility by 0.67 (0.22/0.33).

5.4 Robustness

Anticipatory effects. Figs. 5 and 6 show that the estimates of β_l for $l < -1$ are small and statistically insignificant, which suggests no anticipatory effects on fertility in both generations. This result is confirmed by Appendix C Fig. C1, which demonstrates no pre-trend in the TFR preceding the year of OCP implementation. We further assess anticipatory effects by analyzing birth spacing among mothers in provinces that implemented the OCP after 1981. These mothers are grouped based on their daughters' birth dates: Group 1 (pre-1975 births) and Group 2 (1975-1978 births). Mothers in Group 1 were not influenced by the policy. We aimed to discern whether mothers in Group 2 adjusted their birth intervals in anticipation of the policy change. Upon comparing birth-spacing distributions in Appendix C Fig. C3, we observe similar patterns for mothers in both groups, which indicates no anticipatory behavior.

Common trends for potential outcomes. This is the most important identifying assumption for our event-study design. We comprehensively investigate the robustness of our results to this assumption by conducting three sets of analyses.

First, we control for four socioeconomic and policy shocks in our regression: the Great Famine (1959-1961), the Cultural Revolution (1966-1976), the "Later, Longer, and Fewer" Campaign during the 1970s, and implementation of the Compulsory Education Law (around 1986).[25] These shocks varied in intensity across provinces, and exposure to these

[25] We do not consider the rural household responsibility system reform in the late 1970s, because we focus on urban women. The construction of measurements for different policy shocks is detailed in Appendix E.5.

events also differed among the cohorts we are studying. Our estimate might capture the effects of these shocks if they correlate with both fertility and our measured OCP exposure simultaneously.

Second, we control for interactions between the cohort dummies and a series of variables that measure pre-OCP socioeconomic and political conditions in the regression. Our event-study framework has well accounted for any cohort-invariant and linearly evolved cross-province heterogeneity. Nonetheless, the heterogeneity in these conditions could impact not only the level and linear changes, but also nonlinear changes in fertility across cohorts of different provinces. As shown in Appendix Section C Table C2, whereas OCP timing does not significantly correlate with pre-OCP socioeconomic conditions, it indeed correlates with the geographic distance between the provincial capital and Beijing. Therefore, it is necessary to control for interactions between the cohort dummies and distance to Beijing.

Third, we control for a set of variables that measure socioeconomic conditions to account for potential macroeconomic impacts of the OCP rollout (Li and Zhang, 2007). The OCP may have distinctively influenced the socioeconomic environment for women of the second generation, particularly during their developmental years, with potential cumulative effects over time. Notably, 1970s cohorts, who experienced varied OCP intensities during ages 0-5, likely faced diverse socioeconomic conditions. Such heterogeneities might bias our estimations, given recent findings such as Cotofan et al. (2023), which suggest that socioeconomic conditions, particularly in early adulthood, can shape individual preferences and, consequently, fertility choices. Consequently, we follow Cotofan et al. (2023) and use regional GDP growth and per capita GDP data to construct cohort- and province-specific socioeconomic variables. These variables are measured during the women's impressionable years, typically ages 18-25.[26] Appendix E.5 shows that our main finding remains robust in these three sets of sensitivity analyses.

Early-life OCP exposure of the second generation. One concern with our estimation is that all women in our estimation sample were subject to the OCP before 2014. Provinces that implemented the OCP earlier may tend to enforce the policy more strictly in subsequent years. This may lead to a violation of the common trend hypothesis if the timing of the OCP rollout correlates with subsequent over-time changes in OCP en-

[26]The "impressionable years hypothesis" suggests that ages 18 to 25 are crucial for establishing enduring attitudes, beliefs, and values, and highlights this period's significance due to increased cognitive adaptability (Krosnick and Alwin, 1989; Cotofan et al., 2023).

forcement intensity that directly influence second-generation fertility. Specifically, if OCP exposure for women in the first generation in the 1970s and 1980s, net of province and cohort fixed effects and province-specific linear trends, correlates with the intensity of the OCP for women in the second generation in the 1990s and 2000s, our negative estimates of β_l ($l > 0$) would mechanically reflect the OCP effect in early life for women in the second generation. However, this is unlikely for two reasons. First, policy enforcement has remained stable since the mid-1990s (Section 3.1), leaving little room for cross-cohort variations in OCP exposure for second-generation women. Second, we find no reason to expect a correlation between the timing of OCP implementation for women in the first generation and changes in OCP intensity for their daughters across birth cohorts.

To further alleviate this concern, we conduct two analyses. First, we examine the correlation between OCP exposure of the mothers and intensity of the OCP experienced by their daughters. Specifically, we re-estimate Eq. 2, using the daughters' OCP intensity as the dependent variable. Intensity is measured by the average fine rate the daughter experienced during her early life (6–20) and reproductive ages (20–45) based on her birth province and cohort.[27] Appendix C Fig. C4 shows that the estimates of β_l ($l > 0$) are statistically insignificant.[28]

Second, we examine the correlation between the mothers' OCP exposure and their daughters' fertility before OCP termination. We re-estimate Eq. 2 using the same birth cohorts as in our main sample, but measure their fertility in 2013 based on the mini-census 2015.[29] If there is a significant correlation between the mothers' OCP exposure and their daughters' OCP intensity, estimates of β_l ($l > 0$) will be negatively significant. Our estimates reported in Appendix C Fig. C5 contradict this prediction.

A short window of childbearing after OCP abolition. A key challenge arises from the fact that the OCP was only fully repealed in 2016, which limits our observation of fertility to a 5-year window until the 2020 census. Cohorts born between 1969 and 1980 were already in their mid-30s to late-40s by 2016, and thus entering later reproductive stages. This short window raises concerns about the ability of these cohorts to reflect

[27] Under the OCP, parents in China who exceed their fertility limit are forced to pay a fine, which varies across provinces. See Appendix E.6 for the details of this policy.

[28] Appendix E.6 reports the details.

[29] Table 1 Panel C reports the average fertility of these women before policy relaxation in mini-census 2015, which shows an average of 1.37 children by 2013. We measure the fertility of these women in 2013, which corresponds to the period before the central government initiated relaxation of the OCP in 2014 and introduced the "Selective Two-child Policy," as outlined in Section 3.3.

changes in fertility in the post-OCP era. Also, post-abolition fecundity likely varied across these cohorts. To mitigate these concerns, our main analysis includes province-specific trends for each cohort to address heterogeneous life-cycle fertility patterns. Despite these efforts, uncertainty persists regarding the adequacy of the 5-year observation period to capture genuine fertility behavior in the absence of birth-control policies.

We conduct three analyses to address this concern. First, analyzing national aggregate fertility data reveals a notable trend in second-child birth rates in our study period, as depicted in Fig. 4b. There was a significant rise from 5.7 in 2015 to 10.2 in 2017, with a subsequent decline to almost zero by 2021. The peak in 2017, when rates hit 10.2—approximately half the average for fertile women aged 15-49, as detailed in Fig. 4a—suggests that women in our sample were actively seizing the last chance to fulfill their fertility desires.

Second, we use desired fertility of the second generation as a proxy for their hypothetical fertility in the absence of birth control.[30] The results, depicted in Fig. 7a, show that the estimates of β_l's are significantly negative. Compared with Fig. 6, the impact of maternal OCP exposure on the women's desired fertility appears larger than that on their realized fertility. This difference likely reflects the OCP's restriction on the second generation's fertility for a substantial portion of their fecundity window.[31]

Third, we extend our analysis of desired fertility to third-generation women, who are daughters of second-generation women. Our theory predicts that the OCP affects fertility beyond the second generation. The third generation are teenagers during the survey years and will not be affected by the OCP during their entire reproductive lifespan, which offers an alternative context to test our theoretical prediction. We re-estimate Eq. 2 using desired fertility of the third generation as the dependent variable.[32] Results presented in Fig. 7b show that grandmothers' OCP exposure decreases desired fertility for their grandchildren.

Heterogeneous treatment effects. Even though the timing of the OCP rollout doesn't predict cross-cohort variations in OCP exposure among second-generation women in the

[30] Data regarding desired fertility for second- and third- generation women draw on the CFPS. We provide detailed information on sample construction and summary statistics in Appendix Section D.1.

[31] Using data from the 1987 FIDS, we re-estimate Equation 2 using the desired fertility of first-generation women as the dependent variable. The estimates of β_l's ($l > 0$) are presented in Appendix C, Figure C7a, and are found to be near zero and statistically insignificant. This result aligns with our theoretical prediction, which suggests that it is the actual fertility behavior of mothers—rather than their fertility intentions—that establishes the fertility norms for the succeeding generation.

[32] Appendix Section D.1 provides details on sample construction and summary statistics.

same province, these women on average experienced greater exposure to the OCP during their early lives. If early-life OCP experience alters the responsiveness of second-generation fertility to fertility norms, Eq. 2 would be misspecified by not allowing for heterogeneous treatment effects across provinces.

To address this issue, we employ the estimators proposed by Sun and Abraham (2021) and Callaway and Sant'Anna (2021). If provinces that implemented the OCP at different times exhibit distinct treatment effects given the same relative year, the coefficients obtained using these two estimators would no longer exhibit the same pattern as those estimated using Eq. 2. Appendix C Fig. C6 shows that our results remain robust.[33]

5.5 Mechanism

This section explores the mechanism through which mothers' exposure to the OCP influences their daughters' fertility. In the theoretical analysis, we derive the evolution of fertility norms as the driving force behind the lasting impact of the OCP, assuming similar changes in childbearing costs for women across generations. However, the OCP itself could have altered these costs, dependent on the extent of the mothers' exposure to the policy. If this is true, our main findings could be attributed to these OCP-induced shifts in childbearing costs, rather than to the evolution of fertility norms.

Alternative mechanisms. The literature suggests three alternative mechanisms. The first relates to the theory of the child QQ trade-off (Becker and Lewis, 1973), which we discussed in Section 2.2. The QQ theory predicts that the OCP's role in reducing the first generation's fertility might lead to more investment per child, and consequently enhance the human capital of women in the second generation. This increase in human capital could, in turn, affect the fertility of these women. The second relates to women's labor supply: The OCP-induced decrease in fertility may increase the labor supply of women in the first generation. This may alter the labor supply of their daughters in the second generation through the intergenerational transmission of cultural traits that affects female labor supply (Fernández and Fogli, 2006, 2009). In addition, if the OCP increases the human capital of women in the second generation through the child QQ trade-off, the OCP will also change their labor supply. The potential change in the women's labor supply may

[33]The observed uniformity in behavioral responses across various provinces may stem from a widespread reluctance among women to limit their family to a single child during that period. This interpretation is supported by the empirical analysis presented in Section 5.4 using the 1987 FIDS.

affect their fertility. Third, the literature in economics, sociology, and demography suggests that the OCP would change the living arrangements of the first and second generations (Rosenzweig and Zhang, 2014). Because fertility decreases, mothers in the first generation are more likely to coreside with their daughters. Since mothers can assist in caring for children, the cost of childbearing might decrease for women in the second generation. All three factors may influence women's fertility in the second generation through the change in childbearing cost in our theoretical model.

We empirically examine the three potential mechanisms. We re-estimate Eq. 2 using women's education, labor market outcomes, and coresidence status with parents or parents-in-law as dependent variables.[34] Appendix C Figs. C8-C10 show that the estimates of β_l's ($l > 0$) are all small and statistically insignificant.[35] Our results regarding women's education align with prior research. Comprehensive studies examining the OCP in China have concluded that its effect on children's human capital is minimal (Rosenzweig and Zhang, 2009; Liu, 2014; Li and Zhang, 2017). These results suggest that our main finding is less likely to be driven by the three alternative mechanisms.

Discussions of the fertility norm mechanism. Our model suggests that the OCP restricts mothers' fertility and thus establishes a new fertility norm for future generations. This norm influences the fertility of these subsequent generations by modifying the utility function, rather than through changes to the costs and benefits in the budget constraint. Direct evidence on fertility norm evolution has not yet been provided. However, using desired fertility as a measure of fertility norm[36], our analysis of desired fertility—illustrated in Fig. 7—supports the notion of a fertility norm mechanism.

It's essential to recognize that, theoretically, the fertility norm mechanism is largely unaffected by OCP-induced changes in childbearing costs, as long as these alterations are not substantial. While we have examined three alternative mechanisms influenced by changes in childbearing costs due to the OCP, the potential variations in these costs are vast and often challenging to measure. Nevertheless, the assumption regarding common

[34] To address potential behavioral changes stemming from relaxation of the OCP, we measure the dependent variables—education, labor market performance, and living arrangements of second-generation women—using 2010 census data. Summary statistics are presented in Table 1, Panel D. The results remain robust when dependent variables are replaced by those measured in the 2015 mini-census and and 2020 census.

[35] Appendix C Table C3 reports estimates and corresponding standard errors.

[36] The CFPS solicits desired fertility based on inquiries on "the ideal number of children." If respondents interpret the "ideal number of children" as the number of children they desire under the hypothetically scenario of zero childbearing cost, then this measure of desired fertility would capture the fertility norm. Please see Appendix Section A.2 for a formal proof.

price changes (Assumption 1a) can be adjusted to accommodate variations in childbearing costs based on OCP treatment status.[37] The strength of the fertility norm mechanism roots in Akerlof (1997), which suggests that within a context of "conformity," a "marginal change" in the cost of childbearing would have "literally no effect" on fertility behavior, highlighting the mechanism's robustness.

6 Maternal vs Peer Effects

We have established robust evidence that mothers' exposure to the OCP influences their daughters' fertility in the post-OCP era. As outlined in Proposition 2, we've interpreted these findings to mean that the restricted maternal fertility by the OCP establishes a new norm for daughters. This interpretation assumes the OCP had a uniform effect on the fertility of all mothers for daughters born in the same province and year. Yet, this assumption might not always hold. For example, imagine two women born in 1978 in a province where the OCP was implemented in 1979. If one woman's mother was past childbearing age by 1979, while the other's was not, the OCP's fertility restrictions would only apply to the latter. This variation implies that the OCP's impact on mothers' fertility—and consequently on the daughters' perceived fertility norms—can differ even within the same province and cohort. Under such circumstances, peer influences might also shape fertility norms, meaning that our estimates of β_l's ($l > 0$) capture both maternal and peer effects, as Proposition 3 suggests.[38] In this section, we aim to distinguish quantitatively between maternal and peer influences on the lasting effect of the OCP.

For simplicity, we assume that the proportion of mothers affected by the OCP is λ for a given relative year l across all provinces, which is not observed by researchers. Our estimate of β_l is thus represented as

$$\beta_l = \lambda(n^*_{r,2} - n^u_2) + (1-\lambda)(n^*_{ur,2} - n^u_2), \tag{4}$$

[37] In our model, the upper fertility bound for second-generation women is given by $u^u_2 = \frac{\gamma+\sigma}{1+\gamma+\sigma}\frac{y_2}{p_2}$. The OCP may lead to an increase in p_2 and a corresponding decrease in n^u_2. However, if the OCP-induced rise in p_2 is modest such that $n^u_2 \geq n_1$, the fertility of second-generation women will mirror that of the first generation ($n^*_2 = n_1$), indicating that the fertility norm mechanism continues to operate effectively.

[38] It is important to note that the maternal effect is a prerequisite. By Proposition 3, mothers' OCP exposure would not impact daughters' fertility in the absence of the maternal effect, regardless of the peer effect's presence.

where n_2^u denotes the hypothetical fertility of urban Han Chinese women in the second generation in the absence of the OCP, and $n_{r,2}^*$ and $n_{ur,2}^*$ represent the realized fertility of women whose mothers were and were not, respectively, affected by the OCP.

Our objective is to establish a lower bound (in absolute terms) for the impact of a mother's OCP exposure on her daughter's fertility through the maternal effect *alone*, denoted by $n_2^l - n_2^u$, where n_2^l represents the hypothetical fertility of Han Chinese women whose mothers were restricted by the policy but in the absence of the peer effect, as suggested by Proposition 2:

$$\begin{aligned} n_2^l - n_2^u &\leq n_{r,2}^* - n_2^u \\ &\leq \lambda(n_{r,2}^* - n_2^u) \\ &= \beta_l - (1-\lambda)(n_{ur,2}^* - n_2^u) \\ &\leq \beta_l - (n_{ur,2}^* - n_2^u), \end{aligned} \tag{5}$$

where the first inequality is due to $n_2^l \leq n_{r,2}^*$, as implied by Proposition 3; the second and third inequalities follow from $0 < \lambda < 1$; and the equality is based on Eq. 4. Therefore, $\beta_l - (n_{ur,2}^* - n_2^u)$ serves as a lower bound for $n_2^l - n_2^h$ in absolute value, since both terms are negative.

We have estimated β_l but not $n_{ur,2}^* - n_2^u$, which represents the effect that arises from the diffusion of low fertility norms within the second generation *alone*. Whereas extensive literature focuses on the diffusion of low fertility norms in peer groups (Spolaore and Wacziarg, 2022; Rossi and Xiao, 2023), estimating its impact on urban Han Chinese women is infeasible. This is due to the limitations of census data, which, for a given birth cohort within the same province, do not differentiate between women whose mothers were and were not treated by the OCP.

In the following analysis, we estimate the peer effect, $n_{ur,2}^* - n_2^h$, using minority women. This analysis presupposes that, were it not for the OCP, fertility preferences between the Han and ethnic minorities would be similar. While the credibility of this assumption remains an open question, it is a premise frequently adopted in existing research (Li and Zhang, 2009; Rossi and Xiao, 2023; Li and Zhang, 2007; Wang and Zhang, 2018; Zhang, 2017). Accepting this assumption enables us to estimate a lower bound for the maternal effect's contribution to the lasting effect of the OCP. Specifically, we re-estimate Eq. 2, this time focusing on urban minority women born from 1969 to 1980 in the 2020 census.

Their fertility is used as the dependent variable. For these women, we assign the same policy exposure values to their mothers as we did for the mothers of urban Han women born in corresponding provinces and years.[39]

The results in Appendix C Table C4 inform the peer effect. Minority women, whose Han counterparts' mothers were impacted by the OCP, have lower fertility than those whose peers' mothers were not under the OCP. The β_3 estimate reveals that in 2020, fertility among minority women in provinces that implemented the policy 3 years earlier was 0.053 lower than in women born the same year in provinces that adopted the policy later.[40] Our results align with those of Rossi and Xiao (2023).

We apply Eq. 5 to compute the lower bound of the maternal effect using estimates from the ethnic minority sample. For example, considering the relative year 3 ($l = 3$), the absolute lower bound for $n_2^l - n_2^h$ is $0.03 (= 0.08 - 0.05)$, which constitutes 35% of the total effect (β_3). It is important to note that the estimated coefficient of 0.05 may overestimate the peer effect within the second generation.[41] This coefficient encompasses two elements: the direct peer effect from Han Chinese to minorities within the second generation, and an indirect effect that combines the peer effect from Han Chinese to ethnic minorities in the first generation and the maternal effect from minority mothers to their daughters. The latter component includes a maternal effect. Therefore, the estimate of 0.05 based on minority women may overestimate the peer effect.[42]

7 Future Extensions

We suggest two potential extensions for future research. First, our model assumes that a mother's fertility influences her daughter's only through the modification of fertility norms. Future studies could expand this framework by incorporating additional pathways, such as changes in the daughter's education and labor force participation. While our robustness

[39] For more details on the empirical analysis using the ethnic minority sample, who are culturally similar to the Han Chinese, see Appendix F.1.

[40] Due to the small sample size of urban minority women in the 2020 census, the coefficients of β's are not estimated with precision; this limitation also hinders our investigation of heterogeneous effects based on the share of Han Chinese across regions, as implied by Proposition 3.

[41] This overestimation may not be of great concern, since it could result in an underestimation of the component of maternal influence in the overall effect.

[42] The above analysis quantifies a lower bound on the contribution of the maternal effect to the OCP effect on daughters' fertility. Interestingly, we are also able to establish a lower bound for a structural parameter: the strength of the maternal effect relative to the peer effect, α, in the woman's utility function (Eq. 1). See Appendix F.2 for more details.

analyses did not substantiate these alternate channels in this specific context, they could be significant in other scenarios. Second, there is potential to apply our theoretical framework to study the intergenerational effects of diverse socioeconomic policies that may alter social norms. Such research could yield deeper understanding of the broader implications of policy interventions on societal dynamics.

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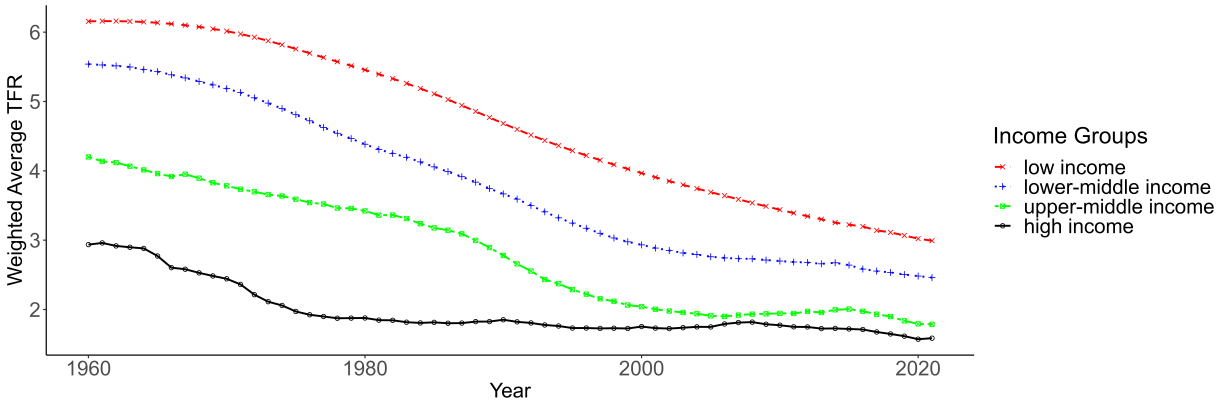
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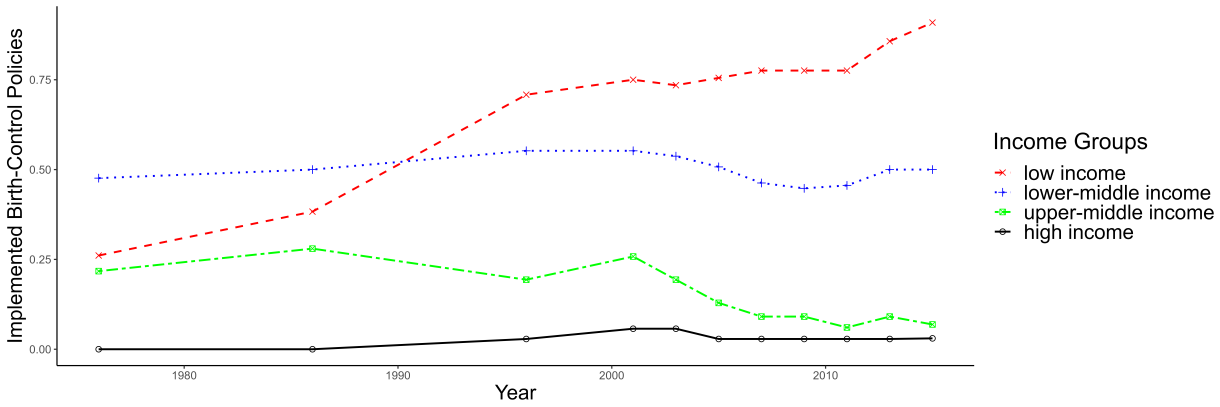
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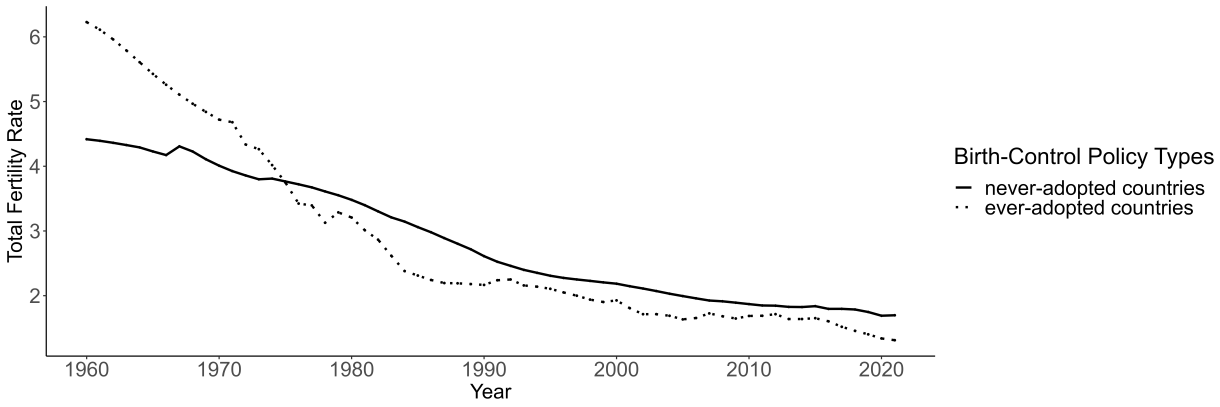
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(a) Total Fertility Rate of Countries, by Income Group



(b) Share of Countries That Implemented Birth-control Policies, by Income Group



(c) Trends in Total Fertility Rates by the Type of Past Birth-Control Policy

Figure 1: Global Patterns of Total Fertility Rates and Birth-control Policies. Figure (a) illustrates weighted average total fertility rates within each income group across time. Figure (b) shows the share of countries implementing birth-control policies within each income group across time. Figure (c) presents the total fertility rates of countries within the upper-middle-income group categorized by birth-control policy type. Fertility data are from the World Bank's World Development Indicators, and countries are classified into four income groups based on their earliest available income classification data from the World Bank. Population policy types are extracted from the United Nations World Population Policies Database. In Figure (b) and Figure (c), we define countries implementing policies to lower fertility as "implementing birth-control policies." In Figure (c), among the 37 identified upper-middle-income countries, birth-control policy information is available for 26. We define an "ever-adopted" group to include countries that have ever adopted birth-control policies but abolished them after 2007. Conversely, a "never-adopted" group includes countries that never implemented birth-control policies during our observation window.

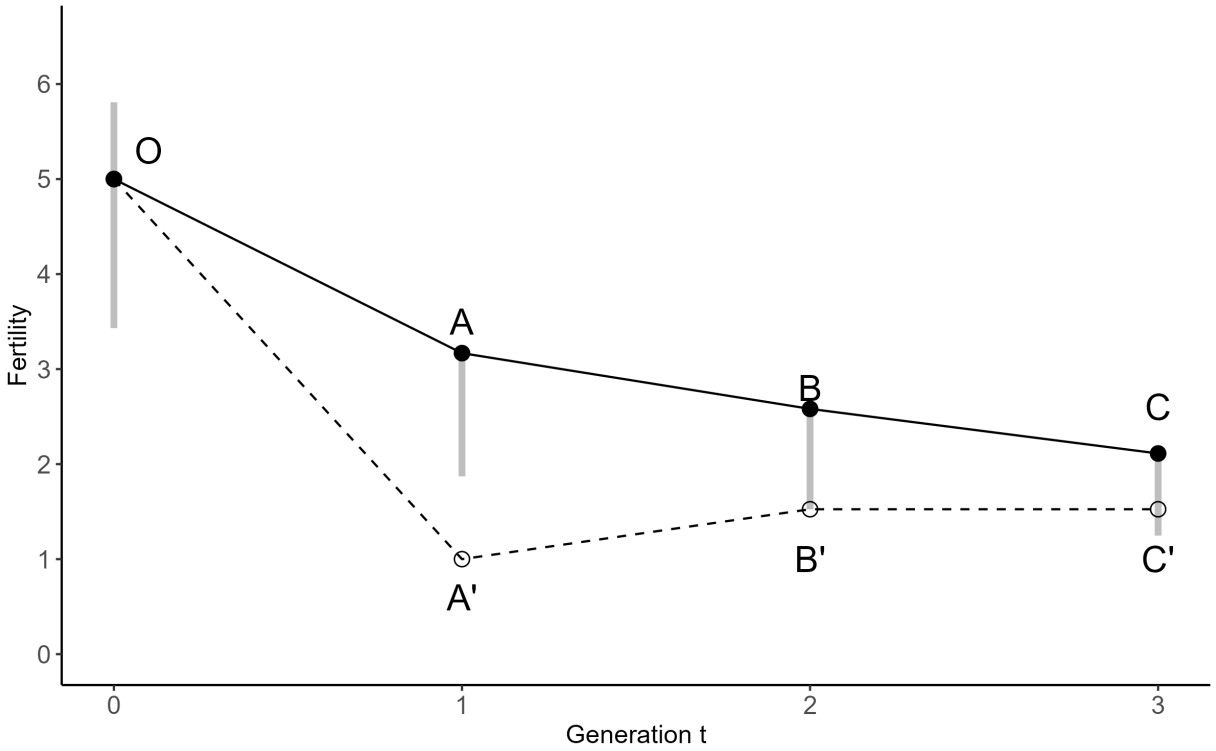
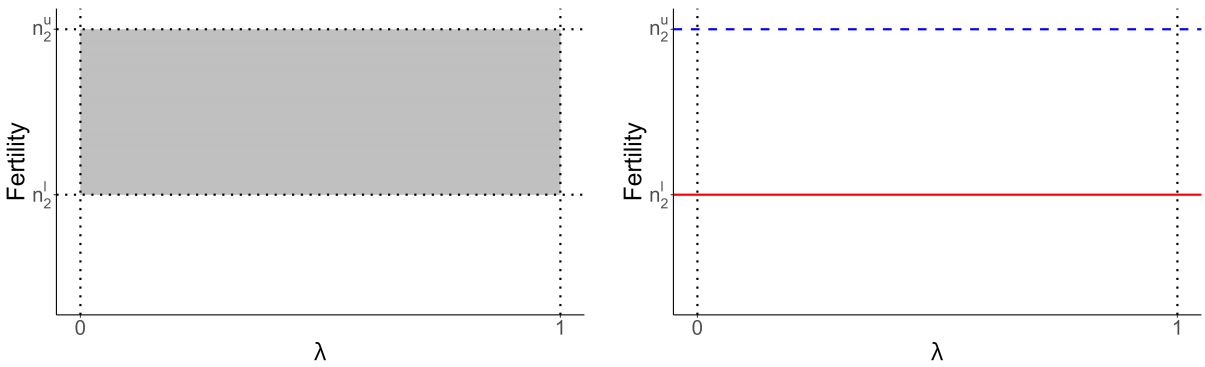
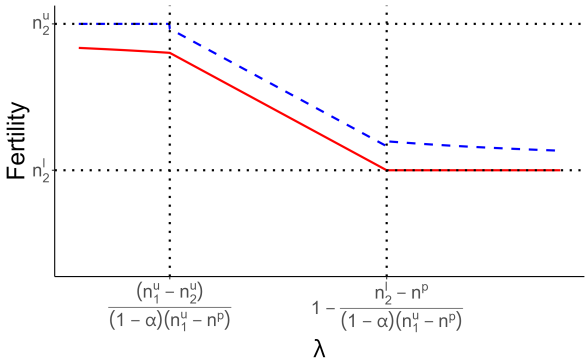


Figure 2: Theoretical Predictions of the Intergenerational Effects of One-shot Birth-control Policies for Generation 1, without Peer Effect. The x-axis denotes generations, the y-axis shows fertility levels, and vertical gray lines represent the fertility interval $[n_t^l, n_t^u]$ for each generation t. The solid line OABC denotes the fertility trend when the birth-control policy never comes into existence. The dashed line OA'B'C' depicts the fertility trend when a mandatory birth-control policy restricts fertility for generation 1, but not for other generations.

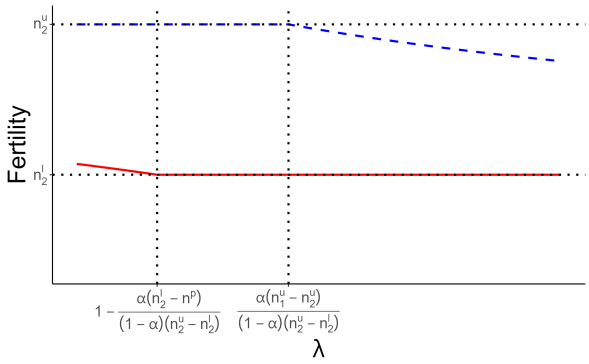


(a) $\alpha = 0$

(b) $\alpha = 1$

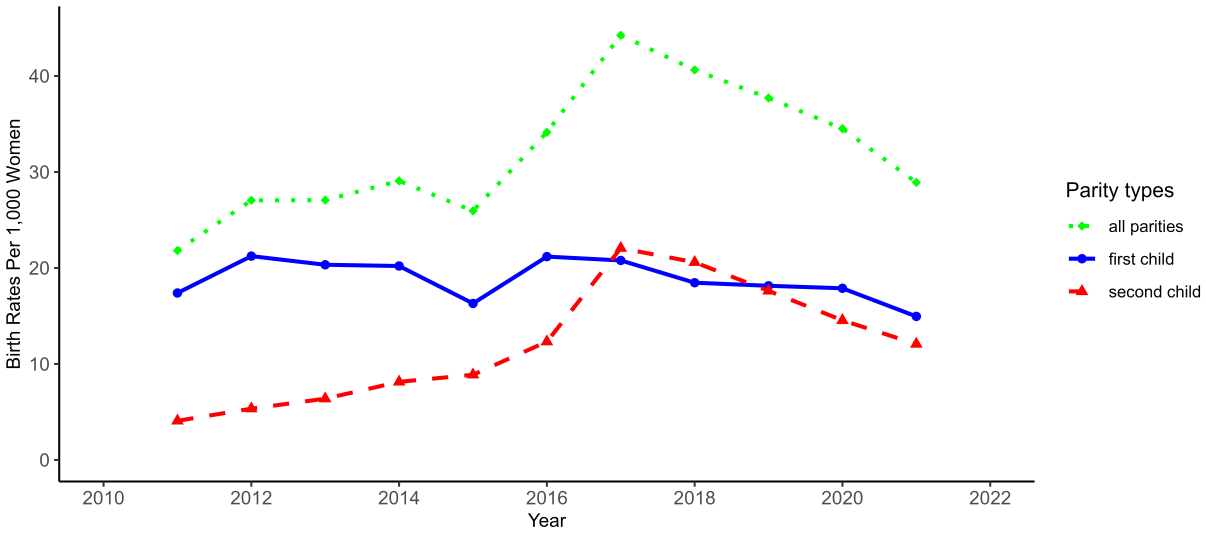


(c) $\alpha \in (0, \frac{n_2^u - n_2^l}{n_1^u - \underline{n}})$

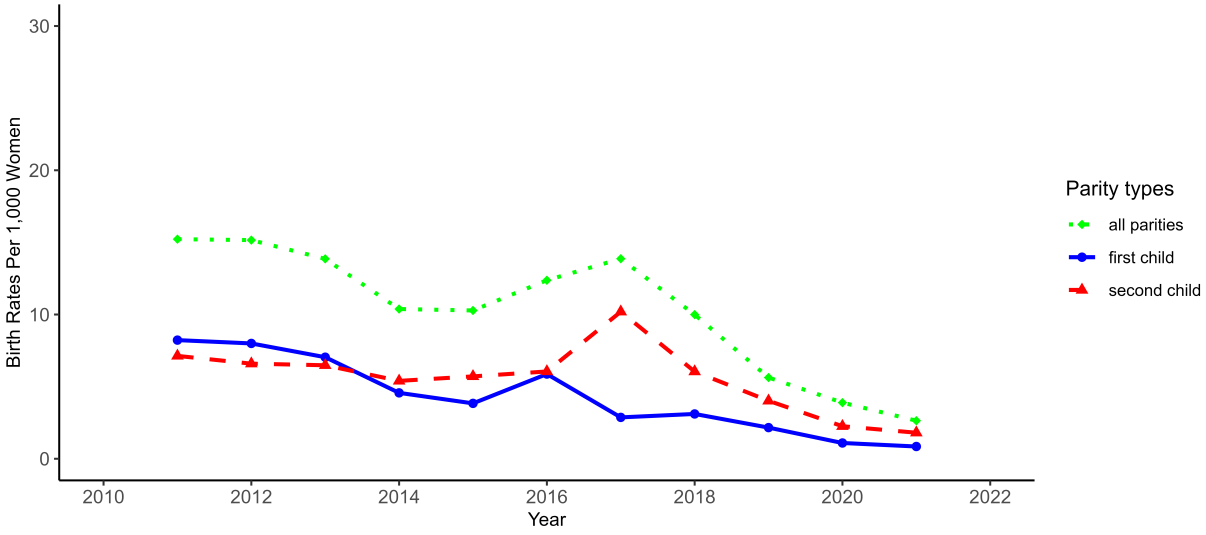


(d) $\alpha \in [\frac{n_2^u - n_2^l}{n_1^u - \underline{n}}, 1)$

Figure 3: Equilibrium Fertility of Generation 2, with both Maternal and Peer Effects. These figures show how the equilibrium fertility of different types of women in generation 2 changes with varying shares of constrained women in generation 1 (λ). Figures (a)–(d) show various circumstances corresponding to different ranges of α, where α denotes the weight of mother's fertility ($n_{i,t-1}$) in determining the fertility norm. The x-axis represents the value of λ ranging from 0 to 1, and the y-axis represents fertility levels ranging from n_2^l to n_2^u. In Figure (a), as $\alpha = 0$, the peer effect results in the same fertility level for all individuals in generation 2. Multiple equilibria exist, and the equilibrium fertility level can take any value between n_2^l and n_2^u. The shaded area represents all possible equilibrium fertility for λ ranging from 0 to 1. In Figures (b), (c), and (d), the equilibrium is unique for given values of α and λ; solid red lines represent equilibrium fertility for individuals with restricted mothers, while dashed blue lines represent equilibrium fertility for those with unrestricted mothers.



(a) Overall Trends



(b) Birth Cohorts Born during 1969-1980

Figure 4: Birth Rates among Urban Chinese Women, by Birth Parity. These two figures depict the birth rates among urban Chinese women for first-born and second-born children from 2011 to 2021. Figures (a) and (b) illustrate the trends in birth rates per 1,000 urban women aged 15-49 during survey years and per 1,000 urban women born during 1969-1980, respectively. Data are from *China Population and Employment Statistical Yearbook 2008-2022*.

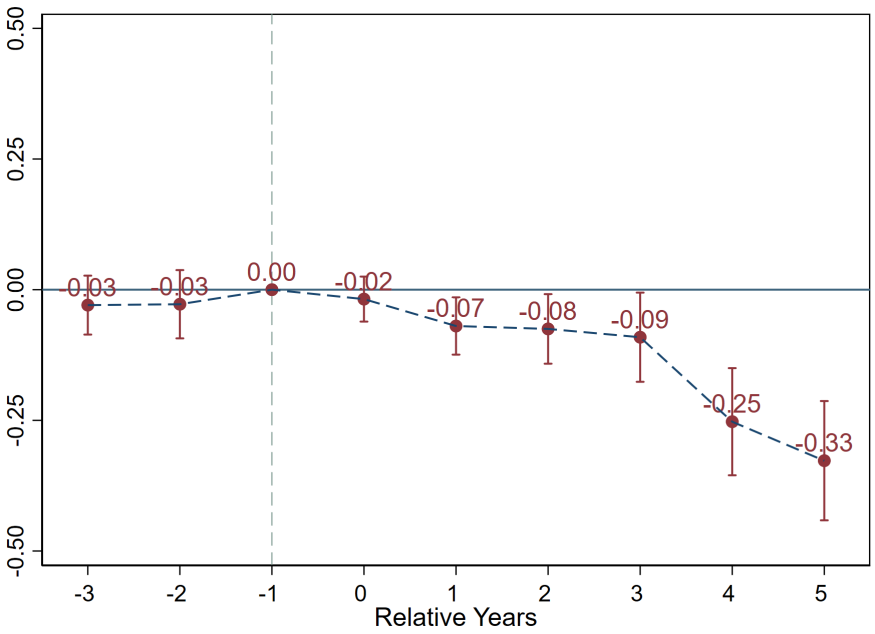


Figure 5: Impact of the OCP Rollout on Second-generation Women's Sibling Size. This figure plots the coefficients estimated using Eq. 2. The dependent variable is second-generation women's sibling size. Data are from the 2005 mini-census and the sample includes urban Han women born during 1975-1980. Control variables include province fixed effects and cohort fixed effects. Robust standard errors are clustered at province level. Vertical bars represent confidence intervals at the 90% level.

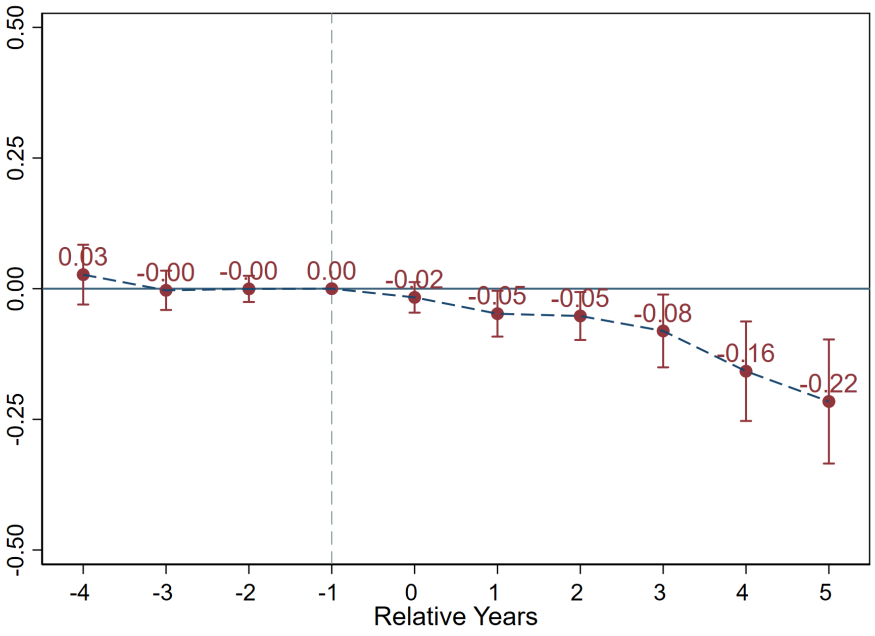
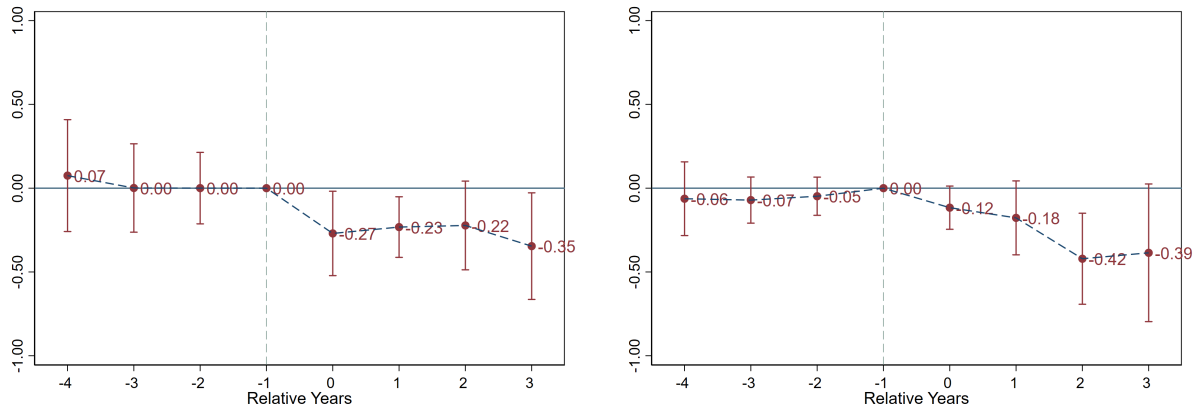


Figure 6: Impact of the OCP Rollout on Second-generation Women's Fertility. This figure plots the coefficients estimated using Eq. 2. The dependent variable is the number of children ever born by 2020. Data are from the 2020 census and the sample includes urban Han women born during 1969 -1980. Control variables include province fixed effects, cohort fixed effects, and province-specific cohort linear trends. Robust standard errors are clustered at the province level. Vertical bars represent confidence intervals at the 90% level.



(a) Second Generation

(b) Third Generation

Figure 7: Impact of the OCP Rollout on Desired Fertility for the Second Generation and Third Generation. These figures plot coefficients estimated using Eq. 2. The dependent variable is desired fertility. Figure (a) shows the impact of the OCP rollout on desired fertility for the second generation. Figure (b) shows the impact of the OCP rollout on desired fertility for the third generation. Data are from CFPS 2012, 2014, and 2018, and samples are restricted to urban Han mothers born during 1969 - 1980. Control variables include province fixed effects, cohort fixed effects, and province-specific cohort linear trends. Robust standard errors are clustered at province level. Vertical bars represent confidence intervals at the 90% level.

Table 1: Summary Statistics of Censuses

Variables	(1) *Mean*	(2) *Std.Dev.*	(3) *N*
Panel A: Second-generation Fertility in Census 2020			
Number of children ever born by 2020	1.456	0.220	75,438
Panel B: First-generation Fertility in Mini-Census 2005			
Sibling size of second-generation women	1.357	0.440	32,636
Panel C: Second-generation Fertility before OCP Relaxation in Mini-census 2015			
Number of children ever born by 2013	1.374	0.136	59,561
Panel D: Second-generation Variables before OCP Relaxation in Census 2010			
Human capital accumulation			
Primary school completion (Yes = 1)	0.994	0.009	198,203
Junior high school completion (Yes = 1)	0.876	0.083	198,203
Senior high school completion (Yes = 1)	0.389	0.123	198,203
College completion (Yes = 1)	0.077	0.051	198,203
Labor market performance			
Working at least one hour in the past week (Yes = 1)	0.758	0.055	198,203
Participating in the labor market (Yes = 1)	0.810	0.053	198,203
Total working hours in the past week	36.350	3.217	189,414
Living arrangement			
Coresiding with parents or parents-in-law (Yes = 1)	0.164	0.068	189,431

Notes: This table presents summary statistics for fertility outcomes and related variables in China population censuses. Panel A shows the number of children ever born to second-generation women in the 2020 census. Panel B shows second-generation women's sibling size, which represents first-generation women's fertility. Sibling size data are only available in the 2005 mini-census. Panel C shows the number of children born to second-generation women before the OCP relaxation in 2013, as observed in the 2015 mini-census. Panel D shows before-relaxation outcomes of second-generation women, including human capital accumulation, labor market performance, and living arrangements in the 2010 census. Panels A, C, and D include urban Han women born during 1969–1980. Panel B include urban Han women born during 1975–1980, because individuals born before 1975 do not report sibling size in the 2005 mini-census.

Table 2: Impact of the OCP Rollout on Fertility Outcomes

	(1)	(2)	(3)
Sample	*mini-Census 2005*	*Census 2020*	*mini-Census 2015*
Dependent Variable	*First-generation Fertility (Second-generation Sibling Size)*	*Second-generation Fertility by 2020*	*Second-generation Fertility by 2013 before OCP Relaxation*
Relative Year -4	-	0.027	-0.028
	-	(0.035)	(0.043)
Relative Year -3	-0.030	-0.003	0.006
	(0.034)	(0.023)	(0.033)
Relative Year -2	-0.028	-0.000	-0.041
	(0.040)	(0.015)	(0.031)
Relative Year -1	-	-	-
	-	-	-
Relative Year 0	-0.018	-0.017	-0.026
	(0.026)	(0.018)	(0.041)
Relative Year 1	-0.070**	-0.048*	-0.018
	(0.033)	(0.027)	(0.045)
Relative Year 2	-0.075*	-0.052*	-0.013
	(0.040)	(0.028)	(0.062)
Relative Year 3	-0.091*	-0.081*	-0.022
	(0.052)	(0.042)	(0.073)
Relative Year 4	-0.253***	-0.158**	-0.049
	(0.062)	(0.058)	(0.098)
Relative Year 5	-0.327***	-0.216***	-0.043
	(0.069)	(0.072)	(0.134)
Observations	32,636	75,438	59,561
Mean of dep. var.	1.357	1.456	1.374

Notes: This table shows the coefficients estimated using Eq. 2. Column (1) shows the impact of the OCP rollout on the sibling size of second-generation women. Data are from the mini-census 2005. Samples are restricted to second-generation Han women with urban Hukou born during 1975–1980, because individuals born before 1975 do not report the number of siblings in the 2005 mini census. Control variables include province and cohort fixed effects. Column (2) shows the impact of the OCP rollout on the number of children ever born to second-generation women in the 2020 census. Column (3) shows the impact of the OCP rollout on the number of children born to second-generation women before the OCP relaxation in 2013, as observed in the 2015 mini-census. Samples in Columns (2) and (3) are restricted to urban Han women born between 1969 and 1980. Control variables include province fixed effects, cohort fixed effects, and province-specific linear cohort trends. Robust standard errors clustered at province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$