Heterogeneity in the Multidimensional Child Quality-Quantity Trade-off and Its Consequences for Intergenerational Mobility

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Abstract

This paper studies the child quality-quantity trade-off by analyzing how family size and child outcomes change in response to a change in the cost of child quantity. I exploit the variation in the monetary penalty for an unauthorized second child under the One-Child Policy in rural China. I find evidence of a trade-off between family size and the health of the firstborn but no evidence for a trade-off between family size and the education or wealth of the firstborn. Further exploring the heterogeneity across parents, I find that the trade-off between family size and education exists only for high-skill workers' children, while the trade-off between family size and wealth exists only for farmers' or low-skill workers' children. Evidence suggests that the heterogeneity arises because of different expected returns to education and different opportunity costs of education when the parents work in different occupations. The results underline the importance of multidimensional child quality and heterogeneity across parental occupations. The heterogeneity in the quality-quantity trade-off contributes to the decline of intergenerational income mobility in China. While all parents are having fewer children, only children of high-skill workers become more likely to find a better job, migrate to cities, and earn a higher income when having fewer siblings.

Keywords: Human capital, Wealth, Fertility, China, One-Child Policy, Intergenerational income mobility.

JEL Codes: J13, I14, I24, J62, J24, O15

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1 Introduction

Family planning policies have been widely implemented in many countries to reduce fertility and control population growth (de Silva and Tenreyro, 2017). The policies are believed to be human capital enhancing through the trade-off between child quality and quantity (Q-Q trade-off hereafter) (Becker and Lewis, 1973). Family planning policies increase the *price* of child quantity. As a result, parents have fewer children but the average quality of the children increases. Rosenzweig and Wolpin (1980) propose a way to test this theory by estimating the effect of an exogenous reduction in child quantity on the outcomes of the children, but empirical studies have not found clear-cut support for the theory. Some studies find that a reduction in child quantity improves child quality (Rosenzweig and Wolpin, 1980; Li et al., 2008; Rosenzweig and Zhang, 2009; Liu, 2014), whereas others find no effect (Black et al., 2005; Angrist et al., 2010) or even the opposite effect (Qian, 2009).[1]

One possible reason behind the mixed evidence is that most of the studies look at a single dimension of child quality and ignore the heterogeneity of the parents. Child quality is multidimensional. The health, cognitive skills, education, wealth, and labor market performance of the children are all part of the quality of the children. When the *price* of child quantity changes, each dimension of child quality may respond in a distinct way depending on the *price* of this quality dimension. In the meanwhile, parents may have different perceptions about the *price* of each quality dimension because they are heterogeneous in terms of their productivity of investing in a quality dimension and how much they value this quality dimension. Thus, estimating the Q-Q trade-off for a single quality dimension for an average parent has limited bearing on the existence or absence of a child Q-Q trade-off.

This paper investigates the heterogeneity in the trade-off between child quantity and different dimensions of child quality. I use the monetary penalty for an unauthorized second child imposed by China's One-Child Policy (OCP) to measure the change in the *price* of an additional child. I then check how family size and the health, education, and wealth of the children change in response to a change in the *price* of child quantity. Although several studies use OCP to test the Q-Q trade-off, most of them use only education as the measure of child quality (e.g. Qian, 2009; Li and Zhang, 2017; Guo et al., 2021).[2] Liu (2014) to my knowledge is the only study

[1] In this chapter, I use child quantity, family size, and fertility interchangeably to express the number of children a couple has.

[2] In addition to the effects of OCP on child quantity and quality, studies have also looked at the effect of OCP on other outcomes, such as children's gender composition (Ebenstein, 2010; Li et al., 2011; García, 2022),

that examines both the health and education of the children. This paper is among the first to look at different dimensions of child quality, and to consider heterogeneity in responses across parents in different occupations.

For each dimension of child quality, I allow the trade-off between child quantity and this quality dimension to be different across parental occupations.[3] Parental occupations not only affect the resources available but also their productivity of investing in and their valuation of each quality dimension. Teachers may be more productive in raising the academic performance of their children. Farms may value more the health and strength of the children. As a result, the occupations of the parents affect the perceived *price* of different dimensions, leading to heterogeneous responses to a change in the *price* of child quantity. Moreover, parental occupations reflect the human capital, income, and social status of the parents. Quantifying the heterogeneous responses across parental occupations would help us understand how family planning policies contribute to decreasing intergenerational income mobility in China, which has raised concerns among economists and policymakers (Fan et al., 2021).

I test the child quality-quantity trade-off by examining how the family size and the quality of the firstborn change in response to a change in the *price* of a second child.[4] I exploit variation in the monetary penalty parents face if they have an unauthorized second child in the ten-year window after the firstborn (hereafter second-child penalty). Although the OCP set a one-child rule for almost every couple in urban China, some couples in rural China are exempted from the one-child rule if they are eligible for a second-child permit (Scharping, 2013). Eligibility criteria for second-child permits are based on characteristics of the couple, such as the gender of the firstborn and ethnicity. Couples who have a second child without the second-child permit have to pay a fine set by the provincial government, which could be as high as several years of household income (Ebenstein, 2010). Both the eligibility criteria and the fine rates vary at the

marriage market dynamics (Huang and Zhou, 2016), and parental migration (Huang et al., 2020). Sibling gender composition is likely to be an alternative channel to child quantity through which OCP affects child quality. In Section 4.4, I provide evidence that the effect of OCP on child quality is not driven by changes in sibling gender composition.

[3]Few studies have looked at the heterogeneity in the Q-Q trade-off across parental occupations. Studies using OCP for identification find that the effect of child quantity on the education of the firstborn varies by urban-rural residential status (Li and Zhang, 2017) and by parental preferences for larger families (Guo et al., 2021).

[4]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 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) and does not require the exclusion restriction. A reduced-form analysis also helps us understand how stricter family planning contributes to intergenerational mobility, which is more policy-relevant.

province-year level, which creates variation in the second-child penalty across couples subject to different eligibility criteria, living in different provinces, and giving birth to the first child in different years. I use this variation to identify the causal effect of the second-child penalty in a triple-difference framework.

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. The CFPS asked the respondents to recall the parents' main occupation during childhood, a period when most of the parental investment decisions are made. The rich data allow me to investigate how the interplay between parental occupation and the second-child penalty affects the long-term outcomes of the firstborn. Because the father is usually the breadwinner and decision-maker in Chinese households, I focus on the father's occupation and distinguish three types of occupations: farmers, low-skill workers, and high-skill workers.

I start by showing the effect of the second-child penalty on family size, health, education, and wealth of all firstborn children while ignoring the heterogeneity across parents. All the outcomes were measured in adulthood. When looking at the wealth of the children, I check how the effect differs on the value of land and other household assets such as housing properties and savings. I find that the second-child penalty leads to a reduction in child quantity. However, the penalty does not have the same effect on all dimensions of child quality. A higher penalty leads to on average better self-reported health and higher stature of the firstborn children but has no significant effect on the education and wealth of the children. These results suggest that there is a trade-off between child quantity and the health dimension of child quality but no trade-off for other quality dimensions.

I then estimate the heterogeneous effect of the second-child penalty across parental occupations. The effect of the penalty on family size and health outcomes does not vary by parental occupation, suggesting that the trade-off for health is not heterogeneous across parental occupations. The same does not hold for the education and wealth of the children. For different types of parental occupations, the trade-off exists for different quality dimensions. Only for children whose father is a high-skill worker, a higher penalty leads to better schooling outcomes, suggesting a trade-off between child quantity and the education dimension. The effect of the penalty on education is also significantly smaller for farmers' children than for low-skill workers' children. The second-child penalty increases the assets, especially housing properties, for children of low-skill workers but not for other children, suggesting that for low-skill parents, there is a trade-off between child quantity and the wealth dimension of child quality. Finally, only for farmers, I detect an increase in the land ownership of the firstborn when the second-child penalty increases.

The heterogeneous Q-Q trade-off has consequences for labor market outcomes of the children and intergenerational income mobility. A higher second-child penalty increases the probability of being employed for all groups of children. Conditional on employment, however, the second-child penalty only improves the occupational status and migration probability of a child when the father is a high-skill worker. The income of high-skill workers' children also increases significantly more than the income of other children in response to a higher second-child penalty. Finally, estimating quantile regressions for personal income, I find that the income distribution becomes more dispersed within each occupational group as a result of a higher second-child penalty. These findings suggest that the second-child penalty imposed by OCP not only reduces intergenerational mobility but also amplifies income inequality.

The last part of the paper discusses the potential reasons behind the heterogeneous responses across parental occupations. I find that the heterogeneous *price* of the education dimension of child quality plays a key role. The returns to education are lower if the father is a farmer or in a low-skill occupation, which explains why children of farmers and low-skill workers own more land or assets but do not attain more education when they have fewer siblings. The opportunity cost of educating the first child is higher when there is no other child to stay with the parents and work on the household farm, which explains the effect on education is the smallest for farmers' children. I also show that credit constraints are important but insufficient to explain the heterogeneous effects on education and wealth.

The results help us reconcile with the mixed empirical findings in the Q-Q trade-off literature.[5] Even among studies using the OCP as the exogenous shock to child quantity, estimates of the Q-Q trade-off fall into a wide range. For example, Liu (2014) shows that the Q-Q trade-off exists only for health and not for education in rural China. This finding is consistent with mine if I ignore the heterogeneity across parents. Li and Zhang (2017) estimate that the trade-off for education is stronger among urban households than among rural households, which can be explained by a larger share of fathers in high-skill occupations in urban China. An important finding of this chapter is that stricter OCP enforcement has a smaller and even negative effect on the high school completion of farmers' children. Qian (2009) finds that OCP increases the school

[5]See Doepke (2015) and Clarke (2018) for surveys of studies estimating the Q-Q trade-off in various settings.

enrollment of the firstborn in rural China, which can be explained by an over-representation of farmers and a focus on the early 1980s when nonfarm employment was rare. Guo et al. (2021) show that the effect of OCP on the education of the firstborn could be positive if the change in family size is desired by the parents. The desire to keep more children to farm the land could result in a stronger desire for larger families among farmers. My findings, hence, are also in line with the findings of Qian (2009) and Guo et al. (2021).

The results also provide insights on factors that explain the human capital investment gaps across parental backgrounds. The literature has documented the importance of credit constraints and parental beliefs about the returns to investments for understanding the investment gaps across parents in developed countries (Caucutt et al., 2017; Lee and Seshadri, 2019; Caucutt and Lochner, 2020; Boneva and Rauh, 2018; Dizon-Ross, 2019; Attanasio et al., 2020). In developing countries, studies show that land rights and customary norms could prevent parents from investing efficiently in their children's education (Jensen and Miller, 2017; La Ferrara and Milazzo, 2017; Bau, 2021). This paper provides a new perspective to study these factors together by exploiting the differences across parental occupations in response to an exogenous change in the *price* of child quantity. The results suggest that the difference in expected returns and costs of educational investments across socioeconomic groups plays a key role in explaining the difference in educational investments in rural China.

Finally, the results have important policy implications for understanding the strong intergenerational associations in human capital, wealth, and income. Several studies investigate the intergenerational associations using data from developed countries (e.g. Chetty et al., 2014; Adermon et al., 2021; Fagereng et al., 2021). A few recent studies have attempted to understand the increase in intergenerational income persistence in China (Alesina et al., 2020; Fan et al., 2021; Jia et al., 2021; Yu et al., 2021). My study complements the work by Yu et al. (2021), which shows that the OCP contributes to the intergenerational transmission of urban-rural inequality. They argue that urban residents, whose fertility is more constrained by the OCP, have fewer children and invest more in their human capital due to the trade-off between child quantity and quality. As a result, the OCP increases the transmission of the urban-rural disparity across generations. My results stress that the implementation of OCP decreased intergenerational mobility even among the rural population. Moreover, the effect is not driven by different OCP enforcement but the heterogeneity in the Q-Q trade-off across socioeconomic groups.

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 document the existence of a multidimensional Q-Q trade-off that is heterogeneous across parental occupations. Section 5 discusses the reasons behind the heterogeneous responses and Section 6 concludes with policy implications.

2 Context

2.1 One-Child Policy

The One-Child Policy (OCP) was introduced in 1979, under which each couple is allowed to have only one child.[6] Provinces implemented the one-child rule by issuing second-child permits and imposing fines for the unauthorized birth of a second child (Ebenstein, 2010).[7] The fines levied on unauthorized births changed over time. Figures 1a plot the fine rates measured in years of household income in 1985 and 1991 by provinces.[8] 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). These changes create variation in policy exposure for parents who had their first child in different places and at different points in time.

The one-child rule was universal when it was first implemented but was gradually relaxed in 1982. Provincial governments set conditions under which couples were exempted from the one-child rule and could apply for a second-child permit (Scharping, 2013).[9] Some provinces

[6]China initiated the first family planning campaign in some provinces in 1954 and the second urban-oriented family planning campaign in 1962. However, these campaigns were short-lived and had limited influence (Scharping, 2013, p.46-49). 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.

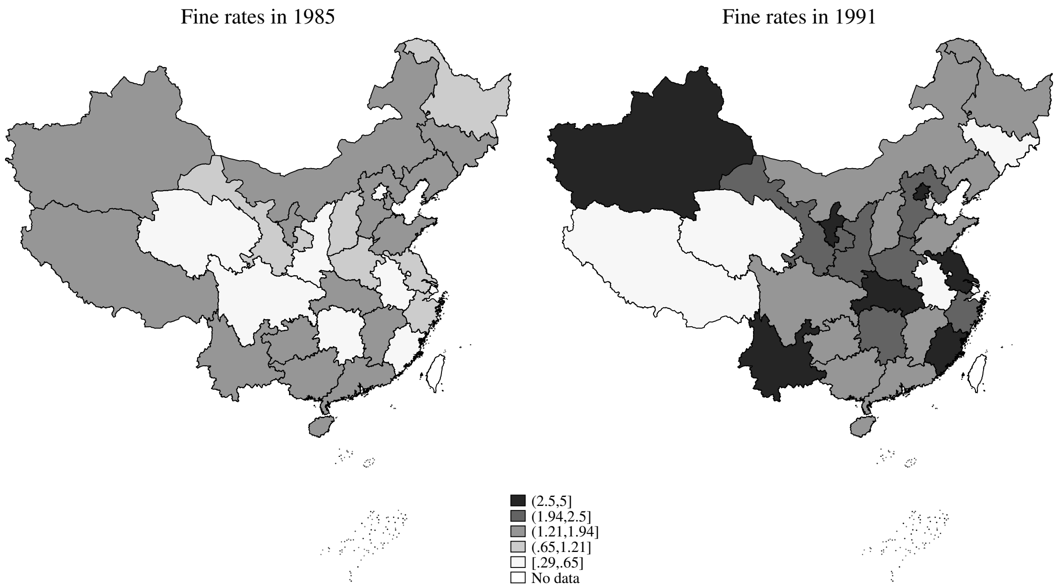
[7]Couples sometimes were also subject to nonmonetary penalties such as losing access to state-provided education, health, and employment. Because it is difficult to measure these penalties, I do not consider them for identification. Rather than identifying the effect of OCP enforcement, this paper identifies the effect of the monetory panelty due to having a second child without a second child permit.

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

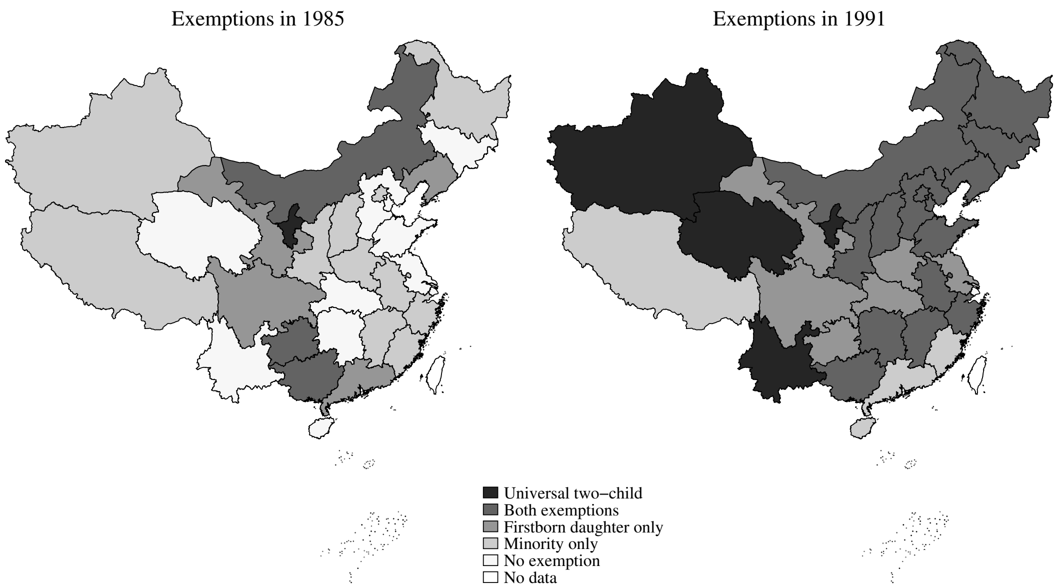
[9]There were a few exemption rules applied nationwide since 1979 (Scharping, 2013). A second child was allowed if the first birth was dead or disabled, the first child was adopted, or the couple remarried. Exemptions also existed for some occupations, such as fishermen, mine workers, and military veterans. Couples with one

Figure 1: Sources of variation in treatment exposure

(a)



(b)



Note: Figure (a) plots the fine rates on unauthorized births in years of 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).

introduced exemptions to couples in rural areas whose firstborn was a girl. In some provinces, exemptions were also granted to ethnic minority groups with a population of less than 10 million people. In a few provinces, exemptions were granted to all married couples at some point in time. In all years between 1979 and 2000, there were very few provinces granted exemptions to urban couples. Because of the limited variation in policy exposure and the significant difference in socioeconomic development between urban and rural areas, I lay my focus on rural couples and children throughout the analysis.

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 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 monetory panelty due to having a second child without a second child permit.

2.2 The *Hukou* system and the land tenure system in China

At present, each person in China has an official record called *hukou* (household registration) that includes the date of birth, place of birth, place of origin (father's or grandfather's place of birth), ethnic identity, and present place of residence. The *hukou* system was initiated in 1958 to divide the populace into people with rural (agricultural) *hukou* and people with urban (non-agricultural) *hukou*. In the beginning, the system was not only serving as a migration control mechanism but also used by the central government for organizing labor and resources for the pursuit of the Great Leap Forward in industrialization. State welfare programs, which were tied to *hukou* status, heavily favored urban residents. Urban *hukou* holders had access to state-guaranteed food supply and state-provided education, health, and employment. Rural *hukou* holders, on the other hand, received land from the government to cultivate, but could not sell or sublease the land (Ngai et al., 2018).

Initially, the *hukou* system was strictly enforced to restrict industrial or geographical labor

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.

mobility. All urban *hukou* holders lived in urban areas and worked in industry or services, while rural *hukou* holders lived on farms and worked in the agricultural sector. Since the early 1980s, rural *hukou* holders have been allowed to develop and work in nonfarm enterprises in the countryside. After 1985, the migration restrictions were also gradually relaxed. In some places, rural residents were also allowed to work in cities and receive urban services and welfare without holding 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, 1999a). There were also a few limited ways of getting around the restrictions on migration to urban areas in which formal education beyond middle school is almost necessary (Zhao, 1999b). Among the most popular was attending high school and then college. Graduation from ordinary specialized high school (*zhong zhuan*) was another way, after which students would be assigned a job in the urban sector with an urban *hukou*. Joining the army was also a popular way to getting an urban *hukou*, and the army placed a significant emphasis on educational achievement.

Under the *hukou* system and the land tenure system in rural China, rural households have the right to use the land which has been allocated to them and transfer the land use rights to their children. However, they do not have the right to sell or sublease it in a land market. The land is collectively owned by the village, and village officials can reallocate the land to other households within the same village if the land is not properly used. The practices of land reallocations lead to over-employment in the agricultural sector and a low rate of migration (Ngai et al., 2018; Zhao, 2020). To secure the land use rights, parents might also want to keep at least one child to work in the agricultural sector. Because of the crucial role of education in promoting nonfarm employment and permanent migration to urban areas, land tenure insecurity could create a disincentive for farmers to educate their children. In Section 5, I discuss how land tenure insecurity might play a role in the Q-Q trade-off for education.

3 Data and empirical strategy

3.1 Data and sample

The main analysis uses data provided by the China Family Panel Studies (CFPS), which was launched in 2010 by the Institute of Social Science Survey of Peking University, China (Xie, 2012). The CFPS is a longitudinal study of a nationally representative sample of Chinese communities, families, and individuals. I restrict the sample to couples with the first child born between 1966 and 1990 and holding a rural *hukou* at age three. These children were between 20 and 44 years old and mostly finished schooling and entered the labor market in 2010.[10] Hence, I can look at their schooling and labor market outcomes using data from the 2010 CFPS survey. 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 because the divorce rate and childless rate were low during the period of study.

CFPS recorded the father's primary occupation when the child was aged 12. I classified the father's primary occupation into three types: farmers, low-skill workers, and high-skill workers. 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 low-skill workers (e.g. salespersons and manufacturing workers), and 6.3% were high-skill workers (e.g. doctors, accountants, and teachers). In Section 5, I discuss how the heterogeneity across parental occupations sheds light on the reasons why different dimensions of child quality respond differently to the second-child penalty.[11]

Table 1 Panel A shows summary statistics of individual characteristics. I observe 2,895 firstborn children, among which 1,310 are boys and 1,585 are girls.[12] I also show the summary statistics for the male and female samples separately and the p-value for gender differences in observed characteristics. Note that although it is well-known that parents conduct sex selection under the OCP (Ebenstein, 2010; Li et al., 2011; García, 2022), it is not prevalent among firstborn children. The sample is balanced by gender in terms of observed characteristics.[13]

[10] The results remain similar if I drop the youngest firstborn children who were not yet 22 years old in 2010, the age when most people complete college education.

[11] My classification of occupations is based on the education-intensity of the occupations. In Section 4.4, I show that my results on the heterogeneity in the Q-Q trade-off are not driven directly by the educational levels of the parents.

[12] Due to the sampling design and non-responses, CFPS over-sampled girls (Xie and Lu, 2015). Using the CFPS sampling weights could adjust for the gender imbalance in the sample. In the presence of potential unmodeled heterogeneous effects, it would be ideal if applying the weights could help identify the population average treatment effect. However, Solon et al. (2015) show that using sampling weights does not necessarily identify the population average treatment effect when estimating causal effects. Both weighted and unweighted estimators identify different weighted averages of the heterogeneous effects, and neither one identifies the population average effect. Hence, I show in the main analysis the unweighted estimates and in Table A.2 in the Appendix the weighted estimates. For most outcome variables, the sign and magnitude of the weighted estimates are similar to those of the unweighted estimates.

[13] In Section 4.4, I show that gender selection among higher-order births, if any, does not challenge the inter-

Table 1: Summary statistics

	Mean			Gender difference
	All	Boy	Girl	p-value
Panel A. Individual charateristics				
Boy (0/1)	0.503	1.000	0.000	
Minority (0/1)	0.089	0.087	0.090	
Age (years)	31.663	31.749	31.576	0.601
Father's age (years)	57.256	57.368	57.141	0.565
Mother's age (years)	55.031	55.147	54.912	0.528
Mother's age at birth (years)	23.482	23.539	23.425	0.548
Father middle school (0/1)	0.374	0.367	0.381	0.497
Father high school (0/1)	0.119	0.124	0.114	0.964
Mother middle school (0/1)	0.192	0.191	0.192	0.328
Mother high school (0/1)	0.042	0.037	0.046	0.528
Father farmers (0/1)	0.660	0.666	0.654	0.609
Father low-skill occupation (0/1)	0.277	0.276	0.278	0.917
Father high-skill occupation (0/1)	0.063	0.058	0.068	0.412
Panel B. Outcome variables				
Number of siblings	1.421	1.220	1.624	
Any sibling (0/1)	0.762	0.677	0.847	
Good health (0/1)	0.605	0.636	0.575	
Height (cm)	164.819	170.218	159.291	
Years of schooling	8.798	9.224	8.367	
High school completion (0/1)	0.269	0.289	0.250	
Cognitive test score (sd)	0.096	0.193	-0.003	
Assets (1,000 *yuan*)	44.009	43.811	44.212	
Housing (1,000 *yuan*)	37.846	37.572	38.129	
Investment (1,000 *yuan*)	6.163	6.239	6.084	
Land (1,000 *yuan*)	4.341	4.880	3.789	
Employed (0/1)	0.622	0.727	0.518	
Treiman scale (0–100)[a]	40.101	40.074	40.141	
High-skill occupation (0/1)[a]	0.122	0.099	0.154	
Urban *hukou* (0/1)	0.175	0.172	0.177	
Annual personal income (1,000 *yuan*)	16.697	21.955	11.133	
Panel C. Treatment variable				
Second-child penalty	0.735	0.982	0.485	
Observations	2895	1310	1585	

Note: 1. Weighted by CFPS sample weights.
2. The sample consists of firstborn children holding rural *hukou* at age 3 and born between 1966 and 1990 from the CFPS data.
3. See Appendix A.1 for details on how the variables are contructed from the CFPS data.
4. Income is adjusted to 2010 prices using the Consumer Price Index.
[a] conditional on being employed.

The primary outcomes of interest are the number of siblings, only child status, height, self-reported health, educational attainment, cognitive test scores, land value per capita, non-land household assets per capita, employment, Treiman scale and skill-intensity of the occupation, urban *hukou* status, and income, all measured in 2010. The Treiman scale is an internationally standardized measure of occupational status developed by Treiman (1977). Because farmland in rural China is collectively owned and cannot be sold in a market, CFPS has computed the value of land using the size and agricultural output of the land in the year before the survey. In addition, following Fan et al. (2021), I average the annual personal income from the 2010, 2012, and 2014 waves of the CFPS to obtain a proxy for lifetime personal income. Income is adjusted to 2010 prices using the Consumer Price Index. Table 1 Panel B shows the mean of the outcome variables for the whole sample and by gender. In Appendix A.1, I provide more details on these variables and how they are constructed from CFPS.

3.2 Measure of the second-child penalty

To identify the quantity-quality trade-off, I calculate the second-child penalty imposed by OCP, which reflects an exogeneous change in the *price* of child quantity. The penalty depends on whether the couple is eligible to apply for a second-child permit and the level of fines imposed on unauthorized births. Because both eligibility and the fine rates changed over time due to local policy changes, the second-child penalty varied across localities, by the birth year of the first child, and by individual characteristics that determine the eligibility for a second-child permit.[14]

Table 2 illustrates how I construct the second-child penalty, exploiting the introduction of the exemptions for couples with only a daughter. Table A.1 in the Appendix provides examples exploiting the exemptions for minority couples. I only consider the ten-year window after the first child's birth because more than 98% of the couples with two or more children in the sample had their second child in this window. Also, the data shows that more than 99% of mothers gave birth to the first child by age 39, leaving at least ten years to have the second birth.

A comparison of columns (1), (3), and (5) in Table 2 indicates how the second-child penalty differs by birth year and gender of the first child in the Liaoning province, where exemptions

pretation of my findings as a trade-off between child quality and quantity.

[14]My measure of the second-child penalty shares a similar spirit to the one used in Guo et al. (2021). They average the fine rates in the ten years after the second child's birth to estimate the effect of a third-child penalty on the first child. They do not consider variations in eligibility because all births after the second one were not eligible for any exemptions. Also, the measure is consistent with the interpretation of OCP as a pricing system for children in García (2022). My measure reflects the average "price" of a second-child permit in the ten years following the first child's birth.

Table 2: Examples of constructing the measure of the second-child penalty

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.						
Province	Liaoning	Hubei	Liaoning	Hubei	Liaoning	Hubei
Year frstborn 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). Gray cells 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.

have been granted to couples with only a daughter since 1985. A couple who had their first child born in 1971 expects to pay a fine equal to 1.21 years of household income only if they had the second child when the first one was between ages 8 and 10. If the couple instead had their first daughter in 1979, they had to pay a fine equal to 1.21 years of household income before the child turned age six, but were exempted afterward. If the first child was instead a boy, the couple would not be exempted. This exemption rule generates variation by gender of the first child in Liaoning. Finally, if the first child was born in 1990, the couple pays no fine for a second child if the first one is a girl, but pays a fine equal to 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 2 show how the second-child penalty differs in the Hubei province. The exemption was introduced in 1991. Hence, the second-child penalty for couples who gave birth to their first child before 1980 is the same for both genders but varies across birth years of the firstborn due to the changes in the level of fine over time. 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, whereas 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.

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

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

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.[15] $Permit_{t+s,g,p}$ is a dummy variable that takes value one if the parents who had the first child in year t belonging to group g are eligible for a second child in province p in year $t+s$. $Fine_{t+s,p}$ is the level of fine imposed on unauthorized births born in year $t+s$ in province p. The intuition is that if a couple had the first child in year t and the second child in $t+s$, the couple pay $Fine_{t+s,p}$ if not exempted from the one-child rule ($Permit_{t+s,g,p} = 0$), and pay nothing if exempted ($Permit_{t+s,g,p} = 1$). I

[15]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 and Zhou (2016) 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.

then take the average over the ten years since the birth of the first child to obtain a measure of the second-child penalty imposed by OCP. Table 1 Panel C shows that the mean of the penalty is 0.74 for the whole sample. The average penalty when the firstborn is a boy is twice the average penalty when the firstborn is a girl.

3.3 Empirical strategy

To identify the causal effect of the second-child penalty, I implement a triple-difference strategy. The triple-difference strategy assumes the differences across the four groups defined by gender and minority status to follow the same trends across provinces if the four groups were treated similarly by OCP. I use group-cohort fixed effects to capture the changes in the differences between any two groups that are common to all provinces. Also, I control for province-cohort fixed effects to capture the differential 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.[16]

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 before birth interacted with gender and minority status. I also allow group-cohort fixed effects to vary with the fine rates in 1979, which would control for the group-specific effect of initial OCP strictness in the province. These controls are denoted by Z_{tgp}. In Section 4.4, I show that the results are also robust to controlling for group-specific effects of other time-variant

[16] Clustering at province-group level or two-way clustering at the province and cohort level with bootstrap methods give similar estimates of standard errors.

socioeconomic factors at the province level.

As a balancing check, I regress pre-determined characteristics of the children on the second-child penalty, controlling for the fixed effects and Z_{tgp}. As shown in Table A.3 in the Appendix, the second-child penalty is not correlated with pre-determined characteristics of the children, including the 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 by father's occupation. I consider the following equation:

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

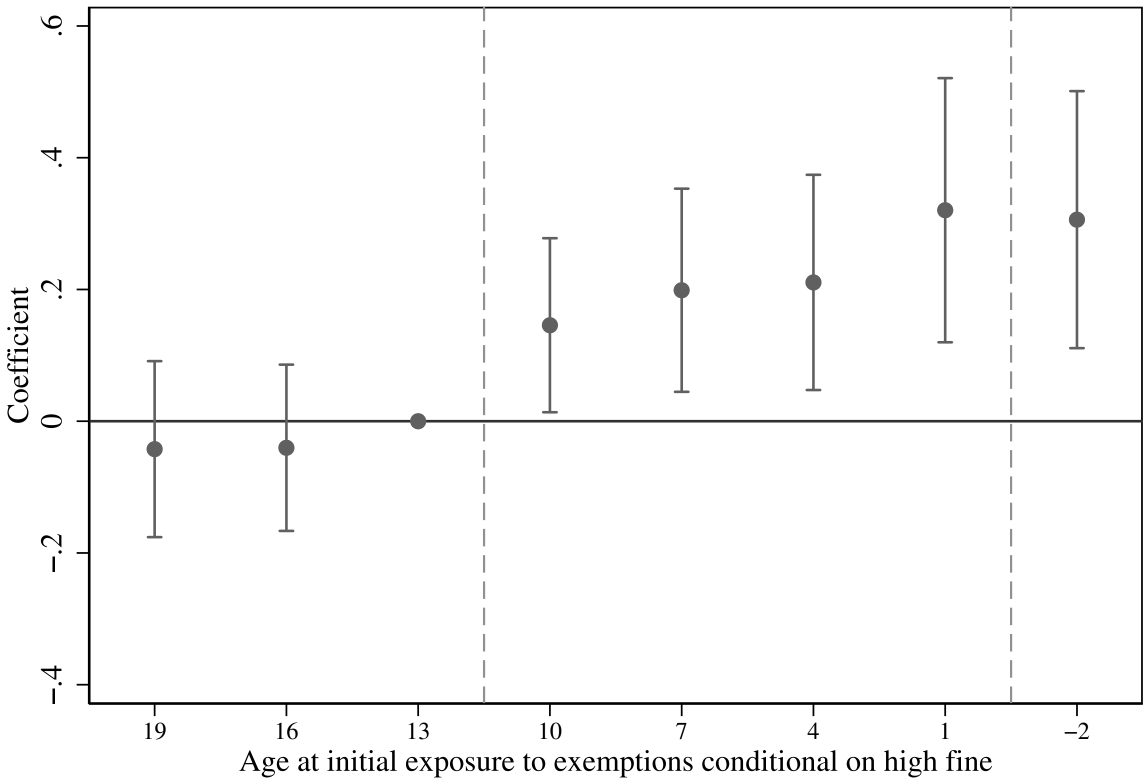
where LS_i is a dummy variable that takes value one if the father's primary occupation is low-skill, and HS_i is a dummy variable that takes value one if the father's primary occupation is high-skill. Notice that the set of individual controls X_i includes LS_i and HS_i. Under the identification assumption, 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 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 the parents becoming eligible for a second-child permit and being exempted from paying a fine greater than one year of household income. In other words, the couple faces a decrease in the *price* of the second child by at least one year of household income. Using the same sample of children as the one used in the main analysis, I compute for each child how old she or he was when the event happened. I then regress a dummy indicator of having at least one sibling on a series of event dummies. The regression is given by the following equation:

$$y_{itgp} = \sum_{j \in J} \xi_j D_j + X_i\beta + Z_{tgp} + V_{pt} + W_{gp} + U_{gt} + \epsilon_{itgp} \quad (4)$$

Figure 2: Event study estimates for the probability of having a sibling



Note: The figure shows the coefficients and 95% 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 became eligible to a second-child permit and exempted from pay a fine greater than one year of household income. The y-axis is the estimate for ξ_j. 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.

where j denotes the age group of child i and $J = \{-3, -2, 1, 4, 7, 10, 13, 16, 19, 20\}$. For j between -2 and 19, D_j is a dummy variable that takes value one if the event happened at ages j, $j+1$ or $j+2$. To reduce the collinearity between the event dummies and birth year fixed effects, I include two end points $j = -3$ and $j = 20$ as open brackets and focus only on the estimates for event dummies between these points. D_{-3} is a dummy variable that takes value one if the event happened three years before birth, and D_{20} is a dummy variable that takes value one if the event happened at or after age 20. The rest of the specification is the same as the triple-difference strategy.

Figure 2 plots the coefficients on each dummy variable D_j and the corresponding 95% confidence interval. The coefficient for age group j on the graph corresponds to ξ_j. It measures how much a couple is more likely to have a second child if the decrease in the second-child penalty happens when the first child is aged j to $j+2$ instead of 13 to 15. The figure shows that the effect on the probability of having a second child is similar if the decrease happens after the first child turns ten years old, suggesting that the decrease in second-child penalty after age 10 of the first child does not matter for the decision to have a second child. In other words, for older cohorts, there is no differential trend between groups facing a high and a low level of second-child penalty. This finding provide supports for the identification assumption of the triple-difference strategy.

The estimates of ξ_1 and ξ_{-2} in Figure 2 suggest that the decrease in the second-child penalty has a similar effect on the probability of having a second child if it happens at age 1 or before the birth of the firstborn. In other words, additional exposure to a lower second-child penalty before the first child's birth would not increase the likelihood of having a second child. The probability of having any sibling then increases almost linearly in the duration of exposure between ages 1 and 10. Hence, the event study also provides support to the measure of the second-child penalty that covers the first ten years of the firstborn child's life.[17]

4 Estimation results

This section presents the main results on how family size and different dimensions of child quality respond to a change in the second-child penalty. I estimate the average effects using equations 2 and the heterogeneous effects by father's occupation using equation 3.

4.1 Effects on sibling composition

Tabel 3 shows the effects of the second-child penalty on family size. The first and third columns show that when the penalty for a second child increases by one year of household income, the firstborn child would have 0.22 fewer siblings and be 15 percentage points less likely to have

[17]The event study using a difference-in-differences (DID) specification is presented in Figure A.1. There is an upward trend even for cohorts initially exposed to exemptions after age 10, whose probability of having a sibling is supposedly unaffected by the introduction of the OCP exemptions. These estimates suggest that if I compare children treated before age ten and after age ten in a DID specification, the estimated effect of the second-child penalty may capture a spurious increasing trend in the outcome for the treated group. Moreover, the DID estimate of ξ_{-2} is larger than the DID estimate of ξ_1. Hence, if I estimate the effect of exposure after the first child's birth in a DID specification, the estimate may also capture the effect of OCP before birth. Exposure before birth may correlate with the timing and characteristics of the firstborn child, causing biases in the DID estimate. A comparison of Figure 2 and Figure A.1 suggests that controlling for province-cohort fixed effects and group-cohort fixed effects helps deal with these issues. This finding provides additional support to the use of the triple-difference strategy.

Table 3: Effects of on the second-child penalty on family size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					First sibling arrive			
	Number of siblings		Any sibling		before age 5	after age 5	Any male sib	Any female sib
Penalty	-0.222**	-0.206*	-0.145***	-0.133***			-0.071*	-0.080
	(0.108)	(0.109)	(0.044)	(0.046)			(0.042)	(0.053)
Low-skill × Penalty		-0.042		-0.029				
		(0.061)		(0.030)				
High-skill × Penalty		0.069		0.024				
		(0.127)		(0.054)				
Penalty ages 1–5					-0.109*	0.031		
					(0.063)	(0.044)		
Penalty ages 6–10					0.021	-0.089***		
					(0.035)	(0.025)		
Penalty birth year					0.099	-0.037		
					(0.073)	(0.081)		
Penalty 1–2 years before birth					-0.153	0.174		
					(0.129)	(0.111)		
Penalty 3–4 years before birth					0.060	-0.100		
					(0.223)	(0.138)		
Penalty 5–6 years before birth					0.736	-0.215		
					(0.574)	(0.427)		
R^2	0.540	0.540	0.589	0.590	0.470	0.279	0.473	0.378
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.

5. See Table A.4 for the total effect of the second-child penalty by parental occupation.

any sibling. Columns (2) and (4) show that the estimated coefficient of the second-child penalty does not vary significantly by parental occupation.[18] Therefore, if the effect on child quality varies by the occupational type of the father, such heterogeneity is not due to different effects on child quantity.

In columns (5) and (6) of Table 3, I provide evidence that the measure of the second-child penalty precisely captures the timing of siblings. I estimate the effects of the penalty at different ages and before the first child's birth. Increasing second-child penalties between ages 1 and 5 reduces the likelihood of having a sibling before age 5 but does not affect the likelihood of having a sibling after age 5. I find the same pattern for the level of second-child penalty between ages 6 and 10. This finding supports the measure of the second-child penalty and the identification strategy. It suggests that a policy change before age five does not correlate with a change in family size happening after age five and vice versa. I also detect no significant effect of second-child penalty before birth, which is consistent with the event study.

Studies have found that OCP not only reduces fertility but also distorts the daughter-to-son ratio (Ebenstein, 2010; Li et al., 2011; García, 2022). If a change in the second-child penalty also alters the gender composition of siblings, the effects on child quality might be driven by changes in sibling gender composition and not changes in family size. The last two columns of Table 3 show that the second-child penalty has similar effects on the probability of having a sister or a brother. This finding supports the interpretation of the results as a trade-off between child quality and quantity. However, there is still a concern that the identification may fail to detect the effect on sibling gender composition because it uses between-gender variation. In Section 4.4, I show that the results are robust if I restrict the analysis to provinces that have less strong son preference. This finding further rules out that the effects of the second-child penalty on child quality are driven by gender selection among higher-order births.

4.2 Effects on human capital and household assets

Next, I show how different quality dimensions of the firstborn, including human capital and physical capital, respond to a change in the second-child penalty.

Table 4 Panels A and B show the effects of the second-child penalty on the human capital of the first child. I use self-reported health status and height to measure health. In particular,

[18] In Table A.4 in the Appendix, I show the total effect of the second-child penalty on family size as well as other outcomes discussed later in the chapter by parental occupation.

Table 4: Heterogeneous effects of the second-child penalty on human capital and assets of the children

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Health outcomes						
	Good health		Height (sd)		Height top quintile	
Penalty	0.093*	0.099*	0.199*	0.220**	0.143***	0.144***
	(0.054)	(0.056)	(0.112)	(0.109)	(0.050)	(0.052)
Low-skill × Penalty		-0.018		-0.047		-0.005
		(0.035)		(0.060)		(0.039)
High-skill × Penalty		0.046		-0.014		0.037
		(0.074)		(0.133)		(0.072)
R^2	0.270	0.270	0.383	0.383	0.302	0.303
Mean dep var	0.589	0.589	0.015	0.015	0.216	0.216
Observations	2893	2893	2807	2807	2807	2807
Panel B. Educational outcomes						
	Years of schooling		Complete high school		Cognitive score (sd)	
Penalty	0.215	0.156	0.008	-0.026	0.082	0.059
	(0.376)	(0.367)	(0.051)	(0.049)	(0.092)	(0.091)
Low-skill × Penalty		0.102		0.074**		0.047
		(0.223)		(0.036)		(0.058)
High-skill × Penalty		0.806*		0.154***		0.160
		(0.432)		(0.052)		(0.115)
R^2	0.549	0.549	0.437	0.440	0.542	0.542
Mean dep var	8.351	8.351	0.245	0.245	0.000	0.000
Observations	2894	2894	2894	2894	2893	2893
Panel C. Household wealth per capita						
			Non-land assets			
	Farmland		Total		Housing	Financial
Penalty	0.936	1.592*	8.516	2.657	1.947	0.709
	(0.871)	(0.886)	(6.444)	(6.060)	(6.051)	(2.326)
Low-skill × Penalty		-1.529***		15.547***	12.640***	2.906
		(0.555)		(4.072)	(3.745)	(1.776)
High-skill × Penalty		-1.310		-10.300	-11.618	1.318
		(1.080)		(13.177)	(13.036)	(2.410)
R^2	0.377	0.379	0.590	0.593	0.585	0.349
Mean dep var	4.663	4.663	48.767	48.767	42.776	5.991
Observations	2851	2851	2763	2763	2763	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. See notes below Table 3 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. See Table A.4 in the Appendix for the total effect of the second-child penalty by parental occupation.

height reflects the cumulation of health outcomes and nutritional status before adulthood and hence is a good measure of parental investments in health and nutrition (Thomas et al., 1996). In developing countries, where work tasks are more manual and physically demanding, height accompanied by greater physical strength is valued in the labor market (Thomas and Strauss, 1997). The odd number of columns in Panel A shows that when the second-child penalty increases by one year of household income, the firstborn child would be 9 percentage points more likely to report to be healthy, 0.2 standard deviations taller, and 14 percentage points more likely to be in the top quintile of the height distribution. Even columns show that the effects on health do not vary significantly by parental occupation. These results suggest that even the more advantaged families could not invest enough in children's health and nutrition in the absence of strict birth control. When the second-child penalty increases, parents have fewer children than they would otherwise have without OCP, and each child receives more health and nutritional investments.

Table 4 Panel B shows the results for education and cognitive outcomes. On average, there is no effect of the second-child penalty on any of these outcomes. However, the average effect masks substantial heterogeneity. When the penalty increases, children of farmers experience no improvement in education and cognitive outcomes. If anything, they become less likely to complete high school. Compared to children of farmers, children of non-farmers become more likely to finish high school, and the difference is larger between farmers and high-skill workers than the difference between farmers and low-skill workers. In Columns (6) to (8) of Table A.4 in the Appendix, I show the total effect for children in each occupation group. When the second-child penalty increases by one year of household income, children of low-skill workers become 5 percentage points more likely to finish high school, and children of high-skill workers become 13 percentage points more likely to finish high school. However, only the effect on children of high-skill workers is statistically significant. For cognitive outcomes that could reflect both parental investments and educational attainment, I find a similar pattern, although the estimates are not statistically significant. Overall, the results show that when the cost of an additional child increases, as compared to farmers and low-skill workers, high-skill workers invest significantly more in their firstborn children's education, in the means of enrolling the firstborn into high school.

In addition to investments in children's human capital, parents could also increase the children's wealth through investing in the capital market such as purchasing properties and savings.

This happens when parents expect higher returns in the market than returns to human capital investments in the children (Heckman and Mosso, 2014). Table 4 Panel C shows the effect of the second-child penalty on the assets of the child's household, which is a proxy for the wealth dimension of child quality. The first two columns show that the second-child penalty has no effect on land assets on average, but has a significant positve effect on the land assets on farmer's children. This finding reflects a trade-off between child quantity and the tranfer of land use rights to the firstborn. The next two columns show that children of low-skill workers own more non-land assets when the second-child panelty increases, while there is no change in the non-land assets of farmers' or high-skill workers' children. The last two columns show that the effect on non-land assets is mainly driven by an increase in housing properties. Although these are not direct measures of parental investments in the physical capital of the children, they could potentially reflect the amount of assets parents give to their children or share with their children.

The results above highlight the importance of multidimensional child quality and heterogeneous parental responses when testing the Q-Q trade-off. In Section 5, I discuss the potential explanations for the heterogeneous effects on different quality dimensions.

4.3 Effects on labor market outcomes

Next, I check if the different effects on human capital later translate into different effects on labor market outcomes. Table 5 shows the effect of the second-child penalty on employment and occupational outcomes. Column (1) shows that the second-child penalty increases the employment of all groups of children. Conditional on being employed, the second-child penalty increases the probability of working in a high-skill occupation more for children whose father is also a high-skill worker (see Column (2)). The second-child penalty also increases the Treiman scale more for children of nonfarmers, and the effect is stronger for children of high-skill workers (see Column (3)). The last column shows that children of high-skill workers also become more likely to hold an urban *hukou*. In other words, the second-child penalty increases their probability of permanent migration to cities that have more skilled and higher-wage labor markets. Overall, the results suggest that the second-child penalty imposed by OCP increases the transmission of occupational status from the father to the firstborn child.

Table 6 shows the effect of the second-child penalty on annual personal income. Columns (1) to (2) show the triple-difference estimates of the effect of the second-child penalty on income and log income. Log income is less sensitive to outliers but can only be constructed for positive

Table 5: Heterogeneous effects of the second-child penalty on labor market outcomes

	(1)	(2)	(3)	(4)
	Employed	High-skill	Treiman scale	Urban *hukou*
Penalty	0.112*	-0.093	-0.619	0.055
	(0.062)	(0.095)	(2.556)	(0.037)
Low-skill × Penalty	-0.043	0.049	2.432*	-0.014
	(0.042)	(0.037)	(1.379)	(0.023)
High-skill × Penalty	-0.017	0.142*	5.772**	0.121*
	(0.079)	(0.083)	(2.927)	(0.069)
R^2	0.346	0.432	0.433	0.422
Mean dep var	0.618	0.113	39.941	0.174
Observations	2845	1757	1785	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 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. See notes below Table 3 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.

income.[19] The estimates suggest that raising the second-child penalty by one year of household income increases children's annual income by 2000 *yuan* (293 USD) if the father is a farmer, by 3300 *yuan* (483 USD) if the father is a low-skill worker, and by 6300 *yuan* (922 USD) if the father is a high-skill worker, but the effect on children of farmers is insignificant. I observe similar patterns using log income as the outcome variable.

To recover an estimate of the second-child penalty's contribution to intergenerational income mobility, I computed the father's income using the occupational type of the father. I use a sample of males born between 1946 and 1965 and calculate the average income and log income by occupational types. I then replace the indicator of the father's occupation with the computed income of the father and estimate the heterogeneous effect of the second-child penalty on children's income and log income by the computed income of the father. The results are shown in Table A.5 in the Appendix. The results suggest that when the penalty increases by one year of household income, the intergenerational income correlation increases by 0.12, and the intergenerational income elasticity estimated using log income increases by 0.06. Fan et al. (2021) estimate that the intergenerational income elasticity increased by 0.049 between the 1970–1980

[19] I find that the second-child penalty does not affect the selection into positive income for all groups of children.

Table 6: Heterogeneous effects of the second-child penalty on lifetime personal income

	(1)	(2)	(3)	(4)	(5)
			Conditional percentiles		
	Income	Log(Income)	25th	50th	75th
Penalty	2.051	0.102	0.977	2.509*	3.677**
	(1.815)	(0.130)	(1.392)	(1.393)	(1.558)
Low-skill × Penalty	1.210	0.021	0.908	0.196	1.694**
	(1.072)	(0.074)	(0.980)	(0.816)	(0.761)
High-skill × Penalty	4.208*	0.131	2.857***	1.740	4.069
	(2.485)	(0.114)	(1.071)	(3.143)	(2.610)
Total effect (Low-skill)	3.261*	0.123	1.885	2.705*	5.370***
	(1.857)	(0.120)	(1.314)	(1.444)	(1.396)
Total effect (High-skill)	6.259**	0.233*	3.833**	4.249	7.746***
	(2.792)	(0.139)	(1.549)	(2.995)	(2.255)
R^2	0.500	0.507	0.037	0.038	0.039
Mean dep var	15.675	9.663			
Observations	2724	2040	2724	2724	2724

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

1. Each column represents a separate regression. Estimates in Columns (1) to (2) are obtained from OLS. Estimates in Column (3) to (5) are obtained from quantile regressions for residuals from a regression of income on the full set of province-group, province-cohort, and group-cohort fixed effects. 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.
3. See notes below Table 3 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. Income is adjusted to 2010 prices using the Consumer Price Index.

birth cohort and 1981–1988 cohort in rural China. In the sample, average second-child penalty increases by 0.3 years of household income between the two cohorts. Hence, the results suggest that the second-child penalty imposed by OCP increases intergenerational income elasticity by 0.018 in rural China, accounting for 37% of the total increase of 0.049.

I also check whether the implementation of the second-child penalty changes the distribution of personal income and whether such change differs by parental occupation using quantile regressions. I estimate quantile triple differences for residuals from regressing income on the full set of province-group, province-cohort, and group-cohort fixed effects. The key assumption here is that fixed effects are viewed as location shift variables that affect all quantiles in the same way (Canay, 2011). Columns (3) to (5) of Table 6 show the results. Raising the second-child penalty shifts the income distributions right for all groups of children, but the shifts are larger for children of nonfarmers and the largest for children of high-skill workers. Moreover, the second-child penalty increases the dispersion of income within each group of children. The positive effect of the penalty is the smallest on the bottom quartile of the income distribution and the largest on the upper quartile. The results suggest that the second-child penalty decreases intergenerational income mobility and amplifies the income inequality both within and across groups of children.

4.4 Robustness

Famliy size or sibling gender composition

So far, I interpret the effect of the second-child penalty as evidence of a trade-off between child quality and quantity. In this section, I provide additional evidence that the effect on child quality is not driven by changes in sibling gender composition, by focusing on provinces without strong son preference.

I calculate the sex ratio among children aged 6–10 from the 1982 Chinese Census. These children were born from 1972 to 1976 before OCP was implemented. I define provinces that had a sex ratio above 1.07 as having strong son preference before OCP and exclude them from the analysis.[20] I then reestimate the effects of the second-child penalty on family size, sibling gender composition, as well as several quality measures using only provinces without strong son preference. The results are shown in Table A.6 in the Appendix. The estimated effects of the second-child penalty on family size are similar to those obtained with the full sample.

[20] The excluded provinces include Anhui, Gansu, Hebei, Jiangsu, Jiangxi, Shaanxi, Shanxi, Tianjin, and Zhejiang.

The penalty also has similar effects on reducing the likelihood of having a male sibling or a female sibling. Moreover, the estimated effects of the penalty on child quality and labor market outcomes do not change much using the restricted sample. Therefore, I conclude that the results can be interpreted as a trade-off between child quantity and quality, rather than a trade-off between child quantity and gender composition.

Socioeconomic development

The identification relies on the assumption that the between-group differences in the outcomes 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 secondary school teachers per capita, and number of health institutes per capita. These variables would be able to 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 Panel A of Table A.7 in the Appendix for a selected set of outcomes. The triple-difference estimates are similar to the estimates obtained without controlling for the group-specific effect of socioeconomic development.

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 parental occupation is actually driven by parental education, I add the interaction of paternal education with the second-child penalty and reestimate equation 3. Panel B of Table A.7 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.[21] The results suggest that using different measures of

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

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 parental occupations, not different parental education. In Section 5, I discuss some implications of this finding for understanding factors underlying the heterogeneous parental responses.

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 exemptions to couples with only one daughter and minority couples. As pointed out by Zhang (2017), the level of fine may correlate with local financial situations and local fertility demand. The province-cohort fixed effects could partly address this issue. However, if changes in local financial situations and local 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

$$Ineligibility_{tgp}^{(1-10)} = 1 - \frac{1}{10}\sum_{s=1}^{10} Permit_{t+s,g,p} \tag{5}$$

This variable measures the fraction of time a couple is not exempted from the one-child rule when the first child is between 1 and 10. 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.8 in the Appendix. 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.8 Panel B. The estimates with this new measure of the second-child penalty are similar to the baseline estimates.

Variation in high exposure only

Recently, de Chaisemartin and D'Haultfœuille (2020) raise concerns about the interpretation of estimates from a specification with two-way fixed effects. According to their study, the DID estimates with group and year fixed effects, for instance, are the weighted sum of the average treatment effects (ATE) for each group each year. The weights associated with the ATEs can be negative, which could be an issue when the ATEs are heterogeneous across groups or years. When negative weights are large and correlated with the heterogeneous treatment effects, the estimated coefficient and all ATEs can have different signs. The issue is more salient with continuous treatment variables due to the lack of groups where treatment remains stable between two periods.

Because the triple-difference strategy also includes two-way fixed effects, the triple-difference estimates could be subject to the same issue. However, it is unclear how to apply their method in a triple-difference strategy that allows heterogeneity by individual characteristics. What I do instead is to design a specification in which the incidence of negative weights should be more limited by only exploiting variation in the second-child penalty when the penalty is greater than one year of household income. In other words, I only estimate ATEs for groups that face a relatively high penalty and assume the ATE to be zero for groups that only face a small second-child penalty. The results are shown in Table A.8 Panel C. With this new measure of exposure, I still observe the pattern that there is no heterogeneous effect on family size and health but exhibits significant heterogeneity in the effect on education, assets, and labor market outcomes. The significance levels and signs of the estimates are similar to those of the baseline estimates too. Hence, it is unlikely that the baseline triple-difference estimates are of a different sign of all ATEs.

5 Discussion

Many factors could lead to the homogenous effects of the second-child penalty on child health and heterogeneous effects on child education and assets. This section discusses how the results can be rationalized in light of different factors underlying parental investment decisions. A simple way to rationalize the findings would be to consider a setting in which parents allocate their income between their own consumption before retirement and investments in children's human capital and for other purposes. Parents can transfer cash and assets to their children. Human capital investments and transfers from the parents enter the children's income-generating function. Parents' utility depends on their own consumption and children's expected income, either because they rely on adult children's care and transfers in old age, or because they are altruistic toward their children.

Next, I discuss several factors that may contribute to the investment gaps by parental occupation in response to a change in the price of child quantity. For each channel, I first discuss why it may lead to heterogeneous responses to the second-child penalty and then provide empirical evidence.

5.1 Heterogeneous returns to education

The results show that when an additional child becomes more expensive, farmers and low-skill workers transfer more land or household assets to the existing children while high-skill workers invest more in 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 investment in human capital to the rate of return on investment in physical capital (Galor, 2012). If the rate of return on investment in physical capital is the same for all parents, the heterogeneity in the effect on education and wealth of the children reflects that the rate of return on investment in children's education is different across parents in different occupations.

To check if indeed the returns to education differ across parental occupations, I estimate the Mincerian returns to education and height for children in different occupation groups. I use a sample of children born between 1966 and 1975 in rural China. I then regress the log of income in 2010 on years of schooling and health and interactions between these human capital measures and dummy indicators for the father's occupation.

Table 7 Panel A shows that the Mincerian returns to education and height are positive and significant. However, Panel B shows that the returns to education are significantly higher among children whose fathers worked in high-skill occupations, but the returns to height do not differ by father's occupation. This exhibits the same patterns as the main findings: the effect of the second-child penalty on health is the same for all groups of children while the effect on education is heterogeneous.[22] Hence, the results provide support for the argument that the heterogeneous returns to education across parental occupations are underlying the heterogeneous effects of the second-child penalty on the firstborn children's education.

[22] Table A.9 shows that Mincerian returns only differ by father's occupation and not by the father's education. This is in line with the finding that the effect of education only varies by father's occupation and not father's education (Panel B of Table A.7).

Table 7: 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	0.056***	0.038
	(0.007)	(0.026)
Low-skill × Human capital	0.001	-0.004
	(0.008)	(0.030)
High-skill × Human capital	0.025**	0.004
	(0.011)	(0.054)
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 Mincerian relationship between human capital and log income. Robust standard errors in parentheses.

2. High-skill is a dummy indicator that takes value one if the respondent's father was employed in a high-skill occupation when the respondent was aged 12, and zero if otherwise.

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 7 also shows that the Mincerian returns to education are not lower for farmers' children than for low-skill workers' children. However, I find that the second-child penalty improves more the education of low-skill workers' children that the education of farmers' children. This finding suggests that there might be other factors that prevent farmers from increasing investments in their firstborn children's education when the cost of an additional child increases.

5.2 Land tenure insecurity

One type of assets that is particularly important for farmers is land. Next, I discuss whether the difference in the effects on education between children of farmers and nonfarmers could be explained by land tenure insecurity faced by farmers.

Education played a significant role in raising the accessibility of urban formal employment to rural people in the late 1970s and early 1980s (Zhao, 1997) and in promoting labor mobility from the agricultural sector to the non-agricultural sector in rural areas (Zhao, 1999a). However, urban formal employment or rural non-agricultural employment of children also increase the risk of losing the land allocated to rural households under China's land tenure system (Ngai et al., 2018; Zhao, 2020). In a similar spirit, land tenure insecurity could raise the opportunity cost of education for children that are supposed to stay home and inherit the land use rights from their parents.

In Appendix A.2, I outline a simple model to show why with land tenure insecurity, a reduction in child quantity could lead to a reduction in the education of the first child. Rather than modelling the price of child quantity, I assume that child quantity is exogeneous and can be either one or two. I do this for simplicity but it is also consistent with the finding that the second-child penalty reduces the probability of having a sibling for all groups of children regardless of their father's occupation. I assume that farmers not only care about their own consumption and children's expected resources but also the expected value of the household land because land is a valuable asset to farmers in the long run. I also assume that the expected value of the land decreases with children's education due to the role of education in promoting non-agricultural employment. The important thing is that parents only need one child to stay in the agriculture sector and inherit the land use rights.

The model generates a testable prediction that for two-child households with insecure land tenure, parents invest more in one child's education and less in the other's to minimize the risk of losing the land while maximizing the expected resources of the children. I use a sample of

Table 8: Education and height differences between the first and the second children, by father's occupation

	(1)	(2)	(3)	(4)	(5)
	Educational attainment				
	Years	Primary	Middle	High	Height (sd)
Farmer × First child	0.766	0.055	0.130**	0.079	-0.059
	(0.512)	(0.050)	(0.067)	(0.053)	(0.116)
Non-farmer × First child	-0.299	-0.021	-0.010	-0.014	-0.042
	(0.630)	(0.054)	(0.076)	(0.076)	(0.155)
R^2	0.284	0.192	0.254	0.172	0.066
Mean dep var farmer	7.141	0.770	0.509	0.155	0.046
Mean dep var non-farmer	9.337	0.903	0.760	0.314	0.208
Observations	523	523	523	523	513

Note: * means significant at 10%, ** significant at 5%, and *** significant at 1%.
1. This table reports the human capital gap between the first and the second child in two-child families. 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 both parents' educational attainment, interactions between gender and an indicator of being the first child, interactions between gender and father's occupation, and birth year dummies.
4. The analysis is based on a sample of children from two-child families, who held a rural *hukou* at age 3 and were born between 1966 and 1975.
5. Primary: Ever attend primary education; Middle: Complete middle school education; High: Ever attend high school education.

children born between 1966 and 1975 in two-child families from the CFPS data to test this prediction. I estimate the education and height differences between the first and the second children by father's occupation, distinguishing between farmers and non-farmers. Table 8 shows that there is an education gap if and only if the father is a farmer. The gap in the probability of completing middle school is the most salient, followed by the gap in the probability of attending high school. The same pattern is not observed in nonfarming families and for height. Therefore, the data supports the model in which land tenure insecurity increases the opportunity cost of education for one of the two children.

Table 8 also shows that when a couple has two children, the firstborn children tend to receive more educational investments from their parents. 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).[23] Hence, the second and younger child, who is supposed to stay home, would bear the opportunity cost of

[23]One concern is that the educational advantage of being the first child may differ by gender of the first child. Girls might be treated similarly regardless of birth order because parents would only transfer properties to sons. In Table 8, I already include interactions between gender and birth order and interactions between gender and father's occupation to control for any potential gender difference that is specific to one occupation or one birth order. I also find that the education gap by birth order does not vary significantly by gender, suggesting that the educational advantage of the firstborn child is not gender-specific.

education due to land tenure insecurity. If farmers could only have one child, the first child and also the only child would instead bear the opportunity cost. If the opportunity cost of education due to land tenure insecurity is large, reducing the number of children from two to one could lead to a reduction in the education of the first child.[24]

In Table A.10 in the Appendix, I show estimates from quantile regressions for educational outcomes that provide additional support to this argument. I find that for farmers' children, the second-child penalty reduces the upper quartile of the education distribution, which corresponds to about 9 years of schooling. An explanation consistent with this finding is that the firstborn children who would otherwise complete middle school and attend high school if they had a younger sibling can not do so and have to stay in the countryside when they have no younger siblings. This finding is consistent with Table 8, which shows that the gap between the first and the second children is the most salient in the probability of completing middle school and attending high school. The increase in land assets observed only among firstborn children of farmers is also consistent with the argument that the second-child penalty increases the probability of staying home and inheriting the land use rights for the first child (see Panel C of Table 4).

5.3 Credit market imperfection

Credit market imperfection is believed to be one of the key reasons why parents, especially the poorer ones, are unable to make optimal investments in children's human capital (Caucutt et al., 2017; Caucutt and Lochner, 2020). Parents work in unskilled occupations or in the agricultural sector earn less and have more limited access to credit markets, which might explain the different effects of the second-child penalty on education of the firstborn. Next, I discuss whether credit market imperfection could drive the heterogeneous responses.

If parents can borrow at the market interest rate, then even the poorest parents would invest in their children such that that the marginal return to the investment is equal to the interest rate (Heckman and Mosso, 2014). In this case, parents would not adjust their investment in response to a change in child quantity. The finding that a higher second-child penalty leads to better health outcomes for all groups of children suggests that all groups of parents in the

[24] An alternative explanation relates to the family custom that parents wish to keep at least one child, usually the younger one, at home to leave with them in old age. Unger (2006) summarizes several studies in sociology showing that this custom disappears faster in richer households than in poorer households in China. This explanation would also fit in the model and be consistent with the data if I assume that the custom of coresidence between parents and one of the children only exists in farming families.

sample are credit constrained in the absence of OCP and unable to make efficient investments in their children's human capital.

However, credit market imperfection is inadequate to explain the heterogeneous responses across parents. First, it is inadequate to explain why the second-child penalty reduces the upper quartile of the schooling distribution for children of farmers (see Table A.10 in the Appendix). Even if parents do not respond because their credit constraints are always binding, we should at most observe no effect on education and not a negative effect. Second, credit constraints fail to explain the differential effects on education between low-skill and high-skill workers's children. If low-skill workers invest less because their credit constraints are always binding, they should not be able to invest in housing and financial assets and transfer assets to the first and only child post-OCP. In contrast, I find that children of low-skill workers own significantly more housing and financial assets when their parents face higher second-child penalties. This finding suggests that parents in low-skill occupations are able to finance their children's education but are unwilling to do so, perhaps because the returns to education are lower than the returns to savings or purchasing houses for their children.

6 Conclusion

This study provides empirical evidence of the heterogeneity in the trade-off between child quantity and different dimensions of child quality in rural China. I find that, on average, the second-child penalty imposed by OCP reduces family size and improves the health of the firstborn children, but do not affect the education and wealth of the children. The insignificant effect on education and wealth masks significant heterogeneity across parental occupations due to different expected returns to education and different opportunity costs of education. In response to a higher second-child penalty, only parents in high-skill occupations invest more in their firstborn children's education. Farmers' firstborn children even experience a small reduction in the probability of completing high school but they own more land in adulthood. Low-skill workers' firstborn children experience little change in educational outcomes but own more housing properties when their parents face a higher second-child penalty. This study also shows that, among children born in rural areas, the implementation of higher second-child penalties contributed to reducing intergenerational mobility (Fan et al., 2021): children born in more advantaged families were better able to attain higher education, work in a skilled occupation, earn a higher income, and eventually get an urban *hukou*, whereas children born in farming families were more often stuck on the farm.

A natural question to ask is if a policy that reduces the *price* of child quantity could promote intergenerational mobility. The results provide some insights into this question. In 2016, the Chinese government officially abolished the OCP nationwide and allowed all couples to have two children. In 2021, the two-child rule was further relaxed, and each couple was allowed to have three children. Despite the abolishment of the OCP in 2016, birth rates kept falling and reached their lowest level in 2019 (National Bureau of Statistics, 2019). A common explanation is that the housing and education costs are so high that couples cannot afford more than one child. The results confirm that when it is expensive to increase the quantity of children, the existing children of low-skill workers own more housing assets while those of high-skill workers attain more education. Because these parents spend a lot on their existing children, the cost of raising another child with equal quality might be too high, and they would not respond to the relaxation of the one-child rule. However, relaxing the one-child rule may effectively raise fertility among farmers because the marginal cost of child quality is relatively low in many dimensions, and they desire children to stay on the household farm. Hence, it is likely that the relaxation of the one-child rule alone would not promote intergenerational mobility but may increase the share of the low-educated children from farming households in the next generation.

Rather than only relaxing the OCP, the government should also implement other policies to raise the education of the farmers' children. The rate of high school completion is extremely low in rural China as compared to that in urban China (Khor et al., 2016; Li et al., 2017). A common explanation is that poor rural parents are unable to finance high school and college education for their children. The results of this study suggest that other factors are also important. Some parents are not unable but rather unwilling to send their children to high school and college, either because the insecure land tenure raises the opportunity cost of children's education, or because the expected returns to education are lower than other investments. These findings suggest that policies that raise the expected returns to education or reduce the opportunity cost of education could be effective to enhance the human capital of rural children, who will ultimately comprise most of China's future labor force (Khor et al., 2016).

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Appendix

Appendix A.1 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 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 estimate the effect on standardized scores from these tests.

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.[25] 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 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. Con-

[25] Notice that this information is only available at the household level. Hence, 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.

ditional on employment, I estimate the effect of the second-child penalty on the Treiman scale of the occupation and whether employed in a high-skill 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, an urban *hukou* makes the high-skill jobs in the urban sector more accessible. In the meantime, finding a high-skill job in urban areas is closely associated with getting an urban *hukou* in recent years. Hence, I estimate the effect of the second-child penalty on urban residency, that is, whether or not holding an urban *hukou* in 2010. This variable also reflects an individual's labor market achievement.

Appendix A.2 A model of Q-Q trade-off with land tenure 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 tenure 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 tenure insecurity. In Section 5, I explain why borrowing constraints could not explain the heterogeneity in the effect on education. The assumption of no saving is reasonable given that I find no effect of the second-child penalty on non-land assets for the children of farmers.

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 inherit the land-use rights. And when the parents have no child to inherit the land-use rights, the land will be returned to the village after both parents lose their ability to farm 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

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

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

$$\begin{aligned} e^{OC} &= 0 \\ h^{OC} &= \frac{a\alpha}{c_h} \end{aligned}$$

Two-child regime

In this regime, each parent has two children.

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

subject to

$$c = y - 2c_h h - c_e(e_1 + e_2)$$
$$e_1 \geq 0$$
$$e_2 \geq 0$$

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 parents can transfer the land to this child. 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 children 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

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

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

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

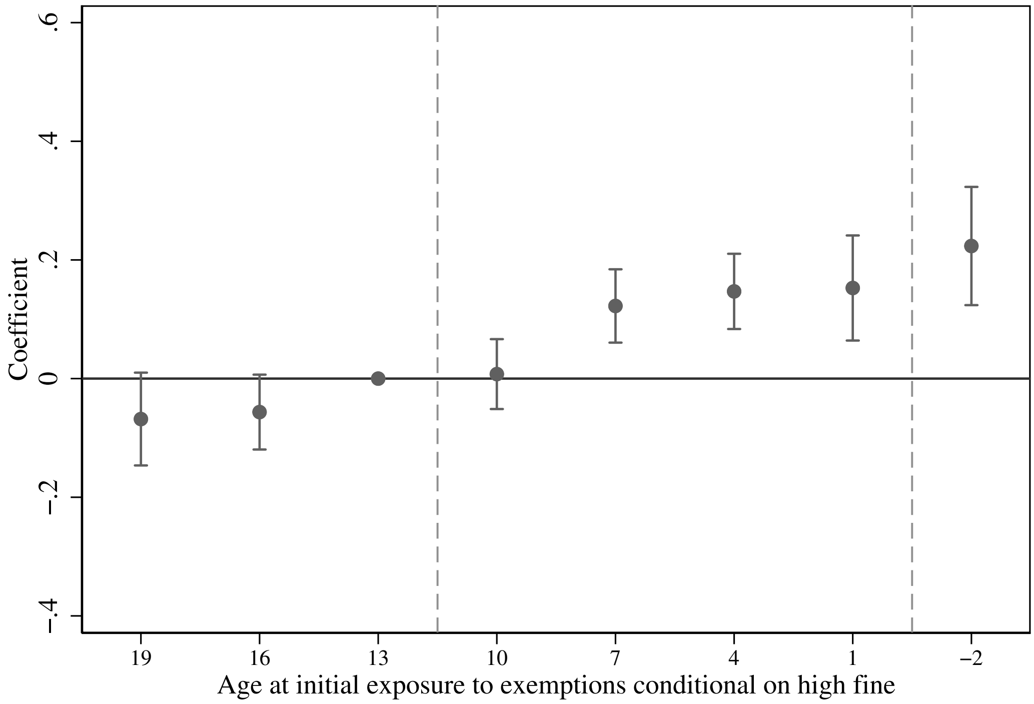
The model generates one testable implication: only families with $\lambda L > 0$ would have an unequal investment in children's education. Farmers are more likely to have $\lambda L > 0$ because they are more likely to own a positive amount of land and also put a positive weight on the value of expected land. Table 8 shows that in farming families with two children, there is indeed an education gap between the firstborn and the secondborn, where the firstborn attains, on average, a higher level of education.

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 8, the effect on the education of the firstborn depends on whether λL is bigger than c_e. If $\lambda L < c_e$, $e^{OC} > e_1^{TC}$. If $\lambda L > c_e$ and $\frac{b\alpha}{\underline{e}} \leq 2c_e$, $e^{OC} = e_1^{TC} = 0$. If $\lambda L > c_e$ and $\frac{b\alpha}{\underline{e}} > 2c_e$, $e^{OC} < e_1^{TC}$. The intuition is as follows. While the reduction in child quantity reduces the direct marginal cost of education, it increases the opportunity cost of education for the first child. If the opportunity cost of education due to land tenure insecurity is higher than the direct cost of education, reducing child quantity from two to one increases the net cost of education for the first child. Hence, when the present value of land is high, a reduction in child quantity from two to one could lead to a reduction in the first child's education.

In Section 5, I combine the model with empirical evidence to discuss how land tenure insecurity may explain the different responses to the second-child penalty between farmers and nonfarmers.

Appendix A.3 Appendix figures

Figure A.1: Difference-in-differences event study estimates



Note: The figure shows the coefficients and 95% confidence intervals (based on standard errors clustered at the province-cohort level) from the event study analysis estimating the following equation:

$$y_{itgp} = \sum_{j \in J} \xi_j D_j + X_i \beta^* + V_t^* + W_{gp}^* + \epsilon_{itgp}^* \tag{6}$$

where V_t^* represents birth year fixed effects and W_{gp}^* represents province-group fixed effects. X_i is the same set of individual controls in estimating equation 2. j denotes the age group of child i and $J = \{-3, -2, 1, 4, 7, 10, 13, 16, 19, 20\}$. For j between -2 and 19, D_j is a dummy variable that takes value one if the event happened at ages j, $j+1$ or $j+2$. An event is defined as the initial exposure to a exemption from paying a fine greater than one year of household income for the second child. 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 became eligible to a second-child permit and exempted from pay a fine greater than one year of household income. The y-axis is the estimate for ξ_j. 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 are exempted from paying a fine greater than one year of household income when the first child is aged j to $j+2$ instead of 13 to 15, conditional on the fixed effects and individual controls.

Appendix A.4 Appendix tables

Table A.1: 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). Gray cells 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.

Table A.2: Triple-difference estimates adjusted by CFPS sample weights

	(1) Siblings	(2) Any sibling	(3) Excellent health	(4) Height (sd)	(5) Height top quintile
Penalty	-0.260*	-0.123**	0.147**	0.131	0.148**
	(0.137)	(0.057)	(0.069)	(0.121)	(0.060)
Low-skill × Penalty	0.016	-0.015	-0.008	-0.048	-0.041
	(0.086)	(0.049)	(0.045)	(0.075)	(0.041)
High-skill × Penalty	0.078	0.055	0.032	0.191	0.105
	(0.166)	(0.061)	(0.099)	(0.184)	(0.087)
R^2	0.603	0.639	0.349	0.475	0.409
Mean dep var	1.471	0.771	0.589	0.015	0.216
Observations	2894	2894	2893	2807	2807
	(6) Schooling	(7) HS completion	(8) Cognition	(9) Land	(10) Nonland
Penalty	-0.000	-0.035	0.066	1.391*	1.408
	(0.443)	(0.062)	(0.120)	(0.837)	(6.216)
Low-skill × Penalty	-0.020	0.065	0.074	-0.848	11.043***
	(0.269)	(0.044)	(0.077)	(0.647)	(4.258)
High-skill × Penalty	1.020*	0.138**	0.246*	-0.899	4.657
	(0.569)	(0.069)	(0.149)	(1.043)	(12.807)
R^2	0.592	0.509	0.579	0.448	0.590
Mean dep var	8.351	0.245	0.000	4.663	48.767
Observations	2894	2894	2893	2851	2763
	(11) Employed	(12) Occu score	(13) High-skill	(14) Urban *hukou*	(15) Income
Penalty	0.116	-2.527	-0.189*	0.016	3.066
	(0.075)	(3.195)	(0.106)	(0.049)	(2.274)
Low-skill × Penalty	-0.051	1.996	0.034	-0.021	1.282
	(0.045)	(1.706)	(0.046)	(0.030)	(1.245)
High-skill × Penalty	-0.018	6.925**	0.171*	0.147*	5.008*
	(0.081)	(3.056)	(0.099)	(0.086)	(2.796)
R^2	0.448	0.528	0.568	0.469	0.551
Mean dep var	0.618	39.941	0.113	0.174	15.675
Observations	2845	1785	1757	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 3 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.3: Balancing test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
						Father's occupation at age 12		
	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	0.415	0.230	-0.498	0.055	0.117	-0.043	0.068	-0.025
	(0.495)	(0.453)	(0.433)	(0.419)	(0.384)	(0.042)	(0.046)	(0.029)
R^2	0.753	0.785	0.325	0.425	0.267	0.328	0.335	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.4: Total effects of the second-child penalty by father's occupation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Siblings	Any sibling	Good health	Height (sd)	Height top quintile	Schooling	HS completion
Farm × Penalty	-0.206*	-0.133***	0.099*	0.220**	0.144***	0.156	-0.026
	(0.109)	(0.046)	(0.056)	(0.109)	(0.052)	(0.367)	(0.049)
Low-skill × Penalty	-0.248**	-0.162***	0.081	0.172	0.139**	0.257	0.048
	(0.113)	(0.047)	(0.059)	(0.120)	(0.055)	(0.415)	(0.057)
High-skill × Penalty	-0.137	-0.109	0.145	0.206	0.181**	0.962	0.128*
	(0.162)	(0.067)	(0.089)	(0.190)	(0.090)	(0.585)	(0.076)
R^2	0.540	0.590	0.270	0.383	0.303	0.549	0.440
Mean dep var	1.471	0.771	0.589	0.015	0.216	8.351	0.245
Observations	2894	2894	2893	2807	2807	2894	2894
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Cognition	Land	Non-land	Employed	Occu score	High-skill	Urban *hukou*
Farm × Penalty	0.059	1.592*	2.657	0.112*	-0.619	-0.093	0.055
	(0.091)	(0.886)	(6.060)	(0.062)	(2.556)	(0.095)	(0.037)
Low-skill × Penalty	0.106	0.063	18.203***	0.070	1.813	-0.044	0.042
	(0.100)	(0.974)	(6.925)	(0.065)	(2.753)	(0.102)	(0.037)
High-skill × Penalty	0.219	0.282	-7.643	0.096	5.153	0.049	0.176**
	(0.148)	(1.541)	(15.534)	(0.090)	(3.945)	(0.133)	(0.080)
R^2	0.542	0.379	0.593	0.346	0.433	0.432	0.422
Mean dep var	0.000	4.663	48.767	0.618	39.941	0.113	0.174
Observations	2893	2851	2763	2845	1785	1757	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 a farmer 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. See notes below Table 3 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: The second-child penalty and intergenerational income mobility

	(1) Income	(2) Log income
Penalty	0.809	-0.032
	(2.122)	(0.263)
Computed income of father × Penalty	0.116*	
	(0.069)	
Computed log income of father × Penalty		0.058
		(0.082)
R^2	0.500	0.507
Observations	2724	2040

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 3 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 computed use the average income of men born between 1946 and 1965 in the same type of occupations.

Table A.6: Family size or sibling gender composition: only provinces without strong son preference

	(1)	(2)	(3)	(4)	(5)	(6)
	Siblings	Any sibling	Any male sib	Any female sib	Height (sd)	HS completion
Penalty	-0.237	-0.175***	-0.101*	-0.086	0.339**	-0.047
	(0.149)	(0.067)	(0.057)	(0.070)	(0.139)	(0.060)
Low-skill × Penalty	0.005	-0.005	-0.048	0.022	-0.018	0.093**
	(0.068)	(0.033)	(0.030)	(0.032)	(0.072)	(0.038)
High-skill × Penalty	0.087	0.035	0.010	-0.000	0.010	0.149***
	(0.135)	(0.057)	(0.063)	(0.066)	(0.152)	(0.057)
R^2	0.599	0.640	0.496	0.421	0.433	0.467
Mean dep var	1.451	0.739	0.523	0.460	-0.027	0.262
Observations	1869	1869	1869	1869	1816	1869
	Land	Assets	Employed	Occu. score	High-skill	Income
Penalty	1.712	10.038	0.115	-1.240	-0.122	3.233*
	(1.132)	(8.046)	(0.085)	(2.898)	(0.104)	(1.855)
Low-skill × Penalty	-1.764**	16.047***	-0.035	2.356	0.044	1.005
	(0.687)	(5.242)	(0.053)	(1.610)	(0.041)	(1.214)
High-skill × Penalty	-0.975	-11.624	0.001	5.146	0.083	4.667
	(1.304)	(16.620)	(0.093)	(3.894)	(0.102)	(3.023)
R^2	0.422	0.625	0.367	0.440	0.449	0.499
Mean dep var	4.889	52.196	0.623	39.807	0.115	16.210
Observations	1843	1782	1830	1160	1140	1759

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 3 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: Robustness checks

	(1) Siblings	(2) Any sibling	(3) Height (sd)	(4) HS completion	(5) Land	(6) Assets	(7) Employed	(8) High-skill	(9) Income
Panel A. Group-specific effects of socioeconomic development									
Penalty	-0.216*	-0.111**	0.244**	-0.030	2.104**	3.816	0.125*	-0.077	1.844
	(0.114)	(0.047)	(0.116)	(0.051)	(1.005)	(5.527)	(0.065)	(0.093)	(1.912)
Low-skill × Penalty	-0.039	-0.027	-0.042	0.076**	-1.561***	15.155***	-0.043	0.048	1.187
	(0.061)	(0.030)	(0.061)	(0.036)	(0.563)	(4.038)	(0.042)	(0.038)	(1.070)
High-skill × Penalty	0.067	0.023	-0.011	0.153***	-1.326	-10.240	-0.016	0.142*	4.214*
	(0.128)	(0.055)	(0.133)	(0.052)	(1.073)	(13.380)	(0.079)	(0.084)	(2.490)
R^2	0.542	0.592	0.386	0.442	0.381	0.595	0.348	0.433	0.501
Panel B. Interactions of the second-child penalty with paternal education									
Penalty	-0.214*	-0.137***	0.215*	-0.038	1.689*	0.361	0.118*	-0.096	2.350
	(0.114)	(0.049)	(0.118)	(0.053)	(0.984)	(6.973)	(0.065)	(0.098)	(1.833)
Low-skill × Penalty	-0.037	-0.028	-0.051	0.074**	-1.499***	15.568***	-0.041	0.059	1.296
	(0.060)	(0.029)	(0.059)	(0.036)	(0.563)	(4.126)	(0.042)	(0.039)	(1.103)
High-skill × Penalty	0.097	0.026	-0.041	0.150***	-1.108	-11.515	-0.003	0.160**	4.819*
	(0.133)	(0.055)	(0.145)	(0.056)	(1.179)	(12.844)	(0.081)	(0.079)	(2.593)
Middle school × Penalty	0.017	0.008	0.008	0.029	-0.257	4.914	-0.012	0.033	-0.792
	(0.055)	(0.023)	(0.057)	(0.036)	(0.606)	(6.219)	(0.049)	(0.038)	(1.172)
High school × Penalty	-0.078	-0.009	0.057	0.006	-0.511	0.977	-0.031	-0.092	-1.542
	(0.135)	(0.049)	(0.117)	(0.053)	(0.825)	(8.160)	(0.059)	(0.069)	(1.749)
R^2	0.540	0.590	0.384	0.441	0.380	0.594	0.347	0.435	0.501
Mean dep var	1.471	0.771	0.015	0.245	4.663	48.767	0.618	0.113	15.675
Observations	2894	2894	2807	2894	2851	2763	2845	1757	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 3 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: Alternative treatment measures

	(1) Siblings	(2) Any sibling	(3) Height (sd)	(4) HS completion	(5) Land	(6) Assets	(7) Employed	(8) High-skill	(9) Income
Panel A. Measuring the second-child penalty using only second-child permit eligibility									
Ineligibility	-0.298	-0.230**	0.440*	0.000	3.144	16.530	0.164	-0.110	1.670
	(0.319)	(0.108)	(0.246)	(0.110)	(1.933)	(19.089)	(0.166)	(0.145)	(3.708)
Low-skill × Ineligibility	-0.124	-0.044	-0.088	0.091	-1.932*	31.504***	-0.110*	0.029	3.930*
	(0.135)	(0.054)	(0.118)	(0.069)	(1.075)	(9.296)	(0.065)	(0.075)	(2.055)
High-skill × Ineligibility	0.135	0.050	-0.171	0.196*	-0.914	10.579	-0.051	0.175	7.875**
	(0.234)	(0.099)	(0.222)	(0.100)	(1.540)	(22.418)	(0.123)	(0.154)	(3.868)
R^2	0.540	0.589	0.383	0.440	0.378	0.594	0.346	0.431	0.501
Panel B. Considering the requirement of a minimum 3-year spacing									
Penalty^s	-0.206	-0.144***	0.240*	-0.030	1.786*	4.300	0.130*	-0.090	1.695
	(0.133)	(0.052)	(0.123)	(0.058)	(0.999)	(6.892)	(0.070)	(0.101)	(2.085)
Low-skill × Penalty^s	-0.055	-0.042	-0.031	0.083**	-1.542**	16.690***	-0.040	0.056	1.379
	(0.066)	(0.031)	(0.063)	(0.037)	(0.606)	(4.501)	(0.044)	(0.038)	(1.135)
High-skill × Penalty^s	0.028	-0.004	-0.029	0.162***	-1.331	-10.882	-0.021	0.148*	4.174*
	(0.130)	(0.053)	(0.137)	(0.053)	(1.161)	(13.990)	(0.082)	(0.086)	(2.518)
R^2	0.540	0.590	0.383	0.442	0.380	0.594	0.346	0.433	0.501
Panel C. Variation in high second-child penalty only									
Penalty × $\mathbb{I}$(Penalty $\geq$ 1)	-0.198**	-0.088**	0.178*	-0.021	0.867	4.666	0.092*	-0.020	1.148
	(0.099)	(0.040)	(0.095)	(0.041)	(0.740)	(5.492)	(0.052)	(0.066)	(1.598)
Low-skill ×	-0.060	-0.042	-0.027	0.093***	-1.234**	14.191***	-0.049	0.054*	1.018
Penalty × $\mathbb{I}$(Penalty $\geq$ 1)	(0.054)	(0.027)	(0.054)	(0.032)	(0.503)	(4.067)	(0.037)	(0.033)	(0.969)
High-skill ×	0.050	0.019	-0.016	0.145***	-1.116	-19.174	-0.027	0.182**	3.360
Penalty × $\mathbb{I}$(Penalty $\geq$ 1)	(0.123)	(0.053)	(0.107)	(0.047)	(0.945)	(13.188)	(0.074)	(0.079)	(2.298)
R^2	0.540	0.590	0.383	0.443	0.379	0.595	0.347	0.435	0.500
Mean dep var	1.471	0.771	0.015	0.245	4.663	48.767	0.618	0.113	15.675
Observations	2894	2894	2807	2894	2851	2763	2845	1757	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 3 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.9: 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	0.057***	0.036
	(0.007)	(0.029)
Low-skill × Human capital	0.001	-0.003
	(0.008)	(0.031)
High-skill × Human capital	0.022*	0.006
	(0.012)	(0.055)
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.10: Distributional effects of the second-child penalty on education

	(1)	(2)	(3)
		Years of schooling	
		Conditional percentile	
	25th	50th	75th
Penalty	0.102	-0.277	-0.679*
	(0.264)	(0.378)	(0.413)
Low-skill × Penalty	0.468**	0.408	0.455
	(0.232)	(0.312)	(0.325)
High-skill × Penalty	1.466**	1.032***	0.794***
	(0.635)	(0.318)	(0.279)
Total effect (Low-skill)	0.571	0.131	-0.224
	(0.367)	(0.414)	(0.420)
Total effect (High-skill)	1.568**	0.755	0.115
	(0.618)	(0.506)	(0.436)
R^2	0.135	0.137	0.139
Observations	2894	2894	2894

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

1. Each column represents a separate quantile regression for residuals from a regression of income on the full set of province-group, province-cohort, and group-cohort fixed effects. 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. 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. See notes below Table 3 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.