The Unintended Consequences of Relaxing Birth Quotas: Theory and Evidence*

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Abstract

This study investigates how relaxing institutional fertility constraints reshapes household fertility choices along both the extensive and intensive margins. We develop an extended Barro-Becker framework showing that household heterogeneity and child-rearing costs generate asymmetric responses to fertility policy changes. Exploiting the staggered relaxation of China's one-child policy across provinces and employing a difference-in-differences framework, we find an asymmetric fertility response: second-child births increase substantially, whereas first-child births decline or are postponed in the short run. Dynamic analyses reveal that the increase in second-child births emerges gradually and becomes most pronounced several months after the effective relaxation of birth quotas, while the decline in first-child births is concentrated among households facing greater economic constraints associated with family formation. Consistent with the theoretical predictions, we further provide evidence that relaxing birth quotas was accompanied by increases in consumer prices, housing prices, and childcare-related labor costs, suggesting that rising child-rearing costs partially offset the fertility gains generated by institutional relaxation. Our findings highlight that removing institutional constraints alone may not be sufficient to promote fertility. Complementary policies that reduce child-rearing costs and ease economic barriers to family formation may therefore be essential for supporting fertility transitions.

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Keywords: birth quotas; fertility transition; child-rearing costs; childbearing decisions; China

1. Conflict of Interest:

The authors declare that they have no conflict of interest.

2. Data availability statement:

Data will be made available on request. The population census data used in this study are publicly available from the National Bureau of Statistics of China.

1 Introduction

China is currently experiencing a profound demographic transition characterized by persistently low fertility and rapid population aging. In response, Chinese authorities have gradually relaxed institutional constraints on fertility over the past decade, evolving from the One-Child Policy (OCP) to the partial two-child policy, the universal two-child policy, and more recently the three-child policy. These reforms were intended to encourage fertility by allowing previously constrained couples to have additional children.

Yet the fertility response to these reforms has been far from uniform. Following the relaxation of birth quotas, second and higher-order births increased substantially, as expected. At the same time, first births continued to decline sharply. Between 2011 and 2019, the number of second and higher-order births increased from 5.4 million to 8.7 million, while first births declined from 10.6 million to 5.9 million (National Bureau of Statistics of China, 2023). This divergence raises an important question: does relaxing birth quotas generate asymmetric fertility responses along different margins of fertility choice? Specifically, does it affect the intensive margin (the decision to have an additional child) differently from the extensive margin (the decision to enter parenthood)? If so, what mechanisms explain these asymmetric responses?

This paper develops a conceptual framework to understand how relaxing birth quotas reshapes household fertility choices. Building on the Barro-Becker framework (Becker and Barro, 1988; Barro and Becker, 1989), we incorporate birth quotas and heterogeneous fertility preferences across households. The framework highlights two competing channels. The first is a limits-raising channel, through which relaxing birth quotas allows households that were previously constrained by the OCP to have additional children. This channel primarily increases higher-order births among households with stronger fertility preferences.

The second is a child-rearing cost channel. Relaxing birth quotas may increase demand for child-related goods and services, including childcare, housing, food, education, and caregiving services. In the short run, these demand pressures may increase equilibrium child-rearing costs. Such cost increases may be particularly important for households considering whether to enter parenthood, especially those facing stronger financial and time constraints. Therefore, relaxing birth quotas may simultaneously increase second-child births while discouraging or delaying first-child births.

This mechanism is especially relevant in China, where the cost of raising children is high and formal family support systems remain limited. According to the YuWa Population Research Institute, the average cost of raising a child to age 17 in China reached 485,000 yuan in 2019, equivalent to 6.9 times per capita GDP (YuWa Population Research Institute, 2022). Child-rearing

costs are even higher in major cities. At the same time, childcare provision, parental leave support, housing assistance, and affordable caregiving services remain insufficient. These conditions make China an important setting for studying whether relaxing institutional fertility constraints can generate unintended equilibrium effects on child-rearing costs.

Empirically, we exploit the staggered implementation of China's partial two-child policy across provinces in 2014 and employ a difference-in-differences framework. This setting provides policy variation across both space and time. Because fertility responses occur with delays after institutional changes, we distinguish between the timing of policy implementation and the timing at which the reform can plausibly affect realized outcomes. We therefore account for the institutional, behavioral, and biological processes through which policy relaxation translates into realized births and economic adjustments. We construct monthly province-level fertility outcomes using China's 2015 population census and adopt recent difference-in-differences methods that account for heterogeneous treatment effects under staggered adoption (de Chaisemartin and D'Haultfoeuille, 2020). We further conduct a range of placebo and falsification tests to support the validity of the identification strategy.

A key feature of our empirical setting is that it allows us to distinguish fertility responses by birth order. Rather than focusing only on aggregate fertility, we separately examine first-child births, second-child births, and total births. We also construct measures of child-rearing costs, including consumer prices, residential housing prices, and childcare-related labor costs, allowing us to investigate whether relaxing birth quotas altered the economic environment in which households make fertility decisions.

Our empirical findings reveal several important patterns. First, relaxing birth quotas substantially increased second-child births. The response emerges gradually and becomes strongest several months after the effective policy change, consistent with the time required for households to adjust fertility plans and for fertility decisions to translate into births. The increase was particularly pronounced among groups with stronger incentives to have additional children, including younger women, households without a male child, and households covered by pension programs. These patterns suggest that relaxing birth quotas primarily released previously constrained fertility demand at the intensive margin.

Second, we found evidence of a short-run decline or postponement in first-child births following the relaxation of birth quotas. The reduction was economically meaningful and was more concentrated among groups facing greater economic constraints associated with entering parenthood, including younger women, less-educated women, and non-homeowners. This finding highlights that entry into parenthood responds differently from higher-order fertility decisions, as first births are more closely related to the economic environment surrounding family forma-

tion.

Third, we provided evidence that relaxing birth quotas was accompanied by increases in child-rearing costs. Consumer prices, housing prices, and childcare-related labor costs increased following the reform. These cost responses emerged before the strongest fertility responses, consistent with the interpretation that changes in the economic environment surrounding child-rearing may partially offset the fertility gains generated by institutional relaxation.

Finally, the aggregate effect on total births was positive, indicating that the increase in second-child births more than offset the decline or postponement of first-child births. However, the aggregate response remained substantially smaller than the increase in second-child births alone. Taken together, these findings demonstrate that relaxing fertility constraints does not generate a uniform increase in fertility. Instead, fertility policy reforms reshape the composition and timing of births across fertility margins by simultaneously releasing constrained fertility demand among some households and altering fertility decisions among others.

This paper contributes to three strands of literature. First, it contributes to the literature on fertility economics and demographic transitions by distinguishing between extensive-margin and intensive-margin responses in fertility choices (Doepke, Hannusch, Kindermann and Tertilt, 2023; Bailey, 2025). Recent research emphasizes that fertility decisions involve multiple dimensions, including whether to enter parenthood, when to have children, and how many children to have. While existing studies have largely focused on fertility levels or completed fertility, we show that the removal of institutional fertility constraints can generate asymmetric responses across these margins. Specifically, relaxing birth quotas facilitates progression to additional births among households with stronger fertility preferences, while potentially delaying entry into parenthood among households facing greater child-rearing constraints. Existing evidence has primarily focused on fertility choices in developed-country contexts, including studies such as Gobbi (2013), Aaronson, Lange and Mazumder (2014), Baudin, De La Croix and Gobbi (2015), and Hwang (2023). However, little is known about how large-scale institutional reforms that remove fertility constraints affect different margins of fertility choices in developing countries. We extend this literature by studying China's relaxation of birth quotas, which provides a unique setting to examine asymmetric fertility responses following the removal of institutional constraints.

Second, this paper contributes to the literature on China's fertility policy reforms and the mechanisms through which institutional changes shape fertility responses. Existing studies have examined the effects of the OCP on outcomes including fertility, household behavior, sex ratios, labor supply, human capital, savings, and marriage markets (Ebenstein, 2010; Li, Yi and Zhang, 2011; Wang, Zhao and Zhao, 2017; Zhang, 2017; Rosenzweig and Zhang, 2009; Qian, 2009; Liu, 2014; Huang, Pan and Zhou, 2023; Huang, Lei and Sun, 2021; Guo, Yi, Zhang and Zhang, 2025; Li

and Shi, 2025; Fang and Liu, 2026). We extend this literature by showing that relaxing birth quotas affects not only fertility decisions but also the resource allocation environment in which households make child-rearing decisions. While existing theoretical studies of China's fertility policy reforms have emphasized channels such as quantity-quality tradeoffs and general-equilibrium wage effects (Liao, 2013; Choukhmane, Coeurdacier and Jin, 2023), we highlight a complementary equilibrium channel operating through child-rearing costs. This mechanism helps explain why relaxing birth quotas may increase higher-order births while discouraging or delaying first births.

Third, this paper provides additional evidence on heterogeneity in fertility responses and the underlying motives for childbearing. By comparing fertility responses across households with and without pension coverage, we examine whether reliance on children for old-age support shapes responses to birth quotas relaxation (Chen *et al.*, 2023). Our findings provide suggestive evidence that second-child fertility responses are more consistent with intergenerational altruism than with a pure old-age security motive (Nugent, 1985; Becker and Barro, 1988; Cigno, 1992; Rendall and Bahchieva, 1998; Rossi and Godard, 2022).

The rest of the paper is organized as follows. Section 2 presents institutional background and descriptive evidence on fertility patterns in China. Section 3 develops the conceptual framework. Section 4 describes the data and key variables. Section 5 presents the empirical strategy. Section 6 reports the empirical results. The final section concludes.

2 Institutional Background and Descriptive Evidence

Before turning to the conceptual framework and causal analysis, we document a set of stylized facts regarding fertility responses following the relaxation of birth quotas in China. Following Hazan and Zoabi (2015), we distinguish between fertility choices along two margins: the extensive margin, referring to the decision to enter parenthood, and the intensive margin, referring to the decision to have an additional child. This distinction is central to our analysis because aggregate fertility measures may conceal substantial heterogeneity across birth orders. While relaxing birth quotas is expected to increase fertility by removing institutional constraints, the descriptive evidence reveals that fertility responses differ substantially across birth orders.

Using population census data from 2000, 2005, 2010, and 2015, together with national birth statistics, we document fertility patterns at both aggregate and subgroup levels. The descriptive evidence reveals a striking divergence between extensive and intensive fertility margins after 2010. While second-child and higher-order births rebounded following the relaxation of birth quotas, first-child births continued to decline. We further show that these patterns vary system-

atically across urban and rural populations, age groups, education levels, and housing ownership status. These descriptive patterns motivate the central question of this paper: why does relaxing institutional fertility constraints stimulate higher-order births while delaying entry into parenthood for some households?

2.1 China's OCP and Its Reforms

The One-Child Policy (OCP) was introduced in China in 1979 and created substantial institutional constraints on fertility choices. Under the policy, most couples were restricted to having only one child, with violations subject to penalties including fines and administrative sanctions (Ebenstein, 2010). The policy was implemented more strictly in urban areas, while rural couples faced somewhat different eligibility rules. Beginning in 1984, some rural couples were allowed to have a second child if their first child was female, a policy commonly referred to as the "1.5-child policy" (Zhang, 2017).

As fertility declined and demographic pressures increased, China gradually relaxed birth quotas. In 2011, couples in which both spouses were only children became eligible to have a second child. In November 2013, eligibility was further expanded to couples in which only one spouse was an only child (Ouyang, 2013). This reform, known as the partial two-child policy, was subsequently implemented by provincial governments throughout 2014. In December 2015, China introduced the universal two-child policy, allowing all couples to have two children and removing remaining birth quota restrictions (Li *et al.*, 2019).

Although the universal two-child policy represented a broader reform, this paper focuses on the partial two-child policy because it generated substantial variation in implementation timing across provinces. This staggered adoption provides a suitable setting for identifying the causal effects of relaxing birth quotas. More importantly, the reform offers an opportunity to examine whether fertility responses were uniform across birth orders or whether removing institutional constraints generated heterogeneous responses along different margins of fertility choice.

2.2 Overall Fertility Patterns

We begin by examining aggregate fertility patterns following the relaxation of birth quotas. Figure 1 presents annual data on first births and second-and-higher order births reported by the National Bureau of Statistics between 2011 and 2019. Two striking patterns emerge. First, second and higher-order births increased substantially following the partial and universal two-child reforms. Second, despite this increase, first births continued to decline throughout the same period.

These contrasting trends provide the first descriptive evidence of asymmetric fertility responses. Rather than generating a uniform increase in fertility, the relaxation of birth quotas appears to have primarily stimulated higher-order births while coinciding with continued postponement or decline in first births. This divergence motivates our subsequent analysis of extensive- and intensive-margin fertility responses.

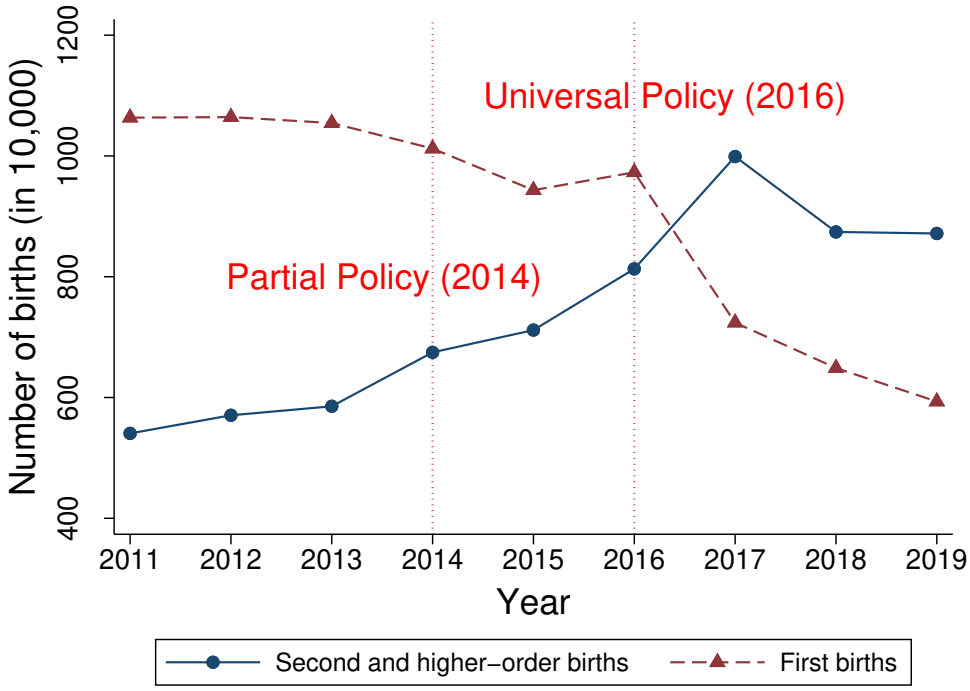


Figure 1: Number of second and higher-order births and first births (2011-2019).

Notes: The solid line denotes the trends in the number of second and higher-order births from 2011 to 2019. The dashed line denotes the trends in the number of first births from 2011 to 2019. Partial Policy (2014) refers to the partial two-child policy that was announced at the national level in November 2013 and was then implemented by local governments at different time points in 2014. Universal Policy (2016) refers to the universal two-child policy that was announced at the national level in December 2015 and was then implemented by local governments at different points in time in 2016. Annual data on the number of second and higher-order births and first births are reported by the National Bureau of Statistics of China.

Figure 2 further examines fertility dynamics using population census data from 2000, 2005, 2010, and 2015. Panel (a) shows that the total fertility rate (TFR) declined steadily throughout the period. Panel (b) reveals that the conditional second-child rate decreased before 2010 but rebounded afterward, indicating a recovery in higher-order fertility following the relaxation of birth quotas. In contrast, Panel (c) shows that first-child fertility continued to decline. Panel (d) documents a persistent increase in the mean age at childbearing (MAC), suggesting continued postponement of family formation.

Taken together, these patterns reveal a clear divergence between extensive and intensive fertility margins. While the relaxation of birth quotas encouraged additional births among households that already had children, it did not reverse the decline in entry into parenthood. This pattern is consistent with the possibility that institutional constraints and child-rearing costs operate through different channels.

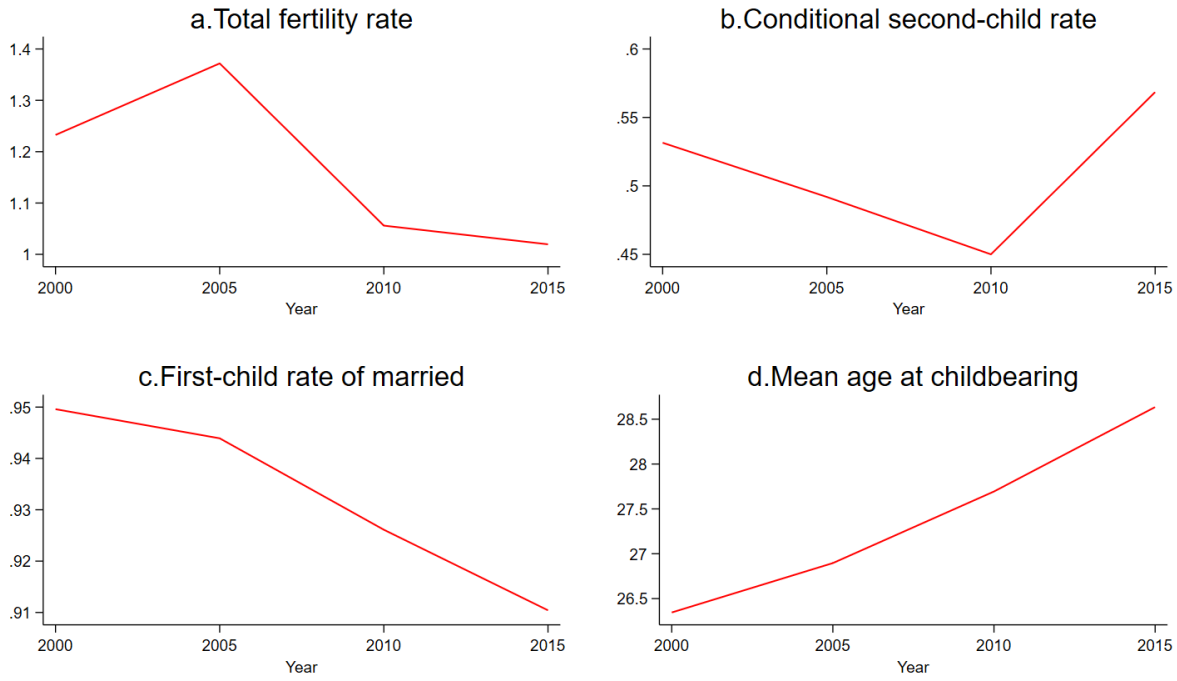


Figure 2: Fertility patterns by extensive and intensive margins (2000–2015).

Notes: Panel (a) shows the total fertility rate (TFR), representing the average number of children per woman. Panel (b) presents the conditional second-child rate, the probability of having a second child given a first child. Panel (c) illustrates the first-child rate among married women, capturing extensive-margin fertility. Panel (d) shows the mean age at childbearing (MAC). All data are derived from China's Population Censuses in 2000, 2005, 2010, and 2015.

2.3 Fertility Patterns by Urban and Rural Residence

Figure 3 examines whether the divergence between extensive and intensive fertility margins differs across urban and rural populations.

Panel (a) shows that rural women consistently exhibited higher fertility levels than urban women throughout the sample period. Both groups experienced fertility declines before 2010, although the decline was more pronounced among urban women. After 2010, fertility stabilized

somewhat among urban women, whereas rural fertility continued to decline gradually.

Panel (b) reveals important differences in intensive-margin responses. Conditional second-child fertility increased in both urban and rural areas after 2010, but the increase was substantially larger among urban women. This pattern suggests that households with greater economic resources and stronger capacity to absorb child-rearing costs were more responsive to the relaxation of birth quotas.

Panel (c) shows that first-child fertility declined for both urban and rural populations. The decline was more pronounced among urban women, consistent with stronger postponement of family formation in environments where child-rearing costs, housing costs, and career constraints are particularly salient. Panel (d) further demonstrates a persistent increase in the mean age at childbearing .

Overall, the urban-rural comparison indicates that asymmetric fertility responses are not confined to a specific population group. Instead, they emerge across both urban and rural China, although the magnitude of responses differs systematically with socioeconomic conditions.

2.4 Heterogeneous Patterns by Age, Skill, and Housing Ownership

Figures 4 and 5 further examine heterogeneity in fertility responses across demographic and socioeconomic groups.

Figure 4 focuses on the intensive margin. Following 2010, second-child fertility increased most strongly among younger women, more-educated women, and homeowner households. In contrast, the response was weaker among older women, less-educated women, and households without housing ownership.

Figure 5 examines the extensive margin. First-child fertility declined most sharply among groups likely to face greater economic constraints, including older women, less-educated women, and non-homeowner households. By contrast, younger women, highly educated women, and homeowners experienced either smaller declines or partial recoveries in first-child fertility.

These heterogeneous responses provide additional evidence that fertility responses depend on household characteristics and economic constraints. Households with stronger fertility preferences and greater ability to absorb child-rearing costs appear more likely to increase higher-order fertility after quota relaxation. In contrast, households facing greater costs associated with entering parenthood appear more likely to delay or reduce first births.

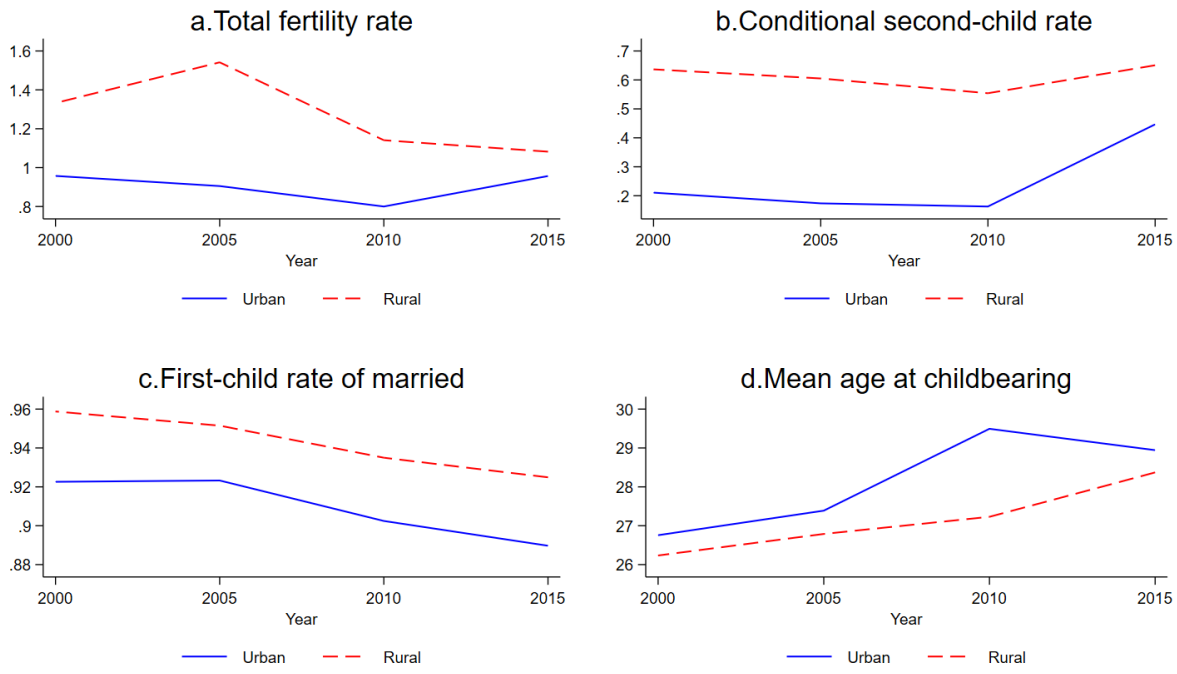


Figure 3: Fertility patterns by extensive and intensive margins, by urban and rural residence (2000–2015).

Notes: Panel (a) shows the total fertility rate (TFR), representing the average number of children per woman. Panel (b) presents the conditional second-child rate, the probability of having a second child given a first child. Panel (c) illustrates the first-child rate among married women, capturing extensive-margin fertility. Panel (d) shows the mean age at childbearing (MAC). Blue lines indicate urban populations, and red lines indicate rural populations. Data are derived from China's Population Censuses in 2000, 2005, 2010, and 2015.

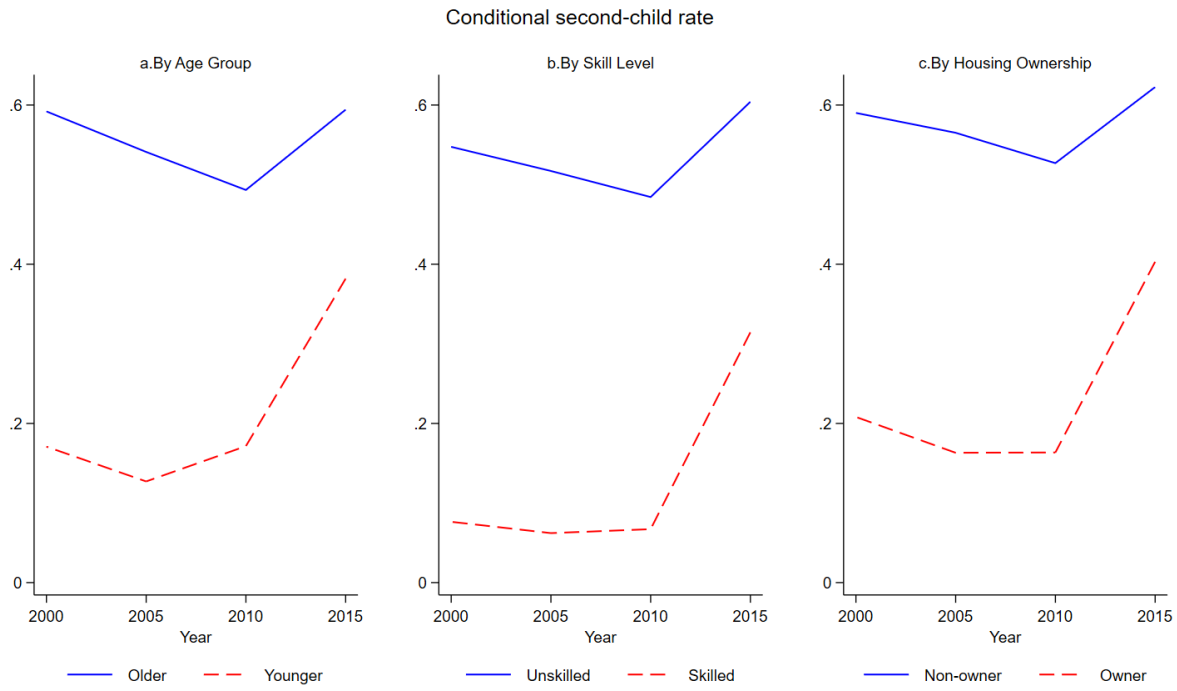


Figure 4: Conditional second-child trends by age, skill, and housing ownership.

Notes: Panels (a)–(c) present the conditional second-child rate, which measures the probability of having a second child given a first birth, by age group (panel a), skill level (panel b), and housing ownership (panel c). Age groups are defined as younger women (27 years and below) and older women (28 years and above); skill level is categorized as skilled (with a college degree) or unskilled (without a college degree); housing ownership distinguishes households that have purchased a house (owner) from those that have not (non-owner). All data are derived from China's Population Censuses in 2000, 2005, 2010, and 2015.

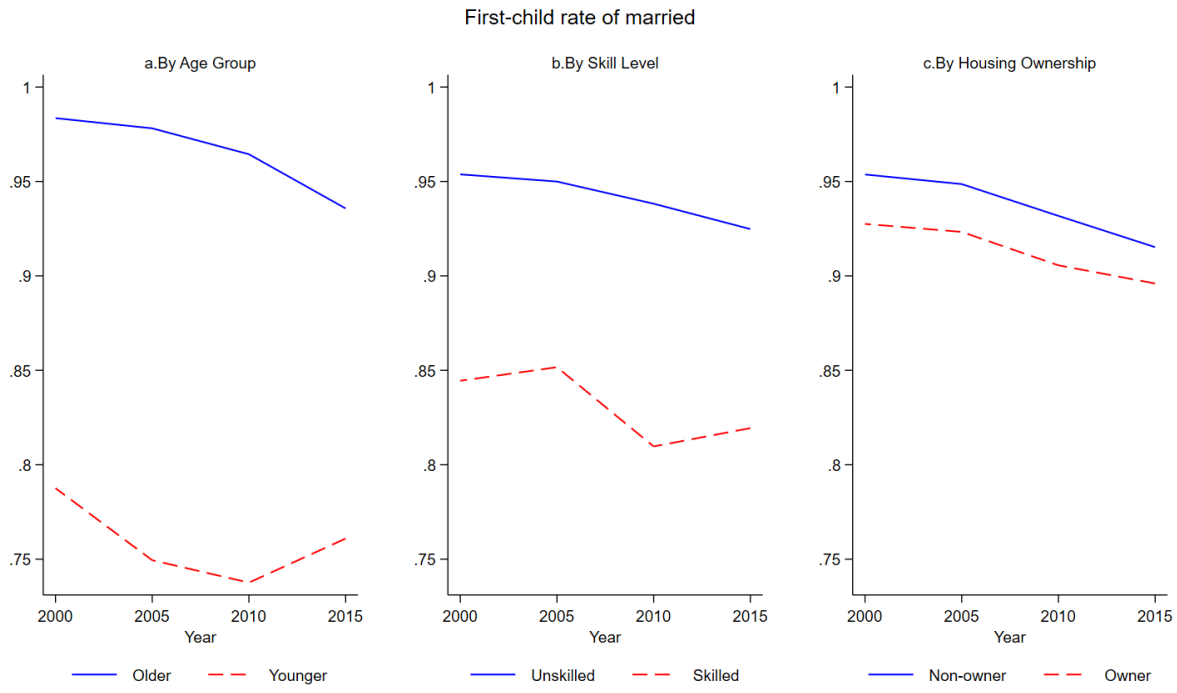


Figure 5: First-child trends by age, skill, and housing ownership.

Notes: Panels (d)–(f) present the first-child rate among married women, capturing extensive-margin fertility, by age group (panel d), skill level (panel e), and housing ownership (panel f). Age groups are defined as younger women (27 years and below) and older women (28 years and above); skill level is categorized as skilled (with a college degree) or unskilled (without a college degree); housing ownership distinguishes households that have purchased a house (owner) from those that have not (non-owner). All data are derived from China's Population Censuses in 2000, 2005, 2010, and 2015.

2.5 Summary of Descriptive Evidence

The descriptive evidence reveals a clear divergence between fertility responses along extensive and intensive margins following the relaxation of birth quotas. While second-child fertility rebounded after 2010, first-child fertility continued to decline, accompanied by a steady increase in the mean age at childbearing. These contrasting patterns suggest that fertility policy relaxation does not simply increase fertility uniformly but reshapes the timing and composition of births.

The divergence is also systematically related to household characteristics. The rebound in second-child fertility is concentrated among groups with stronger fertility preferences and greater economic capacity, whereas declines in first-child fertility are more pronounced among groups facing stronger constraints associated with family formation.

These stylized facts motivate our conceptual framework, which formalizes how institutional constraints and child-rearing costs jointly shape fertility choices. The empirical analysis then examines whether these mechanisms can explain the observed asymmetric fertility responses.

3 Conceptual Framework

In this section, we expand upon the Barro-Becker framework, integrating birth quotas and heterogeneous fertility preferences among households into a dynamic general equilibrium model. This theoretical examination of the economic implications of easing birth quotas lays the foundation for our empirical investigation.

3.1 Households

The economy consists of two distinct types of households, each characterized by different fertility preferences: high-fertility households (α^h) and low-fertility households (α^l), where $\alpha^l < \alpha^h$. For example, households willing to have two children can be considered as high-fertility households, while those preferring only one child can be considered low-fertility households. The population sizes of high and low fertility households are represented by q and $(1-q)$, respectively, where $q \in (0,1)$. For simplicity, we assume uniform fertility preferences within each household, and these preferences remain constant among descendants within the same household over time.

Each individual experiences two distinct life stages: childhood and adulthood. During each stage, adults decide on the desired number of children and raises them accordingly.[1] Specifically, at the onset of $t = 0$, each adult in household α^h selects a number of children, denoted as $n(\alpha^h, 0)$,

[1] To simplify our model, we do not consider gender difference and the intricacies of the fertility process.

to raise during that period. In generation $t = 1$, the offspring, numbering $q \cdot n(\alpha^h, 0)$, mature into adults. Each adult then make decisions regarding the desired number of children to raise during this period, denoted as $n(\alpha^h, 1)$. Consequently, in generation $t = 1$, household α^h consists of $q \cdot n(\alpha,^h 0)$ adults and $q \cdot n(\alpha^h, 0) \cdot n(\alpha^h, 1)$ children. The birth rate of household α^h is determined by $n(\alpha^h, 1)$.

Extending this reasoning to generation t, the total numbers of adults and children in household α^h become $q \cdot n(\alpha^h, 0) \cdot n(\alpha^h, 1) \cdots n(\alpha^h, t-1) = q \cdot \prod_{t=0}^{t-1} n(\alpha^h, t) = qN(\alpha^h, t)$ and $qN(\alpha^h, t+1)$, respectively. Consequently, the birth rate of household α^h is given by $n(\alpha^h, t)$. A similar analysis applies to household α^l, where the total numbers of adults and children, and the corresponding birth rate in generation t are $(1-q) \cdot \prod_{t=0}^{t-1} n(\alpha^l, t) = (1-q)N(\alpha^l, t)$, $(1-q)N(\alpha^l, t+1)$, and $n(\alpha^l, t)$, respectively.

Building upon the frameworks established by Becker and Barro (1988) and Barro and Becker (1989), we adopt a utility function for each adult in generation t within household $\alpha = \{\alpha^h, \alpha^l\}$:

$$U(\alpha, t) = [c(\alpha, t)]^\sigma / \sigma + a(n(\alpha, t)) \cdot n(\alpha, t) \cdot U(\alpha, t+1), \quad \sigma < 1 \tag{1}$$

where $1/(1-\sigma)$ represents the elasticity of intertemporal substitution in consumption, $c(\alpha, t)$ denotes the adult's consumption of the final good, and $U(\alpha, t+1)$ represents the utility attained by each child. Altruism levels vary among parents with different fertility preferences, with higher degrees of altruism corresponding to a greater likelihood of having more children.[2] The function $a(n(\alpha, t))$ quantifies altruism towards each child, defined as:

$$a(n(\alpha, t)) = \alpha \cdot [n(\alpha, t)]^{-\epsilon}, \quad 0 < \epsilon < 1. \tag{2}$$

It's important to note that the degree of altruism decreases as the number of children increases, as indicated by Equation (2).

In a competitive labor market, each adult has one unit of time per period and earns uniform wage rates, denoted as $w(t)$. Parents pass on capital, represented by $k(\alpha, t+1)$, to each child born in generation $t+1$. This capital earns a rental yield at the rate $r(t)$ during generation t. For an adult of household α during generation t, their earnings and inheritance, i.e., $w(t) + [1 + r(t)]k(\alpha, t)$, are allocated among personal consumption $P(t)c(\alpha, t)$, bequests to children $n(\alpha, t)k(\alpha, t+1)$, and child-rearing costs. Here, $P(t)$ represents the prices of the final good.

Raising children incurs costs, denoted as $\beta(\alpha, t)$, which make the total expenses for raising $n(\alpha, t)$ children equal to $n(\alpha, t)\beta(\alpha, t)$. Consequently, the budget constraint for an adult in household α in generation t is expressed as: $w(t) + [1 + r(t)]k(\alpha, t) = P(t)c(\alpha, t) + n(\alpha, t)[\beta(\alpha, t) +$

[2] Fertility preferences can be affected by factors such as individual characteristics and culture.

$k(\alpha, t+1)]$.

The equilibrium conditions, derived from solving the optimization problem, are summarized in the following lemma.

Lemma 1. *The optimal consumption and birth rate are given by, respectively,*

$$c(\alpha,t) = \left(\frac{\sigma}{1-\epsilon-\sigma}\right)\frac{\beta(\alpha,t-1)[1+r(t)]-w(t)}{P(t)}, \quad (t=1,2,\cdots) \tag{3}$$

$$[n(\alpha,t)]^{\epsilon} = \alpha[1+r(t+1)]\left[\frac{P(t)}{P(t+1)}\right]^{\sigma}\left\{\frac{\beta(\alpha,t-1)[1+r(t)]-w(t)}{\beta(\alpha,t)[1+r(t+1)]-w(t+1)}\right\}^{1-\sigma}, \quad (t=1,2,\cdots) \tag{4}$$

Proof. See the Appendix A.1. □

Equation (3) shows that an increase in the net cost of creating a descendant in generation t leads to a rise in consumption per person for that generation. Intuitively, when people are more costly to produce, it is optimal to endow each person produced with a higher level of consumption. Equation (4) indicates that the optimal number of children $n(\alpha,t)$, for all $t \geq 1$, born to each adult of household α, depends on several factors including the degree of altruism, prices of the final good, interest rates, wage rates, and child-rearing costs at different periods $\beta(\alpha,t-1)$ and $\beta(\alpha,t)$. The initial birth rate $n(\alpha,0)$ is determined in (A.6).

We further assume that child-rearing costs are identical across households in the same period,[3] and only the final good is required such that[4]

$$\beta(\alpha,t) = \beta(t) = \mu P(t), \quad \mu > 0. \tag{5}$$

Given this assumption, Equation (4) then shows that the difference in birth rates is driven by unequal fertility preferences in that the macroeconomic factors (i.e., $P(t)$, $r(t)$ and $w(t)$) affect households symmetrically. Therefore, the relation $n(\alpha^h,t) > n(\alpha^l,t)$ must hold for all $t \geq 1$ due to $\alpha^h > \alpha^l$.

3.2 Production

In this economy, there's a single final good produced within a competitive production sector. A typical firm employs labor $L(t)$ and capital $K(t)$ to produce the final good, following the

[3]This assumption simplifies the model analysis and does not seem to conflict with any direct evidence linking child-rearing costs with families' fertility preferences.

[4]The assumption, that raising a child does not necessitate adults to spend time, is vital for guaranteeing the uniqueness of the steady-state equilibrium. For an in-depth discussion, refer to Barro and Becker (1989). Moreover, this assumption proves sufficient in capturing the effect of relaxing birth quotas on child-rearing costs.

Cobb-Douglas technology $Y(t) = [L(t)]^{\eta}[K(t)]^{1-\eta}$. The demand for labor and capital, derived from solving the cost-minimization problem, are given by $L(t) = \eta P(t)Y(t)/w(t)$ and $K(t) = (1-\eta)P(t)Y(t)/r(t)$.

At this point, the numeraire in the economy has not been specified. Moving forward, we choose wage rates as the numeraire, denoting $w(t) \equiv 1$.[5] The prices of the final good and interest rates are expressed as:

$$P(t) = 1/\left\{\eta[k(t)]^{1-\eta}\right\}, \quad \text{and} \quad r(t) = (1-\eta)/\left[\eta k(t)\right], \tag{6}$$

where $k(t) \equiv K(t)/L(t)$ represents the capital stock per capita or per adult.

3.3 The Government

The government employs distinct fertility restrictions, known as birth quotas, to shape population growth. Let's denote the number of children each household has in generation t as $n(\alpha, t)$, subject to the condition $n(\alpha, t) \in [\underline{n}, \overline{n}]$, where $\underline{n}$ and $\overline{n}$ represent the minimum and maximum permissible number of children.

In our model, the government imposes strict control over the maximum number of children allowed for each household, denoted as $\tilde{n}$, where $\underline{n} < \tilde{n} < \overline{n}$. Consequently, the number of children $n(\alpha, t)$ in generation t adheres to the following condition:

$$n(\alpha, t) = \begin{cases} \tilde{n} & \text{if } n(\alpha, t) \geq \tilde{n} \\ n(\alpha, t) & \text{if } n(\alpha, t) < \tilde{n}. \end{cases} \tag{7}$$

3.4 Aggregation

The total number of adults at the beginning of $t = 0$ is defined as $\hat{N}(0) = q + (1-q) = 1$, and the total number of children in generation 0 or the total number of adults in generation 1 is $\hat{N}(1) = qn(\alpha^h, 0) + (1-q)n(\alpha^l, 0)$.

Suppose, at the beginning of generation $t = 1$, the government introduces a fertility policy restricting each household to a maximum of $\tilde{n} = n(\alpha^l, 1) > 0$ children——the number each agent in household α^l is willing to have. This restriction is not binding for α^l households but constraining for α^h-households when $n(\alpha^h, 1) > n(\alpha^l, 1)$ in a policy-free environment.

[5] This choice simplifies our analysis, particularly in capturing the effects of changes in fertility restrictions on prices of the final good and child-rearing costs. If, alternatively, the prices of the final good were chosen as the numeraire, capture the child-rearing costs would require incorporating the time spent by adults, adding complexity to our analysis.

Given this policy, the total number of adults at $t = 2$ and the total birth rate at $t = 1$ are $\hat{N}(2) = q \cdot n(\alpha^h, 0) \cdot \tilde{n} + (1-q) \cdot n(\alpha^l, 0) \cdot \tilde{n}$ and $B(t=1) = \hat{N}(2)/\hat{N}(1) = \tilde{n}$. With this fertility policy in place, all members in households α^h and α^l share the same birth rate starting from $t = 1$, and the birth rates of both types of households no long change. Accordingly, the total birth rate remains constant thereafter. Thus

$$B(t>1) = B(t=1) = n(\alpha^h, t>1) = n(\alpha^l, t>1) = \tilde{n}. \tag{8}$$

Given this consistent population growth rate, the economy is on a balanced growth path, signifying that the growth rate of output per adult is zero.[6]

3.5 Short-term effects of relaxing birth quotas

In this subsection, we explore the short-term effects of relaxing birth quotas, reserving the discussion on long-term effects for Appendix A.3. Suppose that at the outset of generation $t = s > 1$, the government eases the restricted fertility policy from $\tilde{n}$ to $n^* \in (\tilde{n}, n^h)$, where n^h is the birth rate of household α^h in the equilibrium without fertility control.[7]

Upon the introduction of the new fertility policy, agents in both types of households may adjust their fertility decisions in response to the potential change of macroeconomic factors.

We begin by analyzing the impact of the relaxation of birth quotas on the fertility decision of α^l-type households. Utilizing (5) and normalizing wage rate to one, we can simplify (4) to:

$$(\tilde{n})^\epsilon = \alpha^l \cdot [1 + r(s)]. \tag{9}$$

As the new fertility policy is implemented, the prices of the final good and interest rates may deviate from their previous balanced growth path equilibrium values. In this scenario, $n(\alpha^l, s)$ is determined by:

$$\left[n(\alpha^l, s)\right]^\epsilon = \alpha^l \cdot [1 + r(s+1)] \cdot \left[\frac{P(s)}{P(s+1)}\right]^\sigma \cdot \left\{\frac{\mu P(s-1)[1+r(s)] - 1}{\mu P(s)[1+r(s+1)] - 1}\right\}^{1-\sigma}. \tag{10}$$

Combining (9) and (10), we obtain:

$$\left[\frac{n(\alpha^l, s)}{n(\alpha^l, s-1)}\right]^\epsilon = \underbrace{\left[\frac{1+r(s+1)}{1+r(s)}\right]}_{\Phi_1 > 1} \cdot \underbrace{\left[\frac{P(s)}{P(s+1)}\right]^\sigma}_{\Phi_2 < 1} \cdot \underbrace{\left\{\frac{\mu P(s-1)[1+r(s)] - 1}{\mu P(s)[1+r(s+1)] - 1}\right\}^{1-\sigma}}_{\Phi_3 < 1}. \tag{11}$$

[6] We define the decentralized equilibrium in Section A.2.

[7] We consider the case of $n^* < n^h$ as an example; the case $n^* \geq n(\alpha^h, s-1)$ would lead to similar mechanisms.

Intuitively, raising the permitted number of children from $\tilde{n}$ to n^* allows the α^h-type households to have more children. Consequently, the number of children in generation s increases, leading to a decrease in capital per adult in generation $s+1$. This is the partial equilibrium result, denoted as the "limits-raising channel". Equation (11) then reveals that a relaxation of birth quotas generates additional general equilibrium influence by adjusting households' fertility decisions through the following channels.

From Equation (6), it is evident that both the prices of the final good and interest rates in period $s+1$ rise in response. Higher interest rates $r(s+1)$ (relative to $r(s)$) produces two opposing effects on the fertility decisions of α^l-type households.

On the one hand, it tends to raise the birth rate, captured by the aggregate term $\Phi_1 > 1$. Intuitively, increased interest rates $r(s+1)$ encourage households to have more children because fertility represents the rate of investment in these descendants. Higher interest rates imply increased return rates on saving in the form of the number of descendants. Therefore, households may choose to consume less and allocate more income to child-rearing, resulting in an increase in the number of descendants.[8]

On the other hand, higher interest rates can generate a counteracting negative effect on the birth rate of α^l-type households. Due to higher interest rates $1+r(s+1)$, the household faces an increased opportunity cost of raising a child; higher interest rates in the future make current spending on child-rearing more expensive, which is captured by $\Phi_3 < 1$. According to Equation (3), larger child-rearing costs in generation s are associated with higher consumption per adult in generation $s+1$. Then, as shown in Equation (4), the birth rates in generation s need to decrease to ensure an increase in consumption per adult in generation $s+1$.

Moreover, higher prices of the final good $P(s+1)$ (relative to $P(s)$), captured by $\Phi_2 < 1$, tend to decrease the birth rate of α^l-type households. Similarly, Equation (3) implies that higher prices of the final good reduces consumption per adult in generation $s+1$. As a result, households must limit the number of children in generation s to ensure consumption growth in generation $s+1$. We label the latter two negative effects, Φ_2 and Φ_3, together as "child-rearing cost channel". Therefore, if the "child-rearing cost channel" is sufficiently large, the birth rate of α^l-type households will decline, that is, $n(\alpha^l, s) < n(\alpha^l, s-1) = \tilde{n}$. Otherwise, the birth rate of α^l-type households will increase.

As for the impact on the fertility decision of α^h-type households, since the government relaxes the birth quotas on a large scale (i.e., n^* is significantly larger than $\tilde{n}$), supporting the dominance of the "limits-raising channel" (a partial equilibrium outcome) over the other general equilibrium

[8]This effect can also be verified in (A.8); the increased number of descendants comes at the cost of a lower level of consumption per adult. Since $\hat{N}(t)$ or $L(t)$, and $K(t)$ are given in generation t, the level of output would not be affected by the announcement of the new fertility policy.

impacts. Consequently, the birth rate of α^h-type households always increases. Now, let's examine the influence on the total birth rate, given by:

$$\begin{aligned} B(t=s) &= \frac{\hat{N}(s+1)}{\hat{N}(s)} = \frac{q \cdot n(\alpha^h,0) \cdot (\tilde{n})^{s-1} \cdot n^* + (1-q) \cdot n(\alpha^l,0) \cdot (\tilde{n})^{s-1} \cdot n(\alpha^l,s)}{q \cdot n(\alpha^h,0) \cdot (\tilde{n})^{s-1} + (1-q) \cdot n(\alpha^l,0) \cdot (\tilde{n})^{s-1}} \\ &= \frac{q \cdot n(\alpha^h,0) \cdot n^* + (1-q) \cdot n(\alpha^l,0) \cdot n(\alpha^l,s)}{q \cdot n(\alpha^h,0) + (1-q) \cdot n(\alpha^l,0)}. \end{aligned} \tag{12}$$

Since the total birth rate in periods ahead of the new fertility policy is $\tilde{n}$, we have

$$B(t=s) \gtrless B(t=s-1) \Leftrightarrow q \cdot n(\alpha^h,0) \cdot (n^* - \tilde{n}) \gtrless (1-q) \cdot n \cdot (\alpha^l,0) \cdot \left(\tilde{n} - n(\alpha^l,s)\right). \tag{13}$$

Given a sufficiently large relaxation of birth quotas (i.e., $n^* > \tilde{n}$), the total birth rate is likely to increase if (1) the birth rate of low preference households increases (i.e., $n(\alpha^l,s) > \tilde{n}$); (2) the size of household α^h at period $t = 1$ is large relative to that of household α^l, represented by the comparison of $qn(\alpha^h,0)$ and $(1-q)n(\alpha^l,0)$. The following proposition summarizes these results.

Proposition 1. *Relaxing birth quotas (i) increases short-term child-rearing costs; (ii) may increase or decrease the short-term birth rate of low preference households (α^l-type) and the total birth rate; (iii) increases the short-term birth rate of high preference households (α^h-type).*

Proof. Proven in text. □

3.6 Bridging theoretical predictions and empirical testing

To bridge theoretical predictions and empirical testing, we make several simplifications due to data limitations. First, we define fertility preferences based on the number of children desired. Households willing to have two children are categorized as high-fertility households, while those preferring only one child are classified as low-fertility households. Second, we focus on measuring fertility outcomes over a very short period of time. This approach allows us to assess the immediate responsiveness of fertility decisions and potential general equilibrium effects more accurately. However, it may weaken the connection between our empirical findings and the theoretical framework, which considers fertility decisions across two generations. Third, we examine the relaxation of birth quotas in the context of China's transition from the OCP, denoted as $\tilde{n} = 1$, to the universal two-child policy, where $n^* = 2$. According to Proposition 1, our model suggests that this relaxation alleviates restrictions on high-preference households, leading to an increase in second-child births. Nonetheless, the dynamic impact of this policy change extends beyond a mere rise in second-child births. This surge in the number of children could trigger equilibrium effects in the economy, which may subsequently influence the fertility desires of low-preference

households. Consequently, we may observe either an increase or a decrease in first-child births. If the rise in second-child births does not sufficiently offset any decline in first-child births, the overall birth might actually decrease following the relaxation of birth quotas.

4 Data, Variables, and Summary Statistics

4.1 Childbearing outcomes

To examine how the relaxation of birth quotas affects fertility choices along different margins, we construct birth-order-specific fertility outcomes using a 10% random sample from China's 2015 population census conducted by the National Bureau of Statistics (NBS). The census dataset contains approximately 1.37 million individuals and provides detailed retrospective information on childbirth histories for women aged between 15 and 50, including the month and year of each childbirth. This feature allows us to construct fertility outcomes at a monthly frequency, which is important for estimating dynamic responses to the staggered implementation of the policy reform.

A key advantage of the census data is that they allow us to distinguish fertility responses across birth orders. Specifically, we use information on the timing and order of childbirth to construct monthly province-level measures of first-child births, second-child births, and total births. This distinction enables us to examine fertility responses along the extensive margin (the decision to enter parenthood) and the intensive margin (the decision to have an additional child). In addition, the census provides detailed individual characteristics, including age, education, hukou status, fertility history, and social security participation, allowing us to examine heterogeneous fertility responses across demographic and socioeconomic groups.

Following the timing of childbirth, we define monthly births based on the reported month of childbirth and aggregate individual-level births to the province-month level. We apply the sampling weights provided by the NBS to ensure representativeness. Our main empirical dataset is a province-by-month panel covering all 31 provinces from November 2014 to October 2015. For each province-month observation, we construct the number of first-child births, second-child births, and total births, together with corresponding subgroup-specific fertility outcomes.

Although fertility rate adjusts for the age composition of potential mothers, our research design focuses on short-run within-province changes following the reform. Birth counts therefore provide a direct measure of realized fertility responses during the post-policy period. This approach is particularly suitable for our setting because the empirical design exploits within-province variation over a relatively short-time horizon, during which changes in the underlying age structure of women are unlikely to be the primary driver of the estimated effects. As a

robustness checks, we additionally construct annual birth-rate measures using population data.

The census data provide a unique opportunity to study the consequences of relaxing institutional fertility constraints. Unlike aggregate fertility measures, which may mask offsetting responses across birth orders, our birth-order-specific outcomes allows us to identify whether policy relaxation generates asymmetric responses between the extensive margin of entering parenthood and the intensive margin of having additional children.

4.2 Relaxation of birth quotas

The key policy shock in our analysis is the gradual relaxation of China's birth quotas through the partial two-child policy. Before the reform, China's One-Child Policy (OCP) imposed substantial institutional constraints on fertility choices by limiting the number of children that eligible couples could have. Although enforcement rules varied across regions and demographic groups, the policy effectively constrained many households that would otherwise have chosen to have additional children.

Beginning in November 2013, China introduced the partial two-child policy, which relaxed birth quotas for eligible couples in which at least one spouse was an only child. Unlike the universal two-child policy introduced in late 2015, the partial two-child reform initially affected only a subset of households and was implemented gradually across provinces throughout 2014. This institutional feature generated substantial variation in the timing of policy exposure across provinces.

Figure 6a illustrates the timing of policy implementation across provinces. The staggered adoption of the reform provides variation in the timing of exposure to the relaxation of birth quotas. Provinces that implemented the reform earlier experienced the relaxation of birth quotas sooner, whereas provinces that implemented the reform later remained subject to the previous fertility restrictions for a longer period.

This variation in particularly valuable for identification because the reform was implemented as part of a nationwide demographic policy transition rather than being targeted at provinces experiencing short-run fertility changes. By comparing outcomes before and after policy implementation within provinces, while controlling for common time trends and province-specific characteristics, we can identify changes associated with the relaxation of institutional fertility constraints.

We define the initial treatment timing based on the month in which each province implemented the partial two-child policy. Specifically, for province i in month t, the policy adoption indicator equals one if the province had implemented the reform by that month and zero otherwise. Provinces remain exposed after adoption because the relaxation of birth quotas permanently ex-

panded fertility eligibility for affected households. This policy adoption indicator captures the timing of institutional relaxation rather than the timing of realized outcome responses.

Because the policy affects different outcomes through different adjustment processes, we distinguish between policy implementation timing and effective treatment timing. In particular, realized births may respond only after households adjust fertility plans, complete birth-related administrative procedures, conceive, and complete pregnancy. Therefore, as discussed in Section 5, we allow the effective treatment timing to vary across outcomes according to the time required for the policy to affect different outcomes.

A potential concern is that households may have anticipated the reform before its official implementation. We address this issue by examining pre-treatment trends and allowing for flexible dynamics around the implementation date.

Overall, the partial two-child reform provides an institutional setting in which previously constrained households gained greater freedom in fertility choices at different points in time across provinces. This setting allows us to examine not only whether relaxing birth quotas affected aggregate fertility, but also how institutional changes reshaped fertility decisions along the extensive and intensive margins.

4.3 Child-rearing costs

A central mechanism examined in this paper is whether relaxing birth quotas affects the economic environment in which households make fertility decisions. While the removal of institutional fertility constraints may directly encourage additional births among previously constrained households, it may also increase demand for child-related resources and thereby affect child-rearing costs. Because child-rearing costs encompass multiple dimensions, including expenditures on goods, housing, childcare services, and time inputs, we construct several complementary measures to capture changes in the child-rearing cost environment following the policy reform.

First, we examine changes in consumer prices using monthly province-level consumer price indices (CPI) from the National Bureau of Statistics. The CPI captures broad changes in the cost of goods and services faced by households. Changes in consumer prices therefore provide a measure of the broader consumption environment faced by households considering childbearing.

Second, we examine residential housing prices, which represent an important component of family formation costs in China. Housing costs are particularly relevant for fertility decisions because housing is an important component of family formation and larger families often require greater living space. We construct province-level monthly housing price indices using data from the National Bureau of Statistics. These measures allow us to examine whether relaxing birth

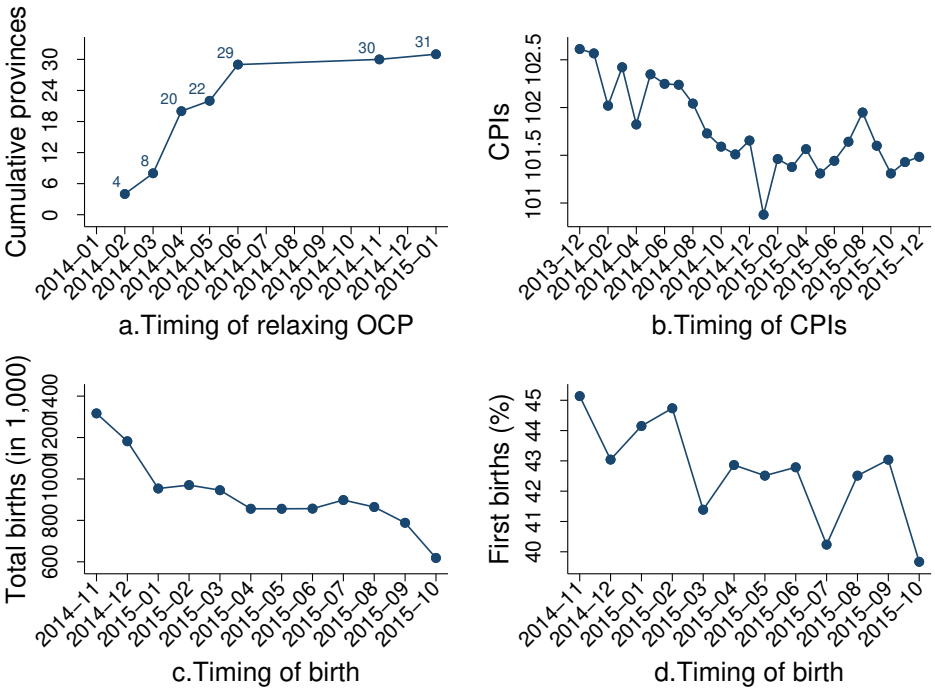


Figure 6: Descriptive summary for the relaxation of birth quotas, CPIs and births

Notes: This graph shows (a) the cumulative number of provinces that relaxed birth quotas over time, (b) the trends in consumer price indices over time, (c) the total number of births (in 1,000 people) over time, and (d) the percentage of first-child births based on total births over time. All variables are at the monthly level. Data on the timing of relaxing birth quotas are collected from official documents. Data on CPIs come from China's National Bureau of Statistics. Data on total births and percentage of first-births from November 2014 to October 2015 are constructed using the 2015 census.

quotas was associated with changes in housing market conditions that may affect households' willingness and ability to have children.

Third, we examine childcare-related labor costs using individual-level information on nanny wages from the China Migrants Dynamic Survey (CMDS). Unlike CPI and housing prices, which capture broader economic conditions, nanny wages provides a more direct measure of the market price of childcare services. Compare with aggregate price indices, nanny wages more directly capture the labor-market costs of purchased childcare. The CMDS contains information on employment characteristics and monthly earnings of migrant workers, allowing us to construct measures of average hourly wages among workers employed in childcare-related occupations.

These three measures capture complementary dimensions of child-rearing costs. CPI reflects general consumption costs, housing prices capture long-term family formation expenses, and nanny wages capture the cost of market-provided childcare services. Together, they allow us to

examine whether the relaxation of birth quotas altered the resource environment surrounding fertility decisions.

For consistency with the fertility outcomes, we construct monthly province-level measures whenever possible and apply the same difference-in-differences framework used in the main analysis. Because child-rearing costs may respond with different timing from fertility decisions, we examine their dynamic responses following policy implementation. Evidence that cost increases precede or coincide with fertility responses would be consistent with the proposed child-rearing cost channel.

Overall, these measures do not aim to capture the complete cost of raising children, which includes both monetary and non-monetary components. Instead, they provide complementary evidence on whether relaxing institutional fertility constraints was accompanied by changes in the economic conditions relevant for household fertility choices.

Table 1: Summary statistics for births

Variables	N	Mean	Median	Std. Dev.	Min	Max
Monthly second-child births	372.00	17,062.00	13,656.85	13,731.51	0.00	75,567.24
Monthly second-child births of women from rural	372.00	10,317.26	7433.91	9905.70	0.00	63,096.05
Monthly second-child births of women from urban	372.00	6744.74	5242.15	5572.66	0.00	32,850.81
Monthly second-child births of women with college degree	372.00	2424.38	1687.54	2587.30	0.00	14,227.99
Monthly second-child births of women without college degree	372.00	14,637.62	11,503.95	12,645.93	0.00	72,435.29
Monthly second-child births of old women	372.00	10,929.86	8135.93	9523.78	0.00	59,647.27
Monthly second-child births of young women	372.00	6132.14	4490.26	5325.93	0.00	26,971.66
Monthly second-child births of women with pension	372.00	12,368.95	9794.39	11,064.35	0.00	65,058.43
Monthly second-child births of women without pension	372.00	4693.05	3287.09	4597.44	0.00	25,136.28
Monthly second-child births of Han women	372.00	15,021.90	9920.99	13,639.28	0.00	75,567.24
Monthly second-child births of minorities women	372.00	2040.10	501.49	3748.31	0.00	23,855.63
Monthly second-child births of native women	372.00	15,962.11	12,735.53	13,259.95	0.00	74,028.93
Monthly second-child births of migrant women	372.00	1099.89	0.00	1924.65	0.00	11,396.63
Monthly first-child births	372.00	12,799.36	10,386.18	10,001.90	0.00	48,436.91
Monthly first-child births of women with college degree	372.00	3733.19	2732.64	3771.27	0.00	23,112.90
Monthly first-child births of women without college degree	372.00	9066.17	7097.01	7709.10	0.00	41,661.64
Monthly first-child births of old women	372.00	4364.47	3297.94	3899.93	0.00	20,908.30
Monthly first-child births of young women	372.00	8434.88	6439.68	7168.76	0.00	36,508.29

Notes: We calculate monthly first-child and second-child births using China's Population Census in 2015. We define women from rural (urban) areas as those women who were (not) granted land-contracting rights. We define old (young) women as those women above (below) the median value of the childbearing age distribution. We define native women as those women registered in the same province, and define migrant women as those women registered in other provinces.

Tables 1 and 2 report summary statistics for the main fertility outcomes and child-rearing cost measures. **Figure 6b** presents the evolution of average CPIs over time, showing a gradual decline from December 2013 to January 2015. **Figures 6c and 6d** present monthly total births and the share of first-child births among total births, respectively. **Figure 7** further presents the evolution of second-child births in treated and control provinces in an event-study framework, providing descriptive evidence on the timing of fertility responses around policy implementation. Together,

Table 2: Summary statistics for child-bearing costs

Variables	N	Mean	Median	Std. Dev.	Min	Max
Overall consumer price indices	372.00	101.52	101.50	0.64	99.90	103.60
Consumer price indices of food	372.00	102.22	102.30	1.33	97.60	105.60
Consumer price indices of tobacco and cigarette	372.00	100.38	99.90	1.98	95.70	106.00
Consumer price indices of clothing	372.00	102.93	102.65	2.07	97.20	115.20
Consumer price indices of household facility, article and maintenance service	372.00	101.07	101.00	0.86	99.20	104.00
Consumer price indices of healthcare	372.00	101.59	101.50	0.94	98.30	105.00
Consumer price indices of transportation and communications	372.00	98.94	99.00	1.27	94.90	103.20
Consumer price indices of education, culture and entertainment	372.00	101.68	101.50	1.60	96.60	108.90
Consumer price indices of housing	372.00	101.12	101.10	1.24	96.90	105.10
Sale price indices of residential buildings	840.00	95.83	95.40	3.10	89.20	123.60
Sale price indices of new constructed residential buildings	840.00	95.63	95.20	3.18	88.80	124.00
By floor area: a. 90 m2 and below	840.00	95.99	95.60	3.28	87.60	124.10
b. From 90 to 144 m2	840.00	95.68	95.30	3.18	89.10	124.80
c. Above 144 m2	840.00	95.00	94.50	3.47	86.60	123.50
Sale price indices of second-hand residential buildings	840.00	96.44	96.15	3.08	85.80	124.30
By floor area: a. 90 m2 and below	840.00	96.67	96.50	3.25	84.80	125.10
b. From 90 to 144 m2	840.00	96.53	96.20	2.97	88.60	124.20
c. Above 144 m2	840.00	95.67	95.50	3.12	86.90	121.80

Notes: We collect data on monthly consumer price indices from August 2014 to July 2015 in each province, and collect data on sale price indices of residential buildings from August 2014 to July 2015 in 70 large and medium-sized cities across all provinces except for Tibet. Both datasets are provided by National Bureau of Statistics of China.

these descriptive statistics and figures summarize the key variables used in our empirical analysis and motivate our examination of fertility responses along the extensive and intensive margins and the role of the child-rearing cost channel.

5 Model Specification and Identification Strategy

To estimate the effects of relaxing birth quotas on fertility outcomes, we exploit the staggered implementation of the partial two-child policy across provinces and employ a different-in-differences framework. The reform provides variation in the timing of exposure to the relaxation of fertility constraints across provinces. Since the reform was implemented as part of a nationwide demographic policy transition rather than in response to province-specific short-run fertility shocks, this variation provides a plausibly exogenous source of identification. Our empirical strategy follows recent advances in staggered difference-in-differences designs and accounts for potential biases arising from heterogeneous treatment effects across groups and over time (de Chaisemartin and D'Haultfoeuille, 2020). For each outcome, we implement the following dynamic difference-in-differences specification:

$$Y_{s,t} = \boldsymbol{\alpha}^{'} I_s + \boldsymbol{\beta}^{'} I_t + \sum_r \gamma_r I_{s,t}^r + \varepsilon_{s,t}, \tag{14}$$

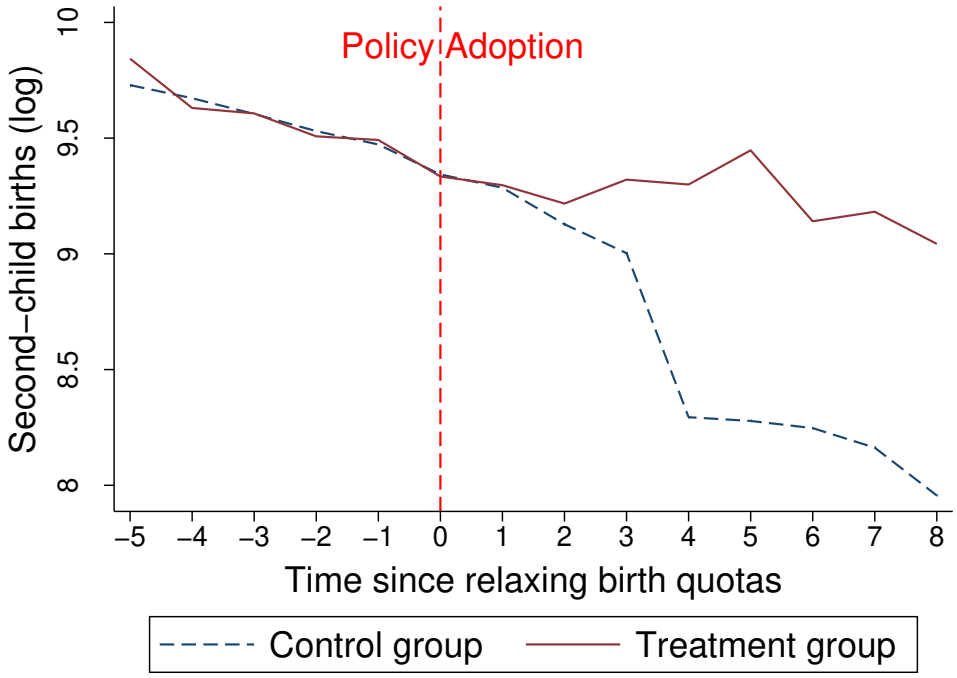


Figure 7: Number of second-child births (in log) over time

Notes: The treatment group consists of provinces that relaxed birth quotas between February 2014 and April 2014. The control group consists of provinces that relaxed the OCP after May 2014. We use the monthly second-child births (in log) between November 2014 and October 2015 as the outcome variable. We define "time since relaxing birth quotas" variable as the time lag between the date when second births occurred and the date 12 months after the province relaxed birth quotas.

where $Y_{s,t}$ denotes the logarithm of monthly first-child births, second-child births, or total births in province s and month t. For child-rearing cost outcomes, $Y_{s,t}$ represents the corresponding price measures described in Section 4. I_s and I_t represent province and month fixed effects, respectively. The indicator variable $I^r_{s,t}$ equals one if province s is in the r-th month after the effective relaxation of birth quotas and zero otherwise. We define the initial treatment timing based on the month when each province implemented the partial two-child policy and adjust the effective treatment date according to the outcome-specific response lag discussed below.

The effective treatment timing differs across outcomes because the relaxation of birth quotas affects fertility decisions and economic conditions through different adjustment processes. For second-child births, we define the effective treatment date as 12 months after provincial policy implementation. This timing reflects both institutional and biological delays in translating fertility policy changes into realized births. Following the relaxation of birth quotas, eligible couples still needed to complete birth-related administrative procedures, adjust their fertility plans, con-

ceive, and complete pregnancy. Therefore, births observed shortly after policy implementation may still reflect fertility decisions made before the reform.

For first-child births, we use an 11-month lag. Unlike second-child births, first births were not directly restricted by birth quotas and did not require households to obtain additional eligibility to have a first child. Therefore, the relevant adjustment period mainly involves fertility planning, conception, and pregnancy. For total births, which combine first-child and higher-order births, we follow the timing used for second-child births because higher-order births represent the most direct response to the removal of birth quotas.

For child-rearing cost outcomes, including consumer prices and housing prices, we adopt the same effective timing as second-child births. This choice reflects the idea that these measures capture the broader resource adjustments associated with the increase in eligible births following the relaxation of birth quotas.

The corresponding dynamic effects are estimated using the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020), which is designed for staggered adoption settings with heterogeneous treatment effects. The coefficient γ_r captures the dynamic effect of relaxing birth quotas r months after the effective treatment date. Standard errors are clustered at the provincial level. To address concerns regarding the relatively small number of clusters, we additionally conduct inference using the wild bootstrap procedure proposed by Cameron *et al.* (2008) and implemented through the Roodman *et al.* (2019) boottest approach. [9]

The identification of the treatment effects relies on the assumption that, absent the policy reform, provinces adopting the partial two-child policy at different points in time would have experienced similar trends in the relevant outcomes. Although provinces differed in the timing of implementation, the reform was part of a nationwide demographic policy transition rather than a response to province-specific short-run fertility shocks. This institutional feature supports the plausibility of the common-trends assumption.

Recent studies show that conventional two-way fixed effects estimators may be biased in staggered adoption settings when treatment effects vary across groups and over time, because they may combine heterogeneous treatment effects with potentially negative weights (Goodman-Bacon, 2018; de Chaisemartin and D'Haultfoeuille, 2020; Sun and Abraham, 2020). To address this concern, our baseline estimates rely on the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020), which is robust to heterogeneous treatment effects under staggered policy adoption.

[9] A potential concern is that birth counts may reflect changes in the population composition of women of childbearing age. Province fixed effects absorb time-invariant differences across provinces. In addition, when using fertility rates constructed from the 2015 Population Census as an alternative outcome measure, our results remain robust (**Figure B.12**). We also conduct micro-level analyses using individual census data and obtain similar results.

We conduct additional analyses to evaluate the validity of the identification strategy. First, we examine whether the relaxation of birth quotas affects fertility outcomes among groups that should be less directly exposed to the reform. Specifically, we focus on inter-provincial migrant women. Under China's hukou system, fertility eligibility was closely linked to registration status, implying that migrants from other provinces should not respond to policy changes in the province where they currently reside. The absence of similar effects among migrant women provides evidence against the possibility that our estimates capture province-specific shocks unrelated to the relaxation of birth quotas.

Second, we conduct placebo tests using earlier Population Census waves from 2005 and 2010. These data allow us to construct comparable province-month fertility outcomes during periods before the partial two-child reform. If our baseline estimates were driven by seasonal fertility patterns or unobserved province-specific trends rather than the policy reform itself, similar effects should appear in these pre-reform placebo periods. The absence of comparable placebo effects supports the interpretation that the estimated fertility responses are associated with the relaxation of birth quotas.

6 Empirical Results

In this section, we examine the effects of relaxing birth quotas on fertility outcomes and the economic environment surrounding child-rearing. We first analyze the impact on second-child births and examine heterogeneity across households with different demographic and socioeconomic characteristics. We than investigate whether relaxing birth quotas affected first-child births, followed by an analysis of changes in child-rearing-related costs, including consumer prices, housing prices, and childcare service costs. Finally, we examine the aggregate effect on total births.

6.1 Pretreatment trends

A key assumption underlying the difference-in-differences framework is the existence of parallel trends before the relaxation of birth quotas. **Figure** 7 present the raw trends in fertility outcomes across provinces with different policy adoption timing, providing suggestive evidence of similar pre-reform patterns.

To formally examine the validity of this assumption, we employ the dynamic difference-in-differences estimator proposed by de Chaisemartin and D'Haultfoeuille (2020). This estimator allows treatment effects to vary across groups and over time and avoids the potential negative

weighting problem associated with conventional two-way fixed effects models under staggered adoption.[10]

If the parallel trends assumption holds, placebo estimates in the pretreatment periods should be close to zero. Figure 8 presents the estimated treatment effects and placebo effects for fertility outcomes. The horizontal axis represents event time relative to the effective relaxation of birth quotas, with negative values indicating pretreatment periods. The vertical axis represents changes in the logarithm of the corresponding outcome variables. The estimated placebo effects are close to zero and statistically insignificant before the reform, providing supportive evidence for the validity of the identification strategy.[11]

6.2 Anticipation effects

A potential concern is that households may have anticipated the relaxation of birth quotas and adjusted their fertility decisions before the official policy implementation. Such anticipation could bias our estimates if couples advanced second-child births or postponed first-child births before the reform.

Several factors suggest that anticipation effects are unlikely to be substantial in our setting. First, before provincial implementation, having an unauthorized second child remained subject to penalties under the previous fertility regime. Therefore, couples were unlikely to substantially change fertility behavior based only on expectations of future policy changes while the existing restrictions remained legally binding.

Second, the timing of provincial implementation was uncertain. Following the announcement of the partial two-child policy in November 2013, local governments determined their own implementation schedules. Although national authorities indicated that implementation would occur gradually, adoption dates varied substantially across provinces, ranging from February 2014 to January 2015. This uncertainty reduced households' ability to precisely predict the timing of policy changes.

Finally, the empirical evidence provides limited support for anticipation effects. We find no significant pretreatment effects in the dynamic estimates. In addition, our falsification analysis using inter-provincial migrants does not reveal comparable fertility responses among groups less directly exposed to provincial policy changes. Therefore, anticipation effects are unlikely to be a major driver of our findings. If some households nevertheless adjusted fertility timing before

[10] For each treatment period, the estimator compares changes in outcomes between newly treated groups and groups that remain untreated. For pretreatment periods, the corresponding placebo estimates compare changes in outcomes before treatment among groups that adopt the reform and groups that have not yet adopted it. See de Chaisemartin and D'Haultfoeuille (2020) for details.

[11] We use the STATA command *did_multiplegt* to estimate and plot the dynamic effects.

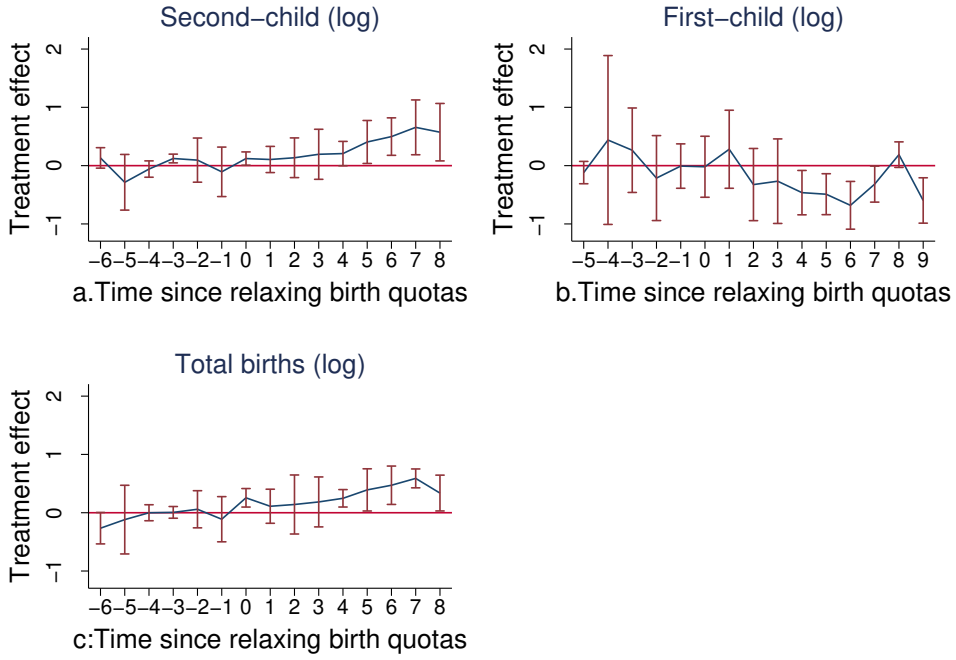


Figure 8: Effects of relaxing birth quotas on second-child, first-child and total births.

Notes: This graph shows the impact of relaxing birth quotas on (a) second-child births, (b) first-child births, and (c) total births. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Both first-child,second-child and total birth variables are in the logarithm form. The panel data on first-child, second-child and total births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

the reform, such behavior would likely attenuate the estimated effects on both second-child and first-child births, making our estimates conservative measures of the policy impact (Malani and Reif, 2015).

6.3 Impact of relaxing birth quotas on second-child births

We first examine how the relaxation of birth quotas affected second-child births, which captures the intensive margin of fertility choice. We estimate equation (14) using monthly province-level second-child births as the outcome variable. The results are reported in **Table B.1** and illustrated in **Figure 8a**.

Because treatment adoption is staggered across provinces, conventional two-way fixed effects estimates may be affected by negative weighting problems when treatment effects vary across groups and over time. Following de Chaisemartin and D'Haultfoeuille (2020), we therefore rely on a dynamic difference-in-differences estimator designed for staggered adoption settings. The estimated dynamic effects are presented in **Figure 8a**.

The results show a substantial increase in second-child births following the relaxation of birth quotas. First, the estimated effects are close to zero and statistically insignificant in the pre-treatment periods, providing support for the parallel-trends assumption. Following the effective relaxation of birth quotas, the estimated effects become increasingly positive over time, indicating that households gradually adjusted their fertility decisions after the removal of institutional constraints.

The response continues to build gradually after the effective relaxation of birth quotas. The estimated effects are relatively modest in the first several months after the effective relaxation of birth quotas, but increase substantially thereafter. The effect reaches approximately 50 percent by the fifth month and around 65 percent by the sixth month, before peaking at approximately 92.7 percent in the seventh month. Although the magnitude declines slightly afterward, the estimated effects remain economically large. The stability of the peak response across alternative bootstrap specifications further suggests that the timing and magnitude of the dynamic response are not driven by a particular inference procedure (**Figure E.1**).

This gradual response pattern is consistent with the institutional, behavioral, and biological processes through which fertility policies affect realized births. Even after birth quotas are relaxed, eligible couples need time to adjust fertility planes, complete relevant procedures, conceive, and complete pregnancy. Therefore, the delayed response suggests that the policy primarily released previously constrained fertility demand rather than generating an immediate mechanical increase in births.

The average treatment effect is also economically meaningful. The estimated coefficient implies an average increase of approximately 37.9 percent in monthly second-child births following the relaxation of birth quotas. Although the statistical significance of this average effect varies across alternative bootstrap procedures, the estimated magnitude remains stable across specifications (Table E.1). This pattern suggests that uncertainty mainly concerns statistical precision rather than the direction or economic magnitude of the estimated effect.

The increase in second-child births is consistent with the existence of substantial pent-up fertility demand among households previously constrained by birth quotas. Supporting this interpretation, aggregate statistics from the National Health Commission show that second-child births increased from 5.11 million in 2013 to 6.52 million in 2015, representing an increase of

approximately 27.6 percent.

To further examine whether the estimated effects reflect the relaxation of local fertility constraints rather than province-specific shocks, we conduct a falsification test using inter-provincial migrant women. Under China's hukou system, fertility eligibility was closely linked to registration status, implying that migrants from other provinces should be less directly affected the relaxation of birth quotas in their current locations. Consistent with this interpretation, we find no comparative increase in second-child births among inter-provincial migrants, while significant effects are observed among local residents (**Figures B.1a and B.1b**) .

Finally, we conduct placebo analysis using counterfactual monthly second-child births constructed from the 2005 and 2010 Population Censuses. As shown in **Figures B.2a and B.2c**, we find no similar effects during these pre-reform periods. These placebo results further reduce concerns that the estimated effects are driven by seasonal fertility patterns or unobserved province-specific trends.

Overall, the evidence indicates that relaxing birth quotas substantially increased second-child births by releasing previously constrained fertility demand. The dynamic response, falsification tests, and placebo analyses consistently support the interpretation that the policy reform generated a meaningful intensive-margin fertility response. This pattern is consistent with the limits-raising channel emphasized in our conceptual framework, whereby relaxing institutional constraints allows previously constrained households to realize their desired fertility.

6.4 Which groups are more responsive to the relaxation of birth quotas?

We next examine whether the fertility response to the relaxation of birth quotas varies across households with different demographic and socioeconomic characteristics. These heterogeneity analyses provide further evidence on the mechanisms underlying the intensive-margin fertility response. Specifically, if the policy primarily released previously constrained fertility demand, we would expect stronger responses among households with stronger incentives to have an additional child or those more likely to have been constrained by birth quotas. The results are reported in Figures B.3 to B.7.

We first examine heterogeneity related to demographic characteristics that may reflect differences in fertility preferences. As shown in **Figures B.3a and B.3b**, the estimated responses is larger among relatively younger women than among relatively older women, where age groups are defined relative to the median of the childbearing age distribution. Younger women are more likely to have greater flexibility in adjusting the timing of additional births, suggesting that the relaxation of birth quotas generated stronger responses among households with greater capacity to respond to the policy change.

We further examine heterogeneity related to the gender composition of existing children. In China, child gender composition has historically influenced fertility decisions, particularly among families seeking an additional child. Consistent with the possibility that gender composition affects demand for an additional child, **Figures B.6a to B.6d** show that the fertility response is larger among couples without a male child or among couples whose existing child is female at the time of the survey. These patterns suggest that relaxing birth quotas allowed households with stronger incentives for an additional child to realize previously constrained fertility preferences.

We next examine heterogeneity related to economic conditions. **Figures B.4a and B.4b** show that the response is larger among women without a college degree than among those with a college degree or above. Similarly, **Figures B.5a and B.5b** indicate that the response is stronger among women who do not own a house than among homeowners. These differences suggest that fertility responses vary across households facing different child-rearing constraints. However, these patterns should be interpreted cautiously, as they may reflect differences in both fertility preferences and economic constraints.

Finally, we investigate whether the response varies with households' reliance on children for old-age support. Access to pensions may reduce fertility incentives if public social security substitutes for the traditional role of children in providing old-age support. Alternatively, if parents derive utility from children beyond old-age security considerations, fertility responses may remain strong even among households covered by pension programs. As shown in **Figures B.7a and B.7b**, second-child fertility responses are larger among couples covered by pension programs than among those without pension access. This pattern provides suggestive evidence that fertility responses are not driven solely by old-age security considerations and is more consistent with the intergeneratonal altruism motive rather than a pure old-age security motive.

Overall, the heterogeneity patterns suggest that the intensive-margin fertility response to relaxing birth quotas was concentrated among households with stronger incentives to have an additional child. Larger responses are observed among younger women, families without a male child, and pension-covered households. These findings are broadly consistent with the limits-raising channel emphasized in our conceptual framework, whereby relaxing institutional constraints enables previously constrained households to realize their desired fertility rather than inducing fertility uniformly across all households. At the same time, differences across socioeconomic groups highlight the importance of considering household characteristics in understanding fertility responses to institutional reforms.

6.5 Does relaxing birth quotas affect first-child births?

As discussed in Section 3, relaxing birth quotas directly affects households' ability to have additional children but does not impose a direct constraint on entry into parenthood. However, the reform may indirectly affect first-child fertility through changes in household fertility timing and the child-rearing cost environment. To examine this possibility, we assume that it takes at least 11 months for the reform to affect realized first-child births ($r = 11$), reflecting the time required for fertility planning, conception, and pregnancy.

Using the same empirical approach as in the analysis of second-child births, we estimate the dynamic difference-in-differences model for first-child births. The results are presented in **Figure 8b**. Because treatment effects may vary across provinces and over time under staggered adoption, we rely on the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) rather than conventional two-way fixed effects estimates.

The results reveal a decline in first-child births following the relaxation of birth quotas. The estimated average treatment effect implies a reduction of approximately 23.5% in monthly first-child births after the effective relaxation of birth quotas. Although the statistical significance of the average effect is sensitive to inference procedures, the estimated magnitude remains stable across alternative bootstrap specifications (**Figure E.2**). Combined with the dynamic estimates showing a persistent decline in first-child births following the effective relaxation of birth quotas, these results provide suggestive evidence that the reform was associated with a short-run decline or postponement in entry into parenthood.

The dynamic estimates further illustrate the timing of this response. We find no immediate decline in first-child births following the effective relaxation of birth quotas. Instead, the negative effects emerge gradually and become more pronounced several months after the reform. The largest decline occurs around the sixth month after the effective relaxation, corresponding to a reduction of approximately 49.3% relative to the pre-reform baseline. The estimated effects remain statistically significant under conservative bootstrap procedures (**Table E.2**). This delayed response pattern is consistent with the interpretation that some households adjusted the timing of first-child fertility decisions following the policy reform.

External demographic evidence is consistent with this pattern. According to data from the National Health Commission of China, the number of first-child births declined from 10.56 million in 2013 to 8.86 million in 2015, representing a decrease of approximately 16.1%. Although multiple factors may contribute to this aggregate decline, the timing is consistent with the short-run reduction in first-child births identified in our empirical analysis.

We further conduct falsification and placebo analyses. Using monthly births among inter-provincial migrants, we find no comparable decline in first-child births following the relaxation

of birth quotas. In contrast, the decline is more pronounced among local residents who were directly affected by the provincial implementation of the reform (**Figures B.1c and B.1d**). In addition, we do not find similar effects when using counterfactual first-child births constructed from the 2005 and 2010 Population Censuses (**Figures B.2b and B.2d**). These placebo results reduce concerns that the estimated decline reflects seasonal fertility patterns or other unobserved province-specific trends.

We next examine whether the decline in first-child births varies across households facing different constraints in entering parenthood. The estimated reduction is larger among younger women, less-educated women, and women without home ownership. In contrast, we find weaker evidence of declines among older women, more educated women, and homeowners. These patterns suggest that first-child fertility decisions are particularly sensitive to economic conditions associated with entering parenthood, including financial constraints and the economic costs associated with entering parenthood. Unlike second-child fertility responses, which are more closely related to releasing previously constrained fertility demand, first-child fertility responses appear to reflect the adjustment of households facing greater costs of starting a family. Details are reported in Figures B.3 to B.6.

Overall, the evidence suggests that relaxing birth quotas generated asymmetric fertility responses across birth orders. While the reform increased second-child births by allowing previously constrained households to realize additional fertility, it simultaneously delayed or discouraged some households from entering parenthood rather than generating a uniform increase in fertility. This pattern highlights the importance of considering both intensive and extensive margins when evaluating the consequences of fertility policy reforms.

6.6 Does relaxing birth quotas affect child-rearing costs?

The previous sections document an asymmetric fertility response following the relaxation of birth quotas: while second-child births increased substantially after the removal of institutional constraints, first-child births declined or were postponed in the short run. A key implication of our conceptual framework is that these two responses may arise simultaneously because fertility policy reforms can affect both households' ability to have additional children and the economic environment in which fertility decisions are made. In particular, relaxing birth quotas may increase demand for child-related sources and thereby alter the economic environment surrounding child-rearing.

To examine this potential mechanism, we investigate whether the relaxation of birth quotas changed the economic environment surrounding child-rearing. We consider several complementary measures capturing different dimensions of this environment, including consumer prices,

residential housing prices, and childcare service costs. Although these measures do not capture the full monetary and non-monetary costs of raising children, they provide complementary evidence on changes in the economic environment surrounding fertility decisions.

6.6.1 Impact of relaxing birth quotas on consumer price indices

We first examine whether relaxing birth quotas affected local consumer prices. A possible mechanism is that the removal of fertility constraints increased demand for child-related goods and services, potentially generating upward pressure on local prices. Therefore, changes in consumer prices provide evidence on broader economic adjustments following the relaxation of fertility constraints.

To estimate the impact of relaxing birth quotas on local CPIs, we estimate the following dynamic difference-in-differences specification:

$$CPI_{s,t} = \boldsymbol{\alpha}^{'} I_s + \boldsymbol{\beta}^{'} I_t + \sum_r \gamma_r I^r_{s,t} + \varepsilon_{s,t}, \tag{15}$$

where $CPI_{s,t}$ denotes the consumer price index in province s and month t, I_s and I_t represent province and month fixed effects, respectively. The indicator variable $I^r_{s,t}$ equals one if province s is observed in the r-th month after the effective relaxation of birth quotas and zero otherwise. We define the effective treatment timing as 12 months after provincial policy implementation, consistent with the timing used in the analysis of second-child births. This timing reflects the adjustment process through which fertility policy changes may translate into increased demand for child-related goods and services and subsequent changes in local prices.

We estimate the dynamic effects using the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020). The results are reported in **Figure 9a**. The estimated effects are close to zero during the pretreatment periods, providing support for the validity of the identification strategy. Following the effective relaxation of birth quotas, consumer prices gradually increase.

The estimated average treatment effect suggests that relaxing birth quotas increases local CPIs by approximately 0.48 units, corresponding to about 32% of the median year-on-year CPI growth rate observed in the sample. The magnitude of the estimated effect remains stable across alternative bootstrap specifications (**Figure E.3**), suggesting that the result is not driven by a particular inference procedure. The dynamic pattern provides additional insight into the potential mechanism. The increase in consumer prices does not occur immediately after the effective relaxation of birth quotas but emerges gradually over subsequent months. This timing is consistent with the interpretation that households gradually adjust fertility plans and increase demand for child-related consumption goods and services. Importantly, these price responses emerge before

the strongest first-child fertility responses documented in the previous sections, suggesting that changes in the economic environment may be an important factor underlying subsequent fertility adjustments.

To further investigate whether the price responses are concentrated in consumption categories more closely related to household resource allocation, we examine CPI subcategories. We find that food prices increase more substantially than categories such as tobacco, alcohol, and clothing. These patterns suggest that the reform affected components of household consumption that are closely related to family formation and child-rearing decisions. The results are reported in **Figures B.8 to B.9**.

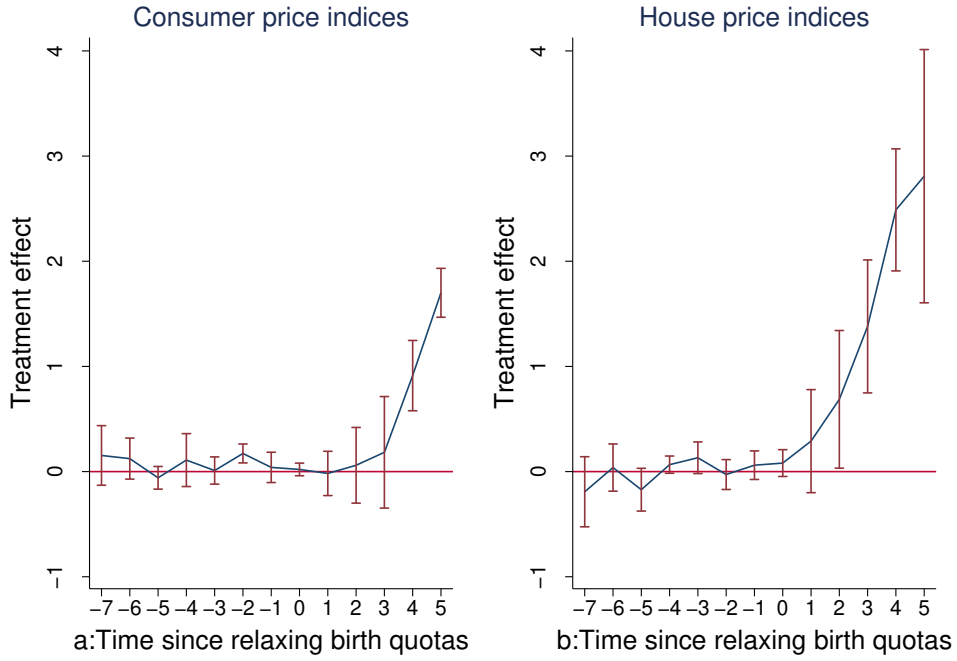


Figure 9: Effects of relaxing birth quotas on consumer price indices and residential real estate sale price indices.

Notes: This graph shows the impact of relaxing birth quotas on (a) consumer price indices and (b) residential real estate sale price indices. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. The panel data on consumer price indices and residential real estate sale price indices from August 2014 to July 2015 come from China's National Bureau of Statistics. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

6.6.2 Impact of relaxing birth quotas on residential real estate sale price indices

Housing represents an important component of the economic costs associated with family formation in China. Decisions regarding marriage, household formation, and childbearing are closely linked to housing conditions, and larger families may require additional living space. Therefore, if relaxing birth quotas increased demand for family-related resources, housing markets may adjust to the resulting changes in household demand. We next examine whether the relaxation of birth quotas affected residential housing prices.

We estimate the following dynamic difference-in-differences specification:

$$HP_{c,t} = \boldsymbol{\alpha}' I_c + \boldsymbol{\beta}' I_t + \sum_r \gamma_r I^r_{c,t} + \varepsilon_{c,t}, \tag{16}$$

where $HP_{c,t}$ denotes the residential real estate sale price index in city c and month t, I_c and I_t represent city and month fixed effects, respectively. The indicator variable $I^r_{c,t}$ equals one if city c is observed in the r-th month after the effective relaxation of birth quotas and zero otherwise. Consistent with the effective treatment timing used in the analysis of second-child births, we define the effective treatment timing as 12 months after provincial policy implementation. This timing captures the period required for households and local housing markets to adjust to changes in family formation demand following the fertility policy reform.

We estimate the dynamic effects using the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020). The results are presented in **Figure 9b**. The estimated effects are close to zero in the pre-treatment periods, providing support for the validity of the identification strategy. Following the effective relaxation of birth quotas, residential housing prices increase gradually.

The estimated average treatment effect implies that relaxing birth quotas increases residential real estate price indices by approximately 1.29 units, corresponding to about 28% of the median year-on-year growth rate observed in the sample. The estimated magnitude remains stable across alternative bootstrap specifications (**Figure E.4**), suggesting that the result is robust to alternative inference procedures. The dynamic pattern provides further evidence on the potential mechanism. Housing prices begin to increase after the effective relaxation of birth quotas and continue rising over subsequent months. Importantly, these housing price responses emerge before the strongest decline in first-child births documented in the previous sections. This timing is consistent with the possibility that changes in housing-related costs contributed to the postponement of fertility decisions among households considering entry into parenthood.

To further examine whether housing market responses vary across different types of residential units, we investigate heterogeneity by housing size. The estimated effects decline monotonically with floor area, with stronger responses observed for smaller residential units. This pattern

is present in both newly constructed and second-hand housing markets (**Figures B.10 and B.11**). These findings suggest that housing market responses were stronger among segments of the market more relevant for households with limited housing resources.

The housing market evidence is also consistent with the heterogeneity patterns observed in first-child fertility responses. Specifically, the decline in first-child births following the relaxation of birth quotas is concentrated among non-homeowners, while we find weaker evidence of comparable effects among homeowners. By contrast, housing ownership plays a limited role in differentiating second-child fertility responses. Together, these patterns suggest that housing-related costs are particularly relevant for decisions regarding entry into parenthood, whereas second-child fertility responses are more closely associated with the release of previously constrained fertility demand.

Overall, the evidence indicates that relaxing birth quotas was accompanied by adjustments in local housing markets. While housing prices do not represent the complete cost of raising children, they capture an important component of the economic environment surrounding family formation. The timing and heterogeneity patterns are consistent with the child-rearing cost channel proposed in our conceptual framework.

6.6.3 Impact of relaxing birth quotas on nanny wages

Finally, we examine childcare service costs using information on nanny wages. Unlike consumer prices and housing prices, which capture broader economic conditions, nanny wages provide a more direct measure of the market price of childcare services. If relaxing birth quotas increased demand for child-rearing resources, childcare markets may respond to increased demand for childcare services, particularly because the short-run supply of childcare workers is relatively limited.

To examine whether relaxing birth quotas affected childcare service costs, we use individual-level information from the China Migrants Dynamic Survey (CMDS). Unlike the previous analyses based on province-month outcomes, this analysis examines whether wages among nanny workers changed relative to other migrant workers following the policy reform. Because the CMDS collects wage information in April of each survey year, we focus our main analysis on provinces where the policy was implemented between January and April 2014, allowing the 2014 survey to capture post-reform exposure.

Specifically, we estimate the following specification:

$$\log(wage_{i,s,t}) = \alpha \cdot Caregiver_{i,s,t} + \boldsymbol{\beta}' I_{i,t} + \gamma_{2012}(Caregiver_{i,s,t} \times I_{i,2012}) + \gamma_{2014}(Caregiver_{i,s,t} \times I_{i,2014}) + \boldsymbol{\delta}' I_{i,s} + \boldsymbol{\theta}' K_{i,s,t} \tag{17}$$

where $\log(wage_{i,s,t})$ denotes the logarithm of monthly wages for migrant worker i in province s and survey year t. $Caregiver_{i,s,t}$ is an indicator variable equal to one if the worker's main occupation is nanny. $I_{i,t}$ and $I_{i,s}$ denote year and province fixed effects, respectively. $I_{i,2012}$ and $I_{i,2014}$ represent survey-year indicators for 2012 and 2014, with 2013 as the reference year. $K_{i,s,t}$ includes individual characteristics, including gender, age, age squared, education, marital status, hukou type, migration duration, and migration duration squared.

This specification compares changes in wages among nanny workers relative to other migrant workers before and after the relaxation of birth quotas. The coefficient of interest is γ_{2014}, which captures the relative change in wages among nanny workers after the relaxation of birth quotas. The coefficient γ_{2012} captures whether nanny workers experienced differential wage changes relative to other migrant workers before the policy reform.

The results are reported in **Table** 3. We find that relaxing birth quotas significantly increased nanny wages. The estimated effect implies an increase of approximately 9.3% in monthly wages among nanny workers relative to other migrant workers. This increase is consistent with the interpretation that the reform increased demand for market-provided childcare services.

We further conduct placebo analyses to examine whether the estimated effect reflects the policy reform rather than differential trends. First, we focus on provinces where the policy had not yet been implemented during the survey period and find no comparable increase in nanny wages. Second, using alternative survey years, we do not find similar effects. These placebo exercises reduce concerns that the estimated wage increase reflects unrelated labor market trends.

The increase in nanny wages complements the evidence from consumer prices and housing markets. Together, these findings suggest that relaxing birth quotas altered the economic environment surrounding child-rearing by increasing demand for child-related resources. Although nanny wages capture only one dimension of childcare costs, they provide additional evidence that childcare markets responded to the relaxation of fertility constraints.

Taken together, the evidence from consumer prices, housing prices, and childcare costs provides consistent evidence supporting the child-rearing cost channel proposed in our conceptual framework. The reform not only relaxed institutional constraints on fertility but also changed the economic environment surrounding child-rearing, which may help explain the asymmetric responses across fertility margins: increasing fertility at the intensive margin while delaying some households' entry into parenthood at the extensive margin.

6.7 Does relaxing birth quotas affect total births?

Up to this point, our analysis has documented asymmetric fertility responses following the relaxation of birth quotas: second-child births increased substantially after the removal of insti-

Table 3: Impact of relaxing the OCP on nanny wages

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-treatment trends	−0.0055	0.0213	0.0211	0.0214	0.0587	−0.0384	−0.0897	0.0189
	(0.0304)	(0.0284)	(0.0284)	(0.0284)	(0.0370)	(0.0660)	(0.0774)	(0.0283)
Treatment effect	0.0743***	0.0891***	0.0892***	0.0891***	0.0739**	0.2028***	−0.0523	0.0224
	(0.0282)	(0.0264)	(0.0264)	(0.0264)	(0.0368)	(0.0575)	(0.0613)	(0.0278)
Nanny (o/1)	−0.3180***	−0.1875***	−0.1857***	−0.1860***	−0.1750***	−0.1838***	−0.1350***	−0.1886***
	(0.0206)	(0.0193)	(0.0193)	(0.0193)	(0.0270)	(0.0399)	(0.0444)	(0.0193)
Observations	360664	360664	360284	360284	200106	63784	86901	352527
R-squared	0.0565	0.1753	0.1768	0.1769	0.1842	0.1596	0.1209	0.2061
F-stat	829.9535	2017.1035	1934.1461	1843.0295	1075.4069	310.4696	442.4326	2178.5219
Individual control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the impacts of relaxing the OCP on nanny wages based on equation (17). Wage variables are in the logarithm form. We use samples in provinces that relaxed the OCP from January to April 2014 for the main analysis. Column 1 reports the estimates without controlling for individual characteristics. Column 2 controls for male, age, age squared and marriage status. Column 3 further controls for hukou type. Column 4 further controls for migration duration and migration duration squared. Columns 5 and 6 estimate the impact by places of origin. Specifically, Column 5 uses samples of migrants registered in the same city. Column 6 uses samples of migrants registered in other provinces Column 7 estimates the placebo effect by using samples in provinces that relaxed the OCP after April 2014. Column 8 estimates another placebo effect by using samples in survey years in 2012, 2013 and 2015. Standard errors are in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

tutional constraints, whereas first-child births declined or were postponed in the short run. An important question is whether the increase in second-child births was sufficient to offset the decline in entry into parenthood. We therefore examine the overall impact of relaxing birth quotas on total births.

Using the same empirical approach as in the previous analyses, we estimate the effect of relaxing birth quotas on total births. The estimated effects on total births are generally positive following the effective relaxation of birth quotas. The estimated average treatment effect implies an increase of approximately 35.5% in monthly total births. Although the statistical precision of this average effect varies across alternative inference procedure, the estimated magnitude remains highly stable across bootstrap specifications (Figure E.5).

The dynamic estimates further reveal the timing of the aggregate fertility response. Similar to the pattern observed for second-child births, the effects on total births emerge gradually after the effective relaxation of birth quotas and increase over subsequent months.

The estimated effect reaches its peak around the seventh month after the effective policy change, corresponding to an increase of approximately 80.2% in total births relative to the pre-reform baseline. The estimated effect remains statistically significant under conservative bootstrap procedures (Table E.5). These results indicate that the increase in second-child births more than offsets the decline in first-child births during the post-reform period.

Taken together, the evidence indicates that relaxing birth quotas increased total births in the

short run, although the aggregate effect was smaller than the corresponding increase in second-child births. More importantly, these results highlight that fertility policy reforms can generate heterogeneous responses across birth orders: relaxing institutional constraints increased second-child births among households previously constrained by birth quotas, while some households postponed entry into parenthood in the short run. Therefore, evaluating fertility policy reforms required considering not only aggregate fertility changes but also the underlying reallocation of births across fertility margins. See **Figure 8c** for details.

6.8 Competing hypotheses

6.8.1 Marriage market

We first examine the marriage market as a potential alternative explanation for the asymmetric fertility responses following the relaxation of birth quotas. A decline in first-child births after the reform could potentially arise if the policy affected marriage formation, as entry into parenthood is closely linked to marital decisions in China. Therefore, changes in marriage outcomes provide a useful test of whether the observed reduction in first-child births reflects changes in marriage formation rather than fertility decisions conditional on marriage.

To examine this possibility, we construct monthly province-level data on the number of marriages and estimate the following dynamic difference-in-differences specification:

$$Marriage_{s,t} = \boldsymbol{\alpha}^{'} I_s + \boldsymbol{\beta}^{'} I_t + \sum_r \gamma_r I^r_{s,t} + \varepsilon_{s,t}, \tag{18}$$

where $Marriage_{s,t}$ represents the number of marriages in province s and month t. I_s and I_t denote province and month fixed effects, respectively. The indicator variable $I^r_{s,t}$ equals one if province s is in the r-th month after the relaxation of birth quotas and zero otherwise.

Unlike fertility outcomes, marriage formation does not involve biological delays. Therefore, we define the effective treatment timing based on the month when each province implemented the partial two-child policy and set the effective lag to zero ($r = 0$). This timing assumption reflects the possibility that forward-looking individuals may adjust marriage decisions relatively quickly following changes in fertility regulations.

Because China's Ministry of Civil Affairs provides provincial marriage statistics only at the quarterly level, we construct monthly marriage outcomes using cubic spline interpolation from October 2013 to September 2014.[12]

[12]While interpolation introduces measurement noise, it allows us to match the monthly frequency of our fertility outcomes and policy timing. It does not mechanically generate differential changes around policy implementation.

We estimate the dynamic effects using the same approach as in the main analysis. The results are reported in **Figure 10**. The estimated effects are close to zero in the pretreatment periods, providing support for the validity of the identification strategy. Following the relaxation of birth quotas, the number of marriages increases gradually over subsequent months.

The increase in marriages is consistent with the view that fertility policies may affect household formation decisions. Specifically, we find that the number of marriages increases by approximately 7.4% in the fourth month after the reform and reaches about 35.1% in the ninth month. The average effect across the post-reform periods implies an increase of approximately 13.4% in local marriages.

This evidence suggests that the decline in first-child births following the relaxation of birth quotas is unlikely to be driven by reduced marriage formation. Instead, the increase in marriages indicates that the reform did not discourage entry into marriage. Combined with the evidence on child-rearing costs, these results are more consistent with the interpretation that some households postponed first-child fertility decisions due to changes in the environment surrounding childbearing rather than changes in marriage opportunities.

Overall, the marriage market analysis helps rule out a competing explanation for the decline in first-child births and is consistent with the interpretation that the asymmetric fertility responses reflect adjustments in fertility decisions following the relaxation of institutional constraints.

6.8.2 Alternative explanations for the short-run fertility response

A large literature emphasizes several socioeconomic factors that shape fertility decisions and long-run fertility transitions. These factors include women's labor market opportunities, returns to human capital, changes in longevity, and the economic value of children (Galor and Weil, 1996, 1999, 2000; Galor and Moav, 2002; Cervellati and Sunde, 2005; Hazan and Berdugo, 2002; Tang *et al.*, 2018). For example, rising returns to education and improvements in women's labor market opportunities may increase the opportunity costs of childbearing and contribute to delayed fertility. Similarly, changes in the returns to child quality, life expectancy, and the economic role of children may influence fertility preferences over longer periods.

However, our empirical setting differs from the long-run demographic processes emphasized in this literature. Our identification strategy exploits short-run fertility responses following a nationwide institutional reform using high-frequency monthly birth outcomes. Therefore, for these socioeconomic factors to explain the estimated effects, the relaxation of birth quotas would need to generate immediate and sufficiently large changes in gender wage differentials, returns to education, expectations about longevity, or other structural determinants of fertility preferences.

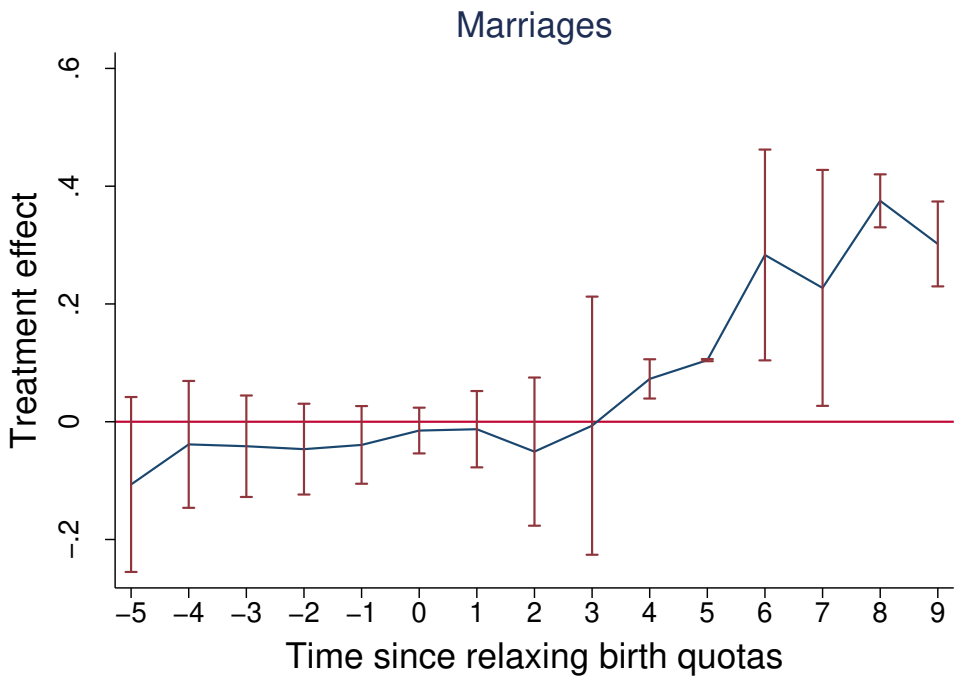


Figure 10: Impact of relaxing birth quotas on local marriages

Notes: This graph shows the impact of relaxing birth quotas on total marriages. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Total marriages variables are in the logarithm form. Since China's Ministry of Civil Affairs only provides the number of marriages at the quarterly level in each province, we resort to the cubic spline interpolation method to calculate the number of marriages at the monthly level from October 2013 to September 2014. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

Such factors are generally slow-moving and unlikely to adjust immediately following a change in fertility regulations.

Indeed, existing studies suggest that fertility policy reforms may influence some socioeconomic outcomes over longer horizons. For example, changes in fertility policies may affect gender-related labor market outcomes and household investments in child quality (Huang *et al.*, 2024; Rosenzweig and Zhang, 2009; Qin *et al.*, 2017). Nevertheless, these adjustments typically occur gradually and are unlikely to account for the immediate fertility responses documented in our analysis.

Therefore, while broader socioeconomic factors are important for understanding China's long-run fertility decline and delayed childbearing, they are unlikely to be the primary drivers

of the short-run responses identified in this study. Instead, our evidence is more consistent with mechanisms operating through the relaxation of institutional fertility constraints, households' adjustments in fertility timing and decisions across birth orders, and changes in the child-rearing environment following the reform.

7 Conclusion

This study investigates the economic and demographic consequences of relaxing fertility constraints. Motivated by an extended Barro-Becker framework, we develop a perspective that fertility policy reforms may generate asymmetric responses across different margins of fertility decisions. Exploiting the staggered implementation of China's partial two-child policy across provinces and employing a difference-in-differences framework, we provide evidence on how relaxing birth quotas reshaped fertility transitions across birth orders and household groups.

Our findings reveal substantial heterogeneity across birth orders. The relaxation of birth quotas generated a substantial increase in second-child births, indicating that removing institutional constraints allowed previously restricted households to realize additional fertility. This intensive-margin response is particularly pronounced among groups with stronger preferences or greater demand for additional births, suggesting that fertility preferences play an important role in shaping responses to institutional reforms.

At the same time, the reform was associated with a short-run decline or postponement in first-child births. This finding highlights an important distinction between entering parenthood and having additional children. While institutional restrictions directly limited higher-order births under the previous fertility regime, first-child decisions were more closely related to the economic environment surrounding family formation. Consistent with this interpretation, we document that relaxing birth quotas was accompanied by increases in child-rearing-related costs, including consumer prices, housing prices, and childcare service costs.

These findings contribute to the understanding of fertility policy by showing that relaxing fertility constraints does not necessarily generate a uniform increase in fertility across households. Instead, institutional reforms may simultaneously release constrained fertility demand among some households while changing the timing and constraints of fertility decisions among others. The effects of fertility policies therefore depend on the interaction between institutional constraints, household fertility preferences, and the economic costs associated with raising children.

The policy implications of our findings extend beyond the conventional focus on increasing the total number of births. Effective fertility policies need to recognize that first births and higher-

order births are governed by different mechanisms. Policies that only relax fertility restrictions may be insufficient to encourage entry into parenthood if households continue to face substantial economic barriers. Reducing housing-related constraints, improving employment stability among young adults, expanding access to affordable childcare, and lowering child-rearing costs may therefore be important complements to fertility policy reforms.

More broadly, our study suggests that fertility policies should be evaluated not only by their effects on aggregate fertility levels. but also by how they shape fertility transitions across birth orders and household groups. Understanding these heterogeneous responses is essential for designing policies that address both the institutional and economic constraints underlying fertility decision.

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Appendix A

A.1 Proof of Lemma 1

To solve the optimization problem, we substitute out for $U(\alpha, t+1)$ (and $U(\alpha, t+2)$, etc.) in equation (1) and rewrite the utility function for household α in generation $t = 0$ as

$$U(\alpha, 0) = \sum_{t=0}^{\infty} \alpha^t \cdot [N(\alpha, t)]^{1-\epsilon} \cdot [c(\alpha, t)]^{\sigma}/\sigma. \tag{A.1}$$

Moreover, the dynastic budget constraint, which equates the present value of all resources to the present value of all expenditures, is expressed as

$$k(\alpha, 0) + \sum_{t=0}^{\infty} d(t)N(\alpha, t)w(t) = \sum_{t=0}^{\infty} d(t)[N(\alpha, t)P(t)c(t) + N(\alpha, t+1)\beta(\alpha, t)], \tag{A.2}$$

where $d(t) = \prod_{t'=0}^{t}[1 + r(t')]^{-1}$. Thus, the optimization problem can be expressed as choosing consumption $c(\alpha, t)$ and number of children $n(\alpha, t)$ to maximize utility $U(\alpha, 0)$ subject to the dynastic budget constraint. The corresponding Lagrange function is

$$\begin{aligned} L = &\sum_{t=0}^{\infty} \alpha^t [N(\alpha, t)]^{1-\epsilon}[c(\alpha, t)]^{\sigma}/\sigma + \lambda(\alpha, 0)[k(\alpha, 0) + \sum_{t=0}^{\infty} d(t)N(\alpha, t)w(t) \\ &- \sum_{t=0}^{\infty} d(t)N(\alpha, t)P(t)c(\alpha, t) - \sum_{t=0}^{\infty} d(t)N(\alpha, t+1)\beta(\alpha, t)], \end{aligned} \tag{A.3}$$

where $\lambda(\alpha, 0)$ is the Lagrange multiplier. The first-order conditions related to $c(\alpha, t)$ and $N(\alpha, t)$ are

$$\alpha^t [N(\alpha, t)]^{1-\epsilon}[c(\alpha, t)]^{\sigma-1} = \lambda(\alpha, 0)d(t)N(\alpha, t)P(t), \quad (t = 0, 1, \cdots) \tag{A.4}$$

and

$$\frac{(1-\epsilon)\alpha^t[c(\alpha, t)]^{\sigma}}{\sigma[N(\alpha, t)]^{\epsilon}} = \lambda(\alpha, 0)[d(t)P(t)c(\alpha, t) + d(t-1)\beta(\alpha, t-1) - d(t)w(t)], \quad (t = 1, 2, \cdots) \tag{A.5}$$

respectively. By iterating (A.4) forward one period, we obtain an equation of $t+1$. Combing the resulting equation and (A.4) yields

$$\left[\frac{c(\alpha, t+1)}{c(\alpha, t)}\right]^{1-\sigma} = \frac{\alpha[1 + r(t+1)]}{[n(\alpha, t)]^{\epsilon}} \cdot \frac{P(t)}{P(t+1)}, \quad (t = 0, 1, \cdots), \tag{A.6}$$

Further, using the form of $\lambda(\alpha, 0)$ from (A.4), we can rewrite (A.5) as (3). It shows that at period of $t \geq 1$, consumption per member of household α is independent of initial capital stock. Moreover, as in Becker and Barro (1988) and Barro and Becker (1989), this extended model also implies that

children are a net financial burden to altruistic parents; that is, the costs of raising a child would exceed that child's lifetime earnings. Finally, combining (A.6) and (3) yields the optimal number of children born such that

$$[n(\alpha,t)]^{\epsilon} = \alpha[1+r(t+1)]\left[\frac{P(t)}{P(t+1)}\right]^{\sigma}\left\{\frac{\beta(\alpha,t-1)[1+r(t)]-w(t)}{\beta(\alpha,t)[1+r(t+1)]-w(t+1)}\right\}^{1-\sigma}, \quad (t=1,2,\cdots). \tag{A.7}$$

A.2 Decentralized Equilibrium

In equilibrium, households maximize utility, firms maximize profits, and all markets clear. In generation $t \geq 1$, the total number of adults and children are $\hat{N}(t) = qN(\alpha^h,t) + (1-q)N(\alpha^l,t)$ and $\hat{N}(t+1) = qN(\alpha^h,t+1) + (1-q)N(\alpha^l,t+1)$, respectively. Therefore, the final good market-clearing condition is

$$C(t) + \mu\hat{N}(t+1) = Y(t), \tag{A.8}$$

where $C(t) \equiv qN(\alpha^h,t)c(\alpha^h,t) + (1-q)N(\alpha^l,t)c(\alpha^l,t)$ is the total consumption of final good, the left-hand side (LHS) and the right-hand side (RHS) of (A.8) are the aggregate demand and supply of final good, respectively. The labor market-clearing condition is $L(t) = \hat{N}(t)$, where the LHS is the total labor demand and the RHS denotes the total labor supply, namely the total number of adults. The total capital stocks is defined as $K(t) = qN(\alpha^h,t)k(\alpha^h,t) + (1-q)N(\alpha^l,t)k(\alpha^l,t)$, and the capital market also clears.

A.3 Long-term effects of relaxing birth quotas

In this subsection, we examine the long-run impact of the population control policy on birth rates. We first show that a change in the population control policy in this model induces a long transitional dynamic and there only exists an asymptotic steady state equilibrium. To see this, we need to characterize the change of the total birth rate. Total number of adults in generation $t+2$ is

$$\hat{N}(s+2) = q\cdot n(\alpha^h,0)\cdot(\tilde{n})^{s-1}\cdot(n^*)^2 + (1-q)\cdot n(\alpha^l,0)\cdot(\tilde{n})^{s-1}\cdot n(\alpha^l,s)\cdot n(\alpha^l,s+1). \tag{A.9}$$

$n(\alpha^l,s+1)$ could deviate from $n(\alpha^l,s)$ because the birth rates of both types of households are unequal since the period of s, which implying that the total birth rate is changing over time. As a result, the prices of final goods and interest rates are continuing to change, causing households to reconsider their fertility decisions. Recall the total number of adults in generation $s+1$ in (12).

We then obtain the total birth rate in generation $s+1$ such that

$$B(s+1) = \frac{\hat{N}(s+2)}{\hat{N}(s+1)} = \frac{q \cdot n(\alpha^h, 0) \cdot (n^*)^2 + (1-q) \cdot n(\alpha^l, 0) \cdot n(\alpha^l, s) \cdot n(\alpha^l, s+1)}{q \cdot n(\alpha^h, 0) \cdot n^* + (1-q) \cdot n(\alpha^l, 0) \cdot n(\alpha^l, s)}. \tag{A.10}$$

Following the same logic, we can conclude that the total birth rate at period of $s+j$ is

$$B(s+j) = \frac{\hat{N}(s+j+1)}{\hat{N}(s+j)} = \frac{q \cdot n(\alpha^h, 0) \cdot n^* + (1-q) \cdot n(\alpha^l, 0) \cdot \frac{n(\alpha^l, s)}{n^*} \cdots \frac{n(\alpha^l, s+j)}{n^*} \cdot n(\alpha^l, s+j+1)}{q \cdot n(\alpha^h, 0) + (1-q) \cdot n(\alpha^l, 0) \cdot \frac{n(\alpha^l, s)}{n^*} \cdots \frac{n(\alpha^l, s+j)}{n^*}}, \quad j > 0, \tag{A.11}$$

where we implicitly assume that n^* is sufficiently large so that $n^* > \max\{n(\alpha^l, s), \cdots, n(\alpha^l, s+j+1)\}$ is satisfied. As time goes by, the total birth rate is growing and approaches n^* for $j \to +\infty$, by applying the L'Hôpital's rule. During this transitional path towards an asymptotic steady state equilibrium, the family structure is varying. Specifically, the fraction of the α^h-type household is gradually rising and reaches one for $j \to +\infty$, while the fraction of the α^l-type household is declining and becomes negligible, approaching zero, for $j \to +\infty$.

Proposition 2. *Starting from the next period after the government relaxes birth quotas, the economy starts its transitional dynamics. Along the transitional path, the total birth rate is rising, the size of α^h-type household is increasing while the size of the α^l-type household is decreasing.*

Proof. Proven in text. □

Appendix B

Table B.1: Impact of relaxing birth quotas on second-child births using the two-way fixed effects model

Variables	(1) Second birth	(2) Native	(3) Migrant	(4) Old	(5) Young	(6) College	(7) No college	(8) Pension	(9) No pension
Treatment effect	−0.007	0.016	0.209	−0.113	0.220	0.030	0.021	−0.001	0.030
	(0.102)	(0.110)	(0.225)	(0.095)	(0.176)	(0.129)	(0.103)	(0.112)	(0.121)
Observations	371	371	174	370	353	291	362	365	340
Adjusted R^2	0.860	0.879	0.653	0.804	0.736	0.455	0.875	0.848	0.632

Notes: This table reports the impact of relaxing birth quotas on second-child births using the two-way fixed effects model. Birth variables are in the logarithm form. Column 1 shows the impact of relaxing birth quotas on second-child births. Columns 2 and 3 show the impact of relaxing birth quotas by migration status. Columns 4 and 5 show the impact of relaxing birth quotas by childbearing age. Columns 6 and 7 show the impact of relaxing birth quotas by education degree. Columns 8 and 9 show the impact of relaxing birth quotas by access to pension. Standard errors are clustered at the provincial level and are in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

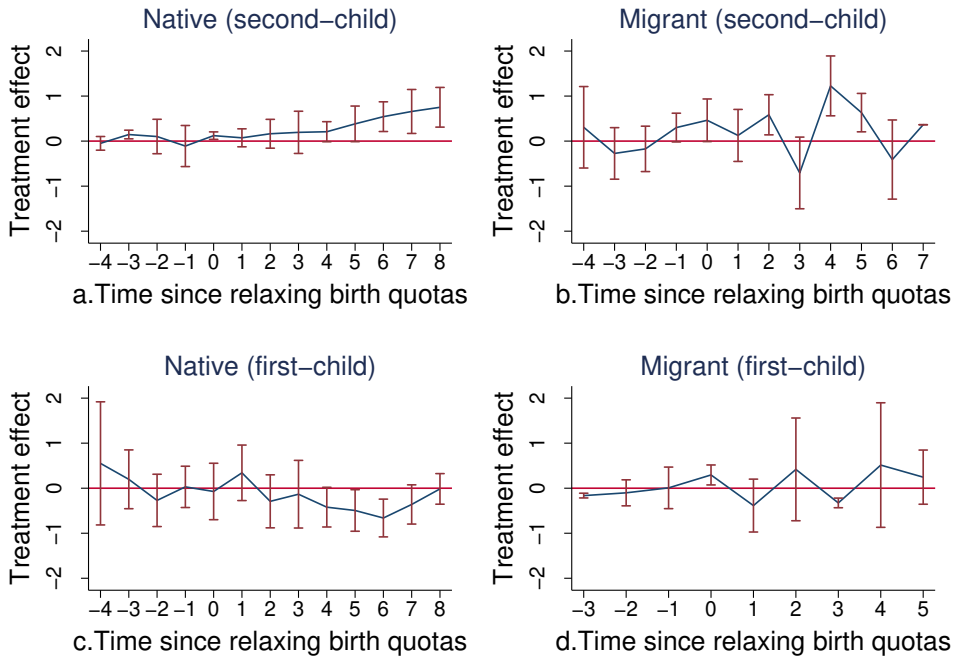


Figure B.1: Migration status and the effects of relaxing birth quotas on second-child and first-child births

Notes: This graph shows the impact of relaxing birth quotas on second-child births of household (a) registered in the same province and (b) registered in a different province, and the impact of relaxing birth quotas on first-child births of households (c) registered in the same province and (d) registered in other province. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. We define migrants as those individuals registered in a different province. Both first-child and second-child birth variables are in the logarithm form. The panel data on first-child and second-child births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

Appendix C

C.1 Recoding the timing of relaxing birth quotas

As already mentioned, using monthly birth data for estimation implies that assumptions are required for the minimum time it takes for the relaxation of birth quotas to noticeably affect local second-child (first-child) births. There are two main reasons. First, without recoding the timing of relaxing birth quotas, we cannot estimate the effect of relaxing birth quotas on childbearing

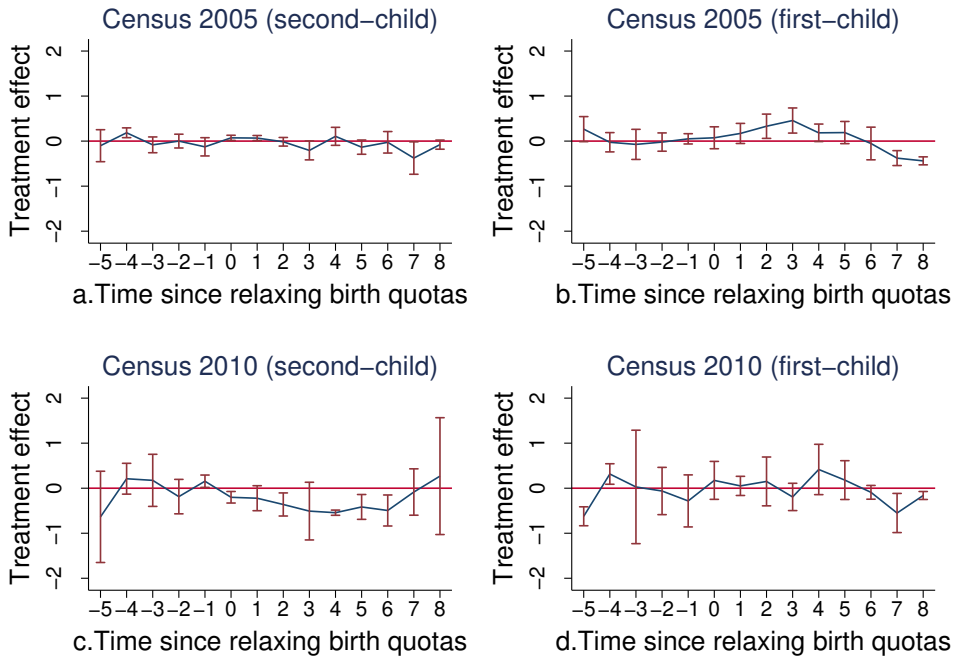


Figure B.2: Placebo analysis on the impact of relaxing birth quotas using censuses 2005 and 2010

Notes: This graph shows the impact of relaxing birth quotas on (a) counterfactual second-child births using census 2005, (b) counterfactual first-child births using census 2005, (c) counterfactual second-child births using census 2010, and (d) counterfactual first-child births using census 2010. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Both first-child and second-child birth variables are in the logarithm form. The panel data on first-child and second-child births from November 2004 to October 2005 are constructed using the 2005 census, and the panel data on first-child and second-child births from November 2009 to October 2010 are constructed using the 2010 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

decisions in a DID setup. This is because the census data provide monthly births from November 2014 to October 2015, while most provinces relaxed birth quotas before November 2014. In other words, the timing of monthly births mostly does not overlap with the timing of relaxing birth quotas. As a result, the untreated provinces will be too few to estimate the effect of relaxing birth quotas on childbearing decisions in a DID setup. Second, even if we have early data on monthly births that overlap the timing of relaxing birth quotas, estimating the effect of relaxing birth quotas on childbearing decisions without recoding the timing of relaxing birth quotas remains problematic because its effect on childbearing decisions would be largely contaminated

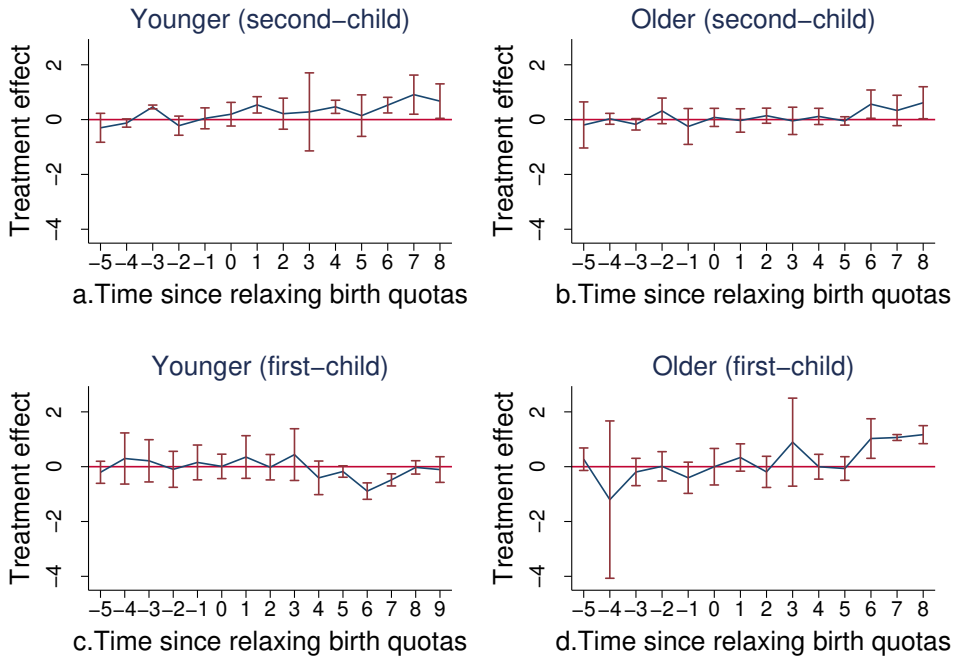


Figure B.3: Childbearing age and effects of relaxing birth quotas on second-child and first-child births.

Notes: This graph shows the impact of relaxing birth quotas on second-child births of (a) couples of relatively early childbearing age (below the median value of the childbearing age distribution), (b) couples of relatively late childbearing age (above the median value of the childbearing age distribution) and first child births of (c) couples of relatively early childbearing age and (d) couples of relatively late childbearing age. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Relatively early childbearing age are defined as women below the median value of the childbearing age distribution. Second-child and first-child births variables are in the logarithm form. The panel data on second-child and first-child births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

by biological effects due to the fertility process. We further discuss how to recode the timing of relaxing birth quotas as follows.

C.2 Time to obtain a birth permit for a second child (1 month)

Under this policy, prospective parents are required to apply for a birth permit in advance of having a second child. According to the official procedure and documents for granting birth

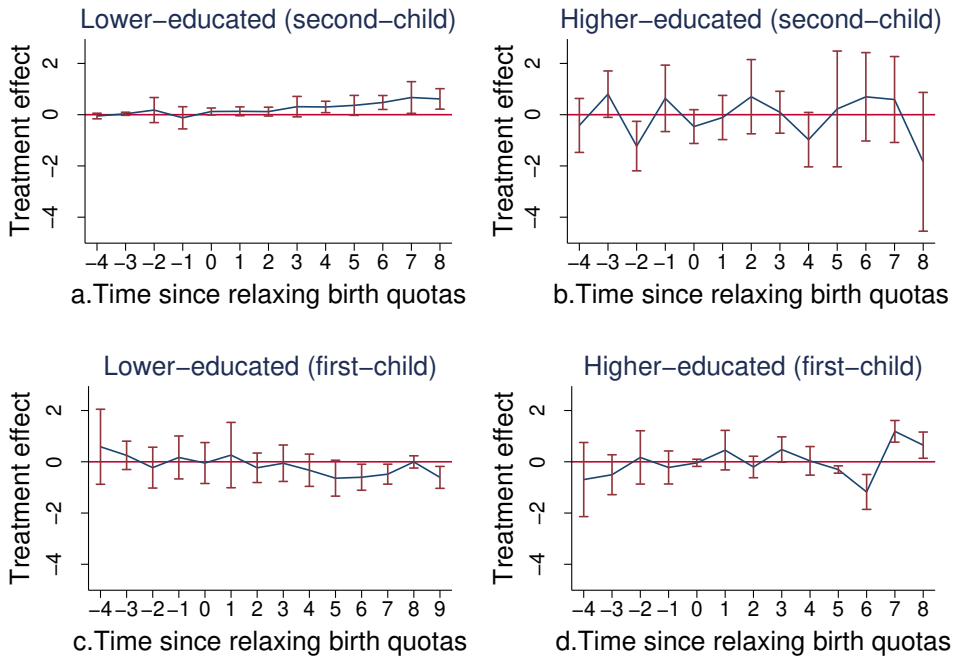


Figure B.4: Education and effects of relaxing birth quotas on second-child and first-child births.

Notes: This graph shows the impact of relaxing birth quotas on second-child births of (a) relatively lower-educated women, (b) relatively higher-educated women and first child births of (c) relatively lower-educated women and (d) relatively higher-educated women. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Relatively higher-educated women are defined as women with a college degree or more. Second-child and first-child births variables are in the logarithm form. The panel data on second-child and first-child births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

permits, the average time for obtaining a birth permit from the local authorities ranges from 15 days to 45 days. Thus, we assume that it takes 1 month on average for prospective parents to obtain a birth permit from local authorities in order to be legally allowed to have a second child.

C.3 Time to pregnancy for a second (first) child (1 month)

Based on previous studies in human reproduction (Olsen, Juul and Basso (1998); Gnoth, Godehardt, Godehardt, Frank-Herrmann and Freundl (2003); Eisenberg, Thoma, Li and McLain (2021)), we assume the minimum time to pregnancy for a second (first) child is 1 month.

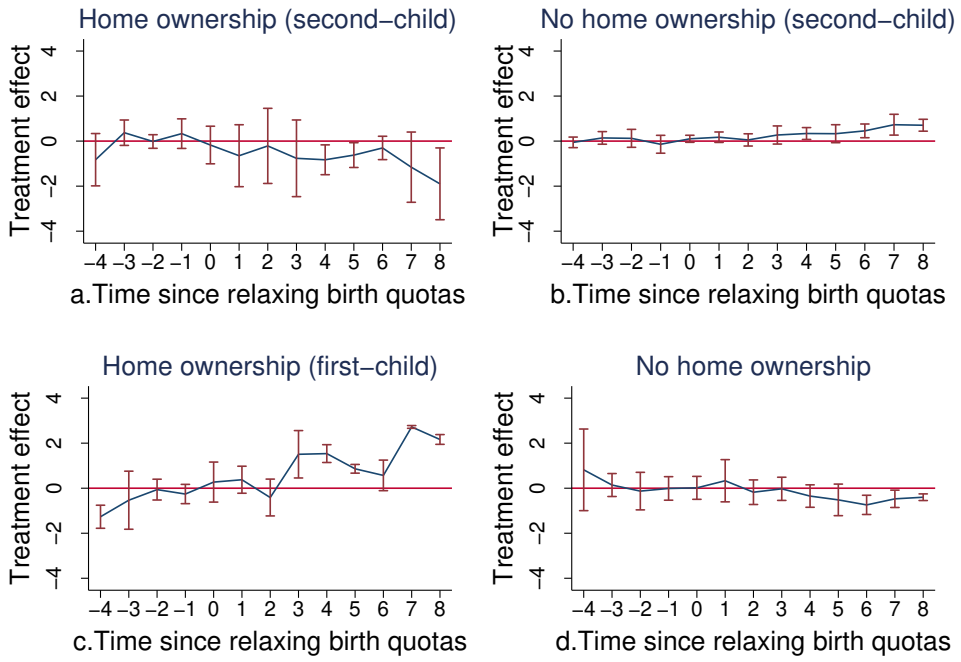


Figure B.5: Homeownership and the effects of relaxing birth quotas on second-child and first-child births.

Notes: This graph shows the impact of relaxing birth quotas on second-child births of household (a) with homeownership and (b) without homeownership and the impact of relaxing the OCP on first-child births of households (c) with homeownership and (d) without homeownership. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Households with homeownership are defined as households that have purchased the house. Both first-child and second-child birth variables are in the logarithm form. The panel data on first-child and second-child births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

C.4 Duration of pregnancy (10 months)

A full-term pregnancy generally lasts between 9 and 10 months. Moreover, our birth data are at the monthly level. For example, birth data in December consists of all births that occurred from December 1 to December 31. Thus, we assume that a pregnancy lasts 10 months on average to capture the impact of relaxing birth quotas on local births.

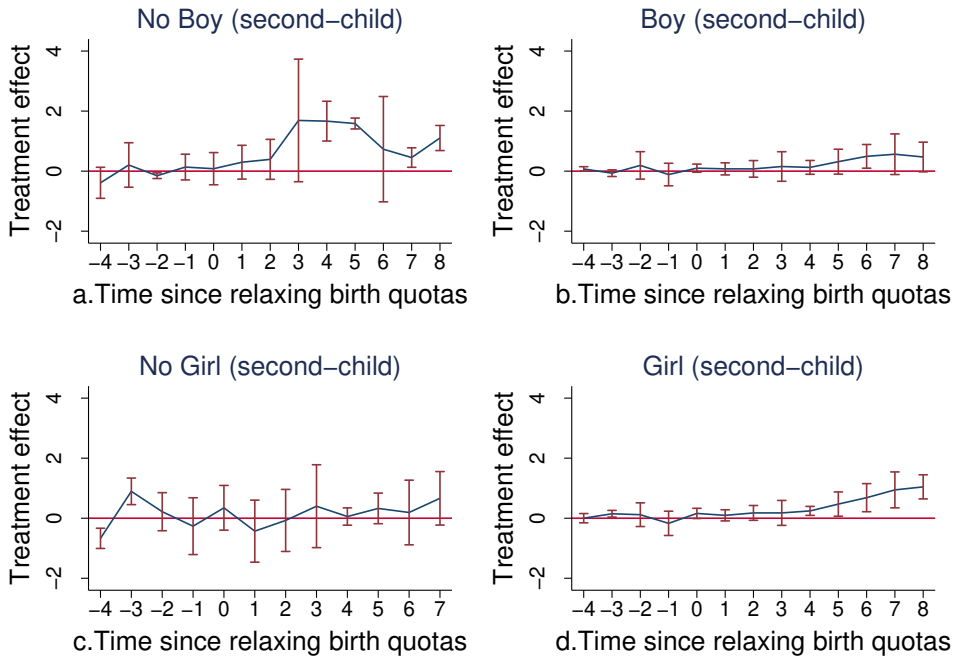


Figure B.6: Gender of children and the effects of relaxing birth quotas on second-child.

Notes: This graph shows the impact of relaxing birth quotas on second-child births of household (a) with no boy, (b) with at least a boy, (c) with no girl and (d) with at least a girl at the time of the survey. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Second-child birth variables are in the logarithm form. The panel data on second-child births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

Appendix D

D.1 Additional evidence

Figure D.1 displays the monthly number of first-child and second-child births based on China's Population Censuses from 2000 to 2015. We observed a gradual decrease in first-child births during this period, while second-child births remained relatively steady until 2010. By 2015, there was a noticeable increase in second-child births, while first-child births saw a significant decline. This trend is not only observed at the national level but also consistently across all provinces.

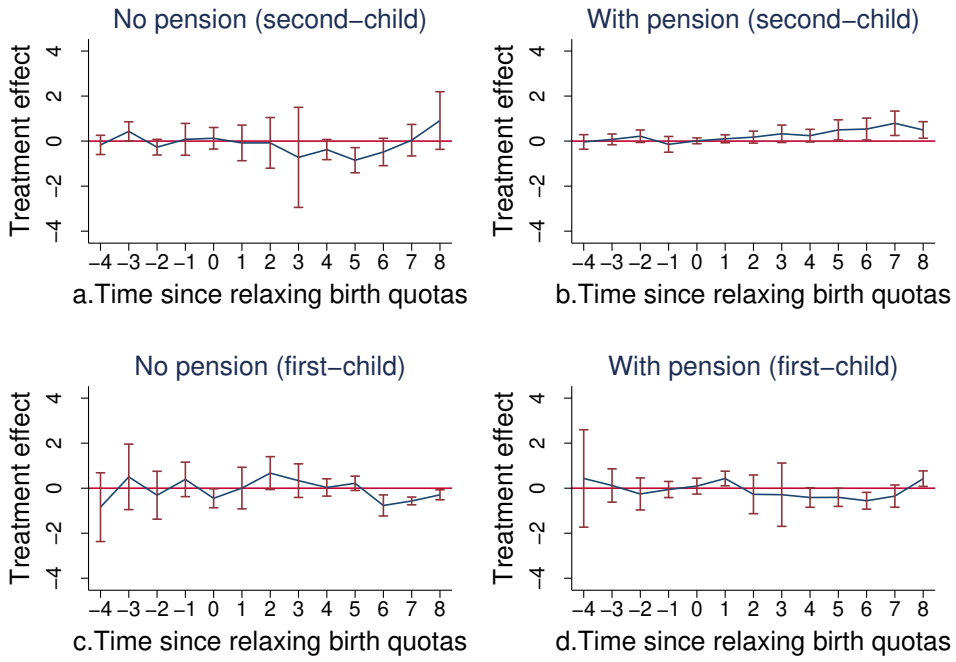


Figure B.7: Access to pension and the effects of relaxing birth quotas on second-child and first-child births.

Notes: This graph shows the impact of relaxing birth quotas on second-child births of household (a) with access to pension and (b) with no access to pension and the impact of relaxing the birth quotas on first-child births of households (c) with access to pension and (d) with no access to pension. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Both first-child and second-child birth variables are in the logarithm form. The panel data on first-child and second-child births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

Appendix E

E.1 Sensitivity to the Number of Bootstrap Repetitions: Second-Child Births

This subsection examines whether the statistical inference for the estimated dynamic effects of the relaxation of birth quotas on second-child births is sensitive to the number of bootstrap repetitions. We recompute the bootstrap standard errors and confidence intervals using alternative numbers of repetitions, while holding the empirical specification, estimation sample, and control variables unchanged.

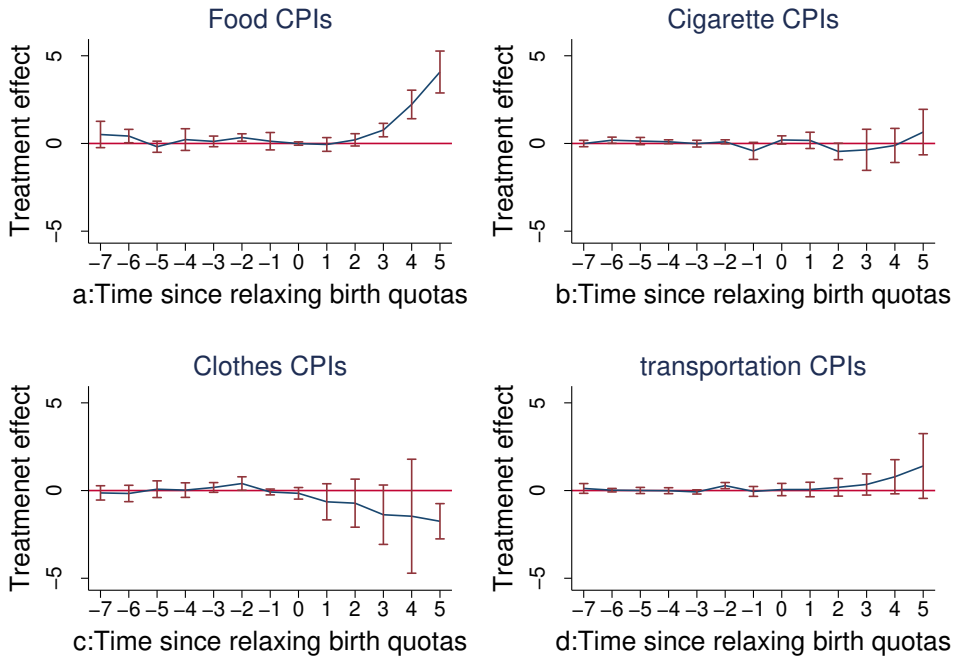


Figure B.8: Effects of relaxing birth quotas on CPIs by expenditure categories

Notes: This graph shows the impact of relaxing birth quotas on CPIs of (a) the food category, (b) the tobacco and alcohol category, (c) the clothing category, and (d) transportation and communications category. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Both first-child and second-child birth variables are in the logarithm form. The panel data on CPIs by expenditure categories from August 2014 to July 2015 come from China's National Bureau of Statistics. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

Figure E.1 reports the estimated dynamic effect in the seventh month after policy implementation under alternative numbers of bootstrap repetitions. This month is examined because the estimated effect reaches statistical significance at this point in the post-treatment period.

To provide the underlying numerical estimates, Table E.1 reports the complete sequence of dynamic effects during the first eight months after policy implementation. The reported standard errors and confidence intervals are based on 500 bootstrap repetitions.

The estimates display a gradual increase in second-child births following the implementation of the policy. The estimated coefficients are positive throughout the first eight post-treatment months, although they are imprecisely estimated during the initial months. The effect becomes statistically significant in month 7, when the estimated coefficient is 0.6568, corresponding to

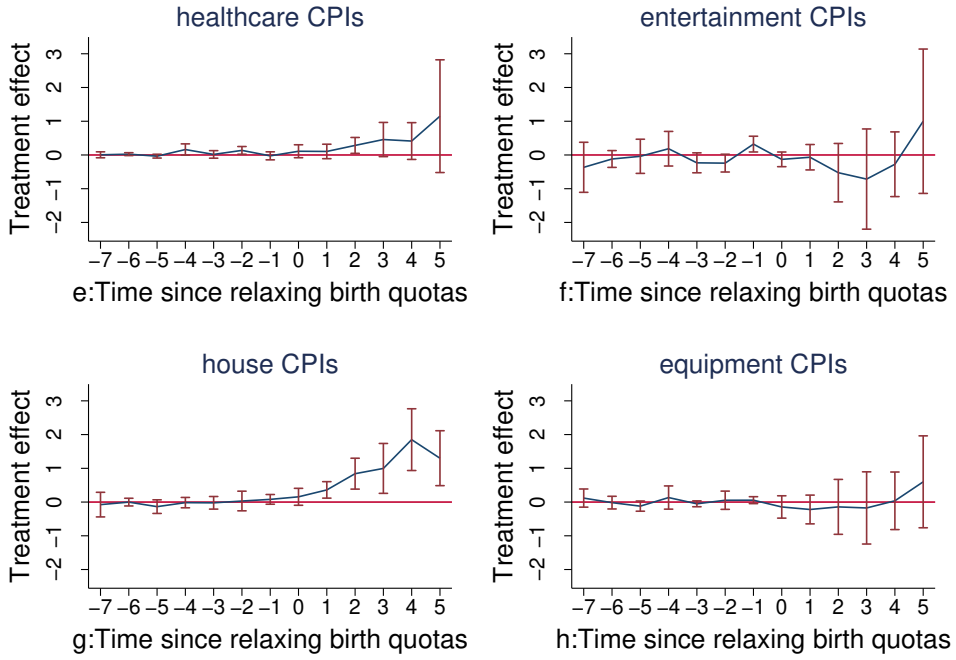


Figure B.9: Effects of relaxing birth quotas on CPIs by expenditure categories (cont.)

Notes: This graph shows the impact of relaxing birth quotas on the CPIs of (e) the health care category, (f) the education, culture, and entertainment category, (g) the housing category, and (h) the household facility, article, and maintenance service category. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Both first-child and second-child birth variables are in the logarithm form. The panel data on CPIs by expenditure categories from August 2014 to July 2015 come from China's National Bureau of Statistics. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

an increase of approximately 92.9 percent in second-child births. The associated 95 percent confidence interval, ranging from 0.1251 to 1.1884, excludes zero.

More importantly, Figure E.1 shows that increasing the number of bootstrap repetitions does not significantly change the estimated month-7 effect or its confidence interval. The conclusion regarding statistical significance therefore remains unchanged across alternative bootstrap settings. These results indicate that the statistical inference for the dynamic effect on second-child births is not driven by the particular number of bootstrap repetitions used in the baseline estimation.

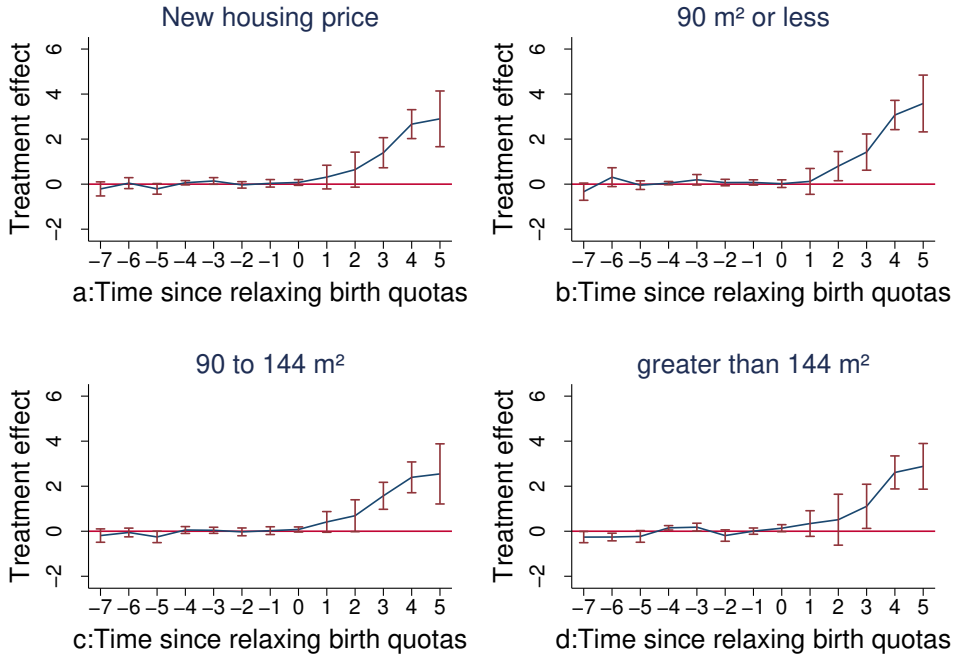


Figure B.10: Heterogeneous effects of relaxing birth quotas on sale prices of newly constructed residential buildings

Notes: This graph shows the impact of relaxing birth quotas on sale prices of (a) newly constructed residential buildings, (b) newly constructed residential buildings with floor area of 90 m^2 or less, (c) newly constructed residential buildings with floor area from 90 to 144 m^2, and (d) newly constructed residential buildings with floor area greater than 144 m^2. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. We collect data on sale price indices of residential buildings from August 2014 to July 2015 that are available in 70 large and medium-sized cities across all Chinese provinces except for Tibet, provided by the National Bureau of Statistics of China. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

E.2 Sensitivity to the Number of Bootstrap Repetitions: First-Child Births

This subsection examines whether the statistical inference for the estimated average effect of the relaxation of birth quotas on first-child births is sensitive to the number of bootstrap repetitions. We recompute the bootstrap standard errors and confidence intervals using alternative numbers of repetitions, while holding the empirical specification, estimation sample, and control variables unchanged.

Figure E.2 reports the estimated average treatment effect on first-child births under alternative

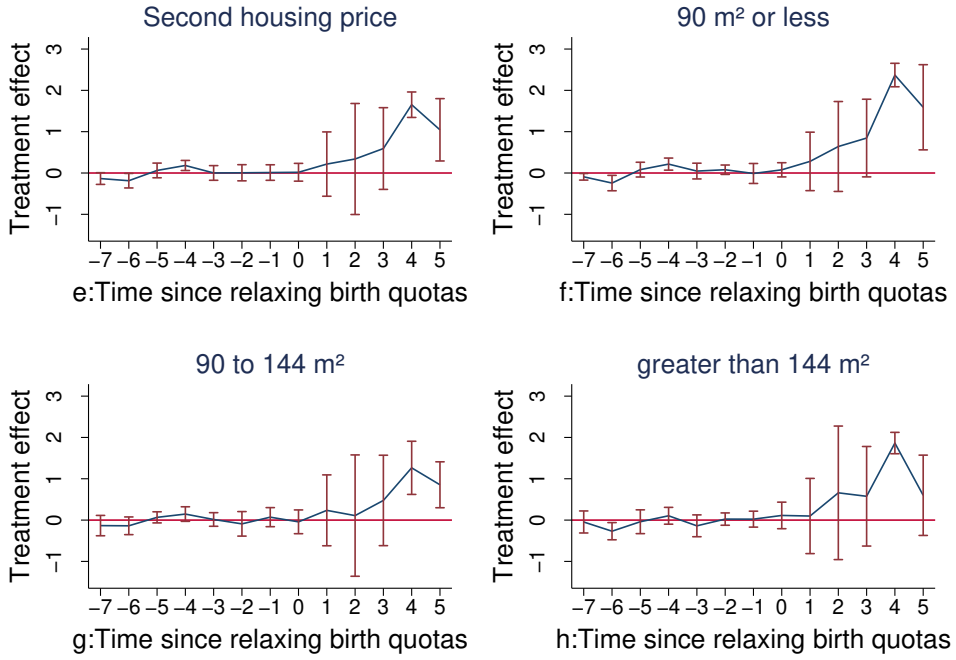


Figure B.11: Heterogeneous effects of relaxing birth quotas on sale prices of second-hand residential buildings.

Notes: This graph shows the impact of relaxing birth quotas on sale prices of (e) second-hand residential buildings, (f) second-hand residential buildings with floor area of 90 m^2 and below, (g) second-hand residential buildings with floor space from 90 to 144 m^2, and (h) second-hand residential buildings with floor space of above 144 m^2. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. We collect data on sale price indices of residential buildings from August 2014 to July 2015 that are available in 70 large and medium-sized cities across all Chinese provinces except for Tibet, provided by the National Bureau of Statistics of China. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

numbers of bootstrap repetitions.

To provide the underlying dynamic estimates, Table E.2 reports the complete sequence of effects during the first nine months after policy implementation. The reported standard errors and confidence intervals are based on 500 bootstrap repetitions.

The dynamic estimates point to a predominantly negative response of first-child births following the relaxation of birth quotas. With the exception of months 1 and 8, the estimated coefficients are negative throughout the first nine post-treatment months. The estimates are generally imprecise during the early months, but the negative effect becomes statistically significant

Table E.1: Dynamic Effects of the Relaxation of Birth Quotas on Second-Child Births

Month	Coefficient	Bootstrap S.E.	p-value	Lower bound	Upper bound	Effect (%)
1	0.1054	0.1861	0.5712	-0.2593	0.4701	11.11
2	0.1354	0.1844	0.4626	-0.2259	0.4968	14.50
3	0.1948	0.3951	0.6220	-0.5796	0.9692	21.50
4	0.2071	0.3041	0.4959	-0.3890	0.8031	23.01
5	0.4058	0.4363	0.3522	-0.4492	1.2609	50.06
6	0.4982	0.4480	0.2661	-0.3799	1.3764	64.58
7	0.6568	0.2713	0.0155	0.1251	1.1884	92.85
8	0.5738	0.4375	0.1897	-0.2837	1.4312	77.49

Notes: This table reports the estimated dynamic effects of the relaxation of birth quotas on second-child births during the first eight months after policy implementation. Bootstrap standard errors, p-values, and 95 percent confidence intervals are based on 500 repetitions. The lower and upper bounds correspond to the 95 percent confidence interval. The percentage effect is calculated as $100[\exp(\widehat{\beta}) - 1]$.

Table E.2: Dynamic Effects of the Relaxation of Birth Quotas on First-Child Births

Month	Coefficient	Bootstrap S.E.	p-value	Lower bound	Upper bound	Effect (%)
1	0.2811	0.2076	0.1757	-0.1258	0.6881	32.46
2	-0.3248	0.2870	0.2577	-0.8873	0.2377	-27.73
3	-0.2667	0.4565	0.5591	-1.1613	0.6280	-23.41
4	-0.4629	0.2856	0.1050	-1.0227	0.0968	-37.06
5	-0.4898	0.4314	0.2562	-1.3353	0.3557	-38.73
6	-0.6805	0.2412	0.0048	-1.1533	-0.2077	-49.36
7	-0.3168	0.3426	0.3551	-0.9883	0.3547	-27.15
8	0.1885	0.1994	0.3443	-0.2022	0.5793	20.75
9	-0.5972	0.4255	0.1604	-1.4312	0.2367	-44.97

Notes: This table reports the estimated dynamic effects of the relaxation of birth quotas on first-child births during the first nine months after policy implementation. Bootstrap standard errors, p-values, and 95 percent confidence intervals are based on 500 repetitions. The lower and upper bounds correspond to the 95 percent confidence interval. The percentage effect is calculated as $100[\exp(\widehat{\beta}) - 1]$.

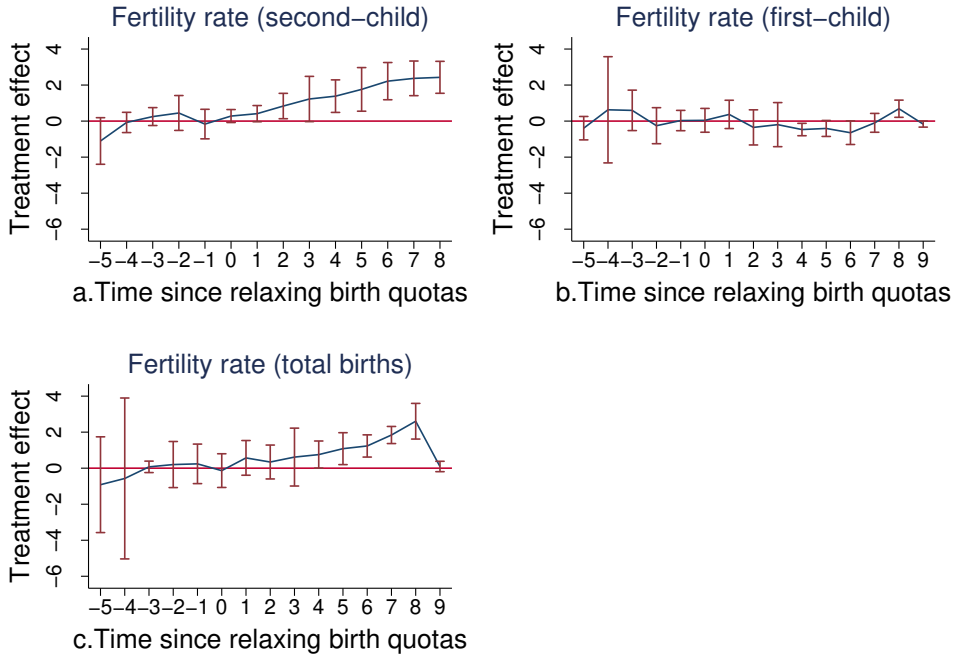


Figure B.12: Impact of relaxing birth quotas on second-child, first-child and total births fertility rates

Notes: This graph shows the impact of relaxing birth quotas on (a) second-child fertility rate (i.e. the number of births divided by the number of women of aged between 15 and 50 years old), (b) first-child and (c) total births fertility rate. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos.The panel data on first-child, second-child and total births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

in month 6. At that point, the estimated coefficient is -0.6805, corresponding to a decline of approximately 49.4 percent in first-child births. The associated 95 percent confidence interval, ranging from -1.1533 to -0.2077, excludes zero.

The absence of statistical significance in most individual months should be interpreted cautiously because the monthly estimates are relatively noisy. Nevertheless, the predominance of negative coefficients, together with the sizeable and statistically significant decline in month 6, is consistent with a short-run reduction or postponement in entry into parenthood following the policy reform.

Figure E.2 further shows whether inference for the estimated average effect depends on the

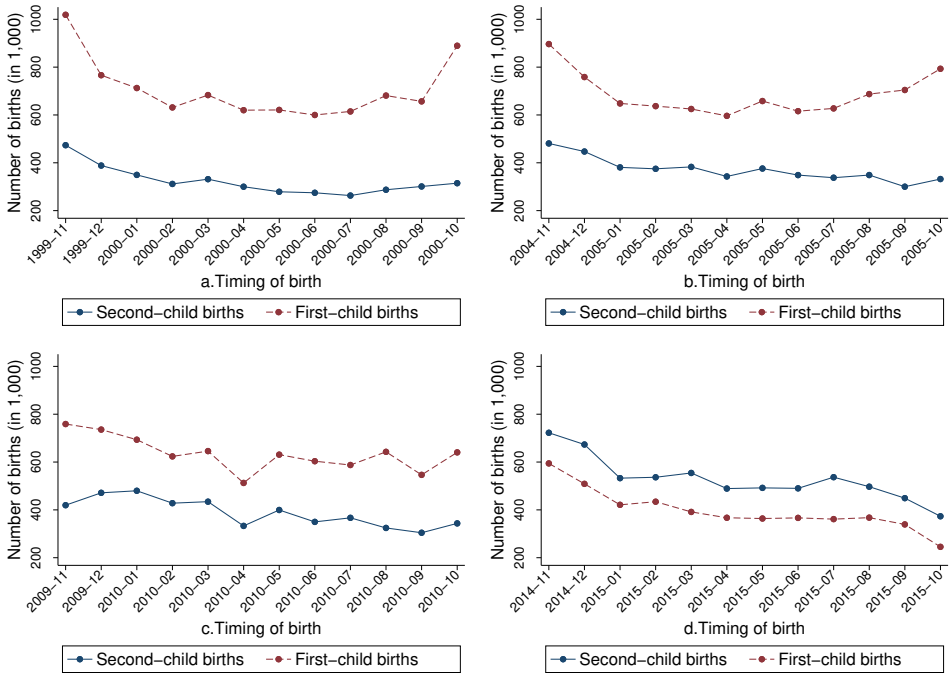


Figure D.1: Number of first-child and second-child births over time (2000 − 2015).

Notes: This graph shows the number of monthly first-child and second-child births (a) from November 1999 to October 2000, (b) from November 2004 to October 2005, (c) from November 2009 to October 2010, and (d) from November 2014 to October 2015. Data on monthly first-child and second-child births in each panel are constructed by the authors using China's Population Censuses in 2000, 2005, 2010, and 2015, respectively.

number of bootstrap repetitions. The point estimate and confidence interval remain broadly stable as the number of repetitions increases. The substantive conclusion regarding the direction and statistical precision of the average effect is therefore not significantly altered by the bootstrap setting used in the baseline estimation.

E.3 Sensitivity to the Number of Bootstrap Repetitions: Consumer Prices

This subsection examines whether the statistical inference for the estimated average effect of the relaxation of birth quotas on consumer prices is sensitive to the number of bootstrap repetitions. We recompute the bootstrap standard errors and confidence intervals using alternative numbers of repetitions, while holding the empirical specification, estimation sample, and control variables unchanged.

Figure E.3 reports the estimated average treatment effect on the consumer price index under alternative numbers of bootstrap repetitions.

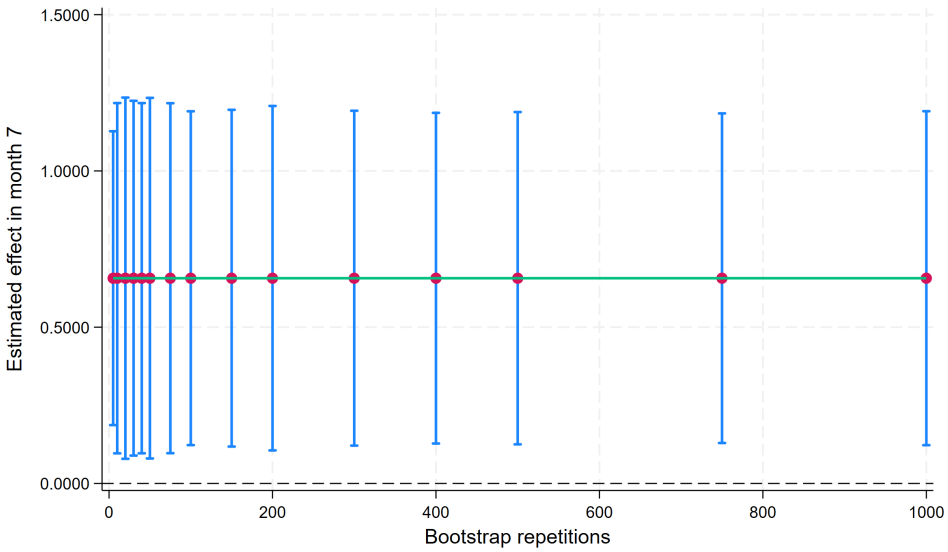


Figure E.1: Sensitivity of the Dynamic Effect on Second-Child Births to the Number of Bootstrap Repetitions

Notes: This figure reports the estimated dynamic effect of the relaxation of birth quotas on second-child births in the seventh month after policy implementation under alternative numbers of bootstrap repetitions. The circles denote the point estimates, and the vertical bars represent the corresponding 95 percent confidence intervals. The empirical specification, estimation sample, and control variables are held constant across all estimates. The dashed horizontal line denotes zero.

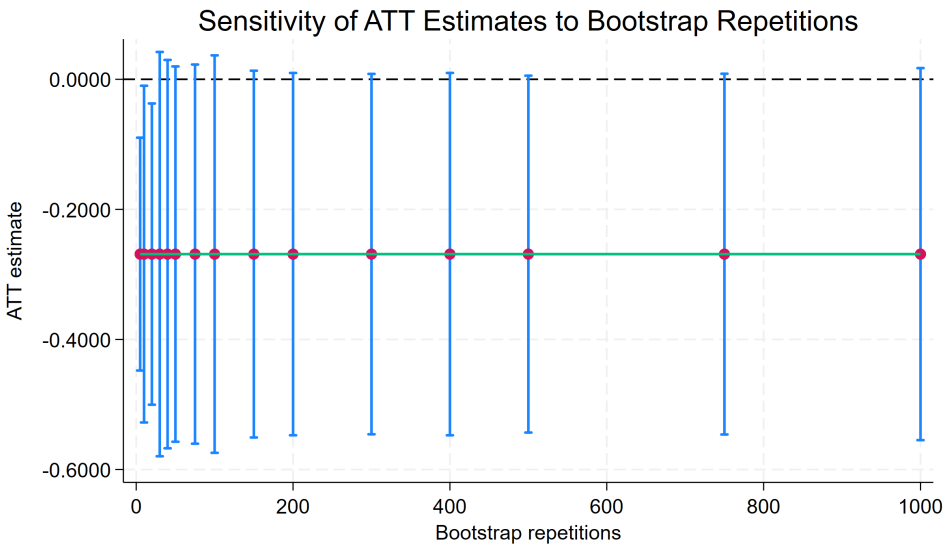


Figure E.2: Sensitivity of the Estimated Average Effect on First-Child Births to the Number of Bootstrap Repetitions

Notes: This figure reports the estimated average treatment effect of the relaxation of birth quotas on first-child births under alternative numbers of bootstrap repetitions. The circles denote the point estimates, and the vertical bars represent the corresponding 95 percent confidence intervals. The empirical specification, estimation sample, and control variables are held constant across all estimates. The dashed horizontal line denotes zero.

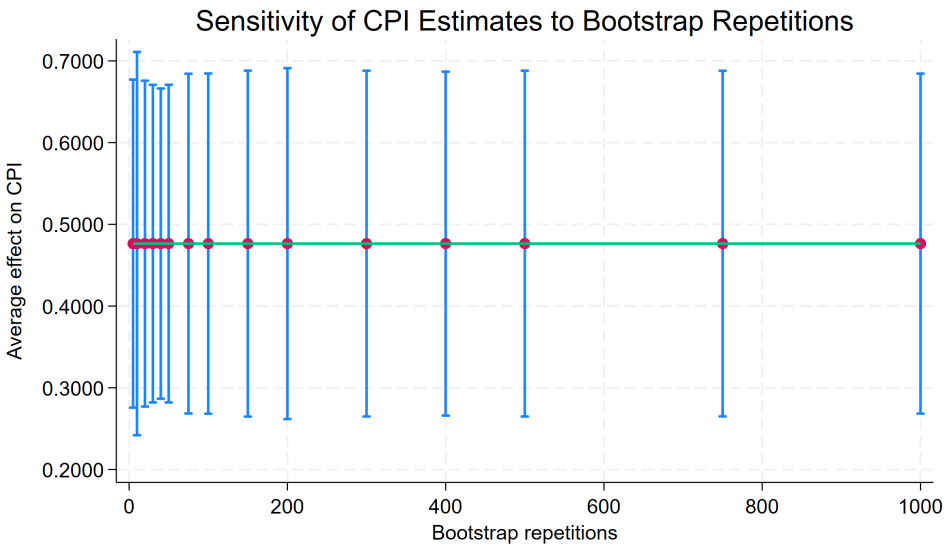


Figure E.3: Sensitivity of the Estimated Average Effect on Consumer Prices to the Number of Bootstrap Repetitions

Notes: This figure reports the estimated average treatment effect of the relaxation of birth quotas on the consumer price index under alternative numbers of bootstrap repetitions. The circles denote the point estimates, and the vertical bars represent the corresponding 95 percent confidence intervals. The empirical specification, estimation sample, and control variables are held constant across all estimates. The dashed horizontal line denotes zero.

To provide the underlying dynamic estimates, Table E.3 reports the complete sequence of effects during the first five months after policy implementation. The reported standard errors and confidence intervals are based on 500 bootstrap repetitions.

Table E.3: Dynamic Effects of the Relaxation of Birth Quotas on Consumer Prices

Month	Coefficient	Bootstrap S.E.	p-value	Lower bound	Upper bound
1	-0.0175	0.1094	0.8729	-0.2319	0.1969
2	0.0598	0.1755	0.7332	-0.2841	0.4038
3	0.1833	0.2278	0.4209	-0.2631	0.6298
4	0.9125	0.2644	0.0006	0.3943	1.4307
5	1.7000	0.2062	0.0000	1.2958	2.1042

Notes: This table reports the estimated dynamic effects of the relaxation of birth quotas on the consumer price index during the first five months after policy implementation. Bootstrap standard errors, p-values, and 95 percent confidence intervals are based on 500 repetitions. The lower and upper bounds correspond to the 95 percent confidence interval. The empirical specification and estimation sample are the same as those used in the corresponding dynamic-effect analysis.

The dynamic estimates indicate that the response of consumer prices is limited during the first three months after policy implementation. The estimated effect is close to zero in month 1 and remains statistically indistinguishable from zero through month 3. The estimates then increase sharply. In month 4, the coefficient rises to 0.9125 and becomes statistically significant at conventional levels, with a 95 percent confidence interval ranging from 0.3943 to 1.4307.

The estimated effect increases further in month 5. The coefficient reaches 1.7000, with a bootstrap standard error of 0.2062 and a 95 percent confidence interval ranging from 1.2958 to 2.1042. These results suggest that the price response emerges with a short delay and becomes both economically sizeable and precisely estimated several months after the policy reform.

Figure E.3 further examines whether inference for the estimated average effect on consumer prices depends on the number of bootstrap repetitions. The point estimate and corresponding confidence interval remain broadly stable as the number of repetitions increases. The substantive conclusion regarding the positive average price effect is therefore not significantly altered by the bootstrap setting used in the baseline estimation.

E.4 Sensitivity to the Number of Bootstrap Repetitions: Housing Prices

This subsection examines whether the statistical inference for the estimated average effect of the relaxation of birth quotas on housing prices is sensitive to the number of bootstrap repetitions. We recompute the bootstrap standard errors and confidence intervals using alternative numbers of repetitions, while holding the empirical specification, estimation sample, and control variables

unchanged.

Figure E.4 reports the estimated average treatment effect on the housing price index under alternative numbers of bootstrap repetitions.



Figure E.4: Sensitivity of the Estimated Average Effect on Housing Prices to the Number of Bootstrap Repetitions

Notes: This figure reports the estimated average treatment effect of the relaxation of birth quotas on the housing price index under alternative numbers of bootstrap repetitions. The circles denote the point estimates, and the vertical bars represent the corresponding 95 percent confidence intervals. The empirical specification, estimation sample, and control variables are held constant across all estimates. The dashed horizontal line denotes zero.

To provide the underlying dynamic estimates, Table E.4 reports the complete sequence of effects during the first five months after policy implementation. The reported standard errors and confidence intervals are based on 500 bootstrap repetitions.

The dynamic estimates indicate a positive and progressively increasing response of housing prices following the relaxation of birth quotas. The estimated effect is positive but statistically imprecise in the first month. By month 2, the coefficient rises to 0.6869 and becomes statistically significant at the 5 percent level, with a 95 percent confidence interval ranging from 0.0828 to 1.2910.

The estimated effect continues to increase over the subsequent months. It reaches 1.3804 in month 3, 2.4882 in month 4, and 2.8091 in month 5. All three estimates are statistically significant at conventional levels, and their confidence intervals exclude zero. This pattern suggests that the response of housing prices strengthens over time rather than appearing as a short-lived adjustment immediately after the reform.

Table E.4: Dynamic Effects of the Relaxation of Birth Quotas on Housing Prices

Month	Coefficient	Bootstrap S.E.	p-value	Lower bound	Upper bound
1	0.2896	0.1862	0.1199	-0.0754	0.6546
2	0.6869	0.3082	0.0258	0.0828	1.2910
3	1.3804	0.4574	0.0025	0.4838	2.2770
4	2.4882	0.5411	0.0000	1.4278	3.5487
5	2.8091	0.8090	0.0005	1.2234	4.3948

Notes: This table reports the estimated dynamic effects of the relaxation of birth quotas on the housing price index during the first five months after policy implementation. Bootstrap standard errors, p-values, and 95 percent confidence intervals are based on 500 repetitions. The lower and upper bounds correspond to the 95 percent confidence interval. The empirical specification and estimation sample are the same as those used in the corresponding dynamic-effect analysis.

Figure E.4 further examines whether inference for the estimated average effect on housing prices depends on the number of bootstrap repetitions. The point estimate and corresponding confidence interval remain broadly stable as the number of repetitions increases. The substantive conclusion regarding the positive average housing-price effect is therefore not significantly altered by the bootstrap setting used in the baseline estimation.

E.5 Sensitivity to the Number of Bootstrap Repetitions: Total Births

This subsection examines whether the statistical inference for the estimated average effect of the relaxation of birth quotas on total births is sensitive to the number of bootstrap repetitions. We recompute the bootstrap standard errors and confidence intervals using alternative numbers of repetitions, while holding the empirical specification, estimation sample, and control variables unchanged.

Figure E.5 reports the estimated average treatment effect on total births under alternative numbers of bootstrap repetitions.

To provide the underlying dynamic estimates, Table E.5 reports the complete sequence of effects during the first eight months after policy implementation. The reported standard errors and confidence intervals are based on 500 bootstrap repetitions.

The dynamic estimates indicate a positive response of total births following the relaxation of birth quotas. The estimated coefficients are positive throughout the first eight post-treatment months and generally increase over time during the first seven months. The estimates are imprecise during the initial months, but the effect becomes statistically significant in month 7.

In month 7, the estimated coefficient is 0.5889, corresponding to an increase of approximately 80.2 percent in total births. The associated 95 percent confidence interval ranges from 0.1454 to

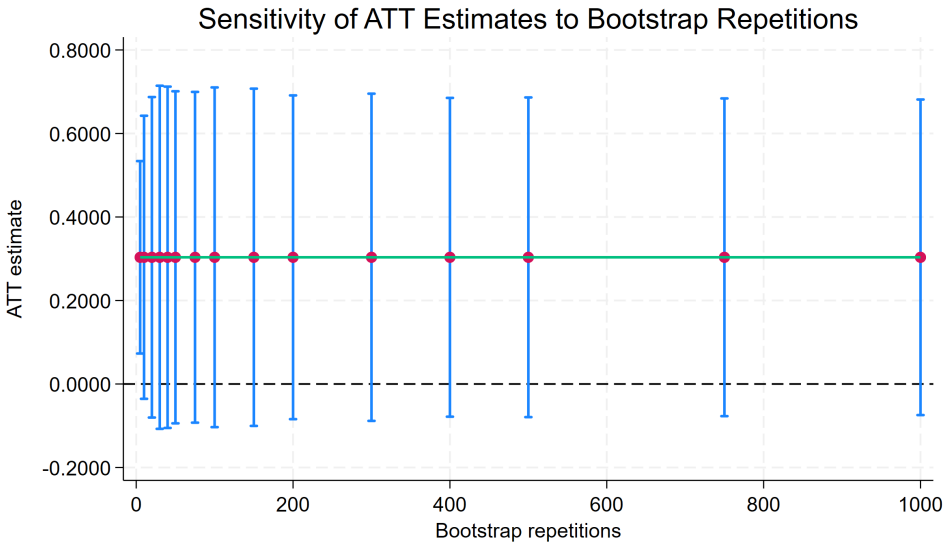


Figure E.5: Sensitivity of the Estimated Average Effect on Total Births to the Number of Bootstrap Repetitions

Notes: This figure reports the estimated average treatment effect of the relaxation of birth quotas on total births under alternative numbers of bootstrap repetitions. The circles denote the point estimates, and the vertical bars represent the corresponding 95 percent confidence intervals. The empirical specification, estimation sample, and control variables are held constant across all estimates. The dashed horizontal line denotes zero.

Table E.5: Dynamic Effects of the Relaxation of Birth Quotas on Total Births

Month	Coefficient	Bootstrap S.E.	p-value	Lower bound	Upper bound	Effect (%)
1	0.1112	0.1500	0.4587	-0.1829	0.4052	11.76
2	0.1412	0.1399	0.3128	-0.1330	0.4155	15.17
3	0.1855	0.3025	0.5396	-0.4073	0.7783	20.38
4	0.2475	0.1763	0.1603	-0.0980	0.5929	28.08
5	0.3917	0.3578	0.2735	-0.3095	1.0930	47.96
6	0.4716	0.3377	0.1626	-0.1903	1.1334	60.25
7	0.5889	0.2263	0.0093	0.1454	1.0324	80.20
8	0.3376	0.2039	0.0978	-0.0620	0.7371	40.15

Notes: This table reports the estimated dynamic effects of the relaxation of birth quotas on total births during the first eight months after policy implementation. Bootstrap standard errors, p-values, and 95 percent confidence intervals are based on 500 repetitions. The lower and upper bounds correspond to the 95 percent confidence interval. The percentage effect is calculated as $100[\exp(\widehat{\beta}) - 1]$.

1.0324 and excludes zero. The estimated effect remains positive in month 8, although its statistical significance weakens, with a p-value of 0.0978.

These results suggest that the increase in second-child births more than offsets the short-run decline or postponement in first-child births, at least over the period captured by the dynamic estimates. At the same time, the aggregate effect is smaller than the corresponding increase in second-child births, which is consistent with the offsetting negative response of first-child fertility.

Figure E.5 further examines whether inference for the estimated average effect on total births depends on the number of bootstrap repetitions. The point estimate and corresponding confidence interval remain broadly stable as the number of repetitions increases. The substantive conclusion regarding the direction and statistical precision of the average effect is therefore not significantly altered by the bootstrap setting used in the baseline estimation.

E.6 Heterogeneity by Policy Exposure Intensity

In the main analysis, we identify the effects of relaxing birth quotas by exploiting variation in the timing of policy implementation across provinces. In this section, we further examine whether the treatment effects vary with the intensity of exposure to the reform.

Specifically, we exploit cross-province differences in the share of couples eligible for the partial two-child policy. The reform relaxed birth quotas only for couples in which at least one spouse was an only child. Therefore, provinces with a larger proportion of eligible couples experienced greater exposure to the policy change.

We estimate an extended difference-in-differences specification that interacts policy exposure with exposure intensity:

$$Y_{s,t} = \boldsymbol{\alpha}^{'} I_s + \boldsymbol{\beta}^{'} I_t + \sum_r \gamma_{4r} I^r_{s,t} + \sum_r \gamma_{5r} \left(I^r_{s,t} \times Intensity_s \right) + \varepsilon_{s,t}, \tag{E.1}$$

where $Y_{s,t}$ denotes the outcome variable, including fertility outcomes, consumer prices, and housing price indices. $I^r_{s,t}$ indicates observations r months after the effective relaxation of birth quotas, following the treatment timing defined in the main analysis. $Intensity_s$ denotes the share of eligible couples in province s, where eligible couples are defined as couples in which at least one spouse is an only child. The coefficients γ_{5r} capture whether the dynamic effects of relaxing birth quotas vary with exposure intensity across provinces.

We estimate the dynamic effects using the same approach as in the main analysis. The results are presented in Figures E.6 and E.7. We find that the positive effect of relaxing birth quotas on second-child births is stronger in provinces with a higher proportion of eligible couples. In

contrast, the negative effect on first-child births becomes weaker as exposure intensity increases. These patterns are consistent with the interpretation that the reform primarily released previously constrained fertility demand while generating heterogeneous adjustments in entry into parenthood.

We further examine whether exposure intensity affects responses in child-rearing costs. The results show that increases in consumer prices and housing prices following the reform are larger in provinces with a higher proportion of eligible couples. These patterns are consistent with stronger adjustments in the economic environment surrounding child-rearing in provinces with greater exposure to the reform.

A potential concern is that the share of eligible couples may capture differences in provincial demographic structures rather than policy exposure itself. To address this concern, we replace the share of eligible couples with the absolute number of eligible couples as an alternative measure of exposure intensity. The results remain qualitatively unchanged, as shown in Figure E.8.

Overall, these additional analyses show that the estimated effects vary systematically with the intensity of exposure to the reform. The results provide further evidence that the fertility and cost responses are closely related to the degree of exposure to the relaxation of institutional fertility constraints.

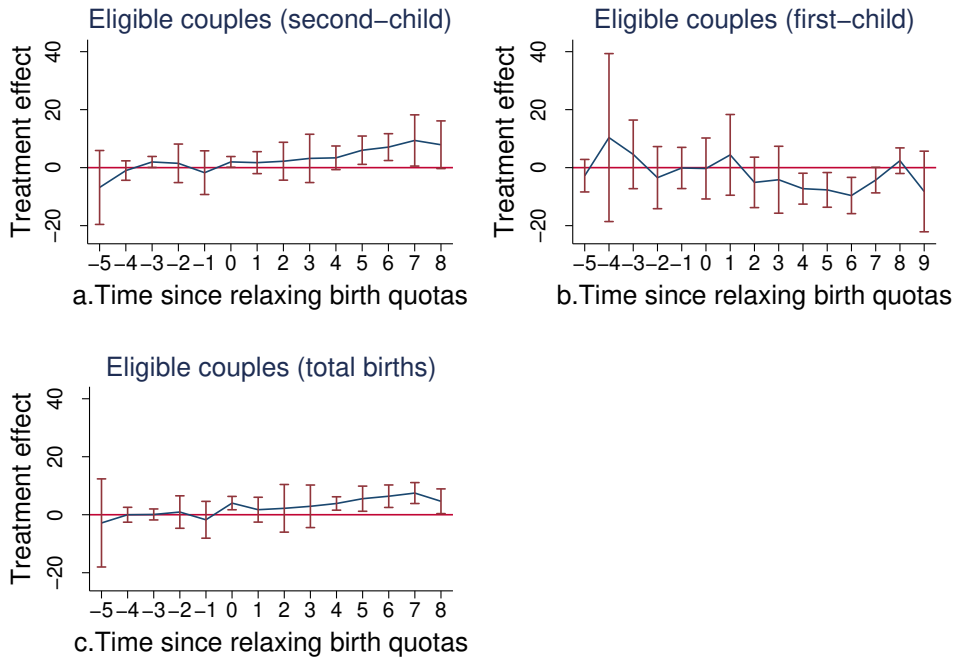


Figure E.6: Effects of eligible couples on the effects of relaxing birth quotas.

Notes: This graph shows how the proportion of eligible couples affects the impact of relaxing birth quotas on (a) second-child births, (b) first-child births and (c) total births. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Eligible couples are defined as couples in which at least one of the marital partners is an only child. Both first-child, second-child and total births variables are in the logarithm form. The panel data on first-child,second-child and total births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

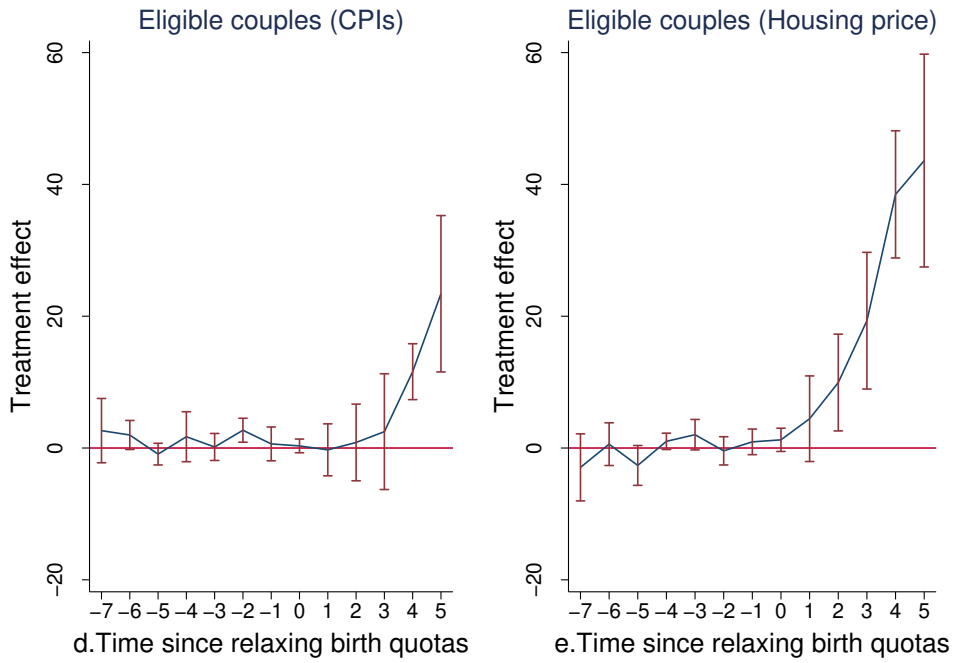


Figure E.7: Effects of eligible couples on the effects of relaxing birth quotas (cont.).

Notes: This graph shows how the proportion of eligible couples affects the impact of relaxing birth quotas on (a) CPIs and (b) housing price indices. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Eligible couples are defined as couples in which at least one of the marital partners is an only child. The panel data on CPIs and housing price indices from August 2014 to July 2015 come from China's National Bureau of Statistics. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

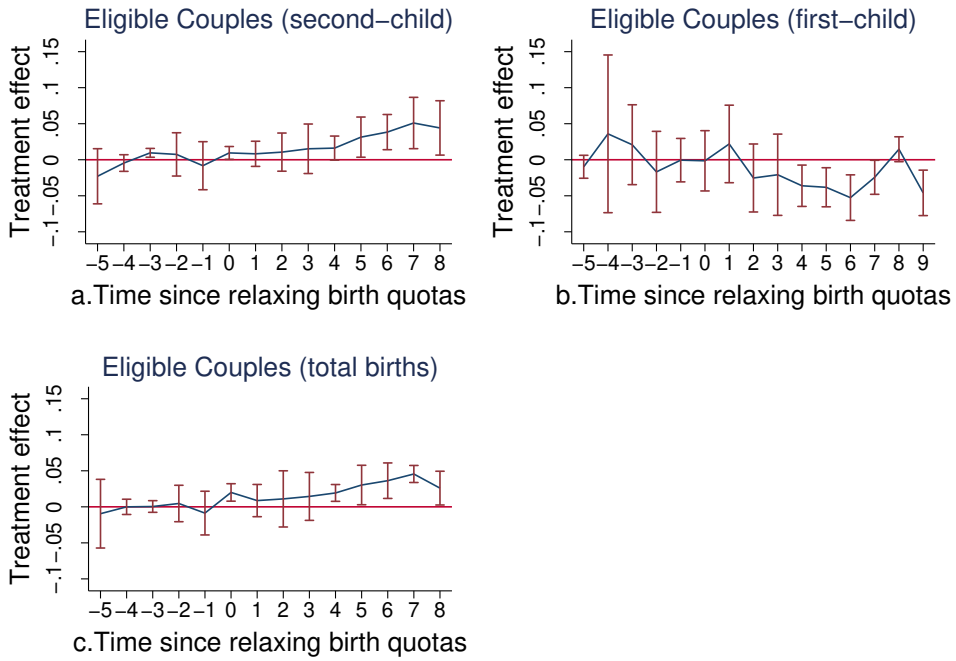


Figure E.8: Effects of eligible couples (in absolute number) on the effects of relaxing birth quotas.

Notes: This graph shows how the absolute number of eligible couples affects the impact of relaxing birth quotas on (a) second-child births, (b) first-child births and (c) total births. In each panel, the horizontal axis represents the values corresponding to the time relative to the (de facto) relaxation of birth quotas, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Eligible couples are defined as couples in which at least one of the marital partners is an only child. Both first-child, second-child , total births and eligible couple variables are in the logarithm form. The panel data on first-child,second-child and total births from November 2014 to October 2015 are constructed using the 2015 census. To draw these figures, we followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).