Family Planning Policies, Heterogeneous Child Quantity-Quality Trade-off, and Intergenerational Mobility

Yun Xiao *

January 31, 2025

Abstract

This article establishes a causal link between family planning policies and intergenerational mobility, arising from heterogeneity in the trade-off between child quantity and quality. Using variations in penalties for unauthorized births under China's One-Child Policy, I find that the policy reduces the likelihood of having a second child and improves the firstborn's health. However, education increases only among children of high-skilled workers, which further increases their income. In contrast, children of farmers or low-skilled workers accumulate more land or housing assets. The increase in policy-induced penalties accounts for one-fourth of the increases in intergenerational correlations in income between the 1969-1978 and 1979-1988 cohorts. Mechanism analysis suggests that the heterogeneous responses are driven by different perceived returns and costs of education associated with paternal occupations. The results underscore the importance of considering heterogeneity in parental responses when evaluating family planning policies.

Keywords: Human capital, Fertility, Wealth, Child quality-quantity trade-off, Intergenerational mobility.

JEL Codes: J13, J24, J62, I24, Q15

*Xiao: University of Gothenburg, Vasagatan 1, 405 30 Gothenburg, Sweden, yun.xiao@economics.gu.se. I am grateful to Hessel Oosterbeek and Pauline Rossi for their advice and guidance. For their helpful comments, I thank Jere Behrman, Aline Bütikofer, Hanming Fang, Anne Gielen, Erik Plug, Giuseppe Sorrenti, as well as participants to seminars at Aalto University, University of Groningen, University of Gothenburg, Uppsala University, University of Queensland, University of Sussex, Durham University, ifo Munich, Jinan University, Renmin University of China, CREST, University of Pennsylvania, GEEZ, Tinbergen Institute, and University of Amsterdam, and AASLE, EWMES, and EEA conferences. I acknowledge financial support from the Adlerbertska Research Foundation. All errors are my own.

1 Introduction

Family planning policies are widely implemented to reduce fertility rates and manage population growth (de Silva and Tenreyro, 2017). These policies are often believed to promote human capital development, by encouraging a shift from child quantity to child "quality," such as health and education. This concept is rooted in the child quantity-quality (Q-Q) trade-off theory, first introduced by Becker and Lewis (1973). According to this theory, as family size increases, the marginal cost of investing in each child's quality rises, resulting in lower parental investments per child in larger families. By reducing family size, family planning policies can incentivize parents to allocate more resources to each child, thereby enhancing human capital at both the household and aggregate levels.

However, the effects of family planning policies may extend beyond promoting human capital development. When family planning policies enable parents to allocate more resources to their existing children, their responses can differ across socioeconomic backgrounds, due to factors such as financial constraints (Caucutt and Lochner, 2020) and differing perceptions of the returns on parental investments (Boneva and Rauh, 2018). Consequently, family planning policies may not only impact average outcomes but also shape the disparities in parental investments across socioeconomic backgrounds, ultimately influencing the persistence of income inequality across generations (Bolt et al., 2021).

In this paper, I investigate the effects of family planning policies on child quantity and quality and the consequences for intergenerational income persistence in the context of China. The issue of rising intergenerational income persistence in China has recently gained significant attention from policymakers and economists (Fan et al., 2021). I link this issue to the stringent implementation of the One-Child Policy (OCP) since 1979. I start by estimating the heterogeneous effects of the OCP by parental backgrounds on child quantity and three dimensions of child quality: health, education, and wealth.[1] I then investigate the long-term consequences of the OCP for labor market outcomes and income. Finally, I quantify the policy's impact on intergenerational income persistence.

To capture the heterogeneity across parental backgrounds, I focus on parental occupations. Parental occupation shapes both preferences and effectiveness in investing in children's educa-

[1] Wealth is not conventionally viewed as a measure of child quality since it is not an intrinsic trait. However, in the Chinese context, it is customary for parents to invest in their children's wealth, such as purchasing properties on their behalf, to improve the attractiveness of the children in the marriage market (Bhaskar et al., 2023). Hence, wealth is a relevant dimension when discussing the Q-Q trade-off in China.

tion and health, which explains why some parents prioritize one dimension over the other. For instance, teachers may exhibit higher efficiency in educational investments, while farmers might prioritize physical fitness over education. Furthermore, parental occupation serves as a better proxy for income than education, which is useful to study intergenerational income persistence when high-quality income data is unavailable (Bütikofer et al., 2022).[2] Thus, analyzing heterogeneity across parental occupations enables a more nuanced examination of how family planning policies mitigate or amplify the socioeconomic disparities in parental investments and influence the persistence of income inequalities across generations.

The OCP was enforced through fines imposed on unauthorised births (Ebenstein, 2010), with permits allowing some couples to have a second child (Scharping, 2013). For instance, members of certain ethnic minority groups or households with specific structures could apply for permits to have a second child without incurring fines. Eligibility criteria for permits and fine rates varied by province and year. I construct a measure of OCP enforcement, referred to as the "second-child penalty," which accounts for both fine rates and a couple's eligibility for second-child permits. This measure reflects the expected cost a couple would incur for having a second child. The second-child penalty varied across couples depending on their eligibility for permits, province of residence, and the birth year of their first child. I leverage this variation to identify the causal effect of the second-child penalty using a triple-difference framework, under the assumption that the difference between groups facing different eligibility criteria should trend similarly across provinces if there were no change in the strictness of the OCP specific to one group. Since the implementation of fines differed between rural and urban areas, and second-child permits were rarely granted in urban China during the period under study, I focus my analysis on rural China.

I use a sample of firstborn children from the China Family Panel Studies (CFPS), a representative survey of Chinese households. The CFPS contains detailed information on a wide range of adulthood outcomes. Important for my study, the CFPS asks the respondents to recall the parents' primary occupation during their childhood, a period when most of the parental investment decisions are made. The rich data allow me to investigate how the effects of the second-child penalty differ by the parents' primary occupation. Because the father is usually the breadwinner and decision-maker in Chinese households, I focus on the father's occupation and distinguish

[2] Using a sample of 2,600 rural men born between 1951 and 1965 in China, I find that occupation explains 40% of the variation in lifetime income, while education accounts for only 13%.

three types of occupations: farming, low-skill occupations, and high-skill occupations.

I start by estimating the impact of the second-child penalty on various outcomes for all firstborn children, all measured in adulthood, without considering differences among parents.[3] Results show that higher second-child penalties reduce family size but affect different dimensions of child quality in different ways. The penalties improve firstborn health but have no significant impact on firstborns' education or wealth in adulthood, suggesting a trade-off between family size and health of existing children but not other quality dimensions.

Next, I evaluate the impact of the second-child penalty across paternal occupations. The effects on family size and health outcomes do not vary by the father's occupation, indicating no heterogeneity in the trade-off between family size and health. However, the trade-off for other quality dimensions is occupation-specific. A higher second-child penalty significantly improves firstborns' education only if the father has a high-skill occupation, with effect size ranging from smallest for farmers' children to largest for children of high-skilled workers. The penalty also increases assets, particularly housing, for children of low-skilled workers, while for farmers, it raises firstborns' land ownership. The heterogeneous effects on education have long-term consequences for labor market outcomes and intergenerational mobility. Compared to other children, children of high-skilled workers tend to have better jobs, greater probability of urban migration, and higher income as adults as a result of increased second-child penalty.

To estimate the relationship between the second-child penalty and intergenerational income persistence, I regress child income or income rank on paternal income or income rank while incorporating the penalty as an interaction term.[4] Since direct data on fathers' income is limited, I follow Bütikofer et al. (2022) and impute income and income ranks using the averages by two-digit occupation and region of residence, derived from a sample of rural men born between 1951 and 1965. The estimates of intergenerational correlations using the imputed data closely aligns with the estimates in Fan et al. (2021), validating its reliability. By interacting paternal income or income rank with the second-child penalty, I estimate the extent to which the penalty increases the intergenerational correlations of income or income ranks.

[3]In the Q-Q trade-off literature, the instrumental variable (IV) approach is commonly used to estimate the causal effect of child quantity on child quality (e.g. Rosenzweig and Zhang, 2009; Liu, 2014; Mogstad and Wiswall, 2016). It requires an excluded IV that affects only child quality through its effect on child quantity. It is difficult to provide enough evidence in support of this exclusion restriction. I take an alternative approach to estimate how the family size and various child quality measures respond to a change in the price of child quantity. This approach follows directly from the Q-Q trade-off model (Becker and Lewis, 1973; Galor, 2012) and does not require the exclusion restriction.

[4]Income rank refers to the percentile ranks ranging from 0 to 100 relative to others born in the same cohort (Chetty et al., 2014).

I find strong evidence that the penalty significantly increases the intergenerational correlations of income and income ranks. A one-standard-deviation increase in the second-child penalty raises the father-child correlation of income by 0.08 and the correlation of income ranks by 0.05. The second-child penalty increases by 0.38 between cohorts born before and after the initiation of the OCP. The father-child correlations of income and income ranks have increased by 0.2 and 0.09, respectively, for these cohorts. Given the estimates of intergenerational correlations, a back-of-the-envelop calculation suggests that the increase in the second-child penalty account for 24% (35%) of the changes in the father-child correlations of income (income ranks) between the pre- and post-OCP cohorts.

In the final part of the study, I delve into potential explanations for the differential impacts of the second-child penalty across parental occupations. The evidence points to two primary factors driving these differential responses. First, Mincerian returns to education—the partial correlation between income and education—are lower for children of fathers engaged in farming or low-skill work compared to high-skill occupations. As a result, high-skilled workers may perceive education as particularly valuable for their children and thus invest more in their education when resource constraints are relaxed by reduced fertility. Second, the opportunity cost of education is higher for children of farmers due to incomplete land markets and insecure land rights. Under China's land tenure system, households risk losing their allocated land if all members shift to non-agricultural employment (Adamopoulos et al., 2024). Since education is critical for non-agricultural jobs, this insecurity creates an opportunity cost of education. This cost is typically borne by the younger child in families with two children, but it shifts to the firstborn when parents are restricted to having only one child. Consequently, reducing the number of children from two to one increases the opportunity cost of education for the firstborn, negatively impacting their educational attainment. I provide multiple pieces of evidence to support these explanations.

This paper adds to our understanding of the decrease in intergenerational mobility in China over the past decades (Fan et al., 2021). Several recent studies have attempted to identify the causes of this decline (Alesina et al., 2020; Jia et al., 2021; Yu et al., 2021). Among them, Yu et al. (2021) is the first to link family planning policies with the persistence of inequality, in particular urban-rural inequality. They show that urban residents, whose fertility is more constrained by the OCP, tend to have fewer children and invest more in their human capital, leading to an increase in the transmission of urban-rural disparities across generations. My study complements their work and extends it in two ways. First, I show that the OCP contributes to the persistence of inequality *within* rural China. Second, I show that family planning policies reduce intergenerational mobility not only through different fertility responses, but also through different responses in parental investments holding fertility constant.[5]

This paper also contributes to the empirical literature on the child quality-quantity (Q-Q) trade-off (Becker and Lewis, 1973). Many studies have examined the empirical relevance of this concept.[6] However, the evidence remains mixed. Some studies find that reducing child quantity enhances existing children's education (Rosenzweig and Wolpin, 1980; Li et al., 2008; Rosenzweig and Zhang, 2009), while others report no effect (Black et al., 2005; Angrist et al., 2010; Liu, 2014), or even a negative effect (Qian, 2009). Additionally, Mogstad and Wiswall (2016) uncover a nonmonotonic relationship between child quantity and the firstborn's education, suggesting a trade-off in larger families but complementarities in smaller families. Similarly, Guo et al. (2021) demonstrate that the Q-Q trade-off diminishes when reductions in child quantity deviate from optimal levels.

My findings help reconcile some of these mixed results. First, I confirm the findings of Liu (2014), which show that health outcomes are more responsive than educational outcomes to changes in child quantity in rural China. I extend this finding by demonstrating that the small and statistically insignificant effects on education are largely due to significant heterogeneity across parental occupations rather than the absence of a Q-Q trade-off. Moreover, I find that landed parents in settings with incomplete land markets may reduce educational investments in their firstborn child when restricted to having only one child, due to the fear of losing the land use rights when the only child leaves agriculture. This is consistent with existing studies showing that the trade-off between child quantity and education is nonmonotonic (Mogstad and Wiswall, 2016), smaller when reductions are suboptimal (Guo et al., 2021), and potentially reversed for farmers (Qian, 2009). Furthermore, the findings reveal an interesting interplay between family size, children's education, and land succession in rural contexts, shedding light on a potential

[5] A related strand of literature examines how reducing family planning costs improves outcomes for children from disadvantaged backgrounds and promotes upward mobility in the United States (Bailey et al., 2019; Seshadri and Zhou, 2022). A key reason for the differing conclusions from my study is that access to low-cost contraceptives mainly enable low-income women to plan their fertility and prevent unintended births, resulting in better initial conditions for the children who are eventually born. In contrast, in my setting, the policy was implemented through imposing costs to births that parents would intend to have. All parents are similarly affected in terms of fertility outcomes and the differential effects on child quality arise from differences in how parents allocate their resources.

[6] See Doepke (2015), Clarke (2018), and Guo et al. (2022) for extensive surveys of empirical studies on the Q-Q trade-off.

mechanism through which family planning policies may slow down structural transformation.

The rest of the paper is organized as follows. The next section summarizes the historical context. Section 3 describes the data, key variables, descriptive statistics, and empirical strategy. In Section 4, I examine the heterogeneity in the trade-off between child quantity and different dimensions of child quality across parental occupations and the consequences for intergenerational mobility. Section 5 discusses the factors driving the heterogeneous responses and Section 6 concludes with policy implications.

2 One-Child Policy

In 1979, the Chinese government introduced the One-Child Policy (OCP), restricting each couple to have only one child.[7] Provinces implemented the one-child rule by issuing second-child permits and imposing fines for the unauthorized births (Ebenstein, 2010). The fines levied on unauthorized births were implemented in terms of annual household income and varied across provinces and over time.[8] Figure 1a plots the fine rates measured in years of household income in 1985 and 1991 by provinces. The fine rates were relatively low in 1985 with little variation across provinces. However, as of 1991, some provinces increased from a fine of about one year of household income to more than 2.5 years of household income (e.g. Hubei), whereas others remained relatively low (e.g. Qinghai).[9] These variations in the fine rates create opportunities to study the effects of policy exposure on parents who had their first child at different times and places.

There were also nonmonetary penalties such as losing access to state-provided education, health, and employment, mostly for urban *hukou* holders.[10] Because it is difficult to quantify

[7]China initiated the first family planning campaign in some provinces in 1954 and the second urban-oriented family planning campaign in 1962. Since the 1970s, China has implemented stricter fertility restrictions through the "Later, Longer, Fewer" campaign from 1971 to 1979 and the OCP from 1979 to 2015. The fertility restrictions were gradually relaxed with the implementation of the universal two-child policy between 2015 and 2021 and the three-child policy in 2021. See Chen and Huang (2020) and Chen and Fang (2021) for the details of the "Later, Longer, Fewer" campaign and Zhang (2017) for a detailed picture of the evolution of the OCP.

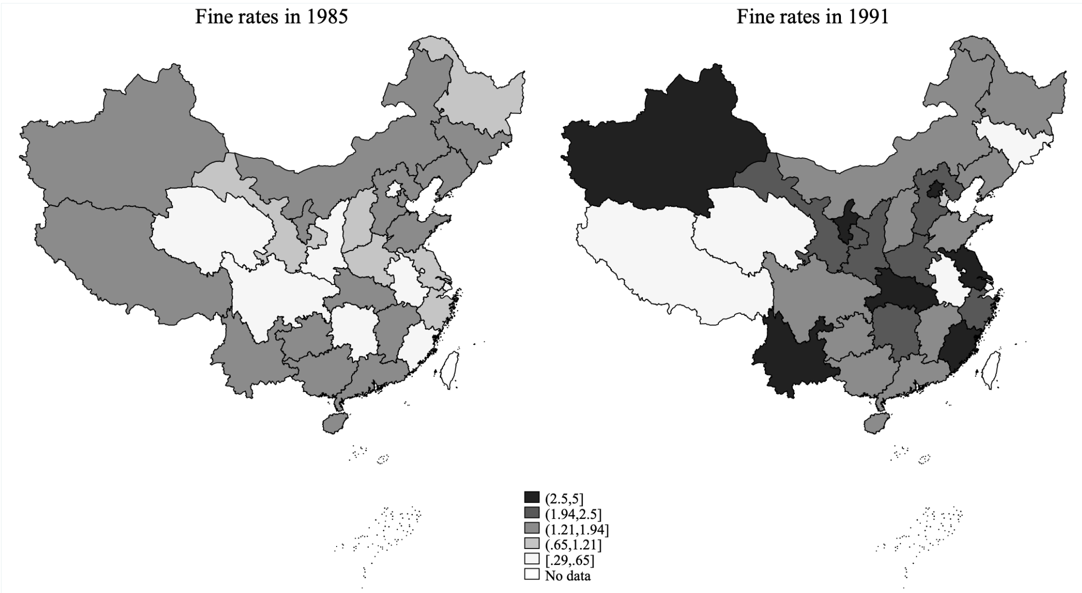
[8]See Ebenstein (2010) for more details on how the fine rates were calculated from provincial regulations.

[9]Average annual income per capita was 627 *yuan* in 1991 in rural Hubei and average household size was 4. Hence, one year of household income corresponds to about 2,500 *yuan* in 1991. This means a couple in rural Hubei had to pay on average 6,250 *yuan* for an unauthorized second child in 1991, about four times the Gross Regional Product per capita. Numbers are taken from the website of the Chinese National Bureau of Statistics and the 1990 Population Census.

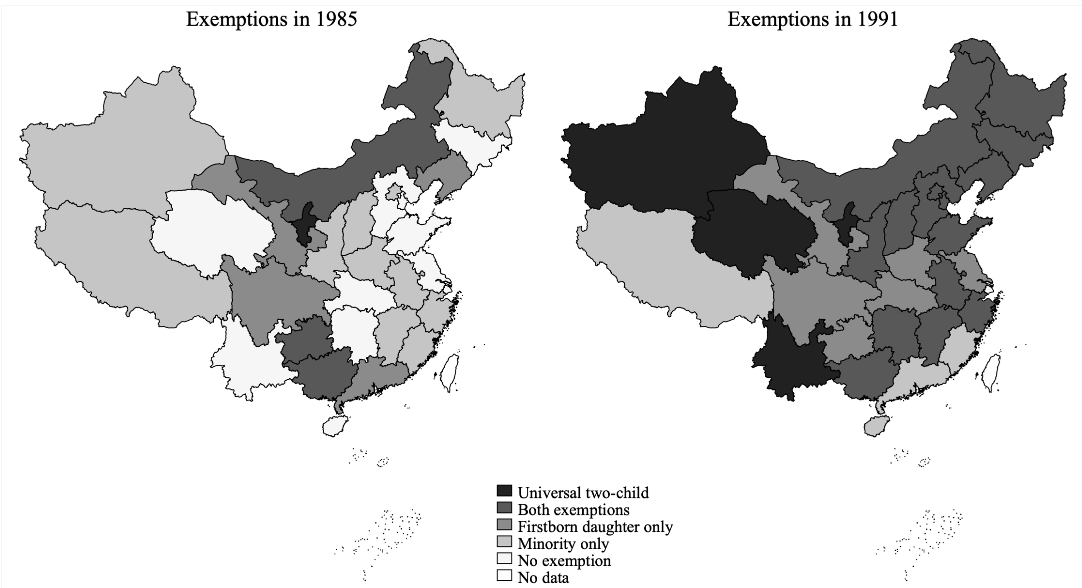
[10]The *hukou* (household registration) is an official record containing a individual's date and place of birth, place of origin (either the father's or grandfather's birthplace), ethnic identity, and current place of residence. The *hukou* system was first established in 1958 to categorize the population into individuals with rural (agricultural) *hukou* and those with urban (non-agricultural) *hukou*. See Appendix A.1 for a detailed description of the *hukou* system.

Figure 1: Sources of variation in treatment exposure

(a)



(b)



Note: Figure (a) plots the fine rates on unauthorized births in terms of annual household income in 1985 and 1991, which are taken from Ebenstein (2010).
Figure (b) plots the exemptions to the one-child rule granted to minority groups and couples with only a firstborn daughter in rural China in 1985 and 1991, which are taken from Scharping (2013).

these penalties, I do not consider them for identification. One concern that is also mentioned by Ebenstein (2010) is that in rural areas, it is difficult for authorities to observe the income of self-employed farmers. Hence, a fine as a multiple of annual income may be difficult to implement. However, there is evidence that authorities imposed higher fertility fines on wealthier farmers due to their greater ability to pay (Scharping, 2013). Ebenstein (2010) argues that the fine rates will be appropriate measures of the strictness of policy enforcement as long as the authorities treat wage earners and self-employed farmers similarly. As this assumption may not hold across rural and urban areas, I focus my analysis on the rural *hukou* holders. Rather than identifying the effect of OCP enforcement, this paper identifies the effect of the the fine parents expect to pay if they want a second child.

The one-child rule was universal in rural China when it was first implemented but was gradually relaxed in 1982.[11] Provincial governments set conditions under which couples were exempted from the one-child rule and could apply for a second-child permit (Scharping, 2013). Some provinces introduced exemptions to rural couples whose firstborn was a girl. In some provinces, exemptions were also granted to ethnic minority groups with populations under 10 million. In a few provinces, exemptions were granted to all married rural couples at some point. Notably, between 1979 and 2000, exemptions for urban couples were extremely rare.

Figures 1b plot the exemption rules for rural minority groups and rural couples with only one daughter in 1985 and 1991 by provinces. In 1985, most provinces had no exemptions or only exemptions for minority couples. In 1991, however, a majority of the provinces introduced exemptions to both minority couples and couples with only one daughter. Some provinces allowed all rural couples to have a second child in 1991, among which only Yunnan is in my data. These exemption rules not only create variation in policy exposure across provinces and over time but also make the exposure differ by ethnicity and gender of the firstborn. In Section 3.2, I provide more details on how I construct a measure of policy exposure reflecting the potential monetary penalty to couples who want a second child under the OCP.

[11]There were a few exemption rules applied nationwide since 1979 (Scharping, 2013). A second child was allowed if the first birth was dead or disabled, the first child was adopted, or the couple remarried. Exemptions also existed for some occupations, such as fishermen, mine workers, and military veterans. Couples with one or both partners being only children were allowed to have a second birth in some provinces. These exemptions happened less frequently, and the information is not available in my data. Hence, they are not considered for identification.

3 Data and empirical strategy

3.1 Data and sample

The main analysis uses data provided by the China Family Panel Studies (CFPS), initiated by the Institute of Social Science Survey at Peking University, China in 2010 (Xie, 2012). The CFPS is a longitudinal survey of Chinese communities, families, and individuals, representative of 95% of the Chinese population. In this study, I focus on children who were born between 1966 and 1990, held a rural *hukou* at age three, and were the first child to their parents. The sample consists of individuals between the ages of 20 and 44 who had almost completed their education and joined the labor force by 2010.[12] Hence, I can analyze their educational and labor market outcomes using the 2010 CFPS survey data. The unit of the empirical analysis is a firstborn child because the treatment and outcome variables are observed at the child level. However, in the discussion, I use the words "couples", "parents", and "firstborn children" interchangeably since divorce and childlessness rates were low during the period of study.

CFPS recorded the parents' primary occupation when the child was aged 12. As fathers are the main decision makers and primary earners in Chinese households in the past, I focus on fathers' occupations throughout the analysis.[13] I classify the father's primary occupation into three types: farming, low-skill occupations, and high-skill occupations.[14] A father is a farmer if he mainly works in the agricultural sector. For a given nonfarm occupation, I compute the fraction of fathers employed in this occupation who ever attended high school. I define a father as working in a low-skill (high-skill) occupation if this fraction is lower (higher) than 50%. In the sample, 66% of the fathers were farmers, 27.7% were in low-skill occupations, and 6.3% were in high-skill occupations.[15] The predominant high-skill occupations include healthcare professionals, financial management staff, administrative staff, and education professionals, collectively representing 76% of all fathers employed in high-skill jobs. The most common low-skill occupations encompass street vendors, market traders, sales personnel, construction workers, and equipment operators, accounting for 51% of fathers engaged in low-skill work.

Table 1 Panel A shows summary statistics of individual characteristics for 2,895 firstborn

[12]When looking at labor market outcomes, I exclude the youngest cohorts who were under 22 in 2010.

[13]In Appendix A.2.1, I show that the heterogeneity is indeed mostly driven by the father's occupation, not the mother's.

[14]Another relevant measure of family background would be parental income. However, income information of the parents are not reported in CFPS unless the parents are living with the children at the survey time.

[15]I categorize occupations based on their level of educational intensity. In Appendix A.2.2, I demonstrate that the heterogeneity observed in the Q-Q trade-off is not driven by the parents' education levels.

children in my data, among which 1,310 are boys and 1,585 are girls. I also show the summary statistics for the male and female samples separately in columns (2) and (3).[16]

The outcome variables of interest are measured in 2010 and include the number of siblings, only child status, height, self-reported health, educational attainment, land value per household member, non-land household assets per household member, employment, Treiman occupational score, urban *hukou* status, and income. The Treiman occupational score is a globally recognized measure of occupational status, originally developed by Treiman (1977). In rural China, farmland is owned collectively and cannot be sold in the market. Therefore, CFPS calculated land value based on the land size and agricultural output in the year before the survey. Additionally, I adopt a proxy for lifetime personal income by averaging the annual personal income from 2010, 2012, and 2014 waves of the CFPS, following Fan et al. (2021). Income is adjusted to 2010 prices using the Consumer Price Index. Panel B of Table 1 presents the means of the outcome variables for the entire sample and by gender. In Appendix A.3, I provide a more comprehensive description of these variables and how they are constructed from CFPS.

3.2 Measure of the second-child penalty

In order to examine the Q-Q trade-off, I utilize the second-child penalty introduced under OCP as an exogenous shift in the cost of having additional children. The second-child penalty reflects the potential cost of a second child induced by OCP measured in annual household income. This penalty is determined by a combination of a couple's eligibility to apply for a second-child permit and the fines that are imposed on unauthorized births. As local policies regarding eligibility and fine rates change over time, the second-child penalty varies by locality, the birth year of the first child, and individual characteristics that determine eligibility for a second-child permit.

I focus on the ten years following the first child's birth since over 98% of the couples with two or more children in the sample had their second child during this window. This approach is similar to that of Guo et al. (2021), who investigate the impact of a third-child penalty on the first child by averaging fine rates over the ten years following the second child's birth. This measure is also consistent with the notion of the OCP as a child pricing system, as proposed by García (2022). Specifically, my measure reflects the average "price" of a second child in the ten years after the birth of the first child.

[16] Note that although it is well-known that parents conduct sex selection under the OCP, it is not prevalent among firstborn children (Ebenstein, 2010). The sample consists more girls due to the sampling design and non-responses (Xie and Lu, 2015).

Table 1: Summary statistics

	Mean		
	All	Boy	Girl
Panel A. Individual charateristics			
Boy (0/1)	0.453	1.000	0.000
Minority (0/1)	0.070	0.066	0.073
Age (years)	32.714	32.914	32.548
Father's age (years)	58.842	58.858	58.830
Mother's age (years)	57.006	57.150	56.886
Mother's age at birth (years)	25.996	25.607	26.317
Father's years of schooling	6.774	6.098	7.332
Mother's years of schooling	5.518	5.053	5.903
Father farmer (0/1)	0.677	0.677	0.676
Father low-skill occupation (0/1)	0.257	0.260	0.254
Father high-skill occupation (0/1)	0.067	0.063	0.070
Panel B. Outcome variables			
Number of siblings	1.470	1.265	1.640
Any sibling (0/1)	0.771	0.686	0.840
Good health (0/1)	0.589	0.624	0.561
Height (cm)	164.483	170.242	159.623
Years of schooling	8.351	8.960	7.847
Complete high school (0/1)	0.245	0.277	0.219
Cognitive test score (sd)	0.000	0.150	-0.124
Employed (0/1)	0.628	0.734	0.541
Occupational score[a] (1-100)	39.977	39.863	40.103
Urban *hukou* (0/1)	0.174	0.174	0.175
Income (1,000 yuan)	15.755	21.453	10.773
Land assets (1,000 yuan)	4.667	5.251	4.182
Non-land assets (1,000 yuan)	48.755	50.379	47.393
Panel C. Treatment variable			
Second-child penalty	0.635	0.879	0.432
Observations	2895	1310	1585

Note: 1. The sample consists of firstborn children holding rural *hukou* at age 3 and born between 1966 and 1990 from the CFPS data.
2. See Appendix A.3 for details on how the variables are contructed from the CFPS data.
[a] conditional on being employed.

The formula used to construct the second-child penalty for a firstborn child born in year t in province p and belonging to group g is given by

$$Penalty_{tgp}^{(1-10)} = \frac{1}{10}\sum_{s=1}^{10} Fine_{t+s,p}(1 - Permit_{t+s,g,p}) \tag{1}$$

Because I only consider the exemptions given to couples with only one daughter or minority couples, g is determined by the gender and ethnicity of the firstborn child.[17] $Permit_{t+s,g,p}$ is an indicator variable that equals one if parents, who have their first child in year t in province p and belong to group g, are permitted to have a second child in year $t+s$. Meanwhile, $Fine_{t+s,p}$ is the amount of fine imposed on unauthorized births that occur in year $t+s$ in province p. The idea is that if a couple has their first child in year t and their second child in year $t+s$, they pay a fine of $Fine_{t+s,p}$ if they are not eligible to a second-child permit ($Permit_{t+s,g,p} = 0$), and pay nothing if they are eligible ($Permit_{t+s,g,p} = 1$). I then take the average penalty over ten years since the birth of the first child to capture the potential penalty over the ten-year window. In Appendix A.4, I use examples to illustrate how the penalty varies across provinces p, gender-ethnicity group g, as well as year of birth t. As shown in Panel C of Table 1, the average penalty, that is, the potential cost for a second child measured in terms of annual household income, is 0.64 across the sample. Additionally, the average penalty is twice as high when the firstborn child is a boy than when the firstborn child is a girl.

In Appendix A.2.3, I introduce several modifications to the measure of the second-child penalty, such as focusing solely on eligibility for free second-child permits and incorporating birth spacing requirements. Restricting the variation in permit eligibility addresses concerns that the changes in fine rates may be endogenous, potentially reflecting local economic conditions and fertility demands (Zhang, 2017). In one modification, I exploit only variation in high levels of second-child penalties to address the negative weights problem that arise from staggered adoption and the lack of a stable control group (de Chaisemartin and D'Haultfœuille, 2020). The results are largely consistent across different measures.

[17] Practically, the exemption is determined by the ethnicity of the parents. If both parents are from the same ethnic group, the child is of the same ethnicity. For inter-ethnicity couples, the child could be of either ethnicity of the parents. Huang et al. (2023) report that there were different implementations for inter-ethnicity couples by provinces under OCP, some exempting these couples from strict birth control and others not. I do not exploit these variations in the data because I only observe the ethnicity of parents for a small sample of children whose parents were registered in the same household in 2010. However, the results do not change if I drop children whose family has at least one member belonging to the exempted minority groups.

3.3 Identification strategy

I employ a triple-difference strategy to estimate the causal effect of the second-child penalty. This strategy assumes that the difference between genders or between the ethnic majority and minority groups would follow the same trends across provinces if the two groups were treated similarly by the OCP. I use group-cohort fixed effects to capture the changes in the differences between two groups that are common to all provinces, and province-cohort fixed effects to account for differential cohort trends across provinces due to factors other than the exemption rules applied to certain groups.

Specifically, I consider the following equation:

$$y_{itgp} = \gamma Penalty_{tgp}^{(1-10)} + X_i\beta + Z_{tgp} + V_{pt} + W_{gp} + U_{gt} + \epsilon_{itgp} \tag{2}$$

where y_{itgp} is the outcome of child i born in year t in province p and belonging to group g. As in a standard triple-difference framework, I control for three-way fixed effects, including province-cohort fixed effects V_{pt}, group-cohort fixed effects U_{gt}, and province-group fixed effects W_{gp}. Notice that province fixed effects, group fixed effects, and cohort fixed effects are subsumed by the three-way fixed effects. I also include a set of individual controls X_i, including ethnicity, parental education, parental age, mother's age at birth, and dummies for the father's occupational types. Standard errors are clustered at the province-cohort level, at which most of the policies were set.[18]

To address the issue that province-level OCP enforcement before the introduction of exemptions may have differential impacts on different groups, I control for the level of fines in the three years preceding the birth of the first child interacted with gender and minority status. Additionally, I allow group-cohort fixed effects to vary with the fine rates in 1979, which controls for group-specific effects of initial OCP strictness in the province. These controls are labeled as Z_{tgp}. In Appendix A.2.4, I show that the findings are also robust when controlling for group-specific effects of other time-varying socioeconomic factors at the province level.

To ensure that the second-child penalty is not correlated with pre-existing characteristics of the children, I run a regression of these characteristics on the penalty, while controlling for the fixed effects mentioned above and Z_{tgp}. As shown in Table A.1 in the Appendix, the second-child penalty is not correlated with pre-determined characteristics of the children, including the

[18]Clustering at province-group level or two-way clustering at the province and cohort level with bootstrap methods yield standard errors that indicate similar significance levels.

father's primary occupation when the child is 12 years old. This finding suggests that the father's primary occupation is exogenous to the second-child penalty, which allows me to estimate the heterogeneous effect of the penalty across paternal occupations. I specify the following equation:

$$y_{itgp} = \gamma_1 Penalty_{tgp}^{(1-10)} + \gamma_2 LS_i Penalty_{tgp}^{(1-10)} + \gamma_3 HS_i Penalty_{tgp}^{(1-10)} \\ + X_i\beta + Z_{tgp} + V_{pt} + W_{gp} + U_{gt} + \epsilon_{itgp} \quad (3)$$

where LS_i is a dummy variable that takes value one if the father's primary occupation is low-skill, and HS_i is a dummy variable that takes value one if the father's primary occupation is high-skill. Notice that LS_i and HS_i are part of individual controls X_i in equations 2 and 3. Assuming that the identification assumption holds, the estimate of γ_1 captures the causal effect of the second-child penalty on the firstborn of a farmer, and the estimate of γ_2 (γ_3) captures the difference in the causal effect between a farmer's child and a low-skill (high-skill) worker's child.

3.4 Linking OCP to intergenerational income mobility

This subsection describes how I estimate the effect of the second-child penalty on intergenerational income mobility. To measure intergenerational income mobility, I estimate the correlations of the father and the child's income as well as the rank-rank correlations based on income percentile ranks (Chetty et al., 2014). The latter measure is known to be less prone to life cycle bias caused by using snapshots of income to proxy lifetime income (Nybom and Stuhler, 2017).

Following the literature (e.g. Fan et al., 2021; Adermon et al., 2021), a standard equation to estimate the intergenerational income persistence is the following:

$$Inc_i = \gamma_0 + \gamma_1 Inc_i^f + u_i \quad (4)$$

where Inc_i is the income or income rank of the child i and Inc_i^f is the income or income rank of his or her father. The coefficient γ_1 captures the intergenerational correlation (IGC) or intergenerational rank correlation (IRC) in income. A larger γ_1 implies greater intergenerational persistence in income, which means less mobility across generations.

Imputation of the father's income and income ranks.—Information on the father's income is missing in CFPS unless the father is registered in the same household as the child. Hence, I only observe the income of the father for a small, selected sample of children. To address this issue, I follow Bütikofer et al. (2022) and construct an alternative measure of father's income based on information on fathers' primary two-digit occupations and region of residence.[19] Specifically, I use a sample of 2,600 rural men born between 1951 and 1965 and calculate the average income for each occupation in each region. This approach allows for regional variations in earnings for each occupation, reflecting differences in economic development levels and industrial structures across regions. In total, there are 170 occupation-region groups, accounting for approximately 44% of the variations in income in this sample.

To construct income ranks for the fathers, I use a similar approach to the one employed for imputing the father's income. Specifically, I transform income into percentile ranks for the sample of rural men born between 1951 and 1965 and calculate the average percentile rank for each occupation within each region. I then impute the father's income rank based on his two-digit occupation and region of residence. I transform the child's income into percentile ranks ranging from 0 to 100, relative to other children in the same cohort. Cohorts are defined as born before or after 1979.

To validate the imputation method based on occupation and region of residence, I estimate the IRC using the imputed data for my sample and compare it with the estimates reported in Fan et al. (2021) for the same cohorts. For rural children born between 1970 and 1980, my estimated IRC is 0.33, which is very close to the 0.35 reported in Panel B Table 5 of Fan et al. (2021). Similarly, for the 1981–1988 cohort, I estimate an IRC of 0.40, compared to 0.39 in Fan et al. (2021). These similar estimates indicate that the imputation of income ranks based on occupation and region of residence provides reliable estimates of the IRC.

Estimation.—To investigate how changes in the second-child penalty affect the father-child correlations in income and income ranks, I estimate the following equation adapted from equation 4:

$$Inc_{itgp} = \alpha_0 Inc_i^f + \alpha_1 Inc_i^f Penalty_{tgp}^{(1-10)} + \alpha_2 Penalty_{tgp}^{(1-10)} + V_{pt} + W_{gp} + U_{gt} + \epsilon_{itgp} \quad (5)$$

In the absence of OCP, the second-child penalty is zero, and α_0 captures the IGC or IRC in

[19] The sample size is insufficient to allow imputation based on occupation and province of residence. Therefore, I use the region of residence instead following the official regional classification from the National Bureau of Statistics of China. Provinces are grouped into four regions as follows: East China includes Beijing, Tianjin, Hebei, Shanghai, Jiangsu, Zhejiang, Fujian, Shandong, Guangdong, and Hainan; Central China includes Shanxi, Anhui, Jiangxi, Henan, Hubei, and Hunan; West China includes Inner Mongolia, Guangxi, Chongqing, Sichuan, Guizhou, Yunnan, Tibet, Shaanxi, Gansu, Qinghai, Ningxia, and Xinjiang; and Northeast China includes Liaoning, Jilin, and Heilongjiang. These regions differ across several dimensions, including geography, economic development, industrial structure, population distribution, and infrastructure.

income. When the government introduces a non-zero second-child penalty, $\alpha_0 + \alpha_1 Penalty_{tgp}^{(1-10)}$ reflects the IGC or IRC in income. Therefore, α_1 captures how much the second-child penalty increases the intergenerational persistence of income. Following equation 4, I do not include individual controls such as parental education and ages. However, I retain the three-way fixed effects to identify the causal effect of the second-child penalty using the triple-difference strategy.

3.5 Event-study analysis

Before presenting the estimation results, I use an event study analysis to provide support for the identification strategy and the measure of the second-child penalty. An event is defined as when a second-child permit is first granted to the parents. When the event happens, the penalty to have a second child reduces to zero.[20] Using the same sample of children as used in the main analysis, I compute the age of each child when the event occurs. I then regress a dummy indicator of having at least one sibling on a series of event dummies. The regression is expressed as follows:

$$y_{itgp} = \sum_{j \in J} \xi_j D_j + X_i \beta + Z_{tgp} + V_{pt} + W_{gp} + U_{gt} + \epsilon_{itgp} \tag{6}$$

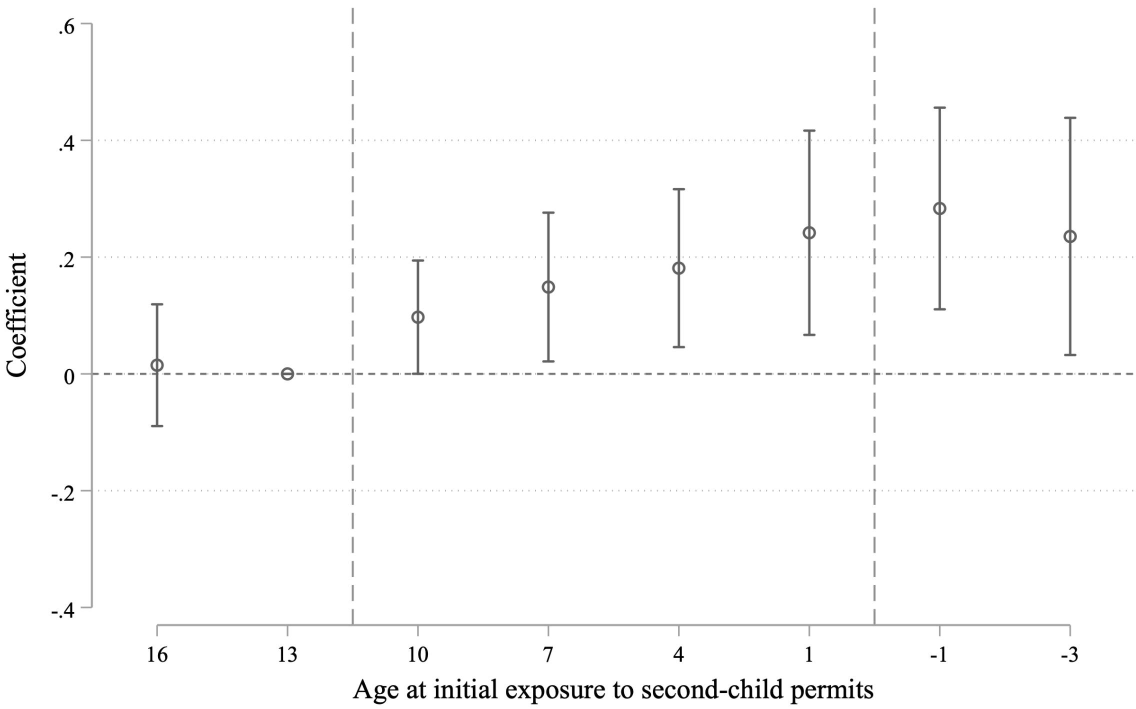
where j denotes the group of child i defined by the age at initial exposure to the second-child permit and $J = \{-4, -3, -1, 1, 4, 7, 10, 13, 16, 19\}$. For j between 1 and 16, D_j is a dummy variable that equals one if the event occurred at ages j, $j+1$ or $j+2$. For $j = -1$ or $j = -3$, D_j equals one if the event occurred at ages j or $j+1$.[21] To reduce the collinearity between the event dummies and birth year fixed effects, I include two endpoints $j = -4$ and $j = 19$ as open brackets and focus only on the estimates for event dummies between these points. D_{-4} is a dummy variable that takes value one if the event happened four or more years before birth, and D_{19} is a dummy variable that takes value one if the event happened at or after age 19. The rest of the specification is the same as equation 2.

Figure 2 displays the coefficients and corresponding 90% confidence intervals for each dummy variable D_j. The coefficient for age group j on the graph corresponds to ξ_j, which measures the

[20] It is challenging to incorporate variations in fine rates in this exercise, as these changes are non-absorbing and continuous. As a robustness check, I redefine the event as first receiving the second-child permit in a year when the fine rate exceeds one year of household income. The results remain consistent, with coefficients that are even larger and more statistically significant (see Figure A.1).

[21] Three ages are grouped together to ensure sufficient observations for each cell. Due to the limited number of observations for events starting four years before birth, only two ages are grouped for $j = 0$ or $j = -2$, providing at least two event points prior to birth.

Figure 2: Event study: timing of second-child permits and the probability of having any sibling



Note: The figure shows the coefficients and 90% confidence intervals (based on standard errors clustered at the province-cohort level) from the event study analysis. The sample consists of children born between 1966 and 1990, who had a rural *hukou* at age 3 and is the first child to their parents. The outcome is the probability of having any sibling. The x-axis is the age group j of the first child since when the parents become eligible to a second-child permit. The y-axis is the estimate for ξ_j in equation 6. The age group 13 is taken as the base group. The estimate for ξ_j represents how much a couple are more likely to have a second child if they become eligible to a second child permit when the first child is aged j to $j+2$ instead of 13 to 15, conditional on three-way fixed effects, individual controls, and controls for pre-birth OCP intensity described in Section 3.3. From left to right, the two dashed lines separates the sample into three groups that are subject to the OCP penalties for different durations during the ten-year window after the birth of the firstborn: always treated, partially treated, and never treated.

effect on the probability of having a second child if the parents become eligible to the second-child permit when the first child is aged j, relative to the baseline ages of 13-15. Depending on the timing of the event, the children and their parents can be divided into three groups that are subject to the OCP penalties for different durations during the ten-year window after the birth of the firstborn: always treated, partially treated, and never treated. Parents who received the second-child permit after their first child turned 10 were subject to the fines during the ten-year window following the firstborn's birth (always treated). In contrast, parents who received the second-child permit before the birth of their firstborn were not subject to the fines throughout the ten-year period (never treated). The two dashed lines in Figure 2 separates the always-treated and never-treated groups from the partially treated groups, who were subject to the fines for part of the ten-year window.

From the event study plot in Figure 2, three key findings emerge. First, the probability of having a second child are similar for the always treated groups, regardless of the timing of the event. This suggests that obtaining a second-child permit after the first child turns 10 does not significantly influence the decision to have a second child. Second, the likelihood of having a sibling rises almost linearly as exposure to OCP penalties decreases. Taken together, the event study estimates show that parents base their decision to have a second child on the eligibility to second-child permits during the ten years after the birth of the first child. Therefore, the event study provides support for the measure of the second-child penalty that covers the first ten years of the firstborn child's life.

Third, the likelihood of having a second child remains similar whether the second-child permit is granted when the first child is one year old or at any point before birth. It is plausible to assume that receiving a second-child permit before the firstborn's birth has minimal additional impact on the probability of having a second child compared to receiving the permit during the year of the firstborn's birth. If this assumption holds, the comparable coefficients for events occurring prior to the firstborn's birth support the parallel trends assumption. In other words, there is no pre-existing trend in the probability of having two children among the "never treated" group—those who were permitted to have two children throughout the ten-year period after the birth of the first child.

In Figure A.2, I replicate the event study analysis for four measures of child quality. The patterns for these quality measures are similar to those observed in Figure 2, although the estimates are notably noisier. The key finding is that trends in all four child quality outcomes remain flat if the second-child permit is granted after the firstborn turns 10. This result aligns closely with the event study for the probability of having any sibling. By contrast, when parents are granted a second-child permit before the firstborn turns 10, the quality outcomes of the firstborn are worse, though these effects are less robust than the observed increase in the probability of having any sibling, as seen in Figure 2. The similar patterns in child quantity and quality outcomes suggest that eligibility for second-child permits induces a trade-off between child quantity and quality, but only when the firstborn is between ages 0 and 10. Overall, the event study analysis supports the use of the second-child penalty as a measure for estimating the Q-Q trade-off induced by the OCP.

4 Estimation results

This section presents three sets of results. The first two subsections provide the main results on how family size and different dimensions of child quality respond to a change in the second-child penalty. The third subsection presents the heterogeneous effects on labor market outcomes. The last subsection discussed the consequences for intergenerational income mobility.

4.1 Effects on family size

The first and third columns of Table 2 show that when the second-child penalty increases by one-year of household income, there is a decrease of 0.24 in the number of siblings and a 14.5 percentage point decrease in the likelihood of having any sibling for the firstborn. The results in columns (2) and (4) show that the estimated effect of the second-child penalty on child quantity does not vary significantly across the occupation of the father. This suggests that if the impact of the penalty on child quality varies by the occupation of the father, it is not a result of a differential impact on child quantity.

Columns (5) and (6) of Table 2 provides additional evidence on the validity of the identification strategy and the accuracy of the second-child penalty measure. I estimate the effect of the penalty at different ages and before the first child's birth. Results indicate that if the second-child penalty increases at ages 1 to 5 of the firstborn, there is a reduced likelihood of having the first sibling before age 5, but no effect on the likelihood of having the first sibling after age 5. Similarly, if the penalty increases at ages 6 to 10, it decreases the probability of having a sibling after age 5 but has no impact on the likelihood of having the first sibling before

Table 2: Effects of on the second-child penalty on family size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					First sibling			
	Number of siblings		Any sibling		bef. age 5	aft. age 5	Any male sib	Any female sib
Penalty	-0.239**	-0.219**	-0.145***	-0.131***			-0.071*	-0.088*
	(0.097)	(0.098)	(0.040)	(0.041)			(0.037)	(0.048)
Low-skill × Penalty		-0.047		-0.032				
		(0.055)		(0.027)				
High-skill × Penalty		0.049		0.018				
		(0.114)		(0.049)				
Penalty ages 1–5					-0.102*	0.028		
					(0.058)	(0.040)		
Penalty ages 6–10					0.018	-0.087***		
					(0.033)	(0.023)		
Penalty birth year					0.065	-0.047		
					(0.088)	(0.083)		
Penalty 1–2 yr. bef. birth					-0.112	0.182*		
					(0.136)	(0.111)		
Penalty 3–4 yr. bef. birth					0.054	-0.110		
					(0.214)	(0.135)		
Penalty 5–6 yr. bef. birth					0.705	-0.208		
					(0.509)	(0.401)		
R^2	0.540	0.541	0.587	0.588	0.468	0.278	0.469	0.379
Mean dep var	1.471	1.471	0.771	0.771	0.644	0.115	0.553	0.478
Observations	2894	2894	2894	2894	2894	2894	2894	2894

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.
2. Penalty is the fine a couple expects to pay for the second child. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation is high-skill if more than 50% of all fathers employed in this occupation have attended high school. The omitted group consists of children whose father was a farmer.
3. Baseline controls include province-cohort fixed effects, province-group fixed effects, group-cohort fixed effects, individual-level controls, and controls for OCP intensity before births. "Low-skill" and "High-skill" dummies are included in all regressions.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

age 5. This pattern suggests that parents' decisions regarding a second child are influenced by the current level of the second-child penalty. Moreover, it supports the validity of the penalty measure and the identification strategy, indicating that a change in the penalty before age five does not affect family size after age five, and vice versa. Finally, I find no significant impact of the second-child penalty before birth, which aligns with the results of the event study.

Previous research has shown that the OCP not only lowers fertility but also skews the ratio of daughters to sons (Ebenstein, 2010; Li et al., 2011; García, 2022). If changes in the second-child penalty also affect the gender composition of siblings, the effect on child quality may be driven by the effect of younger siblings' gender on older children's outcomes. The last two columns of Table 2 show that the second-child penalty has similar effects on the probability of having a younger sister or a younger brother, providing support for interpreting the results as a trade-off between child quality and quantity.

Table 3: Effects of the second-child penalty on child quality

	(1) Height (sd)	(2) HS completion	(3) Land assets	(4) Non-land assets
Panel A. No heterogeneity				
Penalty	0.202**	0.003	0.947	9.365
	(0.100)	(0.045)	(0.752)	(5.818)
R^2	0.379	0.431	0.373	0.588
Panel B. Heterogeneity by father's occupation				
Penalty	0.217**	-0.030	1.597**	3.334
	(0.098)	(0.044)	(0.766)	(5.548)
Low-skill × Penalty	-0.033	0.074**	-1.529***	15.726***
	(0.056)	(0.033)	(0.504)	(3.694)
High-skill × Penalty	-0.000	0.154***	-1.199	-11.324
	(0.114)	(0.048)	(1.000)	(11.777)
R^2	0.379	0.435	0.376	0.591
Mean dep var	0.015	0.245	4.663	48.767
Observations	2807	2894	2851	2763

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.
2. Penalty is the fine a couple expects to pay for the second child. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation is high-skill if more than 50% of all fathers employed in this occupation have attended high school. The omitted group consists of children whose father was a farmer.
3. Baseline controls include province-cohort fixed effects, province-group fixed effects, group-cohort fixed effects, individual-level controls, and controls for OCP intensity before births. "Low-skill" and "High-skill" dummies are included in all regressions.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

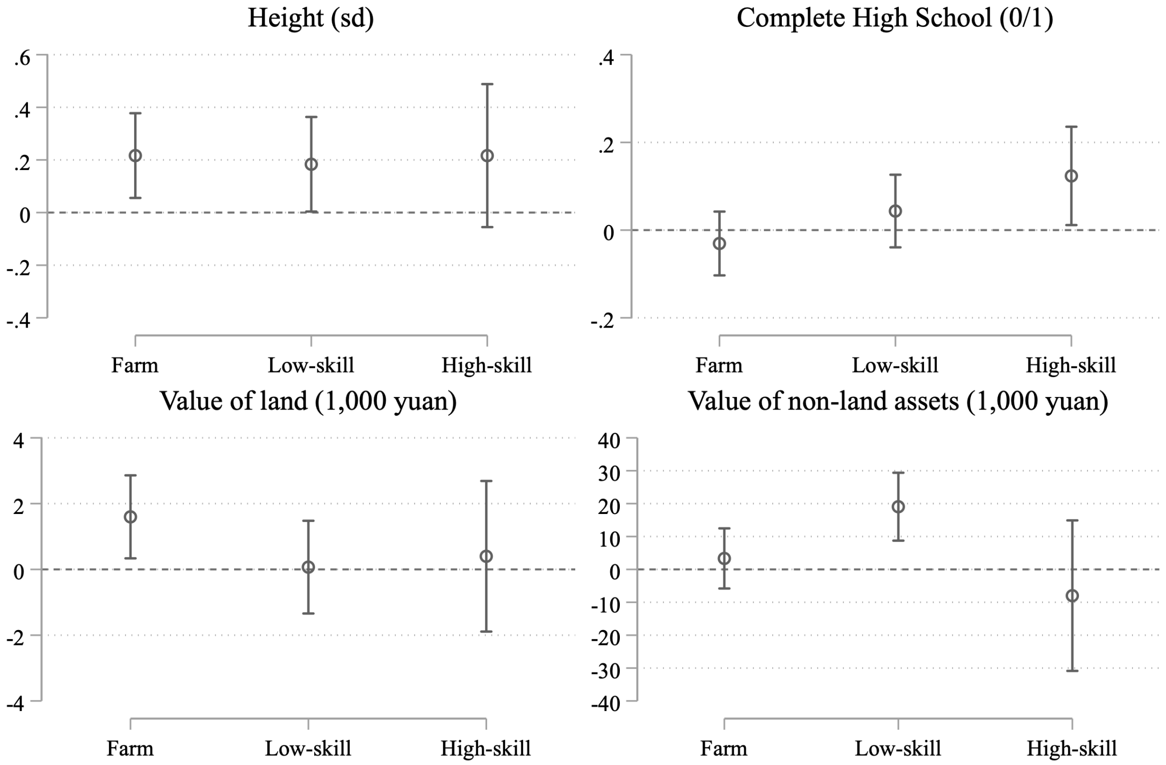
4.2 Effects on human capital and wealth

Next, I show how different quality dimensions of the firstborn, including health, education, and wealth, respond to a change in the second-child penalty.

Health.—Column (1) of Table 3 displays the impact of the second-child penalty on the health of the firstborn child, measured by standardized height. Panel A of Table 3 shows that when the second-child penalty increases by one year of household income, the firstborn child would be 0.2 standard deviations taller. Panel B indicates that the effects of the penalty on height do not vary significantly by the parent's occupation. The upper-left panel of Figure 3 illustrates the estimated effect of the second-child penalty on the firstborn child's height for each occupation group. The magnitude of the health effect is almost the same for all three occupational groups.

Education.—Column (2) of Table 3 presents the findings for education, which is measured

Figure 3: Total effect of the second-child penalty by father's occupation



Note: 1. Subfigures display the estimated effect of the second-child penalty on various outcome variables for different paternal occupations, along with their corresponding 90% confidence intervals based on robust standard errors clustered at the province-cohort level.
2. Farm is a dummy indicator that takes value one if the respondent's father primarily worked as a farmer when the respondent was aged 12, and zero if otherwise. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation is high-skill if more than 50% of all fathers employed in this occupation have attended high school.
3. Baseline controls include province-cohort fixed effects, province-group fixed effects, group-cohort fixed effects, individual-level controls, and controls for OCP intensity before births. "Low-skill" and "High-skill" dummies are included in all regressions.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

by a binary indicator for high school completion. The results in Panel A of Table 3 column (2) indicate that, on average, the second-child penalty does not affect the likelihood of completing high school education. However, the average effect conceals significant heterogeneity, as shown in Panel B. When the penalty increases, farmers' children are somewhat less likely to finish high school, but the effect is relatively minor and statistically insignificant. In contrast, children of nonfarmers are more likely to complete high school, with the difference being more substantial between farmers' children and high-skilled workers' children than between farmers' children and low-skilled workers' children. The upper-right penal of Figure 3 shows the estimated impact of the penalty on education separately for children with fathers in each occupation group. The effect is small and insignificant for children of farmers, and increase gradually as the father's occupation becomes more skilled.

In the top and middle panels of Appendix Figure A.3, I show results using alternative measures of health and education, including an indicator of self-reported good health, an indicator of being the top 20% tallest in the sample (conditional on gender), years of schooling, and cognitive test scores. The results remain qualitatively consistent with those obtained using the primary measures.

Wealth.—The impact of the second-child penalty on the assets of the firstborn child is presented in columns (3) and (4) of Table 3 and the bottom panel of Figure 3. The second-child penalty, on average, has a positive but insignificant impact on land and non-land assets ownership. Panel B shows that the effect on land ownership is solely driven by firstborn children in farming households, who owns significantly more land as the second-child penalty increases. On the other hand, the effect of the second-child penalty on nonland assets is only significant for children of low-skilled workers. The bottom row of Appendix Figure A.3 shows the results when we break down non-land assets into housing assets and financial assets. The effect on non-land assets is primarily driven by an increase in housing assets. The effect on financial assets, mostly savings, is also significant for children with fathers in low-skill occupations, but the difference across occupational groups is small and statistically insignificant.

The results indicate that as the second-child penalty increases, parents have fewer children than they would in the absence of the OCP, leading to improved health outcomes for the firstborn. However, the effects on education and assets are occupation-specific: children of high-skilled workers achieve higher educational attainment, children of farmers acquire more land, and children of low-skilled workers gain more savings and housing assets. The Q-Q trade-off varies across different dimensions of child quality and is heterogeneous across paternal occupations.

4.3 Effects on labor market outcomes

Next, I examine whether the differential impacts on human capital ultimately affect labor market outcomes. Table 4 presents the results of the second-child penalty on employment, occupational outcomes, *hukou* status, and annual personal income. The total effects for each occupational group is visualized in Figure A.4. Column (1) shows that the second-child penalty boosts employment for all children. There is no significant difference across the fathers' occupations. For those who are employed, column (2) shows that the second-child penalty increases the Treiman occupational score for children of non-farmers, with a stronger effect on the children of

Table 4: Heterogeneous effects of the second-child penalty on labor market outcomes

	(1) Employment (0/1)	(3) Occupational score (0-100)	(4) Urban *hukou* (0/1)	(5) Personal income (1,000 yuan)
Penalty	0.120*	-0.697	0.066	2.032
	(0.069)	(2.408)	(0.042)	(1.766)
Low-skill × Penalty	-0.049	2.478*	-0.013	1.565
	(0.041)	(1.280)	(0.025)	(1.128)
High-skill × Penalty	-0.042	6.007**	0.127**	4.110*
	(0.074)	(2.329)	(0.064)	(2.243)
R^2	0.338	0.417	0.421	0.502
Mean dep var	0.643	40.082	0.177	15.915
Observations	2750	1746	2750	2568

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.
2. Penalty is the fine a couple expects to pay if they want to have a second child. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation as high-skill if more than 50% of all fathers employed in this occupation have attended high school. The omitted group consists of children whose father was a farmer.
3. Baseline controls include province-cohort fixed effects, province-group fixed effects, group-cohort fixed effects, individual-level controls, and controls for OCP intensity before births. "Low-skill" and "High-skill" dummies are included in all regressions.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1988 from the CFPS data.

high-skilled workers. Column (3) demonstrates that the second-child penalty leads to a higher probability of obtaining an urban *hukou* for children of high-skilled workers, implying that they have a greater chance of permanent migration to cities with skilled and higher-wage labor markets. Lastly, column (4) shows that the effect on occupation and migration translates into a significantly larger effect on income for the children of high-skilled workers. If the second-child penalty increases by one standard deviation, equivalent to two-third of annual household income, the child's annual income increases by 1,300 *yuan* if the father is a farmer, 2,400 *yuan* if the father is a low-skill worker, and 4,100 *yuan* if the father is a high-skill worker. However, only the increases for non-farmers are statistically significant, as seen in Figure A.4.

4.4 Consequences for intergenerational income mobility

The previous analysis suggests that an increase in the second-child penalty increases the income for children of non-farmers, and the effects are larger for children of high-skilled workers. Since more skilled parents tend to earn more themselves, the higher cost of having a second child may have also changed the intergenerational associations in income. In this section, I investigate the

Table 5: Effects of the second-child penalty on intergenerational persistence

	(1) IGC Child's income (1,000 yuan)	(2) IRC Child's income rank (0-100)
Imputed father's income (α_0)	0.084	
	(0.055)	
Imputed father's income × Penalty (α_1)	0.123**	
	(0.061)	
Imputed father's income rank (α_0)		0.131***
		(0.048)
Imputed father's income rank × Penalty (α_1)		0.081*
		(0.049)
Penalty (α_2)	0.289	-0.805
	(1.895)	(4.001)
R^2	0.461	0.471
Mean dep var	15.915	49.466
Observations	2568	2567
IGC/IRC 1969-1978 cohorts (γ_1^{old})	0.255	0.317
IGC/IRC 1979-1988 cohorts (γ_1^{young})	0.451	0.406
ΔIGC/IRC explained by ΔPenalty ($0.38 * \alpha_1/(\gamma_1^{young} - \gamma_1^{old})$)	24%	35%

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1988 from the CFPS data.
2. In the upper panel, each column represents a separate regression estimated from equation 5. Robust standard errors in parentheses are clustered at the province-cohort level.
3. In the bottom panel, each γ_1 is estimated separately using equation 4.
4. Penalty is the fine a couple expects to pay for having a second child. ΔPenalty is the difference in mean penalty between the 1979-1988 cohorts and 1969-1978 cohorts, which equals to 0.38.
5. Province-group fixed effects, province-cohort fixed effects, and group-cohort fixed effects are included in all regressions, where group is defined by gender and minority status.
6. Father's income or income rank is imputed using the average income or income rank of men born between 1956 and 1965 in the same occupation within the same region.

impact of the second-child penalty on intergenerational income persistence using the approach described in Section 3.4.

The estimates of the α coefficients in equation 5 are presented in Table 5. Recall that α_1 captures how much the second-child penalty increases the intergenerational persistence of income, measured as correlations of father's and child's income (IGC) or rank-rank correlations (IRC). Column (1) shows the effects on IGC in income. When the penalty is zero, the partial correlation between the father's imputed income and the child's income is 0.08. Increasing the penalty by one standard deviation, equivalent to two-third of annual household income, significantly raises this correlation by 0.08. Column (2) presents the results for the IRC based on income percentile ranks. The rank-rank correlation is 0.13 when the penalty is zero and increases by 0.05 with a one-standard-deviation increase in the penalty. Both measures lead to the same conclusion: the implementation of OCP penalties significantly increases intergenerational persistence in income in rural China.[22]

[22]This approach could also be used to investigate how the OCP affects intergenerational wealth mobility. However, imputing fathers' wealth is more challenging, as occupations and region of residence explain only 20–30% of the variation in land ownership or assets. Despite this limitation, I impute fathers' wealth using the

To quantify the contribution of the OCP to the rise in intergenerational income persistence, I estimate the IGC and IRC using Equation 4 for children born between 1969–1978 and 1979–1988. The estimates are reported in the bottom panel of Table 5. The IGC is 0.2 higher and IRC is 0.09 higher for the children born after 1979, suggesting a large increase in intergenerational income persistence in rural China. Given that the second-child penalty increased by 0.38 between the two cohorts, a back-of-the-envelope calculation suggests that changes in the penalty account for 24% of the rise in IGC in income and 35% of the rise in IRC in income in rural China. As a comparison, Yu et al. (2021) show that OCP accounts for 35.4%—51.5% of the rise in IRC in China when considering both rural and urban populations.

5 What drives the heterogeneity?

An important next step is to understand why parents respond differently to fertility restrictions and why more skilled parents prioritize education more. Motivated by existing economic theories and empirical evidence, I discuss three factors that could potentially explain the heterogeneity in the Q-Q trade-off between parents in different occupations: heterogeneous returns to education, land rights insecurity, and credit constraints. A plausible framework to discuss these factors is to consider how parents allocate their income among their own consumption before retirement, investments in their children's human capital, and other expenditures. Additionally, parents may transfer cash and assets to their children, which improves their income-generating ability. Parents derive utility from both their own consumption and their children's expected income. This is either because they anticipate relying on their adult children for care and financial support in their old age or because they are motivated by altruism toward their children.

5.1 Heterogeneous returns to education

In response to higher second-child penalties, farmers and low-skilled workers transfer more land or housing assets their existing children, while high-skilled workers invest more in their children's education. Theoretically, if parents intend to transfer assets to their children in the future, the optimal investment in children's human capital equates the rate of return on human capital investments to the rate of return on physical capital investments (Galor, 2012). If the rate

same methodology and estimate the effects of the second-child penalty on IGC in land and non-land assets. The results, presented in Table A.2, suggest that the penalty also increases the persistence of land and non-land assets across generations.

Table 6: Mincerian returns to human capital

	(1)	(2)
	Dep var: Log(income)	
Measures of human capital:	Years of schooling	Height (sd)
Panel A.		
Human capital	0.059***	0.035***
	(0.003)	(0.013)
R^2	0.223	0.152
Panel B. Heterogeneity by father's occupation		
Human capital	0.057***	0.034**
	(0.004)	(0.016)
Low-skill × Human capital	-0.001	0.004
	(0.008)	(0.030)
High-skill × Human capital	0.024**	0.008
	(0.010)	(0.049)
R^2	0.224	0.152
Observations	3353	3322

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. This table reports observational Minceriam relationship between human capital and log income. Robust standard errors are in parentheses.
2. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation as high-skill if more than 50% of all fathers employed in this occupation have attended high school. The omitted group consists of individuals whose father was a farmer.
3. Other control variables are dummy indicators for paternal occupational types, both parents' educational attainment, gender, number of siblings, and birth year dummies.
4. The analysis is based on a sample of individuals who held a rural *hukou* at age 3 and were born between 1966 and 1975.

of return on physical capital investments is the same for all parents, the heterogeneity in the effect on the education suggests that the rate of return on investment in children's education is different across parents in different occupations. This implies that high-skilled workers perceive a higher return on investment in education for their children compared to farmers and low-skilled workers, leading to different investment strategies among parents from different occupational backgrounds.

To check whether the different perceived returns to education contribute to the heterogeneous effects on education and assets, I estimate the Mincerian returns to education and height while allowing for heterogeneity by paternal occupation. The Mincerian estimates reflect the partial correlations between the log of income and education or height. Using a sample of individuals born in rural China between 1966 and 1975, I regress the log of income in 2010 on years of schooling and height. To account for heterogeneity across paternal occupations, I include interaction terms between these human capital measures (education and height) and dummy variables for the father's occupation.

Table 6 presents the results. Panel A shows that the Mincerian returns to both education and height are positive and significant, suggesting that both human capital measures are positively correlated with adulthood income. Panel B reveals that the Mincerian returns to education are significantly higher for children of high-skilled workers, whereas returns to height do not vary by paternal occupation. These findings suggest that the observed correlation between income and education is stronger when the father has a high-skill occupation. This stronger correlation may influence parental investment decisions if it shapes parents' beliefs about heterogeneous returns to education. Specifically, parents might observe that obtaining a higher level of education is associated with a larger increase in earnings for children of high-skilled parents than for other children—either because the real returns to education are genuinely higher for these children or due to unobserved factors. This perceived higher return to education could motivate high-skilled parents to invest more heavily in their children's education compared to parents in other occupational groups, especially when resource constraints are relaxed by having fewer children.

The patterns in Table 6 align with the main findings: the second-child penalty has a similar impact on health outcomes across all children but exhibits heterogeneous effects on education. Specifically, when restricted to having only one child, high-skilled workers tend to increase investments in education, whereas others prioritize investments in physical assets, reflecting different perceived returns to education. Additionally, as shown in Table A.3 in the Appendix, the Mincerian returns vary by the father's occupation but not by the father's education. This finding is consistent with the results in Appendix A.2.2, which show that the effects on education differ by paternal occupation rather than educational level.

Table 6 indicates that the Mincerian returns on education for children of farmers are not significantly lower than those of children of low-skilled workers. Nonetheless, previous analysis shows that the education of firstborn children of low-skilled workers improves more following an increase in the second-child penalty compared to the education of firstborn children in farming households. This finding implies that there could be other factors limiting farmers' ability or willingness to increase their firstborn children's education when they are restricted to having only one child.

5.2 Land rights insecurity

In this section, I examine land rights insecurity as a potential explanation for the different effects on education between farmers' and low-skilled workers' children.

Land is a critical asset for farmers, and its availability and tenure security can impact their investment decisions. Under China's land tenure system, rural households have the right to use the land allocated to them and transfer land use rights to their children. However, they cannot sell or sublease the land in a market. The land is collectively owned by the village, and officials can reallocate it to other households within the same village if it is not used properly.[23] On the other hand, education played a crucial role in raising the accessibility of formal employment in urban areas to rural people in the late 1970s and early 1980s (Zhao, 1997) and in promoting labor mobility from agriculture to non-agriculture sectors in rural regions (Zhao, 1999). Formal urban employment or rural non-agricultural employment of household members increases the risk of losing the land allocated to rural households (Zhao, 2020; Adamopoulos et al., 2024). As a result, parents may face a trade-off between investing in the education of the children and keeping the children in the agricultural sector.

In Appendix A.5, I outline a simple model to explain how a reduction of child quantity can lead to a decrease in the education of the first child in the presence of land rights insecurity. The model assumes that child quantity is exogenous and can be either one or two, which simplifies the analysis but is also consistent with the finding that the second-child penalty increases the probability of being the only child. There are two key additional assumptions. First, parents are concerned not only with their own consumption and their children's expected resources but also with the perceived future value of household land, which enters the parent's utility function with a weight λ. Second, if land rights insecurity exists, the perceived future value of land decreases as children's education increases, due to the role of education in promoting non-agricultural employment. Notably, it is enough to have only one child to remain in the agricultural sector to secure land use rights. Denoting the current value of land by L, under these two assumptions, land enters the parent's utility function as $\lambda(1-e)L$, where e is the education of the only or less educated child. The model shows that land rights insecurity generates an additional opportunity cost of education equaling to λL for the child who is supposed to stay in the agricultural sector.

The model generates a testable prediction: if $\lambda L > 0$, in families with two children, land rights insecurity generates an educational gap between the siblings. This occurs because parents

[23] In Appendix A.1, I provide a detailed description of the *hukou* system and the land tenure system in China.

Table 7: Education and height differences between the first and second child, by father's occupation

	(1) Years of schooling	(2) High school	(3) Height (sd)	(4) Land assets
Panel A. First son vs second son in two-son families				
Farm × First son	1.283**	0.102*	-0.004	-1.857*
	(0.505)	(0.059)	(0.136)	(1.122)
Non-farm × First son	0.359	0.079	-0.135	-1.158
	(0.587)	(0.090)	(0.166)	(1.649)
R^2	0.280	0.191	0.145	0.120
Mean dep var farm	8.302	0.240	-0.017	5.467
Mean dep var nonfarm	10.048	0.365	0.193	2.982
Observations	351	351	349	348
Panel B. First daughter vs second daughter in two-daughter families				
Farm × First daughter	0.616	0.083	-0.008	0.618
	(0.523)	(0.065)	(0.153)	(1.287)
Non-farm × First daughter	-0.547	0.031	0.182	1.776**
	(0.848)	(0.111)	(0.219)	(0.852)
R^2	0.413	0.331	0.197	0.211
Mean dep var farm	7.726	0.172	-0.043	5.622
Mean dep var nonfarm	11.033	0.554	0.240	1.854
Observations	307	307	298	306

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. This table reports the differences in human capital and land assets by birth order and whether the father is a farmer. The omitted group is a second-born son/daughter. Robust standard errors in parentheses.
2. Farm (Non-farm) is a dummy indicator that takes value one if the respondent's father was employed in an agricultural (non-agricultural) occupation when the respondent was aged 12, and zero if otherwise.
3. Other control variables are birth year fixed effects, gender, and both parents' educational attainment and age.
4. The analysis is based on a sample of children from families with only two children of the same gender who held a rural *hukou* at age 3 and were born between 1966 and 1990.

invest more in one child's education while reducing investment in the other's to mitigate the risk of losing the allocated land while maximizing the expected resources of both children. To test this prediction, I examine a sample of children born between 1966 and 1990 in two-child families. I estimate the differences in education and height between the first and second children based on the father's occupation, distinguishing between farmers and non-farmers. Given potential differential treatment of sons and daughters in land allocations, I analyze the data separately by gender and focus on families with two children of the same gender. The results, presented in Table 7, show an education gap among sons whose fathers are farmers. This gap is evident for both measures of education in farming families with two sons but is small and insignificant in non-farming families, which aligns with the fact that farmers tend to have larger λL. I find no gap for height or in families with two daughters. Overall, the results for two-son families are consistent with the model with land rights insecurity in Appendix A.5, where the education of the less educated son reduces the perceived future value of land from the parent's perspective.

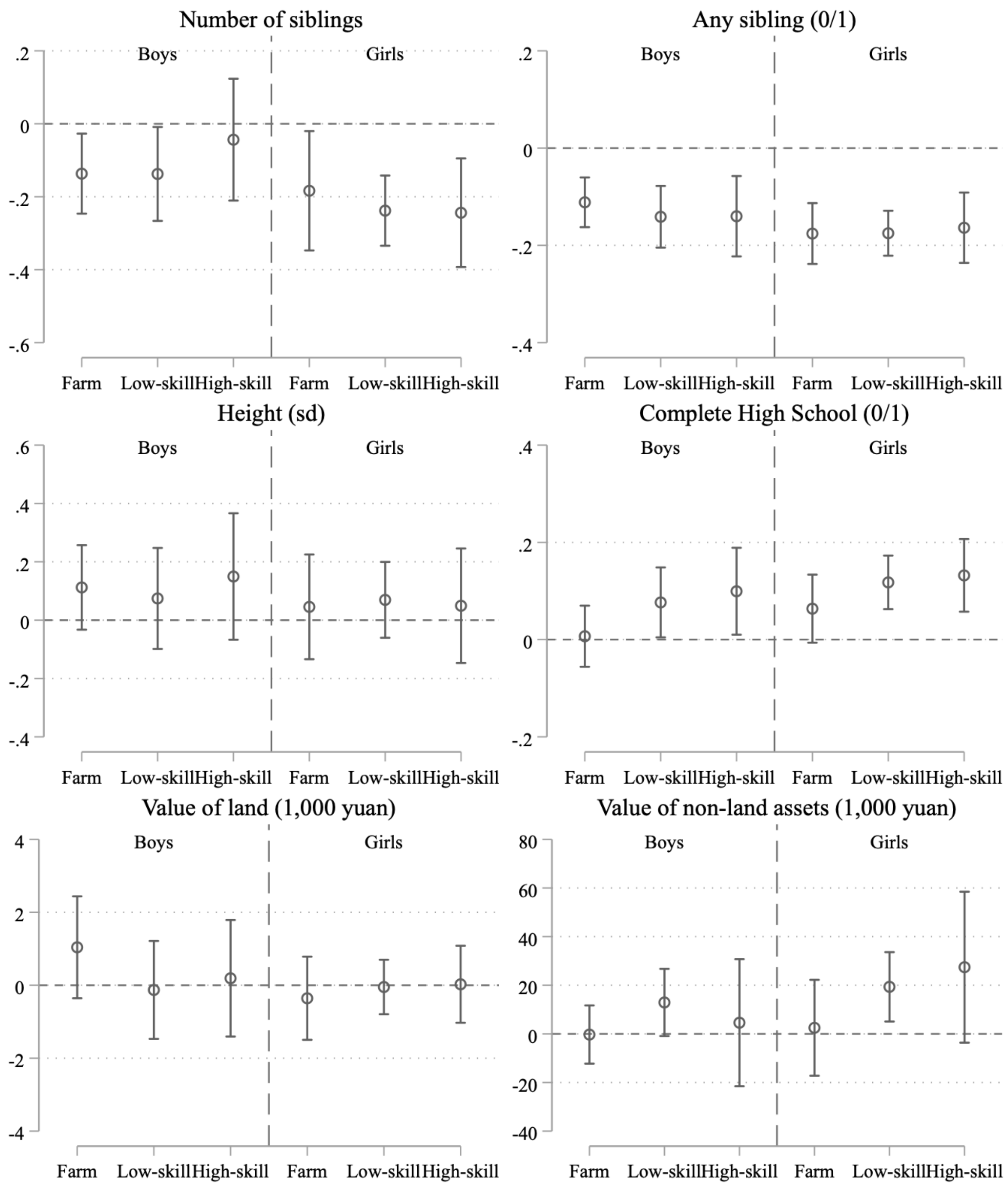
Panel A of Table 7 also shows that farmers who face land rights insecurity are more likely to educate the first son while leave more land to the second son. This finding is in line with sociological studies observing that in pre-OCP China, parents most often chose the youngest son to live with and left their remaining property to the youngest son after their deaths (Unger, 2006).Therefore, the opportunity cost of education due to land rights insecurity is more likely to be borne by the second and younger son, who is expected to stay home. When farmers could only have one son, the cost is shifted to the only child. Hence, reducing the number of children from two to one relaxes the parents' resources constraints, but increases the opportunity cost of education for the firstborn son. If this cost is sufficiently large, reducing the number of children from two to one could result in a reduction in the education of the first child.

Panel B of Table 7 reveals no gap in either education or land assets for daughters from farming households, suggesting that the daughter's education does not impact the perceived future value of land from the parents' perspective. Consequently, reducing the number of children should have no impact on the opportunity cost of education for firstborn daughters, even if their fathers are farmers. This implies that the difference in the educational impact of the second-child penalty between farmers and low-skilled workers should be less pronounced if the firstborn is a daughter rather than a son. To assess this, I estimate the heterogeneous effects of the second-child penalty by gender and the father's occupation.[24] The results are shown in Figure 4. The effects are similar across genders with two notable distinctions. First, the heterogeneous effects on land assets are solely driven by firstborn sons and not daughters. Second, daughters of farmers also experience an increase in education when the second-child penalty is higher, whereas sons do not. The gendered effects on land and education align with the findings in Table 7 indicating that parents trade-off the sons' education for land security but not daughters'.

In summary, the evidence highlights a mechanism through which the presence of a second child increases the education of the firstborn. This mechanism arises from a trade-off in farming households where the education of the younger child is sacrificed to ensure land security. Consequently, the firstborn child receives more education but less land. As the second-child penalty increases and parents are restricted to having only one child, the only child tends to experience

[24]A large component of the variation in the second-child penalty comes from the gender of the firstborn, preventing us from estimating the heterogeneity effects by gender using only the rural sample. Therefore, I add the urban sample to be able to estimate the effects by gender. As exemptions for urban couples were extremely rare between 1979 and 2000, I assume no free second-child permit for urban *hukou* holders. Notice that the implementation of the second-child penalty differed substantially between urban and rural *hukou* holders, and urban and rural China followed very different paths in socioeconomic development. Hence, I exploit the urban-rural variation in second-child penalty only in this exercise.

Figure 4: Total effect of the second-child penalty by father's occupation and gender



Note: 1. Subfigures display the estimated effect of the second-child penalty on various outcome variables by gender and paternal occupations, along with their corresponding 90% confidence intervals based on robust standard errors clustered at the province-cohort level.
2. Farm is a dummy indicator that takes value one if the respondent's father primarily worked as a farmer when the respondent was aged 12, and zero if otherwise. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation is high-skill if more than 50% of all fathers employed in this occupation have attended high school.
3. Baseline controls include province-cohort fixed effects, province-group fixed effects, group-cohort fixed effects, individual-level controls, and controls for OCP intensity before births. "Low-skill" and "High-skill" dummies are included in all regressions.
4. The analysis is based on a sample of firstborn children born between 1966 and 1990 from the CFPS data.

a decline in educational attainment alongside an increase in land assets. This mechanism only applies to sons and not daughters, and hence, the positive effect of the second-child penalty on land assets and minimal effect on education are predominantly driven by boys, not girls, from farming households. This channel may be an alternative explanation for the finding of Qian (2009), who shows that having a second child significantly increases school enrollment of firstborn children in rural China. Consistent with Mogstad and Wiswall (2016) and Guo et al. (2021), the findings show that child quantity and quality may appear complementary at lower birth orders if having only one child is suboptimal from the parents' perspective.

5.3 Credit market imperfection

Studies in developed countries mostly find no evidence of a child Q-Q trade-off (e.g Angrist et al., 2010). One theoretical possibility is that parents use perfect capital markets to fund investment despite resource constraints. Assuming parents have access to credit at the market interest rate, they would invest in their children's human capital until the marginal return to investment equals the interest rate (Heckman and Mosso, 2014). Consequently, parents would not alter their investment in child health or education in response to an exogenous change in child quantity. The finding that a higher second-child penalty leads to improved health outcomes for all groups of children suggests that regardless of occupations, Chinese parents are resource constrained and face credit market imperfections. These imperfections hinder them from making efficient investments in their children's health.

However, credit constraints alone cannot explain the heterogeneity in the effects on education. If farmers and low-skilled workers are more constrained and hence unable to increase educational investments in their children as much as high-skilled workers, they would not be able to invest in housing and financial assets as well. Contrary to this expectation, children of low-skilled workers own significantly more housing and financial assets when their parents face higher second-child penalties. The last plot in Figure A.3 also shows that the second-child penalty increases the financial assets for farmers' children as much as for other children. These findings suggest that parents engaging in farming or low-skill work could still finance their children's education but choose not to do so, potentially because the returns to education are lower than those to savings or purchasing homes for their children.

6 Conclusion

This paper establishes a causal link between family planning policies and the increasing intergenerational income persistence in rural China, driven by heterogeneity in the child quantity-quality trade-off across parental occupations. The OCP imposes penalties for unauthorized births, thereby reducing the likelihood of having a second child. The penalty also affects the outcomes of the firstborn, but in ways that vary by the father's occupation. Higher penalties improve firstborn health universally but increase educational attainment only when the father is employed in a skilled occupation. This boost in education leads to better occupational outcomes, higher migration probability, and increased income for these children. In contrast, for fathers engaged in farming or low-skill work, higher penalties result in greater land or non-land asset ownership for their children. Since father's income also increases with the skill level of father's occupation, the heterogeneous effects on children's income further contribute to an increase in intergenerational persistence of income.

Two mechanisms explain the observed heterogeneous responses. First, the correlation between education and adult income is stronger for children of high-skilled workers. High-skilled parents may perceive education as particularly valuable for their children and hence invest more in that when resource constraints are relaxed by having only one child. Second, land rights insecurity creates an opportunity cost of education for farmers' children, which is typically borne by younger children when farmers have more than one child, but shifted to the firstborn when farmers are restricted to having only one child. These mechanisms explain why the second-child penalty has the smallest effect on education for children of farmers and the largest effect for children of skilled workers, as well as why higher penalties result in firstborns owning more land in farming households.

An important question arises regarding whether a policy that reduces the cost of having more children could foster intergenerational mobility. The Chinese government abolished the OCP since 2015. Yet, birth rates continued to decline, reaching their lowest level in 2023. One explanation is that housing and education costs are so high that many couples cannot afford to have more than one child. This study confirms that when confronted with substantial OCP penalties, parents who could not have a second child tend to increase investments in education or housing for their only child. After years of the OCP, the costs associated with housing and education for an only child may have increased to such an extent that parents find it financially challenging to afford another child. Parents may be reluctant to compromise the wellbeing of their only child by having a second child.[25] Consequently, parents who invest a lot in education or housing of the only child are unlikely to respond significantly to the relaxation of the one-child rule. However, the relaxation of the one-child rule may lead to increased fertility among farmers, who invest less in education or housing for their children and desire for offspring to help sustain the household farm. Therefore, relaxing the one-child rule alone may not trigger an increase in intergenerational mobility, but could potentially contribute to an increase in the proportion of children from farming households with lower levels of education in the next generation.

The results of this study highlight the importance of accounting for heterogeneity in parental responses when evaluating family planning policies. Such policies could increase upward mobility if the primary effects are driven by enabling disadvantaged women to better plan and control their fertility (Bailey et al., 2019). On the other hand, family planning policies could decrease mobility if it triggers a universal decrease in fertility but a heterogeneous response in parental investments, as not all parents channel the resources saved from reduced fertility into children's human capital. If only skilled, wealthier parents invest additional resources in their children's education, this can amplify existing inequalities in parental investments across socioeconomic backgrounds, further increasing intergenerational persistence of socioeconomic status. My findings suggest that policies aimed at increasing the (perceived) returns to education or reducing the opportunity costs of education for children from low socioeconomic backgrounds may help mitigate these investment disparities and enhance intergenerational mobility.

[25] This is likely to happen when there are externalities in parental investments, such that parents care about the *relative* wellbeing of their children as compared to other children. Rossi and Xiao (2023) and Kim et al. (2023) provide evidence of such externalities in China and South Korea. They show that such externalities generate spillovers in fertility decisions, which make low fertility levels "self-sustaining."

References

Adamopoulos, T., Brandt, L., Chen, C., Restuccia, D., and Wei, X. (2024). Land security and mobility frictions. *Quarterly Journal of Economics*, page qjae010.

Adermon, A., Lindahl, M., and Palme, M. (2021). Dynastic human capital, inequality, and intergenerational mobility. *American Economic Review*, 111(5):1523–48.

Alesina, A. F., Seror, M., Yang, D. Y., You, Y., and Zeng, W. (2020). Persistence despite revolutions. Working Paper 27053, National Bureau of Economic Research.

Angrist, J., Lavy, V., and Schlosser, A. (2010). Multiple experiments for the causal link between the quantity and quality of children. *Journal of Labor Economics*, 28(4):773–824.

Bailey, M. J., Malkova, O., and McLaren, Z. M. (2019). Does access to family planning increase children's opportunities? evidence from the war on poverty and the early years of title x. *Journal of Human Resources*, 54(4):825–856.

Becker, G. S. and Lewis, H. G. (1973). On the interaction between the quantity and quality of children. *Journal of Political Economy*, 81(2):S279–S288.

Bhaskar, V., Li, W., and Yi, J. (2023). Multidimensional premarital investments with imperfect commitment. *Journal of Political Economy*, 131(10):2893–2919.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2005). The more the merrier? The effect of family size and birth order on children's education. *Quarterly Journal of Economics*, 120(2):669–700.

Bolt, U., French, E., Hentall-MacCuish, J., and O'Dea, C. (2021). The intergenerational elasticity of earnings: Exploring the mechanisms. IFS Working Paper W21-07, Institute for Fiscal Studies.

Boneva, T. and Rauh, C. (2018). Parental beliefs about returns to educational investments—the later the better? *Journal of the European Economic Association*, 16(6):1669–1711.

Bütikofer, A., Dalla-Zuanna, A., and Salvanes, K. G. (2022). Breaking the links: Natural resource booms and intergenerational mobility. *The Review of Economics and Statistics*, pages 1–45.

Caucutt, E. M. and Lochner, L. (2020). Early and late human capital investments, borrowing constraints, and the family. *Journal of Political Economy*, 128(3):1065–1147.

Chan, K. W. (2009). The Chinese *hukou* system at 50. *Eurasian Geography and Economics*, 50(2):197–221.

Chen, Y. and Fang, H. (2021). The long-term consequences of China's "Later, Longer, Fewer" campaign in old age. *Journal of Development Economics*, 151.

Chen, Y. and Huang, Y. (2020). The power of the government: China's family planning leading group and the fertility decline since 1970. *Demographic Research*, 42:985–1038.

Chetty, R., Hendren, N., Kline, P., and Saez, E. (2014). Where is the land of Opportunity? The Geography of Intergenerational Mobility in the United States *. *The Quarterly Journal of Economics*, 129(4):1553–1623.

Clarke, D. (2018). Children and their parents: A review of fertility and causality. *Journal of Economic Surveys*, 32(2):518–540.

de Chaisemartin, C. and D'Haultfœuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.

de Silva, T. and Tenreyro, S. (2017). Population control policies and fertility convergence. *Journal of Economic Perspectives*, 31(4):205–28.

Doepke, M. (2015). Gary Becker on the quantity and quality of children. *Journal of Demographic Economics*, 81(1):59–66.

Ebenstein, A. (2010). The "missing girls" of China and the unintended consequences of the One Child Policy. *Journal of Human Resources*, 45(1):87–115.

Fan, Y., Yi, J., and Zhang, J. (2021). Rising intergenerational income persistence in China. *American Economic Journal: Economic Policy*, 13(1):202–30.

Galor, O. (2012). The demographic transition: Causes and consequences. *Cliometrica, Journal of Historical Economics and Econometric History*, 6(1):1–28.

García, J. L. (2022). Pricing children, curbing daughters: Fertility and the sex ratio during China's One-Child Policy. *Journal of Human Resources*, forthcoming.

Guo, R., Yi, J., and Zhang, J. (2021). Rationed fertility: Treatment effect heterogeneity in the child quantity-quality tradeoff. Technical report. Unpublished Manuscript.

Guo, R., Yi, J., and Zhang, J. (2022). *The Child Quantity–Quality Trade-Off*, pages 1–23. Springer International Publishing, Cham.

Heckman, J. J. and Mosso, S. (2014). The economics of human development and social mobility. *Annual Review of Economics*, 6(1):689–733.

Huang, W., Pan, Y., and Zhou, Y. (2023). One-Child Policy, marriage distortion, and welfare loss. *Review of Economics and Statistics*, pages 1–47.

Jia, R., Lan, X., and Padró i Miquel, G. (2021). Doing business in China: Parental background and government intervention determine who owns busines. *Journal of Development Economics*, 151:102670.

Kim, S., Tertilt, M., and Yum, M. (2023). Status externalities in education and low birth rates in Korea. *Available at SSRN 3866660*.

Li, H., Yi, J., and Zhang, J. (2011). Estimating the effect of the one-child policy on the sex ratio imbalance in China: Identification based on the difference-in-differences. *Demography*, 48(4):1535–1557.

Li, H., Zhang, J., and Zhu, Y. (2008). The quantity-quality trade-off of children in a developing country: Identification using Chinese twins. *Demography*, 45(1):223–243.

Liu, H. (2014). The quality–quantity trade-off: Evidence from the relaxation of China's one-child policy. *Journal of Population Economics*, 27(2):565–602.

Mogstad, M. and Wiswall, M. (2016). Testing the quantity–quality model of fertility: Estimation using unrestricted family size models. *Quantitative Economics*, 7(1):157–192.

National Bureau of Statistics of China (2010). *China Compendium of Statistics 1949-2008*. China Statistics Press.

Ngai, L. R., Pissarides, C. A., and Wang, J. (2018). China's mobility barriers and employment allocations. *Journal of the European Economic Association*, 17(5):1617–1653.

Nybom, M. and Stuhler, J. (2017). Biases in standard measures of intergenerational income dependence. *Journal of Human Resources*, 52(3):800–825.

Qian, N. (2009). Quantity-quality and the one child policy: The only-child disadvantage in school enrollment in rural China. Working Paper 14973, National Bureau of Economic Research.

Rosenzweig, M. R. and Wolpin, K. I. (1980). Testing the quantity-quality fertility model: The use of twins as a natural experiment. *Econometrica*, 48(1):227–240.

Rosenzweig, M. R. and Zhang, J. (2009). Do population control policies induce more human capital investment? Twins, birth weight and China's "One-Child" policy. *Review of Economic Studies*, 76(3):1149–1174.

Rossi, P. and Xiao, Y. (2023). Spillovers in Childbearing Decisions and Fertility Transitions: Evidence from China. *Journal of the European Economic Association*, 22(1):161–199.

Scharping, T. (2013). *Birth Control in China 1949-2000: Population policy and demographic development*. Routledge.

Seshadri, A. and Zhou, A. (2022). Intergenerational mobility begins before birth. *Journal of Monetary Economics*, 129:1–20.

Treiman, D. J. (1977). *Occupational Prestige in Comparative Perspective*. Academic Press.

Unger, J. (2006). Family customs and farmland reallocations in contemporary Chinese villages. *Social Transformations in Chinese Societies*, 1:113–130.

Xie, Y. (2012). China Family Panel Studies (2010) user's manual. Technical report, Institute of Social Science Survey, Peking University. `http://www.isss.edu.cn/cfps/d/file/EN/Documentation/js/2014-09-18/bedee4470e0469260939e40eac2415c9.pdf`.

Xie, Y. and Lu, P. (2015). The sampling design of the China Family Panel Studies (CFPS). *Chinese Journal of Sociology*, 1(4):471–484. PMID: 29854418.

Yu, Y., Fan, Y., and Yi, J. (2021). One-child policy, differential fertility, and intergenerational transmission of inequality in China. Working paper.

Zhang, J. (2017). The evolution of China's one-child policy and its effects on family outcomes. *Journal of Economic Perspectives*, 31(1):141–60.

Zhao, X. (2020). Land and labor allocation under communal tenure: Theory and evidence from China. *Journal of Development Economics*, 147:102526.

Zhao, Y. (1997). Labor migration and returns to rural education in China. *American Journal of Agricultural Economics*, 79(4):1278–1287.

Zhao, Y. (1999). Labor migration and earnings differences: The case of rural China. *Economic Development and Cultural Change*, 47(4):767–782.

Online Appendix

Appendix A.1 The *Hukou* system and the land tenure system in China

Currently, every individual in China possesses an official record referred to as *hukou* (household registration). This record contains the individual's date and place of birth, place of origin (either the father's or grandfather's birthplace), ethnic identity, and current place of residence. The *hukou* system was first established in 1958 to categorize the population into individuals with rural (agricultural) *hukou* and those with urban (non-agricultural) *hukou.* Initially, the system was employed as a means of controlling migration and organizing labor and resources for industrialization during the Great Leap Forward. The system was also utilized to provide welfare programs, which favored urban residents and were tied to their *hukou* status. Urban *hukou* holders received access to state-provided education, health care, employment, and food supply. Rural *hukou* holders, on the other hand, received land from the government for cultivation, which they could not sell or sublease (Adamopoulos et al., 2024).

Initially, the *hukou* system was strictly enforced. Individuals with urban *hukou* lived and worked in urban areas, while rural *hukou* holders resided in rural areas and worked in the agricultural sector. Since the early 1980s, however, rural *hukou* holders have been permitted to develop and work in nonfarm enterprises in rural areas. Beginning in 1985, migration restrictions were gradually lifted, allowing rural residents to work in cities and access urban welfare services without possessing urban *hukou* (Chan, 2009).

Before the late 1990s, formal education had a strong effect on labor mobility from the agricultural sector to nonagricultural sectors in rural areas (Zhao, 1999). Due to restrictions on migration to urban areas, obtaining formal education beyond middle school was almost necessary for obtaining an urban *hukou.* Popular approaches to obtain an urban *hukou* included attending high school and college or graduating from an ordinary specialized high school (*zhongzhuan*) to be assigned a job in the urban sector. Joining the army was also an option, and the army emphasized educational achievement.

Under the *hukou* system and land tenure system, rural households have the right to use the land allocated to them and transfer land use rights to their children. However, they cannot sell or sublease it in a land market. The land is collectively owned by the village, and officials can reallocate it to other households within the same village if it is not used properly. These practices lead to over-employment in the agricultural sector and low migration rates (Ngai et al., 2018; Adamopoulos et al., 2024). Insecure land tenure could also disincentivize farmers from educating their children, particularly when parents have a strong desire to secure more land and when education is crucial in promoting nonfarm employment and permanent migration to urban areas. In Section 5, I explore how land rights insecurity may influence the trade-off between child quantity and child education for farming households.

Appendix A.2 Robustness

Appendix A.2.1 Maternal occupation

Throughout the analysis, I focus on the father's occupation instead of the mother's for two reasons. First, fathers typically are the ones responsible for important decisions and are the primary income earners in Chinese households, particularly in rural areas during the period under analysis. Second, there are more missing values for the mother's occupation, possibly due to lower labor force participation rates among women. However, it is important to explore whether the observed heterogeneity is driven by the mother's occupation instead of the father's. To investigate this, I classify the mother's occupation into different groups based on the same classification used for the father's occupation. I then add the interaction between the mother's occupation and the second-child penalty to the analysis. The results, presented in Table A.4, indicate that the heterogeneity by paternal occupation remains largely unchanged after controlling for the interaction of maternal occupation with the second-child penalty. Therefore, it appears that the heterogeneity in the child Q-Q trade-off is primarily driven by the father's occupation rather than the mother's.

Appendix A.2.2 Paternal education

Education reflects one's human capital and income and is correlated with occupational choices. In this study, the measure of an occupation's skill intensity is based on the fraction of high school graduates in this occupation. To check if the heterogeneity by paternal occupation is in fact driven by paternal education, I add the interaction of paternal education with the second-child penalty and reestimate equation 3. Panel A of Table A.5 shows the results. I find that the heterogeneity by paternal occupation barely changes after controlling for the interaction of paternal education with the second-child penalty. Moreover, the effect of the penalty does not vary significantly by paternal education.[26] In Panel B of Table A.5, I only include the interactions between the penalty and paternal education. Again, I detect no heterogeneity across paternal education. The results suggest that using different measures of family backgrounds may lead to distinct conclusions about the heterogeneity in the Q-Q trade-off. The heterogeneity in the Q-Q trade-off is mainly driven by different paternal occupations, not different paternal educational levels.

Appendix A.2.3 Alternative measures of policy exposure

Ineligibility to second-child permits only — The identification relies on two variations: the changes in fine rates over time and the introduction of second-child permits to couples with only one daughter and minority couples. As pointed out by Zhang (2017), the fine rates may correlate with local financial situations and fertility demand. The province-cohort fixed effects could partly address this issue. However, if changes in local financial situations and fertility demand affect different groups differently, then the triple-difference estimates could be biased. To deal with the potential biases, I construct a new measure of policy exposure that relies only on the ineligibility to second-child permits and not the level of fines. The measure is given by

$$Ineligibility_{tgp}^{(1-10)} = 1 - \frac{1}{10}\sum_{s=1}^{10} Permit_{t+s,g,p} \tag{7}$$

This variable measures the fraction of time a couple is not eligible to a second-child permit during the ten years after the birth of the first child. I reestimate equation 3 but replace $Penalty_{tgp}^{(1-10)}$ with $Ineligibility_{tgp}^{(1-10)}$. The results are shown in Panel A of Table A.6. Using only the ineligibility to second-child permits for identification does not change the key findings.

Three-year birth spacing — The OCP imposes restrictions on not only fertility but also the space between the first and the second child. Officially, the second-child permit is only granted if the second child is born after a given period since the first child is born. I do not consider this variation because the information on how the birth-spacing requirement was enforced is not available. However, as a robustness check, I construct an alternative measure of the second-child penalty assuming that second-child permits are only granted for a birth spacing of three years. The results are shown in Table A.6 Panel B. The estimates with this new measure of the second-child penalty are similar to the baseline estimates.

[26]Controlling maternal education instead or both maternal and paternal education interacted with the second-child penalty gives similar results.

Variation in high exposure only — A recent study by de Chaisemartin and D'Haultfœuille (2020) has raised concerns about the interpretation of estimates obtained from a specification with two-way fixed effects. The authors highlight that in a difference-in-differences (DID) setting with group and year fixed effects, the estimated coefficients can be interpreted as a weighted sum of the average treatment effects (ATE) for each group and year. However, the weights associated with the ATEs can be negative, which can be problematic when the ATEs are heterogeneous across groups or years. If the negative weights are large and correlated with the heterogeneous treatment effects, the estimated coefficient and all ATEs can have different signs. This issue is particularly salient with continuous treatment variables since there may not be enough groups where the treatment remains stable between two periods.

The issue raised by de Chaisemartin and D'Haultfœuille (2020) also applies to the triple-difference strategy, which includes two-way fixed effects. Although it is unclear how to adapt their method to a triple-difference strategy that allows for heterogeneity by individual characteristics, I address this issue by using a specification that limits the incidence of negative weights. Specifically, I only use variation in the second-child penalty when the penalty is greater than one year of household income, estimating ATEs only for groups that face a relatively high penalty and assuming the ATE to be zero for groups that face a small penalty. The results of this analysis are shown in Panel C of Table A.6. Despite using a different measure of exposure, I find a similar pattern to that of the baseline estimates: there is no heterogeneous effect on family size and health, but significant heterogeneity in the effect on education, assets, and labor market outcomes. Moreover, the significance levels and signs of the estimates are consistent with those of the baseline estimates, suggesting that the baseline triple-difference estimates are unlikely to have a different sign from all ATEs.

Appendix A.2.4 Socioeconomic development

The identification relies on the assumption that the between-group differences in the outcomes would have trended similarly in all provinces in the absence of changes in the strictness of OCP enforcement specific to one group. This assumption would be violated if there were other changes in socioeconomic factors that affected different groups differently and correlated with the introduction of the exemptions or the changes in fine rates. To address this issue, I include four variables measuring time-variant provincial characteristics when the child was aged 12, including the log of gross regional product per capita, population density, number of high school teachers per capita, and number of health institutes per capita. These variables capture province-specific changes in socioeconomic development. The data are from the National Bureau of Statistics of China (2010). I interact these variables with gender and minority status to allow the effects to differ by gender and ethnicity. The results are shown in Table A.7 in the Appendix. The triple-difference estimates are similar to the estimates obtained without controlling for the group-specific effect of socioeconomic development.

Appendix A.3 Outcome variables

Table 1 Panel B shows the mean of the outcome variables for the whole sample and by gender. Below I provide some details on how the variables are constructed from CFPS.

Family size—The number of siblings is reported by the children, whereas the variable "Any sibling" is a dummy variable indicating having at least one sibling. Because the respondent also reported each sibling's gender and age, I create four other measures of sibling composition: whether the respondent has a brother, whether the respondent has a sister, whether the first sibling was born before the respondent turned age five, and whether the first sibling was born when the respondent was aged six or older.

Human capital—I consider three types of human capital: health, education, and cognitive development. To measure health status, I utilize information from questions on self-rated health and self-reported height. I generate a dummy that takes value one if the respondent reports being in excellent health. In the analysis, to adjust for gender differences in height, I normalize height by gender and also create a dummy variable that takes value one if the respondent's height is among the top 20% in the sample, conditional on gender. I use years of schooling and a dummy variable for high school graduates to measure educational attainment. In addition, CFPS administrated a math test and a word test to measure cognitive development. I construct a measure of cognitive development using the average of the two test scores.

Assets—In addition to human capital investments, parents could also make transfers to children to improve their quality of life in adulthood. While I cannot observe parental transfers to children, I observe the assets the child owned in 2010, which could partly reflect parental investments in assets and transfers to children.[27] I estimate the effect of the second-child penalty on the value of non-land assets, which is the sum of the value of housing properties, the value

[27]Notice that this information is only available at the household level. It is possible that the child and the parents live together and own the assets together. I find no significant effect of the second-child penalty on coresidence.

of financial assets, and the value of other assets. I also look at the effect of the second-child penalty on land value. But because the land is collectively owned in China, the value of land is measured by the productivity of the land allocated to the household in 2009. If no land is allocated to the household, the value is zero.

Occupational outcomes— My first occupational outcome is whether or not employed. Conditional on employment, I estimate the effect of the second-child penalty on the Treiman scale of the occupation. The Treiman scale of each occupation is taken from Treiman's Standard International Occupational Prestige Scale, which reflects the level of power and privilege associated with each occupation (Treiman, 1977). Finally, I estimate the effect of the second-child penalty on urban residency, that is, whether or not holding an urban *hukou* in 2010. Because an urban *hukou* makes the high-skill jobs in the urban sector more accessible, this variable also reflects an individual's labor market achievement.

Appendix A.4 Examples of the second-child penalty

Table A.8 provide examples illustrating how I construct the second-child penalty, taking advantage of the exemptions introduced for couples with only one daughter. I also provide examples using exemptions for minority couples in Table A.9. A comparison of columns (1), (3), and (5) in Table A.8 demonstrates how the second-child penalty differs by birth year and gender of the first child in the Liaoning province, where exemptions have been granted to couples with only a daughter since 1985. A couple who had their first child born in 1971, regardless of gender, would face a fine equal to 1.21 years of household income only if they had their second child after the first one turned age 8. However, if the firstborn was a daughter born in 1979, the couple would have to pay the same fine before the child turned age six, but would be exempted afterward. In contrast, if the first child was a boy, the couple would not receive any exemptions. Thus, this exemption rule creates variation by the gender of the first child in Liaoning. Finally, if the first child was born in 1990, the couple would not face any fines for having a second child if the first one is a girl. However, if the first child is a boy, the couple would have to pay a fine of either 1.2 or 5 years of income, depending on the age of the first child when they have the second.

The even columns in Table A.8 show how the second-child penalty differs by birth year and gender of the first child in the Hubei province, where exemptions for couples with only one daughter were introduced in 1991. For couples who gave birth to their first child before 1980, the second-child penalty is the same for both genders but varies across the birth years of the firstborn due to the changes in the level of fine over time. However, gender differences in the penalty emerge for cohorts born after 1980. The last column shows that couples with a firstborn daughter born in 1990 were fully exempted from the one-child rule, while couples with a firstborn boy born in 1990 had to pay a fine equal to 2.83 years of household income for the second child.

Appendix A.5 A model of Q-Q trade-off with land rights insecurity

I build a simple model upon Becker and Lewis (1973) to show how the results for children of farmers can be rationalized in a setting with land rights insecurity. Parents' utility depends on their own consumption, child quantity, and average child quality, but child quantity is taken as given. I consider two regimes. In the first regime, each couple has two children. They choose the amount of health investment h, which is the same for both children. They choose the amount of educational investment e, which can differ by children. In the second regime, each couple has only one child. I assume there is no saving or borrowing in both regimes to emphasize the role of land rights insecurity. It is a reasonable assumption given that credit constraints could not explain the heterogeneity in the Q-Q trade-off, as I discuss in Section 5.3.

One-child regime

The decision-maker in the model is a parent. and each parent has one child in this regime. For simplicity of exposition, I assume a linear utility function:

$$U = c + \alpha I + \lambda(1 - e)L$$

subject to

$$c = y - c_h h - c_e e$$

$$e \geq 0$$

where c is the consumption of the parent, y is the income of the parent, h is the investment in the health of the only child, and e is the investment in the education of the only child. c_h and c_e are the unit costs of health and education investments.

The child's expected resource I is given by

$$I = w(h, e) = a \log h + b \log(e + \underline{e})$$

where $\underline{e}$ if the minimum level of education one can attain at no cost, for instance, free compulsory education. The child's expected resources enter the parent's utility function with weight α, which can be thought of as capturing both altruism and a reduced-form representation of the parent's consumption in old age.

$(1-e)L$ is the expected value of land, which enters the parent's utility function with a weight λ. Education plays a significant role in promoting rural-urban migration or working outside of the agricultural sector. If the child migrates or finds a permanent job in the non-agricultural sector, the child cannot farm the land. Because the parent will lose the ability to farm the land in old age, the household will eventually lose the land. Hence, I assume that the expected value of land decreases with e, the education level of the only child.

Solutions

Interior solution. When $e \geq 0$ and $h > 0$, it can be shown that the investments in health and education are given by

$$e^{OC} = \frac{b\alpha}{c_e + \lambda L} - \underline{e}$$
$$h^{OC} = \frac{a\alpha}{c_h}$$

No education. When $\frac{b\alpha}{c_e+\lambda L} \leq \underline{e}$, parents choose to not invest in education. The investments in health and education are given by

$$e^{OC} = 0$$
$$h^{OC} = \frac{a\alpha}{c_h}$$

Two-child regime

In this regime, each parent has two children. The parent's utility is given by:

$$U = c + \alpha \bar{I} + \lambda(1 - \min\{e_1, e_2\})L$$

subject to

$$
\begin{aligned}
c &= y - 2c_h h - c_e(e_1 + e_2) \\
e_1 &\geq 0 \\
e_2 &\geq 0
\end{aligned}
$$

I assume that the parent spends the same on the health of both children but can spend differently on the education of the two children, denoted by e_1 and e_2.

Now the present value of land is $(1 - \min\{e_1, e_2\})L$. This specification assumes that as long as there is one child staying in the agricultural sector, the household can keep the land. To maximize the probability of retaining the land, parents would keep the less educated child in the agricultural sector. Hence, only the education of the less educated child matters.

With two children, parents care about the average expected resources of the children. Hence,

$$\bar{I} = \frac{1}{2}(w(h, e_1) + w(h, e_2)) = a \log h + \frac{b}{2} \log(e_1 + \underline{e}) + \frac{b}{2} \log(e_2 + \underline{e})$$

Solutions

Interior solution. Assume that $e_1 \leq e_2$. When $e_1 \geq 0$, $e_2 \geq 0$, and $h > 0$, the investments in health and education are given by

$$
\begin{aligned}
e_1^{TC} &= \frac{b\alpha}{2c_e} - \underline{e} \\
e_2^{TC} &= \frac{b\alpha}{2(c_e + \lambda L)} - \underline{e} \\
h^{TC} &= \frac{a\alpha}{2c_h}
\end{aligned}
$$

No education for one child. When $\frac{b\alpha}{2(c_e+\lambda L)} \leq \underline{e}$ and $\frac{b\alpha}{2c_e} \geq \underline{e}$, parents choose to not invest in education of one child. The investments in health and education are given by

$$
\begin{aligned}
e_1^{TC} &= \frac{b\alpha}{2c_e} - \underline{e} \\
e_2^{TC} &= 0 \\
h^{TC} &= \frac{a\alpha}{2c_h}
\end{aligned}
$$

No education for both children. When $\frac{b\alpha}{2c_e} \leq \underline{e}$, parents choose to not invest in education of both children. The investment in health and education is given by

$$e_1^{TC} = 0$$
$$e_2^{TC} = 0$$
$$h^{TC} = \frac{a\alpha}{2c_h}$$

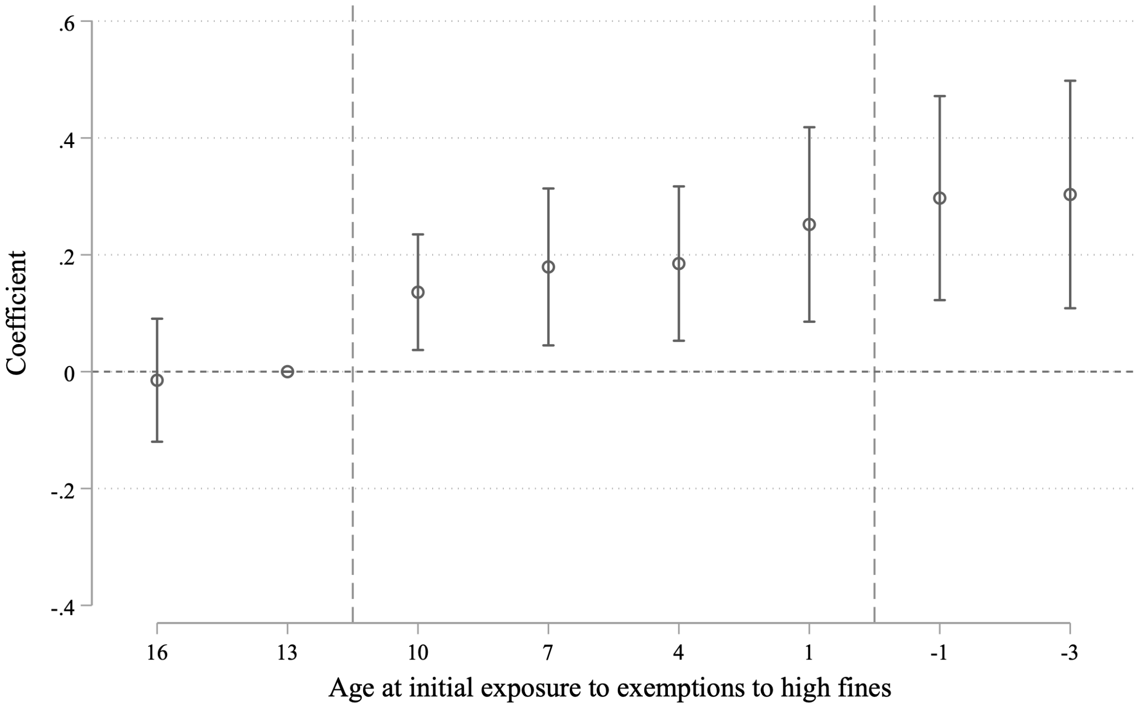
The model generates one testable implication: only families with $\lambda L > 0$ would have an unequal investment in children's education. Farmers are more likely to have $\lambda L > 0$ because they are more likely to own a positive amount of land and also put a positive weight on the value of expected land. Table 7 shows that in farming families with two sons, there is indeed an education gap between the first and second sons, where the first son attains, on average, a higher level of education. I do not find the same birth order difference for daughters, suggesting that the education of daughters does not matter for keeping the land.

It also predicts that when moving from the two-child regime to the one-child regime, there will be an increase in health investment h for the first child. Given that the firstborn attains more education, as shown in Table 7, the effect on the education of the firstborn depends on whether λL is bigger than c_e. If $\lambda L < c_e$, $e^{OC} > e_1^{TC}$, namely having a younger sibling decreases the firstborn's educational attainment. In this case, there exists a Q-Q trade-off for education. If $\lambda L > c_e$ and $\frac{b\alpha}{\underline{e}} \leq 2c_e$, $e^{OC} = e_1^{TC} = 0$, namely having a younger sibling or not does not matter for the firstborn's education. If $\lambda L > c_e$ and $\frac{b\alpha}{\underline{e}} > 2c_e$, $e^{OC} < e_1^{TC}$, that is, having a younger sibling increases the firstborn's educational attainment.

The intuition is as follows. The reduction in child quantity from two to one reduces the marginal cost of education by c_e but also increases the opportunity cost of education for the first child by λL. If the opportunity cost λL is higher than the marginal cost of education c_e, reducing child quantity from two to one increases the net cost of education from the parent's perspective. Hence, when parents own a lot of land now (large L) and care about the expected future value of land (large λ), a reduction in child quantity from two to one could lead to a reduction in the first child's education. In Section 5.2, I combine the model with empirical evidence to discuss how land rights insecurity may explain the different responses to the second-child penalty between farmers and non-farmers.

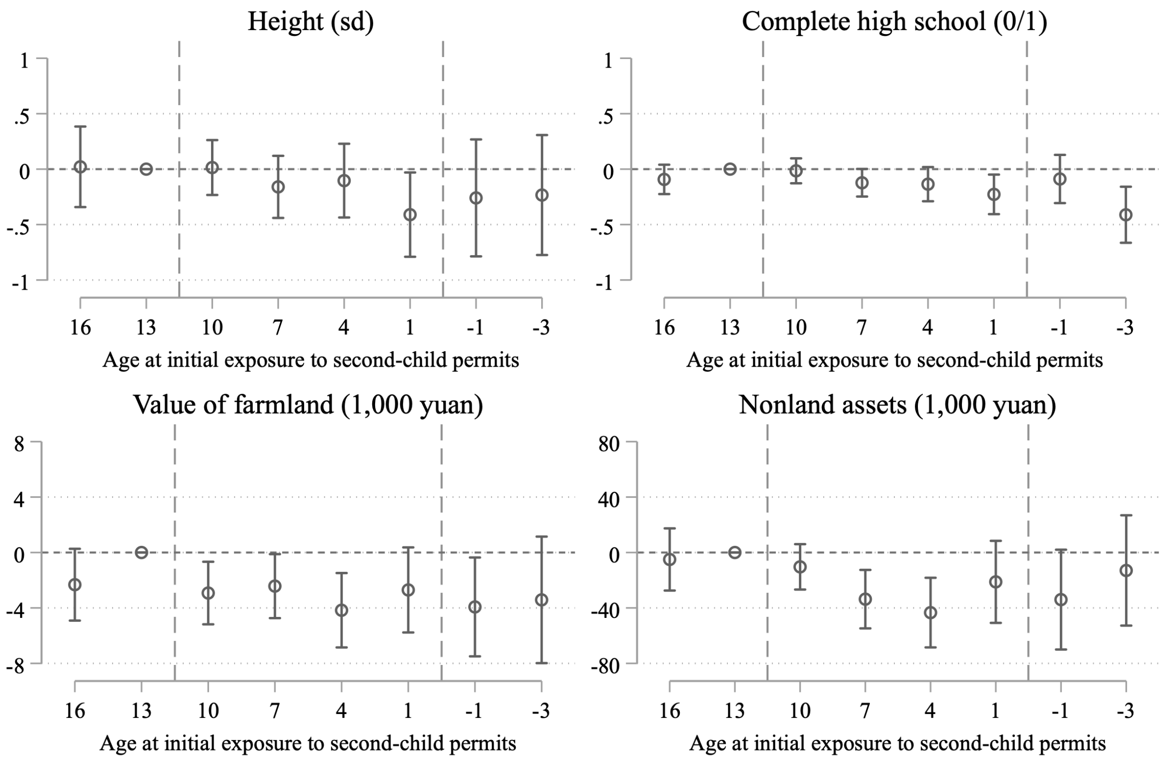
Appendix A.6 Appendix figures

Figure A.1: Event study: timing of exemptions to high fines and the probability of having any sibling



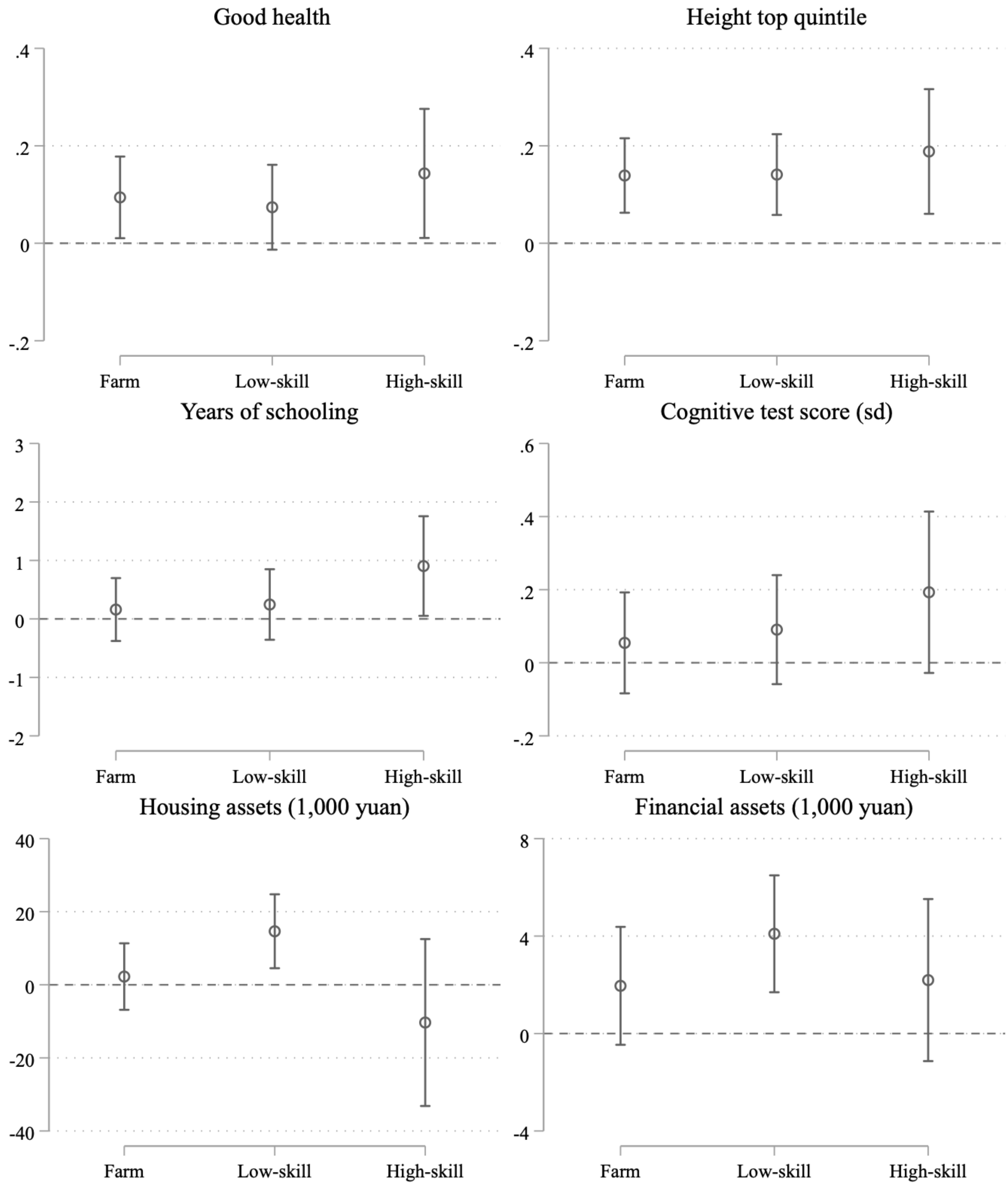
Note: The figure shows the coefficients and 90% confidence intervals (based on standard errors clustered at the province-cohort level) from the event study analysis. The sample consists of children born between 1966 and 1990, who had a rural *hukou* at age 3 and is the first child to their parents. The outcome is the probability of having any sibling. The x-axis is the age group j of the first child since when the parents are exempted from paying fines greater than one year of household income. The y-axis is the estimate for ξ_j in equation 6. The age group 13 is taken as the base group. The estimate for ξ_j represents how much a couple are more likely to have a second child if they become eligible to a second child permit when the first child is aged j to $j+2$ instead of 13 to 15, conditional on three-way fixed effects, individual controls, and controls for pre-birth OCP intensity described in Section 3.3. From left to right, the two dashed lines separates the sample into three groups that are subject to the OCP penalties for different durations during the ten-year window after the birth of the firstborn: always treated, partially treated, and never treated.

Figure A.2: Event study estimates for child quality outcomes



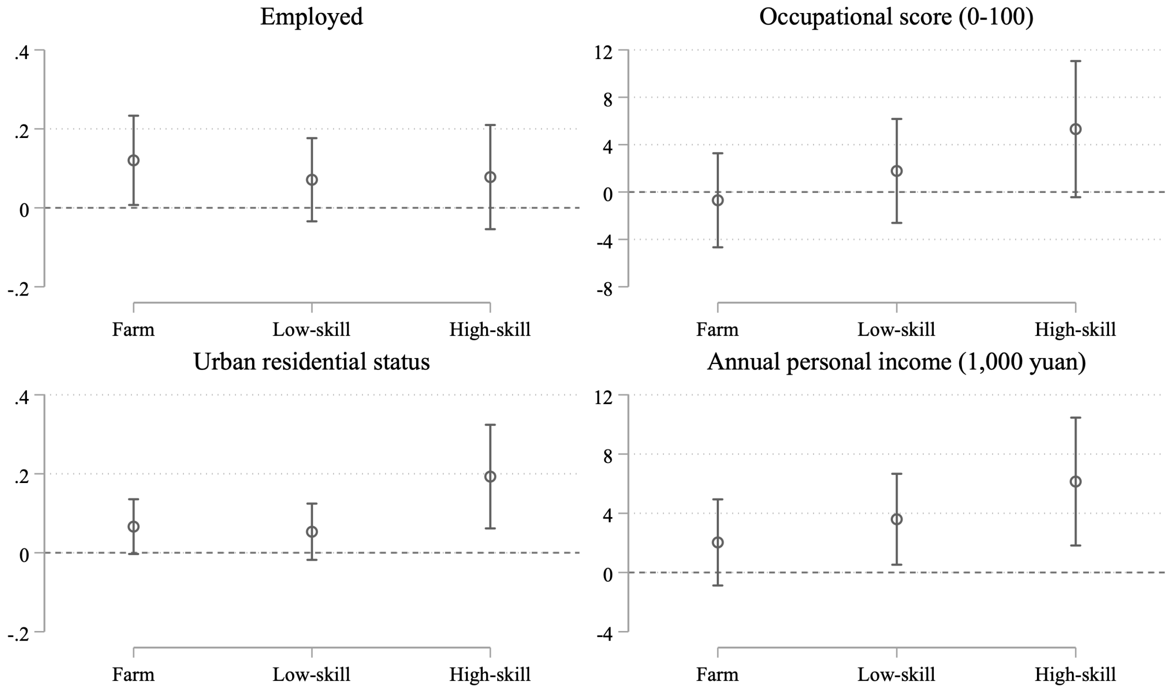
Note: The figure shows the coefficients and 90% confidence intervals (based on standard errors clustered at the province-cohort level) from the event study analysis. The sample consists of children born between 1966 and 1990, who had a rural *hukou* at age 3 and is the first child to their parents. The x-axis is the age group j of the first child since when the parents became eligible to a second-child permit. The y-axis is the estimate for ξ_j using equation 6. The age group 13 is taken as the base group. The estimate for ξ_j represents the difference in the outcome of the firstborn child if the parents become eligible to a second child permit when the firstborn child is aged j to $j+2$ instead of 13 to 15, conditional on three-way fixed effects, individual controls, and controls for pre-birth OCP intensity described in Section 3.3.

Figure A.3: Total effect of the second-child penalty by father's occupation on alternaltive measures of child quality



Note: 1. Subfigures display the estimated effect of the second-child penalty on various outcome variables for different paternal occupations, along with their corresponding 90% confidence intervals based on robust standard errors clustered at the province-cohort level.
2. Farm is a dummy indicator that takes value one if the respondent's father primarily worked as a farmer when the respondent was aged 12, and zero if otherwise. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation is high-skill if more than 50% of all fathers employed in this occupation have attended high school.
3. Baseline controls include province-cohort fixed effects, province-group fixed effects, group-cohort fixed effects, individual-level controls, and controls for OCP intensity before births. "Low-skill" and "High-skill" dummies are included in all regressions.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

Figure A.4: Total effect of the second-child penalty by father's occupation



Note: 1. Subfigures display the estimated effect of the second-child penalty on various outcome variables for different paternal occupations, along with their corresponding 90% confidence intervals based on robust standard errors clustered at the province-cohort level.
2. Farm is a dummy indicator that takes value one if the respondent's father primarily worked as a farmer when the respondent was aged 12, and zero if otherwise. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation is high-skill if more than 50% of all fathers employed in this occupation have attended high school.
3. Baseline controls include province-cohort fixed effects, province-group fixed effects, group-cohort fixed effects, individual-level controls, and controls for OCP intensity before births. "Low-skill" and "High-skill" dummies are included in all regressions.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1988 from the CFPS data.

Appendix A.7 Appendix tables

Table A.1: Balancing test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
						Father's primary occupation		
	Father's age	Mother's age	Father's education (year)	Mother's education (year)	Mother's age at birth	Farm	Low-skill	High-skill
Penalty	0.472	0.242	-0.410	0.091	0.129	-0.040	0.065	-0.024
	(0.444)	(0.409)	(0.400)	(0.375)	(0.346)	(0.038)	(0.041)	(0.026)
R^2	0.753	0.785	0.327	0.425	0.268	0.329	0.336	0.214
Mean dep var	58.306	55.950	5.741	3.541	23.385	0.677	0.257	0.066
Observations	2856	2823	2862	2834	2794	2894	2894	2894

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.

1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.

2. Penalty is the fine a couple expects to pay for the second child. Farm is a dummy indicator that takes value one if the respondent's father was employed in an agricultural occupation when the respondent was aged 12. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation as high-skill if more than 50% of all fathers employed in this occupation have attended high school.

3. All regressions include controls include province-cohort fixed effects, province-group fixed effects, group-cohort fixed effects, province-level average fine rates in 1979 interacted with gender-cohort and minority-cohort fixed effects, as well as province-level fine rates in the three years before birth interacted with gender and minority dummies.

4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

Table A.2: The second-child penalty and intergenerational wealth mobility

	(1) Land assets	(2) Non-land assets
Imputed father's land assets	0.147**	
	(0.059)	
Imputed father's land assets × Penalty	0.211***	
	(0.066)	
Imputed father's non-land assets		0.092
		(0.056)
Imputed father's non-land assets × Penalty		0.141**
		(0.059)
Penalty	-0.141	-2.198
	(0.960)	(6.007)
R^2	0.356	0.579
Mean dep var	4.663	48.767
Observations	2851	2763

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.
2. Penalty is the fine a couple expects to pay for the second child. Farm is a dummy indicator that takes value one if the respondent's father was a farmer when the respondent was aged 12.
3. See notes below Table 2 for the list of control variables.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.
5. Father's income is imputed use the average income of men born between 1946 and 1965 in the same type of occupations.

Table A.3: Mincerian returns to human capital by father's occupation and education

	(1)	(2)
	Dep var: Log(income)	
Measures of human capital:	Years of schooling	Height (sd)
Human capital	0.058***	0.033**
	(0.004)	(0.016)
Low-skill × Human capital	-0.001	0.003
	(0.008)	(0.031)
High-skill × Human capital	0.021**	0.009
	(0.010)	(0.052)
Middle school × Human capital	-0.009	0.001
	(0.009)	(0.040)
High school × Human capital	0.022	-0.020
	(0.014)	(0.061)
R^2	0.225	0.152
Observations	3353	3322

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.

1. This table reports observational Mincerian relationship between human capital and log income. Robust standard errors in parentheses.
2. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation as high-skill if more than 50% of all fathers employed in this occupation have attended high school. Middle school (high school) is dummy indicator that takes value one if the respondent's father finished 9 (12) years of schooling. The omitted group consists of children whose father was a farmer who did not finish at least 9 years of schooling.
3. Other control variables are both parents' educational attainment, interactions between gender and an indicator of being the first child, gender, number of siblings, and birth year dummies.
4. The analysis is based on a sample of children whose father was not a farmer, who held a rural *hukou* at age 3 and were born between 1966 and 1975.

Table A.4: Paternal occupation *vs* maternal occupation

	(1) Siblings	(2) Any sibling	(3) Height (sd)	(4) HS completion	(5) Land	(6) Assets	(7) Employed	(8) Occu. score	(9) Urban *hukou*	(10) Income
Penalty	-0.245**	-0.133***	0.222**	-0.027	1.401*	0.804	0.107	-0.051	0.065	2.774
	(0.105)	(0.044)	(0.102)	(0.044)	(0.847)	(5.630)	(0.072)	(2.639)	(0.042)	(1.794)
Father low-skill × Penalty	-0.068	-0.032	-0.012	0.072**	-1.258**	10.520**	-0.061	3.089**	-0.019	2.374**
	(0.060)	(0.025)	(0.060)	(0.034)	(0.558)	(4.586)	(0.043)	(1.296)	(0.025)	(1.055)
Father high-skill × Penalty	0.021	0.017	0.002	0.157***	-1.032	-15.932	-0.044	6.771***	0.122**	5.021**
	(0.119)	(0.050)	(0.115)	(0.050)	(0.833)	(12.088)	(0.078)	(2.535)	(0.061)	(2.115)
Mother low-skill × Penalty	0.191**	0.038	-0.181	-0.023	4.302	2.854	0.130*	-2.017	0.021	-1.436
	(0.079)	(0.045)	(0.114)	(0.057)	(3.125)	(12.992)	(0.070)	(2.346)	(0.068)	(2.668)
Mother high-skill × Penalty	0.093	0.017	-0.068	0.016	-0.155	15.913**	0.080	-1.003	-0.000	-0.952
	(0.070)	(0.031)	(0.087)	(0.038)	(0.671)	(7.997)	(0.049)	(2.708)	(0.052)	(1.797)
Mother occu. missing × Penalty	0.101	0.002	0.049	-0.037	-0.364	5.166	0.001	-2.781*	-0.013	-3.618***
	(0.079)	(0.031)	(0.070)	(0.044)	(0.698)	(5.738)	(0.042)	(1.440)	(0.035)	(1.335)
R^2	0.545	0.590	0.381	0.438	0.387	0.595	0.341	0.420	0.427	0.506
Mean dep var	1.471	0.771	0.015	0.245	4.663	48.767	0.643	40.082	0.177	15.915
Observations	2894	2894	2807	2894	2851	2763	2750	1746	2750	2568

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.
2. Penalty is the fine a couple expects to pay for the second child. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation as high-skill if more than 50% of all fathers employed in this occupation have attended high school. Middle school (high school) is dummy indicator that takes value one if the respondent's father finished 9 (12) years of schooling.
3. See notes below Table 2 for the list of control variables.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

Table A.5: Paternal occupation *vs* paternal education

	(1) Siblings	(2) Any sibling	(3) Height (sd)	(4) HS completion	(5) Land	(6) Assets	(7) Employed	(8) Occu. score	(9) Urban *hukou*	(10) Income
Panel A. Adding interactions of the second-child penalty with paternal education										
Penalty	-0.219**	-0.130***	0.217**	-0.041	1.785**	-1.235	0.122*	-1.626	0.084*	2.339
	(0.103)	(0.044)	(0.107)	(0.047)	(0.878)	(6.523)	(0.068)	(2.481)	(0.045)	(1.851)
Low-skill × Penalty	-0.042	-0.033	-0.043	0.070**	-1.438***	16.392***	-0.049	2.770**	-0.018	1.611
	(0.054)	(0.026)	(0.055)	(0.032)	(0.525)	(3.843)	(0.041)	(1.353)	(0.025)	(1.154)
High-skill × Penalty	0.100	0.029	-0.033	0.143***	-1.001	-12.485	-0.031	5.432**	0.138**	5.165**
	(0.120)	(0.050)	(0.129)	(0.052)	(1.106)	(11.446)	(0.077)	(2.616)	(0.067)	(2.321)
Middle school × Penalty	0.025	0.007	0.015	0.029	-0.336	5.614	-0.009	2.343*	-0.043	-1.629
	(0.050)	(0.022)	(0.052)	(0.033)	(0.562)	(5.528)	(0.038)	(1.364)	(0.030)	(1.127)
High school × Penalty	-0.082	-0.009	0.057	0.015	-0.622	0.529	-0.040	0.607	-0.032	-3.288*
	(0.123)	(0.044)	(0.112)	(0.047)	(0.752)	(7.359)	(0.059)	(2.443)	(0.046)	(1.707)
R^2	0.544	0.590	0.382	0.436	0.377	0.595	0.335	0.425	0.423	0.503
Panel B. Only interactions of the second-child penalty with paternal education										
Penalty	-0.230**	-0.149***	0.193	-0.008	1.095	6.651	0.101	0.256	0.060	2.988
	(0.112)	(0.047)	(0.119)	(0.055)	(0.967)	(7.018)	(0.064)	(2.671)	(0.038)	(1.818)
Middle school × Penalty	0.022	0.010	0.006	0.035	-0.336	3.906	-0.011	2.113	-0.018	-0.518
	(0.054)	(0.022)	(0.056)	(0.033)	(0.567)	(5.793)	(0.045)	(1.366)	(0.025)	(1.147)
High school × Penalty	-0.060	-0.005	0.045	0.042	-0.872	-0.041	-0.033	0.449	-0.005	-0.370
	(0.129)	(0.048)	(0.109)	(0.049)	(0.760)	(8.749)	(0.057)	(2.210)	(0.041)	(1.683)
R^2	0.540	0.590	0.384	0.437	0.377	0.591	0.346	0.428	0.420	0.499
Mean dep var	1.471	0.771	0.015	0.245	4.663	48.767	0.618	39.941	0.174	15.675
Observations	2894	2894	2807	2894	2851	2763	2845	1785	2894	2724

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.
2. Penalty is the fine a couple expects to pay for the second child. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation as high-skill if more than 50% of all fathers employed in this occupation have attended high school. Middle school (high school) is dummy indicator that takes value one if the respondent's father finished 9 (12) years of schooling.
3. See notes below Table 2 for the list of control variables.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

Table A.6: Alternative treatment measures

	(1) Siblings	(2) Any sibling	(3) Height (sd)	(4) HS completion	(5) Land	(6) Assets	(7) Employed	(8) Occu. score	(9) Urban *hukou*	(10) Income
Panel A. Measuring the second-child penalty using only second-child permit eligibility										
Ineligibility	-0.374	-0.240**	0.456**	-0.017	3.594**	17.196	0.194	-4.041	0.035	1.212
	(0.293)	(0.098)	(0.222)	(0.098)	(1.788)	(17.514)	(0.149)	(4.832)	(0.099)	(3.314)
Low-skill × Ineligibility	-0.125	-0.048	-0.068	0.087	-1.841*	30.976***	-0.094	2.470	-0.021	3.815*
	(0.122)	(0.049)	(0.107)	(0.063)	(0.971)	(8.406)	(0.060)	(2.234)	(0.048)	(1.998)
High-skill × Ineligibility	0.095	0.035	-0.123	0.201**	-0.787	8.455	-0.084	7.934**	0.196**	7.681**
	(0.210)	(0.089)	(0.193)	(0.091)	(1.346)	(19.843)	(0.110)	(3.449)	(0.094)	(3.544)
R^2	0.540	0.586	0.379	0.433	0.374	0.591	0.338	0.414	0.420	0.502
Panel B. Considering the requirement of a minimum 3-year spacing										
Penalty^s	-0.202	-0.145***	0.238*	-0.033	1.682*	5.238	0.126	0.299	0.089*	1.907
	(0.130)	(0.051)	(0.122)	(0.058)	(0.946)	(7.132)	(0.079)	(3.189)	(0.051)	(2.227)
Low-skill × Penalty^s	-0.058	-0.042	-0.025	0.082**	-1.522**	16.726***	-0.048	2.624*	-0.017	1.824
	(0.066)	(0.031)	(0.065)	(0.037)	(0.610)	(4.390)	(0.047)	(1.522)	(0.029)	(1.296)
High-skill × Penalty^s	0.015	-0.009	-0.010	0.167***	-1.279	-12.189	-0.041	6.133**	0.129*	4.084
	(0.131)	(0.054)	(0.131)	(0.053)	(1.165)	(13.927)	(0.083)	(2.806)	(0.073)	(2.556)
R^2	0.539	0.588	0.379	0.435	0.375	0.592	0.337	0.416	0.420	0.501
Panel C. Variation in high second-child penalty only										
Penalty × $\mathbb{I}(\text{Penalty} \geq 1)$	-0.195**	-0.088**	0.175*	-0.022	0.792	5.614	0.087	2.052	0.096**	1.174
	(0.098)	(0.040)	(0.094)	(0.041)	(0.716)	(5.619)	(0.058)	(2.064)	(0.038)	(1.703)
Low-skill × Penalty × $\mathbb{I}(\text{Penalty} \geq 1)$	-0.061	-0.042	-0.021	0.091***	-1.221**	14.147***	-0.054	2.780**	0.006	1.318
	(0.054)	(0.027)	(0.056)	(0.032)	(0.505)	(4.003)	(0.039)	(1.237)	(0.026)	(1.111)
High-skill × Penalty × $\mathbb{I}(\text{Penalty} \geq 1)$	0.040	0.016	-0.012	0.149***	-1.069	-19.041	-0.048	6.210**	0.097	3.583
	(0.123)	(0.054)	(0.103)	(0.048)	(0.964)	(13.029)	(0.076)	(2.463)	(0.064)	(2.344)
R^2	0.540	0.587	0.379	0.436	0.375	0.593	0.337	0.420	0.420	0.500
Mean dep var	1.471	0.771	0.015	0.245	4.663	48.767	0.618	39.941	0.174	15.675
Observations	2894	2894	2807	2894	2851	2763	2845	1785	2894	2724

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.

1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.

2. Penalty is the fine a couple expects to pay for the second child. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation as high-skill if more than 50% of all fathers employed in this occupation have attended high school. The omitted group consists of children whose father was a farmer.

3. See notes below Table 2 for the list of control variables.

4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

Table A.7: Controlling for group-specific effects of socioeconomic development

	(1) Siblings	(2) Any sibling	(3) Height (sd)	(4) HS completion	(5) Land	(6) Assets	(7) Employed	(8) Occu. score	(9) Urban *hukou*	(10) Income
Penalty	-0.216*	-0.111**	0.244**	-0.030	2.104**	3.816	0.125*	1.106	0.046	1.844
	(0.114)	(0.047)	(0.116)	(0.051)	(1.005)	(5.527)	(0.065)	(2.581)	(0.039)	(1.912)
Low-skill × Penalty	-0.039	-0.027	-0.042	0.076**	-1.561***	15.155***	-0.043	2.314*	-0.013	1.187
	(0.061)	(0.030)	(0.061)	(0.036)	(0.563)	(4.038)	(0.042)	(1.388)	(0.022)	(1.070)
High-skill × Penalty	0.067	0.023	-0.011	0.153***	-1.326	-10.240	-0.016	5.698*	0.121*	4.214*
	(0.128)	(0.055)	(0.133)	(0.052)	(1.073)	(13.380)	(0.079)	(2.973)	(0.069)	(2.490)
R^2	0.542	0.592	0.386	0.442	0.381	0.595	0.348	0.437	0.422	0.501
Mean dep var	1.471	0.771	0.015	0.245	4.663	48.767	0.618	39.941	0.174	15.675
Observations	2894	2894	2807	2894	2851	2763	2845	1785	2894	2724

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. Each column represents a separate regression. Robust standard errors in parentheses are clustered at the province-cohort level.
2. Penalty is the fine a couple expects to pay for the second child. Low-skill (resp. High-skill) is a dummy indicator that takes value one if the respondent's father was employed in an low-skill (high-skill) nonfarm occupation when the respondent was aged 12, and zero if otherwise. I consider an occupation as high-skill if more than 50% of all fathers employed in this occupation have attended high school. Middle school (high school) is dummy indicator that takes value one if the respondent's father finished 9 (12) years of schooling.
3. See notes below Table 2 for the list of control variables.
4. The analysis is based on a sample of firstborn children holding rural *hukou* and born between 1966 and 1990 from the CFPS data.

Table A.8: Examples of constructing the measure of the second-child penalty: gender

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.						
Province	Liaoning	Hubei	Liaoning	Hubei	Liaoning	Hubei
Year firstborn girl eligible	1985	1991	1985	1991	1985	1991
Birth year	1971	1971	1979	1979	1990	1990
Fine age 1	0	0	1.21	1.21	**1.21**	**2.83**
Fine age 2	0	0	1.21	1.21	**5**	**2.83**
Fine age 3	0	0	1.21	1.21	**5**	**2.83**
Fine age 4	0	0	1.21	1.21	**5**	**2.83**
Fine age 5	0	0	1.21	1.21	**5**	**2.83**
Fine age 6	0	0	**1.21**	1.21	**5**	**2.83**
Fine age 7	0	0	**1.21**	1.21	**5**	**2.83**
Fine age 8	1.21	1.21	**1.21**	0.94	**5**	**2.83**
Fine age 9	1.21	1.21	**1.21**	0.94	**5**	**2.83**
Fine age 10	1.21	1.21	**1.21**	0.94	**5**	**2.83**
Panel B. Second-child penalty by gender of the firstborn						
Girl	0.36	0.36	0.61	1.13	0.00	0.00
Boy	0.36	0.36	1.21	1.13	4.62	2.83

Note: Panel A: Year firstborn girl eligible is the year since which a couple with only a daughter is eligible to have a second child (Scharping, 2013). Fine age s is the level of fine imposed on unauthorized second births when the firstchild was aged s using data from Ebenstein (2010). Cells in bold indicate that at these ages of the first child, couples with only a daughter can have a second child without paying the fine whereas couples with a son have to pay the fine. Panel B shows the second-child penalty constructed using equation 1 for boys and girls.

Table A.9: Examples of constructing the measure of the second-child penalty: ethnicity

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.						
Province	Liaoning	Hubei	Liaoning	Hubei	Liaoning	Hubei
Year minority eligible	1988	2001	1988	2001	1988	2001
Birth year	1971	1971	1979	1979	1990	1990
Fine age 1	0	0	1.21	1.21	**1.21**	2.83
Fine age 2	0	0	1.21	1.21	**5**	2.83
Fine age 3	0	0	1.21	1.21	**5**	2.83
Fine age 4	0	0	1.21	1.21	**5**	2.83
Fine age 5	0	0	1.21	1.21	**5**	2.83
Fine age 6	0	0	1.21	1.21	**5**	2.83
Fine age 7	0	0	1.21	1.21	**5**	2.83
Fine age 8	1.21	1.21	1.21	0.94	**5**	2.83
Fine age 9	1.21	1.21	**1.21**	0.94	**5**	2.83
Fine age 10	1.21	1.21	**1.21**	0.94	**5**	2.83
Panel B. Second-child penalty by ethnicity of the firstborn boy						
Minority boy	0.36	0.36	0.97	1.13	0.00	2.83
Non-minority boy	0.36	0.36	1.21	1.13	4.62	2.83

Note: Panel A: Year minority eligible is the year since when a couple belonging to minority groups with less than 10 million population is eligible to have a second child (Scharping, 2013). Fine age s is the level of fine imposed on unauthorized second births when the firstchild was aged s using data from Ebenstein (2010). Cells in bold indicate that at these ages of the first child, minority couples can have a second child without paying the fine whereas other couples have to pay the fine. Panel B shows the second-child penalty constructed using equation 1 for minority boys and non-minority boys.