Fertility, Child Gender, and Parental Migration Decision: Evidence from China*

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Abstract

We study how the number and gender of children affect rural-to-urban parental migration in China. Exploiting variation from the "1.5-child policy," which allowed a second birth only if the firstborn was a girl, we separately identify the causal effects of fertility and child gender based on a novel semi-parametric method. Using the 2010 population census, we find that an additional child increases the likelihood of joint parental migration by 12.1 percent, while the presence of a son raises it by a further 11.6 percent. These comparable magnitudes show that gender matters as much as fertility in shaping migration decisions. The results suggest that the higher financial demands of larger families—especially those with boys—outweigh the emotional costs of separation, motivating parents to migrate. Traditional IV estimates would be severely downward biased if only the number of children were considered.

Keywords: Parental migration, Fertility, Child gender, One-child policy, Semi-parametric model

JEL Codes: J61, J13, C14

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1 Introduction

A large body of research has shown that both the number and gender of children shape a wide range of household decisions. Fertility and child gender influence parental labor supply (Angrist and Evans, 1998; Adda, Dustmann, and Stevens, 2017), marital stability (Bedard and Deschenes, 2005; Cáceres-Delpiano, 2006; Jena, 2006; Lundberg, McLanahan, and Rose, 2007; Dahl and Moretti, 2008; Genicot and Hernandez-de Benito, 2025), household savings and consumption (Wei and Zhang, 2011; Oliveira, 2016; Choukhmane, Coeurdacier, and Jin, 2023), and investments in children's education (Becker, 1993; Rosenzweig and Zhang, 2009; Barcellos, Carvalho, and Lleras-Muney, 2014). However, evidence on how children affect parental migration remains limited.

This issue is particularly relevant in countries experiencing rapid urbanization (Lucas, 1997; Selod and Shilpi, 2021). In China, migration has been especially extensive: in 2020, roughly 376 million people were migrants.[1] Unlike standard labor supply decisions, migration involves a distinct trade-off: parents must weigh potential income gains against the cost of leaving their children behind, which arises from high childcare costs and limited access to public education in urban areas (Zhang et al., 2014; Cortes, 2015). In 2015, the number of left-behind children in China was estimated at around 70 million (Huang and Zhang, 2022), a situation associated with higher dropout rates, lower academic achievement, and poorer physical and mental health (Antman, 2012b; Lu, 2014; Zhao et al., 2014; Fellmeth et al., 2018). Understanding how fertility and child gender shape parental migration choices is therefore crucial for analyzing household labor allocation, child well-being, and structural transformation in developing countries.

In this paper, we separately estimate the effects of fertility and child gender using a natural experiment from China's One-child Policy (OCP). Since the 1980s, the massive rural-to-urban migration in China has created a unique setting for studying migration decisions. Meanwhile, in

[1] Main Data of the Seventh National Population Census, https://www.stats.gov.cn/zt_18555/zdtjgz/zgrkpc/dqcrkpc/ggl/202302/t20230215_1903996.html

most rural areas, the OCP took the form of a "1.5-child policy," which allows a second child only if the firstborn is a girl, making fertility highly dependent on the gender of the first child. If both fertility and child gender can influence parental migration, it is essential to study their effects separately. Existing research has typically examined only one dimension at a time: Zhang (2017) investigates the effect of fertility on rural-to-urban migration in China, whereas Li and Yi (2015) focuses on child gender. Since fertility and child gender are endogenously correlated, these estimates may reflect a combination of both effects rather than a clean measure of either. Generally, using OCP-related single IV (Qian, 2009; Huang, Lei, and Sun, 2021) can lead to a violation of the exclusion restriction because OCP exacerbated gender selection and changed the number and the gender of children at the same time (Ebenstein, 2010; Jiang and Zhang, 2021).

In this study, we first motivate our analysis using a simple conceptual model in which parents weigh the income gains from migration against the emotional and logistical costs of leaving their hometown and children. Fertility affects both sides of this trade-off: having more children increases household expenses, which may push parents to migrate for higher wages (Du, Park, and Wang, 2005), while institutional constraints such as the Hukou system make it difficult to bring children to urban areas, creating separation costs that discourage migration (Song, 2014). The gender composition of children can further influence this decision. Sons often carry greater financial and emotional expectations, given traditional pressures for men to acquire housing and pay bride price before marriage, which may increase parents' incentive to migrate (Wei and Zhang, 2011; Li and Yi, 2015). At the same time, strong son preference can raise the emotional cost of separation, making it harder to leave sons behind. Thus, the net effect of child number and gender on parental migration is theoretically ambiguous and requires empirical evaluation. This model does not impose any policy restriction background and can be used in countries other than China.

In the second step, we empirically investigate the impact of the number and gender of children on parental migration. To address the endogeneity of both the number and gender of children, we propose a novel semi-parametric method. Leveraging the exogenous change in fertility induced by the 1.5-child policy—where the gender of the first child determined whether a second child could be legally born—we focus on the margin from the first to the second birth. This margin is highly representative, as families with one or two children account for nearly 90% of households in the age cohorts we study. Using China's 2010 population census, we find that an additional girl increases the probability that both parents migrate by 12.1% (1.17 percentage points), while having a boy rather than a girl will have an additional effect of 11.6% (1.13 percentage points). These results indicate that the magnitudes of the effects of fertility and child gender are comparable. Intuitively, the findings are consistent with a scenario in which the positive income effect of having more children and boys outweighs the negative separation cost, motivating parents to migrate.

As we have argued, when there are two endogenous variables—the number of children and their gender— simple off-the-shelf methods that rely on a single OCP-related IV are insufficient to clearly identify either causal effect. To separately identify the effects of fertility and child gender on parental migration, we develop a semi-parametric method that exploits China's "1.5-child policy" as a natural experiment. Under this policy, rural couples are permitted to have a second child only if their firstborn is a girl, creating quasi-random variation in fertility. This provides our first source of exogenous variation. To address the second endogenous variable—child gender—we further exploit variation in the gender of the second child. While second-child gender may be subject to selection due to sex-selective practices at higher birth orders, we mitigate this concern by adopting a weakened "selection-on-observables" assumption: conditional on observed household characteristics and latent fertility types, second-child gender is assumed exogenous. Following Angrist, Imbens, and Rubin (1996) and Kline and Walters

(2016), we classify households into three behavioral types—always-takers, never-takers, and compliers—based on their fertility responses to the gender of the first child. We then semi-parametrically estimate the local average treatment effects (LATE) of both the number and gender of children within each type. Specifically, we identify the fertility effect among compliers by comparing the expected migration outcomes of compliers who have only a firstborn boy with those who have a firstborn girl followed by a second-born boy. We further identify the child gender effect, defined as the differential impact of having a boy versus a girl on parental migration decisions within behavioral types, holding the number of children constant.

Our study extends the current literature in several ways. First, we contribute to the literature on the determinants of household migration by providing the first causal estimates of both fertility and child gender effects on parental migration decisions. Prior research has largely focused on socioeconomic determinants of migration, including wage differentials, labor skills, employment opportunities, risk attitudes, information availability, social ties, and cultural or language barriers (Schlottmann and Herzog, 1981; Dustmann, 2003; Korinek, Entwisle, and Jampaklay, 2005; Jaeger et al., 2010; Belot and Ederveen, 2012; Dustmann et al., 2023). More recent work examines demographic factors, with particular attention to how sibling structure influences adult children's migration in the context of elder care arrangements (Antman, 2012a; Stöhr, 2015; Zhao and Zhong, 2019). Another emerging line of research highlights the "move-with-children" margin, where parents relocate to higher-opportunity neighborhoods to enhance children's long-run outcomes (Chetty and Hendren, 2018). Our setting further features "migration without children": due to institutional constraints and high urban childrearing costs, rural parents often leave children behind and migrate primarily for income. While a few studies have examined the role of children in shaping migration outcomes, they typically consider either fertility or child gender in isolation. Our findings show that ignoring the interaction between the two can lead to biased conclusions.

Second, we contribute to the literature on the effects of children on parental labor supply and the left-behind children issue. Existing studies in this area mainly emphasize the motherhood penalty—showing how children reduce female labor force participation and earnings (Kleven, Landais, and Søgaard, 2019; Kleven, Landais, and Leite-Mariante, 2024). In contrast, our study highlights a different mechanism through which children affect parents' labor supply—namely, the decision to migrate to urban areas for work. This type of labor allocation involves prolonged physical separation from children and therefore requires parents to weigh income gains against substantial separation costs. Our analysis thus bridges the labor supply literature and the literature on left-behind children by demonstrating how concerns over child well-being shape rural parents' migration decisions, thereby influencing China's structural transformation (Huang and Zhang, 2022; Sieg, Yoon, and Zhang, 2023; Du, 2024; Schroeder and Zhang, 2025).

Third, we contribute to the growing literature on the consequences of the OCP (Zhang, 2017). A core difficulty in this literature is that both the number and gender composition of children are endogenous. In the Chinese context, most empirical studies rely on exogenous variation generated by the OCP, but OCP-related instruments—such as first-child gender or local enforcement intensity—tend to affect both fertility and child gender simultaneously (Qian, 2009; Huang, Lei, and Sun, 2021; Li and Yi, 2015). This violates the exclusion restriction and makes it difficult to isolate either effect. Our paper proposes a novel semi-parametric identification strategy that explicitly addresses the joint endogeneity of fertility and child gender, which has received limited attention in the OCP literature. While we apply this method to parental migration, our approach can readily be extended to other OCP-related outcomes such as parental labor force participation, educational investment, or marriage stability. More broadly, this framework can be applied to other low-fertility settings characterized by strong son preference or restrictive fertility policies, offering a new lens for separately estimating the effects of child number and gender on household decisions.

2 Background and Data

2.1 "Hukou" System and China's Population Policy

In China, all households must register in the national-wide household registration system called Hukou, according to their place of origin. There are two categories of Hukou, rural and urban. Social welfare systems, such as the education system and the public medical insurance system, are separated for people with different Hukou types. Therefore, sending children without local urban Hukou to public schools in cities is very difficult. Many rural migrant workers are forced to leave their children behind at home with their grandparents. In China, people call it "Left-behind children problem." In this situation, children can be an essential factor affecting rural parents' migration decisions. We only consider rural-to-urban migration in this study.

China began its birth control efforts in the early 1970s with a voluntary program known as "later, longer, and fewer." In 1979, it formally introduced the mandatory OCP. At first, almost all families in China were allowed only one child, and the second or higher-parity births were penalized.[2] As shown in Figure 1, China's birth rate declined sharply following the implementation of the OCP. Figure 2 further illustrates the pronounced gender imbalance among newborns, driven by son preference and gender selection after the OCP. During the mid-1980s, a few provinces in China slightly relaxed the OCP. Several special situations were exempted from punishment for the second birth. One of the critical and prevailing exemptions was that families in rural areas with a first-born girl were allowed to give birth to a second child (if the first child was a boy, the second birth would still incur punishment). The exemption was a response and a compromise to the protest of peasant families who needed male labor force to farm the land. This policy is named "1.5-child policy." Afterward, the fertility policy remained relatively stable and was rigorously implemented nationwide until the relaxation of the OCP,

[2] To guarantee the efficiency of the implementation, fertility control achievement was put into the list of local officers' evaluation sheet, which would be an important reference for the promotion. The punishment for the violation includes a pecuniary fine, a reduction in social security, and some extreme cases, forced abortion and sterilization. For more details, see Zhang (2017).

with the selective two-child policy introduced in 2014 and the universal two-child policy in 2016. Figure 3 illustrates the timeline of fertility control policies in China. Since our analysis is based on the 2010 census, the children in our sample were born during a period when the OCP was strictly enforced and relatively stable, with no major policy adjustments.

The 1.5-child policy was implemented in the majority of the rural areas in China. Of the 31 provinces in mainland China, 25 provinces offered this exemption to at least some of the rural population in their provinces, and 19 offered this exemption to all the rural population in their provinces (Gu et al., 2007). Table 1 lists the provinces and the corresponding exemptions. The 19 provinces that offered the exemption to all the rural people account for 71.3% of the total GDP and 74.9% of the total population in mainland China. Figure 4 depicts the geographic distribution of provinces with different policies. The figure clearly shows that this exemption covers the majority of the populated and economically active parts of the country.

2.2 Data

Data Source Our data are from the 0.35% sample of the 2010 Chinese Population Census. Based on a household-level survey, the census provides rich information on the surveyed households and the individuals in the household. At the household level, the census provides basic demographic information, such as the address, the number of people registered in the household, and the living conditions. At the individual level, the census contains information on individuals' age, sex, the region of residence, education, relationship to household head, and working status. We explain the reason of using the census data in more details in Appendix A.

Sample Construction To identify the fertility effect and gender effect of children on the rural-to-urban parental migration, we first restrict our sample to households that consist of a household head and his/her spouse who are both holding rural Hukou. Second, we confine the sample to households in provinces that offer 1.5-child policy to all rural citizens to utilize the exogenous variation of fertility induced by the policy. Third, we retain only the households with the household head and his/her spouse aged above 35—to select those who have completed their fertility, and below 65—to guarantee that both parents remain in the labor force. Fourth, we confine our sample to households in which all children are less than or equal to 18 years old. The existence of adult children is not imperative to our research because a grown-up child can independently earn money and support their parents. They will have quite a different implication concerning the effect on parents' migration decision compared with that of juvenile children. Fifth, we restrict our sample to households in which the number of children listed under the household hukou (not necessarily children who are currently physically residing in the same household at the time of the census survey) is equal to the number of children born by the mother to obtain an accurate account of children number and birth order.[3] Sixth, we exclude households in which at least one of the parents has been divorced and those with twins. Seventh, as the population policy is only applicable to Han Chinese but not to minorities, we further restrict our sample to Han Chinese.[4] After applying all these restrictions, our final sample consists of 65,195 households. Among them, 1.69% are childless, 33.24% have one child, 53.87% have two children, and the remaining 11.20% have three or more children. These statistics clearly show that the dominant margin of fertility variation lies between having one and two children, which makes this transition both empirically relevant and closely aligned with the 1.5-child policy. Moreover, the implications of children on parental migration decisions are substantially different for childless families. Our empirical analysis focuses exclusively on households with one or two children. We conduct several robustness checks by relaxing the sample restrictions in Appendix D.1, and find that the results remain robust.

Key Variables We use three dummy variables to measure rural-to-urban parental migration behavior. The first dummy equals one if both parents migrate and zero otherwise. The second dummy equals one if only the father migrates, and the third equals one if only the mother

[3]For more details, please refer to Appendix A.

[4]According to the census data in 2010, Han people consists of 91.5% of the whole population.

migrates. Migrants are defined as individuals who reside in a community different from their Hukou registration community. The detailed definition of migration is provided in Appendix D.1.4. Table 2 presents summary statistics of the main variables.

3 Conceptual Model

Before proceeding to the empirical analysis, we construct a simple conceptual model to illustrate how both the number of children and the number of boys may influence parental migration. This model motivates our empirical strategy and provides a framework to test several key assumptions in our research design.

For family i, they can choose between $j = \{\text{stay}, \text{mig}\}$, which results in different wages. We assume that the wage at the migration destination w_{mig}, is higher than the wage in the hometown w_{home}. We further assume that the utility function is additively separable, with a Type 1 Extreme Value distributed migration preference shock ε:

$$u_i(w_j, c_i, b_i, \tau_{ij}, \varepsilon_{ij}) = \tilde{u}_{ij}(w_j, c_i, b_i, \tau_{ij}) + \varepsilon_{ij}, \quad \varepsilon_{ij} \sim \text{T1EV} \tag{1}$$

Given the wages in different locations w_j, the number of children c_i, the number of boys b_i, and the physical migration cost τ_{ij} (which equals zero if j=stay), the probability of migration choices can be written as:

$$P_{ij} = \frac{exp(\tilde{u}_{ij})}{\sum_{j' \in J} exp(\tilde{u}_{ij'})} \tag{2}$$

A simple way to parametrize the model is to assume a linear utility function:

$$\begin{aligned} u_i(w_j, c_i, b_i, \tau_{ij}, \varepsilon_{ij}) =& w_j + \beta_1 n_i + \beta_2 b_i + \gamma_1 w_j n_i + \gamma_2 w_j b_i \\ & + \gamma_3 \mathbf{1}(j = \text{mig}) \cdot n_i + \gamma_4 \mathbf{1}(j = \text{mig}) \cdot b_i + \tau_{ij} + \varepsilon_{ij} \end{aligned}$$

The first three terms capture the direct effects of wages and children on parents' utility, while the next four terms represent the interaction effects of wages, children, and migration. In this specification, parents face a tradeoff between earning higher income through migration and bearing the separation costs associated with leaving their children behind. Specifically, γ_1 and γ_2 capture the income effects of children and boys on parental migration decisions: more children or boys may incentivize parents to migrate for higher earnings. In contrast, γ_3 and γ_4 capture the separation cost effects: more children or boys may increase the emotional costs experienced by parents and the developmental costs incurred by children, which arise from being left behind while their parents migrate. In this model, we assume that parents leave children in the hometown when they migrate. The issue of bringing children along will be analyzed in Section 6.3 and Appendix F.1.

With this simple theoretical model, we can derive the partial effects of the number of children and the number of boys on migration by taking the following derivatives:

$$\frac{\partial P_{i,\text{mig}}}{\partial n_i} = [\gamma_1(w_{\text{mig}} - w_{\text{home}}) + \gamma_3] \cdot P_{i,mig}(1 - P_{i,mig}) \tag{3}$$

$$\frac{\partial P_{i,\text{mig}}}{\partial b_i} = [\gamma_2(w_{\text{mig}} - w_{\text{home}}) + \gamma_4] \cdot P_{i,mig}(1 - P_{i,mig}) \tag{4}$$

The terms $[\gamma_1(w_{\text{mig}} - w_{\text{home}}) + \gamma_3]$ and $[\gamma_2(w_{\text{mig}} - w_{\text{home}}) + \gamma_4]$ are the partial effects of children and boys on migration. β_1 and β_2 are not included because they do not vary with migration choices and cancel out in the calculation. We refer to $\gamma_1(w_{\text{mig}} - w_{\text{home}})$ and $\gamma_2(w_{\text{mig}} - w_{\text{home}})$ as the income effects, and to γ_3 and γ_4 as the separation effects. We assume $\gamma_1, \gamma_2 > 0$ and $\gamma_3, \gamma_4 < 0$. Thus, if the wage difference between the migration destination and home is sufficiently large, or if parents place more weight on wages (γ_1, γ_2) than on separation costs (γ_3, γ_4), they are more likely to migrate when they have more children or boys. We summarize the theoretical prediction for the effects of children and boys on parental migration in the following proposition:

Proposition 1 *Assume that $\gamma_1, \gamma_2 > 0$ and $\gamma_3, \gamma_4 < 0$, then the number of children has a positive effect on the probability of parental migration if:*

$$\gamma_1(w_{mig} - w_{home}) + \gamma_3 > 0 \tag{5}$$

Similarly, the number of boys has a positive effect on the probability of parental migration if:

$$\gamma_2(w_{mig} - w_{home}) + \gamma_4 > 0 \tag{6}$$

In general, the effects of the number of children and the number of boys on parental migration depend on the relative importance of wage gains and separation costs. In Appendix Section F.1, we extend the model by allowing for children migrating with parents and for father-mother separation. Despite these extensions, the fundamental tradeoff between income and separation costs remains unchanged. In the following sections, we empirically investigate this tradeoff using available data.

4 Empirical Strategy

4.1 Simple IV Identification of the Effect of Number of Children

Our empirical study aims to identify the effects of the number and the gender of children on parental migration decisions. In the first step, we only consider the effect of the number of children. We apply a common IV approach—using first-child gender as an instrument for fertility, while initially assuming that child gender does not directly affect migration. We will later show that the method does not work in the situation when the effect of the child gender cannot be ignored and then propose a new semi-parametric method.

Specifically, we have the following regression:

$$Mig_{il} = \beta_0 + \beta_1 c_{il} + \boldsymbol{X}_{il}\beta_2 + u_{il}, \tag{7}$$

where Mig_{il} is a dummy variable of whether the parents in household i with home location l choose to migrate. c_{il} is the number of children in household i with home location l. $\boldsymbol{X}_{il}$ denotes a vector of observed controls, including the first child's age, timing of marriage, parental education levels, parental ages, and a set of fixed effects for parental hukou location, parental birthplace, and survey location. u_{il} is the unobserved term.

To identify β_1, we try to address the endogeneity of fertility by exploiting the 1.5-child policy in rural areas, which allows the first-born child's gender to serve as an instrument for fertility. Let z_{il} be a dummy variable equal to one if the first child in household i is girl and zero otherwise. We use z_{il} as an instrument for c_{il}. The key assumption is the absence of gender selection at the first birth, which we argue holds for rural families in provinces subject to the 1.5-child policy. We formally test this assumption in Section 4.6.3.

Table 3 confirms a strong first stage for the IV estimation: having a first-born girl increases the number of children by approximately 0.35 compared to having a first-born boy. Table 4 presents the IV estimation results from a linear probability model, showing that the effect of the number of children on both parents' migration and male parent migration is small and statistically insignificant, while the effect on female parent migration is negative and statistically significant at the 10 percent level. According to the IV estimates, the causal link between fertility and migration appears weak.

Nevertheless, caution is needed when using the simple IV identification strategy. If child gender directly affects parental migration decisions, the exclusion restriction is violated, and the IV estimate of the fertility effect will likely be biased by the child gender effect. Simply controlling for child gender—such as the number of boys—in the regression does not solve this problem, because the number of boys is itself endogenous. First, gender-based abortion occurs at the second or higher-order births. More importantly, the number of boys is a post-treatment variable, creating a bad control problem: although the first child's gender is random unconditionally, the number of boys is determined after this randomization. Conditioning on the number of boys thus breaks the randomness of the first child's gender, contaminating the estimation. Detailed analysis and results are provided in Appendix B.

4.2 Semi-parametric Identification Strategy

In this section, we propose a new semi-parametric model to address the endogeneity of fertility and child gender, and separately identify the two effects in a heterogeneous treatment effect framework.

Our model is an extension of Angrist, Imbens, and Rubin (1996) and Kline and Walters (2016)'s idea of treatment effects by behavioral types. The exogenous policy shock we employ is China's 1.5-child policy. Under this fertility policy, the gender of the first child can be considered as random, and the treatment on the second child enables us to classify households into three fertility behavioral types, namely, always-takers, compliers, and never-takers. We assume that the gender of the second child is also random conditional on some observables and the fertility behavioral types. Therefore, households in each type are (conditionally) identically distributed across different numbers of children and boys. Then we can directly compare households in the same fertility behavioral type with different numbers of children and boys.

We aim to identify the LATE of an additional girl on parents' migration decision for compliers, which we define as the effect of the number of children (the effect of one more child when we keep the number of boys constant). Moreover, we attempt to estimate the differential effect of one boy as opposed to one girl on parents' migration decision for always-takers, compliers, and never-takers, respectively, which we define as the effects of the gender of children (the effect of one boy compared with one girl when we keep the number of children constant).

4.3 Semi-parametric Model Setup

We consider a general migration decision equation as follows:

$$Mig_i = f(c_i, b_i, X_i, u_i), \tag{8}$$

and we have a first-stage fertility equation as follows:

$$c_i = g(z_i, X_i, e_i). \tag{9}$$

e_i is the unobserved term in the fertility decision equation, such as unobserved fertility preference and ability to deal with government punishment, and so on. z_i is the gender of the first child, which equals one if it is a girl. b_i is the number of boys in household i. The definition of other variables remains the same as in the simple IV estimation.

Based on this model setup, we can explicitly explain the failure of the simple IV identification. First, the unobserved term in migration decision u_i and the unobserved term in fertility decision e_i may be correlated with each other, which is the source of endogeneity of the number of children c_i. As there is no gender selection at the first birth, u_i and e_i are independent of the first child gender z_i. Hence, previous studies use z_i as an IV for c_i. However, b_i is correlated with z_i and Mig_i, so the IV estimate is biased if b_i is omitted from the regression. Moreover, this problem cannot be addressed by simply controlling for b_i, as b_i itself is endogenous and z_i is correlated with u_i conditional on b_i. The endogeneity of b_i comes from two sources. On the one hand, there may be gender selection at the second birth. On the other hand, b_i is a post-determined variable. We need to come up with a new strategy to solve this problem.

Following Angrist, Imbens, and Rubin (1996), we define three behavioral types in our model: always-takers, those who always choose to give birth to the second child regardless of the gender of their first child, i.e., $c_i(z_i = 1) = c_i(z_i = 0) = 2$; never-takers, those who always choose not to give birth to the second child regardless of the gender of their first child, i.e., $c_i(z_i = 1) = c_i(z_i = 0) = 1$; compliers, those who choose to give birth to the second child if the first is a girl but do not if the first is a boy, i.e., $c_i(z_i = 1) = 2, c_i(z_i = 0) = 1$.[5] Factors such as the household's fertility preference (observed factors in X_i and unobserved factors e_i) and social capital (e.g., ability to bear government punishment) will determine its type.

[5]We assume that there are no defiers in our model. In other words, no household would choose to have a second child if the first child is a boy but refrain from doing so if the first child is a girl.

As Pinto (2015) points out, the number of possible response types will grow exponentially if we expand the birth tree, which will prevent us from a meaningful identification of treatment effects for various types. Therefore, we decide to drop all households with more than two children. Another reason for us to do this is that families giving birth to more than two children are highly likely to be qualified for other local specific exemptions or even have illegal channels to avoid punishment. This small group of people may behave differently from the always-takers and compliers as defined in our setting.

For convenience, we denote Mig_{icb} as the potential migration decision for household i when $c_i = c$ and $b_i = b$. That is, $Mig_{icb} = f(c_i = c, b_i = b, X_i, u_i)$. It refers to the migration decision that the household would make if they had c children and b boys, regardless of how many children/boys they have in reality. For example, Mig_{i10} represents the potential migration decision for household i, if they had one child and zero boy. Moreover, we denote the subscripts A, N, and C as always-takers, never-takers, and compliers, respectively, and denote n_A, n_C, and n_N as the number of always-takers, compliers, and never-takers in the population, respectively.

Our model aims to identify the LATEs of the number of children and the gender of children. First, we attempt to identify the treatment effect of an additional child on parental migration decision, which is the LATE of the number of children, denoted as $FLATE$. As only compliers change their fertility choice when being treated, $FLATE$ can be identified only for compliers. Specifically, we take an additional girl as the baseline, so $FLATE$ is defined as the difference between the potential outcomes of the compliers with a first girl and a second boy and the compliers with only a first boy.

$$FLATE = E[Mig_{i21}|C] - E[Mig_{i11}|C]. \tag{10}$$

Second, we aim to identify the gender heterogeneity of the treatment effects between boys and girls on parental migration, which is the LATE of the gender of children, denoted as $BLATE$. We identify $BLATE$ for always-takers, compliers, and never-takers, respectively. Taking compliers as an example, we define $BLATE_C$ as the difference between the potential outcomes of the compliers with a first girl and a second boy and the compliers with a first girl and a second girl.

$$BLATE_{A1} = E[Mig_{i22}|A] - E[Mig_{i21}|A], \tag{11}$$

$$BLATE_{A2} = E[Mig_{i21}|A] - E[Mig_{i20}|A], \tag{12}$$

$$BLATE_C = E[Mig_{i21}|C] - E[Mig_{i20}|C], \tag{13}$$

$$BLATE_N = E[Mig_{i11}|N] - E[Mig_{i10}|N]. \tag{14}$$

We can also derive an average $BLATE$ by integrating all the $BLATEs$ over types using the number of households in each type as weights.

4.4 Identification without Gender Selection

In this subsection, we discuss how to separately identify the two effects when there is no gender selection at any birth order. We extend the model to a case with gender selection in Section 4.5.

Figure 5 shows that a set of values for vector (z,b,c) will determine a node on the birth tree. In other words, a specific node on the tree refers to a group of households with the same number of children, number of boys, and gender of the first child. When there is no gender selection at any birth order, and households are randomly assigned with a boy or a girl with the probability of $\frac{1}{2}$ at each birth, the distributions of different types of households are shown in Figure 5 and Table 5. Taking households with a first-born boy as an example, never-takers and compliers will stop giving birth to the second child, but always-takers will continue to give birth to the second child. Always-takers with a first-born boy are randomly assigned with a second-born boy or a second-born girl. The same logic can be applied to households with a first-born girl.

We obtain Equation (15) according to the number of households in different cells in Table 5.

$$\begin{cases} n(z=1,b=0,c=1) = \frac{1}{2}n_N \\ n(z=0,b=1,c=1) = \frac{1}{2}n_N + \frac{1}{2}n_C \\ n(z=0,b=1,c=2) = \frac{1}{4}n_A \end{cases} \tag{15}$$

For Equation (15), the number of households at each node is a summation of the number of households in different types at that node.[6] Then, we can further identify the probability distribution of any specific type of households at each node, i.e., $P(A|z_i,b_i,c_i)$, $P(N|z_i,b_i,c_i)$ and $P(C|z_i,b_i,c_i)$.

Owing to the randomness of child gender at each birth, each specific type of households is identically distributed at different nodes on the birth tree. That is, the distributions of X_i and u_i are identical across all nodes on the birth tree conditional on types. Therefore, we propose that, conditional on any specific type of household, the expectation of the potential migration decision remains the same across the gender of the first child z_i, the number of boys b_i, and the number of children c_i, i.e., $E[Mig_{icb}|z_i,b_i,c_i,Type_i] = E[Mig_{icb}|Type_i]$. For instance, $E[Mig_{i21}|z_i=1,b_i=1,c_i=2,A] = E[Mig_{i21}|z_i=1,b_i=0,c_i=2,A] = E[Mig_{i21}|A]$.

We can derive the expectation of potential migration decisions at each node as follows:

$$E[Mig_i|z_i=0,b_i=1,c_i=2] = E[Mig_{i21}|A] \tag{16}$$

$$E[Mig_i|z_i=0,b_i=2,c_i=2] = E[Mig_{i22}|A] \tag{17}$$

$$E[Mig_i|z_i=0,b_i=1,c_i=1] = P(N|z_i=0,b_i=1,c_i=1)E[Mig_{i11}|N] + P(C|z_i=0,b_i=1,c_i=1)E[Mig_{i11}|C] \tag{18}$$

[6]In this case, the number of households in each type is over-identified. Thus, we are able to use the information of some nodes, although not all of them, to identify n_N, n_A, and n_C. In our data set, the differences of the estimated n_N, n_A, and n_C across different choices of nodes are negligible.

$$E[Mig_i|z_i = 1, b_i = 0, c_i = 1] = E[Mig_{i10}|N] \quad (19)$$

$$E[Mig_i|z_i = 1, b_i = 0, c_i = 2] = P(A|z_i = 1, b_i = 0, c_i = 2)E[Mig_{i20}|A] \quad (20)$$
$$+ P(C|z_i = 1, b_i = 0, c_i = 2)E[Mig_{i20}|C]$$

$$E[Mig_i|z_i = 1, b_i = 1, c_i = 2] = P(A|z_i = 1, b_i = 1, c_i = 2)E[Mig_{i21}|A] \quad (21)$$
$$+ P(C|z_i = 1, b_i = 1, c_i = 2)E[Mig_{i21}|C]$$

The conditional expectation at each node is a weighted sum of the conditional expectations of potential outcomes for various types of households at that node. All expectations on the left-hand side can be derived from the data, and all conditional probabilities on the right-hand side can be identified using the number of households in each type.

Therefore, we can identify our targeted $FLATE$ and $BLATE$ by solving the system of Equations (16)-(21). In order to solve the equation system, we need to make two assumptions

Assumption 1 *For the always-taker group, the expectation of the boy's partial effect on migration decision remains the same for from no boy to one boy, and from one boy to two boys. That is,* $E[Mig_{i22}|A] - E[Mig_{i21}|A] = E[Mig_{i21}|A] - E[Mig_{i20}|A]$.

This is a mild assumption, because it only postulates that b_i enters into the migration decision function linearly for always-takers. Hence, $BLATE_{A_1} = BLATE_{A_2}$.

Assumption 2 *The expectation of the boy's partial effect on migration decision remains the same across the never-taker and always-taker groups. That is,* $E[Mig_{11}|N] - E[Mig_{10}|N] = E[Mig_{22}|A] - E[Mig_{21}|A]$.

The economic intuition of this assumption is that always-takers and never-takers have the same attitude toward boys concerning migration decision. This assumption is reasonable because the difference between never-takers and always-takers is more likely to lie on fertility preference than on boy preference. Therefore, boys are likely to have similar effects on parental migration decision. However, it is possible that migration preference, gender preference, and fertility preference are correlated. Therefore, we further test these two assumptions in detail in Section 4.6. Using our data and the conceptual model introduced in Section 3, we validate Assumptions 1 and 2 and find that they are reasonable within the context of our analysis. One of the special cases satisfying Assumptions 1 and 2 is that the migration decision is a linear function regarding the number of boys, which is the underlying assumption of all previous studies on OCP. However, it is not necessary to impose such a restrictive condition in our method.

To simplify the notation, we denote the conditional expectation of migration decision at each node (z,b,c), $E[Mig_i|z_i = z, b_i = b, c_i = c]$ as y_{zbc}. After solving the system of Equations (16)-(21), we can identify all the aforementioned treatment effects with the following propositions.[7]

Proposition 2 *If Assumption 1 and Assumption 2 hold, we can identify the Local Average Treatment Effect of an additional child as:*

$$FLATE = \frac{(n_A + n_C)y_{112} - (n_A + n_N)y_{012} - (n_N + n_C)y_{011} + n_N(y_{022} + y_{101})}{n_C} \tag{22}$$

Proposition 3 *We can identify the gender heterogeneity of the Local Average Treatment Effect between boys and girls, for different types of households as follows:*

(1) $BLATE_A = y_{022} - y_{012}$ *for always-takers*

(2) If Assumption 1 holds,

$BLATE_C = \frac{(n_A+n_C)(y_{112}-y_{102})-n_A(y_{022}-y_{012})}{n_C}$ *for compliers*

(3) If both Assumption 1 and Assumption 2 hold, $BLATE_N = BLATE_A = y_{022} - y_{012}$ *for never-takers.*

[7] The proof is shown in Appendix C.2.

4.5 Identification with Gender Selection

4.5.1 Setup with Gender Selection

The previous subsection shows the identification of treatment effects without considering gender selection at both the first and the second births. However, this is not the case in China, where, according to empirical evidence, gender-based abortion is a severe problem at second or higher-order births. In this section, we construct a general identification strategy based on the previous one but taking gender selection at the second birth into account.

At the first birth, all the types of households do not implement gender selection, and the probability of having a first-born boy is equal to P_0, a natural rate of birth gender ratio which can be derived from the data. At the second birth, we assume that compliers and always-takers choose to carry out gender selection because of gender preference. However, the selection process is imperfect due to the limited access to detection/selection technology or government supervision. The gender of the second birth is determined by the following function:

$$\mathbf{1}(Boy)_i = h(X_i, \eta_i), \tag{23}$$

where the indicator function on the left-hand side equals one if the second birth of household i is a boy. On the right-hand side, η is a vector of unobserved factors, such as the luck of not being spotted by the government.

We call the realized distorted gender ratio at birth as the "success rate", which can vary across types. We define the average probability of getting a second-born boy for always-takers as P_{A_0} when $z = 0$ and P_{A_1} when $z = 1$, and the average probability of getting a second-born boy for compliers as P_C. The three probabilities remain unknown because we cannot observe the illegal gender selection process.

The distribution of different types of households is shown in Figure 6 and Table 6. At the first birth, the assignment of birth gender remains random with the natural rate of P_0. Therefore, a proportion of $1-P_0$ always-takers, never-takers, and compliers have a first-born girl. Never-takers comply with one girl, while always-takers and compliers choose to give birth to a second child. At the second birth, the existence of gender selection distorts the gender ratio. Hence, $(1-P_0)P_{A_1}$ of always-takers have a first girl and a second boy, and the remaining $(1-P_0)(1-P_{A_1})$ of always-takers have a first-born girl and a second-born girl. Similarly, $(1-P_0)P_C$ of compliers have a first-born girl and a second-born boy, and the remaining $(1-P_0)(1-P_C)$ of compliers have a first-born girl and a second-born girl. The same logic can be applied to the households with a first-born boy.

Then, we can obtain Equation (24) according to the number of households in different cells in Table 6 with gender selection at the second birth.

$$\begin{cases} n(z=1,b=0,c=1)=(1-P_0)n_N \\ n(z=0,b=1,c=1)=P_0n_N+P_0n_C \\ n(z=0,b=1,c=2)=P_0(1-P_{A_0})n_A \\ n(z=0,b=2,c=2)=P_0P_{A_0}n_A \\ n(z=1,b=0,c=2)=(1-P_0)(1-P_{A_1})n_A+(1-P_0)(1-P_C)n_C \\ n(z=1,b=1,c=2)=(1-P_0)P_{A_1}n_A+(1-P_0)P_Cn_C \end{cases} \tag{24}$$

4.5.2 Assumption for Gender Selection

Owing to the endogeneity of the gender selection process, the distributions of X_i and u_i are no longer identical across all nodes on the birth tree conditional on a specific type. Therefore, we do not have $E[Mig_{icb}|z_i,b_i,c_i,Type_i]=E[Mig_{icb}|Type_i]$ at nodes with gender selection. For example, $E[Mig_{i21}|z_i=1,b_i=1,c_i=2,A]\neq E[Mig_{i21}|A]$, as always-takers with a higher preference for boys are more likely to sort into the node with a first-born girl and a second boy $(z_i=1,b_i=1,c_i=2)$. Consequently, the conditional expectations of the potential migration decisions for certain type of households at certain node is $E[Mig_{icb}|z_i, b_i, c_i, Type_i]$, rather than $E[Mig_{icb}|Type_i]$.

To solve this problem, we assume that there is no further selection for success rate within types after conditioning on observed controls X. After controlling for X, compliers (always-takers) who have a second boy and compliers (always-takers) who have a second girl are not systematically different in the unobserved term in the migration function (u_i). Specifically, the unobserved factors η_i in the gender selection equation are independent of the unobserved factors u_i in the migration equation within types. Then we have $E[Mig_{icb}|z_i = z, b_i = b, c_i = c, A, x_i] = E[Mig_{icb}|A, x_i]$ and $E[Mig_{icb}|z_i = z, b_i = b, c_i = c, C, x_i] = E[Mig_{icb}|C, x_i]$. This is a crucial assumption, which is similar to the "selection on observables" assumption in the standard matching method but in a weakened version because we also control for fertility behavioral types. Moreover, the data clearly show that the sex ratio of the second child for households with a first-born boy is very close to 1:1 (95.3:100). To simplify the model, we can safely assume that gender selection does not occur for always-takers at the second birth when the first child is a boy.

Assumption 3 *Always-takers do not implement gender selection when they have a first-born boy, that is, $P_{A_0} = P_0$. Compliers and always-takers may implement gender selection at the second birth if their first child is a girl. However after controlling for observables, conditional on types, their success of gender selection depends on exogenous factors that are independent of u_i, i.e., $\eta_i \perp u_i|A$ and $\eta_i \perp u_i|C$.*

This assumption is not as strong as one may think. The "selection on observables" is assumed within the households with the same fertility type. Therefore, to a certain extent, we simultaneously and implicitly control for the unobserved characteristics of households concerning fertility choices. Assumption 3 implies that the unobserved term of gender selection η_i and the unobserved term of migration u_i are independent after controlling for both observed X

and fertility behavioral types.

We then obtain a similar system of equations of conditional expectations as in the case without gender selection (Equations (16)-(21)).[8] In the equations below, all the probabilities and numbers of households are conditional on the fertility behavioral type and X. Subscripts A, C, and N represent that the variable is conditional on the corresponding fertility behavioral type, and subscript x represents that the variable is conditional on X.[9]

$$E[Mig_i|z_i=0,b_i=1,c_i=2,X] = E[Mig_{i21}|A,X] \tag{25}$$

$$E[Mig_i|z_i=0,b_i=2,c_i=2,X] = E[Mig_{i22}|A,X] \tag{26}$$

$$\begin{aligned} E[Mig_i|z_i=0,b_i=1,c_i=1,X] =& \frac{n_{Nx}}{n_{Nx}+n_{Cx}}E[Mig_{i11}|N,X] \\ &+\frac{n_{Cx}}{n_{Nx}+n_{Cx}}E[Mig_{i11}|C,X] \end{aligned} \tag{27}$$

$$E[Mig_i|z_i=1,b_i=0,c_i=1,X] = E[Mig_{i10}|N,X] \tag{28}$$

$$\begin{aligned} E[Mig_i|z_i=1,b_i=0,c_i=2,X] =& \frac{(1-P_{A_1x})n_A}{(1-P_{A_1x})n_{Ax}+(1-P_{Cx})n_{Cx}}E[Mig_{i20}|A,X] \\ &+\frac{(1-P_{Cx})n_{Cx}}{(1-P_{A_1x})n_{Ax}+(1-P_{Cx})n_C}E[Mig_{i20}|C,X] \end{aligned} \tag{29}$$

$$\begin{aligned} E[Mig_i|z_i=1,b_i=1,c_i=2,X] =& \frac{P_{A_1}n_{Ax}}{P_{A_{1x}}n_{Ax}+P_{Cx}n_{Cx}}E[Mig_{i21}|A,X] \\ &+\frac{P_{Cx}n_{Cx}}{P_{A_1x}n_{Ax}+P_{Cx}n_{Cx}}E[Mig_{i21}|C,X] \end{aligned} \tag{30}$$

4.5.3 Assumption for Identifying Gender Selection Probability

Another challenge in identification when incorporating gender selection is estimating P_{A_1x} and P_{Cx}—the probabilities that the second child is a boy for always-takers and compliers, respectively. To address this, we construct a prediction model for the probability that a household is an always-taker. We begin with households whose first child is a boy ($z=0$), where always-takers can be directly identified: those who have a second child are always-takers, while those who stop after the first child are not. This enables us to compare household characteristics between always-takers ($z=0, c=2$) and non-always-takers ($z=0, c=1$). Using these households

[8] We expand the conditional expectations of the potential migration decisions at each node (left-hand side) in terms of the conditional expectations of the potential migration decisions for different fertility behavioral types at that node (right-hand side) weighted by the probabilities of different types at that node.

[9] For example, $n_{Nx}=n(N,x)$, $n_{Cx}=n(C,x)$, and $\frac{n_{Nx}}{n_{Nx}+n_{Cx}}=P(N|z_i=0,b_i=1,c_i=1,X=x)$.

as the training dataset, we employ a machine learning algorithm to train a prediction model for whether a household is an always-taker conditional on observed predetermined features X_A (e.g., household characteristics, parents' age, education, etc.).[10] We then apply the trained model to households at nodes $(z=1, b=1, c=2)$ (households with a first girl and a second boy) and $(z=1, b=0, c=2)$ (households with two girls) to predict their probabilities of being always-takers. With these predictions, we can recover the distribution of always-takers, never-takers, and compliers across all nodes of the birth tree.

To guarantee that we can obtain a good prediction model and achieve the identification using observed X_A, we need to further make an assumption on X_A.

Assumption 4 *We can observe a set of predetermined features X_A such that b will not provide with more information about being an always-taker after conditioning on X_A for households with a first-born girl and two children. That is, $P(A|X_A, b, c=2, z=1) = P(A|X_A, z=0)$.*

Subsequently, we obtain P_{A_1x} by dividing the sum of the predicted probability as follows:[11]

$$P_{A_1x} = \frac{P(A, b=1|c=2, z=1, X)}{P(A, b=1|c=2, z=1, X) + P(A, b=0|c=2, z=1, X)}. \tag{31}$$

The intuition of this assumption is that X_A contains a sufficient set of variables to predict differences between the always-taker and the complier. Once we condition on X_A, b and c add no further information. Formally, a sufficient condition for Assumption 4 is $P(A|X_A, b, c, z) = P(A|X_A)$ where X_A is a sufficient statistic for predicting always-takers.

4.5.4 Point Identification under Assumptions 3 and 4

We are now identifying all the expectations and probabilities conditional on X. Therefore, in order to get the averages of the treatment effects within types, we need to identify the distribution of X conditional on types, that is, $P(X|Type)$. $P(X|A)$ and $P(X|N)$ are easy to

[10] In machine learning field, people usually use the term "features" or "predictors" to describe the independent variables in the model. If we need to predict a certain outcome Y with a set of inputs X_A, we call the data set used to estimate the prediction model as the training dataset. Moreover, X_A is a superset of X which includes all possible predetermined characteristics.

[11] The detailed analysis for the identification of P_{A_1} is shown in Appendix C.3.

identify because we have the gender of the first child z_i to be independent of X and fertility behavioral types, which leads to that $P(X|A) = P(X|A, z = 1) = P(X|z = 1, c = 2)$ and $P(X|N) = P(X|N, z = 0) = P(X|z = 0, c = 1)$. The intuition of these two equations is that always-takers and never-takers are randomly separated into families with a first-born girl or a first-born boy. All the families at node (z=1,b,c=2) are always-takers; and all the families at node (z=0,b,c=1) are never-takers. After $P(X|A)$ and $P(X|N)$ are derived, $P(X|C)$ can be identified as $P(x|C) = \frac{P(X)-P(X|A)P(A)-P(X|N)P(N)}{P(C)}$, where the unconditional type probabilities are $P(A) = \frac{n_A}{n}$, $P(N) = \frac{n_N}{n}$ and $P(C) = \frac{n_C}{n}$.

Thus, we can derive a point identification for each treatment effect presented above under Assumptions 1-4 when gender selection is permitted at the second birth.[12]

Proposition 4 *If Assumptions 1, 2, 3 and 4 hold simultaneously, we can identify the LATE of an additional child as:*

$$FLATE = \int_X \frac{(P_{A_1x}n_{Ax} + P_{Cx}n_{Cx})y_{112x} - (P_{A_1x}n_{Ax} + P_{Cx}n_{Nx})y_{012x} - P_{Cx}(n_{Nx} + n_{Cx})y_{011x} + P_{Cx}n_{Nx}(y_{022x} + y_{101x})}{P_{Cx}n_{Cx}} P(X|C)dx \quad (32)$$

Proposition 5 *We can identify the gender heterogeneity of the LATE between boys and girls, for different types of households as follows:*

(1) $BLATE_A = \int_X (y_{022x} - y_{012x})P(X|A)dx$ *for always-takers*

(2) If Assumption 1, 3 and 4 hold,

$$BLATE_C = \int_X \left[\frac{y_{112x}(P_{A_1x}n_{Ax}+P_{Cx}n_{Cx})-P_{A_1x}n_{Ax}y_{012x}}{P_{Cx}n_{Cx}} - \frac{[(1-P_{A_1x})n_{Ax}+(1-P_{Cx})n_{Cx}]y_{102x}-(1-P_{A_1x})n_{Ax}(2y_{012x}-y_{022x})}{(1-P_{Cx})n_{Cx}}\right]P(X|C)dx$$

for Compliers

(3) If both Assumption 1 and Assumption 2 hold, $BLATE_N = \int_X (y_{022x} - y_{012x})P(X|N)dx$ *for never-takers.*

All treatment effects, probabilities, and y_{zbcx} above are conditional on controls X. To derive

[12] The derivation is the same as what we did without gender selection.

the average effect, we need to integrate over X with respect to $P(X|type)$.

4.6 Discussion on Model Assumptions

As we have shown, several assumptions are required to achieve identification of the treatment effects. To enhance the credibility of our method, we validate and test Assumptions 1, 2, 3, and 4 in this section.

4.6.1 Test of Assumption 1: Linearity

First, we test Assumption 1 using our conceptual model in Section 3. Assumption 1 concerns the linearity of the boy effect on migration for always-takers. To evaluate whether this linearity holds, we compute the second-order derivative of the migration probability ($j = mig$) with respect to the number of boys using the conceptual model in Section 3:

$$\frac{\partial^2 P_{i,mig}}{\partial b_i^2} = \beta^2 P_{i,mig}(1-P_{i,mig})(1-2P_{i,mig})$$

$$\beta = \gamma_2(w_{mig} - w_{home}) + \gamma_4$$

Linearity implies that this second-order derivative should be close to zero. Hence, if $\beta^2 P_{i,mig}(1-P_{i,mig})(1-2P_{i,mig}) \approx 0$, the linearity assumption is likely to hold. In other words, the assumption is plausible if the migration probability $P_{i,mig}$ for always-takers is close to 0, 1/2, or 1, or if the overall partial effect of boys, β, is close to zero. This constitutes our first test.

We apply this test to both the full sample and a subset of identified always-takers (households with two children whose first child is a boy). In the full sample, the estimated migration probability $P_{i,mig}$ is 0.097; for the always-taker subset, $P_{i,mig}$ is 0.118. Then the scaling factors $P_{i,mig}(1-P_{i,mig})(1-2P_{i,mig})$ are 0.07 and 0.08, respectively. For β, we can approximate its value using the first-derivative condition in Equation (4), which yields an estimate of about $\beta \approx 0.125$. These values are all close to zero, indicating that the second-order derivative is likely small in magnitude, both in absolute terms and relative to the first-order derivative. Therefore, the linearity assumption appears to be a reasonable approximation in our empirical context.

4.6.2 Test of Assumption 2: Homogeneity

Assumption 2 requires some homogeneity in the partial effect of boys on migration between never-takers and always-takers. As shown in Equation (4):

$$\frac{\partial P_{i,\text{mig}}}{\partial b_i} = [\gamma_2(w_{\text{mig}} - w_{\text{home}}) + \gamma_4] \cdot P_{i,mig}(1 - P_{i,mig})$$

the boy effect on migration depends on a constant and the migration probability $P_{i,mig}$. Thus, we can directly test whether, conditional on control variables, the migration probabilities $P_{i,mig}$ differ substantially between always-takers and never-takers. If they do not differ much, then the boy effect on migration is unlikely to vary significantly across fertility types. If the difference is large, then Assumption 2 may not hold.

To implement this test, we leverage the random assignment of the first child's gender. Specifically, families whose first child is a boy and who go on to have a second child constitute a random sample of always-takers, while families whose first child is a girl and who do not have a second child represent a random sample of never-takers. Based on this classification, we construct a sample consisting of two groups: (1) families with two children whose first child is a boy (always-takers), and (2) families with only one child who is a girl (never-takers).

We then examine the relationship between the migration probability $P_{i,mig}$ and an indicator variable for being an always-taker, controlling for a rich set of pre-determined household characteristics. These controls include the age and education of both the female and male household heads and province FE. The results, presented in Table 7, indicate that the coefficient on the always-taker indicator is close to zero and statistically insignificant. This suggests that, conditional on observables, the migration probabilities are similar between the two groups. Hence, the partial effect of boys on migration does not appear to vary systematically between always-takers and never-takers, supporting the plausibility of Assumption 2.[13]

4.6.3 Test of Assumption 3: Randomness of Child Gender

Part 1 of Assumption 3 requires that the gender of the first-born child is randomly assigned, which is crucial for our identification strategy. We note that this assumption is more plausible in provinces implementing the 1.5-child policy, where parents with a first-born girl were granted the opportunity to have a second child. This institutional feature reduced the incentive to engage in gender selection at the first birth. Nevertheless, some scope for gender selection may still remain, as the gender of the second child is not guaranteed. To address this concern, we draw on both existing evidence from the literature and an empirical test using our sample.

First, a large body of existing evidence supports the randomness of the gender for the first-born child (Ebenstein, 2010; Sun and Zhao, 2016; Huang, Jiang, and Sun, 2024). Notably, gender selection practices in China typically occur at higher parities, rather than at the first birth. Ebenstein (2010), using data from China's population censuses in 1982, 1990, and 2000, find that the sex ratio of firstborn children is balanced across censuses.

Second, to assess the plausibility of the firstborn gender being randomly assigned, we conduct a balance test using our estimation sample. Specifically, we regress firstborn gender on parental age, parental years of schooling, and indicators for whether either parent attained the highest schooling level (or dropped out), while controlling for county fixed effects and parental birth-province fixed effects. As reported in Table 8, none of these observable characteristics significantly predict firstborn gender. The joint F-test is also statistically insignificant. In addition, The raw proportion of firstborn girls in our sample is 0.495, which is well within the expected biological range (Banister, 2004). Appendix G further examines the sex ratio across different regions and by children's ages. These results support the validity of Part 1 of Assumption 3.

Part 2 of Assumption 3 relies on a selection-on-observables assumption for gender selection

[13]We must clarify that this test is only a partial test because it relies on the underlying assumption that γ_2 and γ_4 are constants. In this case, all the differences between always-takers and never-takers comes from τ_{ij} and ε_{ij}.

at the second birth. This is intrinsically untestable. Appendix C.8 discusses a possible violation of this assumption and its implication to our conclusion. We further discuss the advantage of using this assumption in our method compared with the assumptions we have to make in the IV method in Appendix C.1.

4.6.4 Robustness of Assumption 4: Set Identification

One problem of Assumption 4 is that we need an excellent feature set X_A, which may not be available. This is an untestable assumption. Therefore, we can take a step back and turn to set identification as a robustness check. If P_{A_1x} and P_{Cx} do not depend on X, that is, the distorted gender ratio does not vary across X within always-takers or compliers, we can use an upper bound and a lower bound for P_{A_1} and P_C, respectively, to investigate the behavior of our estimation within the bounds. We find that our main conclusion holds within the reasonable bounds. For more details, please refer to Appendix C.5, C.6, and C.7.

5 Estimation

In this section, we put the identification strategy of the model with gender selection into the application using our data from China.

As discussed in Section 4.2, to conduct point estimation of the $LATEs$ of interest, we need to estimate the number of households in each type, the conditional expectation of migration decision at each node, and the selection probability of each type conditional on X. The control variable set of X we use is the same as the one in the simple IV estimation.

First, given that we know which node on the birth tree each household belongs to, we can estimate the number of households in each type by substituting the number of households in different nodes calculated from the data into Equation (24). Then we obtain the estimation results as follows:

$$\hat{P}_0 = \frac{\sum_i \mathbf{1}(z_i = 0, X = x_i)}{\sum_i \mathbf{1}(z_i = 0, X = x_i) + \sum_i \mathbf{1}(z_i = 1, X = x_i)} \tag{33}$$

$$\hat{n}_{Nx} = \frac{1}{1-\hat{P}_0}\hat{n}_{101} = \frac{1}{1-\hat{P}_0}\sum_i \mathbf{1}(z_i = 1, b_i = 0, c_i = 1, X = x_i) \tag{34}$$

$$\hat{n}_{Ax} = \frac{1}{\hat{P}_0}(\hat{n}_{012} + \hat{n}_{022}) = \frac{1}{\hat{P}_0}[\sum_i \mathbf{1}(z_i = 0, b_i = 1, c_i = 2, X = x_i) \tag{35}$$

$$+ \sum_i \mathbf{1}(z_i = 0, b_i = 2, c_i = 2, X = x_i)]$$

$$\hat{n}_{Cx} = \frac{\hat{n}_{011} - \hat{P}_0 \hat{n}_{Nx}}{\hat{P}_o} \tag{36}$$

However, the dimension of X is too high, and there are too few observations available at each grid $X = x_i$. As a result, we approximate the conditional probabilities directly by the unconditional counts.[14]

Second, we can further estimate the conditional expectation of migration decision y_{zbcx} at each node from the data with a partial linear model.[15] A fully nonparametric estimation is not feasible because the dimension of vector X is too high.

$$\begin{aligned} \hat{y}_{zbcx} =& \hat{E}[Mig_i | x_i = x, (z_i = z, b_i = b, c_i = c)] \\ =& x'\hat{\beta} + \hat{g}(z, b, c) \end{aligned} \tag{37}$$

Third, we need to estimate the selection probability $P_{A_1 x}$ given a set of features X_A. Instead of using $P_{A_1 x}$, that is, the selection probability conditional on X, we also approximate it by the unconditional counterpart. This is a pure prediction problem. As the set of characteristics X_A can be large, a machine learning method can be utilized. The algorithm we choose is the L-1 penalized Logit model, which is a combination of the logistic model and the LASSO model. It can be presented as follows.

[14] To check for the robustness, we also run all the estimations for an unconditional model, that is, when both the probabilities and the expectations are unconditional (getting rid of the control X). The estimated treatment effects are qualitatively similar. The results are available upon request.

[15] The details of estimation for this partial linear model are shown in Appendix C.4. The advantage of the first two simplifications in our estimation is that the linearly separable form of the conditional expectation function for migration results in an elimination of the $x'\hat{\beta}$ term in the estimation of the treatment effects. For example, for $BLATE_A$, $y_{022x} - y_{012x} = x'\hat{\beta} + \hat{g}(0,2,2) - [x'\hat{\beta} + \hat{g}(0,1,2)] = \hat{g}(0,2,2) - \hat{g}(0,1,2)$. The same thing happens for all the average treatment effects. Furthermore, all probabilities are approximated using unconditional counterparts, which means that all the conditional average treatment effects will not depend on X in the estimation. Specifically, there is no need to take the average over the distribution of X within types, which reduces the computational burden by removing the need to estimate $P(X|type)$.

We assume that the conditional probability function is in a Logit form. However, we do not know what predictors to put in the function, so we need to conduct a variable selection process. Given λ, We try to find the best β to maximize the L-1 regularized sum of log-likelihood:

$$\max_{\beta} \sum ln[\frac{e^{x_i'\beta}}{1+e^{x_i'\beta}}] - \frac{1}{2}\lambda \|\beta\|_1 \tag{38}$$

In a situation with a large number of predictors, a standard Logit model may suffer from a typical over-fitting problem with poor out-of-sample behavior. To avoid this problem, the penalized term in the L-1 penalized Logit model can serve as a filter to drop those characteristics in X_A with little explanatory power. The tuning parameter λ determines how hard we penalize the over-fitting problem. We choose the best λ by 10-folds cross-validation.[16] After obtaining the selected predictors and the corresponding coefficients β by maximizing (38), we can calculate the predicted probabilities using the Logit model.

We can then derive an estimate of probability of being always-takers $\hat{P}_i(A|X_A)$ for each household. By substituting the estimated $\hat{P}_i(A|X_A)$ in Equation (31),[17] we can get the estimate for the selection probability P_{A_1} as follows:

$$\hat{P}_{A_1} = \frac{\sum\limits_{i\in(z=1,b=1,c=2)} \hat{P}_i(A|X_A)}{\sum\limits_{i\in(z=1,b=1,c=2)} \hat{P}_i(A|X_A) + \sum\limits_{i\in(z=1,b=0,c=2)} \hat{P}_i(A|X_A)} \tag{39}$$

6 Empirical Results

6.1 Main Results from the Semi-parametric Estimation

In this subsection, we report the estimation results for the point estimation under Assumption 1-4. First, we display the estimates of the number of households in different types according to Equations (33)-(36) as follows: $\hat{n}_N = 11,233$, $\hat{n}_C = 19,624$, $\hat{n}_A = 25,938$. Second, we show the estimation results of selection probability using the L-1 penalized Logit model. Appendix E

[16]In consideration of computational burden, we only train a model for the original data set and use this model for each draw during the process of bootstrap, rather than training a new model once we have a new draw. This may introduce some biases in the estimation of standard error, but we claim that it will not be very severe because the predicted P_A is pretty stable as we change drawn samples.

[17]See Appendix C.3 for details.

shows the detailed feature set choices. Figure 7 shows a 10-fold cross-validation prediction accuracy for different choices of tuning parameter λ.[18] The best λ approximately equals 2 with an in-sample cross-validation accuracy of 0.77. Then we get the estimated selection probabilities: $\hat{P}_{A_1} = 0.787$, $\hat{P}_C = 0.673$.

To investigate whether the prediction model works well, we implement an out-of-sample check to predict the probability of being an always-taker for the households at the node (z=1,b=0,c=1), who are never-takers for sure. If our prediction model is reliable, then most of these households should be classified as non-always-takers. With 5,257 observations at the node, on average, the predicted probability of being an always-taker is 0.29; the accuracy of classifying the observations to be a non-always-taker is 0.746, slightly lower than the in-sample prediction above.[19]

Table 9 shows the estimation results for $FLATE$ and $BLATE$ with the estimated selection probabilities. For the effect of the number of children, one additional girl in the household will increase the probability that both parents migrate by 12.1% (1.17 percentage points), and the father migrates by 13.0% (2.27 percentage points), though the first one is not precisely estimated.

Compared with the results in the simple IV estimation, the effect of the number of children becomes considerably more positive. Specifically, the estimate on father migration becomes statistically significant at the 10% level, and the magnitude is about twenty times as large as the corresponding simple IV estimate.

For the effect of the child gender, almost all estimates are both economically and statistically significant. The differential effect of one boy, as opposed to one girl, increases the probability that both parents migrate by 11.6% (1.13 percentage points), the father migrates by 6.0% (1.04 percentage points), and the mother migrates by 8.2% (0.99 percentage points). The child gender

[18] In a classification model, people usually classify observations with conditional probabilities higher than 0.5 to be Class 1 and observations with conditional probabilities less than 0.5 to be Class 0. By comparing the in-sample prediction of the model to the true class of the observation in the sample, we can calculate an overall accuracy of the model for in-sample prediction.

[19] To enhance the comparability of our results, we estimate the naive IV regression controlling for the covariates selected by the Lasso method.

effect is comparable to the fertility effect in terms of absolute values and is statistically more significant.

All point estimates of the effects of the number and gender of children on parents' migration are positive, which suggests that having more children may be associated with a higher likelihood of parental migration to urban areas. While the estimates are not always statistically significant and some are small in magnitude, the direction of the effects is consistent with the interpretation that, on average, the income gains from migration may offset the costs of separation. Moreover, parents with more sons are more likely to migrate, possibly because they anticipate the competitive marriage market their sons will face in the future (Wei and Zhang, 2011; Li and Yi, 2015). The implementation of the OCP led to a highly skewed sex ratio at birth in China, increasing the competitiveness of the marriage market for boys. To enhance their sons' prospects, parents may need to save more and seek higher-paying jobs in cities, highlighting the importance of the income effect from sons in parental migration decisions. Another notable pattern is that the effect is more pronounced for males than for females. According to our conceptual model, this suggests that fathers are more responsive to income incentives, whereas mothers are more sensitive to the separation costs associated with children.

6.2 Comparison with the Simply IV Estimation

While the IV estimates in Table 4 suggest that the number of children has a small and insignificant effect on both parents' and male parent migration, and a significantly negative effect on female parent migration, the semi-parametric results yield positive point estimates for both the number of children and the number of boys across all three migration measures. This contrast highlights that the simple IV strategy, which does not account for child gender composition, may face challenges in satisfying the exclusion restriction. Specifically, the instrument—having a first-born girl—not only increases the likelihood of higher fertility, but may also reduce the expected number of boys in the household. If the number of boys is positively associated with migration decisions, omitting this variable could induce a negative correlation between the instrument and the error term in the second stage, potentially leading to a downward bias in the estimated fertility effect on migration. Intuitively, the fertility effect and the child gender effect offset each other in the simple IV estimation in Section 4.1.[20]

6.3 Robustness

External Validity for non-1.5-Child Policy Provinces We try to address the concern that our focus on provinces with the 1.5-child policy may limit external validity. We extend our analysis to include all provinces regardless of policy regime. The results remain robust in this expanded sample (Table D2), suggesting that our conclusions are not solely driven by the unique features of 1.5-child policy regions.

Redefine Rural-to-urban Migration Regarding the definition of rural-to-urban migration, our baseline sample consists of individuals who hold rural hukou but are no longer living in their hukou village. While this definition may include some rural-to-rural migrants, rural-to-rural migration is relatively rare in China, and the majority of rural hukou holders who migrate do so toward urban destinations. To strengthen the identification of rural-to-urban migration, we re-estimate our main models using a stricter definition that excludes rural-to-rural migrants. Details are provided in Appendix D.1.4. The results based on this refined sample remain consistent with our main findings, as shown in Table D5, indicating that our estimates are robust to the definition of migrant status.

Accounting for Migration Timing Although our main empirical strategy relies on a static model due to data limitations, we recognize the importance of accounting for the timing of migration relative to fertility decisions (Démurger and Xu, 2011). Ideally, a dynamic framework would allow us to disentangle whether fertility choices preceded or followed migration. How-

[20]Indeed, if the exclusion restriction holds and there is no contamination, the naive IV identifies the complier's $FLATE$. However, the estimates are contaminated because the first child's gender affects parental migration not only through fertility but also by altering the gender composition of children. The first child's gender influences gender composition not just for compliers, but also for always-takers and never-takers. As a result, the estimated $FLATE$ incorporates all the $BLATE$s, not solely that of the compliers.

ever, no dataset in China currently provides comprehensive information on both migration and fertility histories at the individual or household level. In the absence of such panel or retrospective data, we adopt a pragmatic approach by exploiting available information on migration duration.

Specifically, we conduct a robustness check using a refined subsample of households in which parents' migration clearly occurred *after* the birth of their youngest child. To construct this subsample, we use the categorical information on migration duration available in the 2010 census. By excluding parents whose reported migration duration exceeds the age of their youngest child, we can reasonably ensure that all childbirths occurred prior to migration. This restriction helps mitigate concerns regarding reverse causality—namely, that migration could have influenced fertility decisions—by focusing on households where fertility is likely exogenous to migration. The estimation results using this restricted sample are consistent with those from our main analysis, suggesting that our baseline findings are not driven by the potential endogeneity of migration timing. The estimates are reported in Table D6.

Discussion on Left-behind Children We extend our conceptual framework and empirical results to distinguish between rural-to-urban migration with and without children, recognizing that these alternatives impose distinct financial, emotional, and institutional costs in Appendix F. Migrating without children allows parents to minimize urban living expenses and may offer children better schooling opportunities in their hometowns, but comes at the cost of prolonged separation. In contrast, bringing children along reduces emotional strain but significantly increases the burden of urban childrearing—particularly under Hukou-based restrictions on access to education and healthcare. Descriptive statistics highlight that separation is the prevailing arrangement: 83% of migrant fathers and 73% of migrant mothers leave their children behind, and even among households in which both parents migrate, 71% of children do not accompany them. These patterns underscore the structural and economic barriers that shape migration decisions.

In the empirical estimation, we find that the number and gender of children significantly influence parental migration without children, suggesting that the income benefits from migration—especially for families with sons—often outweigh the emotional costs of separation. In contrast, these fertility-related effects are substantially attenuated and statistically insignificant in cases where parents migrate with children, indicating that the high cost of raising children in urban areas offsets potential labor market gains. Therefore, our main results are generally driven by left-behind children.

7 Conclusion

In this paper, we examine the effect of children on parents' migration decision, specifically, focusing on both the number and the gender of children. We propose a new semi-parametric method to identify these two effects separately. Although the model is used to analyze the parental migration decision in this paper, it can be applied to many other household decisions, including parents' marital decision, labor market decision, and household savings.

Our results show that having more children will encourage rural-to-urban parental migration, which is compatible with the situation that the positive income effect dominates the negative effect of separation cost. For both boys and girls, the effects are more significant on fathers' migration decisions than for mothers' migration decisions. Moreover, the gender of children plays an essential role in determining parents' migration decision and cannot be neglected. Compared with girls, boys bring a larger effect on the migration probability of their parents. Comparing the results with the ones estimated in the simple IV estimation, we find that the simple IV method widely used in previous studies confounds the two effects and causes them to offset each other, which results in a downward bias. This comparison has an important implication for research on OCP. Explicitly, any misuse of the policy instrument may give us a contaminated estimate and mask the real effect. Any effort to reduce the population growth in developing countries is likely to hinder rather than promote urbanization.

The paper has some limitations. First, to derive an exact point identification, we need to have a rich dataset which can provide us a good prediction model to estimate gender selection probability. Second, the model only fits well in a society with a strong preference for boys and a strict fertility control policy. In such a case, we are safe to assume that there is no defiers in the population. Third, due to data limitations, our analysis adopts a static framework and does not capture migration and fertility dynamics. Investigating these dynamics represents an important and promising avenue for future research as more detailed data become available. Despite the limitations, our work is the first attempt to address the endogeneity of both the number and gender of children and separately estimate these two effects. Further research should focus on solving these problems in other settings.

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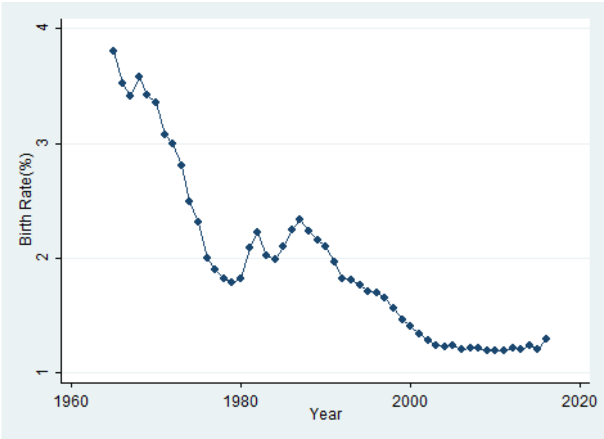

Figure 1: China's Birth Rate 1965-2016 (%)
Notes: Data from World Bank

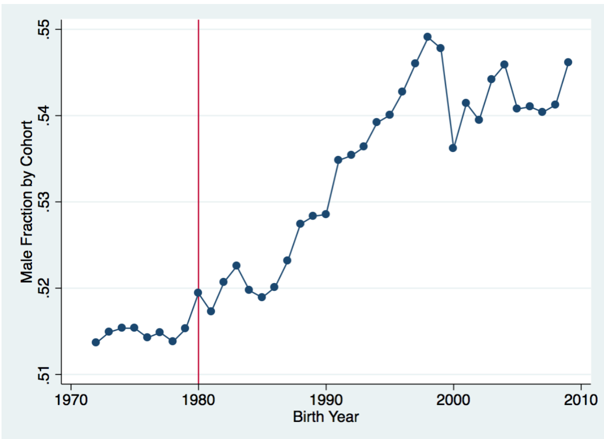

Figure 2: China's Gender Imbalance 1980-2010
Notes: Using data from China Census 1982, 1990, 2000, and 2010, we calculate the gender ratios for neonates from 1972 to 2009. The red line represents the starting time of the OCP.

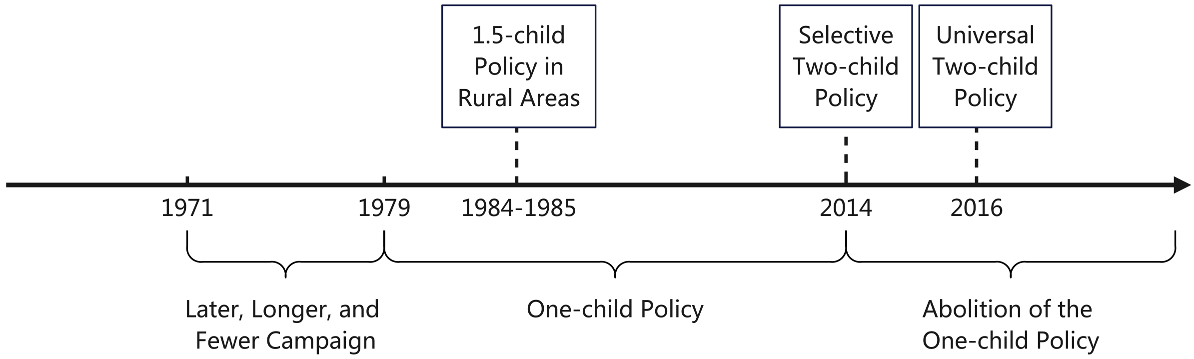

Figure 3: Timeline of Fertility Control Policies in China

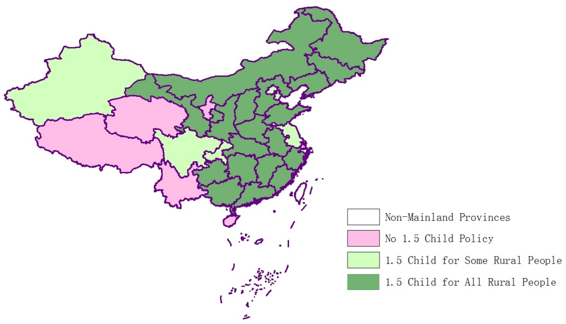

Figure 4: The Distribution of Provinces with 1.5-Child Policy

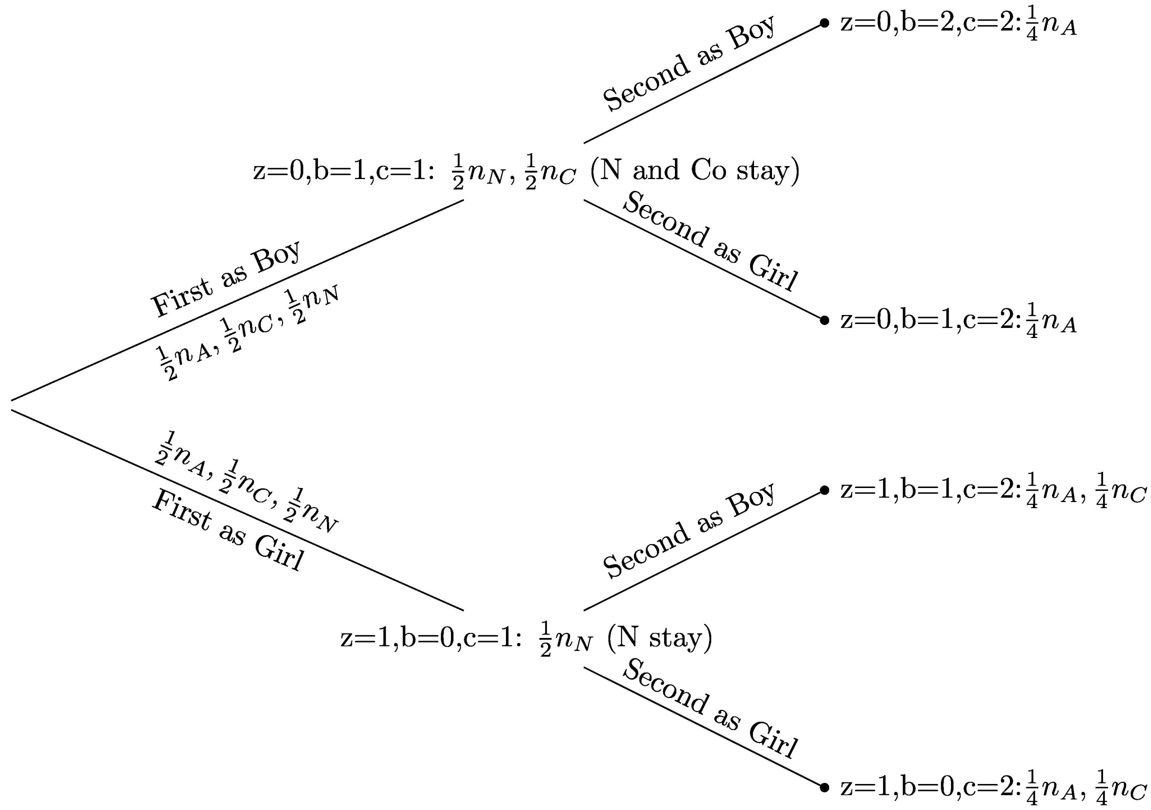

Figure 5: Birth Tree

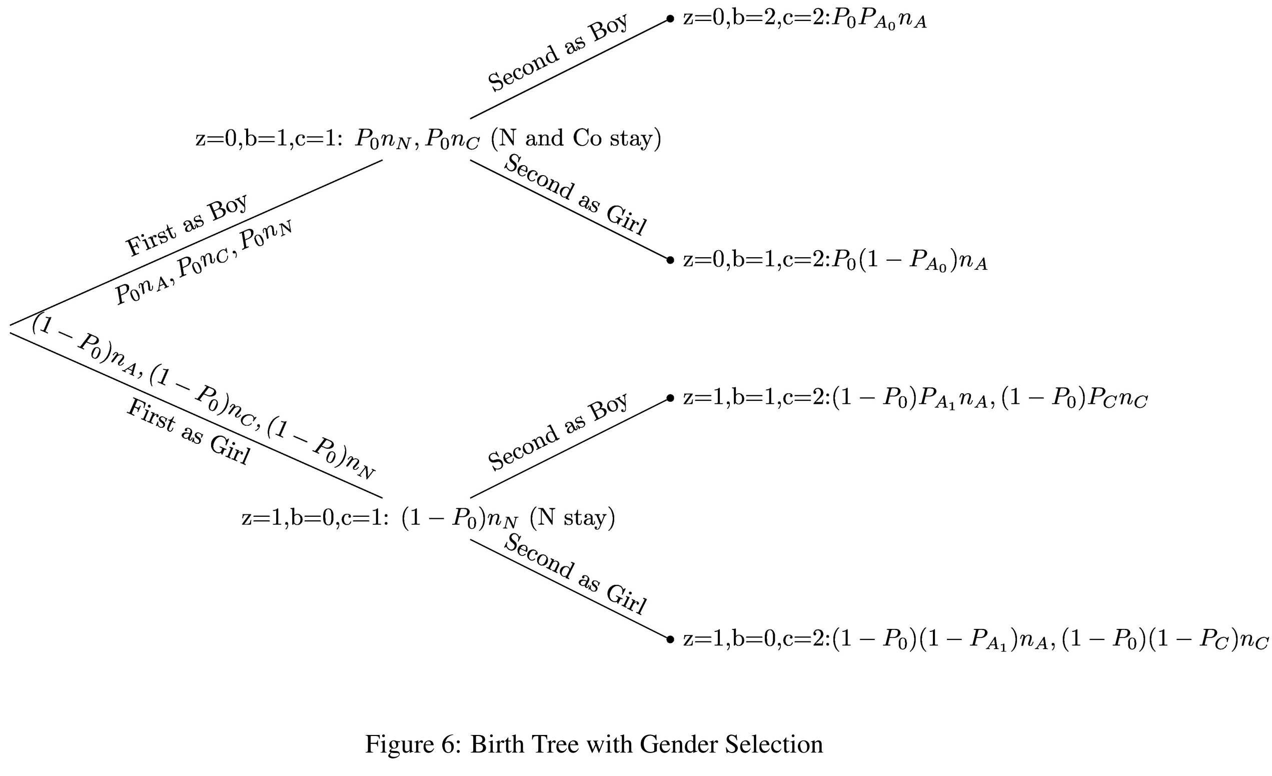

Figure 6: Birth Tree with Gender Selection

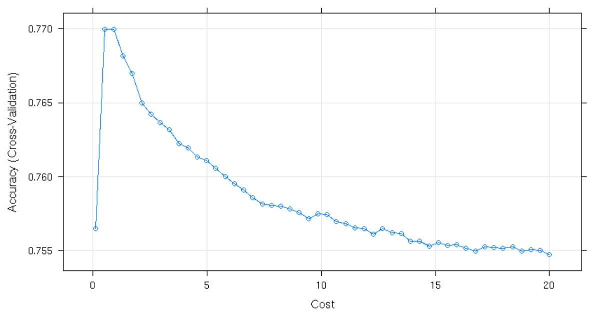

Figure 7: CV Accuracy for Different Choices of Cost Parameter λ

Table 1: List of Provinces offering 1.5-Child Policy

Province	All 1.5	Some 1.5	Province	All 1.5	Some 1.5
Beijing	NO	YES	Henan	YES	YES
Tianjin	NO	YES	Hubei	YES	YES
Hebei	YES	YES	Hunan	YES	YES
Shanxi	YES	YES	Guangdong	YES	YES
Inner Mongolia	YES	YES	Guangxi	YES	YES
Liaoning	YES	YES	Hainan	NO	NO
Jilin	YES	YES	Chongqing	NO	YES
Heilongjiang	YES	YES	Sichuan	NO	YES
Shanghai	NO	NO	Guizhou	YES	YES
Jiangsu	NO	YES	Yunnan	NO	NO
Zhejiang	YES	YES	Tibet	NO	NO
Anhui	YES	YES	Shaanxi	YES	YES
Fujian	YES	YES	Gansu	YES	YES
Jiangxi	YES	YES	Qinghai	NO	NO
Shandong	YES	YES	Ningxia	NO	NO
Xinjiang	NO	YES			

Table 2: Summary Statistics

Panel A: Distribution of Households by Number of Children		
Category	**# of Households**	**Percentage (%)**
No child	1,100	1.69
One child	21,673	33.24
Two children	35,121	53.87
Three or more children	7,301	11.20

Panel B: Summary Statistics for Households with One or Two Children				
Variable	**Mean**	**Std.Dev.**	**Min**	**Max**
Dependent Variables				
If both parents migrate	0.097	0.297	0	1
If mother migrates	0.121	0.326	0	1
If father migrates	0.174	0.379	0	1
Interested Independent Variables				
Number of children in the household	1.618	0.486	1	2
Number of boys in the household	0.923	0.555	0	2
Family Characteristics				
Age of the first-born child	14.389	3.039	0	18
Father's age	40.355	3.587	35	65
Mother's age	39.174	3.038	35	64
If father is literate	0.995	0.069	0	1
If mother is literate	0.979	0.144	0	1
Father's education	8.652	1.667	0	19
Mother's education	8.009	1.985	0	19

Data source: the 2010 census. Panel A reports the distribution of households by number of children. Panel B presents descriptive statistics for the sample of families with one or two children, totaling 56,794 households.

Table 3: First Stage Results for the Simple IV Estimation

	# of children in the household		
	(1)	(2)	(3)
first child sex(=1 if girl)	0.346***	0.352***	0.369***
	(0.020)	(0.020)	(0.020)
Parental Demographic Controls	No	Yes	Yes
Family Composition Controls	No	Yes	Yes
Geographic Information Controls	No	No	Yes
Prob>F	0.000	0.000	0.000
Number of Observations	56,794	56,794	56,794

Notes: Parental demographic controls include the first child's age, the parents' ages, years of marriage, and education-level fixed effects for both parents. Family composition controls include the number of co-residing parents and parents-in-law, as well as the number of siblings. Geographic information controls include fixed effects for both parents' hukou registration prefectures, the current survey prefecture, and the parents' provinces of birth. Standard errors in parentheses are clustered at the prefecture level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 4: IV Estimation Results for Household's Parents' Migration

	Both	Male	Female
	(1)	(2)	(3)
#children	-0.0117	0.0010	-0.0140*
	(0.0073)	(0.0081)	(0.0082)
Parental Demographic Controls	Yes	Yes	Yes
Family Composition Controls	Yes	Yes	Yes
Geographic Information Controls	Yes	Yes	Yes
Number of Observations	56,794	56,794	56,794

Notes: Parental demographic controls include the first child's age, the parents' ages, years of marriage, and education-level fixed effects for both parents. Family composition controls include the number of co-residing parents and parents-in-law, as well as the number of siblings. Geographic information controls include fixed effects for both parents' hukou registration prefectures, the current survey prefecture, and the parents' provinces of birth. Column (1) reports the IV estimation results when the dependent variable is *Both parents' Migration*. Column (2) reports the IV estimation results when the dependent variable is *father's Migration*. Column (3) reports the IV estimation results when the dependent variable is *mother's Migration*. Standard errors in parentheses are clustered at the prefecture level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 5: Distribution of Different Types of Households without Gender Selection

	$z_i = 1$ (G)	$z_i = 0$ (B)
$c_i = 1$	$b_i = 0$: $\frac{1}{2}n_N$	$b_i = 1$: $\frac{1}{2}n_N$, $\frac{1}{2}n_C$
$c_i = 2$	$b_i = 0$: $\frac{1}{4}n_A$, $\frac{1}{4}n_C$	$b_i = 1$: $\frac{1}{4}n_A$
	$b_i = 1$: $\frac{1}{4}n_A$, $\frac{1}{4}n_C$	$b_i = 2$: $\frac{1}{4}n_A$

Table 6: Distribution of Different Types of Individuals with Gender Selection

	$z_i = 1$ (G)	$z_i = 0$ (B)
$c_i = 1$	$b_i = 0$: $(1-P_0)n_N$	$b_i = 1$: $P_0 n_N$, $P_0 n_C$
$c_i = 2$	$b_i = 0$: $(1-P_0)(1-P_{A_1})n_A$, $(1-P_0)(1-P_C)n_C$	$b_i = 1$: $P_0(1-P_{A_0})n_A$
	$b_i = 1$: $(1-P_0)P_{A_1}n_A$, $(1-P_0)P_C n_C$	$b_i = 2$: $P_0 P_{A_0} n_A$

Table 7: Test of Assumption 2: Homogeneity

Dependent Var.	Both	Male	Female
	(1)	(2)	(3)
Being an Always Taker	0.007	0.006	-0.003
	(0.009)	(0.009)	(0.008)
Father's Education Year	0.005***	0.008***	0.003
	(0.002)	(0.002)	(0.002)
Father's Age	-0.004***	-0.005***	-0.003**
	(0.001)	(0.001)	(0.001)
Mother's Education Year	0.003	0.001	0.003*
	(0.002)	(0.002)	(0.001)
Mother's Age	-0.003***	-0.003**	-0.004***
	(0.001)	(0.001)	(0.001)
Number of Observations	19,055	19,055	19,055

Notes: The estimation sample comprises two groups of families: (1) two-child families with a first-born boy (always-takers) and (2) one-child families with a first-born girl (never-takers). Column (1) reports results using *both parents' migration* as the dependent variable, Column (2) reports results using *father's migration*, and Column (3) reports results using *mother's migration*. All regressions control for the age and years of schooling of the female and male household heads, as well as province fixed effects. Standard errors, reported in parentheses, are clustered at the province level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 8: Balance Tests: Regression of the First Child's Gender on Household Characteristics

Dependent Var.	First Child Being a girl (=1)	
	(1)	(2)
Father's Age	0.001	0.001
	(0.001)	(0.001)
Father's Years of Schooling	-0.002	-0.002
	(0.002)	(0.002)
Father Graduated (=1)	-0.009	-0.009
	(0.013)	(0.013)
Mother's Age	0.002*	0.002*
	(0.001)	(0.001)
Mother's Years of Schooling	-0.001	-0.001
	(0.002)	(0.002)
Mother Graduated (=1)	0.004	0.004
	(0.011)	(0.011)
P-value of the F-test	0.3044	0.2979
Observations	51,374	51,371
R-squared	0.0052	0.0052

Notes: The estimation sample is the same as in the semi-parametric estimation. The dependent variable is a binary indicator for whether the first child is a girl. Controls include county-level fixed effects and birth-province fixed effects for both parents. We conduct F-tests for the joint significance of these characteristics and report the corresponding p-values. Standard errors, reported in parentheses, are clustered at the province level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 9: Semi-parametric Estimation Results with Estimated Selection Probability

	Both	Male	Female
	(1)	(2)	(3)
$FLATE$	0.0117	0.0227*	0.0003
	(0.0102)	(0.0132)	(0.0111)
$BLATE_C$	0.0038	0.0036	0.0099
	(0.0086)	(0.0120)	(0.0093)
$BLATE_A$	0.0153***	0.0140**	0.0099*
	(0.0051)	(0.0065)	(0.0056)
$BLATE$	0.0113***	0.0104**	0.0099**
	(0.0038)	(0.0051)	(0.0041)
Number of Observations	56,794	56,794	56,794

Notes: All estimations are conducted with estimated selection probability. Column (1) reports the semi-parametric estimation results when the dependent variable is *Both Parents' Migration* for $FLATE$, $BLATE_C$, $BLATE_A$, and $BLATE$, respectively. Column (2) reports the semi-parametric estimation results when the dependent variable is *Father's Migration*. Column (3) reports the semi-parametric estimation results when the dependent variable is *Mother's Migration*. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Appendix

A More Details on the Census Data

The census data aligns well with the objectives of our analysis. First, the dataset allows us to identify rural-to-urban migration within households. In the census, individuals are surveyed based on both their residence location and hukou location. This means that migrants who live elsewhere but maintain hukou registration in the sampled community are still included in the household roster. As a result, both migrant and non-migrant members of the same household are captured in the survey, allowing us to track migration behavior accurately.

Second, we can infer child information and maternal fertility history using the available household structure. Although the census does not directly link parents to children, the relationship variable identifies the household head, the spouse of the household head, and their children. Based on this information, we can determine the number of children currently listed under the household's hukou registration and obtain their demographic details. While we lack data on children whose hukou are registered elsewhere, China's household registration policy requires that juvenile children be registered under one of their parents or a senior family member. Since children cannot hold independent hukou, this institutional rule ensures that young children—regardless of their current physical location—are recorded under their parents' household hukou. This design substantially mitigates concerns about potential selection bias due to children's migration or co-residence status. Moreover, women aged 15 to 64 are required to report both the number of children ever born and the number of surviving children. By combining this self-reported fertility history with the child-level information in the household, we can reconstruct a complete record of maternal fertility. For consistency and accuracy, we restrict our sample to households in which the number of children ever born equals the number of surviving children, and both match the number of children listed under the hukou registration.[21] This sample restriction enhances internal consistency and allows us to reliably identify the gender composition and birth order of children in each household.[22] Lastly, the census provides rich socioeconomic information at both the household and individual levels, further supporting our empirical analysis.

[21] Child mortality rates in China have been very low in recent decades. According to World Bank data, the under-five mortality rate was only 1.6% in 2010. See: https://data.worldbank.org/indicator/SH.DYN.MORT?locations=CN. Consequently, the number of households where the reported births differ from the number of surviving or registered children is small, and excluding these cases does not materially affect our estimates. Nonetheless, we have conducted robustness checks without this restriction, and the main results remain stable in both magnitude and statistical significance.

[22] For families with missing child information, this is likely due to grown-up children having moved out of their parents' household. Since our focus is on the effect of young (juvenile) children on parental behavior, this does not pose a major limitation for our study.

B A Naive Revision

A naive way to fix the problem is to directly control for the child gender effect in the regression and instrument the number of children with an OCP-related IV. However, this method is also problematic. As parents can illegally implement gender selection for their children, the gender of children is also an endogenous variable that can be correlated with unobserved family characteristics. Besides, the number of boys is itself a post-determined endogenous variable even if there is no gender selection. The reason is that the number of boys is determined after the realization of the first child gender, which are called "bad controls" by Angrist and Pischke (2008). We show a detailed analysis in Appendix C.1.

We run the following regression, instrumenting the number of children by the gender of the first child:

$$Mig_{il} = \beta_0 + \beta_1 c_{il} + \beta_2 b_{il} + \boldsymbol{X}_{il}\beta_3 + u_{il}, \tag{40}$$

where b_{il} refers to the number of boys in household i from location l. The results of the first-stage regression are displayed in Table B1, which support a strong positive relationship between the gender of the first child and the number of children in the household. The results of the revised IV estimation are shown in Table B2. After controlling for the number of boys in the household, the point estimates in the IV estimation increase. However, the estimates are still biased and smaller than the estimates from our method (Table 9) because the number of boys in the household is endogenous. The endogeneity comes from two kinds of sources. First, conditional on the gender of the first child z_i and X_{il}, the number of boys b_i is not independent of the unobserved term u_{il}. Second, conditional on the number of boys b_i and X_{il}, the gender of the first child z_i is no longer independent of the unobserved term u_{il}. Intuitively, the number of boys in the household is determined after the realization of the first-child gender lottery. Although the randomness of the first child gender guarantees a random division of all the households according to the first child gender, the number of boys post-determined after the first birth will be affected by the realization of the first child gender. Therefore, after controlling for the number of boys b_{il} in the regression, the distribution of u_{il} is no longer identical for households with a first-born boy and households with a first-born girl.

Table B1: First Stage Results for Revised IV Estimation with Linearly Controlling for Boy Number

	#children in the household		
	(1)	(2)	(3)
first child sex(=1 if girl)	0.714***	0.707***	0.662***
	(0.0210)	(0.0210)	(0.0205)
#boys	Yes	Yes	Yes
Parental Demographic Controls	No	Yes	Yes
Family Composition Controls	No	Yes	Yes
Geographic Information Controls	No	No	Yes
Prob>F	0.000	0.000	0.000
Number of Observations	56,794	56,794	56,794

Notes: Parental demographic controls include the first child's age, the parents' ages, years of marriage, and education-level fixed effects for both parents. Family composition controls include the number of co-residing parents and parents-in-law, as well as the number of siblings. Geographic information controls include fixed effects for both parents' hukou registration prefectures, the current survey prefecture, and the parents' provinces of birth. Standard errors in parentheses are clustered at the prefecture level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table B2: Revised IV Estimation Results with Linearly Controlling for Boy Number

	Both	Male	Female
	(1)	(2)	(3)
#children	-0.0016	0.0120**	-0.0085
	(0.0045)	(0.0048)	(0.0058)
#boys	Yes	Yes	Yes
Parental Demographic Controls	No	Yes	Yes
Family Composition Controls	No	Yes	Yes
Geographic Information Controls	No	No	Yes
Prob>F	0.000	0.000	0.000
Number of Observations	56,794	56,794	56,794

Notes: Parental demographic controls include the first child's age, the parents' ages, years of marriage, and education-level fixed effects for both parents. Family composition controls include the number of co-residing parents and parents-in-law, as well as the number of siblings. Geographic information controls include fixed effects for both parents' hukou registration prefectures, the current survey prefecture, and the parents' provinces of birth. Column (1) reports the IV estimation results when the dependent variable is *Both Parents' Migration*. Column (2) reports the IV estimation results when the dependent variable is *Father's Migration*. Column (3) reports the IV estimation results when the dependent variable is *Mother's Migration*. Standard errors in parentheses are clustered at the prefecture level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

C Technical Details of the Empirical Research Design

C.1 Endogeneity of b_i and Comparison to Off-The-Shelf Methods

We now explain in detail why the first child gender z_i is not independent of u_i conditional on the number of boys b_i. We can see from Table 5, conditional on $b_i = 1$, when $z_i = 1$, the households are composed of a quarter of the always-takers and a quarter of the compliers $(\frac{1}{4}A, \frac{1}{4}C)$. Similarly, conditional on $b_i = 1$, when $z_i = 0$, the households are composed of a half of the never-takers, a half of the compliers, and a quarter of the always-takers $(\frac{1}{2}N, \frac{1}{2}C, \frac{1}{4}A)$. As the always-takers, never-takers, and compliers have different e_i, the distribution of e_i conditional on $b_i = 1$ is different for $z_i = 0, 1$. Therefore, conditional on b_i, z_i is correlated with e_i. As e_i and u_i may also be correlated,[23] z_i may be correlated with u_i conditional on b_i. Similarly, we can prove that b_i may be correlated with u_i conditional on z_i. Hence, we cannot simply control for b_i to address the child gender effect.

As discussed, both the number and the gender of children are endogenous in the migration decision equation. The concern is what the advantage of our semi-parametric method is compared to the IV revision method discussed in Appendix B, which conducts an IV estimation with linear control of the number of boys. Under this revised IV estimation method, if we want to get a consistent estimate, we need to make two assumptions on the number of boys b_{il}. First, conditional on X_{il} and the gender of the first child z_{il}, the number of boys b_{il} is independent of the unobserved u_{il}, which is intuitively similar, but stronger than the "selection on observables" assumption we make. Second, conditional on X_{il} and b_{il}, the gender of the first child z_{il} is independent of the unobserved u_{il}.

However, as long as the endogeneity of fertility exists, that is, e_i is correlated with u_i, both of the two assumptions above do not hold.[24] In contrast, our method works as long as Assumption

[23] Some common unobserved factors affect fertility decision and migration decision at the same time. For instance, an individual with higher acceptance of modern values may not only have less children but may also be more likely to migrate to urban areas. Thus, e_i is correlated with u_i

[24] Mathematically, we need the assumption that for W_{il}=[X_{il} C_{il} b_{il} z_{il}], we have $W_{il} \perp u_{il}$ jointly.

3 holds. Assumption 3 in our identification method states that there is no endogenous gender selection after controlling for the observed characteristics and the fertility behavioral types, which is weaker than the usual "selection-on-observable" assumption in the matching method.

Generally speaking, the revised IV estimate is still biased, and no off-the-shelf method is feasible in our context. The empirical results of this off-the-shelf method are displayed in Appendix B, which shows that the point estimates are slightly improved, but still biased.

C.2 Identification of LATEs without Gender Selection

First of all, we can directly derive from Equations (16), (17), and (19) that $E[Mig_{i21}|A] = y_{012}$, $E[Mig_{i22}|A] = y_{022}$ and $E[Mig_{i10}|N] = y_{101}$. By combining Equation (16) and Equation (21), we can derive the identification of $E[Mig_{i21}|C]$ as follows:

$$E[Mig_{i21}|C] = \frac{(n_A + n_C)y_{112} - n_A y_{012}}{n_C} \tag{41}$$

Under Assumption 1, we have $E[Mig_{i22}|A] - E[Mig_{i21}|A] = E[Mig_{i21}|A] - E[Mig_{i20}|A]$. As a consequence, we can identify $E[Mig_{i20}|A] = 2E[Mig_{i21}|A] - E[Mig_{i22}|A] = 2 \cdot y_{012} - y_{022}$, where $E[Mig_{i22}|A] - E[Mig_{i21}|A]$ is the partial effect of the number of boys on migration for always-takers. By substituting the expression of $E[Mig_{i20}|A]$ into Equation (20), we can identify $E[Mig_{i20}|C]$ as follows:

$$E[Mig_{i20}|C] = \frac{(n_A + n_C)y_{102} - n_A(2 \cdot y_{012} - y_{022})}{n_C} \tag{42}$$

Then we can identify the differential effect of one boy as opposed to one girl on parental migration decision for compliers as $E[Mig_{i21}|C] - E[Mig_{i20}|C]$.

However, without imposing additional assumptions, we cannot distinguish between $E[Mig_{i11}|N]$ and $E[Mig_{i11}|C]$ in this semi-parametric model. If we want to identify the LATE of fertility, we need Assumption 2.

If both Assumptions 1 and 2 hold, we can derive $E[Mig_{i11}|N]$ as follows:

$$y_{022} - y_{012} = E[Mig_{i11}|N] - y_{101} \tag{43}$$

$$\Rightarrow \quad E[Mig_{i11}|N] = y_{022} - y_{012} + y_{101} \tag{44}$$

Substituting it into Equation (18) we get:

$$E[Mig_{i11}|C] = \frac{(n_N + n_C)y_{011} - n_N(y_{022} - y_{012} + y_{101})}{n_C} \tag{45}$$

Hence, we can identify $FLATE$ as follows:

$$\begin{aligned} &E[Mig_{i21}|C] - E[Mig_{i11}|C] \\ =&\frac{(n_A + n_C)y_{112} - (n_A + n_N)y_{012} - (n_N + n_C)y_{011} + n_N(y_{022} + y_{101})}{n_C} \end{aligned} \tag{46}$$

C.3 Identification of P_{A_1}

P_{A_1x} can be identified as follows:

$$P_{A_1x} = \frac{P(A, b = 1|c = 2, z = 1, X)}{P(A, b = 1|c = 2, z = 1, X) + P(A, b = 0|c = 2, z = 1, X)}, \tag{47}$$

where

$$P(A, b = 1|c = 2, z = 1, X) = \int_{X_A \setminus X} P(A|X_A, z = 1, b = 1, c = 2)P(X_A \setminus X, b = 1|c = 2, z = 1, X)dx_A \setminus x, \tag{48}$$

$$P(A, b = 0|c = 2, z = 1, X) = \int_{X_A \setminus X} P(A|X_A, z = 1, b = 0, c = 2)P(X_A \setminus X, b = 0|c = 2, z = 1, X)dx_A \setminus x. \tag{49}$$

If Assumption 4 holds, we can have Equations (48) and (49) as follows:

$$P(A, b = 1|c = 2, z = 1, X) = \int_{X_A \setminus X} P(A|X_A, z = 0)P(X_A \setminus X, b = 1|c = 2, z = 1, X)dx_A \setminus x \tag{50}$$

$$P(A, b = 0|c = 2, z = 1, X) = \int_{X_A \setminus X} P(A|X_A, z = 0)P(X_A \setminus X, b = 0|c = 2, z = 1, X)dx_A \setminus x \tag{51}$$

where $P(A|X_A, z = 0)$ can be derived from the upper part of the tree, and $P(X_A \setminus X, b = 1|c =$

$2, z = 1, X) = \frac{P(X_A \backslash X, b=1, c=2, z=1, X)}{P(c=2, z=1, X)}$ can also be derived from the data. We can then identify Equation (47) by incorporating Equations (50) and (51).

A sufficient condition for Assumption 4 is that $P(A|X_A, b, c, z) = P(A|X_A)$ where X_A is a sufficient statistic in predicting always-takers.

C.4 How to Incorporate Controls X in the Estimation

To incorporate the control of X for y_{zbcx}, that is, the conditional expectations of the potential migration decisions at each node, we use a partial linear model developed by Robinson (1988). Let Z be a vector representing the node. $Z = (z, b, c)$ is a vector including three elements: the gender of the first child, the number of children and the number of boys. We do not impose any functional form assumption on Z in the migration decision function but assume that X enters into the function linearly.

$$Mig_i = x_i'\beta + g(Z_i) + u_i \tag{52}$$

where $E[u_i|X_i, Z_i] = 0$. We can express y_{zbc} as follows:

$$\begin{aligned} y_{zbcx} =& E[Mig_i|x_i = x, (z_i = z, b_i = b, c_i = c)] \\ =& x'\beta + g(Z) \end{aligned} \tag{53}$$

The estimation of $g(Z)$ will be affected by the choice of our control variables. In principle, we can incorporate X into the conditional expectation function non-parametrically. However, as the location fixed effect dummies increase the dimension considerably, a fully non-parametric method becomes infeasible. Considering that the partial effect of X is not our target, we sacrifice some model functional form flexibility in the migration decision to avoid the curse of dimensionality.

To estimate y_{zbcx}, the following steps are employed. Step 1: We derive estimates of $E(Mig_i|Z_i)$ and $E(x_i|Z_i)$ using the sample average at the corresponding node. Step 2: We regress $Mig_i - \hat{E}(Mig_i|Z_i)$ on $x_i - \hat{E}(x_i|Z_i)$ and get an estimate of $\hat{\beta}$. Step 3: We calculate $Mig_i - x_i'\hat{\beta}$ for each

i and get the estimate of $\hat{g}(Z_i) = \frac{1}{n} \sum_{j \in Z_i} (Mig_j - x_j' \hat{\beta})$.

To obtain the estimates of all the treatment effects, we can plug in the estimates of y_{zbcx} above. Standard errors and confidence intervals will be derived by bootstrapping with 500 replications.

C.5 Details on Set Identification

One problem of Assumption 4 is that we need an excellent feature set X_A, which may not be available. Therefore, we can take a step back and turn to set identification. If P_{A_1x} and P_{Cx} do not depend on X, that is, the distorted gender ratio does not vary across X within always-takers or compliers, we can use an upper bound and a lower bound for P_{A_1} and P_C, respectively, to investigate the behavior of our estimation within the bounds. For the effect of the number of children, we rearrange the terms inside Equation (32) and get the $FLATE$ as follows:

$$FLATE = \frac{P_{A_1}}{P_C} \int_X \frac{n_{Ax}(y_{112x} - y_{012x})}{n_{Cx}} P(X|C)dx + k, \tag{54}$$

where P_{A_1} and P_C are the distorted gender ratios, and k is a constant not related to P_{A_1} and P_C. Given that P_{A_1} and P_C are negatively correlated, this equation is a monotonic function of P_{A_1}. As a consequence, the bound for P_{A_1} is just the bound for $FLATE$. We define $P^l_{A_1}$ as the lower bound and $P^u_{A_1}$ as the upper bound. We can use Equation (24) to derive the corresponding bounds for P^u_C and P^l_C.

Proposition 6 *If Assumptions 1, 2 and 3 hold simultaneously and in addition $P_{A_1x} = P_{A_1}$ and $P_{Cx} = P_C$, which do not depend on X, then we can bound FLATE as follows:*

$$FLATE^1 = \int_X \frac{(P^l_{A_1} n_{Ax} + P^u_C n_{Cx}) y_{112x} - (P^l_{A_1} n_{Ax} + P^u_C n_{Nx}) y_{012x} - P^u_C (n_{Nx} + n_{Cx}) y_{011x} + P^u_C n_{Nx} (y_{022x} + y_{101x})}{P^u_C n_{Cx}} P(X|C)dx \tag{55}$$

$$FLATE^2 = \int_X \frac{(P^u_{A_1} n_{Ax} + P^l_C n_{Cx}) y_{112x} - (P^u_{A_1} n_{Ax} + P^l_C n_{Nx}) y_{012x} - P^l_C (n_{Nx} + n_{Cx}) y_{011x} + P^l_C n_{Nx} (y_{022x} + y_{101x})}{P^l_C n_{Cx}} P(X|C)dx \tag{56}$$

$FLATE$ lies between $FLATE^1$ and $FLATE^2$.

However, for $BLATE$, we do not have a good monotonic property for P_{A_1}. That is, the bound of P_{A_1} is not necessarily the bound of $BLATE$. We check for several different grids to see how $BLATE$ changes with different choices of P_{A_1}. Our main conclusion holds in all situations. We discuss how to choose the bounds reasonably in the estimation in Appendix C.6.

C.6 Bound Estimation

In this subsection, we retreat from the attempt to obtain the point estimate of the treatment effect and turn to bound estimation. One challenge is that we need to choose the bounds for P_{A_1}.[25]

The most straightforward bound is that $P_0 \leq P_{A_1} \leq 1$, and $P_0 \leq P_C \leq 1$. It means that the selection rate should be larger than the natural sex ratio and smaller than 1, which leads to a very simple linear programming problem.

$$\begin{cases} n(z=1,b=1,c=2) = (1-P_0)P_{A_1}n_A + (1-P_0)P_C n_C \\ P_0 \leq P_{A_1} \leq 1 \\ P_0 \leq P_C \leq 1 \end{cases} \tag{57}$$

We can substitute $\hat{P}_0$, $\hat{n}_A$, and $\hat{n}_C$ which are estimated using Equations (33)-(36) and solve for a bound for P_{A_1} and P_C. The resulting bounds are as follows:

$$\begin{cases} 0.532 \leq P_{A_1} \leq 0.871 \\ 0.532 \leq P_C \leq 0.980 \end{cases} \tag{58}$$

As we can see from Proposition 4, the estimate of $BLATE_C$ contains a term involving $(1-P_C)$ in the denominator. Therefore, if P_C is too close to 1, this term will be magnified and exploded. To find a meaningful bound for our targeted treatment effect, we need additional criteria. One of the conditions we can add is $P_{A_1} \geq P_C$, which claims that always-takers are those who care less about government rules. Therefore, always-takers are more likely to use forbidden gender

[25] In this subsection, we still approximate the conditional probabilities directly by unconditional counts.

detection technologies and do gender selection. As a consequence, the bounds are as follows:

$$\begin{cases} 0.725 \leq P_{A_1} \leq 0.871 \\ 0.532 \leq P_C \leq 0.725 \end{cases} \tag{59}$$

This restriction seems to be too strong and shrinks the bounds too much. When conducting estimations, we relax the upper bound of P_C to 0.9. In the meantime, as discussed in Section 4.6.4, the effect of the child gender is not a monotonic function of P_{A_1}, so we will do more estimations with different P_{A_1} in Appendix C.7 to check the robustness.

C.7 Results for Bound Estimation

The bounds we choose for the two selection probabilities are the ones we mentioned in Appendix C.6.[26]

$$\begin{cases} 0.593 \leq P_{A_1} \leq 0.871 \\ 0.532 \leq P_C \leq 0.9 \end{cases} \tag{60}$$

Estimation results for P_{A_1} upper bound (correspondingly, P_C lower bound) and P_{A_1} lower bound (correspondingly, P_C upper bound) are shown in Tables C1 and C2, respectively. Considering the effect of the number of children, we can see that the estimates are smaller when P_{A_1} achieves its upper bound. All the estimates are still more substantial than those in the simple IV estimation (Tables 4 and B2). The estimates of the effect of the gender of children are still positive and comparable with the effect of the number of children in terms of absolute magnitude for both the upper bound and lower bound estimation. In general, the conclusions we draw from the point estimation still hold in the bound estimation.

The monotonicity of the effect of the number of children guarantees that for any reasonable selection probability we may choose, we can always claim that the simple IV estimates are downward biased. However, concerning the effect of the gender of children, we need to check more choices of P_{A_1} to see the possible estimates of $BLATE$. We show the estimates of our

[26] Here we relax the upper bound of P_C to be 0.9 rather than 0.725 to make the bounds more flexible.

model when P_{A_1} equals to 0.8, 0.75, 0.7, and 0.65, respectively (Table C3-C5).

Even though we cannot analytically prove the monotonicity of the child gender effect in terms of P_{A_1}, the monotonicity seems to hold not only for the fertility effect but also for the child gender effect in our sample, according to the estimation results in Table C3-C5. (The child gender effect for always-takers will not be changed when we vary P_{A_1} because it is not a function of P_{A_1}). According to the results, we can safely conclude that all the main conclusions in the main text are valid no matter which value we use for P_{A_1}.

Table C1: Semi-parametric Estimation Results with $P^u_{A_1} = 0.871$

	Both	Male	Female
$FLATE$	0.0126	0.0215	-0.0003
	(0.0128)	(0.0164)	(0.0141)
$BLATE_C$	0.0083	0.0030	0.0084
	(0.0098)	(0.0133)	(0.0108)
$BLATE_A$	0.0155***	0.0139**	0.0102*
	(0.0052)	(0.0064)	(0.0057)
$BLATE$	0.0130**	0.0102	0.0095
	(0.0055)	(0.0071)	(0.0061)
Number of Observations	56,794	56,794	56,794

Notes: All estimations are conducted with $P^u_{A_1} = 0.871$. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table C2: Semi-parametric Results with $P^l_{A_1} = 0.593$

	Both	Male	Female
$FLATE$	0.0113	0.0253**	0.0015
	(0.0079)	(0.0099)	(0.0085)
$BLATE_C$	-0.0296	0.0009	0.0198
	(0.0445)	(0.0568)	(0.0502)
$BLATE_A$	0.0155***	0.0139**	0.0102*
	(0.0052)	(0.0064)	(0.0057)
$BLATE$	-0.0001	0.0094	0.0135
	(0.0126)	(0.0163)	(0.0143)
Number of Observations	56,794	56,794	56,794

Notes: All estimations are conducted with $P^u_{A_1} = 0.593$. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table C3: Semi-parametric Estimation Results for Both Parents' Migration with Different P_{A_1}

P_{A_1}	0.871 (Upper Bound)	0.8	0.787 ($\hat{P}_A$)	0.75	0.7	0.65	0.593 (Lower Bound)
$FLATE$	0.0126	0.0121	0.0120	0.0119	0.0117	0.0115	0.0113
	(0.0128)	(0.0109)	(0.0104)	(0.0100)	(0.0092)	(0.0085)	(0.0079)
$BLATE_C$	0.0083	0.0053	0.0040	0.0024	-0.0021	-0.0095	-0.0296
	(0.0098)	(0.0084)	(0.0086)	(0.0095)	(0.0132)	(0.0213)	(0.0445)
$BLATE_A$	0.0155***	0.0155***	0.0155***	0.0155***	0.0155***	0.0155***	0.0155***
	(0.0053)	(0.0052)	(0.0052)	(0.0052)	(0.0052)	(0.0052)	(0.0052)
$BLATE$	0.0130**	0.0120***	0.0115***	0.0110***	0.0094**	0.0069	-0.0001
	(0.0055)	(0.0043)	(0.0039)	(0.0035)	(0.0034)	(0.0051)	(0.0126)
Number of Observations	56,794	56,794	56,794	56,794	56,794	56,794	56,794

Notes: Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table C4: Semi-parametric Estimation Results for father's Migration with Different P_{A_1}

P_{A_1}	0.871 (Upper Bound)	0.8	0.787 ($\hat{P}_A$))	0.75	0.7	0.65	0.593 (Lower Bound)
$FLATE$	0.0215	0.0229*	0.0233*	0.0236*	0.0242**	0.0248**	0.0253**
	(0.0164)	(0.0139)	(0.0132)	(0.0126)	(0.0116)	(0.0107)	(0.0099)
$BLATE_C$	0.0030	0.0040	0.0042	0.0043	0.0043	0.0036	0.0009
	(0.0133)	(0.0116)	(0.0119)	(0.0129)	(0.0176)	(0.0276)	(0.0568)
$BLATE_A$	0.0139**	0.0139**	0.0139**	0.0139**	0.01389**	0.0139**	0.0139**
	(0.0064)	(0.0064)	(0.0064)	(0.0064)	(0.0064)	(0.0064)	(0.0064)
$BLATE$	0.0102	0.0105*	0.0106**	0.0106**	0.0106**	0.0104	0.0094
	(0.0071)	(0.0057)	(0.0052)	(0.0048)	(0.0048)	(0.0069)	(0.0163)
Number of Observations	56,794	56,794	56,794	56,794	56,794	56,794	56,794

Notes: Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table C5: Semi-parametric Estimation Results for mother's Migration with Different P_{A_1}

P_{A_1}	0.871 (Upper Bound)	0.8	0.787 ($\hat{P}_A$))	0.75	0.7	0.65	0.593 (Lower Bound)
$FLATE$	-0.0003	0.0003	0.0005	0.0007	0.0010	0.0012	0.0015
	(0.0141)	(0.0120)	(0.0113)	(0.0108)	(0.0099)	(0.0092)	(0.0085)
$BLATE_C$	0.0084	0.0097	0.0101	0.0107	0.0121	0.0142	0.0198
	(0.0108)	(0.0091)	(0.0093)	(0.0103)	(0.0146)	(0.0238)	(0.0502)
$BLATE_A$	0.0102*	0.0102*	0.0102*	0.0102*	0.0102*	0.0102*	0.0102*
	(0.0057)	(0.0057)	(0.0057)	(0.0057)	(0.0057)	(0.0057)	(0.0057)
$BLATE$	0.0095	0.0100**	0.0102**	0.0103**	0.0108***	0.0116**	0.0135
	(0.0061)	(0.0047)	(0.0042)	(0.0038)	(0.0036)	(0.0057)	(0.0143)
Number of Observations	56,794	56,794	56794	56,794	56,794	56,794	56,794

Notes: Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

C.8 The Failure of Assumption 3

In this section, we investigate the robustness of Assumption 3, which states that there is no endogenous gender selection after conditioning on observed variables X and fertility behavioral type. We will discuss what the implication is if Assumption 3 fails.

Considering the situation in which Assumption 3 fails for always-takers and compliers with a first-born girl, that is, even if we control for observed characteristics X, always-takers/compliers with a first-born girl and a second boy, are different from those with a first-born girl and a second girl in some unobserved factors in the migration decision equation. For Chinese households, we assume that $Cov(\eta_i, \varepsilon_i|A,X) < 0$ and $Cov(\eta_i, \varepsilon_i|C,X) < 0$, which means that households with a stronger preference for boys are those with weaker migration preference. For instance, families with more traditional values may be less likely to move to cities and more likely to have boys.

In the context of a negative correlation between η_i and ε_i, households with lower migration preferences are more likely to conduct gender selection to have a second boy. As a result, we have:

$$E[Mig_{i21}|A, z=1, b=1, c=2, X] < E[Mig_{i21}|A, X] \tag{61}$$

$$E[Mig_{i20}|A, X] < E[Mig_{i20}|A, z=1, b=0, c=2, X] \tag{62}$$

$$E[Mig_{i21}|C, z=1, b=1, c=2, X] < E[Mig_{i21}|C, X] \tag{63}$$

$$E[Mig_{i20}|C, X] < E[Mig_{i20}|C, z=1, b=0, c=2, X] \tag{64}$$

With the four inequalities above, we can determine the sign of the biases in the estimated conditional expectations of the potential migration decisions for different types. Considering Equations (25)-(30), without Assumption 3, the first four equations are unchanged,[27] which means that we can correctly identify $E[Mig_{21}|A,X]$ and $E[Mig_{20}|A,X]$. The last two equations will be altered because $E[Mig_{icb}|z_i, b_i, c_i, Type_i] = E[Mig_{icb}|Type_i]$ does not hold for nodes

[27] For $E[Mig_{i21}|A,X]$, it can be identified by y_{012x}; for $E[Mig_{i22}|A,X]$, it can be identified by y_{022x}; and for $E[Mig_{i20}|A,X]$, it can be identified by the difference between y_{022x} and y_{012x}. The same logic applies to $E[Mig_{i11}|N,X], E[Mig_{i11}|Co,X]$ and $E[Mig_{i10}|N,X]$. Thus the bias only happens to $E[Mig_{i21}|C,X]$ and $E[Mig_{i20}|C,X]$.

(z=1,b=1,c=2) and (z=1,b=0,c=2) due to the gender selection at the second birth. Equations (29) and (30) are altered respectively as follows:

$$
\begin{aligned}
y_{102x} =& \frac{(1-P_{A_1x})n_{Ax}}{(1-P_{A_1x})n_{Ax}+(1-P_{Cx})n_{Cx}} E[Mig_{i20}|A,z=1,b=0,c=2,X] \\
&+ \frac{(1-P_{Cx})n_{Cx}}{(1-P_{A_1x})n_{Ax}+(1-P_{Cx})n_{Cx}} E[Mig_{i20}|C,z=1,b=0,c=2,X], \qquad (65)\\
y_{112x} =& \frac{P_{A_{1x}}n_{Ax}}{P_{A_1x}n_{Ax}+P_{Cx}n_{Cx}} E[Mig_{i21}|A,z=1,b=1,c=2,X] \\
&+ \frac{P_{Cx}n_{Cx}}{P_{A_1x}n_{Ax}+P_{Cx}n_{Cx}} E[Mig_{i21}|C,z=1,b=1,c=2,X]. \qquad (66)
\end{aligned}
$$

Therefore, if we still solve for the conditional expectations of the potential migration decisions for compliers using Equation (29) and (30), we will mistakenly identify $E[Mig_{i21}|C,X]$ as follows:

$$
\frac{(P_{A_1x}n_{Ax}+P_{Cx}n_{Cx})y_{112x}-P_{A_{1x}}n_{Ax}E[Mig_{i21}|A,X]}{P_{Cx}n_{Cx}}, \qquad (67)
$$

(48) is smaller than $E[Mig_{i21}|C,z=1,b=1,c=2,X]$, which can be identified as below:

$$
\frac{(P_{A_1x}n_{Ax}+P_{Cx}n_{Cx})y_{112x}-P_{A_1x}n_{Ax}E[Mig_{i21}|A,z=1,b=1,c=2,X]}{P_{Cx}n_{Cx}}. \qquad (68)
$$

Given that Inequality (63) holds, we conclude that (67)<(68)<$E[Mig_{i21}|C,X]$. Therefore, $E[Mig_{i21}|C,X]$ will be downward biased. The same logic can be applied when identifying $E[Mig_{i20}|C,X]$ but in the opposite direction, which results in an upward bias. In total, we will have a downward bias in the identification of $FLATE$ and $BLATE$, according to Equations (10) and (13).

As all the estimates are positive under Assumption 3, the real effect of the number of children and the effect of the gender of children will be more positive. All the main qualitative conclusions remain valid.

D Supplementary Results for Robustness Checks

D.1 Robustness Checks for Sample Restriction

D.1.1 External Validity for All Provinces

We focus on communities subject to the 1.5-child policy because the assumption of the randomness of first-child gender is more reliable in these provinces. To address the concern that policy adoption may be systematically related to province-level demographic or socioeconomic conditions, we regress an indicator of whether a province implemented the 1.5-child policy on key provincial characteristics. The independent variables include GDP, GDP growth rate, industrial structure (shares of primary and secondary sectors), population size, rural population share, and birth rate. The results, reported in Table D1, show that none of these variables are statistically significant, and an F-test indicates joint insignificance. This evidence alleviates concerns that the implementation of the 1.5-child policy was systematically driven by provincial demographic or socioeconomic conditions. Nonetheless, we cannot fully rule out the influence of unobserved factors, so we conduct additional robustness checks below.

Finally, to address the concern regarding the external validity of our findings, we conduct an additional analysis using a broader sample that includes all provinces, regardless of whether they implemented the 1.5-child policy. Table D2 shows that the results remain robust in this expanded sample, suggesting that our main conclusions are not solely driven by the specific characteristics of the 1.5-child policy provinces.

D.1.2 Married Couples

We restrict our sample to married couples, because the divorce rate in China during our study period (the 2000s) was extremely low. According to official statistics, the national divorce rate remained between 0.9‰ and 2.0‰ with an average of around 1.4‰ over the decade. For

Table D1: Correlations of 1.5-Child Policy Implementation with Macro Indices

Dependent Var.	Whether Implemented the 1.5-Child Policy (=1)		
	(1)	(2)	(3)
GDP	0.001	0.000	-0.001
	(0.001)	(0.001)	(0.002)
GDP Growth Rate	0.029	0.002	0.011
	(0.027)	(0.031)	(0.033)
Population		-0.000	-0.000
		(0.000)	(0.000)
Rural Population Ratio		0.000	0.000
		(0.000)	(0.000)
Birthrate		0.013	0.039
		(0.035)	(0.043)
Share of Primary Industry in GDP			0.034
			(0.029)
Share of Secondary Industry in GDP			0.041
			(0.033)
P-value of the F-test	0.4061	0.3430	0.4220
Number of Observations	31	31	31
R-squared	-0.0046	0.0305	0.0128

Notes: The dependent variable is a binary indicator for whether the 1.5-child policy was implemented in a given province, while the independent variables capture the province's demographic and socioeconomic characteristics. Data on the independent variables come from *A Compilation of Statistical Information on the Six Decades of New China*, which we manually compiled. All variables are averaged over 1980–1983, because starting in 1984 some provinces began implementing the 1.5-child policy. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table D2: Semi-parametric Estimation Results with All Provinces

	Both	Male	Female
	(1)	(2)	(3)
$FLATE$	0.0142	0.0313**	0.0022
	(0.0116)	(0.0144)	(0.0126)
$BLATE_C$	0.0053	0.0034	0.0153
	(0.0106)	(0.0144)	(0.0116)
$BLATE_A$	0.0123**	0.0140**	0.0074
	(0.0053)	(0.0065)	(0.0057)
$BLATE$	0.0102***	0.0107**	0.0098***
	(0.0033)	(0.0046)	(0.0035)
Number of Observations	71,653	71,653	71,653

Notes: We consider all provinces in this table. All estimations are conducted with estimated selection probability. Column (1) reports the semi-parametric estimation results when the dependent variable is *Both Parents' Migration* for $FLATE$, $BLATE_C$, $BLATE_A$, and $BLATE$, respectively. Column (2) reports the semi-parametric estimation results when the dependent variable is *Father's Migration*. Column (3) reports the semi-parametric estimation results when the dependent variable is *Mother's Migration*. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

instance, in 2000, the divorce rate was only 1.0‰ and even by 2010, it was still as low as 2.0‰. As such, restricting the sample to married couples does not introduce significant selection bias and is unlikely to undermine the validity of our migration analysis.

D.1.3 Relaxing Survival and Age Restrictions on Children

We note that child mortality rates in China were very low during the 2000s. According to the data from the World Bank, the mortality rate of children under five was 1.6% in 2010.[28] As a result, households where the number of children ever born differs from the number of children alive constitute a very small fraction of the sample. Excluding these households does not materially affect the estimation results. Nevertheless, to further assess the robustness of our findings, we re-estimate our main models without imposing this restriction. The results, presented in Table D3, confirm that our findings remain robust.

To alleviate concerns about sample selection based on child age, we conduct an additional robustness check by relaxing the age restriction on the first child from 18 to 25. This broader sample includes families with older children who are still listed in the hukou record and helps ensure that our findings are not driven by the censoring of older children. Again, as shown in Table D4, our core results remain robust, reinforcing the reliability of our identification strategy.

To further examine if the fourth and fifth restrictions lead to a biased sample, we conduct a robustness check with a sample in which both parents are aged between 35 and 40 years old. In this sample, households in which all children are 18 years old or younger account for more than 95% of all households; households in which the number of children living in the household is equal to the number of children ever born account for 97.5% of all households in the sample. Therefore, these two restrictions should have a minor effect on the estimates using this robustness-check sample. In this setting, our main conclusions still hold. The results of the robustness check are available upon request.

[28] Please refer to https://data.worldbank.org/indicator/SH.DYN.MORT?locations=CN

Table D3: Semi-parametric Estimation Results without Restricting the Number of Children Ever Born to Equal the Number of Surviving Children

	Both	Male	Female
	(1)	(2)	(3)
$FLATE$	0.0099	0.0200	-0.0008
	(0.0108)	(0.0148)	(0.0112)
$BLATE_C$	0.0043	0.0031	0.0101
	(0.0092)	(0.0131)	(0.0096)
$BLATE_A$	0.0150***	0.0134**	0.0094**
	(0.0051)	(0.0065)	(0.0055)
$BLATE$	0.0113***	0.0099**	0.0097***
	(0.0039)	(0.0056)	(0.0040)
Number of Observations	57,481	57,481	57,481

Notes: We expand the sample by not restricting the number of children ever born to equal the number of surviving children. All estimations are conducted with estimated selection probability. Column (1) reports the semi-parametric estimation results when the dependent variable is *Both Parents' Migration* for $FLATE$, $BLATE_C$, $BLATE_A$, and $BLATE$, respectively. Column (2) reports the semi-parametric estimation results when the dependent variable is *Father's Migration*. Column (3) reports the semi-parametric estimation results when the dependent variable is *Mother's Migration*. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table D4: Semi-parametric Estimation Results with First-born Child less than 25

	Both	Male	Female
	(1)	(2)	(3)
$FLATE$	-0.0020	0.0080	-0.0070
	(0.0076)	(0.0099)	(0.0086)
$BLATE_C$	0.0069	0.0014	0.0137*
	(0.0073)	(0.0094)	(0.0080)
$BLATE_A$	0.0051*	0.0076**	0.0018
	(0.0029)	(0.0038)	(0.0033)
$BLATE$	0.0056**	0.0057*	0.0054**
	(0.0024)	(0.0032)	(0.0027)
Number of Observations	106,712	106,712	106,712

Notes: We expand the sample by extending the age cutoff for the first child from 18 to 25. All estimations are conducted with estimated selection probability. Column (1) reports the semi-parametric estimation results when the dependent variable is *Both Parents' Migration* for $FLATE$, $BLATE_C$, $BLATE_A$, and $BLATE$, respectively. Column (2) reports the semi-parametric estimation results when the dependent variable is *Father's Migration*. Column (3) reports the semi-parametric estimation results when the dependent variable is *Mother's Migration*. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

D.1.4 Definition for Rural-to-urban Migration

The 2010 census collects information on individuals both at their current place of residence (chang zhu di) and their hukou registration location (hu ji di). This dual-location structure allows us to identify migration status based on discrepancies between current residence and hukou registration. Specifically, we rely on Questions R6 and R7 from the census questionnaire. Question R6 records the respondent's place of residence at the time of the census, while Question R7 records the location of the respondent's hukou registration. First, we retain only households surveyed in their hukou registration location, that is, those with $R7 = 1$. Then, we define an individual as a migrant if $R6 \neq 1$ and $R6 \neq 2$ (that is, they are currently residing in locations outside their hukou-registered village).

However, the census only provides detailed migration destination codes at the county level. We do not have enough information to classify villages into urban/rural categorizations. Therefore, in our main analysis, we define migrants as individuals holding rural hukou who are currently not residing in their hukou village (i.e., $R6 \neq 1$ and $R6 \neq 2$). This broad definition inevitably includes some rural-to-rural migrants, which could raise concerns about the precision of our identification strategy.

To address this concern, we conduct robustness checks by narrowing the definition to more strictly capture rural-to-urban migration. Specifically, we identify migrants who (1) hold rural hukou, (2) report $R6 \neq 1$ and $R6 \neq 2$ (currently residing in locations outside hukou village), and (3) whose destination is classified as urban based on the 6-digit county-level administrative code of the current residence. We link these codes to the official region name list and classify the destination as urban if the county name contains the labels "qu" (district) or "shi" (city), and as rural otherwise (e.g., "xian" (county) or "qi" (banner)). Our results remain robust under this more restrictive definition, as shown in Table D5.

Table D5: Robustness analysis: New Definition of Rural-to-urban Migrants

	Both	Male	Female
	(1)	(2)	(3)
$FLATE$	0.0039	0.0299*	-0.0002
	(0.0095)	(0.0128)	(0.0099)
$BLATE_C$	0.0104	0.0051	0.0126
	(0.0080)	(0.0103)	(0.0087)
$BLATE_A$	0.0071	0.0131**	0.0054
	(0.0049)	(0.0068)	(0.0052)
$BLATE$	0.0082**	0.0104**	0.0079**
	(0.0034)	(0.0044)	(0.0036)
Number of Observations	53,842	54,336	55,003

Notes: We exclude the rural-to-rural migration. All estimations are conducted with estimated selection probability. Column (1) reports the semi-parametric estimation results when the dependent variable is *Both Parents' Migration* for $FLATE$, $BLATE_C$, $BLATE_A$, and $BLATE$, respectively. Column (2) reports the semi-parametric estimation results when the dependent variable is *Father's Migration*. Column (3) reports the semi-parametric estimation results when the dependent variable is *Mother's Migration*. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

D.2 Controlling for Migration Timing

The dynamics of the migration and fertility decisions are important and that the validity of our results should be tested accordingly. The Census does provide some coarse but informative data on individual migration duration. Specifically, it includes categorical variables indicating how long a person has been living at their current place of residence, with categories such as: less than 6 months, 6 months–1 year, 1–2 years, 2–3 years, 3–4 years, 4–5 years, 5–6 years, and over 6 years. We use this information to construct a more refined subsample in which we restrict attention to families where the migration of the parents occurred after the birth of the youngest child. This restriction helps mitigate concerns that fertility decisions may have been made after, migration. In doing so, we aim to isolate households where fertility decisions were likely made prior to migration, thereby reducing potential bias from reverse causality. That is, we focus our analysis on migration decisions that follow completed fertility behavior, rather than households in which migration could have influenced decisions regarding having additional children. Reassuringly, when we re-estimate our main models using this more restricted sample in Table D6, our findings remain robust.

To further address the dynamic nature of the migration decision, we conducted a set of heterogeneity analyses by dividing the main sample into four subsamples based on whether the household has at least one child (either the first or second child) in the age ranges of 0–6, 7–12, 13–15, or 16–18. This grouping strategy allows us to focus more precisely on the influence of children's developmental stages. The estimation results are presented in Table D7. In the 0–6 age group, neither the boy effect nor the fertility effect is statistically significant, consistent with the idea that parents are less likely to migrate when their children are very young and require intensive care. However, starting with the 7–12 age group, we find a statistically significant and positive boy effect on parental migration, which continues through the 13–15 and 16–18 age groups. In contrast, the fertility effect becomes significant only in the 16–18 group, suggesting that the overall migration response to having more children emerges primarily when children are older and more self-sufficient. To further rule out the potential influence of preschool-aged children on parental migration decisions—given that younger children may impose greater caregiving demands—we further restrict the sample to households in which all children are older than age 6. The results shown in Panel E of Table D7 remain robust under this restriction, confirming that our main findings are not driven by the presence of very young children in the household.

Table D6: Semi-parametric Estimation Results Accounting for Migration Duration

	Both	Male	Female
	(1)	(2)	(3)
$FLATE$	0.0127	0.0226*	0.0011
	(0.0102)	(0.0131)	(0.0110)
$BLATE_C$	0.0060	0.0084	0.0130
	(0.0085)	(0.0116)	(0.0093)
$BLATE_A$	0.0160***	0.0137**	0.0104**
	(0.0051)	(0.0063)	(0.0056)
$BLATE$	0.0126***	0.0119**	0.0113***
	(0.0085)	(0.0051)	(0.0041)
Number of Observations	56,608	56,459	56,604

Notes: We consider parents making migration decisions after the birth of their last child. All estimations are conducted with estimated selection probability. Column (1) reports the semi-parametric estimation results when the dependent variable is *Both Parents' Migration* for $FLATE$, $BLATE_C$, $BLATE_A$, and $BLATE$, respectively. Column (2) reports the semi-parametric estimation results when the dependent variable is *Father's Migration*. Column (3) reports the semi-parametric estimation results when the dependent variable is *Mother's Migration*. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table D7: Semi-parametric Estimation Results with Estimated Selection Probability, by Child's Age

	Both	Male	Female
	(1)	(2)	(3)
	Panel A: At Least One Child Aged between 0-6		
$FLATE$	0.0941	0.3699	0.1419
	(0.6821)	(1.0301)	(0.7044)
$BLATE$	0.0046	0.0016	0.0097
	(0.0073)	(0.0089)	(0.0077)
# of Obs.	12,752	12,752	12,752
	Panel B: At Least One Child Aged between 7-12		
$FLATE$	-0.0079	-0.0037	-0.0201
	(0.0198)	(0.0260)	(0.0210)
$BLATE$	0.0104**	0.0061	0.0070
	(0.0050)	(0.0065)	(0.0054)
# of Obs.	25,581	25,581	25,581
	Panel C: At Least One Child Aged between 13-15		
$FLATE$	0.0045	-0.0044	-0.0209
	(0.0180)	(0.0218)	(0.0193)
$BLATE$	0.0170***	0.0124*	0.0146**
	(0.0061)	(0.0081)	(0.0063)
# of Obs.	24,665	24,665	24,665
	Panel D: At Least One Child Aged between 16-18		
$FLATE$	0.0212	0.0375*	0.0194
	(0.0147)	(0.0204)	(0.0164)
$BLATE$	0.0094	0.0156**	0.0086
	(0.0059)	(0.0074)	(0.0062)
# of Obs.	23,818	23,818	23,818
	Panel E: All Children Aged Above 5		
$FLATE$	0.0166	0.0285**	0.0028
	(0.0115)	(0.0144)	(0.0125)
$BLATE$	0.0144***	0.0163***	0.0096*
	(0.0052)	(0.0064)	(0.0058)
# of Obs.	44,042	44,042	44,042

Notes: We analyze the effect by children's age. All estimations are conducted with estimated selection probability. Column (1) reports the semi-parametric estimation results when the dependent variable is *Both Parents' Migration* for $FLATE$, $BLATE_C$, $BLATE_A$, and $BLATE$, respectively. Column (2) reports the semi-parametric estimation results when the dependent variable is *Father's Migration*. Column (3) reports the semi-parametric estimation results when the dependent variable is *Mother's Migration*. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

E Detailed Feature Set

The feature set X_A consists of 8,573 variables, largely due to the expansion of categorical variables—particularly high-dimensional county-level fixed effects. These variables span three key dimensions that are central to fertility behavior: (1) Parental demographics: parents' ages, years of marriage, and education level fixed effects for both parents; (2) Family composition: number of co-residing parents and parents-in-law, as well as the number of siblings; (3) Geographic information: fixed effects for parents' hukou registration counties, survey counties, and current counties of residence, along with parents' birth provinces. This comprehensive set of variables captures important sources of heterogeneity in fertility preferences and behavior. For instance, geographic fixed effects flexibly absorb local variation in policy enforcement and cultural norms; family structure variables proxy for intergenerational influences and traditional gender roles; and individual demographics reflect life-cycle stage and socioeconomic status. Together, these dimensions ensure that the model is equipped with rich information to construct an effective predictor of fertility behavior.

We now provide more information on prediction performance and on the variables included in the model. In total, 1,415 variables enter the final prediction model. The most influential predictors are the location-related fixed effects—specifically, hukou registration counties, current residence counties, and survey counties—which contribute substantially to predictive power. Other important predictors include parental education and the number of co-residing older-generation family members.

F Migrant and Left-behind Children

In China, migrant children and left-behind children are important social issues. Migrant children refer to children who migrate together with their parents, while left-behind children refer to those whose parents migrate for work, but who remain at home, separated from their parents. These children are typically cared for by grandparents or other relatives. According to existing studies, during the 2000s, the majority of children in migrant families were left-behind children, accounting for approximately two-thirds of the total (Huang and Zhang, 2022). In the main analysis, we abstract from this distinction and focus solely on parents' migration decisions. In this section, we further extend both our conceptual model and empirical analysis to explicitly consider families with migrant children and left-behind children separately.

F.1 Extended Model

We extend our conceptual model to incorporate children's migration choices and to distinguish between fathers' and mothers' migration. The basic framework remains unchanged, except that households now face five possible alternatives: (1) both parents and children migrate, resulting in a migrant child (denoted as *mig*); (2) both father and mother migrate, but children stay behind (denoted as *left*); (3) only the father migrates (denoted as *f_mig*); (4) only the mother migrates (denoted as *m_mig*); and (5) neither parents nor children migrate (denoted as *stay*).

The utility function can then be written as follows:

$$
\begin{aligned}
u_i(w_{jm}, w_{jf}, c_i, b_i, \tau_{ij}, \varepsilon_{ij}) = & w_{jm} + w_{jf} + \beta_1 n_i + \beta_2 b_i + \gamma_1 (w_{jm} + w_{jf}) n_i + \gamma_2 (w_{jm} + w_{jf}) b_i \\
& + \gamma_3 \mathbf{1}(j = \text{mig}) \cdot n_i + \gamma_4 \mathbf{1}(j = \text{mig}) \cdot b_i \\
& + \gamma_5 \mathbf{1}(j = \text{left}) \cdot n_i + \gamma_6 \mathbf{1}(j = \text{left}) \cdot b_i \\
& + \gamma_7 \mathbf{1}(j = \text{f\_mig}) \cdot n_i + \gamma_8 \mathbf{1}(j = \text{f\_mig}) \cdot b_i \\
& + \gamma_9 \mathbf{1}(j = \text{m\_mig}) \cdot n_i + \gamma_{10} \mathbf{1}(j = \text{m\_mig}) \cdot b_i \\
& + \tau_{ij} + \varepsilon_{ij}
\end{aligned}
$$

Here, w_{jm} denotes the mother's wage and w_{jf} the father's wage. γ_3 and γ_4 capture the additional costs associated with migrating with children/boys—for example, higher parenting time and living expenses in urban areas compared to rural areas. γ_5 and γ_6 capture the separation costs when parents migrate but children stay behind, reflecting the lack of parental care. γ_7 and γ_8 capture the separation costs when only the father migrates, while γ_9 and γ_{10} capture the corresponding costs when only the mother migrates. Other components remain the same as in the simple model in the main context.

We can then derive the marginal effects by taking derivatives. For instance, for the case of $j = \text{mig}$, we obtain:

$$
\begin{aligned}
\frac{\partial P_{i,\text{mig}}}{\partial n_i} = & P_{i,\text{mig}} \cdot \Big[P_{i,\text{left}} \cdot (\gamma_3 - \gamma_5) + P_{i,\text{f\_mig}} \cdot \big(\gamma_1 (w_{\text{mig},m} - w_{\text{home},m}) + \gamma_3 - \gamma_7 \big) \\
& + P_{i,\text{m\_mig}} \cdot \big(\gamma_1 (w_{\text{mig},f} - w_{\text{home},f}) + \gamma_3 - \gamma_9 \big) \\
& + P_{i,\text{stay}} \cdot \big(\gamma_1 (w_{\text{mig},f} - w_{\text{home},f} + w_{\text{mig},m} - w_{\text{home},m}) + \gamma_3 \big) \Big]
\end{aligned}
\tag{69}
$$

Equation (69) is complicated, but the underlying logic is similar to our simple model. Since $P_{i,mig}$ is positive, the overall effect of the number of children on the probability of both parents and children migrating depends on the sign of the bracketed expression. This sign, in turn, reflects the tradeoff between the gains in parents' wages and the losses from migration/separation costs when comparing $j = \text{mig}$ with each of the other alternatives.

In the first term $P_{i,left} \cdot (\gamma_3 - \gamma_5)$, we compare the case where both parents and children migrate (j = mig) with the case where parents migrate but children stay behind (j = left). As long as $\gamma_3 > \gamma_5$—meaning that the relative cost of migrating with children is less negative than leaving them behind—an increase in the number of children raises the probability of both parents and children migrating relative to the parents-only migration case. Here, $P_{i,left}$ serves as the weight of this alternative.

In the second term, we compare the scenario in which both parents and children migrate (j = mig) with the case where only the father migrates (j = f_mig). Whether the probability of j = mig increases relative to j = f_mig depends on the relative magnitudes of the wage effect, $\gamma_1(w_{mig,m} - w_{home,m})$, and the corresponding cost difference, $(\gamma_3 - \gamma_7)$. The additional wage component, $\gamma_1(w_{mig,m} - w_{home,m})$, arises because in the j = f_mig scenario the mother remains at home, earning a lower wage.

In the third term, we compare the case where both parents and children migrate (j = mig) with the scenario in which only the mother migrates (j = m_mig). Whether j = mig becomes more likely relative to j = m_mig depends on the relative magnitude of the wage effect, $\gamma_1(w_{mig,f} - w_{home,f})$, and the corresponding cost difference, $(\gamma_3 - \gamma_9)$. The wage component is now $\gamma_1(w_{mig,f} - w_{home,f})$ because, in this comparison, it is the father—not the mother—who remains at home earning a lower wage.

In the final term, we compare the scenario in which both parents and children migrate (j = mig) with the case where the entire family remains at home (j = stay). Whether the probability of j = mig increases relative to j = stay depends on the tradeoff between the wage effect, $\gamma_1(w_{mig,f} - w_{home,f} + w_{mig,m} - w_{home,m})$, and the cost term, γ_3. The wage component now reflects the foregone earnings of both parents, since in the j = stay scenario both remain at home. The cost term simplifies to γ_3, as staying at home entails no migration or separation costs.

Given the complexity and the largely repetitive structure of the analysis, we do not report the detailed results for the effects of boys and other alternatives. The key takeaway remains consistent with the simple model: the marginal effect of the number and gender of children on parental migration is determined by the tradeoff between wage gains and migration or separation costs.

F.2 Empirical Results for Migrant and Left-behind Children Families

We present our empirical results on parental migration with or without children in Table F1. Column (1) reports results for parents migrating while leaving all children behind. Column (2) reports results for parents migrating while leaving at least one child behind. Column (3) reports results for parents migrating with all children. Column (4) reports results for parents migrating with at least one child.

In general, we find that additional children or boys increase the probability of parental migration without children, suggesting that income gains from migration outweigh the emotional and caregiving costs of separation. In contrast, additional children or boys have little or no significant impact on the probability of migrating with children, indicating that income gains are roughly offset by the extra costs of raising children in urban areas. Thus, our main results are primarily driven by left-behind children.

Specifically, when we define migration as migrant without any children, we find a significant positive effect of fertility on migration, as well as a strong son preference effect—suggesting that having more children, especially sons, increases the likelihood that parents choose to migrate while leaving them behind. This is consistent with a scenario where the cost of separating from children is relatively low compared to the income benefits of urban employment. When we slightly relax the definition to migrant without at least one child, we find qualitatively similar results, with an even larger and more significant fertility effect. This further supports the idea that more children make it more likely for parents to migrate while leaving some or all of them at home.

In contrast, when we shift to defining migration as migrant with all children, we observe a significant negative fertility effect, and the son preference effect remains statistically significant but its magnitude is much smaller. This suggests that as the number of children increases, it becomes increasingly difficult for parents to bring all of them along, likely due to the prohibitive costs of raising multiple children in urban settings—such as schooling, housing, and living expenses. Although migrating with children avoids the emotional burden of separation, the economic costs appear to dominate, deterring such arrangements. Finally, when we define migration as migrant with at least one child, both the fertility and son preference effects become statistically insignificant.

To further explore the potential mechanisms behind the left-behind pattern, we examine the role of the first-born child's gender in shaping parental migration decisions in two-child families. In particular, we test the hypothesis that an older sister may take care of a younger sibling, thereby reducing the need for parents to bring the younger child along. Consistent with this conjecture, our regression results (Table F2) show that having a first-born daughter significantly increases the likelihood that the second child is left behind when parents migrate. This finding suggests that elder sisters can partially substitute for parental caregiving, lowering the cost of leaving younger siblings in the hometown.

Table F1: Semi-parametric Estimation Results with Heterogeneous Migration Types

	Migrate Without Any Children	Migrate Without at Least One Child	Migrate With All Children	Migrate With at Least One Child
	(1)	(2)	(3)	(4)
$FLATE$	0.0145	0.0232***	-0.0128**	-0.0009
	(0.0081)	(0.0090)	(0.0055)	(0.0085)
$BLATE_C$	0.0053	0.0097	-0.0055	0.0009
	(0.0073)	(0.0079)	(0.0052)	(0.0069)
$BLATE_A$	0.0121**	0.0117**	0.0046*	0.0061
	(0.0044)	(0.0049)	(0.0027)	(0.0043)
$BLATE$	0.0098***	0.0110***	0.0012	0.0043
	(0.0028)	(0.0033)	(0.0018)	(0.0029)
# of Obs.	53,384	55,195	52,789	47,832

Notes: We separately examine the effects on migrant and left-behind children. All estimations are conducted using the estimated selection probability. Column (1) reports results for both parents migrating without any of their children (leaving both children behind). Column (2) reports results for both parents migrating without at least one child (leaving at least one child behind). Column (3) reports results for both parents migrating with both of their children. Column (4) reports results for both parents migrating with at least one child. Standard errors in parentheses are estimated using a bootstrap method. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F2: Effect of First-Born Child's Gender on the Likelihood of Parental Migration Without the Second Child

	Male	Female
	(1)	(2)
First Girl (=1)	0.0126*	0.0295**
	(0.0067)	(0.0122)
First Child's Age	-0.0017	0.0016
	(0.0014)	(0.0026)
Father's Years of Schooling	-0.0001	0.0015
	(0.0022)	(0.0040)
Mother's Years of Schooling	0.0000	0.0049
	(0.0019)	(0.0035)
Father's Age	0.0022	0.0019
	(0.0015)	(0.0027)
Mother's Age	-0.0003	-0.0031
	(0.0017)	(0.0032)
Obs.	5,082	2,811

Notes: We restrict the sample to two-child families in which the first child does not migrate with the parents and is older than six. In Column (1), the dependent variable is whether the father migrates without the second child (1 = without, 0 = with). In Column (2), the dependent variable is whether the mother migrates without the second child (1 = without, 0 = with). All regressions control for prefecture fixed effects. Standard errors in parentheses are clustered at the prefecture level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

G Sex Ratio at First Birth

We examine how the sex ratio at first birth varies across birth cohorts in our sample, applying the sample restrictions described in Section 2.2. Specifically, Figure G1 shows the sex ratio for first births in provinces with different policies. We find that in 1.5-child policy provinces, the sex ratio consistently falls within the natural range of about 100–106 boys per 100 girls (Banister, 2004). We focus on children older than 8 because, among parents aged 35–65, the share of those with a firstborn child younger than 8 is very small—less than 4%—and therefore not representative. For this reason, we exclude them from the figures. The figure also shows the corresponding sex ratios for first births in one-child policy provinces under the same sample restrictions, which are more variable and generally higher.

Furthermore, Figure G2 compares the sex ratio at first birth between urban and rural samples within the 1.5-child policy provinces, showing that the urban sex ratio is higher than the rural one. Finally, in Figure G3, we compare our main sample (parents aged 35–65) with a younger cohort (parents aged below 35) from rural households in the same 1.5-child policy provinces, finding that the younger cohort consistently exhibits a higher sex ratio. In Figure G3, we restrict the first child to be younger than 15 because, in China, women can only legally marry at age 20; therefore, for parents aged below 35, firstborn children older than 15 are very rare and not representative.

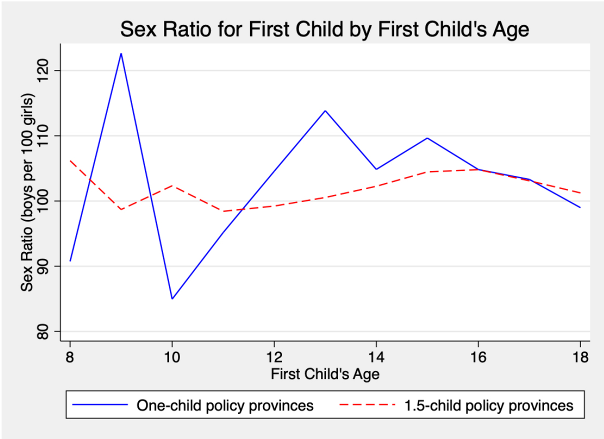

Figure G1: Sex Ratio by Policy Types

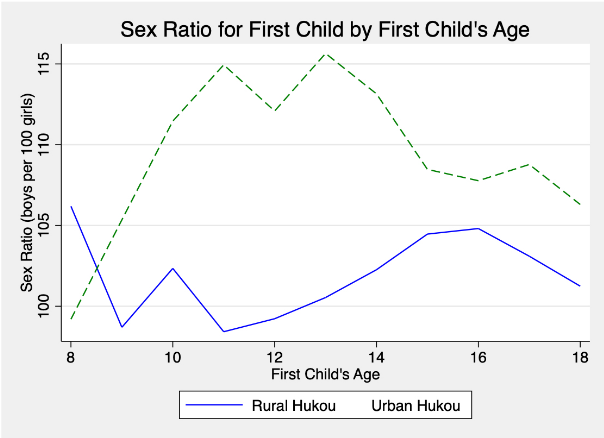

Figure G2: Sex Ratio by Hukou Types

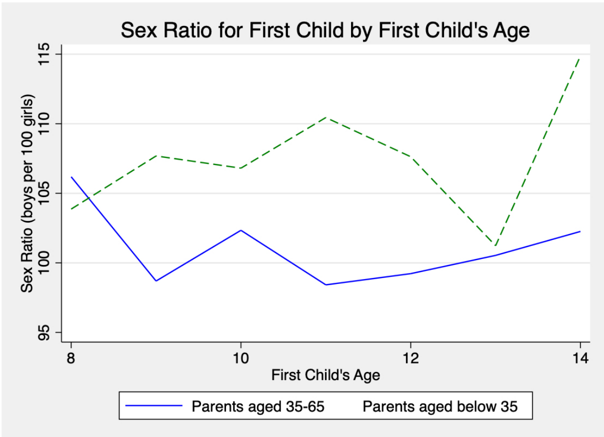

Figure G3: Sex Ratio by Parental Age Groups