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Three Essays on Human Capital Investment in China

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Abstract

This dissertation consists of three empirical essays on human capital investment issues in China. The first essay examines the trade-off between child quantity and quality in rural China, exploiting a source of exogenous variation in family size generated by the temporary relaxation in China's one-child policy in the mid-1980s. The relaxed population policy allowed a rural couple to have a second child if the first-born was a girl. Exploiting this policy change, this essay creates IVs for family size from the sex-composition of the first two children in a family. The IV results indicate that rural parents hardly face a trade-off between child quantity and quality, at least in terms of their monetary investments in children's education. These results imply that relaxing the one-child policy, as has been proposed by many researchers as a solution to the "missing girls" problem, is unlikely to cause reductions in parental investments in children's education. The second essay investigates the impact of parental education on children's academic skills acquired in basic education (grades 1-9) in rural China. It uses the scores on a cognitive ability test as an error-ridden measure of child ability, and then instruments this ability measure using IVs generated from the Great Chinese Famine (1958-61). It finds that parental education has a statistically significant impact on children's academic skills, even after controlling for child ability. Moreover, while father's education matters for child math skills for both boys and girls, mother's education matters only for girls. These results imply that promoting rural women's education may be an effective way to reduce the gender gap in math skills. The third essay estimates the causal impact of mother's education on standardized child height, exploring the Chinese Cultural Revolution (1966-76) to create IVs for mother's education. The preferred IV estimates indicate that the loss in mother's education due to the Chinese Cultural Revolution led to a 0.3 standard deviation decrease in child height. This loss is substantial, in a magnitude similar to the effect of being exposed in early childhood to the Chinese Great Famine (1959-61).

Keywords

educational sociology, Asian studies, women's studies, labor economics, demography

Disciplines

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Abstract

This dissertation consists of three empirical essays on human capital investment issues in China. The first essay examines the trade-off between child quantity and quality in rural China, exploiting a source of exogenous variation in family size generated by the temporary relaxation in China's one-child policy in the mid-1980s. The relaxed population policy allowed a rural couple to have a second child if the first-born was a girl. Exploiting this policy change, this essay creates IVs for family size from the sex-composition of the first two children in a family. The IV results indicate that rural parents hardly face a trade-off between child quantity and quality, at least in terms of their monetary investments in children's education. These results imply that relaxing the one-child policy, as has been proposed by many researchers as a solution to the "missing girls" problem, is unlikely to cause reductions in parental investments in children's education.

The second essay investigates the impact of parental education on children's academic skills acquired in basic education (grades 1-9) in rural China. It uses the scores on a cognitive ability test as an error-ridden measure of child ability, and then instruments this ability measure using IVs generated from the Great Chinese Famine (1958-61). It finds that parental education has a statistically significant impact on children's academic skills, even after controlling for child ability. Moreover, while father's education matters for child math skills for both boys and girls, mother's education matters only for girls. These results imply that promoting rural women's education may be an effective way to reduce the gender gap in math skills.

The third essay estimates the causal impact of mother's education on standardized child height, exploring the Chinese Cultural Revolution (1966-76) to create IVs for mother's education. The preferred IV estimates indicate that the loss in mother's education due to the Chinese Cultural Revolution led to a 0.3 standard deviation decrease in child height. This loss is substantial, in a magnitude similar to the effect of being exposed in early childhood to the Chinese Great Famine (1959-61).

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Chapter 1. Introduction

This dissertation consists of three empirical essays on the intergenerational transmission of human capital in China. More specifically, they examine how Chinese parents make their decisions on investments in their children's education and how parents' education affects their children's education and health outcomes.

Given the important role human capital (both educational attainment and health status), plays in economic development,[1] identifying the determinants of and the barriers to parents' investments in their children's human capital is certainly of both academic and policy interest. The first essay (Chapter 2), "Relaxed Population Policy and Parental Investments in Children's Education" examines the impact of one potential barrier to investments in child education: namely, family size. Large family size is often thought to dilute parental resources for investments in children's education, given the well-documented negative correlation between family size and child educational outcomes. Yet such a correlation does not necessarily imply causation operating from large family size to low child educational outcomes, because fertility decisions and investment decisions are usually jointly made within the household. China's temporary relaxation of its one-child policy in the second half of the 1980s provides an opportunity to estimate the casual impact of family size on parental monetary investments in child education. Under the relaxed policy, a second child was allowed if the first-born child was a girl. Exploiting this policy change, Chapter 2 uses the sex-composition of the first two children in a family to create instrumental variables (IV) for family size. In contrast to the

[1] See Hanushek and Woessmann (2008) and Strauss and Thomas (1998) for thorough reviews on the roles of education and health play in economic development, respectively.

results from ordinary least-squares regressions, which find a strong negative impact of family size on child education, the IV regressions find little impact of family size. These results have important policy implications. China's one-child policy has caused many unintended demographic problems, such as the "missing girls" problem, which has led many researchers to propose relaxations of the policy. One major concern is that these relaxations might cause reductions in parental investments in children's education. Yet Chapter 2 provides evidence that this possibility is likely to be negligible.

In addition, Chapter 2 finds that parental education, especially mother's education, is a key determinant of parental investments in child education. Yet, can parental education be translated into better child educational outcomes via these investments? Although a positive correlation between parental education and child human capital outcomes is often documented in previous empirical studies, such a correlation might simply reflect the influence of a child's unobserved genetic endowment on his or her human capital outcomes. Better-educated parents are likely to have better genetic endowments and are thus likely to have better-endowed children. The second and the third essays focus on how to control for unobserved genetic endowments, when investigating the impact of parental education on child human capital outcomes.

The second essay (Chapter 3), "Parental Education and Child Academic Skills in Rural China" investigates the impact of parental education on child academic skills acquired in the basic education stage (grades 1-9), measured by achievement test scores, in rural China. To control for unobserved child ability, this paper applies the multiple indicator method developed by Blackburn and Neumark (1992). It uses the scores of a cognitive ability test (*CAT*) as an error-ridden measure of child ability. This error-ridden ability measure is then instrumented by a set of IVs generated from the 1958-61Great Chinese Famine. Controlling for child ability, parental education still has a statistically significant impact on children's academic skills, although its impact seems quite small in magnitude. Moreover, the impacts of father's education and mother's education differ systematically across child gender groups – while father's education has a significantly positive effect on child math skills for both boys and girls, mother's education matters only for girls. The impact of mother's education, combined with a significant gender gap in math skills found in the Chapter, implies that not only education, but also the gender gap in education, can be transmitted intergenerationally.

The third essay (Chapter 4), "Interrupted Maternal Education and Child Health: The Long Run Health Impact of the Chinese Cultural Revolution" further investigates the impact of parental education on child health, another important aspect of child human capital. It tests the causal relationship between mother's education and child height (standardized by age and sex), using the 1966-76 Chinese Cultural Revolution as a natural experiment. The empirical results in this paper confirm a significant casual effect of mother's education on child height and imply a substantial loss in child health caused by the educational interruptions during the Chinese Cultural Revolution. Using the IVs generated from the Chinese Cultural Revolution, the preferred IV estimates in this paper indicate that the loss in mother's education due to the Chinese Cultural Revolution led to over 0.3 standard deviations' decrease in child height. Such a loss in child height is substantial; this magnitude is similar to the effect of being exposed in early childhood to the Chinese Great Famine in 1959-61, the greatest famine in human history.

In summary, the results of this dissertation suggest that boosting parental education, especially mother's education, in China will improve not only the well-being of the targeted generation – the future better-educated parents – but also the well-being of the next generation – the children of these better-educated parents.

Chapter 2. Relaxed Population Policy, Family Size and Parental Investments in Children's Education in Rural China[2]

2.1. Introduction

Reduction in fertility rates is thought to play a fundamental role in the economic growth of developing countries (e.g. World Bank 1994; Li and Zhang 2007). One widely-cited hypothesis consistent with this claim is that a reduction in the fertility rate can induce more investments in the human capital of children, as it frees up limited household resources for such investments. The growth experiences of East Asian economies in recent decades appear to support this hypothesis. Since the 1970s, high economic growth rates have been accompanied by both restrained population growth and increased educational attainment in such economies as China, Hong Kong, Japan, and South Korea. At the same time, the development of the "new household economics", in particular the theory of the trade-off between child quantity and quality (Becker and Lewis 1973), lays out the microeconomic foundation of this hypothesis. Further, this hypothesis has gained empirical support from a number of studies (e.g. Rosenzweig and Wolpin 1980; Blake 1989; Li, Zhang, and Zhu 2008) that find a *negative* association between individual children's human capital, e.g. measured by their educational attainment, and the number of children in their family.

Not only does this hypothesis offer insights into the mechanisms of intrahousehold allocation of resources, but it also has great relevance to population policy in developing countries, including contemporary China. This hypothesis mirrors the rationale behind

[2] An earlier version of this essay was presented at the 2012 Midwest Economists Association annual meeting in Evanston, IL, on March 31, 2012.

China's one-child policy, which was established in 1979.[3] After more than thirty years since it was first enacted, the one-child policy has resulted in many unintended demographic problems, including an escalating imbalance in the sex ratio (also known as the "missing girls" problem), a distorted marriage market, and a rapidly aging population (Ebenstein and Sharygin 2009). These problems have led many scholars to propose relaxations in China's population policy, in particular to allow for more children in each family (e.g. Attané 2002; Wang 2005). One argument against such proposals, however, is that the relaxation of China's one-child policy might cause reductions in parental investments in the human capital of later generations, especially when there is a strong trade-off between child quantity and quality.

Clearly, the understanding of the potential effects of future changes in China's population policy hinges on understanding parental behaviors regarding investments in children's human capital in response to changes in family size. This paper attempts to shed some light on this issue by testing the trade-off between child quantity and quality faced by parents. At the same time, it overcomes two problems that may have led to misunderstandings of parental behaviors in the literature. First, probably due to the lack of data on parental investments, previous studies have focused almost exclusively on child human capital *outcomes*, such as educational attainment, test scores and even wages. These measures, usually observed long after parental investments had been made, are likely to be tainted by factors that are not under the direct control of the parents, such as improvements in school quality and developments in local labor markets. The

[3] The official document that launched the one-child policy, "An Open Letter to Members of the Chinese Communist Party and Chinese Communist Youth League on Controlling Population Growth", argued that "rapid population growth results in difficulties in providing food, clothing, housing, transportation, *education*, *medical care,* and employment for the population."(Wang 2005; italics added)

influences of these factors on child human capital outcomes will likely disguise the true trade-off faced by parents, thereby leading to a misunderstanding of parental behaviors. With data on the direct measures of parental investments available, namely monetary investments in children's education, this paper is able to circumvent this problem.

Secondly, and more importantly, fertility decisions and child human capital investment decisions are likely to be *jointly* made by the parents. There are many factors, such as parental health and child genetic endowments, that affect both parents' fertility decisions and their human capital investment decisions. When these factors are unobserved to the econometrician but (perhaps imperfectly) observed by parents, conventional empirical approaches, e.g. ordinary least-squares regressions, are likely to yield biased estimates of the causal impact of family size on parental investments in their children's human capital. Consistently estimating such a causal effect requires a source of exogenous variation in family size.[4]

This paper explores the temporary relaxation of China's one-child policy in rural China from the mid-1980s to the early 1990s to obtain a source of exogenous variation in family size. Since its inception in 1979, the one-child policy was strictly enforced in both urban and rural China during its first five years. Yet international controversy and domestic resistance led the central government to officially relax the one-child policy in rural China in 1984, allowing rural couples to have more children if certain conditions were met. In most rural areas, a rural couple was allowed to have a second child if the

[4] Recent studies, inspired by Rosenzweig and Wolpin's (1980) seminal work, routinely explored the incidence of twin births to obtain exogenous variation in family size (e.g. Black, Devereux, and Salvanes 2005; Li, Zhang, and Zhu 2008). To the extent that twinning is unexpected by parents, the births of twins do generate exogenous variation in the number of children in the family. Yet a pitfall follows immediately from this approach, especially when focusing on child educational outcomes: an n-parity twin may not be comparable to an *n*-parity singleton. In fact, twins are generally found to have worse endowments at birth and lower human capital in adulthood than their singleton counterparts (e.g. Angrist and Evans 1998; Rosenzweig and Zhang 2009). Twin births are used in section 2.6 for robustness checks.

first-born was a girl. This relaxation, which was clearly in response to rural couples' son-preference, implies that the sex-composition of the earlier-born children in the family will have strong predictive power for the size of this family.[5] Thus, variables characterizing the sex-composition of earlier-born children serve as instrumental variables for family size.[6]

Using data collected in rural areas in Gansu, a poor province in northwest China, this chapter finds that an exogenous increase in the number of children, in particular, from two to three, will lead to a 40%-50% increase in parents' monetary investments in children's education, implying little trade-off between child quantity and quality. Such results remain similar when other instruments for family size, e.g. indicators of twin births, are used. In contrast, ordinary least-squares regressions, which are likely to suffer from omitted variable and simultaneity biases, yield a much stronger trade-off: couples who already have two children increase their monetary investments in children's education by only around 30% in response to one additional child born to them. Also, this chapter finds that while parents do not reduce the average level of their investments, they do seem to reallocate their investments in favor of the youngest child in the family.

This chapter unfolds as follows. The next section provides a simple conceptual framework for thinking about the trade-off that parents face between child quantity and quality, followed by a section describing the empirical strategy for estimating such a

[5] Unfortunately, son-preference also causes an identification problem. Given the birth quota of two (children) in many rural areas, the second fetus of a rural woman was subjected to sex-selective abortion given son-preference. See below for more discussions on this point.

[6] A similar strategy has been used by Angrist and Evans (1998) and Conley and Glauber (2006), but in a very different setting. Exploiting parents' preferences on the (balanced) sex composition of their children in the United States, these authors use sex composition of the first two children as instruments for the probability of having a third child. More recently, Lee (2008) uses the sex of the first-born as the instrument for having a second child in South Korea.

trade-off. Section 2.4 describes the data analyzed in this essay. Sections 2.5 through 2.7 report and discuss empirical findings. The final section concludes.

2.2. Conceptual Framework

This section develops a simple conceptual framework for thinking about the trade-off between child quantity and quality faced by the parents, and how different measures of child quality might lead to different estimates of such a trade-off.

Consider a simple model of the trade-off between child quantity and quality, in the spirit of Becker and Lewis (1973). A household with a fixed amount of financial resources, *m*, faces the following expenditure constraint:

$$c + H = c + \sum_{i=1}^{n} h_i = c + n \times \bar{h} \leq m \qquad (1)$$

where *c* is the total household expenditure on a consumption good, which also serves as the numeraire, *n* is the number of children, and *H* is the total expenditure on investments in children's education (i.e. $H = \sum_{i=1}^{n} h_i$, where h_i is the monetary investments in child *i*'s education, *i* = 1, 2,…, *n*). *H* can also be further expressed as $H = n \times \bar{h}$, i.e. the product of the number of children in the household (*n*) and the average educational investments in each child ($\bar{h} = \sum_{i=1}^{n} h_i / n$).

The interaction between *n* and $\bar{h}$ suggests the trade-off between child quantity and quality of children, when (1) is binding. Given the fixed amount of financial resources, the household has to reduce its investments in each child in response to an increase in the number of children, if it wants to keep the level of household consumption from declining. More importantly, the interaction term between $\bar{h}$ and n implies that the marginal cost of increasing the level of $\bar{h}$ depends on the number of children (i.e. $\frac{\partial H}{\partial \bar{h}} = n$), and vice versa. The trade-off between child quantity and quality implies that $\frac{\partial \bar{h}}{\partial n} < 0$ in equilibrium. Thus, a consistent estimate of the relationship between $\bar{h}$ and n, i.e. $\frac{\partial \bar{h}}{\partial n}$, provides a direct measure of the trade-off between child quantity and quality.

Alternative measures of the trade-off between child quantity and quality have also been used in the literature. The most commonly used measures include (a) $\frac{\partial h_i}{\partial n}$, which captures the relationship between parental investments in an individual child, h_i, and n, and (b) $\frac{\partial q_i}{\partial n}$, which captures the relationship between in an individual child's human capital outcomes, q_i, and n. While these alternative measures could be informative, they are likely to capture the effects of factors that are not under the control of parents, thereby yielding misleading estimates of the trade-off between child quantity and quality as well as possible misunderstandings of parental behaviors.

To see why this is the case, consider first measure (a), $\frac{\partial h_i}{\partial n}$, the relationship between h_i and n. The sign of $\frac{\partial h_i}{\partial n}$ is theoretically ambiguous. This is because $\frac{\partial h_i}{\partial n}$ captures not only the quantity-quality trade-off of children, but also the effects of resource allocation among children (which may or may not be a result of the quantity-quality trade-off). The existence of a trade-off between child quantity and quality (i.e. $\frac{\partial \bar{h}}{\partial n} < 0$) does not necessarily implies that $\frac{\partial h_i}{\partial n} < 0$; $\frac{\partial h_i}{\partial n}$ could even be positive for some *i*. For example, when facing credit constraints, parents may choose to send their older children to the labor market earlier (before they have completed their desired level of education), and then reallocate their financial resources (including money earned by the older children) toward their younger children. In this case, $\frac{\partial h_i}{\partial n}$ may pick up some birth order effects, because children from large families are more likely to be of higher birth order than children from small families. Therefore, $\frac{\partial h_i}{\partial n}$ serves as a noisy measure of the trade-off between child quantity and quality.

Next, consider measure (b), $\frac{\partial q_i}{\partial n}$, the relationship between q_i and *n*. One can easily see that $\frac{\partial q_i}{\partial n}$ is likely to suffer from the same problems mentioned above, e.g. capturing resource allocation effects. But it also has other problems. Suppose that child *i*'s educational *outcome*, q_i, is produced by the following education production technology:

$$q_i = \pi(h_i, \mathbf{k}, \mathbf{f}, \mathbf{s}) \tag{2}$$

where π is the production function for a child educational outcome, such as a test score. Presumably, a child's educational outcome is not only a function of h_i, but also a function of other inputs such as child endowments (**k**, e.g. child gender and ability), family background (**f**, e.g. parental education), and school characteristics (**s**, e.g. teacher qualifications).[7]

Equation (2) implies that $\frac{\partial q_i}{\partial n} = \frac{\partial \pi}{\partial h_i}\frac{\partial h_i}{\partial n}$. This means that the relationship between q_i and n captures not only the relationship between h_i and n, but also the marginal productivity of h_i, $\frac{\partial \pi}{\partial h_i}$, a function of factors in **k**, **f**, and **s**, some of which are not under the control of parents. If the marginal product of h_i, $\frac{\partial \pi}{\partial h_i}$, is large, e.g. when the children live in an area with high school quality, one might estimate a large negative impact of n on q_i, even when the impact of n on h_i is actually small. Thus, $\frac{\partial q_i}{\partial n}$ is a noisier measure of the quantity-quality trade-off of children than $\frac{\partial h_i}{\partial n}$. Without an consistent estimate of $\frac{\partial \pi}{\partial h_i}$, one cannot recover $\frac{\partial h_i}{\partial n}$ from the estimate of $\frac{\partial q_i}{\partial n}$, let alone $\frac{\partial \bar{h}}{\partial n}$, the more direct measure of the trade-off between child quantity and quality.

Simply put, the observed/estimated trade-off between child quantity and quality depends on what relationship is being used to measure this trade-off. Since the hypothesis of interest in this essay is directly related to parental investment behaviors, this essay estimates the causal effect of family size (n) on the average investments in child

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[7] Factors in **s** could be correlated with n. For example, conditional on the age of the first child in the family, large families will, on average, have more young children than small families. Thus, in an area where the quality of local schools improves over time, children from larger families will, on average, receive better education from local schools. If parents adjust their investments in their children's education in response to changes in school quality, then n will pick up some effects of school quality on parental investments in children's education. This implies that one should at least control for school quality in empirical analysis. Since in rural China, each village has only one primary school and at most one middle school, village dummies are included in all regressions in the empirical analysis below.

education ($\bar{h}$), which provides the most direct measure of how parents respond to changes in family size.

2.3. Estimation Strategy

2.3.1. Estimating equations

The theory of the quantity-quality trade-off of children not only predicts a negative relationship between $\bar{h}$ and *n*; it also predicts that parents are likely to reallocate their resources among their children in response to changes in family size, suggesting a birth order effect due to the reallocation of resources (e.g. Hanushek 1992). To avoid confounding the impact of family size with that of birth order, due to the fact that family size is always equal to the birth order of the last-born child, parents' monetary investments in children's education are aggregated at the household level. Following Lee (2008), this essay estimates the following (investment) equation:

$$\ln H = \beta_0 + n\beta_1 + \mathbf{x}\boldsymbol{\beta}_2 + \mathrm{u} \qquad (3)$$

where *H* is the yearly total parental monetary investments and *n* is the number of children. Vector **x** includes three sets of variables that potentially affect parental investment decisions: family background variables (**f**), child characteristic variables (**k**) and community variables/school characteristics (**s**). Finally, *u* is the error term, which captures the effects of unobserved factors that affect parental investment decisions (see below for further discussion on the error term). Given that *n* represents the household's optimal fertility outcome, equation (3) can be interpreted as a *conditional demand function* for investments in children's education.

If equation (3) is corretly specified, the parameter β_1 measures the percentage change in parental investments *caused* by an exogenous increase in the number of children.[8]

Recognizing that, by the chain rule, $\beta_1 = \frac{\partial \ln H}{\partial n} = \frac{\partial \ln(n \times \bar{h})}{\partial n} = (\frac{1}{n} + \frac{1}{\bar{h}}\frac{\partial \bar{h}}{\partial n}) = \frac{1}{n}(1 + \varepsilon_{\bar{h},n})$,

the elasticity of $\bar{h}$ with respect to n, $\varepsilon_{\bar{h},n}$, can be recovered as $\hat{\varepsilon}_{\bar{h},n} = \bar{n} \times \hat{\beta}_1 - 1$, where $\bar{n}$ is the sample mean of the number of children.

To estimate β_1 consistently, one needs to overcome two endogeneity problems. First, parental fertility decisions may depend on parental investments. The interaction term between $\bar{h}$ and n in equation (1) implies that the marginal cost of investing in more children's education depends on the level of $\bar{h}$ (i.e. $\frac{\partial H}{\partial n} = \bar{h}$), which creates a simultaneity problem.

Second, since n is likely to be jointly determined with H, it is likely to be correlated with unobserved factors in u that affect both n and H, thus creating an omitted-variable bias problem. Parental health is one example of such factors. Healthier parents might invest more money in their children's education. This is because they can spend more time helping with their children's homework, which might increase their spending on instructional materials, if time and monetary investments are compliments in education production. Healthier parents might also demand more children, because the cost of childrearing will be lower for them than for parents with health problems. When

[8] Conceptually, it is important to distinguish the number of school-aged children and the number of children of any age, as they will lead to different interpretations of β_1. But in the data used in this paper, the difference is very small.

parental health is not collected in the data (but is known to the parents), simple econometric techniques, e.g. ordinary least-squares (OLS), are likely to yield biased estimates of β_1 (and thus of $\varepsilon_{\bar{h},n}$).

To obtain consistent estimates of β_1, this essay estimates equation (3) jointly with the following first-stage (fertility) equation, using an instrumental variable (IV) method:

$$n = \alpha_0 + \mathbf{x}\boldsymbol{\alpha}_1 + \mathbf{z}\boldsymbol{\alpha}_2 + v \qquad (4)$$

where **z** is a set of excluded instrumental variables for *n*, which are uncorrelated with the error term *u* in (3), and are not outcomes of parental investments. This essay constructs the set of instrumental variables by exploring the temporary relaxation of China's population policy from the mid-1980s to the early 1990s, which allows for more children in a rural family once certain conditions are met. The major feature of this relaxation exploited by this essay is that it allows rural couples' son-preference to play a role in their fertility decisions.[9] In an area where most of the couples have at least two children, which is the case in the study area in this essay, son-preference implies that the sex-composition of the first two children in a family will have strong predictive power for family size. To the extent that the sex-composition of these children is not chosen by the parents, it can be used to construct the set of instrumental variables needed.[10]

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[9] When only one child was allowed and the one-child policy was strictly enforced, there is no variation in family size that can be used to identify β_1. Yet this does not mean that son-preference has no impact on rural women's fertility behavior. For example, the rigorous implementation of the one-child policy in the early 1980s had led some women, desperate for a son, to kill their newly-born daughters (Greenhalgh 1986), facing a risk of a prison sentence up to 13 years (Arnold and Liu 1986).

[10] A potential threat to this identification strategy is the prevalence of sex-selective abortions in China. See below for more discussions on this point.

Not only does the estimation of equation (4) provide an opportunity to identify β_1, it is itself of policy interest under the identification strategy that explores rural Chinese couples' son-preference (as opposed to other strategies exploring twin births, etc). Under this strategy, equation (4) provides information on how China's traditional values, i.e. son-preference, affect couples' fertility decisions. Such information is important in considering the effects of potential changes in population policy in China, and in other countries that share similar traditional values, especially the Confucian heritage, with China. Section 2.5 provides results from a variety of estimates of this equation.

The next two subsections briefly review the evolution of China's one-child policy and discuss the instrumental variables for family size in more detail, especially how to overcome the potential threats to the validity of these instrumental variables introduced by the prevalence of sex-selective abortions in rural China.

2.3.2. Institutional background: the one-child policy and its relaxation

China's one-child policy was announced in 1979 and officially established in September 1980 via the "Open Letter" issued by the Central Committee of the Chinese Communist Party, aiming to limit the size of China's booming population to be 1.2 billion by the year of 2000. The policy allowed each couple, urban or rural, only *one* child, which implies a very sharp drop in the total fertility rate (TFR) given that the TFRz in China was still above 3.5 in 1975. Although the policy was immediately controversial and encountered resistance from different levels of local administrations, its implementation was extraordinarily strict during its first five years (Greenhalgh 1986). Drastic measures, including mandatory intrauterine device (IUD) insertion for women with one child, abortion for unauthorized pregnancies, and sterilization for couples with two or more children, were widely used to control family size during this period (Greenhalgh 1986). By the mid-1980s, the national TFR had reached its lowest level since the founding of the People's Republic of China (Table 2-1).[11]

By 1984 domestic resistance and international controversy had led the central government to adopt a substantial relaxation in the one-child policy (Greenhalgh 1986; Hardee-Cleveland and Banister 1988). On April 13, 1984, the Communist Party Central Committee issued Central Document 7, which made two important changes in the policy. First, rather than forcing all communities (i.e. villages in rural areas and residential districts in urban areas) to adhere to a single state-derived policy, the central government placed greater emphasis on developing and implementing policies to suit local needs. This resulted in substantial variations in the content of the local policies and the strictness of the implementation of these policies across communities. Secondly, the relaxed policy allows Chinese couples, especially rural ones, to have a second child under certain conditions. The provincial policy that allows for a second child in Gansu, the study area of this essay, is presented in Table 2-2. While the conditions allowing for a second child vary across localities, most rural areas allow for a second child in a family if the first-born child is a girl.

[11] Not all researchers agree that the reduction in TFR was due to China's population policy. Some economists (e.g. Johnson 1994) argued that economic development has contributed greatly to the decline of China's fertility rates, by reducing the demand for children. Yet during the time period 1985-1991 when the one-child policy was temporary relaxed, rural China experienced the lowest economic growth rate (2.8%) since its rural reform in 1978 (Naughton 2007: pp. 211). Also, within a decade (1970-80) with no substantial economic growth, China's TFR was reduced by about 60%, when China was adopting a "Later (marriage)-Longer (birth spacing)-Fewer (children per family)" policy. One could, at best, argue that the reduction in TFR in the early 1980s was a continuation of the effects of the "Later-Longer-Fewer" policy, but not mainly due to economic growth.

The relaxation of the one-child policy was meant to "close up a large hole (i.e. to reduce third and higher-order births)" by "opening a small hole (i.e. to allow more couples to have a second child)" (Greenhalgh 1986). However, not only did the opening of the small hole keep the "large hole" open, but it also brought about a large number of out-of-plan second births in rural China. In 1986-87, 94.3% of rural women aged 15-39 who already had one child gave birth to a second child (Feeney et al. 1989), while the family planning program only allowed 1.62 children per woman on average at that time (Attané 2002). Meanwhile, 49.6% rural women who had two children went on to have a third child (Feeney et al. 1989), although a third child has never been allowed for Han Chinese couples since 1980. As a result, actual fertility exceeds substantially the State Family Planning Commission's projected fertility level in most provinces (Attané 2002); it is estimated that more than 40 percent of annual births were in breach of the plan in the 1980s (SFPC 1988; 1992). Consistent with these figures, Table 2-1 shows that despite its continuously declining trend from 1970 to 1985, the national TFR in the late 1980s rebounded to its level in 1980. More strikingly, the TFR in rural China reached 2.81 in the late 1980s, which was much higher than its level in 1980, when the one-child policy was first introduced. Probably due to the recognition of the rebounding fertility rates, the central government has tightened the enforcement of the family planning program nationwide since the early 1990s. Renewed attempts to curb population had been successful, at least in terms of crude birth rates, which declined from 23% in 1987 to 18% in 1993 (State Statistical Bureau 1994). No relaxation in policy occurred in the 1990s (Short and Zhai 1998).

2.3.3. Son-preference and instrumental variables for family size

The temporary relaxation of the one-child policy allowed rural couples' son-preference to play an important role in their fertility decisions. In fact, explicitly allowing a rural couple to have a second child if the first-born is a girl clearly reflects the government's consideration of rural couples' son-preference (Greenhalgh 1986). Rural Chinese couples' desire for a son implies that the sex-composition of the earlier-born children in a rural family has strong predictive power for its family size: a rural couple would have an incentive to continue childrearing until a son is born.[12] Since the majority of couples in rural China have two or three children during the second half of the 1980s, which is also the case in rural Gansu (see Table 2-3), the key instrumental variables used in this essay are constructed based on the sex-composition of the first *two* children in a family.[13] In this context, β_1 measures the impact of having a *third* child on parental investments in child education. Given the current official estimate of TFR of 1.8 children per childbearing age woman (the figure in 2009), β_1 has direct implications for the potential effect of a policy that allows for one more child per family.

However, the prevalence of sex-selective abortions against female fetuses in rural China imposes a potential threat to the validity of such instruments.[14] Sex-selective

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[12] Son-preference can take either the form of the desire for *one son* or the desire for *as many sons as possible*. However, using data from the One-Per-Thousand Fertility Survey conducted in 1982, Arnold and Liu (1986) showed that there is no evidence of a desire to have two or more sons, at least measured by contraception use. See Section 2.5 for more results on this hypothesis.

[13] Angrist and Evans (1998) adopt similar instrumental variables strategy in their study on the effects of family size on American women's labor supply behaviors. However, while they can use all sex combinations of the first two children as valid instrumental variables, not all sex combinations of the first two children can be used as valid instrumental variables in the context of this paper, due to the possibility of sex-selective abortion in rural China, as discussed further below.

[14] Since the inception of the one-child policy, sex-selective abortion has become a cost-saving strategy for rural couples in response to 1) financial punishments for out-of-plan births, and 2) the declined demand for children due to economic development triggered by China's rural reform since 1978. The combination of son-preference and the availability of prenatal sex-determination technology provided by ultrasound equipment has resulted in the prevalence of sex-selective abortions in rural China since the mid-1980s

abortion implies that the sex-composition of the children might be manipulated by their parents, and thus it might be correlated with unobserved factors that affect parental investments.

Fortunately, not all births were subject to sex-selective abortion. First, under the relaxed one-child policy, the first birth in a rural family is unlikely to be subject to sex-selective abortion. This can be seen in Table 2-4, which reports the pattern of the sex-ratio at birth (i.e. the number of male births per 100 female births) by birth order in Gansu, using the computer tabulation data from the 1990 Population Census of Gansu Province.[15] A close examination on the sex-ratio at birth for children born in 1989 and 1990 reveals that sex-selective abortions typically occurred at second or higher-order pregnancies, but *not* at the first pregnancy. The sex-ratios at first birth in the three six-month periods reported in the (computer-tabulated) Census Data (102.7 in the first half of 1989, 102.4 in the second half of 1989, and 104.3 at first half of 1990) are all within the normal range of sex-ratio at birth in the absence of sex-selective abortions (i.e. 102 to 105). In contrast, sex-ratios at birth jump dramatically at the second birth to over 117 and then increase gradually with birth order, suggesting the occurrence of sex-selective abortions at the second and higher-order pregnancies. That sex-selective abortions did not occur at the first pregnancy is not surprising: even if the first child is a girl, the couple still has a chance to have a son at the second birth without violating the population policy. Thus, one would expect the sex of the first child to be exogenously determined.

(Chu, 2001).

[15] Only the published computer tabulation data (Population Census of Gansu Province and Computer Centre of Gansu Province Statistical Bureau 1993), but not the original Census data, are available to the author.

Second, under the assumption that rural couples' son-preference takes the form of the desire for *one* son (as opposed to the desire for *many* sons), the second birth in a family *conditional on the first-born being a boy* (which is unlikely subject to sex-selective abortion, as shown above) is also unlikely to be subject to sex-selective abortion.[16] This can be seen in Table 2-5, which tabulates the number of the second-born boys and girls conditional on the sex of the first-born, using data collected in a random sample of rural households in Gansu Province (see the next section for more details about the data).[17] When the first-born is a boy, the incidence of the second-born being a girl is almost equal to that of the second-born being a boy, yielding a sex-ratio at birth of 102.4. In contrast, when the first-born is a girl, the incidence of a second-born being a girl is only about 67% of the incidence of the second-born being a boy, suggesting that sex-selective abortions of female fetuses occurred at the second birth when the first-born is a girl.

To sum up, conditioning on the first-born being a son, the sex-composition of the first two children generates two plausible instruments for family size: the indicator of the first-born being a boy and the second-born being a girl (Boy-girl) and the indicator of the first-born being a boy and the second-born being a boy (Boy-boy). Since these two instrumental variables sum up to the dummy variable for "the first child being a boy", the "baseline case" in regression is "the first child being a girl". Recall that the validity of these instruments hinges on the assumption that rural couples' son-preference takes the form of the desire for at least *one* son, this assumption will be examined in section 2.5.

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[16] If rural couples desire many sons, then even when the first-born is a boy, they might choose to abort female fetuses in higher-order parities.

[17] Unfortunately, the distribution of the sex of the second-born conditional on the sex of the first-born is not available in the published computer tabulation of the 1990 Gansu Population Census data.

Also, the validity of these instruments will be checked below using other instrument variables such as the indicators of twin births.

2.4. Study Area and Data

2.4.1. Study area

Gansu province, the study area of this essay, is a poor province located in northwestern China (see Figure 2-1 for its geographical location in China). Gansu consists of areas of flat Loess Plateau, Gobi desert, mountainous and hilly areas, and vast grasslands. In 2004, its population was 25.4 million, about three fourths of whom reside in rural areas. In that year, Gansu's rural per capita net disposable income ranked 30 out of 31 provinces, with only Guizhou province showing lower incomes (National Bureau of Statistics 2005).

By 1997, there had been three family planning regulations issued in Gansu. The first, "Regulation of Family Planning Trial Implementation", was issued in July 1979. Two revised versions were issued in November 1989 and September 1997, respectively. While under no circumstances is a third child allowed for Han Chinese couples by these regulations, under the second and third regimes, a second child is allowed if one or more conditions listed in Table 2-2 are met.

In retrospect, the implementation of the one-child policy encountered great difficulties in rural Gansu (Attané 2002). Gansu Province reported in 1986 that "Family planning work is in a backward state in one-third of our counties" (*China Daily*, February 2, 1988).[18] By 1993, Gansu's family planning rate was only 66.7% in rural Gansu,

[18] "Family size increasing in some provinces". *China Daily*, February 2, 1988: 3, cited in Hardee-

although it had reached 99% in cities.[19] The difficulty of implementing the one-child policy can also be seen in the fertility data. According to the State Family Planning Commission (SFPC), the actual fertility for women aged 25-29 was 2.36, while the completed fertility authorized by SFPC was 1.58. These two figures yield a ratio of 1.49 in Gansu, which was the second highest in China (Attané 2002).[20]

2.4.2. Data

The data used in this essay come from the Gansu Survey of Children and Families (GSCF). The survey collected information from 2,000 sample children, who were aged 9-12 in the year of 2000 in rural Gansu. A stratified sampling strategy was used to select 20 counties from all non-urban, non-Tibetan counties in the province. Within each of the 20 counties, 5 villages were then selected. Within each of the 100 sample villages, 20 children were then randomly selected from the full cohort of 9 to 12 year-old children. Separate questionnaires were administered to the sample children, their parents, and local village leaders, as well as to teachers and principals of the schools that the sample children were enrolled in at the time of the survey. Follow-up surveys were conducted in 2004, 2007 and 2009.

One concern regarding the GSCF survey methodology is that a random sample of children aged 9-12 in 2000 (as opposed to targeting a random sample of rural households), might lead to oversampling households with more children. Yet this concern is negligible, because only six percent of the targeted children (and thus their households)

Cleaveland and Banister (1988).

[19] http://www.gsjsw.gov.cn/html/gsrksj/15_52_20_527.html (accessed July 25, 2011)

[20] The highest one was Jiangxi province (1.55 = 2.36/1.52) and the third and fourth ones were Guangxi province (1.40 = 2.21/1.57) and Hubei province (1.39 = 2.16/1.55).

in the data are of fourth or higher birth order. Also, Table 2-6 shows that the birth patterns in the late 1980s found in the GSCF data (columns (3)-(4)) are very close to those found in the 1990 (computer tabulated) Population Census data (columns (1)-(2)).[21]

The primary data used in this essay are from the second wave of the survey (GSCF-II) conducted in 2004, because this wave collected detailed information on parental investments in children's education, and most of the children were still in secondary school. These data are supplemented with additional information collected in the first wave (GSCF-I) in 2000.[22]

2.4.3. Sample

In the empirical analysis below, the sample is restricted to include only households whose children were all of school age or younger (under age 18) at the time of survey. Coincidently, this sample includes only those households whose children were all born after 1984, when Document 7 was issued. In other words, all the children in this restricted sample were born during the period of the relaxed population policy. The resulting sample size is 1582 households. A few households are further excluded as follows. First, 68 households with parents who are members of an ethnic minority, mainly *Hui*, are excluded, because the one-child policy did not apply to ethnic minorities until the late 1980s.[23] Second, two outlier households, one with an extremely low household income

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[21] The provincial total in the Population Census data indicates slightly more (5 percentage points) first births and fewer second and third births than in the GSCF data. This is mainly due to the fact that the provincial total includes births in the cities, where the one-child policy was strictly enforced and thus the births that occurred in cities are mostly first births. In column (2) where the data are restricted to include only counties (excluding cities), the number of first births in 1989 is very similar to that found in the GSCF data, shown in columns (3) and (4).

[22] The panel data method using both GSCF-I and GSCF-II is not very helpful for the purpose of this paper. This is because between 2000 and 2004, only 8 children were born in these households. In other words, there is almost no variation in the number of children between 2000 and 2004.

[23] A household is coded as "ethnic minority" if the father or the mother of the targeted child is not Han

amount (measured as per capita expenditure) and the other with an extremely high income, were excluded. The resulting sample, called the “main sample”, includes 1512 households.

The empirical analysis below focuses on households that have at least two children, for three reasons. First, one-child families account for only 7% of the families in the main sample. Second, one-child families are likely to have been issued one-child certificates and thus have enjoyed considerable subsidies from their local governments (e.g. health care), which makes their children different from other children “at the starting line” (Arnold and Liu 1986).[24] Third, and most importantly, the sex-composition of the first two children cannot be used as an instrumental variable if one-child families are included. Among the remaining households who have at least two children, those who do not have any children enrolled in school are also excluded, since any trade-off between child quantity and quality is not observed for these households. The final sample includes 1373 households. Most of the empirical analysis below is based on this final sample; other samples are used for robustness checks as needed.

2.4.4. Summary statistics

The dependent variable of primary interest is the yearly total expenditure on investments in children’s education, whose components can be seen in Panel A of Table 2-7. On average, total parental investments in child education account for about 11% of

Chinese.

[24] One might think that these households may not have completed their fertility processes and thus have not received such benefits. But the GSCF data indicate that it has been at least thirteen years since the mothers in these families gave birth to their only children. The distribution of years since first birth for mothers who have only child is as follows: 13 years: 10 (8.93%); 14 years: 31 (27.68%); 15 years: 32 (28.57%); 16 years: 25 (22.32%); 17 years: 14 (12.25%).

total household expenditure, which imposes a sizable financial burden on rural families. The "fixed costs" of education, i.e. tuition and school fees, account for more than 50% of parental investments in child education, yet other investments exhibit greater variation than do tuition and fees, especially when aggregated at the household level (Panel B, Table 2-7). This suggests that the variations in parental investments are mainly driven by their "voluntary" investments, but not the "fixed costs" charged by rural schools.

The explanatory variable of primary interest is the number of children.[25] Table 2-3 shows that the majority (about 68%) of households interviewed in the GSCF survey have two or three children, which is consistent with the national and provincial patterns. One might expect educational investments to be very sensitive to the number of children in the family, since investments in children's education account for more than one tenth of total household expenditure. Yet the summary statistics reported in Panel C, Table 2-7 lend little support for this expectation. Instead, they show that while the educational investments per child decrease with family size, they are very similar for households with two to four children. Of course, this observation is only suggestive; more reliable results of the impacts of family size will be obtained in regressions that include a large set of other explanatory variables.

To avoid omitted variable biases and to increase the precision of the estimates, the regressions below include many other explanatory variables in addition to family size, including family background variables and child characteristic variables. The definitions and summary statistics of these variables can be found in Table 2-8.

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[25] A common issue in studies on the impacts of family size is that the observed family size may not equal completed family size. This issue is unlikely to cause serious problems here. This is because among the mothers included in the main sample, only 4.7% gave birth to a child within 7 years prior to year 2004. The official requirement for birth spacing is usually 4-5 years in rural China, so if these mothers desired for more children, many of them will have given birth to more children in the 7 years prior to 2004.

Finally, a note on the distinction between pre-natal son-preference and post-natal son-preference is in order. Parents' post-natal son-preference implies that parental investments in child education will depend on the distribution of child gender among the children. Among the 1866 mothers interviewed during the GSCF survey, 278 (15%) agreed that "It is useless to send daughters to school, since they will eventually marry out." Thus, variables characterizing the distribution of child gender (i.e. the same mean and variance of child sex for all children in the household) are included in the regressions. The identification condition of the causal impact of family size on parental investments, then, relies on the fact that the sex-composition of the first two children has strong predictive power for family size, even after controlling for the distribution of child sex. The results of testing such a condition are provided below.

2.5. Empirical Results

2.5.1. Son-preference and family size

The validity of the instrumental variables, Boy-boy and Boy-girl, constructed from child sex-composition relies on the assumption that rural Chinese couples' son-preference takes the form of the desire for at least one son, as opposed to the desire for many sons. Thus, it is worth examining the impacts of rural couples' son-preference on their fertility decisions under the relaxed one-child policy, before turning to the results of estimating the investment equation (3). This is done by estimating the fertility equation (4). Table 2-9 presents the results. Columns (1)-(2) include all the one-child families in the regressions, while columns (3)–(8) exclude them. Since the means and the variances of child characteristics at the household level are likely to be outcomes of the sex-composition of the earlier-born, they are excluded from all the regressions reported in Table 2-9.

Columns (1)-(4) estimate the impacts of the sex of the first child on family size. The results clearly reflect rural couples' desire for sons: on average, households whose first-borns are girls have about 0.3 more children than do those households whose first-borns are boys. These impacts are consistent across specifications: the estimated impacts in regressions without controlling for any family background variables (columns (1) and (3)) are very close to those in regressions with a large set of family background variables included (columns (2) and (4)). Also, as expected, the impacts of the first-born being a girl decline when the one-child families are excluded.

More interesting patterns of rural Chinese couples' son-preference are revealed in columns (5)-(8), where the households are divided into four sub-groups, based on the sex-composition of their first two children. Columns (5) and (6) indicate that the significant impacts of the first-born being a girl on family size, shown in columns (1)-(4), are almost entirely driven by the demand for children of households whose first two children are both girls. Having two girls in a row has a strong positive impact on family size: on average, these households demand 0.7 more children than the households whose first-born is a boy (i.e. the omitted category in columns (5) and (6)). In sharp contrast, having a boy at the second birth and a girl at the first birth has almost no impact on family size.

Columns (7) and (8) focus on households whose first-borns are boys, treating the households whose first-borns are girls as the omitted category. Two results are worth noting. First, having a boy at the first birth has a significantly negative impact on family size (as has been seen in columns (1)-(4)). Second, conditional on the first-born being a boy, the discrepancy in the impacts of the sex of the second-born is much smaller than that in the case where the first-born is a girl (columns (5) and (6)). These results clearly suggest that a rural couple will stop childbearing once a son is born, which is consistent with the desire for at least *one so*n, as opposed to the desire for *as many sons as possible*. This form of son-preference implies that Boy-boy and Boy-girl are valid instruments for family size.[26]

There are several explanations for this form of son-preference during the period of relaxed population policy. First, although sons are traditionally thought be responsible for providing old-age support for their parents in China, the demand for sons is not unlimited and it declines as the economy develops. One important reason for having many sons in traditional rural China is to guarantee the survival of at least one son; thus, when the survival rate of sons increased with the improvements in medical conditions in rural China, the demand for sons declined greatly (Arnold and Liu 1986). China's first ever marriage law, the Marriage Laws of 1982, mandates that daughters are also responsible for their parents' old-age support, which further reduces the demand for sons. In fact, among the 1866 mothers who filled out the "mother form" during the GSCF survey, only 908 (48.7%) agreed with the statement that "Parents should rely on their sons for old-age support", the rest of whom either chose "Don't agree" (515; 27.6%) or "No opinion" (443; 23.7%). In rural China today, once the old-age support for the parents is guaranteed by having a living son, a rural couple's demand for sons is usually met. Second, raising sons could be very costly, which prevents rural parents from having many sons. In fact, a

[26] Technically speaking, this is more restrictive than needed. Even if the parents desire many sons, these instruments will still be valid as long as they are not correlated with the error term in equation (4). Section 2.6 provides several checks of this requirement.

second son is considered to be a "heavy burden" in some rural areas, because a second son requires a new house at the time of his marriage, which may cost up to 10 years of annual household income (Greenhalgh et al. 1994).

2.5.2. Trade-off between child quantity and quality

Now turn to the main results of this essay. Table 2-10 reports main results of estimating equation (4), with different specifications. Columns (1)–(3) report results from OLS regressions and columns (4)-(6) report results from 2SLS regressions, using Boy-boy and Boy-girl as instruments for family size. Columns (1) and (4) include only a set of child characteristics in regression. Columns (2) and (5) add to the regressions a set of commonly-used family background variables, such as parental education, family income and landholding. Columns (3) and (6) include additional family background variables that not only potentially affect child investment but also are related to the conditions under which more children are allowed by the relaxed one-child policy (Table 2-2), to see if the policy has any direct effect on parental investments in child education (other than only through its effects on family size). These variables include parental age (related to condition (3)), indicators of handicapped parents (related to condition (6)), indicators of biological parents (related to conditions (2) and (4)), and indicators of children living with their grandparents from the mother side (related to condition (8)). In all regressions, village fixed effects are controlled for to account for differences in local prices, school quality and the local implementation of the one-child policy.

There are three important findings. First, OLS regressions are likely to *underestimate* the increase in parental investments in child education in response to an increase in family size, thereby *overestimating* the trade-off between child quantity and quality faced by the parents.[27] This can be seen in the comparison of the OLS results (columns (1)-(3) of Table 2-10) and the 2SLS results (columns (4)-(6) of Table 2-10). The OLS regressions estimate that in response to an additional child, the parents will increase their investments in their children's education by 27-31%. In contrast, the 2SLS regressions estimate a much larger increase (by 42-48%) in parental investments. Converted into elasticity measures (at the sample mean of family size, 2.2 children), the OLS regressions estimate a strong trade-off between child quantity and quality, ranged from -0.40 to -0.32, whereas the 2SLS results estimate almost no such trade-off (elasticity ranged from -0.09 to 0.06).

Second, the estimated impacts of the commonly-used family background and child characteristic variables are consistent with the common findings in the literature, and these impacts are very similar across specifications; this suggests that the family size effects reported above are not driven by the uniqueness of the GSCF data. Mother's education, but not father's education, has a significantly positive effect on parental investments. Household income also has significantly positive effect on parental investments. Since household income is measured as the log of per capital expenditure, the coefficient on it represents the elasticity of parental investments with respect to household income. The income elasticity estimated is around 0.30, suggesting that child quality is a normal good (as opposed to a luxury good), conditional on child quantity. Child characteristics, especially, the means and the dispersions of the children's completed years of schooling and their age, significantly affect parental investments, reflecting the effects of child-spacing and cohort effects on parental investments.

[27] Similar results are also found in Lee (2008) who analyzes data from South Korea.

Third, many of the conditions specified in the provincial population policy (Table 2-2) allowing for more children are likely to have direct effects on parental investments. For example, households with biological mothers tend to invest 30% more money in child education than do households with non-biological mothers. Mother's age and the indicator of handicapped mothers are also very close to being significant. This finding suggests that these policy-related variables cannot be used to construct additional instruments for family size, as they violate the exclusion restriction. Thus, to facilitate over-identification tests for the validity of Boy-boy and Boy-girl as instruments for family size, one needs to find another source of exogenous variations in family size.

2.6. Robustness Checks

The results in Table 2-10 indicate that parents in rural Gansu do not face a strong trade-off between child quantity and quality, at least in terms of monetary investments in children's education. Given the current fertility rate of 1.8 in China, this suggests that allowing for one more child per family would not lead to substantial reductions in the average level of parental investments in children. Yet, how reliable is this estimate of the impact of family size on investments in education? This section conducts several robustness checks to answer this question. The results are reported in Table 2-11.

2.6.1. Twin births as instruments

Although the key instruments for family size, Boy-boy and Boy-girl, pass Hansen's over-identification tests in columns (4)-(6) in Table 2-10, it is still possible that they are (jointly) invalid instruments. For example, some might argue that even the first birth could be subject to sex-selective abortions (recall that the indicators Boy-boy and Boy-girl add up to the indicator of the first-born being a boy) and are correlated with the error in equation (4), although Tables 2-4 and 2-5 present evidence against such a possibility.[28] When instrumental variables other than Boy-boy and Boy-girl are available, this possibility can be checked via an over-identification test that uses the additional instrumental variables.

A plausible test can be conducted using a commonly-used source of exogenous variation in family size, namely, the occurrence of twin births.[29] Twins are identified in the data when there are two children in the same household that have the same birth years and birth months. Columns (1) and (2) in Table 2-11 use indicators of twin births as instruments for family size. Since births in high-order birth parities depend on parents' fertility decisions, they might be correlated with unobserved factors determining parental investments. Thus, only twin births in the first two parities are counted as twin births here. Column (1) uses the indicator of twin births in the first two parities as the only excluded instrument variable for family size, which estimates an 41% increase in parental investments in response to an additional child ($\varepsilon_{\bar{h},n}$ = -0.1). Since the indicator of twin births is highly significant in the first-stage regression (F-stat = 30.30), it is used along with Boy-boy and Boy-girl in column (2) to facilitate an over-identification test. The results in this column provide no evidence against the validity of Boy-boy and Boy-girl as excluded instruments for family size: Hansen's over-identification test yields a J-statistic of 0.29, with a corresponding p-value equal to 0.86. A more convincing test is

[28] Even if the first birth is subject to sex-selective abortion, these instruments will still be valid if they are not correlated with the error term in equation (4), i.e. if parents' prenatal son-preference is not correlated with unobserved factors that affect their post-natal investment behaviors.
[29] This paper does not focus on twin births, because twin births account for a very small proportion (78/4599=1.7%) of the total number of child births in the data used in this paper.

conducted in column (3), where the indicator of twin births in the *first* parity is used along with Boy-boy and Boy-girl as instruments for family size. The results in column (3) are very close to the results in columns (1)-(2).

2.6.2. Sample attrition

The above tests find no evidence against the validity of Boy-boy and Boy-girl as excluded instruments for family size. But in order to use these instrumental variables, one has to exclude all the one-child families from the analysis. Although one-child families account for less than 10 percent of the families in rural Gansu, it is unclear whether the exclusion of the one-child families affects the results found in Table 2-10. As a check, column (4) Table 2-11 estimates equation (3) including these one-child families, using the indicator of the first-born being a girl as the instrument for family size. As mentioned above, this instrument has limited predictive power for family size ($F = 6.59 < 10$, the rule of thumb value) once the distribution of child characteristics, especially the mean and the sample variance of the distribution of child sex, are controlled for. Notice, however, that the distribution of child sex is never significant in the regression results reported in Table 2-10, so dropping these variables is unlikely to lead to a nonzero correlation between the instrument and the error term. As can be seen in Table 2-11, column (4), this regression yields a result that is very close to that in Table 2-10, column (6).

2.6.3. Sampling methodology

Finally, there is a concern regarding the sampling methodology of the GSCF survey. Recall that the GSCF families were drawn based on a random sample of 2000 children aged 9-12 in 2000. Since the number of children depends on parental fertility decisions, such a sampling strategy (i.e. targeting children rather than households) will likely result in a non-random sample when the targeted children are of high birth orders. There are 98 families (6%) whose targeted children are of third or higher order, and 600 families (38%) whose targeted children are second-born. To see how this sampling methodology affects regression results, Table 2-11, column (5), estimates equation (4) where the sample is limited to children who are the first-borns in their families. Table 2-11, column (6), repeats the regression in column (5) in Table 2-11, but using the indicator of twin births in the first two parities as the instrument. The results in these two columns are very similar to those reported in Tables 2-10 and 2-11.

To sum up, the results reported in columns (4)-(6) of Table 2-10 as well as the validity of excluded instruments for family size used in these columns, Boy-boy and Boy-girl, have passed a series of robustness checks using additional instruments and different samples. More importantly, the estimated impacts of family size remain almost unchanged across the different sets of instruments for family size. Two points should be noted when interpreting these results. First, the 2SLS results can be interpreted as the impacts of family size on the households that are affected by the instruments used in the first-stage, i.e. the "local average treatment effects" (LATE) of the instruments. For example, the results using Boy-boy and Boy-girl as instruments measure the LATE for the households whose family sizes are larger because their first-born are girls; the results using twin births as instruments measure the LATE for the households whose family sizes are larger because they have twins. Second, the identification using twin births as instruments estimates the "treatment effect on the non-treated", because there are no “never-takers” in response to twin births (Angrist and Pischke. 2009, pp. 171). Thus, based on these interpretations, the similar impacts of family size found using different sets of instruments suggest that family size has a *homogenous* impact on parental investments in rural Gansu. This also justifies the over-identification tests using different sets of instruments that presumably affect different households (Angrist and Pischke 2009: pp.167).

2.7. Discussion: Family Size Impact versus Birth Order Impact

The results reported in Table 2-10 have passed a series of robustness checks, yet it may be premature to draw policy recommendations based solely on these results. Even if rural parents do not reduce their investments in their children’s education in response to a change in family size, they are still likely to reallocate their resources among their children, implying a birth order effect associated with a change in family size. This section reports the results of estimating birth order effects, using several variants of the following specification:

$$\ln h_{ik} = \gamma_0 + n_k\gamma_1 + bo_{ik}\gamma_2 + \mathbf{x}_k\boldsymbol{\gamma}_3 + \mathrm{u}_{ik} \qquad (5)$$

where h_{ik} is the educational investments received by the *i*th child in the *k*th family, bo_{ik} is this child’s birth order, and n_k is the family size of the *k*th family, which is instrumented using Boy-boy and Boy-girl.[30] Since there is no means to perfectly control for

[30] Note here that since the dependent variable is $\ln(h_i)$, $\gamma_1 = \partial\ln(h_i)/\partial n = \varepsilon_{hi,n} / n$, which is different from β_1 in equation (3) .

"possible" endogeneity in birth order, the results reported below are only suggestive and should be interpreted with caution.

Table 2-12, columns (1)-(6), reports the results. All regressions exclude children from families with twins, as it is difficult to assign birth order to twins. Column (1) estimates equation (6) using data on all individual children; it shows that family size has little impact on parental investments received by individual children, which is consistent with the findings reported above. It also shows that compared to the first-born children, later born children receive more investments, although the differences are not significant. Columns (2) and (3) repeat the exercise using only the first- and second-born children, respectively, to see if the impacts of family size differ for children with different birth orders. Again, no significant impacts of family size are found. But the coefficients on family size seem to suggest that parents might reallocate resources away from the first-born to the second-born.

To obtain better estimates of the birth order effects, columns (4) and (5) control for family fixed effects, eliminating the influences of all unobserved factors at the household level.[31] Column (4) reveals that the second-born children receive significantly more parental investments than the first-born. Since the dummy for girls is also significant, column (5) includes a set of interaction terms between child gender and each birth order. None of the interaction terms are significant. Nor do they change the impact of being the second born and its significance.

For many households, children of higher birth order are likely to be the last-born, so it is likely that parents reallocate educational investments in favor of the *last*-born not

[31] Even after controlling for family fixed effects, birth order could be correlated with unobserved factors such as genetic endowments that are not observed by the econometricians but more or less observed by the parents.

necessarily the *second*-born. Column (6) investigates this possibility and it finds that, the last-born children receive much more educational investments than do the earlier-born. Columns (7) and (8), excluding five-child and then four-child families, yield similar results, whish suggests that the impact of being the last-born is not driven by a small number of large families.

In short, the results reported in Table 2-12 suggest that while parents do not reduce the average level of investments in their children's education in response to an exogenous change in family size, they do tend to reallocate monetary resources toward the last-born.[32]

2.8. Concluding Remarks

In response to the many unintended demographic problems caused by China's one-child policy, such as the "missing girls" problem, many researchers have recently proposed to relax this policy, by allowing more children for each family. Yet, the theory of the trade-off between child quantity and quality predicts that such relaxation might cause substantial reduction in investments in the human capital of later generations. This essay empirically tests this hypothesis using data collected in rural China. To obtain exogenous variations in family size, this essay explores rural Chinese couples' son-preference, which played an important role in their fertility decisions in a period when China's one-child policy was temporarily relaxed, to create instrumental variables for family size. More specifically, the sex-composition of the first two children in a family is used to construct such instruments.

[32] A regression not reported here, which includes a dummy for the last-born son, finds no additional significant impact of being the last-born son over the impact of the last-born child. In fact, last-born daughters are slightly more favored by the parents, although the impact is not statistically significant.

The most important finding is that an exogenous increase in family size, in particular, from two to three, will increase parental investments in children's education by 40-50%, which suggests that rural parents do *not* face a strong quantity-quality trade-off of children. This finding is robust across a wide variety of specifications and different instruments used, including the indicators of twin births. In contrast, conventional least-squares methods, which might suffer from simultaneity and omitted variables problems, yield a much stronger trade-off.

Focusing on parents' behaviors, the findings in this chapter may help reconcile some puzzles in the literature. Previous empirical studies of the quantity-quality trade-off of children fail to reach a consensus, even when comparable methods are used. For example, although both exploited twin experiments in analyzing large scale data sets, Black, Devereux and Salvanes (2005) and Li, Zhang and Zhu (2008) find very different results. Using data on the entire population of Norway, Black et al. find that family size has little impact on children's educational attainment. In contrast, Li, Zhang, and Zhu, who also used a twins experiment, find a strong negative impact of family size using China's Census Data. In their attempt to reconcile this contrast, Li, Zhang and Zhu cited the gap in the levels of economic development between Norway and China as an explanation for finding different results. They argued that parents in a less-developed country might face a stronger quantity-quality trade-off of children. Yet the present essay finds that even in one of the poorest areas in China, parents do not reduce their investments in children's education in response to an increase in family size, and thus parental behaviors are unlikely the explanation for finding different impacts of family size.

It is thus likely that the observed negative impact of family size found in China is due to factors that are not under parents' control. The evolution of the education system is one such factor. In fact, China's formal education system was disrupted for a decade during the Chinese Cultural Revolution in 1966-1976. Due to the coincidence between the establishment of the one-child policy and the launch of economic reform in the late 1970s, the 1980s witnessed both declining fertility rates and increasing educational attainment for China's population. The opposite directions of the trends in fertility rates and educational attainments in the reform era might explain the negative impact of family size found in Li, Zhang and Zhu (2008), who focus on children aged 6~17 in 1990 (i.e. born in 1973~1984 and reached school age in the 1980s).

Finally, a note on the policy implication from the findings in this essay is in order. One might be tempted to propose relaxation of the one-child policy, as it might help mitigate many unintended demographic problems but will not cause reductions in the average monetary investments in children's education. However, relaxing the one-child policy might induce a reallocation of financial resources among living children, especially in favor of the youngest child in a rural family, as suggested by the birth order effects found in this essay. The costs and the benefits of such reallocation of resources certainly demand more research and better data, which, at a minimum, should include detailed information on the returns to parental investments in children's education.

Figure 2-1: Geographical Location of Gansu Province in China

Table 2-1: Total Fertility Rates (TFR) in China and Gansu Province: 1970-90

Year		1970	1975	1980	1985	1990
			A: China			
Total:	(a)	5.75	3.58	2.32	2.12	2.31
	(b)	--	3.80	2.54	2.27	--
	(c)	--	3.57	2.24	--	--
Urban:	(a)	3.22	1.76	1.20	1.27	1.47[a]
	(b)	--	2.12	1.52	1.23	--
	(c)	--	1.78	1.15	--	--
Rural:	(a)	6.31	3.97	2.56	2.36	2.81[a]
			B: Gansu Province			
Total		6.71	3.53	2.56	--	2.34
Urban:		3.81	1.49	1.07	--	1.45
Rural:		7.45	4.00	2.92	--	2.57

Sources: China (Panel A): (a): Attané (2002); (b): 1987 survey (Feeney et al. 1989); (c): 1982 survey (Feeney et al. 1989). Gansu (Panel B): (1)1970-1980: Su (1988), pp. 98. (2) 1990: Wu et al. (1994).
Note. a: data in 1988.

Table 2-2: Conditions for Having a Second Child in Gansu Province and Related Variables in Empirical Analysis

Conditions
(1) First child medically diagnosed as handicapped
(2) Peasants with one daughter
(3) Previously medically determined to be infertile and is pregnant after age 30[a] after adopting a child. This condition is changed to be "after age 35" in the 1997 regulation.
(4) Remarried couple, one party had only one child and the other had no children.
(5) Returned overseas Chinese or residents of Hong Kong, Macau, and Taiwan
(6) One member of the couple is handicapped.
(7) Minority couple, including one member of the couple belonging to an officially recognized minority population group.
(8) Rural male marries a rural female with no male siblings and lives with the parents-in-law
(9) The household lives in remote and mountainous areas

Source: Gu et al. (2007), and the official website of the Family Planning Commission of Gansu: http://www.gsjsw.gov.cn

Table 2-3: Distribution of Rural Families by Family Size in Gansu Province

Sample	(1) All households		(2) Main sample		(3) Main sample, Han Chinese only	
1 child	117	(6.10%)	115	(7.27%)	106	(7.00%)
2 children	1,190	(62.04%)	1,071	(67.70%)	1,030	(68.03%)
3 children	490	(25.55%)	337	(21.30%)	320	(21.14%)
4 children	95	(4.95%)	47	(2.97%)	46	(3.04%)
5+ children	26	(1.30%)	12	(0.76%)	12	(0.80%)
Total	1,918	(100%)	1,582	(100%)	1,514	(100%)

Sources: author's tabulation, using GSCF data.

Table 2-4: Sex Ratio at Birth by Birth Order in Gansu: January 1, 1989-June 30, 1990

Number of births	Boys	Girls	Sex ratio at birth
Birth order	A: Jan. 1 - June 30, 1989		
1st	51,856	50,505	102.7
2nd	45,354	38,553	117.6
3rd	22,356	18,908	118.2
4th	6,873	5,386	127.6
5th +	3,753	2,751	136.4
	B: July 1 - Dec. 31, 1989		
1st	65,812	64,241	102.4
2nd	49,639	42,428	117.0
3rd	23,550	20,182	116.7
4th	6,858	5,511	124.4
5th +	3,495	2,515	139.0
	C: Jan. 1 - June 30, 1990		
1st	47,890	45,910	104.3
2nd	38,741	30,721	126.1
3rd	19,046	15,493	122.9
4th	6,057	4,459	135.8
5th +	3,199	2,431	131.6

Source: Author's tabulation, using published computer tabulation data from the 1990 Population Census of Gansu Province (Population Census of Gansu Province, and Computer Centre of Gansu Province Statistical Bureau. (eds). 1993).

Table 2-5: Distribution of the Sex of the Second-born Conditional on the Sex of the First-born

	1st Child = Girl	1st Child = Boy
2nd Child = Boy	476	339
2nd Child = Girl	321	331
Sex-ratio of 2nd child	148.3	102.4

Source: Author's tabulation, using GSCF data.

Table 2-6: Distribution of Families by Parities of Births in 1988-90 in Rural Gansu

Sample	(1) Province total (1989)		(2) County total (1989)		(3) GSCF (1989)		(4) GSCF (1988-1990)	
1st Birth	232,416	(44.81%)	173,511	(39.79 %)	314	(40.46%)	870	(41.47%)
2nd Birth	175,950	(33.17%)	152,951	(35.07%)	320	(41.24%)	862	(41.09%)
3rd Birth	84,967	(16.02%)	76,505	(17.54%)	123	(15.85%)	304	(14.49%)
4th Birth	24,829	(4.68%)	22,154	(5.08%)	16	(2.06%)	53	(2.53%)
5th + Birth	12,298	(2.32%)	10,985	(6.33%)	3	(0.39%)	9	(0.43%)
Total	530,460	(100%)	436,106	(100%)	776	(100%)	2,098	(100%)

Source: Author's tabulation using the 1990 Population Census of Gansu Province (columns 1-2) and the GSCF data.
Note. The Population Census data (columns (1) and (2)) include ethnic minority groups, while GSCF data (columns (3) and (4)) exclude all ethnic minority families, which explains why the percentage of families with higher order (i.e. 3rd +) births in the first two columns is higher than that in the last two columns.

Table 2-7: Summary Statistics of Monetary Investments on Children's Education

Educational investments (Unit: RMB *Yuan*[a])	Mean	Std. dev.	Min.	Max.
	A. Individual level		(N = 4,335)	
Total investment	402.68	781.20	0	12,500
Tuition and fees	217.48	459.66	0	9,500
School supplies	41.46	80.18	0	2,700
Transportation	25.44	95.47	0	2,000
Dining	66.91	205.44	0	3,000
Tutor	10.01	52.55	0	2,000
Uniform	26.35	31.91	0	500
Other	13.65	53.25	0	1,000
	B. Household level		(N = 1,549)	
Total investment	878.85	1188.84	0	16,400
Tuition and fees	470.03	665.02	0	9,920
School supplies	94.90	135.91	0	3,000
Transportation	52.17	137.96	0	2,000
Dining	148.20	368.38	0	7,000
Tutor	20.86	93.58	0	3,000
Uniform	62.90	58.97	0	600
Other	29.78	101.35	0	1,500
	C. Household level, by family size			
Investment per child				
1 child	641.53	1078.01	0	6,922
2 children	399.36	474.34	0	6,025
3 children	384.37	554.01	0	5,467
4 children	395.88	620.21	0	3,385
≥ 5 children	111.70	51.63	51.67	220

Note. a:1 U.S. *Dollar* = 8.27 *Yuan* in 2004

Table 2-8: Definitions and Summary Statistics of Key Variables; Main Sample (N=1512)

Variables	(1) Definition	(2) Mean	(3) Std. Dev.	(4) Min.	(5) Max
Dependent variables					
ln(*H*)	Log (monetary investments[a] in child education in the 2nd semester of the 2003-04 academic year). Unit: *yuan* [b].	6.404	0.866	3.555	9.705
Explanatory variables					
n	Number of school-aged (6-18) children in 2004.	2.23	0.654	1	6
FaEdu	Father's completed years of schooling.	6.579	2.978	0	14
MoEdu	Mother's completed years of schooling.	4.154	3.015	0	12
FaAge	Father's age in years.	40.2	3.906	30	62
MoAge	Mother's age in years.	38	3.35	29	68
HHinc	Log (per capita expenditure excluding educational investment); unit: *yuan*.	7.073	0.683	4.995	9.911
Land	Per capita landholding; unit: *mu*[c].				
FaBio	Dummy, =1 if the father is a biological father.	0.988	0.109	0	1
MoBio	Dummy, =1 if the mother is a biological mother.	0.984	0.127	0	1
FaHand	Dummy, =1 if the father is handicapped.	0.033	0.180	0	1
MoHand	Dummy, =1 if the mother is handicapped.	0.033	0.180	0	1
Grandparents	Dummy, =1 if living with grandparents from the mother's side.	0.012	0.112	0	1
Mean(ChEdu)	Sample mean of children's completed years of schooling (one year before the survey) in the household.	5.943	1.748	0.5	11
Var(ChEdu)	Sample variance of children's completed years of schooling (one year before the survey) in the household.	2.193	2.808	0	25
Mean(ChAge)	Sample mean of child age in years.	14.454	1.983	7.5	18
Var(ChAge)	Sample variance of child age in years.	3.510	4.903	0	58.37
Mean(Girl)	Sample mean of the indicator of a child being a girl.	0.456	0.288	0	1
Var(Girl)	Sample variance of the indicator of a child being a girl.	0.165	0.112	0	0.5

Note. a: the sum of all items displayed in Table 2-7.
b: 1 US dollar = 8.27 *yuan* in 2004.
c: 1 *mu* = 1/15 hectare.

Table 2-9: OLS Results of the Impacts of Son-preference and Family Background on Family Size

Sample	(1) (n ≥1)	(2) (n ≥1)	(3) (n ≥2)	(4) (n ≥2)	(5) (n ≥2)	(6) (n ≥2)	(7) (n ≥2)	(8) (n ≥2)
First born = Girl	0.364***	0.339***	0.299***	0.270***				
	(0.036)	(0.034)	(0.033)	(0.032)				
Girl-girl					0.721***	0.675***		
					(0.056)	(0.055)		
Girl-boy					0.031	0.020		
					(0.027)	(0.028)		
Boy-boy							-0.362***	-0.321***
							(0.043)	(0.039)
Boy-girl							-0.236***	-0.219***
							(0.034)	(0.032)
HHinc		-0.117***		-0.152***		-0.121***		-0.150***
		(0.029)		(0.028)		(0.026)		(0.028)
Land		-0.072***		-0.070***		-0.046**		-0.071***
		(0.022)		(0.021)		(0.018)		(0.021)
FaEdu		0.016***		0.011**		0.007		0.011*
		(0.006)		(0.005)		(0.005)		(0.005)
MoEdu		-0.011*		-0.005		-0.002		-0.005
		(0.006)		(0.006)		(0.005)		(0.006)
Other controls	N	Y	N	Y	N	Y	N	Y
Village FE	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.27	0.30	0.25	0.29	0.41	0.44	0.25	0.29
N	1512	1511	1408	1405	1407	1404	1407	1404

Note. 1. Other controls include FaAge, MoAge, FaBio, MoBio, FaHand, MoHand and Grandparents (see Table 2-8 for definitions). 2.Robust standard errors clustered at the village level are reported. 3.*** significant at 1% level; ** significant at 5% level; * significant at 1% level.

Table 2-10: Impacts of Family Size on Parental Investments on Children's Education (Sample: n≥2)

Specification	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Family size	0.268***	0.309***	0.310***	0.416***	0.481***	0.470***
	(0.036)	(0.037)	(0.037)	(0.136)	(0.143)	(0.137)
FaEdu		0.007	0.008		0.005	0.005
		(0.008)	(0.008)		(0.008)	(0.008)
MoEdu		0.015**	0.015**		0.015**	0.015**
		(0.006)	(0.006)		(0.006)	(0.006)
HHinc		0.272***	0.274***		0.298***	0.298***
		(0.036)	(0.036)		(0.042)	(0.042)
Land		-0.005	-0.006		0.005	0.004
		(0.013)	(0.013)		(0.015)	(0.015)
FaAge			0.005			0.003
			(0.007)			(0.006)
MoAge			-0.011			-0.012*
			(0.007)			(0.007)
FaHand			-0.037			0.105
			(0.081)			(0.081)
MoHand			0.130			0.005
			(0.105)			(0.084)
FaBio			-0.213			0.028
			(0.135)			(0.170)
MoBio			0.342**			0.169
			(0.135)			(0.130)
Grandparents			-0.058			-0.070
			(0.164)			(0.161)
Mean(ChEdu)	0.395***	0.354***	0.355***	0.401***	0.358***	0.358***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.022)	(0.023)
Var(ChEdu)	0.076***	0.071***	0.072***	0.073***	0.068***	0.069***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Mean(ChAge)	-0.112***	-0.099***	-0.094***	-0.121***	-0.109***	-0.101***
	(0.017)	(0.018)	(0.019)	(0.019)	(0.019)	(0.020)
Var(ChAge)	-0.016**	-0.015**	-0.015**	-0.020**	-0.020**	-0.019**
	(0.008)	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)
Mean(Girl)	0.009	0.044	0.055	-0.087	-0.059	-0.044
	(0.061)	(0.055)	(0.055)	(0.104)	(0.097)	(0.094)
Var(Girl)	0.016	0.027	0.038	0.048	0.059	0.060
	(0.179)	(0.172)	(0.169)	(0.174)	(0.169)	(0.165)

(Continued on next page)

Table 2-10 (Continued):

Specification	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Excluded instruments				Boy-boy Boy-girl	Boy-boy Boy-girl	Boy-boy Boy-girl
Partial R-squared of excluded instruments				0.054	0.049	0.052
F-stat of joint sig. of excluded instruments				35.82***	32.46***	34.50***
Hansen's J-stat (p-value)				0.003 (0.956)	0.267 (0.605)	0.244 (0.622)
Imputed $\hat{\varepsilon}_{\bar{h},n}$	-0.406	-0.320	-0.318	-0.085	0.058	0.034
N	1,373	1,370	1,370	1,373	1,370	1,370
R-squared	0.588	0.617	0.620	n.a.	n.a.	n.a.

Note. 1.Standard errors are clustered at the village level. 2. *** significant at 1% level; ** significant at 5% level; * significant at 1% level.

Table 2-11: 2SLS Results of the Impacts of Family Size on Parental Investments on Children's Education, Using Different Instruments and Samples

Sample	(1) ($n \geq 1$)	(2) ($n \geq 2$)	(3) ($n \geq 2$)	(4) ($n \geq 1$)	(5) ($n \geq 2$); 1[target child = 1st born]	(6) ($n \geq 2$); 1[target child = 1st born]
Instruments	Twins in first two parities	Boy-boy Boy-girl Twins in first two parities	Boy-boy Boy-girl Twin birth in 1st parity	1[First-born = Girl]	Boy-boy Boy-girl	Twins in first two parities
Family size	0.408*** (0.146)	0.449*** (0.116)	0. 469*** (0.133)	0.466*** (0.108)	0.417** (0.163)	0.500** (0.193)
F-stat of excluded instruments for *n*	30.30***	29.34***	23.59***	100.37***	18.08***	28.25***
Hansen's J-stat (P-value)	n.a.	0.290 (0.8649)	0.434 (0.8048)	n.a.	1.472 (0.2250)	n.a.
N	1,465	1,370	1,370	1,465	732	827
Imputed $\hat{\varepsilon}_{h,n}$	-0.10	-0.01	0.03	0.03	-0.08	0.10

*** significant at 1% level; ** significant at 5% level; * significant at 1% level.

Table 2-12: Impacts of Birth Order on Parental Investments on Children's Education: Individual Level Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	all	1st born	2nd born	all	all	all	$n \leq 4$	$n \leq 3$
Family size	0.015	-0.071	0.102					
	(0.514)	(0.562)	(0.656)					
1st born						0.028	0.016	-0.022
						(0.090)	(0.093)	(0.096)
2nd born	0.190			0.299***	0.233***			
	(0.414)			(0.072)	(0.093)			
3rd born	0.063			0.166*	0.026			
	(0.888)			(0.089)	(0.117)			
4th born	0.046			0.197	0.243			
	(1.434)			(0.195)	(0.191)			
5th + born	0.718			0.542	0.315			
	(1.865)			(0.392)	(0.513)			
Last born						0.527***	0.527***	0.498***
						(0.078)	(0.081)	(0.085)
Girl	-0.263***	-0.193	-0.311***	-0.285***	0.684 [a]	-0.189***	-0.176***	-0.168***
	(0.093)	(0.161)	(0.104)	(0.064)	(0.606)	(0.066)	(0.067)	(0.068)
Girl×birth order	N	N	N	N	Y	N	N	N
Household FE	N	N	N	Y	Y	Y	Y	Y
Observations	3,047	1,323	1,323	3,151	3,151	3,151	3,090	2,938

Note: 1. All regressions exclude families with twins. 2. All regressions include child age, age-squared, sex and completed years of schooling, and the full set of family background variables. All family level variables, including family size, drop out when family fixed effects are controlled for in columns (4)-(6). 3. Columns (1)-(3) control for village fixed effects.
4. *** significant at 1% level; ** significant at 5% level; * significant at 1% level. 5. Standard errors are clustered at the family level.

Chapter 3. Parental Education and Child Academic Skills in Rural China[33]

3.1. Introduction

Education is widely viewed as a key determinant of individual and household incomes in developing countries (Hanushek and Woessmann 2008). Estimates of the economic returns to investments in the quantity of education, measured by years of schooling, for developing countries are usually higher than those found in developed countries. Although considerably less studied than the quantity of education, the *quality* of education (or acquired academic skills), usually measured by achievement test scores, has also been found to have significant impacts on individual and household incomes in developing countries (e.g. Glewwe 1996; Jollife 1998). In many developing countries, such as China, where one's educational attainment depends crucially on the results of competitive exams, the quality of education is itself a key determinant of the quantity of education one might acquire. Only students who have acquired sufficient academic skills can pass these examinations and thus advance to the next level of education. Given the importance of academic skills in developing countries, increasing efforts from academia and policy circles have been devoted to estimating the determinants of academic skills.

The role parental education plays in the acquisition of child academic skills receives much attention in the literature on the intergenerational transmission of human capital. This is not only because a robust, positive correlation between parents'

[33] An earlier version of this essay was a selected paper presented at the 2009 AAEA & ACCI Joint Annual Meeting in Milwaukee, WI, under the title "Family Background, Ability and Student Achievement in Rural China".

educational attainment (e.g. years of education and degree completed) and their children's educational outcomes (e.g. school enrollment and test scores) has been documented in the literature, but also because parental education is itself a variable subject to policy interventions. For example, a number of developing countries have adopted policies and intervention programs that aim to increase women's years of education. The rationale behind these interventions is that they will improve not only the well-being of the targeted generation – the future better-educated mothers – but also the well-being of the next generation – the children of these better-educated mothers. However, the effectiveness of such interventions, at least in terms of their impacts on children's educational outcomes, relies crucially on whether the correlation between parental education and child educational outcomes is *causal*.

The causal effect of parental education on child academic skills is of particular interest in the case of rural China. A salient feature of the contemporary Chinese economy is the great urban-rural divide in income and educational attainment (Knight and Li 1996). Rural parents usually have a much lower level of education than their urban counterparts; meanwhile, rural children are usually outperformed by urban children in high school and college entrance exams, which may in turn widen the urban-rural income gap in the future. If parental education does have a significant causal impact on child academic skills, boosting rural residents' educational attainment may be an effective way to shrink the great urban-rural divide. Unfortunately, the empirical knowledge needed for such a policy recommendation is largely nonexistent, as little has been done to understand the causal impact of parental education on student learning in rural China. Although Chapter 2 of this dissertation finds that parental education, especially mother's, has a significant impact on parental investments in child education, it is unclear whether such an impact translates into a significant improvement in child academic skills.

This essay attempts to fill this void by analyzing a rich data set recently collected by the Gansu Survey of Children and Families.[34] This data set allows this essay to control for unobserved child innate cognitive ability (referred to as child ability below) when estimating the impact of parental education on child academic skills. Child innate cognitive ability, or more accurately the genetic endowment related to cognitive development, is likely to be correlated with parental education. This is because (1) children's and their parents' genetic endowments are presumably correlated due to gene transmission and (2) parents' educational attainment are partly determined by their genetic endowment. Without controlling for child ability, the impact of parental education estimated using simple regressions could be spurious: it may simply capture the impact of unobserved child ability on child academic skills.

Since an error-ridden measure of child ability, scores on a cognitive ability test (*CAT*), is available in the data, this essay adopts the instrumental variable (IV) method proposed by Blackburn and Neumark (1992), namely the multiple indicator method, as the primary empirical method. Unlike the standard IV method, which leaves child ability in the error term and then finds IVs for *all* explanatory variables that are potentially correlated with child ability (not only parental education but also other family background variables), the multiple indicator method includes the error-ridden indicator of the child ability (*CAT* scores) in the model and then finds IVs only for this indicator. In this sense, the multiple indicator method substantially reduces the burden of finding IVs.

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[34] See Chapter 2 and the Data section below for more details about this data set.

The error-ridden nature of the ability measure (*CAT* scores) calls for a source of exogenous variation in child ability (genetic endowment), but not in the error in its measure, to deal with the measurement error problem. To exploit such a source of exogenous variation, information generated from the Great Chinese Famine (1958-61) is used to create IVs for the error-ridden indicator. The rationale is that the information on parents who survived the famine (and how long they endured the famine) during their early childhood is likely to be correlated with their genetic endowments (e.g. better endowed parents were more likely to survive the famine during early childhood) and thus correlated with their children's genetic endowment via gene transmission.

This essay finds that parental education has a statistically significant impact on child academic skills, even after controlling for child ability. Yet the impacts of father's education and mother's education differ for boys and girls: whereas father's education has a significant effect for boys and girls, mother's education has a significant impact only for girls. Given a gender gap in math skills also found in this essay, the differential impacts of father's and mother's education imply that low level of mothers' education (mothers' in rural China have completed significantly fewer years of education than rural fathers) can widen the gender gap in education for the next generation. Given these findings, boosting rural residents' education, especially women's, is likely to reduce the gender gap in education in rural China and may eventually shrink the great urban-rural divide in educational attainment in contemporary China.

The rest of the essay proceeds as follows. The next section provides a conceptual framework for thinking about how parental education affects child academic skills. Section 3.3 discusses potential identification problems, followed by two sections that develop identification strategies to deal with these problems. Section 3.6 describes the data. Section 3.7 reports and discusses empirical findings. The final section concludes.

3.2. Conceptual Framework

Parental education can affect child academic skills in several different ways. Parental education can *directly* affect child academic skills, serving as one *input* to produce child academic skills. For example, parental education is directly transferred into child academic skills when parents use what they learned in school to teach their children. Parental education can also affects child academic skills *indirectly* through its impacts on other educational inputs such as tutoring services and home computers. Thus, there are several different relationships between parental education and child academic skills that are of academic and policy interest, capturing different pathways by which parental education affects child academic skills (Glewwe and Kremer 2006). Different sets of explanatory variables, and perhaps different estimation methods, may be needed to estimate these different relationships.

3.2.1. Production relationship

Technologically speaking, one can think of child academic skills as being produced according to a well-behaved (e.g. differentiable and concave) *production* function:

$$H = H^{P}(\mathbf{I}; \mathbf{K}, \mathbf{F}, \mathbf{Q}), \qquad (1)$$

In equation (1), H stands for the academic skills acquired by a child, measured by his or her achievement test scores at some point in time; superscript P denotes $H^P(\cdot)$ as a *production* function. The vector $\mathbf{I} = (I_1, I_2, \ldots, I_J)$ is a set of educational inputs I_j , j = 1, 2,… J, such as time spent in school, tutoring services and home computers; **I** may also include past inputs and achievements. The vector **K** is a set of child characteristics such as gender, age and (genetically determined) cognitive ability. The vector **F** is a set of family background characteristics, the most important of which for this study is parental education. The vector **Q** is a set of school characteristics such as teacher quality, the quality of school facilities, and pedagogical methods. While all variables in vector **I** can be directly chosen by the parents, many (if not all) variables in **K**, **F** and **Q** are not directly under parental control – they are exogenously determined.[35] These exogenous variables can be considered as "fixed inputs" in education production. If all variables in **I**, **K**, **F** and **Q** are observable, accurately measured, and available in the data, ordinary least-squares (OLS) regressions consistently estimate the *direct* effects of these variables on child academic skills. However, since no survey can collect information on all relevant

[35] School quality **Q** could be endogenous if households had the freedom to choose among all the schools that were available to them (e.g. Glewwe and Jacoby 1994). In rural China, however, two policies greatly reduce this possibility. First, the current school enrollment policy restricts enrollment exclusively to schools within each residential locality. Among the sampled children in the data analyzed in this paper, over 95% percent of the children in basic education go to the nearest school. Second, although in theory it is still possible that the entire household moves to a location with high-quality schools, this possibility is very unlikely due to rural China's land tenure policies. These policies imply that moving the entire household to another location has the risk that this household will lose its land altogether, thus forfeiting a stream of future land income. Under the Household Responsibility System (*HRS*), farmland is owned by villages/collectives but not households. Households may farm their assigned land, but are not allowed to sell it. If a household stops farming, it must return its land to the village (Yang 1997). One might also argue that since a rural household can move to an urban area to enjoy higher income, giving up its land entirely might be an optimal decision. However, even when the entire household moves to an urban area, it's almost impossible for its child to enjoy better education. This is because the household registration (*Hukou)* system prevents children with rural *Hukou* from entering urban schools which are only open to urban residents who are entitled urban *Hukou* (Wu and Treiman 2004). Finally, a relatively easier (if possible) way is to send the child to live with relatives living in an area where school quality is higher. Yet in the data used in this paper, almost all children in basic education (grades 1-9) live in the households where their parents reside.

variables in education production, there is always a possibility for omitted variable biases.

3.2.2. Demand relationship

Alternatively, one can derive a *demand* relationship between parental education and child academic skills by solving a household's optimization problem. Consider a household that maximizes the following (quasi-concave) utility function:

$$\underset{C,\mathbf{I}}{Max}\; U = U(C,\, H)\,, \qquad (2)$$

where C is the composite household consumption (i.e. the numeraire), and H is the academic skills of the child in the household. When solving problem (2), the household faces two constraints: the technology constraint (1) and the budget constraint (3):

$$C \;+\; \mathbf{I'P} \;=\; M\;, \qquad (3)$$

where **P** is the price vector for educational inputs **I**, and M is household income.

Solving problem (2) subject to constraints (1) and (3) yields a set of demand functions:

$$C \;=\; C\;(\mathbf{P},\, M;\, \mathbf{K},\; \mathbf{F}, \mathbf{Q})\,, \qquad (4)$$

$$I_j \;=\; I_j(\mathbf{P},\, M;\, \mathbf{K},\; \mathbf{F}, \mathbf{Q})\,, j = 1, 2, \ldots \text{J}. \qquad (5)$$

Equation (4) is the demand function for household consumption, and equation (5) is the demand function for the *j*-th educational input. Substituting (4) and (5) into equation (1) yields the demand function (or the value function) for child academic skills:

$$H = H^{P}(\mathbf{I}(\mathbf{P},\ M;\ \mathbf{K},\ \mathbf{F},\mathbf{Q}));\mathbf{K},\ \mathbf{F},\mathbf{Q}). \qquad (6a)$$

Since each element in **I** is a function of the set of exogenous variables (**P**, M, **K**, **F**, **Q**), equation (6a) can be expressed as:

$$H = H^{D}\ (\mathbf{P},\ \mathrm{M}, \mathbf{K},\ \mathbf{F},\mathbf{Q}), \qquad (6b)$$

where the superscript *D* denotes $H^{D}(\bullet)$ as a *demand* function, a function of only exogenous variables. Since the right-hand sides of (6a) and (6b) are equal at the household equilibrium, equating them and taking total differentiation of both sides of the resulting equation with respect to a variable in (**P**, M, **K**, **F**, **Q**), one can derive the *total* impact of this variable. For example, the total impact of mother's education (*MoEdu*) a variable in **F** is

$$\frac{\partial H^{D}}{\partial(MoEdu)} = \frac{\partial H^{P}}{\partial(MoEdu)} + \sum_{i} \frac{\partial H^{P}}{\partial I_{j}} \frac{\partial I_{j}}{\partial(MoEdu)}.$$

The first term on the right-hand side is the direct impact of mother's education on child academic skills and the second term is its indirect impact through its impact on educational inputs I_j; these two terms sum to the total effect of mother's education.

This essay opts to estimate the impact of parental education in the demand relationship (6b), for two reasons. First, the demand equation is probably more relevant for policy analysis (Blau 1999), as it accounts for parents' *behavioral* adjustments to **I** through the optimization process, in response to exogenous changes in $(\mathbf{P}, \text{M}; \mathbf{K}, \mathbf{F}, \mathbf{Q})$. In some cases, parental education has only indirect impacts, but no direct impacts, on child academic skills – for example, when children have completed more years of education than their parents – these indirect impacts can be captured only in the demand function. Second, the data requirement for estimating a demand function is probably less demanding than that for estimating a production function (1), because all inputs **I**, observed and unobserved, have been substituted out in the demand equation. Yet, consistently estimating the demand function equation (6b) still needs to overcome a number of identification issues, as discussed in the next section.

3.3. Identification Issues

Linearizing equation (6b) yields the following statistical model of the demand for child academic skills:

$$H = \mathbf{X}'\boldsymbol{\beta} + \alpha \cdot \text{G} + \varepsilon, \qquad (7)$$

where $\mathbf{X} = (\mathbf{P}, M; \mathbf{K}, \mathbf{F}, \mathbf{Q})$ is the set of exogenous variables other than child ability, G (genetic endowment), which is separately expressed in (7) for ease of discussion on identification strategy below. The error term ε includes all unobserved factors that potentially affect *H*. These unobserved factors may cause identification problems when estimating (7). This section discusses these problems, and the next section discusses the empirical strategy to deal with them.

3.3.1. Unobserved child ability

Child ability can never be directly observed. When G is unobserved, it becomes part of the composite error term ($\alpha \cdot G + \varepsilon$). Leaving child ability in the error term may lead to biased estimates of the impacts of parental education (as well as other parental human capital variables) because parental education is presumably positively correlated with child ability. Such a correlation arises through the link between parents' and their children's genetic endowments: better-endowed parents are likely to be better-educated and they will are also likely to have better-endowed children. Without appropriately controlling for child ability in estimation, parental education might pick up some impact of child ability on child academic skills.

3.3.2. Unobserved school characteristics

Like child ability, some school characteristics (**Q**) cannot be observed. Although often treated as inputs in education production, school characteristics cannot be substituted out in equation (6b), because they are usually not under parental control. Since no survey can collect all school characteristics, it is always possible that some relevant school characteristics are left in the error term.[36] It has been recognized that the omission of relevant school characteristics may bias the estimated impacts of observed

[36] Unfortunately, the literature on education production function is inconclusive about which school characteristics are relevant (Hanushek 1995, 2003; Glewwe 2002)

school characteristics (e.g. Glewwe et al. 2004).[37] More importantly in the context of this essay, the omission of relevant school characteristics may lead to biased estimates of the impacts of parental education. For example, if parents and their children went to the same school whose quality varies little over time, their educational attainment might both be affected by the quality of this school.

3.4. Identification Strategy

Given the focus of this essay placed on the impact of parental education, but not the impact of school characteristics, one simple yet effective way to control for unobserved school characteristics is to control for school fixed effects. This also effectively controls for the impacts of all other variables (e.g. prices **P**) that vary at the school level. This section is then devoted to the discussion on empirical methods for controlling for unobserved child ability.

Ideally, one can eliminate the correlation between parental education and child ability by randomly assigning different years of education to a sample of parents when they are of school age, track them over time, administrate tests to their children, and then compare their children's test scores. No doubt, such an experiment would be extremely expensive and time consuming, if not impossible to implement. In most cases, including this essay, one can rely only on observational data to assess the impact of parental education. Thus, the rest of this section briefly reviews methods that can be used to control for unobserved ability using observational data, in order of increasing applicability and plausibility based on available data.

[37] By comparing prospective estimates and retrospective estimates using data in Kenya, Glewwe et al (2004) find evidence of omitted school characteristic bias.

3.4.1. Natural experiments of adoptees

Although the ideal experiment seems impractical, the idea of breaking the genetic link between parental education and child ability is conceptually sound. The "natural experiments" methods try to mimic the ideal experiment. The first kind of natural experiment explores data on adoptees, whose genes are in theory not correlated with those of their adoptive parents. Using adoptees data from Rwanda, de Walque (2009) finds that adoptive parents' education, especially mothers', has a significant impact on adoptees' schooling outcomes. While this method best mimics the ideal experiment, it is not free of problems. First, parental education and child ability can still be correlated if the adoption process involves some sort of selection – for example, better-educated parents may adopt better-endowed (e.g. taller) children. This problem may be more serious in developing countries, where functional adoption systems have not been fully developed. Second, adoptive parents and biological parents might have very different motivations for child education. For example, adoptive parents may invest more in their adopted children, if these parents opted for adoption precisely because they were infertile but desperately desired children.

3.4.2. Instrumenting parental education

The second method, mostly adopted in the health literature, exploits such natural experiments as school construction programs – which boosted parent's education – to create IVs for parental education (e.g. Currie and Moretti 2003, and Chou et al. 2009). This approach effectively breaks the link between parental education and child ability, if the newly-constructed schools admitted parents not based on their genetic endowment.

Yet it still has a potential problem, which may lead to violation of the exclusion restriction for IVs: if parents and their children attended schools in the same school district, school construction programs will have direct impacts on children's educational outcomes.

Good data sets that contain information on natural experiments rarely exist. A more commonly available, yet perhaps less convincing, set of IVs for parental education comes from parents' family background, such as their parents' education and whether a father is the oldest son (e.g. Sander 1995; Leigh and Dhir 1997; Park and Kang 2008). While these variables usually explain the variation in parental education reasonably well, they are also likely to be correlated with parents' genetic endowment, and thus their children's genetic endowment. On a more positive note, since these IVs are positively correlated with parental education, the IV estimates provide an upper bound of the true impact of parental education.

3.4.3. Proxy variable method

Unlike the above methods which leave child ability in the error term, other methods include a measure of child ability (if available) in the regression model. The first and most commonly used one is the proxy variable approach, which replaces the child ability (G) in equation (7) with some measure of it, such as scores on cognitive ability tests (*CAT*) of some kind. Kingdon (1996), for example, uses scores on the Raven's Colored Progressive Matrices test as a proxy variable for child ability in her study on student achievement in India. Including a proxy for child ability certainly mitigates the omitted variable problem to some extent. However, it creates a new problem: any ability measures might reflect the influence of environmental factors (American Psychological Association 1995), and thus proxy variables based on *CAT* scores are usually error-ridden. In the worst-case scenario, including an error-ridden variable may contaminate the estimated effects of *all* explanatory variables (Wooldridge 2010: pp.80).

To illustrate, suppose that *CAT* scores measure true ability (G) with some error (*error*) in the population:

$$CAT = G + error,$$

which implies that the relationship $G = CAT - error$ holds in the population. Inserting this relationship into equation (7), the equation being estimated is, then:

$$\begin{aligned} H &= \mathbf{X'\beta} + \alpha \cdot G + \varepsilon \qquad (7a) \\ &= \mathbf{X'\beta} + \alpha \cdot CAT + (-\alpha \cdot error + \varepsilon), \end{aligned}$$

The minimum assumption needed for the proxy variable method to be valid is Cov(*CAT*, *error*) = 0 (Wooldridge 2010: pp.80), that is, the measure error in *CAT* is not correlated with *CAT*, the ability measure. This is unlikely to hold true, since it is the error that drives the ability measure to differ from the true ability. Note that Cov(*CAT*, *error*) = 0 implies that Cov(*G*, *error*) ≠ 0, meaning that the true ability is correlated with the error in the ability measure, which is also unlikely to be true.

When Cov(*CAT*, *error*) = 0 fails to hold, Cov(*CAT*, *error*) ≠ 0 follows. The correlation between *CAT* and *error* may contaminate the estimated effects of *all* explanatory variables (Wooldridge 2010: pp.80). In the best-case scenario, where all explanatory variables in equation (7) except *CAT* are uncorrelated with *G*, the only problem is the attenuation bias in the estimated coefficient of *CAT* (the coefficients on other variables will still be consistent). Unfortunately, in the context of this essay, the key explanatory variables in **X**, parental education, are presumably correlated with *G*, suggesting that *error* in *CAT* is likely to cause problems in estimation.

3.4.4. Indicators method

The error-ridden nature of ability measures calls for IV solutions. Blackburn and Neumark (1992) proposed the multiple indicator method: when two ability measures ("indicators" in Blackburn and Neumark's term), e.g. scores of two cognitive ability tests, $CAT_1 = G + error_1$ and $CAT_2 = G + error_2$, are available, one can use one indicator as an IV for the other.[38] Unlike the proxy variable method, the multiple indicator method allows the ability measure (indicator) to be correlated with the measurement error in it, i.e. Cov (CAT_k, $error_k$) $\neq 0$, k = 1, 2, which seems plausible. The assumption needed is that Cov ($error_1$, $error_2$) = 0, which will be violated, for example, if both *error*'s capture the influence of children's test-taking skills.

Alternatively, one can use a set of other IVs (rather than another indicator) that are highly correlated with *G* (after partialling out **X**) but not with *error* in *CAT*, to instrument *CAT*. If such IVs exist, they are probably better IVs than another indicator, because then one is less concerned with the possibility that the two indicators pick up children's test-taking skills. The study by Glewwe and Jacoby (1994) on student

[38] In their application of the multiple-indicator approach to their study on the returns to education, Blackburn and Neumark (1992) use two *indicators* for innate ability, namely an IQ test score and the score of the knowledge of the working world (KWW) test, with one serving as the IV for the other.

achievement in Ghana adopts such a method. The authors use parents' Raven's scores, as well as other exogenous variables, to instrument children's Raven's scores, the ability measure (indicator) in their study. One threat to their approach is, however, the possibility that both parents' and their children's Raven's scores pick up the influence of some within- family factors that are not part of their cognitive ability.

This essay constructs the set of IVs for *CAT* scores by exploring the Great Chinese Famine that occurred in 1958-61, which introduced an exogenous shock to the distributions of parents' genetic endowment and thus their children's (*G*). Since such a shock is unlikely to be correlated with measurement errors (*error*) in children's *CAT* scores, it can be used to create plausible IVs for *CAT* scores. The famine and the set of famine-generated IVs are discussed in more detail in the next section.

Clearly, the applicability of the methods discussed above depends on data availability. Unfortunately, methods exploiting natural experiments are not applicable given the data available for this essay. But the availability of an ability measure, scores of a cognitive ability test (see Section V for more details about this test), enables one to apply a number of methods discussed above, including the proxy variable method and the multiple indicator method. In addition, the availability of information on parents' family background allows one to instrument parents' education using parents' family background variables. While all applicable methods will be used in data analysis below, the multiple indicator method with one indicator and a set of other IVs is the preferred method. This method is preferred, because given the available data, it serves as a refinement of both the proxy variable method and the multiple indicator method with two indicators.

3.5. Great Chinese Famine, 1958-61, and Famine-Generated IVs

The preferred method demands a set of IVs for the *CAT* scores. The Great Chinese Famine, which occurred in 1958-1961, generated an exogenous shock to the distributions of parental and child genetic endowment, and thus provides candidate IVs needed for estimation.

3.5.1. Famine

The Great Chinese Famine followed immediately after the Great Lead Forward (GLF) agricultural crisis in 1958.[39] A bumper harvest was reported in 1958, when the national grain output reached its peak of 200 million tons. However, in 1959 grain production dropped below the reference level in 1956-57 in 21 out of the 28 provinces in mainland China with available data; the reduction was more than 10% in 12 provinces. Grain production continued declining over the next several years: national grain output declined by 12% in 1959, 26% in 1960, and 24% in 1961, relative to the 1956-57 reference level. China's grain production recovered slowly starting in 1962, but not until 1965 did grain output return to the pre-GLF level (Peng 1987).

In response to the sharp decrease in agricultural output, food allocation policies established during this period gave urban grain supplies higher priority over rural grain supplies. Despite the decline in grain output, the procurement rate was raised. Procurement was extremely heavy in 1959, although it was eased up slightly in 1960 (Peng 1987). Heavy procurement deprived the rural population of access to food grains, exacerbating the original shock of output decline in the rural areas (Peng 1987; Becker

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[39] Detailed analysis of the causes of the agricultural crisis and the famine can be found in Peng (1987), Lin (1990), Becker (1996), Lin and Yang (1998), Kung and Lin (2003), and DiKötter (2010), among others.

1996; Lin and Yang 2000). As a result of the GLF agricultural crisis, and of the subsequent political decisions regarding interregional food allocation, famine occurred throughout the country in 1958-61. A sharp increase in the national crude death rates (CDR) and a sharp decrease in crude birth rates (CBR) during 1958-61 are clearly shown in Panel (A) of Figure 3-1 (adopted from Banister 1987). It has been estimated that the Chinese Great Famine claimed some 30 million excess deaths and about 30 million lost births (Ashton et al. 1984, Banister 1987, and Peng 1987). It has now been recognized as the most devastating famine in human history (Dikötter 2010).

Gansu Province, the study area of this essay, was seriously affected by the famine. The time series patterns of CBR and CDR in Gansu province, shown in Panel (B) of Figure 3-1, mimor the national patterns shown in Panel (A). Worse yet, among 28 provinces in mainland China with available data, Gansu experienced the sharpest reduction in grain output in 1959, and it was the second earliest province (next to Sichuan Province) to encounter mortality crisis (Peng 1987).[40] Despite the food shortage in Gansu, the administration extracted 361 thousand tons of grain from the province to support the national urban food supply between 1959 and 1960 (Walker 1984). At the same time, the residence registration (*Hukou*) system established in 1958 was rigorously implemented to prevent rural residents from migrating to urban areas where the food shortage was not as dire. As a consequence, death rates increased dramatically in Gansu from 11.1 per 1,000 people in 1957 (the reference year) to 21.1 per 1,000 people in 1958 and then peaked at 41.3 per 1,000 people in 1960. The death rate in Gansu in 1958 was

[40] This is perhaps because Zhang Zhongliang, the political leader in Gansu in the late 1950s, was one of the most zealous supporters of the Great Leap Forward movement (Dikötter 2010).

the third highest among all 28 provinces with available data (Lin 1990). Fertility loss was also high in Gansu (Peng 1987).[41]

3.5.2. Demographic shocks of the famine

The dramatic demographic shock from the Chinese Great Famine had important consequences. One is the reduced cohort size for people who were born during the famine years. This implies that a set of children of same age that are randomly selected from the population, the number of parents born during the famine years (and then survived to have had children) would be smaller than the number in the counterfactual state had the famine not occurred. Figure 3-2 visualizes such an implication, displaying the birth year distributions for mothers' (panel (A)) and fathers' (panel (B)) of 2000 children (born in 1987-90) randomly selected in rural Gansu, during the Gansu Survey of Children and Families (GSCF). The estimated Kernel (dashed curves) and Normal (solid curves) densities are imposed for visual comparison. An apparent discrepancy between the Kernel density and the Normal density exists for both fathers' and mothers' birth year distributions, which occurs at years between the late 1950s and the early 1960s, corresponding to the famine years.

Yet, does such a discrepancy indeed reveal the demographic shocks of the famine? Two questions need to be answered before one can conclude that it does. First, are the patterns of parents' birth year distributions shown in Figure 3-2 unique for Gansu, thus picking up some Gansu fixed effects, or are they consistent with the patterns found

[41] Peng (1987) defined "a period of fertility crisis" as any year in which the total fertility rates for all women up to 39 years old was more than 15% below the average value of the mid-1950s, and "total fertility loss" as the sum of the significant (more than 15%) annual percentage shortfalls during the years of crisis. Peng estimated that the total fertility loss was 129.7 percent in 1959-61.

elsewhere in China? Given that the famine was a nationwide disaster, the patterns found in Figure 3-2 should be similar to those found elsewhere in China. Second, even if the patterns found in Gansu are similar to those found in the rest of China, are they similar to the patterns one would find when few parents experienced the famine? In other words, does the apparent discrepancy between the Kernel and Normal densities in Figure 3-2 indeed indicate demographic shocks brought about by the famine?

Figure 3 answers the first question. Panels (A) and (C) of Figure 3-3 plot the birth year distributions for the parents of a sample of rural children born in 1987-90 in eight Chinese provinces other than Gansu (Liaoning, Jiangsu, Shandong, Henan, Hebei, Hunan, Guangxi, and Guizhou), whose information was collected by the China Health and Nutrition Survey (CHNS).[42] These eight provinces were chosen to represent regions with different locations, cultural backgrounds and economic conditions in China, so that together they provide an overall picture of China. One can see again in these panels a clear discrepancy between the Kernel and Norman densities for the years between the late 1950s and the early 1960s, mirroring the patterns shown in Figure 3-2 for rural Gansu. Such a similarity suggests that the birth year distributions for parents in rural Gansu mimic the overall pattern in rural China. If Figure 3-3 captures demographic shocks of the famine in rural China, Figure 3-2 should also capture such shocks in rural Gansu.

Yet, rephrasing the second question above slightly: did the discrepancy between the Kernel and Normal densities that occurred around the famine years found in Figures 3-2 and 3-3 (Panels (A) and (C)) a result of the famine? If not, one would expect to see a similar pattern for a sample of children whose parents were born far later than the famine

[42] The CHNS data have a larger coverage of geographical areas and larger sample size than the GSCF data. However, the variables of primary interest such as test scores and ability measures are not available in the CHNS data. For this reason, the analysis of this paper is mainly focused on the GSCF data.

years.[43] Panels (B) and (D) of Figure 3-3, which display the parents' birth year distributions for rural children born in 2003-06 in the eight CHNS provinces, provide an answer to this question. Since these children are much younger than those born in 1987-90, almost all of their parents are too young to have been born in the years between the late 1950s and the early 1960s.[44] Thus, this sample of children serves as a good comparison group for children born in 1987-90, in terms of their parents' birth year distributions. As can be seen in Panels (B) and (D), parents' birth year distributions for rural children born in 2003-06 are very close to Normal distributions: the estimated Kernel density of the birth year distributions follows closely the Normal density in these panels. This pattern suggests that the discrepancy between the Normal and Kernel densities found in Figure 3-2 and Panels (A) and (C) of Figure 3-3 indeed capture the demographic shocks of the famine.

3.5.3. Famine-generated IVs

Given the demographic shocks of the famine, it is likely that the cohorts of parents born during the famine (the "famine-born cohorts") are very different from the rest of the population in rural Gansu. In particular, because the famine may have "selected" out people with better genes to survive, the famine-born cohorts may have better genetic *endowment* than those who did not survive the famine (including those who

[43] People who were born much earlier than the famine are not a good comparison group because the years prior to the famine years were in a time period full of wars, including the Japanese invasion (1937-45) and the civil war in China (1945-49), which also claimed millions of victims.
[44] For example, if a mother were born in 1960, she would have been 43 in 2003 and 46 in 2006, which are close to the upper limit of women's childbearing age.

were even conceived because of the famine) and those who were born long before or after the famine.[45] [46]

Since a subset of parents of the sample GSCF children belong to the famine-born cohorts, their birth year information can be used to create IVs for the *CAT* scores, the indicator of child ability. Recall that the *CAT* scores is the sum of the true child ability and its measurement error: $CAT = G + error$. If the famine affected parents' genetic endowment, whether a parent was born during the famine is likely to be correlated with his or her children's ability endowment *G*, but not correlated with the measurement error in *CAT*. Therefore, the famine, combined with parents' birth year information, generates plausible IVs for *CAT*. Specifically, the "famine-generated IVs" consist of a set of birth year dummy variables for parents born in 1958-60 – these parents lived their entire first year of life, the most vulnerable period in a child's development, during the famine. Also included as IVs for *CAT* is the provincial CDR in a parent's birth year, which measures the risk of death when a parent was born.

The key identifying assumption when using the famine-generated IVs is that, conditioning on **X** variables in (7), the famine affected child academic skills only through its effects on *G*, children's genetic endowment. In other words, conditioning on **X** variables, the famine should have no direct impacts on child academic skills, which is the

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[45] The parents of the famine-born cohorts might also have higher ability and thus have more resources to help the famine-born cohorts survive the famine.

[46] In addition to the selection effect, the famine can also have a somewhat offsetting effect on the famine-born parents' ability through poor prenatal nutritional intake. Recent studies on the long-term impacts of the Chinese famine (e.g. Chen and Zhou 2007) indicate that people born during the famine period have significantly lower heights than they would otherwise have had, which implies low nutrition intakes during the famine. Note however that there is no contradiction between the argument on the "selection" effects and that in Chen et al. (2007). The famine not only could select people with better endowment, but also could affect their lives badly afterwards. The idea here is that their high endowment could have been masked by the long term negative impact of famine. Yet their genetic endowment, which is presumably higher than average, would still be transmitted to their children. The IV methodology here takes advantage of such a transmission mechanism.

standard exclusion restriction. While it is difficult to imagine how the famine can directly affect child academic skills, if some **X** variables affected by the famine are omitted in regression, the famine-generated IVs are likely to pick up impacts of these variables, thus violating the exclusion restriction. The Great Chinese Famine has been found to have a wide range of impacts on survivors' human capital outcomes, including their health status, education and income (e.g. Chen and Zhou 2007). It might also affect parents' motivation for their children's education. These variables may all have a direct impact on child academic skills. Thus, in order for the famine-generated IVs to be valid, the empirical analysis below includes a large set of explanatory variables in each regression.

3.5.4. Performance of famine-generated IVs

The famine-generated IVs perform reasonably well in the first-stage regression, as can be seen in Table 3-A1, Columns (1)-(4), in the Appendix to Chapter 3. First, consistent with the hypothesis of the "selection effect" of famine, having a parent, especially a mother, born in the famine and then survived has a significantly positive impact on a child's *CAT* score. Second, these IVs, jointly significant at the 0.000 level, have strong predictive power for *CAT* scores, thereby eliminates the concern that they are weak IVs. Third, including birth year dummies for parents born in 1957 (prior to the famine) and 1961(when the famine started to fade out) as a simple falsification test (Table 3-A1, columns (3) and (4)) has little impact on the coefficients on the famine-generated IVs and their standard errors. In fact, the coefficients on the 1957 and 1961 birth year dummies are slightly negative, further supporting the hypothesis of the selection effect of famine. Finally, coefficients on the famine-generated IVs and their standard errors remain almost unchanged with or without including a large set of family background and child characteristics (columns (2) and (4)). This suggests that the famine-generated IVs are not correlated with these explanatory variables, which supports the assumption that these IVs are indeed exogenous.

Finally, it should be pointed out that the famine is not the *only* source of exogenous variation in *CAT* (through *G*), but it does provide *one* source of exogenous variation in *CAT*, as required by the standard IV method. Different sets of IVs may exploit different sources of variation in *CAT*, thereby yielding different impacts of child ability. But as long as the IVs can purge out the correlation between *CAT* and *error*, one would still obtain consistent estimates of the impact of parental education, the variable of primary interest in this essay. Which part of *G* is being exploited has little bearing on the estimated impact of parental education.

3.6. Data

3.6.1. Data and sample

The data analyzed in this essay come from the Gansu Survey of Children and Families (GSCF). The survey focuses on a sample of 2000 children, who were aged 9-12 in year 2000 in Gansu, a poor province in Northwestern China.[47] Follow-up surveys were conducted in 2004, 2007, and 2009. A stratified sampling strategy was used in 2000 to select 20 counties from all non-urban, non-Tibetan counties in the province. Within each of the 20 counties, 5 villages were selected. Within each of the 100 sample villages, 20

[47] Gansu province is a typical province in northwestern China. It consists of areas of flat Loess Plateau, Gobi desert, mountainous and hilly areas, and vast grasslands. In 2004, its population was 25.4 million, about three fourths of whom reside in rural areas. In that year, its rural per capita net disposable income ranked 30 out of 31 provinces in mainland China, with only Guizhou province having a lower income (National Bureau of Statistics 2005).

children were then randomly selected from the full cohort of 9 to 12 year-old children. Separate questionnaires were administered to the sample children, their parents, village leaders, as well as teachers and principals of schools that the GSCF sample children were enrolled at the time of survey. This essay primarily uses data from the second wave of the survey (GSCF-II), which was collected in 2004; these data are supplemented with additional information collected in the first wave (GSCF-I).

This analysis focuses on the subsample of children who were enrolled in grades 1-9, the basic education stage in China, in 2004. The main reason to exclude upper-secondary school students is due to data constraints: the achievement tests administrated to them, which measure their academic skills, were designed only for students in the basic education. Students who already attended upper-secondary school (i.e. grades 10-12) took the tests designed for grade 9 students, which is likely to introduce a substantial amount of measurement errors in their test scores. In fact, only 4.5% of the sample children attended high school in year 2004; results including these students (not shown below) are very similar to those excluding them.

3.6.2. Variables

Section 2 of this chapter outlines the sets of variables that should be included in the demand function for child academic skills. This subsection describes these variables in more detail. Table 3-1 provides definitions and summary statistics of the variables used in the analysis.

(a) Child academic skills. The dependent variables of interest, child academic skills (*H*), are measured by scores on math and Chinese (reading) tests administrated to the sampled children in 2004. The tests were designed by experts at the Gansu Educational Commission to cover the range of official primary and low-secondary school curricula. To ensure that the tests assessed an appropriate range of knowledge given the child's level of education, separate exams were given to children attending different grades.[48] The test scores were then adjusted to fit a common scale to allow for comparisons among all sampled children. In 2004, any sampled children that were interviewed in 2000 but had dropped out of school by 2004 were given the tests that corresponded to the highest grade they had ever attained. The inclusion of these children helps reduce sample selection biases due to dropping out of school. Finally, for ease of interpretation, test scores are standardized to have mean zero and standard deviation of unity in the analysis.

(b) Family background variables. The explanatory variables of primary interest are father's and mother's education, measured by their years of schooling.[49] Other family background variables (**F**) include parents' occupation, health status (as measured by whether they are handicapped), age in years and relationship type (whether they are the biological parents of the sample children), as well as household income (M) and per capita landholding. Household income is measured by yearly per capita household

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[48] There were six different sets of tests, three for primary school (i.e. grades 1-6) students and three for lower-secondary school (i.e. grades 7-9) students. For primary school students, each test was designed for students in two grades: students in grades 1 and 2 took the same tests, students in grades 3 and 4 took the second set of tests, and students in grades 5 and 6 took the third set. For lower-secondary school students, each set of tests was designed for students in each grade.

[49] In addition to asking the highest degree completed by the parents, as done in most household surveys conducted in China, the GSCF also collected data on the number of years spent in pursuing their highest degree. These two variables allow one to impute a more accurate measure of years of schooling than in other studies in China. Due to China's education policy in the 1960s and the 1970s, many rural parents spent only two years, rather than three years, in low-secondary schools. Thus, assigning three years of low-secondary education to parents who reported completion of lower-secondary education, as in the common practice in previous studies in China, will likely to overestimates parents' education.

expenditure in *Yuan*.[50] Per capita landholding is included because, in theory, it can affect parental investments in children's education, e.g. time spent helping children with homework. Parents with higher per capita land endowment may spend less time on child education if they face some obstacles to hire outside labor for farm production. Variables that capture parents' motivation for their chilren's education are also included: they are created as a set of dummy variables for parents' aspiration for the highest level of education (lower-secondary school, upper-secondary school, and college and university education) they want their children to attain.

(c) Child characteristics. Child endowments (**K**) include gender and child age in months, and age-squared. Dummies for child birth years are included to capture nonlinear cohort effects of age on academic skills. These variables are exogenously determined at birth. Also included in the set of **K** variables are children's health and nutritional status (measured by their height-for-age z-scores) and birth order (numbers of older and younger siblings). In theory, these two variables could be endogenously determined by parents' choice regarding how many children to have and how much to invest in each child's health. However, dropping these variables may lead to omitted variables bias. To address this issue, this essay reports regression results with and without these variables (see Section 4 for results). In fact, results with or without these variables are very similar; these variables are themselves never statistically significant in any of the regressions.

[50] In household surveys, measures of household income are often collected by asking respondents to report sources and amounts of income, which is inaccurate due to recall bias, imputation, seasonality and the confusion regarding the concept of income among farm households; the latter two are especially troublesome when surveys are conducted in rural areas. Household consumption expenditure would be a more accurate measurement of household wealth, as it is a less sensitive topic during household surveys and requires no knowledge of costs and profits for farm households (Deaton 1997).

(d) Child ability and its measurement. This essay uses scores on a cognitive ability test (*CAT*) as an indicator of child cognitive ability. The test was administrated to all sample children in 2000, when they were of 9 to 12 years old. The test was developed by researchers at the Institute for Psychology at the Chinese Academy of Social Sciences, and was designed to be independent of academic background (Brown 2006).[51] A typical question on the *CAT* asks a child to extract and analyze information from several sources within a given amount of time. An example question is, "What do an elbow and a knee have in common?" The skills needed to correctly answer this type of question (i.e. extracting information from daily observations) are not explicitly required by the standard curriculum for children age 9 to 12.

While the *CAT* scores may capture a child's innate cognitive ability reasonably well, one would expect that training in school, especially in math classes, would also affect this child's performance on the cognitive ability test. In this sense, the *CAT* scores are likely to measure child ability with error. As discussed above, this essay deals with such measurement error by exploiting exogenous variation in the true ability to create IVs for the *CAT* scores. There are two candidates. The first is the scores on a literacy test (*LIT*) administered in 2004. Similar to the *CAT*, the *LIT* was also designed by researchers at the Institute for Psychology at the Chinese Academy of Social Sciences. While the *LIT* scores may also capture a child's innate cognitive ability well, it may not be an ideal IV for *CAT*. This is because both the *CAT* and *LIT* scores may capture the impact of a child's test-taking skills that are not part of his or her innate cognitive ability. The second, and

[51] The cognitive test scores have been used to measure child innate ability in other context. For example, Brown (2006) uses the same ability measure to control for child innate ability when investigating the educational investment behavior of parents using the GSCF-I data. However, Brown does not deal with potential measurement error in the cognitive ability test score.

probably better, set of IVs is the famine-generated IVs discussed in section 4 above. For comparison, results from using either set of IVs are presented below.

(e) School characteristics. Finally, as mentioned above, school characteristics (**Q** variables), as well as **P** variables that vary at the school level, are controlled for using school fixed effects. Note that for the children who had dropped out of school between 2000 and 2004, there is no school information for them in the 2004 panel, which makes it difficult to create school dummies for these children. The strategy here is to assign the codes of the villages where these children resided in 2000 as school codes for them. This strategy effectively controls for school fixed effects to a large extent, for two reasons. First, each village in rural Gansu usually has only one primary school and one lower-secondary school. Second, China's policy on basic education enrollment is strictly based on locality of residence (i.e. villages in rural areas).[52] These two facts imply that, in most cases, information on the location of the village a drop-out child lived in, combined with the information on the highest grade this child ever attended, can uniquely identify the school he or she attended before dropping out.

3.7. Empirical Results

This section presents and discusses the main findings of this essay. Although the multiple indicator method using the famine-generated IVs to instrument the *CAT* scores is the preferred method, results from four other estimation methods discussed above are also presented.

[52] Another possible way is to replace school codes in 2004 by school codes in 2000. But one problem with this approach is that many children who dropped out of school had finished lower-secondary education by 2004, which means the schools they attended in 2000 were their primary schools, not their lower-secondary schools.

3.7.1. Methods and Specifications

All tables discussed in this section present results using all five methods. For ease of comparison, all tables present them in the following order. The first method (M1) is the OLS regression, leaving child ability in the error term with no attempt to control for it. The second (M2) is the proxy variable method, replacing child ability (*G*) in (7) with the *CAT* scores as a proxy for *G*. The third method (M3) is a variant of the multiple indicator method: one indicator of child ability, the *CAT* scores, is instrumented by another indicator, the *LIT* scores. The fourth method (M4) is the preferred method, another variant of the multiple indicator method: the *CAT* scores are instrumented by the famine-generated IVs. The last method (M5) uses parents' family background variables as IVs for parental education. The set of IVs include their parents' (i.e. grandparents of the GSCF children's) education, the numbers of their older and younger siblings, and a dummy for whether a parent is the oldest child (definitions and summary statistics of these IVs can be seen in Table 3-A2).

Each method has two specifications. The first, parsimonious one includes only parental education, child age and gender as explanatory variables, without including any other family background or child characteristic variables in the regression. The second specification includes the full set of explanatory variables described in Section 5 (see Table 1). In Tables 3-2 and 3-3, where the main findings of this essay are presented, odd-numbered columns present results using the parsimonious specification, while even-numbered columns present results using the second specification. In Tables 3-4 and 3-5, where heterogeneous effects of parental education are explored, only results using the second specification are reported. All regressions control for school fixed effects. To account for intra-school correlation of the error term in achievement test scores, standard errors are adjusted for clustering at the school level.

Before turning to the main findings, it is worth assessing the performance of these methods in controlling for child ability. One way to do so is to compare the estimated ability impacts across methods and to see how different methods affect the estimated impacts of parental education. Basic results are shown in Tables 3-2 and 3-3.

Turn first to Table 3-2 for estimated ability impacts on child math skills. Three methods, M2, M3, and M4, explicitly control for child ability in regression. As shown in the table, the proxy variable method (M3) estimates that a one-standard-deviation increase in the distribution of child ability is associated with an increase of 0.22 standard deviations in a child's math skills (columns (1)-(2)). Since the ability impacts estimated by this method are likely to suffer from attenuation biases (due to measurement errors in the *CAT* scores), one would expect M2 to underestimate the impact of child ability on math skills. As expected, when the *CAT* scores are instrumented, either by the *LIT* scores (columns (5)-(6)) or by the famine-generated IVs (columns (7)-(8)), the estimated ability impacts increase to 0.50 and 0.30 standard deviations (of the distribution of math scores), respectively. Note that ability impacts estimated using M3 (columns (5)-(6)) are likely to be overestimated, because measurement errors in the *CAT* and the *LIT* scores could be correlated, for example, both capturing test-taking skills. Ability impacts estimated using the preferred method (M4: columns (7)-(8)), although insignificant, lie between the estimates using M2 (lower bound) and M3 (upper bound), which seem quite plausible. A similar pattern is found in Table 3-3 for child reading skills.

In any case, how one controls for child ability has little impact on the estimated impacts of parental education. Two methods that leave ability in the error term, M1 (columns (1)-(2)) and M5 (columns (9)-(10)), also yield similar impacts of parental education. Note that method M5 (using parents' family background variables to instrument parental education) is likely to overestimate the impacts of parental education, since parents' family background is presumably positively correlated with parental ability and thus child ability. As expected, the estimated impacts of parental education using M5 are usually higher than those using other methods in Tables 3-2 and 3-3, and especially so in Tables 3-4 and 3-5, as discussed below. In short, the general pattern of results using different methods is consistent with expectation based on the discussions in Section 4, and there is evidence that the preferred method (M4) performs the best (although its advantage over other methods is not overwhelming).

Finally, the robustness of the estimated impact of parental education to different methods suggests that parental education is not closely correlated with child ability in rural China, which implies that it is not correlated with parental ability. One explanation for this lack of correlation is that the education system when parents were of school age was not as competitive as today's system – ability was not a key determinant for educational attainment in rural China several decades ago.

3.7.2. Impacts of parental education: basic findings and puzzles

Now one is in a position to discuss the basic findings of this essay. Two findings are most notable. First, father's education has a significantly positive impact on children's academic skills, both math (Table 3-2) and reading skills (Table 3-3), which is robust to methods and specifications. One additional year of father's education increases children's math skills by 0.017 to 0.022 standard deviations (of the distribution of math scores) and their reading skills by 0.012 to 0.015 standard deviations (of the distribution of Chinese scores). The estimated impacts of father's education, although statistically significant (at least at 10% level) in all columns in Tables 3-2 and 3-3, appear to be quite small. Yet given that parents in rural China have completed only a few years of education, these impacts still represent a large potential for improving children's academic skills. In the GSCF sample, for example, fathers have completed only 7 years of schooling on average. Had they been able to complete secondary education like their urban counterparts, their children could have scored 0.10 (= 0.020×5) standard deviations higher on the math test and 0.08 (= 0.015×5) standard deviations higher on the reading test, other things being equal.

Secondly, and somewhat surprisingly, mothers' education does not have a significant impact on their children's academic skills, either math or reading skills, in most of the specifications; its impact is also smaller than that of father's education. This finding is puzzling given the common finding in the literature, where mother's education has usually been found to have a larger (and statistically more significant) impact on child educational outcomes than father's education (Behrman and Knowles 1999; Behrman and Rosenzweig 2002).

The context of rural China, especially the absolutely low level of education received by rural women, offers some insights into this puzzle. Traditional Confucian thought in China has suppressed the demand for women's education for more than two thousand years. Even today, rural women's educational attainment remains very low. In the GSCF sample, the average level of mother's education is only 4 years (Table 3-1), and more than on fourth (560) of the mothers in this sample have no formal education. This very low level of rural women's education provides two explanations for the lack of significant impacts of mother's education. First, rural mothers' education may be too low to be functional to help their children to learn, since functional literacy and numeracy are usually not achieved through fewer than four years of education (e.g. Simmons 1976). Secondly, the range of mother's education may be too small to yield any precise estimates of the impact of mother's education in the regressions.[53] Note that these explanations have distinct testable predictions. The first implies that a stronger (and more significant) impact of mother's education would be found in a subsample with better-educated mothers. In contrast, the second explanation predicts that little significant impact will be found in this subsample because mother's education has even less variation in this (truncated) sample.

3.7.3. Heterogeneous impacts of parental education

To see which explanation is more sensible, the GSCF sample is split into two subsamples by the median level of mother's education (four years). Same regressions reported in Tables 3-2 and 3 are run on these two subsamples, whose results are reported in Table 3-4. Consistent with the first explanation, the functional literacy and numeracy hypothesis, and against the second explanation, mother's education does have a significant impact on math skills (but not no reading skills) for children of better-

[53] The lack of variation in mother's education yields large standard errors of its estimated impacts – generally larger than the standard errors of the estimated impact of father's education in Tables 2 and 3.

educated mothers (Panel A), but has essentially no impact for children whose mothers have less than four years of education (Panel B).

The functional literacy and numeracy hypothesis would be further supported if a similar pattern is found for father's education. Because the sample with poorly educated fathers (< four years of education) is quite small (< 300), nothing is precisely estimated in this sample (results not shown). Yet the results in Panel C of Table 3-4 indirectly support the functional literacy and numeracy hypothesis. Dropping children of poorly-educated fathers' yields a much larger impact of father's education than that reported in Table 3-2, which implies a much smaller impact of father's education in the sample with poorly-educated fathers. In short, the functional literacy and numeracy hypothesis explains reasonably well the lack of significant impact of mother's education found in Tables 3-2 and 3-3. This hypothesis also helps explain why so many rural mothers did not have any formal education: if they cannot complete at least four years of education, why bother investing in the first three years?

Note that in the subsample with better-educated mothers, mother's education has a larger impact on child math skills than father's education, which is consistent with the common finding in the literature. This also reveals that the absolutely low level of mother's education in rural China disguises its true impact. Note also that even in this subsample (with better-educated mothers), almost all children (95.4%) have already completed more years of education than their mothers, yet their mothers' education still has a significant impact on their math skills. For children who have completed more years of education than their mothers, their mothers' education is unlikely to have a direct impact on their math skills; it is more likely to affect their math skills indirectly. Two such indirect impacts are possible. First, better-educated mothers might invest more in educational inputs, such as extra learning materials and discussions with teachers about their children's academic performance (Brown 2006). Secondly, and more importantly, mother's education may have affected educational investments made in the *past*, which presumably contribute greatly to children's current stock of academic skills.

A note on the impacts of child endowments is in order. A gender gap, at least significant at the 10% level, is found for child math skills (but not for reading skills).[54] Other things being equal, girls scored about 0.1 standard deviations lower on the math test than boys, on average. This gap is worth emphasizing given the estimated impacts of parental education, because it takes an additional five years of father's education, or even more years of mother's education, to compensate the disadvantage of being a girl. Since child ability and school quality have been controlled for in regression, one could interpret this gender gap as discrimination against girls in math education in rural China. Finally, none of other child characteristics has a significant impact on child academic skills; with or without these child characteristics, the estimated impacts of parental education remain similar.

3.7.4. Intergenerational transmission of gender gap in education.

The gender gap in acquired math skills (Table 3-2), as well as the low level of mother's education compared to father's education (Table 3-1), suggests that females have been discriminated against in education over generations. Yet is the gender gap in education intergenerationally transmitted? In other words, will the gender gap in one

[54] Brown and Park (2002) also find a significant gender gap in education in China.

generation widen the gender gap in the next generation? If the answer is yes, one would expect mother's education to have a larger impact for girls than for boys, because girls of poorly-educated mothers were left further behind in the gender gap. At the same time, one would expect father's education to have either similar impacts for girls and boys or a larger impact for boys than for girls. This is because father's education would either play no role in widening the gender gap for the next generation or play a role in widening the gender gap by raising boys' academic skills relative to girls'.[55]

To examine this issue, Table 3-5 introduces interaction terms between parental education and child gender in regression (columns (1)-(5) for math skills and columns (6)-(10) for reading skills). Interaction terms between *CAT* scores and parental education are also included to allow the impacts of parental education to vary with child ability (although the impacts of parental education do not seem to depend on child ability).[56] The results suggest that the gender gap in child academic skills was transmitted, at least partly, from the gender gap that occurred in their parents' generation. A significant gender gap in child math skills is again revealed in Table 3-5; meanwhile, mother's education has a significant impact only on girls' math skills. One additional year of mother's education raises girls' math scores by 0.030 standard deviations, but has essentially no impact on boys' academic skills, either math or reading skills. In contrast, father's education has a significantly (at least at the 10% level) positive impact on both math and reading skills for both boys and girls, which suggests that father's education plays no role in widening the gender gap in child academic skills. It is estimated that one

[55] In the context of other developing countries including Ghana and Brazil, Thomas (1994) finds that father's education matters more for boy's height and mother's education matters more for girls' height.
[56] These interactions are instrumented by the interaction terms between parental education and IVs for *CAT* scores when using M2-M4.

additional year of father's education increases a child's math and Chinese scores by about 0.020 standard deviations (of the distribution of math and Chinese scores) for both girls and boys.

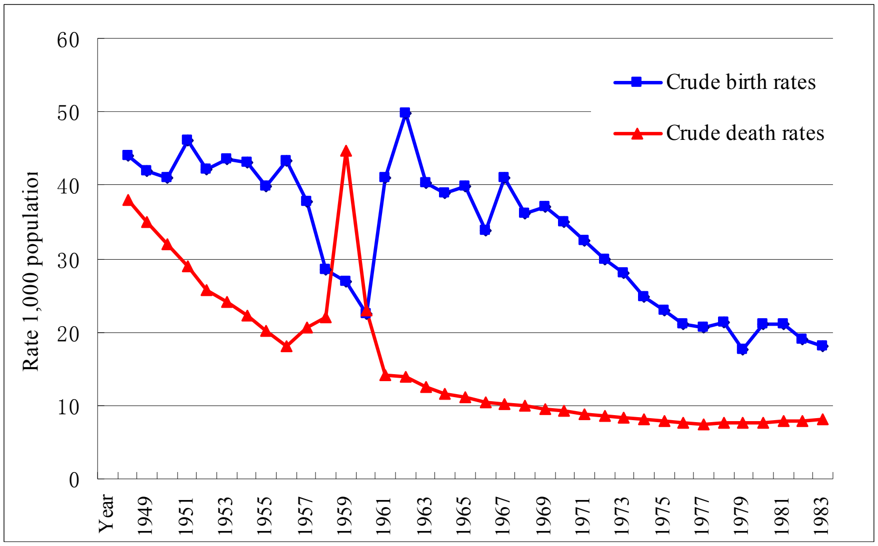
Yet why mother's education matters only for girls in the first place? A possible explanation is that better-educated mothers are a household role model for girls, providing high motivation for their daughters – who would be discriminated against otherwise – to study hard. That mother's education matters exclusively for girls' education also suggests that girls of better-educated mothers are more likely to jump out of the gender gap. Although *eight to nine* additional years of mother's education would be needed to eliminate the gender gap, it is not impossible to achieve. Given the average years of education for rural mothers (four years), the gender gap in child math skills would have been eliminated had they completed their upper-secondary education. Completing upper-secondary education has not been required by China's Compulsory Education Law, but it is achievable, perhaps in the near future – it has been (almost) universally achieved in urban China.

3.8. Concluding Remarks

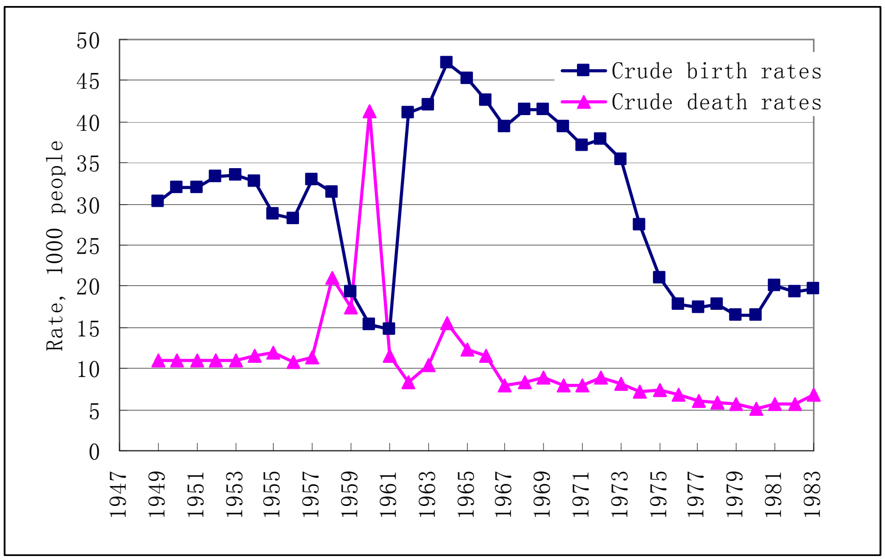
While there is increasing consensus that academic skills play an important in generating individual and household incomes in developing countries, little is known about the determinants of child academic skills in rural China. Much of the effort in this essay is thus devoted to investigating the role parental education plays in the acquisition of academic skills for children in rural China. To obtain credible estimates of the impact of parental education, this essay applies a number of econometric methods to control for unobserved child ability, a potential confounding factor. It finds that parental education has a significant impact on child academic skills, even after controlling for child ability.

This essay also finds that the context of rural China offers important insights into the role of parental education in a poor economy. First, the extremely low level of mother's education, suppressed by traditional Confucian thought and the low economic returns to education in rural China, disguises the true impact of mother's education. Mother's education has a significant impact on child academic skills only when it reaches the four-year threshold for functional literacy and numeracy. This implies that fruitful intervention programs should be designed not only to attract girls to school, but also to keep them in school long enough. In this sense, China's current effort of strictly enforcing its Compulsory Education Law (provided that subsidies are available to poor households) is definitely desirable. Second, not only can education be transmitted intergenerationally, but the gender gap in education can also be transmitted intergenerationally. Mother's education is found to increase only girls' academic skills, and thus girls of poorly-educated mothers suffer more from the gender gap in academic skills. To help these girls escape the gender gap, many more investments in boosting rural women's educational attainment are clearly needed.

In conclusion, rural China has a pressing need, as well as much room, for improvements in rural education. The findings of this essay suggest that programs that aim to boost women's educational attainment may be an efficient way to close up the gender gap in child academic skills in rural China, and perhaps eventually to close up the great urban-rural divide in educational attainment in China.

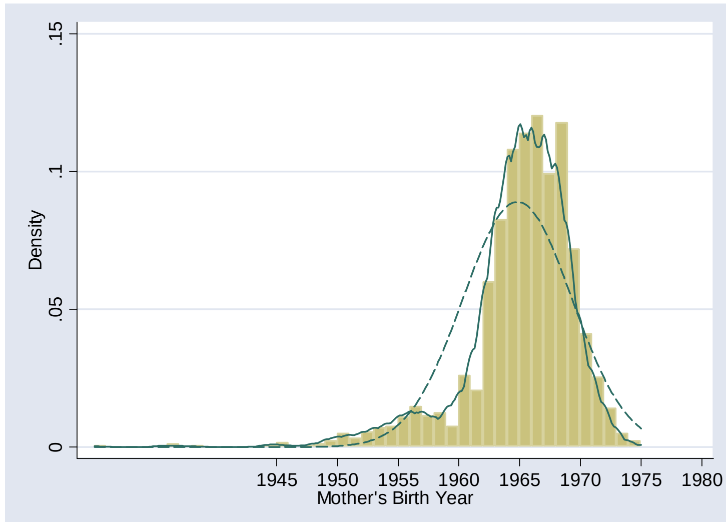


(A) Crude Birth Rates and Crude Death Rates in China: 1949-83

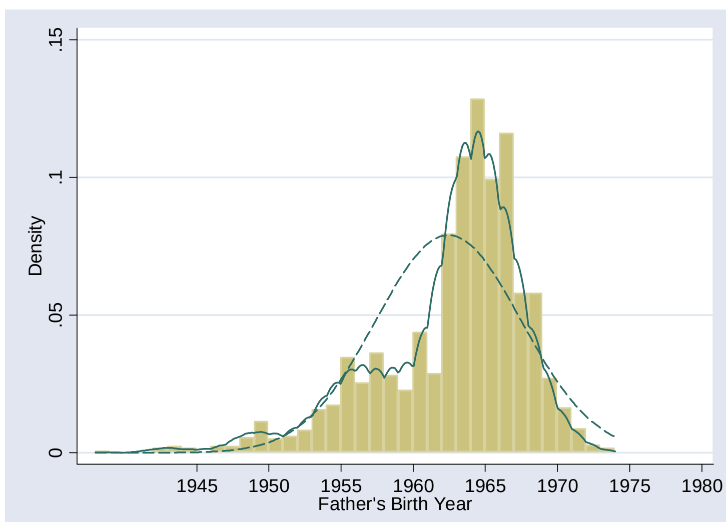


(B) Crude Birth Rates and Crude Death Rates in Gansu: 1949-83

Figure 3-1: Crude Birth Rates and Crude Death Rates in China and Gansu: 1949-83
Sources: Panel (A): Banister (1987). Panel (B): Gansu Statistical Yearbook (2001).

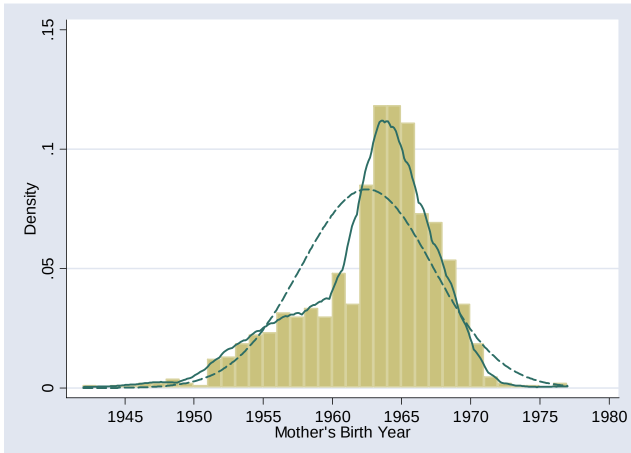


(A) Distribution of mother's birth years

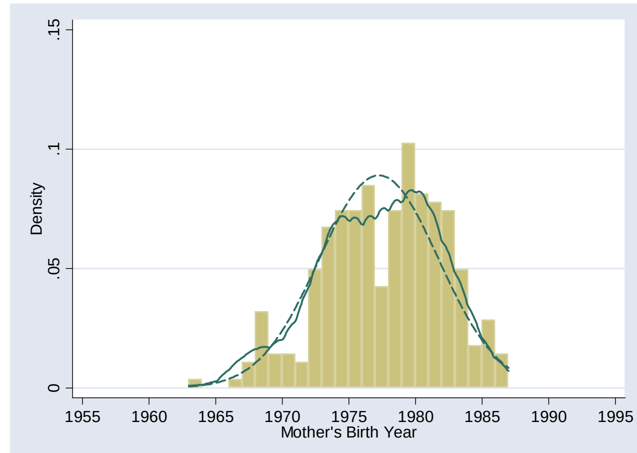


(B) Distribution of father's birth years

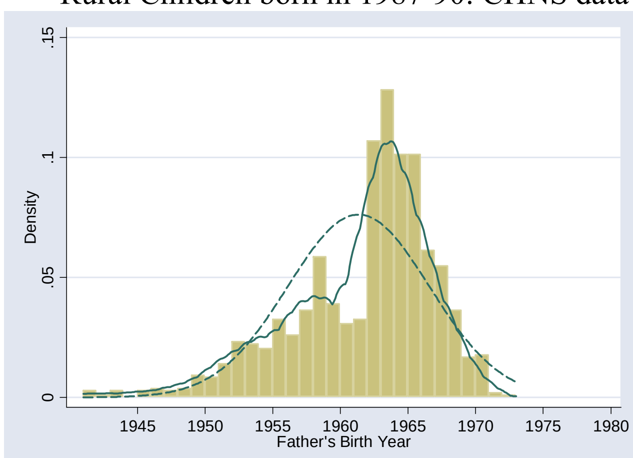
Figure 3-2: Distributions of Parental Birth Years: Rural Children Born in 1987-90; GSCF Data



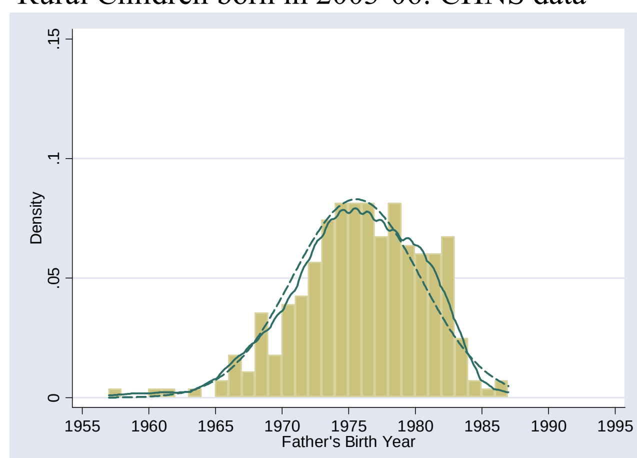
(A) Distribution of mother's birth years:
Rural Children born in 1987-90: CHNS data



(B) Distribution of mother's birth years:
Rural Children born in 2003-06: CHNS data



(C) Distribution of father's birth years:
Rural Children born in 1987-90: CHNS data



(D) Distribution of father's birth years:
Rural Children born in 2003-06: CHNS data

Figure 3-3: Distributions of Parental Birth Years: Rural Children in Eight Other Provinces; CHNS data

Table 3-1: Variable Definition and Summary Statistics

Variable	Definition	Mean	Std. Dev.	Min	Max
Math skills	Standardized score in mathematics test	0	1	-1.32	2.57
Reading skills	Standardized score in Chinese test	0	1	-1.92	2.62
FaEdu	Father's education measured in years	6.94	3.51	0	14
MoEdu	Mother's education measured in years	4.12	3.49	0	12
FaFarmjob	Dummy, =1 if father's primary occupation is farming	0.63	0.48	0	1
MoFarmjob	Dummy, =1 if mother's primary occupation is farming	0.95	0.22	0	1
FaBio	Dummy, =1 if father is the biological father of the child	0.99	0.10	0	1
MoBio	Dummy, =1 if mother is the biological mother of the child	0.92	0.28	0	1
FaHandicap	Dummy, =1 if father is handicapped	0.04	0.20	0	1
MoHandicap	Dummy, =1 if mother is handicapped	0.04	0.20	0	1
FaAge	Father's age in years	41.54	5.06	30	60
MoAge	Mother's age in years	39.18	4.49	29	54
Log(Pcexp)	Log (yearly expenditure per capita in *yuan*[a])	6.81	0.71	4.34	9.62
Log(Pcland)	Log (Land holding per capita measured in *mu*[b])	0.47	0.74	-3.40	2.35
CompMid	Dummy, =1 if parents' aspiration for the highest level of education the child could finish is low-secondary school	0.09	0.28	0	1
CompHigh	Dummy, =1 if parents' aspiration of the highest level of education the child could finish is upper-secondary school	0.24	0.43	0	1
CompCol	Dummy, =1 if parents' aspiration of the highest level of education the child could finish is college/university education	0.66	0.47	0	1
Female	Dummy, =1 if the child is female	0.47	0.49	0	1
Age	Child's age measured in months	180.57	14.84	148	238
Age^2	Squared child's age measured in months, divided by 100	324.27	46.29	228	432
Ability	Child's innate cognitive ability measured (proxied) by the scores of the cognitive ability test (*CAT*), standardized by age and gender	0	1	-1.73	2.56
HAZ	Child's height-for-age z-scores	-1.23	1.09	-5.96	1.96
OldSib	Number of older siblings of the child	0.72	0.79	0	4
YoungSib	Number of older siblings of the child	0.62	0.71	0	5

Notes. (a) *Yuan* is the Chinese currency: one US *dollar* = 8.27 *Yuan* in 2004. (b) *Mu* is the metric used in China to measure land size: one hectare = 15 *mu*.

Table 3-2: Impacts of Parental Education on Children's Math Skills

Method	(M1) Ability Omitted		(M2) Ability Proxied by CAT		(M3) CAT Instr.by LIT		(M4) CAT Instr.by Famine-IV		(M5) Par. Educ. instr. by family background	
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FaEdu	0.018***	0.021***	0.018***	0.020***	0.017**	0.019**	0.019***	0.020***	0.020***	0.022***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
MoEdu	0.018**	0.018*	0.015*	0.015*	0.008	0.009	0.014	0.013	0.014*	0.014
	(0.009)	(0.009)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)
FaFarmjob		0.032		0.036		0.050		0.036		0.030
		(0.055)		(0.052)		(0.055)		(0.052)		(0.053)
MoFarmjob		-0.149		-0.160		-0.169		-0.175		-0.147
		(0.115)		(0.112)		(0.116)		(0.111)		(0.111)
FaBio		0.127		0.003		-0.096		-0.079		0.179
		(0.209)		(0.213)		(0.268)		(0.286)		(0.261)
MoBio		-0.055		-0.057		-0.081		-0.059		-0.078
		(0.070)		(0.071)		(0.096)		(0.090)		(0.092)
FaHandicap		0.065		0.027		-0.018		0.000		0.070
		(0.103)		(0.106)		(0.139)		(0.141)		(0.131)
MoHandicap		0.037		0.085		0.101		0.122		0.041
		(0.110)		(0.110)		(0.139)		(0.144)		(0.130)
FaAge		0.009		0.009		0.006		0.008		0.009
		(0.008)		(0.008)		(0.009)		(0.009)		(0.009)
MoAge		0.002		0.002		0.005		0.004		0.003
		(0.010)		(0.010)		(0.010)		(0.010)		(0.010)
Log (Pcexp)		0.068		0.063		0.038		0.064		0.077*
		(0.045)		(0.045)		(0.046)		(0.046)		(0.045)
Log (Pcland)		-0.056		-0.037		-0.038		-0.024		-0.047
		(0.057)		(0.056)		(0.060)		(0.060)		(0.058)

CompMid		0.168		0.170		-0.086		0.206		0.145
		(0.292)		(0.277)		(0.307)		(0.256)		(0.241)
CompHigh		0.207		0.210		-0.034		0.255		0.201
		(0.263)		(0.250)		(0.299)		(0.250)		(0.235)
CompCol		0.179		0.171		-0.069		0.197		0.172
		(0.267)		(0.254)		(0.297)		(0.249)		(0.232)
Ability	(omitted)	(omitted)	0.218***	0.208***	0.507***	0.508***	0.289	0.344	(omitted)	(omitted)
			(0.035)	(0.037)	(0.041)	(0.044)	(0.225)	(0.237)		
Female	-0.083	-0.096*	-0.096*	-0.100*	-0.112**	-0.116**	-0.092**	-0.091*	-0.080*	-0.092*
	(0.051)	(0.058)	(0.050)	(0.056)	(0.046)	(0.051)	(0.046)	(0.049)	(0.045)	(0.049)
HAZ		0.036		0.028		0.022		0.022		0.036
		(0.022)		(0.022)		(0.024)		(0.025)		(0.023)
OldSib		0.055		0.052		0.040		0.048		0.053
		(0.039)		(0.039)		(0.042)		(0.042)		(0.041)
YoungSib		0.048		0.041		0.047		0.040		0.047
		(0.046)		(0.047)		(0.049)		(0.047)		(0.047)
Age	-0.036	-0.002	-0.052	-0.024	-0.103	-0.057	-0.068	-0.028	-0.028	0.008
	(0.117)	(0.134)	(0.111)	(0.128)	(0.117)	(0.124)	(0.111)	(0.118)	(0.112)	(0.118)
Age^2	0.014	0.004	0.017	0.009	0.030	0.0166	0.022	0.009	0.013	0.001
	(0.033)	(0.038)	(0.031)	(0.036)	(0.033)	(0.035)	(0.031)	(0.033)	(0.031)	(0.033)
F-test for joint sig. of excluded IVs										
Endo. Var. = CAT					676.56***	661.47***	28.872***	26.714***		
Endo. Var. = FaEdu									25.65***	21.10***
Endo. Var. = MoEdu									28.78***	26.04***
Over-identification test										
Sargan statistic							10.614	5.201	9.047	7.692
(p-value)							(0.1564)	(0.6354)	(0.5277)	(0.6589)
R-squared	0.109	0.133	0.141	0.161	--	--	--	--	--	--
Obs.	1,678	1,532	1,678	1,532	1,608	1,466	1,649	1,508	1,664	1,525

Note. 1. All regressions control for children's birth year and school fixed effects. 2. Standard errors, adjusted for within-school correlation, are in parentheses. 3.*** Significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 3-3: Impacts of Parental Education on Children's Chinese Skills

Method	(M1) Ability Omitted		(M2) Ability Proxied by CAT		(M3) CAT Instr.by LIT		(M4) CAT Instr.by Famine-IV		(M5) Par. Educ. instr.by Par. Fam. background	
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FaEdu	0.013*	0.015**	0.013*	0.015**	0.012*	0.014*	0.012*	0.015**	0.013*	0.015**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
MoEdu	0.009	0.005	0.007	0.004	0.004	0.000	0.008	0.003	0.003	-0.003
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
FaFarmjob		0.048		0.050		0.054		0.054		0.042
		(0.049)		(0.048)		(0.050)		(0.049)		(0.048)
MoFarmjob		-0.054		-0.059		-0.053		-0.059		-0.059
		(0.093)		(0.091)		(0.106)		(0.104)		(0.102)
FaBio		0.329*		0.279		0.092		0.188		0.227
		(0.178)		(0.173)		(0.245)		(0.267)		(0.239)
MoBio		-0.077		-0.077		-0.077		-0.084		-0.074
		(0.077)		(0.079)		(0.088)		(0.084)		(0.084)
FaHandicap		-0.092		-0.107		-0.172		-0.103		-0.098
		(0.107)		(0.107)		(0.127)		(0.132)		(0.119)
MoHandicap		-0.005		0.0142		0.0783		0.030		-0.002
		(0.128)		(0.124)		(0.127)		(0.135)		(0.119)
FaAge		-0.013*		-0.013*		-0.015*		-0.012		-0.014*
		(0.008)		(0.008)		(0.008)		(0.008)		(0.008)
MoAge		0.026***		0.025***		0.027***		0.020**		0.026***
		(0.008)		(0.008)		(0.009)		(0.010)		(0.009)
Log (Pcexp)		0.029		0.025		0.014		0.014		0.030
		(0.042)		(0.041)		(0.043)		(0.043)		(0.043)
Log (Pcland)		-0.004		-0.001		0.005		0.012		0.017
		(0.050)		(0.049)		(0.055)		(0.056)		(0.059)

CompMid		-0.098		-0.097		-0.089		0.020		-0.104
		(0.263)		(0.257)		(0.280)		(0.239)		(0.221)
CompHigh		-0.032		-0.031		-0.032		0.086		-0.038
		(0.253)		(0.247)		(0.273)		(0.234)		(0.215)
CompCol		-0.078		-0.081		-0.080		0.031		-0.082
		(0.247)		(0.242)		(0.270)		(0.232)		(0.213)
Ability	(omitted)	(omitted)	0.081**	0.085**	0.265***	0.274***	0.094	0.218	(omitted)	(omitted)
			(0.039)	(0.042)	(0.037)	(0.040)	(0.208)	(0.222)		
Female	0.018	0.039	0.013	0.038	0.013	0.039	0.011	0.034	0.014	0.037
	(0.042)	(0.041)	(0.041)	(0.042)	(0.042)	(0.047)	(0.043)	(0.046)	(0.040)	(0.045)
HAZ		0.030		0.027		0.018		0.019		0.031
		(0.021)		(0.021)		(0.022)		(0.023)		(0.021)
OldSib		-0.006		-0.008		-0.010		-0.012		-0.008
		(0.032)		(0.033)		(0.038)		(0.039)		(0.037)
YoungSib		0.011		0.008		0.009		0.000		0.006
		(0.042)		(0.042)		(0.044)		(0.044)		(0.043)
Age	-0.089	-0.098	-0.095	-0.102	-0.131	-0.115	-0.093	-0.111	-0.095	-0.010
	(0.095)	(0.105)	(0.095)	(0.104)	(0.106)	(0.113)	(0.102)	(0.110)	(0.101)	(0.108)
Age^2	0.025	0.026	0.026	0.027	0.035	0.030	0.026	0.029	0.027	0.027
	(0.027)	(0.029)	(0.026)	(0.029)	(0.030)	(0.032)	(0.028)	(0.031)	(0.028)	(0.030)
F-test for joint sig. of excluded IVs						(Same as in Table 3-2)			(Same as in Table 3-2)	
R^2	0.008	0.022	0.014	0.029	--	--	--	--	--	--
Obs.	1,678	1,532	1,678	1,532	1,608	1,466	1,678	1,508	1,664	1,525

Note. 1. All regressions control for children's birth year fixed effects and school fixed effects. 2. Standard errors, adjusted for within-school correlation, are in parentheses. 3.*** Significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 3-4: Impacts of Parental Education on Children's Math and Chinese Skills: Heterogeneous Effects

Method	(M1) Ability Omitted	(M2) Ability Proxied by CAT	(M3) CAT Instr.by LIT	(M4) CAT Instr.by Famine-IV	(M5) Par. Educ. instr. by fam. background	(M1) Ability Omitted	(M2) Ability Proxied by CAT	(M3) CAT Instr.by LIT	(M4) CAT Instr.by Famine-IV	(M5) Par. Educ. instr. by Par. Fam.background
	Standardized Math scores					Standardized Chinese scores				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Mother's education ≥ 4 years (median)										
FaEdu	0.019*	0.019*	0.020*	0.022**	0.024**	0.015	0.015	0.016	0.015	0.015
	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
MoEdu	0.037**	0.032**	0.024	0.033**	0.034**	-0.001	-0.002	-0.009	-0.006	-0.010
	(0.015)	(0.015)	(0.015)	(0.016)	(0.017)	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)
R^2	0.152	0.170	--	--	--	0.010	0.012	--	--	--
Obs.	935	935	896	930	930	935	935	896	930	930
B: Mother's education < 4 years (median)										
FaEdu	0.019*	0.015	0.011	0.017	0.023**	0.010	0.008	0.002	0.009	0.010
	(0.010)	(0.009)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
MoEdu	-0.021	-0.013	-0.010	-0.006	-0.103**	0.003	0.007	0.007	0.002	-0.044
	(0.031)	(0.030)	(0.032)	(0.032)	(0.051)	(0.029)	(0.029)	(0.030)	(0.030)	(0.047)
R^2	0.063	0.131	--	--	--	0.014	0.036	--	--	--
Obs.	743	743	712	719	734	743	743	712	719	734
C: Father's education ≥ 4 years										
FaEdu	0.039***	0.037***	0.032***	0.039***	0.041***	0.020**	0.019**	0.018*	0.017*	0.021**
	(0.011)	(0.010)	(0.011)	(0.011)	(0.012)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)
MoEdu	0.019**	0.017**	0.011	0.019**	0.017*	0.008	0.008	0.005	0.009	0.001
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
R^2	0.117	0.150	--	--	--	0.010	0.013	--	--	--
Obs.	1,377	1,377	1,322	1,362	1,370	1,377	1,377	1,322	1,362	1,370

Note. 1.The sample in Panel A includes children whose mothers have exactly four years of education. This is done to be consistent with the hypothesis of functional literacy and numeracy. 2. Standard errors in parentheses; adjusted for within-school correlation. 3.*** Significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 3-5: Impacts of Parental Education on Children's Math and Chinese Skills

Dependent Variable	Standardized Math scores					Standardized Chinese scores				
Method	(M1) Ability Omitted	(M2) Ability Proxied by CAT	(M3) CAT Instr.by LIT	(M4) CAT Instr.by Famine-IV	(M5) Par. Educ. instr. by Par. family background	(M1) Ability Omitted	(M2) Ability Proxied by CAT	(M3) CAT Instr.by LIT	(M4) CAT Instr.by Famine-IV	(M5) Par. Educ. instr. by Par. family background
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FaEdu	0.021**	0.019**	0.017	0.020*	0.026**	0.016*	0.016*	0.014	0.021**	0.024**
	(0.008)	(0.009)	(0.011)	(0.011)	(0.011)	(0.009)	(0.008)	(0.010)	(0.011)	(0.010)
MoEdu	0.003	0.001	-0.007	0.004	-0.003	-0.003	-0.004	-0.009	-0.005	-0.017
	(0.013)	(0.012)	(0.011)	(0.011)	(0.012)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
Female	-0.237**	-0.264***	-0.314***	-0.247**	-0.207*	-0.027	-0.033	-0.045	-0.044	0.017
	(0.101)	(0.100)	(0.112)	(0.120)	(0.113)	(0.094)	(0.092)	(0.105)	(0.102)	(0.104)
Female	0.001	0.003	0.006	0.002	-0.007	-0.001	-0.001	-0.003	-0.002	-0.015
× FaEdu	(0.014)	(0.013)	(0.015)	(0.015)	(0.015)	(0.013)	(0.013)	(0.015)	(0.014)	(0.014)
Female	0.032**	0.034**	0.037**	0.033**	0.039**	0.018	0.018	0.025*	0.022	0.029**
× MoEdu	(0.0160)	(0.015)	(0.015)	(0.015)	(0.015)	(0.014)	-0.033	(0.015)	(0.014)	(0.014)
Ability	(omitted)	0.185***	0.484***	0.116	(omitted)	(omitted)	0.053	0.346***	-0.421	(omitted)
		(0.061)	(0.090)	(0.365)			(0.074)	(0.082)	(0.349)	
Ability	(omitted)	0.013*	0.008	-0.007	(omitted)	(omitted)	0.004	-0.009	0.018	(omitted)
× FaEdu		(0.007)	(0.010)	(0.029)			(0.008)	(0.009)	(0.028)	
Ability	(omitted)	-0.014	-0.008	-0.005	(omitted)	(omitted)	0.000	-0.002	0.056	(omitted)
× MoEdu		(0.010)	(0.010)	(0.036)			(0.007)	(0.009)	(0.035)	
R^2	0.137	0.169	--	--	--	0.024	0.031	--	--	--
Obs.	1,532	1,532	1,466	1,508	1,524	1,532	1,532	1,466	1,508	1,524

Note. 1.Standard errors in parentheses; adjusted for within-school correlation.
2.*** Significant at 1% level; ** significant at 5% level; * significant at 10% level.

Chapter 4. Interrupted Maternal Education and Child Health: the Long Run Health Impact of the Chinese Cultural Revolution

4.1. Introduction

Child health is a key indicator of the quality of life in developing countries.[57] For example, child height, an indicator of long term child health status, is closely correlated with mental development, adult health, educational attainment, and even wages during adulthood (Thomas, Strauss, and Henriques 1991). Among the determinants of child health, mother's education attracts much attention from social scientists, because a robust and positive association between mother's education and child health has long been documented in the literature of intergenerational transmission of human capital.[58] This robust association has led many researchers (e.g. Schultz 1993; World Bank 1993) to recommend investments in women's education as a means to improve the health of the next generation, and thus their quality of life in the long run, in developing countries. Yet such a strong association also implies that educational interruptions encountered by women might have detrimental impacts on their children's health (e.g. Heaton 2008).

These impacts, however, can be realized only when the documented strong association between maternal education and child health is indeed *causal* (i.e. a *nurturing* effect of mother's education exists). To the extent that healthier women complete more years of education, such a strong association may simply reflect the impacts of mothers'

[57] Child health is itself a key indicator of economic development. Among the eight Millennium Development Goals (MDGs) that were adopted by the 189 members of the United Nation (UN) in 2000, four goals are related to child health (Todaro and Smith 2005). These four goals are: to reduce child mortality, to improve mother's health, to combat diseases and to eradicate hunger.
[58] In fact, mother's education is positively correlated with many child human capital outcomes. Chapters 2 and 3 of this dissertation find that mother's education has significant impacts on parental monetary investment in children's education and their acquired academic skills.

health endowments on child health (via the *nature* effect of mother's education).[59] Some researchers, e.g. Desai and Alva (1998), have questioned the causal interpretation of this association. But their questions have not been appropriately addressed, mainly due to the lack of suitable data to test this causation. After all, to test the causal effect of mother's education (i.e. to separate the nurturing effect form the nature effect), suitable data need to include information that is correlated with mothers' education but not correlated with their health *endowment*. Yet, such data are usually difficult to collect.

This essay explores the Great Proletarian Cultural Revolution of China (or the Chinese Cultural Revolution, CR), which occurred from 1966 to 1976, and uses it as a unique natural experiment to test such causality. This decade-long political campaign brought about serious disruptions to China's regular education system. During the Cultural Revolution, most schools were closed for two to six years; universities stopped normal recruitment for eleven years. As a consequence, the formal education of a great number of school-age children was interrupted. These interruptions generate exogenous variation in women's education that can be used to test the causality of interest in this essay.

This essay makes two empirical contributions. First, it contributes to the recent literature that tests the causal effects of mother's education on child health using natural experiments. Yet unlike most of these recent studies which exploit school construction events as natural experiments (e.g. Currie and Moretti 2003; Chou et al. 2010), this essay provides new evidence from school closure events. To the extent that both school construction and school closure events provide exogenous variation in mother's

[59] Throughout this essay, health endowment refers to one's genetic endowment related to health.

education, the new evidence provided in this essay serves as a "robustness check" of the results in the previous studies.[60]

Secondly, this essay contributes to a growing literature on the long run impacts of interrupted education on subsequent human capital formation (e.g. Ichino and Winter-Ember, 2004; Meng and Gregory 2002, 2007; Heaton 2008). Specifically, this essay contributes to the understanding of the long run impacts of the education interruptions caused by the Chinese Cultural Revolution.[61] This essay is the first to investigate the long run impacts of the Chinese Cultural Revolution on the well-being of the *next* generation, which has been largely ignored in the current literature. Previous studies on the Chinese Cultural Revolution focus almost exclusively on its impacts on the earnings of individuals who encountered educational interruptions (e.g. Meng and Gregory 2007; Zhang, Liu and Yung 2007; Giles, Park, and Wang 2008). The common finding in these studies is that whereas a generation of individuals suffered from a substantial loss in educational attainment due to the Chinese Cultural Revolution, the loss in educational attainment did not translate into substantial losses in their earnings.[62] The present essay, in contrast, shows that the loss in mothers' educational attainment has caused great loss in their children's health, as measured by child height (standardized by child sex and age). Using data collected in the China Health and Nutrition Survey (CHNS), this essay

[60] For example, one common finding in these studies is that the estimated effects of mother's education using instrumental variable approaches are usually larger than the effects estimated using ordinary least squares regressions (Chou et al. 2010).

[61] The local average treatment effect (LATE) interpretation of the instrumental variables (IV) approach suggested by Imbens and Angrist (1994) allows one to identify and estimate precisely the average loss in child health due to a one-year reduction in educational attainment for a mother who had to reduce her education because of the Chinese Cultural Revolution.

[62] One explanation is that education was not greatly rewarded in the urban labor market until the late 1990s. For example, Zhang et al. (2005) show that not until 1998 did the return to education in urban China reach the level of world average (9.7%) and the low-income country average (10.9%). An arguably more consistent estimate, 7.5% in 2002, is found in Appleton, Song and Xia (2005), who used panel data to control for unobserved individual and occupation attributes.

estimates that the Chinese Cultural Revolution reduced mothers' educational attainment by over two years on average, which, in turn, reduced their children's height by over 0.3 standard deviations (of the height distribution of a population of healthy children). This indicates a substantial loss in child health. The magnitude of such child health loss is comparable to the estimated effect of being exposed to the Chinese Great Famine in 1959-61 (Chen and Zhou 2007), the greatest famine in human history, in one's early childhood.

This essay unfolds as follows. The next section lays out the empirical framework for testing the causal effect of mother's education on child health. Section 3 discusses the natural experiment generated from the Chinese Cultural Revolution. Section 4 describes the data used in the empirical analysis. The fifth section presents and discusses the empirical findings. The final section concludes.

4.2. Empirical Framework

A simple empirical framework provides a useful starting point for thinking about the causal effect of mother's education on child health, and how to consistently estimate it. Mother's education can affect child health in many different ways. For example, better-educated mothers might have better knowledge about health care and nutrition, have healthier behaviors, and provide safer and more sanitary living environments for their children (e.g. Behrman and Deolalikar 1988, 1990; Thomas, Strauss and Henriques 1991; Glewwe 1999; Currie and Moretti 2003). Yet from a policy perspective, perhaps the most important effect of mother's education is the sum of all the effects mentioned above, i.e. the *total* effect of mother's education on child health. This total effect can be estimated from a reduced form statistical equation linking child *i*'s health status and his or her mother's educational attainment ($MoEdu_i$, measured as years of schooling), as follows:

$$HAZ_i = \beta_0 + \beta_1 MoEdu_i + \mathbf{X}_i^{'}\boldsymbol{\beta}_2 + \varepsilon_i \, . \qquad (1)$$

The key dependent variable of interest in this essay is the height-for-age z-score of child *i*, HAZ_i, which is defined as follows:

$$HAZ_i = \frac{h_{ij} - \bar{h}_j}{\sigma_j}, \qquad (2)$$

where h_{ij} is the observed height of child i in group j, where a group is defined according to child i's sex and age (measured in months). Parameters $\bar{h}_j$ and σ_j are the mean and the standard deviation of the height in group j, respectively, using healthy American children as the reference population.

In equation (1), **X** is a set of observed variables that potentially affect a child's health beyond the impacts of mother's education (via the above mentioned channels such as better health knowledge, etc.), such as local economic and/or public health conditions. The error term ε includes all variables that could affect child health but are not available in the data. For example, a mother's genetic endowments are important factors for her child's health but are difficult (if not impossible) to measure, and therefore, they are left in the error term. Also included in the error term are random health shocks and measurement errors in the dependent and explanatory variables, especially in *MoEdu*.

The coefficient β_1 in equation (1) measures the causal effect of interest in this essay. Yet estimating equation (1) using simple econometric techniques such as Ordinary Least Squares (OLS) regressions will likely produce biased results. This is because mother's education is likely to be correlated with the unobserved health endowments included in the error term ε. To estimate β_1 consistently, one needs to eliminate the correlation between *MoEdu* and ε. A useful strategy is to find suitable instrumental variables (IV) that can provide a source of variation in mother's education that is uncorrelated with the mother's health endowment.[63] In other words, the following first-stage regression should be estimated:

$$MoEdu_i = \alpha_0 + \mathbf{Z}_i'\boldsymbol{\alpha}_1 + \mathbf{X}_i'\boldsymbol{\alpha}_2 + e_i, \qquad (3)$$

where **X** is the same set of variables as in equation (1), and e_i is the error term in the first-stage equation. **Z** is a set of suitable instrumental variables (IV) that are uncorrelated with ε in equation (1), but can provide significant predictive power for *MoEdu* beyond that of **X** (i.e. $\boldsymbol{\alpha}_1$, the set of coefficients on **Z**, must be jointly significant in (3)).

Recent studies that test the causal effect of mother's education, e.g. Currie and Moretti (2003) and Chou et al. (2010), obtain instrumental variables (**Z**) by exploiting school construction programs as natural experiments. This essay adopts a similar strategy. As school construction events can exogenously increase school-aged children's

[63] Chen and Li (2009) use a different strategy. They use adoptee data collected in China to estimate the causal effect of mother's education on child height. Their rationale is that the genetic linkage between mothers and their children's health are nonexistent in for adoptees (if the parents can not choose their adoptees).

educational attainment, school closure events can exogenously reduce their educational attainment. Therefore, both events provide exogenous variation in individuals' education and can be explored to construct instrumental variables for mother's education. This essay constructs instrumental variables by exploring school closure events that occurred during the Chinese Cultural Revolution, which took place from 1966 to 1976, as will be discussed in detail in the next section.

Using the Chinese Cultural Revolution as a natural experiment to generate instrument variables for mother's education allows one to investigate the long run impacts of interrupted maternal education, due to the Cultural Revolution, on child health. The local average treatment effect (LATE) interpretation of the instrumental variables (IV) approach recently developed in the econometrics of program evaluation (e.g. Imbens and Angrist 1994)[64] implies that estimated β_1 based on IVs generated from the Chinese Cultural Revolution estimate the average loss in child health due to one-year reduction in the educational attainment of a mother whose schooling was disrupted because of the Cultural Revolution.[65]

4.3. Natural Experiment

The Chinese Cultural Revolution was initiated in May 1966, aimed to purge "capitalist-roaders" from the Chinese Communist Party (CCP) and "counter-

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[64] See Imbens and Wooldridge (2009) for a thorough review of recent developments in the econometrics of program evaluation.

[65] Note the monotonicity condition needs to hold for the IV estimates to have the LATE interpretations. The monotonicity condition requires that the Cultural Revolution should potentially affect all individuals' educational attainment in the same direction, had these individuals been exposed to the Cultural Revolution in school age. This assumption is reasonable because one would expect that anyone that encounters education interruptions will complete fewer years of education than in the counterfactual case had the Cultural Revolution not occurred. It is difficult to imagine that the exposure to education interruption will increase one's educational attainment.

revolutionaries" from China's entire society. During this decade-long political campaign, school systems in both urban China and rural China were seriously affected, although in very different ways.

4.3.1. School interruptions in the cities

The large scale school interruption in urban China is well-documented (e.g. Unger 1982; Pepper 1996). It can be divided into the following four periods: (1) 1966-68. Education at all levels was stopped; no teaching was carried out and no new students were recruited. (2) 1968-69. Primary and junior high schools were reopened. Children aged 7-9 could begin primary school and students who would have completed primary school in 1966-68 were able to attend junior high school. However, schools had two extra cohorts of students to accommodate, with limited classroom space. As a partial solution, students who were of normal graduation age for lower secondary or upper secondary school were given diplomas even though they missed several years of schooling. Later, most of these students, the so-called "educated-youths" were sent to the countryside to be "re-educated by peasants" due to the lack of employment opportunities in the cities.[66] At the same time, in the reopened schools, the original national standardized curriculum and teaching materials were completely abolished. Not until 1971 were updated provincial curricula made available. The original 6-3-3 curriculum (i.e. six years of primary, three years of lower secondary and three years upper secondary education) was cut to be 5-2-2, although schools in most cities added another year at the lower secondary level (i.e. 5-3-2) in the 1970s to cope with the shortage of urban jobs. (3) 1972-76. Senior high schools

[66] It is estimated that from 1969-1976, some 17 million educated-youths were sent to rural areas; the majority of them managed to return by the late 1970s (Zhou and Hou 1999).

resumed the admission of new graduates *directly* from lower secondary schools. But those who missed several years of normal education in lower secondary school were not allowed to proceed to upper secondary school, most of whom were sent to the rural areas as educated-youths. Colleges and universities also began restricted and small-scale admission, based upon students' political attitudes or their family backgrounds rather than on academic qualifications. (4) 1977-81. After the Cultural Revolution officially ended in 1976, colleges and universities began to admit new students based on their academic merit. National College Entrance Examinations were resumed in 1977, and everyone who had missed their chance to obtain a college education because of the Cultural Revolution (e.g. the sent-down youths) were entitled to take the exams.

Based on the events described above, Table 4-1 summarizes the expected interruptions[67] encountered by urban individuals born in different years, assuming they had the potential to complete upper secondary education had the Cultural Revolution not occurred.[68] It can be seen that urban individuals born in the mid-1950s suffered the most from educational interruptions.[69] The last column estimates the total years of interruptions encountered by an urban individual. It can be seen that some urban

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[67] Note that Table 4-1 lists the *expected*, as opposed to the *actual*, education interruptions encountered by these individuals. Without further information, it is difficult to estimate the *actual* education interruptions they encountered. One reason is that some individuals might have completed their optimal levels of education before the outbreak of the Cultural Revolution, and thus they did not suffer from education interruptions. This also applies to Table 4-4 below.

[68] This assumption comes from the fact the urban China had achieved universal primary school enrollment and almost universal lower-secondary school enrollment by 1965, the eve of the Chinese Cultural Revolution (Unger 1982).

[69] Take the cohort born in 1953 for example. This cohort was expected to enroll in primary school in 1960 at age 7, and to complete primary education in 1966. Yet this cohort could not enroll in lower secondary school because secondary schools were closed in 1966-68, and thus their lower secondary school entrance was delayed. When they entered lower secondary school in 1968, they had lost one year lower secondary education because the curriculum was reduced. After they completed lower secondary education in 1970, they could not enroll in upper secondary schools because these schools were not reopened until 1972. And even when upper secondary schools were reopened, they admitted only new graduates from lower secondary schools. Thus, the 1953 cohort will lose all three years of upper secondary education.

individuals encountered as many as eight years of education interruption. For urban China, education at the upper secondary (including specialized schools) and the tertiary level were the most seriously interrupted (Deng and Treiman 1997). The national number of student enrollment in specialized schools dropped from 547,400 in 1965 to 38,500 in 1969; the progression rate from senior high schools to universities was as high as 45.6% in 1965,[70] but then dropped sharply to between 4.2% and 4.7% in 1971-77 (Pepper 1996: pp.417). The estimated loss of human capital is substantial. It has been estimated that had the Cultural Revolution not occurred the urban school system would have produced two million more specialized school graduates and one million more graduates from universities and colleges (Encyclopedia of China: Education 1994: pp.556).

4.3.2. Radical education reforms in the countryside

Although boosting mass education in rural China was always on the Chinese Communist Party (CCP)'s political agenda in the Maoist era, the path of development of China's rural education system was full of twists and turns. The effort to boost rural enrollment was made as early as in the Great Leap Forward (GLF) movement in 1958-61, whose education component was known as "the Cultural Revolution of 1958" (Pepper 1996: pp. 279). The major change in education policy of the 1958 Cultural Revolution was the establishment of a large number of collectively-run agricultural middle schools[71] in 1958-59 (see Table 4-2) and new elementary schools that sprung up everywhere "like bamboo shoots after a spring rain" (Pepper 1996: pp. 281). Many agricultural middle schools were closed in 1961-63 (Table 4-2) due to the economic crisis that followed

[70] Note that this figure appears to be quite high. But it is reasonable given the fact the progression rate of lower secondary student entering academic upper secondary schools was only 25.6% in 1965.
[71] Here, agricultural middle schools include rural vocational schools.

immediately after the GLF, but revived in 1964-65. In 1965, there were more than 60,000 agricultural middle schools nationwide, almost tripling the number in 1958 (22,579).

Considered as a product of "Bourgeois ideology", however, all agricultural middle schools built in the late 1950s were closed during the initial stage of the Chinese Cultural Revolution (1966-68) (Pepper 1996; Zhang 2009). Although there are no statistics that indicate how many rural students dropped out of school due to this closure event, a rough estimate is over one million.[72] Also closed in this period were many rural primary schools (Ma and Long 1999).

After the initial years of political campaigns, during which "old cultures had been destroyed" by 1968,[73] the state needed a new school system. Rural China soon became the major experiment field for the radical education reform of 1969. The directives on the 1969 education reform required that every village-level collective (production brigade) should build its own complete primary school and that each commune should build its own combined low/upper secondary school. Despite the limited funding resources available from the state, most counties managed to complete these tasks, an important reason being that many of these commune-run secondary schools were built on the foundation of the previously closed agricultural middle schools.[74] The quantitative gain

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[72] Pepper (1996: pp. 416) reports that there were 9.34 million ordinary secondary school students, 0.55 million specialized school students, and 4.43 million agricultural middle school students enrolled in 1966. But the total number of specialized schools and agricultural middle school students in 1966 (agricultural middle schools were not counted separately during the Cultural Revolution years) dropped to 0.47 million. In 1966, the number of ordinary secondary school students increased to 12.50 million, but even if the increase (3.16 = 12.50-9.34) in the number of ordinary secondary school students were all caused by former agricultural middle school students being transferred to ordinary middle schools, there would still be a gap of 1.27 million between the decrease in agricultural middle school students and the increase in ordinary middle school students. These students could not enroll in colleges and universities because they were all closed.

[73] The Ninth Party Congress in April, 1969, claimed victory of the Cultural Revolution.

[74] Also, to construct these collectively-run schools, most school teachers in state-run schools were sent-down to their origins (villages) of birth to conduct teaching; many sent-down youths were also hired to teach.

of the 1969 rural education reform was remarkable (see Table 4-3). The national number of rural complete lower/upper secondary schools soared from 604 in 1965 to 11,819 in 1971, and continued to grow to 50,916 in 1977.

In retrospect, rapid expansion of rural schools seems to represent the general theme of China's rural education system in the Maoist era. Both the "Cultural Revolution in 1958" and rural education reform in 1969 aimed to achieve universal secondary school enrollment in rural China. In fact, the latter campaign was viewed as a revival of the short-lived former campaign (Pepper 1996: pp. 279). The initial phase of the Chinese Cultural Revolution in 1966-68, along with the GLF crisis in 1961-63, however, broke the continuity of these two campaigns to expand rural school systems, and thus represented the major interruptions to the long-term plan to rapidly expand the rural school system in China. Table 4-4, similar to Table 4-1, summarizes the expected education interruptions experienced by the cohorts at school age around the Cultural Revolution years, for rural residents. It lists the expected years of education interruptions encountered by rural individuals born in different years, assuming that the "counterfactual" of China's rural education system was one in which that the peak years of rapid school expansion extended from 1958 uninterrupted to the early 1970s.

4.3.3. Natural experiment

The Chinese Cultural Revolution can be viewed as an experiment in which different shocks in the supply of educational opportunities were assigned to individuals according to their years of birth. Under the assumption that the masses did not anticipate the outbreak and the long duration of the Chinese Cultural Revolution, these shocks can be considered to be *exogenous*, i.e. uncorrelated with ε in equation (1). The interplay of the timing of school interruptions and the birth years of school-aged children during the Cultural Revolution divide them into several cohort groups (CR_g, g = 1, 2, ... , 5 for both urban and rural individuals; see Tables 4-1 and 4-4) which received different shocks. Naturally, in this experiment, "an individual belonging to the *g*th CR group" defines the *g*th treatment.[75] The definitions of these treatment groups are listed in Tables 4-1 and 4-4.

The comparison of the (mean) educational attainment of cohort groups who encountered these shocks (the *treatment* groups) to the (mean) educational attainments of individuals who did *not* encounter these shocks (the *control* groups), provides exogenous variation in individuals' educational attainment. Control groups should be chosen in a way that they are similar to the treatment groups in all aspects, except that they did not encounter education interruptions. At minimum, the control group should consist only of individuals born *before* the Cultural Revolution, for two reasons. First, for those who were born before the Cultural Revolution, their genetic endowment will not be affected by the Cultural Revolution. Second, individuals born before the Cultural Revolution were fully exposed to the Cultural Revolution and thus would have experienced the "general Cultural Revolution effects" in aspects other than education, which were also experienced by the individuals in the treatment groups.

Keeping these considerations in mind, there are two proper control groups: (1) the group of individuals born in 1962-66 (the After-CR group), and (2) the group of individuals born in 1942-46 (the Before-CR group).[76] The After-CR group consists of

[75] Note that under the assumption that their parents could not have anticipated the outbreak of the Cultural Revolution, their years of birth (and thus the treatments they received) were unlikely to be the results of the Cultural Revolution. Thus, there is no worry of the problem of "selection into the Cultural Revolution".
[76] These control groups are also used in the recent studies by Meng and Gregory (2007) and Han, Suen and

those whose education was not interrupted, even though they were born before the Cultural Revolution and were attending school during the Cultural Revolution. These individuals started their primary education after schools were reopened (in 1968) and finished their secondary school education after colleges and universities resumed normal recruitment (in 1977). The Before-CR group is the group of individuals who had completed their upper secondary education just before the outbreak of the Cultural Revolution. This group would have entered universities by 1965, the eve of the Cultural Revolution. The reason to restrict the Before-CR group to individuals born after 1942 is that the individuals born before 1942 received part of their education in the Pre-Liberation era (i.e. before 1949), while the individuals born after 1942 received all their education under the Maoist regime.

For rural residents, the Before-CR and After-CR are also suitable control groups, although with somewhat different interpretation. The After-CR group consists of those who were fully exposed to the radical education reform of 1969, entering primary school after 1969 and entering secondary schools in the peak years of school expansion (Table 4-4). The Before-CR group consists of individuals whose lower secondary education was exposed to the peak years of another school expansion campaign, i.e. the “Cultural Revolution of 1958”. These two cohorts represented those who were exposed to the peak years of rapid school expansion at their school age, the normal theme of China’s rural education system in the Maoist era.

The first-stage regression equation (3), adopting treatments (CR_g, g =1, 2, …, 5) in this natural experiment as IVs, can be re-expressed as follows:

Zhang (2009).

$$MoEdu_i = \alpha_0 + \mathbf{CR}_i'\boldsymbol{\gamma} + f(\mathbf{Y}_i) + \mathbf{X}_i'\boldsymbol{\alpha} + v_i, \qquad (4)$$

where **X** is the same as in equation (1). **CR** is a set of cohort dummy variables (i.e. treatments, CR_g, g =1, 2, …, 5). The function $f(\mathbf{Y}_i)$ is a high-order polynomial function of mother i's year of birth ($\mathbf{Y}_i$), used to control for the general time trend in people's educational attainment, the importance of which is emphasized by Bound and Jaeger (1996).[77] The error term v_i captures the variation in mother's education that is not explained by the general time trend, the **X** variables, and the cohort dummies **CR**. As explained above, the set of **CR** variables can be considered as exogenous, and thus can serve as a set of valid instrumental variables for *MoEdu* as long as they are jointly significant in (4).

In this setting, the identification condition of the second-stage effect of maternal education, β_1 in equation (1), comes from the between-cohort variations in individuals' educational attainment, after controlling for a general time trend $f(\mathbf{Y}_i)$. More specifically, the part of cohort-specific *deviation* from the general time trend of education due to the Cultural Revolution is used to achieve the identification of β_1. To visualize this identification strategy using between-cohort variations, Figure 4-1 plots the average years of schooling for all individuals born between 1935 and 1968 using data collected in the first wave (in 1989) of the China Health and Nutrition Survey (discussed in more detail in the Data section below), separately for urban (Panel a) and rural residents (Panel c). It can be seen that despite an increasing trend in educational attainment, a drop occurred to both urban and rural individuals born in the second half of the 1940s. A similar pattern is

[77] See also Ichino and Winter-Ember (2004) and Meng and Gregory (2007) for examples.

also shown by Deng and Treiman (1997: Figure 1). Figure 4-1, panels (b) and (d), plots the corresponding fitted average years of schooling after running regressions using Equation (4). It can be seen clearly that, the treatment cohorts completed significantly fewer years of schooling than the control groups after controlling for a time trend. The difference between the (mean) educational attainment of the treatment groups and that of the control groups provides the source of between-cohort variation in individuals' educational attainment, which is needed to identify β_1.

Viewed differently, as can be seen in panels (b) and (d) in Figure 4-2, the deviation from the time trend creates a discontinuity structure in the time trend of the mean educational attainment. Thus, the estimation of equation (4) in the first-stage regression can be viewed as an application of the regression discontinuity (RD) design (Angrist and Pischke 2009: pp. 259) to identify β_1.[78] Given the discontinuity structure in the mean educational attainments across cohort groups, the robustness of the estimate of β_1 using cohort dummies as IVs can be checked using the sample of individuals born around the cut-off points (one at the late 1940s the other at the late 1950s) of such discontinuity structure.

4.4. Data

4.4.1. Survey

The data set used in this essay comes from the China Health and Nutrition Survey (CHNS) project, an ongoing international collaborative project between the Caroline

[78] It should be pointed out that the discontinuity structure in educational attainment created by the Cultural Revolution is a fuzzy RD, as opposed to a sharp RD, because the Cultural Revolution affected the *mean* (or expected) educational attainment for each cohort group, but not necessarily each individual's actual educational attainment. Some individuals, for example, might have attended school earlier or later and thus received shocks that are different from the shocks received by other individuals of the same age.

Population Center at the University of North Carolina at Chapel Hill and the National Institute of Nutrition and Food Safety at the Chinese Center for Disease Control and Prevention. The survey covers nine provinces, including Liaoning, Heilongjiang, Jiangsu, Shandong, Henan, Hubei, Guangxi and Guizhou. These provinces vary substantially in geography, economic development, public resources, as well as healthcare service conditions (see Figure 4-2 for the geographical distribution of these provinces). A multistage, random cluster process was used to draw the sample surveyed in each of the provinces. Currently, there are about 4,400 households in the overall survey, covering approximately 20,000 individuals. Information is collected from households, communities and health/family planning facilities. The first wave of data collection took place in 1989, and data have been collected from the same households in subsequent six waves in 1991, 1993, 1997, 2000, 2004, and 2006. From 1997 onward, households that emerged from the original households were added to the survey, and new households from the original sites were added to replace households who were no longer participating in the survey.

4.4.2. Sample

The main sample of children used in this essay consists of children aged 16 or younger at first the time they were interviewed. This essay computes HAZ scores for these children using growth standards created by the World Health Organization (WHO).[79] The WHO recommends age 19 as the adult cut-off point for underweight and wasting, but the cut-off point of stunting (i.e. HAZ<0) is unclear. This essay chooses the

[79] Growth standards for children 5-19 years old were made available in 2007; they are closely aligned with the original growth reference data for children 0-5 years old.

upper bound to be age 16, simply because individuals above 16 are considered “adults” in China, and are allowed to work legally. In the analysis below, all the children whose HAZ scores are greater than 6 or less than -6 were dropped, because their heights are likely to be measured with a substantial amount of error. Note that given the panel data structure of CHNS, each child was usually interviewed more than once. Thus, the CHNS data collected more than one height-for-age score for most of the children. The panel structure of the CHNS data is not helpful in identifying β_1, though, because there is insufficient variation in the educational attainment of the same mother over time.[80] Also, to avoid linking multiple HAZ scores of the same child with his or her mother’s education, this essay uses only the first height-for-age score observed in the CHNS survey.

Table 4-5 provides definitions and summary statistics of the key variables used in the regression analysis in the next section. Two features of the table are worth noting. First, Chinese children are undernourished (HAZ < 0) on average. This is not surprising given the fact that China is ranked 2nd in the total number of undernourished people.[81] Second, mothers in the treatment groups (Column 3) completed fewer years (1.8 for urban mothers and 1.6 for rural mothers) of education than mothers in the control groups (Column 4), and their children are shorter than the children of mothers’ in the control groups. Was the difference between child heights caused by the difference in mother’s education, then? The next section attempts to answer this question.

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[80] Han, Suen and Zhang (2009) suggested the possibility of education re-investment behaviors for individuals who encountered education interruptions during the Cultural Revolution, which implies that there might be variation in mother’s education over time. However, an earlier version of the present essay shows that education re-investments are mostly conducted by urban men who participated in the CHNS survey. The majority of urban women, rural men and women participated in the CHNS survey did not conduct such investments. Results are available upon request.

[81] The Economist, December 29, 2004. “Hunger.” http://www.economist.com/node/3523810 (Accessed on October 22)

4.5. Empirical Results

This section reports the main empirical findings of this research. Since the mechanisms of the education interruptions were different for urban and rural individuals, regressions are run separately for urban and rural children. Later, comparisons will be made to achieve more insights.

4.5.1. First-stage results

First-Stage Results indicate the impact of the Cultural Revolution on the educational attainment of different cohorts. To avoid unnecessary digression, this section reports only results that are relevant to the purpose of this essay, based upon the data on mothers.[82] Table 4-6 reports these first-stage regression results.

The results reported in Column (1) are of main interest here. They report the regression results using the entire sample of mothers whose children were no more than age 16 in all CHNS surveys, respectively for urban mothers and rural mothers, using cohort dummies CR_g (i.e. treatment dummies) as instrumental variables (i.e., **Z** in Equation (3)). These results convey two important messages. First, these CR cohort dummies are all strongly significant at the 1% level, providing strong predictive power for *MoEdu* beyond that of all other explanatory variables (discussed below). This implies that they are good candidates for suitable IVs, provided that these cohort dummies are exogenous (see the discussion in Section 3 above). Second, these columns indicate that on average mothers who encountered education interruptions suffered from a reduction in educational attainment of over two years. Figure 4-3, visualizes these results (Panel A for

[82] An earlier version of this essay analyzes the impacts of the Cultural Revolution on individuals' educational attainment, across gender groups and residential localities (i.e. urban areas versus rural areas). Results are available upon request.

urban mothers, and B for rural mothers), imposing the fitted regression lines (solid) over the actual mean years of education (dashed). The fitted lines capture reasonably well the ups and downs in the actual mean years of education, especially the drops occurred to both urban and rural mothers born in the second half of the 1940s.

4.5.2. Second-stage results

Table 4-7 reports the regression results of estimating Equation (1), for urban children aged 16 or younger, using a variation of specifications (results for rural children are reported in Table 4-8). Columns (1)-(3) report OLS results and columns (4)-(7) report 2SLS results. All specifications include a polynomial function of children's birth year and a polynomial function of mother's birth year to control for the time trend in child height and the effect of mother's age on child health. The general pattern of the results in Table 4-7 is that the estimated effects from the 2SLS method are at least twice as large as those from the OLS method.

Turn first to Panel A in Table 4-7, using the sample of all urban children aged 16 or younger interviewed in all waves. Column (1) includes *MoEdu* as the only explanatory variable. This column shows a significantly positive impact of mother's years of schooling on urban children's HAZ scores. To see whether mother's education captures any influence of local economic and/or medical conditions, a set of community dummies are added in column (2).[83] The estimated impact of mother's education remains almost unchanged. To see whether mother's education captures any influence of mother's

[83] For the urban sample, community refers to *city* (*county seat*) of residence in 1989. This is because public service facilities are supposed to serve all residents in the same city (*county seat*). For the rural sample, in contrast, community refers to *village*. This is because rural residents were much more spread apart, and thus public facilities in one village may not be accessible for residents who reside in another village.

genetic endowment, mother's height (measured in centimeters) and per capita household income are added to Column (3) as a very crude check.[84] The coefficient on mother's education still remains very close to that in Column (1). The results indicate that one additional year of mother's education is associated with a 0.06 standard deviations' increase in urban children's height. Such estimate is similar to the findings of a recent study by Chen and Li (2009).

Columns (4)-(7) report results using Two-Stage Least-Squares (2SLS) regressions including all explanatory variables included in Column (3). Column (4) estimates equation (1) using the cohort dummies (**CR**) as IVs. This column indicates that one additional year of mother's education leads to an increase of 0.17 standard deviations in child height. The magnitude of this impact is about three times as large as that found in OLS regressions. However, there are several concerns of the validity of the IVs (**CR**) used in this column. First, the F-statistic of testing the joint significance of IVs in the first-stage is only 7.35. Although significant at any conventional level, it is somewhat below the rule-of-thumb value 10. This might cause bias in the 2SLS estimate. Second, these cohort dummies might capture some effects on child health beyond the effects via mother's education. For example, the cohort dummy for individuals born in 1959-61 might pick up some effects of the Great Chinese Famine (1959-61) other than the effects of the Cultural Revolution.[85] Even though the control groups were carefully chosen, the

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[84] Although mother's height reflects partly mother's health and genetic endowments and income may capture partly endowments related to cognitive ability that is not captured by mother's education, they measure these endowments with error. In fact, whether these endowments can be measured in survey data is open to question.

[85] This is not a major concern for urban individuals, because the Great Chinese Famine was mainly a disaster in rural areas (Lin and Yang 2000). Note that although the Great Chinese Famine significantly affected the health status of rural individuals who were exposed to in it early childhood (Chen and Zhou 2007), the impact of the famine on rural individuals' educational attainment via its impact on health seems very small. Chen and Zhou (2007) estimate that individuals whose early childhood was exposed to the

extent to which such potential problems can be reduced is unclear. To address these problems, another IV, which reflects only the impact of Cultural Revolution on educational attainment, namely, the expected years of interruptions (see Table 4-1, Column (9), for details), is used in Column (2). The result is very similar to that reported in Column (1). Note that the F-statistic for this instrument (14.70) is larger than the rule-of-thumb number. The first-stage result using this instrument is shown in Table 4-6, Column (2).

Another check is done by examining the "discontinuity" sample. Recall that the identification condition in this essay relies on the discontinuity structure caused by the events in the Cultural Revolution (see Figures 4-1 and 4-3). Column (6) excludes two cohort groups, i.e. CR_2 (born in 1948-50) and CR_3 (born in 1951-55), from the regression. The effect of maternal education becomes slightly smaller (0.13) when the treatment dummies are used as IVs, but is still significant at 5% level. The result using the discontinuity sample with expected years of interruption (Column 7) yields a smaller impact of mother's education (0.12), yet it is still larger than the OLS estimates.

Next, turn to the possible issue of sample selection due to migration (Panel B). As mentioned in the Data section above, starting from the 1997 CHNS wave, new households were added to replace households who no long participated in the survey. These newly added households might include some households who migrated to the

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famine are 3 centimeters (equivalent to 0.4-0.5 standard deviations of the height distribution of a population of healthy adults) shorter than individuals born after the Chinese Famine. Zhao and Glewwe (2010) estimate that a one standard deviation reduction in standardized child height led to a 0.2-year reduction in educational attainment of rural children in China. Combing these estimates, one can roughly estimate that the Famine would have reduced the educational attainment of rural individuals born in 1959-61 by about 0.08-0.1 years. This estimate is similar the one found in Meng and Qian (2006). Yet This is quite small compared to the results shown in Table 4-6, column (5), for CR_g (i.e. born in 1959-61). Thus, one can conclude that most of the reduction in educational attainment of this cohort is due to school interruptions during the Cultural Revolution, not the Famine.

urban areas since the mid-1990s. To reduce the potential impact of this problem, Panel B in Table 4-7 restricts the sample to those who were interviewed only in the first three waves, i.e. 1989, 1991, and 1993, before large scale migration occurred.[86] The regression results reported in this panel are similar to the ones reported in Panel A. It can be seen in this panel that the estimates are very similar across columns when the sample was restricted to be the first three waves. The impact of mother's education reduces somewhat (0.16), but is significant at 5% level in columns (1) and (2) when the entire sample (as opposed to the discontinuity sample) is used. The results using the discontinuity sample are quantitatively similar, although not as significant.

In summary, the 2SLS estimates of the impact of mother's education on the health of urban children seem quite similar across different samples and instruments used, which supports the interpretation that the estimates indicate a causal relationship between mother's education and child height. The preferred specification (Panel A, Column (4)), where more children were included in the analysis, indicates that one additional year of a mother's education leads to a 0.17 standard deviation increase in an urban child's height (conditional on child sex and age).

Table 4-8 repeats all exercises done in Table 4-7, this time for rural children aged 16 or younger. The pattern of results is very similar to that found in Table 4-8: The 2SLS results are generally larger than the OLS results, and are very consistent using different instruments and different sample restrictions. The preferred specification (Panel a,

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[86] Large scale rural-to-urban migration did not occur until the early 1990s (Naughton 2007). In fact, China's urban economy did not start its rapid growth until after 1992, when Deng Xiaoping made his famous trip to southern China and delivered his speeches on speeding up economic development. Thus, there might not be strong incentives for rural individuals to migrate to the urban areas by 1993. In this sense, the vast majority of households interviewed between 1989 and 1993 should reside in the same communities in which they were born.

Column (4)) indicates that one additional year of a mother's education leads to a 0.14 standard deviation increase in a rural child's height (standardized by child sex and age).

Another issue, for both urban and rural children, is the potential impact of the One-Child policy adopted in 1979. This policy might have affected mothers' childrearing behaviors, as it might change the trade-off between child quality and quantity. This essay addresses this problem in two ways. First, the trade-off between child quality and quantity implies that the number of children in the family will affect each child's quality. Yet the effect of mother's education remains similar when the variable "number of children" is added, and the number of children is itself insignificant in this regression (results not shown). Another check is to add a dummy variable indicating a child was born after 1979 in the regression. However, adding this variable does not change the results much. Note that the variable "number of children" could be endogenous in both the urban and rural samples. This is because the number of children that a mother has given birth to is likely to be correlated with her educational attainment. Also note that the variable "born after 1979" might be endogenous for urban children, but not for rural children. This is because urban children's years of birth might be affected by the Cultural Revolution. A study by Zhou and Hou (1999), for example, shows that the send-down experiences had greatly affected the educated youths' life-course events, such as the marriage age. However, this does not apply to the rural parents, who were not part of the sent-down campaign.

Since rural children's years of birth are unlikely to be affected by the Cultural Revolution, the impact of the One-Child policy can be further investigated by restricting one's attention on children born after 1979, especially pre-school children (i.e. aged 6 or younger).[87] Table 4-9 reports results for the sample children analyzed in Table 4-8, excluding those who had reached school age when first interviewed. The results are very similar regardless of the sample being analyzed. For example, Column (1), using cohort dummies (**CR**) as instruments, yields an estimate of 0.17 of the impact of mother's education on child height, which is similar to that for all children under 16 (Table 4-8, Column (4)). The results in Table 4-9 are also very similar across different samples, although statistical significance is reduced due to the smaller sample.

4.5.3. Discussion

The above analysis provides persuasive evidence of the existence of a causal effect of maternal education on child height (standardized by sex and gender). Now one is in the position of comparing results obtained using different samples (e.g. urban versus rural) and different econometric methods (e.g. OLS versus 2SLS) to interpret these results. First, the impacts of urban mother's education (Table 4-7) are slightly higher than those of rural mothers (Table 4-8). This is not surprising, because urban women tend to acquire more years of schooling than rural women. To the extent that local economic/public health conditions were appropriately controlled for (by using community fixed effects), the impacts of mother's education are likely realized through mother's ability to process and understand information on health (e.g. Thomas, Strauss and Henriques 1991; Glewwe 1999). Since functional literacy and numeracy are usually not achieved through fewer than four years of school (Simmons 1976), the levels of education completed by

[87] This also allows one to examine the issue of individual differences in growth patterns, which can also be checked by using the sample of rural children under age 6. Including older children, e.g. who had reached puberty, might introduce individual differences in growth patterns (e.g. Mangyo 2008). Schooling might also affect child health.

more-educated urban women will likely be more useful in child rearing. Yet what should be noted is that the impacts of urban mother's education and rural mother's education are indeed very similar in magnitude.[88] This suggests that the identification strategy used in this essay is valid: different first-stage mechanisms yield similar second-stage results.

Second, the estimates obtained from 2SLS regressions are larger than those obtained from OLS regressions. This is also what Currie and Moretti (2003) and Chou et al. (2010) found using school construction events as natural experiments.[89] A simple explanation is that mother's education is measured with error, which leads to attenuation bias in the estimates of its impact on child health. More importantly, it is likely that the education acquired during the Cultural Revolution was relevant to child rearing. During the Cultural Revolution, promotion decisions based on school entrance exams were completely abolished, and the knowledge required for these entrance exams were no longer taught in schools. Instead, more practical knowledge and skills were taught in schools during the Cultural Revolution (Unger 1982; Pepper 1996). Such practical knowledge might be more useful for rearing children. In fact, Zhang, Liu and Yung (2007) find that the returns to education for the cohorts whose education was interrupted are greater than that for the cohorts who did not experience education interruptions, even when the impacts of genetic endowment was controlled for using a sample of urban twins

[88] This comparison also emphasizes the role of rural women's education in terms of child health. Monetary returns to education found in rural China remain very low (Gregory and Meng, 1995), especially compared to the rapid increase in monetary returns to education in urban China (Appleton, Song and Xia, 2005; Zhang et al. 2005). Yet the returns to rural women's education in terms of child health found here are very close to their urban counterparts, which suggests that studies focusing on the monetary returns to rural education are likely to understate the returns to rural education.

[89] In fact, Chou et al. (2010) summarize a large set of papers testing the causal effects of education on health (either the effect of parental education on child health or the effect of one's education on his or her own health), and conclude that the 2SLS results are at least as large as the OLS results. Unfortunately, due to different measures of child health used in these studies, it is impossible to compare the magnitudes of the effects of mother's education on child health across studies.

in China. Their explanation also attributes the phenomenon to the more practical knowledge learned during the Cultural Revolution.

4.6. Long Term Health Impacts of the Chinese Cultural Revolution

Using the Chinese Cultural Revolution as a natural experiment, the above analysis confirms the existence of a strong causal effect of mother's education on child health (measured by child height-for-age z scores). A question follows naturally: How much child health was lost due to the education interruptions caused by the Chinese Cultural Revolution, via the causal linkage between mother's education and child health? The recent development of the econometric methods of program evaluation (e.g. Imbens and Angrist 1994; Imbens and Wooldridge 2009) offers several candidate answers to this question. The causal relationship identified using instrumental variables (IV) generated by the Cultural Revolution measures the Local Average Treatment Effect (IV) of the Cultural Revolution on child health. This section estimates three kinds of long run impacts of education interruptions[90] based on this interpretation.

The first is the LATE estimate itself of the causal effect of mother's education on child health. It measures the average loss in child health due to the Chinese Cultural Revolution for mothers whose educational attainments were reduced by one year due to the education interruptions they experienced. Note that the variation in mother's education used to identify this effect is the deviation from the counterfactual case in the absence of education interruptions. Thus, this measure has a broader interpretation. It

[90] The long run impacts discussed here refer only to the impacts on individuals, as opposed to the society as a whole. Some effects of mother's education, e.g. taking effects via marrying better-educated husbands, affect only individual children but might not greatly affect the whole population if the supply of husbands remains fixed and if the effects of fathers' education on child health is approximately linear. Estimating the health impact of the Cultural Revolution on the entire Chinese society is beyond the scope of this essay.

estimates the loss in child health attributable to a constrained educational decision when such decision is due to an increase in constraints such as education interruptions during the Chinese Cultural Revolution. Examples include school closure events due to funding limitations (Heaton 2008), natural disasters such as earthquake and hurricane, or civil wars (e.g. Shemyakina 2009). Table 4-10, Column (1), displays this impact.

The second impact estimates the average impact of the Cultural Revolution on child health for a mother in the group that is potentially affected by the Cultural Revolution. Intuitively, this is the LATE impact multiplied by the average reduction in educational attainment suffered by the mothers who were of school age during the Cultural Revolution. Table 4-10, Column (2), displays this impact for each cohort that encountered education interruptions during the Cultural Revolution. It can be seen that the loss in child health is over 0.5 standard deviations (of the distribution of height of healthy children). To gauge such loss, consider the findings in the recent paper by Chen and Zhou (2007), which estimates the loss in health suffered by individuals whose early childhood was exposed to the China's Great Famine, 1959-61, the greatest famine in human history. It is estimated that these individuals are three centimeters shorter than individuals born after the famine. Note that three centimeters in height is equivalent to about 0.4-0.5 standard deviations in the distribution of height of a healthy adult population. Thus, assuming the loss in child height will be largely translated into the loss in adult height,[91] the loss in child height estimated in this essay (Table 4-10) implies a comparable loss in adult height due to education interruptions of one's parents that is a

[91] For example, Thomas and Strauss (1998) argue that adult height is largely determined during early child development. In fact, the correlation between height in childhood and adulthood is approximately 0.7 for both men and women (Case and Paxon 2008).

similar magnitude to the loss in height due to the exposure in one's early childhood to the greatest famine in human history.

The last impact of interest is related to possible earnings loss due to the reduction in height. Since there is no consensus on the estimation techniques and empirical findings of the monetary returns to height (e.g. Schultz 2005), and there are virtually no studies linking height and earnings for China, this impact can only be estimated in a suggestive way. An upper bound of this loss can be roughly estimated using evidence from other low income countries.[92] The highest monetary return to height in the recent review article by Schultz (2005) is the Ghana case, where an increment of one centimeter in height is associated with 6 to 8 percent increase in wages. Taking the average return to adult height in Ghana, 7%, Table 4-10, Column (10) estimates that the upper bound of the loss in earnings due to the Cultural Revolution for some cohorts could be over 20%. Again, this estimate should be treated as suggestive, rather than conclusive.

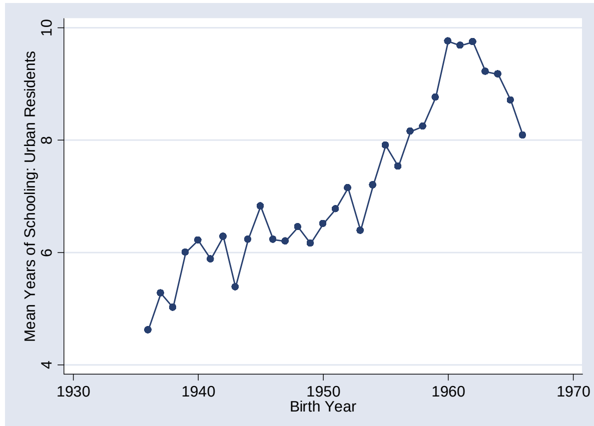
4.7. Conclusion

The coexistence of low educational attainment of Chinese women and the large number of undernourished Chinese children, together with the documented strong association between mother's education and child health, raises an important policy question: Will investments in Chinese women's education improve Chinese children's health? This essay sheds some light on this question by empirically testing the causal effect of maternal education on child height (standardized by age and sex), an indicator of long term health status, using the Cultural Revolution as a natural experiment. The results

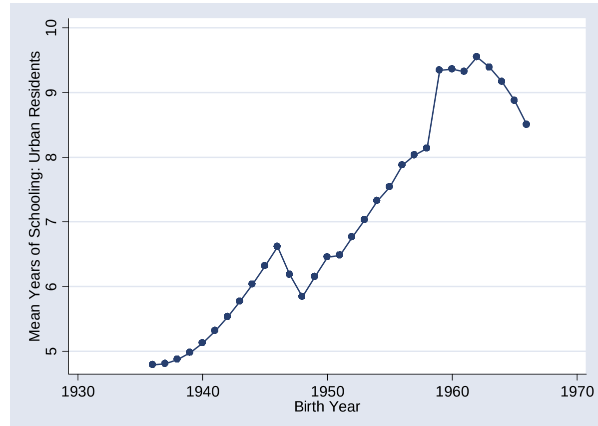
[92] The lower bound is of course zero, because the monetary returns to height are zero in some developing countries (Schultz 2005).

show that one additional year of mother's education raises child height (standardized by child age and sex) by about 0.17 standard deviations for urban children aged 16 or younger, and by 0.14 standard deviations for their rural counterparts. These results are consistent across alternative specifications and samples. Thus, the answer to the above question is clear: investments in mothers' education will significantly improve child health in China. Given the significant causal effect of mother's education on child health revealed by this research, one would expect that achieving universal completion of nine years of basic education by 2010, a goal recently set by the Chinese government, will greatly improve the health of the next generation in China in the future.

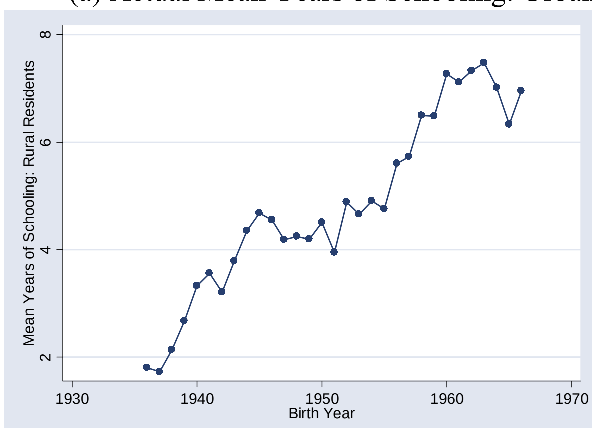
Yet, the existence of a strong causal relationship between mother's education and child health also implies that serious interruptions to women's education will have detrimental effects on their children's health. Based on the LATE-IV interpretation offered by the recent development of the econometrics of program evaluation, the above casual effects of mother's education imply a substantial loss in child health due to the Chinese Cultural Revolution. The educational loss borne by mothers whose education was interrupted by the Cultural Revolution is translated into a 0.3 standard deviations' reduction in child height. It is of a magnitude similar to the effect of being exposed in early childhood to China's Great Famine in 1959-1961, the greatest famine in human history. To this extent, the decade-long Cultural Revolution not only cost the generation in China who suffered from it at the time, but it also cost the next generation large losses in health and human capital accumulation.



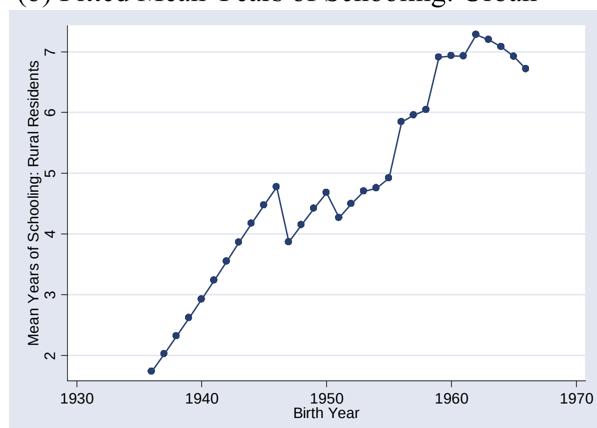
(a) Actual Mean Years of Schooling: Urban



(b) Fitted Mean Years of Schooling: Urban



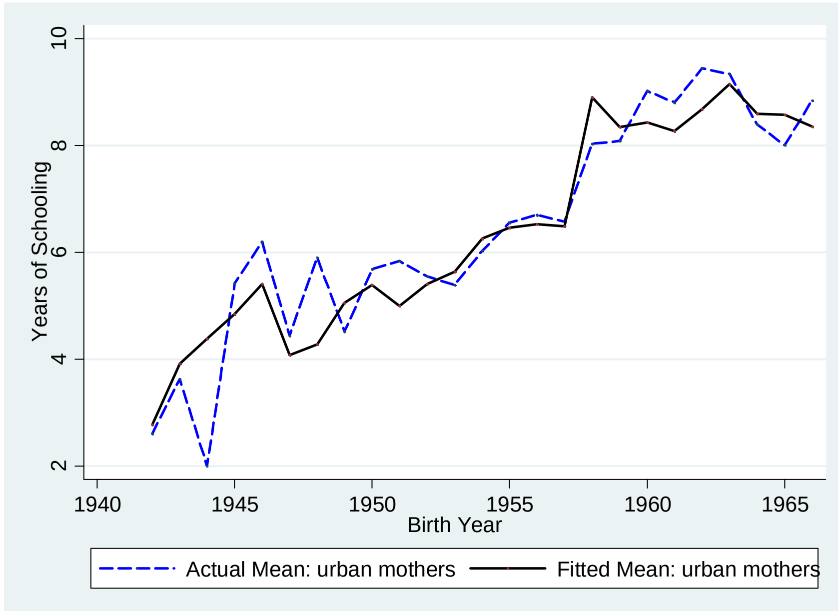
(c) Actual Mean Years of Schooling: Rural



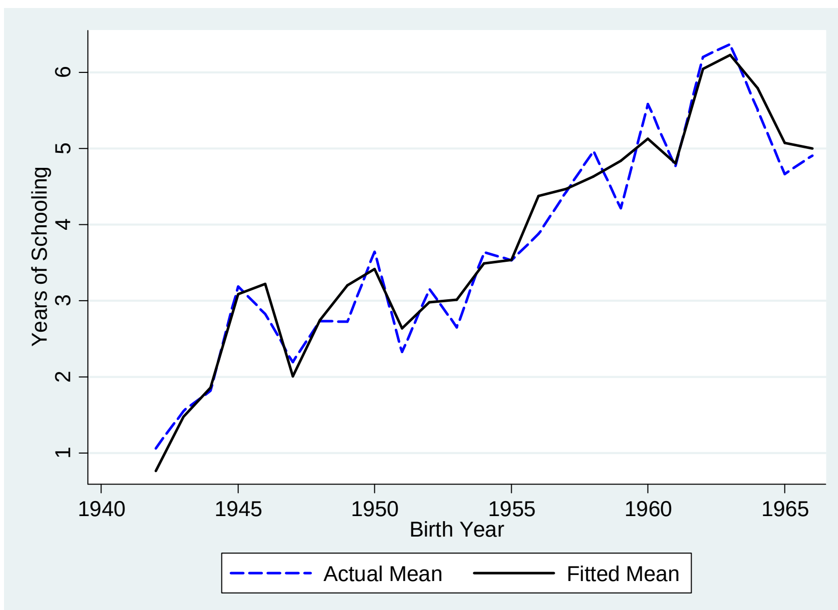
(d) Fitted Mean Years of Schooling: Rural

Figure 4-1: Mean Years of Schooling: Urban and Rural Individuals

Figure 4-2: Provinces Participating in the China Health and Nutrition Survey (CHNS)



(A) Urban Mothers



(B) Rural Mothers

Figure 4-3: Visualization of the First-Stage Regressions

Table 4-1: Expected Education Interruptions: Urban Residents Born in 1947-61

Treatment Groups	(1) Birth Year	(2) Prim. School Starting Year	(3) Low Sec. school starting year	(4) Upper Sec. school starting year	(5) Expected Years of delayed Enrollment	(6) Expected Years Interrupted in Primary School	(7) Expected Years Interrupted in Lower Sec. School	(8) Expected Years Interrupted in Upper Sec. School	(9) Expected Years Interrupted = (5) + (6) + (7) + (8)
CR_1: Delayed University Entry	1947	1954	1960	1963					
CR_2: Interrupted Upper Sec. Education	1948	1955	1961	1964				1	1
	1949	1956	1962	1965				2	2
	1950	1957	1963	1966				3	3
CR_3: Interrupted Lower and Upper Sec. Education	1951	1958	1964	1967			1	3	4
	1952	1959	1965	1968			2	3	5
	1953	1960	1968	1970	2		1	3	6
	1954	1961	1968	1970	1	1	2	3	7
	1955	1962	1968	1970		2	3	3	8
CR_4: Interrupted Lower and Upper Sec. Education	1956	1963	1969	1971		3	3	1	7
	1957	1964	1970	1972		3	2	1	6
	1958	1965	1971	1973		3	1	1	5
CR_5: Interrupted Primary Education	1959	1968	1973	1976	2	1			3
	1960	1968	1973	1976	1	1			2
	1961	1968	1973	1976		1			1

Note. 1. This table assumes (1) an urban child started schooling at age 7; (2) schools were reopened uniformly in Fall 1968; and (3) every child had the potential to attend senior high school. The number of years interrupted in Column (9) is calculated as the horizontal sum of the numbers in columns (5)-(8). 2. The 1953 and 1954 birth cohorts also suffered from school interruption at the primary education level. But the major interruptions occurred at their secondary education level.

Table 4-2: Specialized and Agricultural Middle Schools in China, 1957-65

	Specialized		Agricultural	
Year	Schools	Students	Schools	Students
1957	1,320	777,939	--	--
1958	3,113	1,470,000	20,023	1,999,900
1959	3,706	1,469,613	22,302	2,189,900
1960	6,225	2,215,869	22,579	2,302,000
1961	2,843	1,203,017	7,260	611,700
1962	1,514	534,911	3,715	266,600
1963	1,355	451,360	4,303	307,800
1964	1,611	531,557	15,108	1,123,400
1965	1,265	547,447	61,626	4,433,400

Data Source: Pepper (1996): p.285.

Table 4-3: Secondary Schools in China, 1963-81

	Lower Secondary Schools Only			**Upper Secondary and Complete Secondary Schools**		
Year	**Cities**	**County Towns**	**Rural/Commune**	**Cities**	**County Towns**	**Rural/Commune**
1963	2,594	2,899	9797	1,414	2,408	481
1965	--	--	--	1,315	2,193	604
1971	--	--	--	863	1,479	11,819
1973	1,927	2,174	63,858	5,139	4,301	19,925
1975	1,809	2,450	80,126	6,170	5,015	27,935
1976	1,941	3,670	126,006	7,008	5,734	47,793
1977	1,883	3,217	131,265	7,610	6,377	50,916
1978	2,699	3,328	107,103	7,106	6,106	36,003
1981	3,395	3,442	75,434	6,069	5,951	12,427

Data Source: Pepper (1996): p.425 and p. 487.

Table 4-4: Expected Education Interruptions: Rural Residents Born in 1947-61

Definitions of Cohort Groups	(1) Birth Year	(2) Primary School Starting Year	(3) Lower Secondary School Starting Year	(4) Interrupted Primary Education	(5) Interrupted Lower Secondary Education	(6) Expected Years of Interruption = (4) + (5)
CR_1: Post-GLF crisis at Lower Sec. Education	1947	1954	1960		2	2
	1948	1955	1961		3	3
	1949	1956	1962	2	2	4
	1950	1957	1963	3	1	4
CR_2: Post-GLF crisis at Prim. ; Agric. Middle School Closure at Lower Sec. level	1951	1958	1964	3	1	4
	1952	1959	1965	3	2	5
CR_3: Post-GLF crisis at Prim. ; Agric. Middle School Closure at Lower-Sec. Level	1953	1960	1966	3	3	6
	1954	1961	1967	3	2	5
	1955	1962	1968	3	1	4
CR_4: Prim. Education Interrupted in 1966-68; 1969 Education Reform at Lower-Sec. Level	1956	1963	1969	3		3
	1957	1964	1969	3		3
	1958	1965	1970	3		3
CR_5: Prim. Education Interrupted in 1966-68; 1969 Education Reform at Prim. Level	1959	1966	1971	3		3
	1960	1967	1972	2		2
	1961	1968	1973	1		1

Note. This table assumes: (1) rural children started schooling at age 7; and (2) every child had the potential to attend junior high school. The variable "expected years of interruption" includes years during the post-GLF crisis in 1961-63, years when agricultural middle schools were closed, i.e. years exposed the chaotic years in 1966-68. (3) The 1946 birth cohort experienced education interruption from the post-GLF crisis in 1961 at the third year of lower secondary level, but they were also exposed to the school expansion in 1958-59. This cohort is included in the control group.

Table 4-5: Variable Definitions and Summary Statistics for Children under Age 16

Variables	(1) Definition	(2) Whole Sample (Mothers born in 1942-66)	(3) Treatment Groups (Mothers born in 1947-61)	(4) Control Groups (Mothers born in 1942-46 or 1962-66)
A: Urban				
HAZ	Child height-for-age z-scores	-1.153	-1.234	-1.004
		(1.454)	(1.336)	(1.640)
Girl	Dummy, =1 if the child is female	0.473	0.491	0.442
		(0.499)	(0.500)	(0.497)
Child Age	Child age measured in months	8.82	10.01	6.549
		(5.12)	(4.66)	(5.11)
Mother's Education	Mother's years of schooling	7.213	6.591	8.324
		(4.035)	(4.205)	(3.445)
Mother's Height	Mother's height in centimeters	156.37	155.95	157.13
		(5.924)	(6.082)	(5.551)
Mother's Age	Mother's age measured in years	31.83	34.25	27.50
		(5.83)	(3.70)	(6.41)
Household Income	Per capita household income in *yuan*	2008.86	1709.56	2470.563
		(2672.57)	(1866.68)	(3426.94)
A: Rural				
HAZ	Child height-for-age z-scores	-1.392	-1.473	-1.238
		(1.409)	(1.268)	(1.635)
Girl	Dummy, =1 if the child is female	0.479	0.478	0.482
		(0.500)	(0.500)	(0.500)
Child Age	Child age measured in months	8.61	9.81	6.31
		(5.16)	(4.76)	(5.13)
Mother's Education	Mother's years of schooling	4.417	3.864	5.480
		(3.732)	(3.704)	(3.552)
Mother's Height	Mother's height in centimeters	154.87	154.49	155.62
		(5.871)	(5.947)	(5.646)
Mother's Age	Mother's age measured in years	32.81	34.75	29.07
		(6.10)	(3.75)	(7.77)
Household Income	Per capita household income in *yuan*	922.13	812.23	1096.91
		(1007.79)	(802.27)	(1235.42)

Note. 1. One US dollar = 8.70 *yuan* in 1994.
2. Mean values are reported, standard deviations in parentheses.

Table 4-6: First-Stage Regression Results:
Dependent Variable: Mothers' Years of Schooling

	(1)	(2)	(3)	(4)
Sample	Children in All Waves		Children in 1989, 1991, and 1993 Waves	
A: Urban Mothers				
CR_1: Born 1947	-2.105***		-2.778***	
	(0.903)		(0.934)	
CR_2: Born 1948-50	-2.024***		-2.443***	
	(0.740)		(0.775)	
CR_3: Born 1951-55	-2.847***		-3.430***	
	(0.658)		(0.712)	
CR_4: Born 1956-58	-2.936***		-2.967***	
	(0.544)		(0.609)	
CR_5: Born 1959-61	-1.143***		-1.190***	
	(0.380)		(0.452)	
Expected Years		-0.287***		-0.335***
Interrupted		(0.059)		(0.063)
R^2	0.33	0.32	0.34	0.34
Sample Size	1,969	1,969	1,548	1,548
B: Rural Mothers				
CR_1: Born 1947-50	-1.535***		-1.684***	
	(0.367)		(0.390)	
CR_2: Born 1951-52	-2.750***		-2.901***	
	(0.421)		(0.453)	
CR_3: Born 1953-55	-2.935***		-3.192***	
	(0.412)		(0.446)	
CR_4: Born 1956-58	-2.200***		-2.257***	
	(0.380)		(0.409)	
CR_5: Born 1959-61	-1.726***		-1.733***	
	(0.318)		(0.339)	
Expected Years		-0.399***		-0.435***
Interrupted		(0.069)		(0.074)
R^2	0.39	0.43	0.42	0.44
Sample Size	2,606	2,606	2,261	2,261

Note. 1. All regressions include child gender, a high-order polynomial function of child age, a high-order polynomial function of mother's birth year, mother's height and a set of community dummies.
2. Robust standard errors, clustered at community level, are in parentheses.
3. * significant at 10%; ** significant at 5%; *** significant at 1%.
4. Definitions of CR_g cohorts differ for rural mothers and urban mothers. See Tables 4-1 and 4-4 for definitions.

Table 4-7: Two-Stage Least Squares (2SLS) Results of Impacts of Mother's Education on Child Height (HAZ): Urban Children under Age 16

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Specification	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Sample		Whole Sample		Whole Sample		"Discontinuity" Sample	
IV	--	--	--	CR Cohort Dummies	Expected Years Interrupted	CR Cohort Dummies	Expected Years Interrupted
A: Children in All Waves							
MoEdu	0.069***	0.063***	0.053**	0.170***	0.171**	0.134**	0.116*
	(0.012)	(0.011)	(0.014)	(0.068)	(0.060)	(0.065)	(0.059)
Comm. FE.	N	Y	Y	Y	Y	Y	Y
Obs.	1,969	1,969	1,969	1,969	1,969	1,227	1,227
F-test of joint sig. of IV(s) in 1st-Stage	--	--	--	7.35***	14.70***	13.34***	20.99***
				(0.0000)	(0.0000)	(0.0000)	(0.0000)
R^2	0.11	0.14	0.16	--	--	--	--
B: Children in 1989, 1991, and 1993 Waves							
MoEdu	0.070***	0.067***	0.057***	0.167**	0.159**	0.143*	0.152
	(0.014)	(0.016)	(0.014)	(0.071)	(0.068)	(0.075)	(0.095)
Comm. FE.	N	Y	Y	Y	Y	Y	Y
Obs.	1,458	1,458	1,458	1,441	1,441	853	853
F-test of joint sig. of IV(s) in 1st-Stage	--	--	--	6.69***	16.11***	10.57***	13.70**
				(0.0000)	(0.0000)	(0.0000)	(0.0000)
R^2	0.11	0.14	0.14	--	--	--	--

Note. 1. All regressions include child gender, high-order polynomials of mother's birth year and child age, and a set of community (city) dummies. Columns (3)-(7) also include mother's height and household income as regressors while columns (1)-(2) do not. 2. Robust standard errors, clustered at community level, are in parentheses. 3. * significant at 10%; ** significant at 5%; *** significant at 1%. 4. The discontinuity sample in columns (6) and (7) excludes cohorts CR_2 (born in 1948-50) and CR_3 (born in 1951-55) from the whole sample

Table 4-8: Two-Stage Least Squares Results of Impacts of Mother's Education on Child Height (HAZ): Rural Children under Age 16

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Specification	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Sample	Whole Sample			Whole Sample		"Discontinuity" Sample	
IV	--	--	--	CR Cohort Dummies	Expected Years Interrupted	CR Cohort Dummies	Expected Years Interrupted
A: Children in All Waves							
MoEdu	0.049***	0.028***	0.028***	0.142**	0.141*	0.163	0.160
	(0.008)	(0.010)	(0.014)	(0.068)	(0.082)	(0.123)	(0.148)
Obs.	2606	2606	2606	2606	2606	1442	1442
F-test of Joint Sig. of IV(s) in 1st-Stage	--	--	--	11.51***	32.15***	11.75***	12.13***
				(0.0000)	(0.0000)	(0.0000)	(0.0000)
R^2	0.08	0.14	0.16	--	--	--	--
B: Children in 1989, 1991, and 1993 Waves							
MoEdu	0.046***	0.027**	0.037***	0.128**	0.135*	0.137	0.154
	(0.010)	(0.011)	(0.009)	(0.059)	(0.080)	(0.103)	(0.137)
Obs.	2268	2268	2253	2253	2253	1174	1174
Joint F-test of IV(s) in 1st-Stage	--	--	--	11.92***	32.23***	12.34***	12.10***
				(0.0000)	(0.0000)	(0.0000)	(0.0000)
R^2	0.03	0.14	0.17	--	--	--	--

Note. 1. All regressions include child gender, high-order polynomials of mother's birth year and child age, and a set of community (village) dummies. Columns (3)-(7) also include mother's height and per capita household income as regressors while columns (1)-(2) do not. 2. Robust standard errors, clustered at community level, are in parentheses. 3. * significant at 10%; ** significant at 5%; *** significant at 1%.4. The discontinuity sample in columns (6) and (7) excludes cohorts CR_3 (born in 1951-55) and CR_4 (born in 1956-58) from the whole sample.

Table 4-9: Two-Stage Least Squares Results of Impacts of Mother's Education on Child Height (HAZ): Rural Children under Age 6

	(1)	(2)	(3)	(4)
Specification	2SLS	2SLS	2SLS	2SLS
Sample	Whole Sample	Whole Sample	"Discontinuity" Sample	"Discontinuity" Sample
IV	CR Dummies	Exp. Years Interrupted	CR Dummies	Exp. Years Interrupted
A: Children in All Waves				
MoEdu	0.166**	0. 165	0.128	0.158
	(0.083)	(0.114)	(0.086)	(0.128)
Obs.	1134	1134	848	848
F test of joint Sig. of	7.46***	22.69***	9.18***	13.04***
IV(s) in 1st-Stage	(0.0000)	(0.0000)	(0.0000)	(0.000)
B: Children in 1989,1991,1993 Waves				
MoEdu	0.162**	0.171	0.129	0.123
	(0.078)	(0.117)	(0.080)	(0.122)
Obs.	1039	1039	774	774
F test of joint Sig. of	7.10***	20.03***	6.26***	13.76***
IV(s) in 1st-Stage	(0.0000)	(0.0000)	(0.0000)	(0.0000)

Note. 1. All Regressions include child gender, mother's height, high-order polynomial functions of mother's birth year and child age, per capita household income and a set of community (village) dummies.
2. Robust standard errors, clustered at community level, are in parentheses.
3. * significant at 10%; ** significant at 5%; *** significant at 1%.
4. The discontinuity sample in columns (6) and (7) excludes cohorts CR_3 (born in 1953-55) and CR_4 (born in 1956-58) from the whole sample.

Table 4-10: Long Run Impacts of Education Interruptions during the Chinese Cultural Revolution

	(1)	(2)	(3)
	Impact I: One year impact on child health	Impact II: Total impact on child health due to the Cultural Revolution	Impact III: Impact related to potential earnings
A: Urban Sample			
CR Cohorts	0.173		
CR_1: Born 1947		0.364	0.15
CR_2: Born 1948-50		0.350	0.15
CR_3: Born 1951-55		0.493	0.21
CR_4: Born 1956-58		0.508	0.21
CR_5: Born 1959-61		0.198	0.08
Weighted Average		0.431	0.18
B: Rural Sample			
CR Cohorts	0.142		
CR_1: Born 1947-50		0.218	0.09
CR_2: Born 1951-52		0.391	0.16
CR_3: Born 1953-55		0.417	0.17
CR_4: Born 1956-58		0.312	0.13
CR_5: Born 1959-61		0.245	0.10
Weighted Average		0.327	0.14

Note. 1. Numbers in Column (1) are from Column (4) in Tables 4-7 and 4-8.
2. Numbers in Column (2) are obtained by multiplying the numbers in Column (1) by the coefficients on each cohort dummy in Table 4-6, Columns (1) and (5).
3. Column (3) assumes that one standard deviation reduction in child height due to the Cultural Revolution will be translated into one standard deviation reduction in adult height, and that the monetary return to 1 centimeter in height is 7%.

Chapter 5. Conclusions

This dissertation empirically investigates issues regarding the intergenerational transmission of human capital in China. The first essay examines the impact of family size, a potential barrier to investments in children's education, on parental monetary investments in children's education. Exploiting the temporary relaxation in China's one-child policy between the mid-1980s and the late 1980s, which allowed rural couples to have a second child if the first-born was a girl, this essay creates instruments for family size from the sex-composition of the first two children in a family. It finds that an exogenous increase in family size, in particular, from two to three, will increase parental investments in children's education by 40-50%. This suggests allowing rural parents to have one more child, which has been recently proposed by many researchers, is unlikely to induce them to reduce their investments in each child. In addition, this essay finds that parental education is a key determinant of parental investments in children's education.

Yet, can parental education be translated into better child educational outcomes via these investments? Much of the effort in the second essay is devoted to investigating the role parental education plays in the acquisition of academic skills for children in rural China. To obtain credible estimates of the impact of parental education, this essay applies a number of econometric methods to control for unobserved child ability, a potential confounding factor. It finds that parental education has a significant impact on child academic skills, even after controlling for child ability. This essay also finds that the context of rural China offers important insights into the role of parental education in a poor economy. First, the extremely low level of mother's education, suppressed by traditional Confucian thought and the low economic returns to education in rural China, disguises the true impact of mother's education. Mother's education has a significant impact on child academic skills only when it reaches the four-year threshold for functional literacy and numeracy. This implies that fruitful intervention programs should be designed not only to attract girls to school, but also to keep them in school long enough. Second, not only can education be transmitted intergenerationally, but the gender gap in education can also be transmitted intergenerationally. Mother's education is found to increase only girls' academic skills, and thus girls of poorly-educated mothers suffer more from the gender gap in academic skills. To help these girls escape the gender gap, many more investments in boosting rural women's educational attainment are clearly needed.

A related question is: If mother's education matters greatly for children's educational outcomes, does it also matter for other aspects of children's human capital? The third essay sheds some light on this question by empirically testing the causal effect of maternal education on child health (measured by child height, standardized by age and sex), using the Chinese Cultural Revolution (1966-76) as a natural experiment. The results show that one additional year of mother's education raises child height (standardized by child age and sex) by about 0.17 standard deviations for urban children aged 16 or younger, and by 0.14 standard deviations for their rural counterparts. In short, mother's education does matter for child health.

In conclusion, rural China has a pressing need, as well as much room, for improvements in rural education. Given the significant causal effect of mother's education on child human capital outcomes revealed by this dissertation, one would expect that achieving universal completion of nine years of basic education by 2010, a goal recently set by the Chinese government, will greatly improve the well-being of the next generation in China in the future. One would expect to see improvements in children's performance at school, reductions the gender gap in child academic skills in rural China, as well as better child health outcomes in China as a whole.

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Appendix

Table 2A-1: Impacts of Family Size on Parental Investments on Children's Education; Individual Level Results (Sample: n ≥2)

Specification	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Family size	-0.698***	-0.649***	-0.639***	-0.262	-0.218	-0.259
	(0.104)	(0.101)	(0.102)	(0.560)	(0.543)	(0.565)
2nd born	1.523***	1.501***	1.502***	1.280***	1.266***	1.296***
	(0.139)	(0.136)	(0.136)	(0.350)	(0.319)	(0.339)
3rd born	2.446***	2.415***	2.418***	1.828**	1.821**	1.897**
	(0.236)	(0.232)	(0.231)	(0.827)	(0.769)	(0.802)
4th born	3.338***	3.256***	3.263***	2.301	2.255*	2.385*
	(0.374)	(0.374)	(0.373)	(1.403)	(1.315)	(1.369)
5th born	3.147***	3.032***	2.996***	1.625	1.564	1.703
	(0.785)	(0.777)	(0.785)	(2.106)	(2.044)	(2.034)
Girl	-0.220**	-0.217**	-0.217**	-0.269***	-0.264***	-0.259**
	(0.087)	(0.086)	(0.086)	(0.103)	(0.096)	(0.102)
ChAge	-0.095*	-0.106**	-0.107**	(0.175)	-0.183*	(0.175)
	(0.050)	(0.051)	(0.050)	(0.117)	(0.110)	(0.115)
ChEdu	0.695***	0.704***	0.705***	0.687***	0.696***	0.698***
	(0.044)	(0.044)	(0.044)	(0.045)	(0.049)	(0.044)
FaEdu		0.021	0.019		0.017	0.016
		(0.014)	(0.013)		(0.014)	(0.014)
MoEdu		0.043***	0.045***		0.043***	0.045***
		(0.014)	(0.014)		(0.014)	(0.014)
HHinc		0.392***	0.389***		0.440***	0.431***
		(0.071)	(0.070)		(0.102)	(0.093)
Land		-0.040	-0.038		-0.022	-0.022
		(0.026)	(0.026)		(0.037)	(0.033)
FaAge			-0.001			-0.003
			(0.014)			(0.013)
MoAge			-0.039**			-0.041**
			(0.019)			(0.019)
FaHand			-0.068			-0.045
			(0.191)			(0.188)
MoHand			-0.120			0.088
			(0.225)			(0.229)
FaBio			0.630			0.645
			(0.477)			(0.490)
MoBio			-0.295			-0.297
			(0.314)			(0.306)
Grandparents			-0.110			-0.164
			(0.311)			(0.329)

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Table 2-A1 (Continued): Impacts of Family Size on Parental Investments on Children's Education; Individual Level Results (Sample: n ≥2)

Specification	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Mean(ChEdu)	0.010	-0.075	-0.069	0.029	-0.061	-0.056
	(0.063)	(0.064)	(0.063)	(0.067)	(0.068)	(0.063)
Var(ChEdu)	0.046**	0.039*	0.039*	0.040*	0.033	0.034
	(0.021)	(0.021)	(0.020)	(0.024)	(0.022)	(0.022)
Mean(ChAge)	-0.495***	-0.457***	-0.420***	-0.431***	-0.393***	-0.362***
	(0.067)	(0.067)	(0.069)	(0.108)	(0.099)	(0.113)
Var(ChAge)	-0.078***	-0.075***	-0.067***	-0.086***	-0.083***	-0.074***
	(0.014)	(0.014)	(0.014)	(0.017)	(0.015)	(0.017)
Mean(Girl)	0.335**	0.384**	0.403**	0.148	0.213	0.255
	(0.164)	(0.162)	(0.160)	(0.290)	(0.268)	(0.267)
Var(Girl)	-0.069	-0.055	-0.056	0.087	0.093	0.081
	(0.411)	(0.401)	(0.401)	(0.399)	(0.363)	(0.388)
Excluded instruments				Boy-boy Boy-girl	Boy-boy Boy-girl	Boy-boy Boy-girl
Partial R-squared of excluded IVs				0.026	0.025	0.026
F-stat of joint sig. of excluded IVs				42.04***	39.53***	41.81***
Hansen's J-stat				0.021	0.116	0.072
(p-value)				(0.884)	(0.733)	(0.788)
Observations	3,232	3,224	3,224	3,231	3,223	3,223
R-squared	0.299	0.312	0.315			

Note. 1. Standard errors clustered at the village level are reported. 2. *** significant at 1% level; ** significant at 5% level; * significant at 1% level.

Table 3A-1: Results of the First-stage Regressions (M4)
Dependent variable = Standardized CAT scores

Specification	(1)	(2)	(3)	(4)
CDR in father's birth year	-0.001	-0.001	-0.001	-0.001
	(0.010)	(0.011)	(0.010)	(0.011)
CDR in mother's birth year	-0.032***	-0.032***	-0.032***	-0.033***
	(0.007)	(0.008)	(0.008)	(0.009)
Father born in 1957			0.008	-0.063
			(0.065)	(0.075)
Father born in 1958	0.364**	0.426**	0.357**	0.400**
	(0.149)	(0.166)	(0.154)	(0.168)
Father born in 1959	0.133	0.148	0.131	0.137
	(0.188)	(0.201)	(0.191)	(0.203)
Father born in 1960	0.051	0.060	0.0441	0.054
	(0.335)	(0.353)	(0.335)	(0.355)
Father born in 1961			-0.065	-0.034
			(0.162)	(0.167)
Mother born in 1957			-0.091	-0.251
			(0.201)	(0.218)
Mother born in 1958	0.615***	0.537***	0.608***	0.529***
	(0.177)	(0.197)	(0.179)	(0.201)
Mother born in 1959	0.462	0.365	0.458	0.361
	(0.283)	(0.335)	(0.281)	(0.330)
Mother born in 1960	1.002***	0.935***	0.988***	0.966***
	(0.255)	(0.295)	(0.265)	(0.301)
Mother born in 1961			0.0224	0.102
			(0.160)	(0.171)
FaEdu	0.002	0.003	0.003	0.003
	(0.006)	(0.007)	(0.006)	(0.007)
MoEdu	0.020**	0.018**	0.019**	0.017**
	(0.008)	(0.008)	(0.008)	(0.008)
FaFarmjob		-0.015		-0.015
		(0.048)		(0.047)
MoFarmjob		0.051		0.059
		(0.113)		(0.112)
FaBio		0.583***		0.582***
		(0.183)		(0.184)
MoBio		0.001		0.002
		(0.074)		(0.075)

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Table 3-A1 (Continued): Results of the First-stage Regressions (M4)

Dependent variable	Standardized CAT scores	Standardized CAT scores	Standardized CAT scores	Standardized CAT scores
Specification	(1)	(2)	(3)	(4)
FaHandicap		0.216*		0.212*
		(0.114)		(0.116)
MoHandicap		-0.234**		-0.241**
		(0.096)		(0.097)
FaAge		-0.004		-0.003
		(0.007)		(0.007)
MoAge		0.008		0.009
		(0.008)		(0.009)
Log (Pcexp)		0.043		0.045
		(0.040)		(0.041)
Log (Pcland)		-0.061		-0.062
		(0.067)		(0.068)
CompMid		0.018		0.003
		(0.158)		(0.160)
CompHigh		0.003		-0.017
		(0.146)		(0.147)
CompCol		0.057		0.042
		(0.143)		(0.141)
Female	0.066	0.024	0.067	0.024
	(0.044)	(0.049)	(0.042)	(0.048)
HAZ		0.043**		0.043**
		(0.018)		(0.018)
OldSib		0.024		0.027
		(0.039)		(0.039)
YoungSib		0.026		0.027
		(0.038)		(0.038)
Age	0.093	0.077	0.089	0.068
	(0.109)	(0.107)	(0.111)	(0.110)
Age^2	-0.020	-0.016	-0.018	-0.013
	(0.030)	(0.030)	(0.031)	(0.031)
R^2	0.044	0.062	0.044	0.064
Obs.	1,649	1,508	1,649	1,508

Note. 1. Standard errors in parentheses; adjusted for within-school correlation.
2.*** Significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 3A-2: Variable Definitions and Summary Statistics (M5)

Variable	Variable Definition	Mean	Std. Dev.	Min	Max
FaFaEdu	Years of education of father's father	3.26	4.03	0	16
FaMoEdu	Years of education of father's mother	0.87	2.35	0	16
FaOldSib	Father's number of older siblings	1.96	1.77	0	10
FaYoungSib	Mother's number of younger siblings	2.04	1.52	0	10
Fa1stChild	Dummy, = 1 if the child's father is the first child in his family	0.25	0.43	0	1
MoFaEdu	Years of education of mother's father	2.97	3.73	0	15
MoMoEdu	Years of education of mother's mother	0.91	2.30	0	15
MoOldSib	Father's number of older siblings	2.03	1.80	0	10
MoYoungSib	Mother's number of younger siblings	2.00	2.00	0	8
Mo1stChild	Dummy, = 1 if the child's mother is the first child in her family	0.22	0.42	0	1