ESSAYS IN EMPIRICAL DEVELOPMENT ECONOMICS

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ESSAYS IN EMPIRICAL DEVELOPMENT ECONOMICS

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This dissertation is a set of three independent essays on empirical development economics, with a focus on China. The first chapter examines the effect of maternal education on infant health by exploiting exogenous variation in women's exposure to the wholesale closure of rural high schools immediately after the Cultural Revolution, from 1977 to 1984. The second chapter explores the long-term effect of prenatal exposure to the 1978-84 land reform on academic performance, as captured by college entrance exam scores. The third chapter examines the effect of land reform on the sex ratio imbalance by comparing the sex of the second child between families with a first girl and those with a first boy before and after the reform.

BIOGRAPHICAL SKETCH

Shuang Zhang is a PhD student in Department of Economics at Cornell University. She has a BA degree in Economics from Shanghai University of Finance and Economics and a MA degree in Economics from Fudan University, in China. Her research interests are development, health and education, with a focus on China.

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CHAPTER 1

MOTHER'S EDUCATION AND INFANT HEALTH: EVIDENCE FROM CLOSURE OF HIGH SCHOOLS IN CHINA

1.1 Introduction

Maternal education could affect infant health by increasing household income or directly improving health behaviors. A better educated woman earns more in the labor market and is also more likely to marry a man with more schooling and higher earnings. An increased household income would enable a woman to have better access to health care services and spend more on health inputs, especially during pregnancy. Because better educated women may acquire more health information or process it better, they may use health inputs more efficiently or have better allocations of these inputs (Grossman, 1972; 2006). More educated women are also more likely to avoid risky behaviors. For example, an additional year of schooling induced by college openings was found to decrease maternal smoking by over 30 percent (Currie and Moretti, 2003).

The robust positive association between maternal education and child health has been widely documented (e.g., Strauss and Thomas, 1995; Gakidou at al., 2010) and has motivated policy-makers to promote female education in developing countries (World Bank, 1993). Such education policies will be effective in improving infant health to the extent that maternal education has a causal effect on child health, which has been debatable due to possibly omitted variables, such as a woman's predetermined socioeconomic status. Studies that exploit various school expansion programs as exogenous sources of a decline in the costs of schooling find a positive effect of maternal education on infant health

in the US, Indonesia and Taiwan (Currie and Moretti, 2003; Breierova and Duflo, 2004; Chou et al. 2010). In contrast, Lindeboom et al. (2009) using changes in British compulsory schooling laws do not find an effect, and McCray and Royer (2011) comparing women born just before and just after the school entry date in the US also show no effect of maternal education on infant health. Thus, the design-based literature offers mixed findings.[1] A missing part of this literature is that, little is known about whether education interventions targeting higher levels of schooling in developing countries, e.g., high school, could improve infant health.[2]

In this paper, I provide new evidence on the effect of maternal education on infant health by exploiting the largest negative supply-side shock in history: the nationwide closures of rural high schools in post-Mao China, which induced a sharp increase in the costs of high school education for the rural population. Immediately after the death of Mao Zedong in 1976, an education reform was initiated in 1977 to close rural secondary schools built during the Cultural Revolution, with the most acute reductions occurring in high schools (Pepper, 1996). With an official justification of better serving the economic reform, the education reform completely reverted the priorities of the Cultural Revolution to concentrate education resources on selected urban schools. Figure 1 shows that the total number of high schools decreased by more than 80 percent between 1977 and 1983; the decline was larger for rural high schools, the target of the closure movement. A distinct feature of these closures is that they were not driven by the change in the overall demand for high schools. Figure 2 shows that both overall population and rural population aged 15-18 had been increasing from

[1] These studies use different education interventions or policies targeting at different margins of schooling, and the subpopulation affected by each intervention is different.

[2] In the 2000 Census of China, for example, only 8 percent of the rural population had ever completed high school.

1977 to 1983.

The identification strategy focuses comparisons on rural women born quarters apart but who had different exposures to the closures of high schools within county. The variation in women's exposure status across narrowly defined cohorts comes from two sources. First, when the closures of high schools started in a given county, individuals aged 18 who had just graduated from high school were relatively unexposed, while those aged 17 were more exposed to the shock. The second source of variation is generated by the school entry date policy. From 1967 to the late 1970s, one should be 7 years old at the beginning of the spring semester, usually in February or March, to start primary school. Individuals born in the first quarter went to schools one year earlier and thus graduated one year earlier, which would allow them to avoid the closures, compared to those born in the second quarter of the same year. Consequently, a sharp decline in high school completion is expected for individuals at age 17 years and 9 months (age 17.75 hereafter) by the first quarter in the first year of the closures. Assuming women born quarters apart are otherwise similar, I can compare high school completion and infant health between women just above and just below age 17.75 within county. The relationship between maternal education and infant health is assessed by relating the differences in infant health to the differences in high school completion induced by the closures.

To perform these comparisons, I have collected unique data on the year the closures of rural high schools started by county from two-thirds of all county gazetteers ever published in which local history was recorded from 1949 to the 1980s. While the central decision to close rural high schools applied to every county, the timing of the closures varied by county from 1977 to 1984. These

data are combined with the 1992 Chinese Children Survey microdata, a national representative survey that covers a wide range of infant health outcomes and parental characteristics. The analysis sample includes 400 counties that were rural at the time of the closures and that are matched in both of these data. The matched data allow me to assign the exposure status to each rural woman by her age in the first quarter of the first year of the school closures, and then examine the effects of her exposure to the closures on educational attainment and infant health.

There are four main reasons the identification strategy provides credible evidence on the causal effect of maternal education on infant health. First, the identifying assumption on the similarity of women on either side of age 17.75 within county is plausible because their parents were unlikely to strategically plan births to avoid the shock. The closure decision was made shortly after the end of Mao's era and was unpredictable in more than ten years ago. Moreover, the school entry date was changed from September 1st to the spring semester in 1967, before which year women around 17 at the time of the closures had been born. Due to this policy change, women born in the first quarter are not more planned than those born in the second quarter.[3] Second, this assumption is also testable by examining the continuity of predetermined familial characteristics. I provide evidence that these women and their parents on either side of the age cutoff are similar along many predetermined dimensions. Third, to confound the effects of the high school closures, other post-Mao reforms should have closely followed the timing of the school closures by county and have had differential impacts on women just older and just younger than 17.75 in the first quarter of the reform year. I provide evidence to rule out two possibly alterna-

[3]The criticism on An grist and Kruger (1991) regarding seasonality of birth (Bound et al., 1995) is less of a concern in this study. Also, quarter of birth fixed effects are controlled for.

tive hypotheses: the 1978-84 land reform and closures of rural middle schools after the Cultural Revolution. Finally, internal migration had been under strict control in China from the 1950s until the first relaxation in 1985 (Wang, 2005). A natural concern about endogenous mobility induced by policy changes is less important for the school closures between 1977 and 1984.

Consistent with expectations for educational attainment, the results show a sharp drop in high school completion for women at age 17.75 in the first quarter of the first year of the school closures. The decline of 2.7 percentage points from 12 percent at age 18 suggests that, 22.5 percent of women on the margin were affected by the negative shock. Estimates are precise, indicating a strong first stage. Similar to McCray and Royer (2011), I find negligible effect of a woman's exposure to the closures on her selection into motherhood and maternal age at first birth, reducing concerns about endogenous sample selection. Using the discontinuous change in high school completion induced by the closures, I do not find strong evidence that one more year of maternal high school education improves infant health, as measured by prematurity, low birthweight, neonatal mortality and infant mortality. Lastly, I examine the income channel, as captured by labor market outcomes and associative mating in the marriage market, and potential changes in health behavior, measured by prenatal care. I find no evidence to support both channels.

Interpreting my findings requires considerations of the research design in this paper. First, the estimated effects are specific to a subpopulation of women affected by the high school closures. When local rural schools were closed, parents could send their children to urban schools in the same county to continue schooling. Because the closures of rural schools increased the costs of complet-

ing high school, my estimates are most relevant to women from low socioeconomic background who were more likely to comply with the closures. Second, in the narrow comparisons among women on either side of age 17.75, the margin of maternal education affected by the closures is one more year in high school, which accounts for half of the high school education at that time,[4] and a high school diploma.

The principle contribution of this paper is in its ability to uniquely isolate the effect of maternal high school education on infant health by exploiting the unexpected and massive school closures in rural China. While the existing quasi-experimental studies in developing countries focus on interventions targeting lower levels of schooling (Breierova and Duflo, 2004; Chou et al., 2010), this study is among the first to identify the effect of maternal education at the higher end of the education distribution.

Moreover, the identification strategy, as well as the particular context in China, allow me to separate possible mechanisms through which maternal education could affect infant health, which in turn casts doubt on two arguments in this literature. The first argument is that maternal education improves infant health through women's learning in school. However, it is very difficult to disentangle the effect of learning from staying in school longer and thus less likely to be pregnant under age 18, which could also improve infant health (Royer, 2004). Primary school or middle school opening programs could be different from interventions at the exit of high school in terms of separating these two channels. For example, in Breierova and Duflo (2004), women's exposure to the primary school construction program reduced the number of children born before age 15. It is unclear whether the improvement in infant health is due

[4]The years of schooling in high school had been 2 years from the 1960s until 1983.

to women's actual learning or the endogenous reduction in teenage pregnancy. In contrast, in this paper using closures of high schools, women on the margin were very close to 18. I therefore do not expect an effect on teenage pregnancy, and indeed, I do not find an effect.

The second argument is that maternal education could directly improve health behavior (Grossman, 1972). Empirically, it is also not easy to separate direct change in health behavior from income change, as they are often closely related. Using microdata from a 1992 survey, when China was still under the transition from a planned economy to a market oriented economy, I would expect that the income channel is very weak. My findings on labor market outcomes are consistent with previous studies that find zero or even negative earnings returns to education in the same period (Gregory and Meng, 1995; Li and Hang, 1998). This is a unique setting where separating these two channels becomes possible. If one more year in high school could improve maternal health behavior, even without an income channel, we would still expect a positive effect on infant health. However, we do not.

The rest of this paper is organized as follows. Section 2 introduces the background of the closures of rural high schools in China and discusses the assignment of treatment status. Section 3 describes the county-level data on the timing of the closures and the microdata on mothers and children. Section 4 presents the identification strategy. Section 5 presents results by outcomes of interests. Robustness checks are presented in Section 6. The last section concludes.

1.2 The Closures of Rural High Schools

First introduced in this section are the closures of rural high schools right after the Cultural Revolution from the late 1970s through the early 1980s. I then discuss key institutional information for determining one's exposure status to the school closures within county, which directly guides the identification strategy in this study. Lastly, using data I collected on the timing of the school closures by county and county characteristics prior to the shock, I provide quantitative evidence for understanding the regional variation in the timing of the closures.

1.2.1 Historical accounts on the closures of rural high schools

In May 1966, Mao Zedong launched the Cultural Revolution, a decade-long social movement that brought a radical agenda to politics and education policies. An essential goal of the Cultural Revolution was to eliminate differences between peasants and the remainder of the population (Deng and Treiman, 1997). The revolution in the education system was to deliberately destroy the old "elite education system" and construct a new "mass education system" (Pepper, 1996). Colleges were closed from 1966 to 1972 and college entrance examinations were eliminated for an entire decade. No formal teaching was carried out and all levels of schools stopped recruiting new students in cities from 1966 to 1968 (Meng and Gregory, 2002). Education policy priorities shifted to peasant youth by popularizing middle schools and expanding high schools in the countryside.[5] To speed up the expansion at a lower cost, junior high classrooms were attached to primary schools and senior high classrooms were added to middle

[5]See Andreas (2004), Han (2001), Pepper (1996) and Thogersen (2002) for details of the school expansion in rural areas during the Cultural Revolution.

schools in rural communes, which enabled rural children to go to secondary schools within walking distance (Han, 2001; Thogersen, 2002). In the rural population, the middle school enrollment rate increased from 34 percent in 1965 to 75 percent in 1976 and the high school enrollment rate rose from 9 percent to 62 percent (Ministry of Education, 1984).

Soon after Mao's death in September of 1976, the post-Mao shift in ideology in the Chinese Communist Party led to a complete reversal of education policies during the Cultural Revolution (Pepper, 1996). An education reform to reconstruct the education system was proposed by Deng Xiaoping in 1977 (Ministry of Education, 1984). Immediately, a college entrance examination was reintroduced at the end of that year. Education priorities were changed from expanding secondary education in the countryside to developing a few urban-based and quality-oriented secondary schools in urban areas, which concentrated the best teachers and resources for training the most talented students (Ministry of Education, 1984).

The decision to close rural secondary schools built during the Cultural Revolution was made by the central government (Pepper, 1996). The cutbacks were most severe at the high school level, for which the national enrollment rates in 1981-1982 were down by two thirds from those in 1978.[6] The official justification of closing rural high schools was that, to best serve economic development, high schools should be college preparatory, and because only a small proportion of students were able to enter college, it was unnecessary to keep a large enrollment rate in high schools, particularly in rural areas where students hardly had a chance to compete (Pepper, 1990).[7] Urban teachers who were sent to teach

[6]Ganging Daily, Oct. 12th, 1981.

[7]In Lia Hui county in Hebe province, education officials also argued that "The commune high schools were eliminated to concentrate forces to run one good "keypoint" school in the

in rural high schools during the Cultural Revolution and the send-down urban youth returned to cities after 1976, which also accelerated the closures of rural schools (Pepper, 1996).[8] At the local level, from 1977 to the early 1980s, county governments gradually started to close rural high schools, and the closures usually continued for two or three years, until only one or a few high schools were kept in the urban area of every county.[9]

A natural concern is related to education quality of these rural schools built during the Cultural Revolution. Compared to education in other time periods, the regular curricula in high schools during the Cultural Revolution were shortened and political education was given a higher weight. On the other hand, urban teachers were sent to teach in rural schools, and some of the urban send-down youth also taught in rural schools. They might provide better teaching quality than rural teachers. Moreover, Andreas (2004) argues that, while "better quality" in education reform referred to a higher promotion rate from high schools to colleges, rural high schools built during the Cultural Revolution were to develop practical curricula that were more relevant for employment and income growth in the countryside. Whether there is an improvement in education quality after the Cultural Revolution is also not clear. In Li et al (2005) using a twins sample in cities in 2005, they find zero earnings return to high school education alone, while there is sizable positive return to college education. Overall, the concern about changes in education quality in high school remains an empirical question and requires data on individual earnings to test.

county town. We set our sights on the college examinations and took the best teachers to the only one high school. The aim was to produce most talented students who could pass the college examinations" (Andreas, 2004).

[8]The "send-down" movement during the Cultural Revolution resulted in one out of every three urban adolescents (most aged 16–19), approximately 17 million youths, being sent to the countryside for manual labor for from 1 or 2 to as many as 10 years during the period 1966-76 (Li et al., 2010).

[9]The chapter on secondary education from various issues of county gazetteers.

1.2.2 Determining one's exposure status within county

one's exposure to the closures of high schools depends on one's year of birth, quarter of birth and the first year of the closures in the county of birth. Three pieces of institutional information are crucial to assign one's exposure status within county.

First, who were affected by the closures of rural high schools? Little is known about individual exposure to the shock from existing studies on this particular event (Pepper, 1996; Andreas, 2004). To answer this question, I conducted interviews with local teachers who taught in high schools in the late 1970s in three counties of Shaun province. All of the interviewees recalled that when rural high schools were closed, most students simply dropped out, even though they were allowed to continue in the few urban high schools in town. Teacher Lou said,

> At the beginning, the high school in town became very crowded as students from rural communes came in, but many of them disappeared after a month or two. They stopped coming because the travel and boarding costs were too high for rural families and students had to walk for two or three hours to school. While it was the proximity of high schools in local communes that made rural children able to go during the Cultural Revolution, the only high school in town was not a real alternative for most students living in rural areas.

The important message is that individuals who just graduated from high school by the first year of the closures were able to avoid the shock, yet the students in

their final year of high school were the oldest affected.

To determine the age of the oldest cohorts exposed to the school closures, the second piece of information is age at graduation from high school from the late 1970s to the early 1980s. The minimum entry age of primary schools was set at 7 in 1951.[10] From the mid 1960s to 1982, students spent five or six years in primary schools, two or three years in middle schools and two years in high schools.[11] The years of schooling in primary schools or middle schools could vary across counties, or even vary across schools within the same county because some schools were chosen to implement trials that involved shortened years of schooling.[12] Therefore, age at graduation from high school was not universal and varied from 16 to 18 at the time of the closures. Although the exact number of years in schools is not observed either at the county level or at the individual level, I can compare individuals at 18 in the first year of the closures who mostly had graduated with those at 17 who were the oldest ones at risk of dropout when the negative shock arrived.

Lastly, the school entry date policy enables me to narrow down these comparisons among individuals born quarters apart. The school entry date was in the spring semester, usually in February or March, from 1967 to the late 1970s.[13] Individuals born in the first quarter were more likely to go to school one year earlier and thus graduate one year earlier, compared to those born in the other three quarters of the same year. Because graduation was in January, no matter in which month the first few schools were closed, those at 18 by the first quarter in the first year of the closures would mostly have graduated, while those

[10] *China Education Yearbook: 1949-1981.*

[11] *China Education Yearbook: 1949-1981.*

[12] *Major Educational Events in People's Republic of China: 1949-1982.*

[13] *Major Educational Events in People's Republic of China: Primary Education; Major Educational Events in People's Republic of China: Secondary Education.*

born in the other three quarters of the same year were still likely to be in the final year and at risk of dropout. Thus, the finest comparisons are essentially among individuals born in the first and second quarter of the same year in the same county. A common concern is that parents might strategically plan births to ensure an earlier school entry date of their children. It is unlikely in this setting because the school entry date changed from September 1st to the spring semester in 1967, and individuals around 17 from the late 1970s to the early 1980s were born before this policy change.

Putting it all together, within county, the oldest cohort exposed to the closures of high schools were 17 years and 9 months in the first quarter of the first year of the closures. For example, in counties that closed schools in 1979, individuals born in the first quarter of 1961 were 18 and had just graduated by the first quarter of 1979, while those born in the second quarter of the same year were 17.75 and were the oldest affected.

1.3 Data

Two core datasets are used in this study. One is unique data on the timing of the school closures by county that I collected from county gazetteers. The other is the 1992 Chinese Children Survey conducted by the State Statistics Bureau of China and UNICEF, a nationally representative microdata on child health and parental characteristics.

1.3.1 County-level data on the timing of the closures

The timing of the closures of rural high schools is recorded on county gazetteers. Following a thousand-year long tradition of recording local history, the most recent collection of county gazetteers that compile county records from 1949 to the 1980s have been published since 1985 (Vermeer, 1992). County gazetteers document local events and statistics about geography, politics, economy and culture, which are originally from official archives and policy documents of county governments (Xue, 2010). In the chapter on education, records are very rough for the ten years of the Cultural Revolution, which keeps me from exploring the expansion of rural schools. In contrast, records in the post-Mao period are more available and detailed, which are valuable for studying the closures of rural high schools.[14]

I have collected the year each county started to close rural high schools from 1247 county gazetteers, which represent 68 percent of all county gazetteers ever published in the recent collection. The other 32 percent do not report the year of closure, or some of them report "the late 1970s" or "the early 1980s", which are not useful for the identification strategy. I plot the percentage of counties that started to close rural high schools from 1977 to 1984 by the solid line in Figure 3. While a very small proportion of counties closed schools as early as 1977 or after 1981, the vast majority of county governments made the closure decisions between 1978 and 1981. More than half of the counties had closed rural high schools by 1979 and over 96 percent had closed by 1981. There are 400 rural counties in 27 provinces matched by using these county-level data with the 1992 Chinese Children Survey microdata. To define a rural sample, I

[14]These records are also more reliable because they are not under the restriction of "be rough, not in detail" that is placed on recording politically sensitive periods.

use the administrative code in the 1982 Census to exclude cities and districts in 1982, which is closest to the time of the closures.[15] The dashed line in Figure 3 shows that these matched counties are representative of all counties collected in the distribution of the timing.

1.3.2 1992 Chinese Children Survey

The 1992 Chinese Children Survey (the 1992 CCS hereafter) covers 522,371 households from 1088 counties in 29 provinces. Although information from both parents is available, this study focuses on mothers for two reasons. First, the mother's education has been found to have a larger effect on infant health than the father's (Chou et al., 2010). Second, the data quality for women is much better than that for men in the 1992 CCS.[16] In my analysis sample of women, more than 97 percent were married, but only 70 percent of them could be matched to their husbands by unique personal id.

For every individual, the highest level of completed education is reported, while years of schooling are not observed. As discussed in Section 2, years of schooling in primary schools and middle schools varied in the 1960s and 1970s. It is therefore not possible to construct a consistent measure on one's years of schooling from the reported education level. I measure maternal education by high school completion, the margin directly affected by the closures of high schools and that I can precisely measure for the whole sample. Notably, the years of schooling in high school had been 2 years from the 1960s until 1983

[15] An administrative unit is defined as a county if the last two digits of the county code in 1982 is between 21 and 80 (definition of the State Bureau of Statistics).

[16] More than 80 percent of the survey forms were answered by women.

when it was changed to 3 years.[17] In the narrow comparisons among individuals on either side of 17.75 in the first quarter between 1978 and 1981, their years of schooling in high school did not change due to the 1983 policy change.

In the 1992 CCS, every woman who had ever been pregnant since 1976 reported a complete pregnancy history for every birth and detailed birth outcomes (yet not restricted to births after 1976). I use four measures of poor infant health that are commonly studied in the related literature. The first one is the incidence of prematurity because premature infants are at higher risk of death in the first year, especially in the first month. Because gestational age is reported in completed months, I define prematurity as gestational age less than 9 months. I also examine neonatal mortality and infant mortality rates. The last measure is low birthweight, which is widely used to measure poor infant health and is linked to later life consequences.[18] If low birthweight is affected by maternal education, we would expect long-term intergenerational effects as well. The limitation of the last measure is that only 36 percent of mothers reported the birth weight of their first birth (in grams). Low birthweight is defined as less than 2500 grams in this subsample.

Because one's county of birth is not observed in the 1992 CCS, I match the microdata with the county-level data on the timing of the school closures by using a woman's county of residence in 1992. One might be concerned that a woman's county of birth might not be her county of residence in 1992 if she migrated. The concern is less important because internal migration had been under strong restrictions by the early 1990s.[19] In the 1990 Census, the migration rate among rural women in the birth cohorts of my analysis sample is as low as

[17] *Major Educational Events in People's Republic of China: Secondary Education.*
[18] See Almond et al. (2005) for a review on this literature.
[19] See a detailed discussion on internal migration in China in Section 4.2.

5 percent .

In the sample of 400 matched counties, a woman's exposure status is assigned by her year of birth, quarter of birth and the timing of the school closures. The analysis sample includes rural women at 12-23 by the first quarter in the first year of the closures, who were 24 quarters above 17.75 and 24 quarters below. They were born between 1953 and 1970 and at 22-39 in 1992. Among all women in the sample, 92 percent of them had ever been pregnant since 1976 and are therefore in the sample of mothers.

The sample of births for analyzing infant health includes the first births. My findings are comparable with those in other quasi-experimental studies using the sample of first Born's (Currie and Moretti, 2003; McCray and Royer, 2011). Few other restrictions are made on the sample of births except for a minor one. Because some birth outcomes of nonsingleton births, for example, birth weight and infant mortality rate, could be very different from those of singleton births, I exclude nonsingleton births (1.41 percent).[20]

The summary statistics of women and mothers in the sample are presented in Table 1, as well as tabulations for the first births and for all births. There are 7.3 percent of women who have completed high school and only 0.17 percent have a college degree, suggesting that women with a high school education are at the very high end of the education distribution among these cohorts. Compared to all women in the sample, mothers are slightly less likely to complete high school and to work off-farm. The marriage rates in both samples are very high, which are 97 and 99.96 percent, respectively. Infant health measures of first births do not differ much from those in the sample of all births. Compared

[20]I also find that having multiple births is unrelated to a woman's exposure to the school closures.

to all births, the first births have slightly lower birth weight but are less likely to experience neonatal or infant death. First-time mothers are slightly more likely to have prenatal visits, especially in the first trimester, and to deliver their births in hospital.

1.4 Empirical Framework

1.4.1 Identification strategy

Assuming a woman's decision to become a mother is unaffected by her exposure the school closures, the identification strategy exploits the fact that mothers on either side of 17.75 by the first quarter of the year the school closures started had different exposures to the closures within county. The first stage compares high school completion of mothers and the reduced form examines infant health of the first births. The difference in high school completion induced by the closures is related to differences in infant health outcomes to quantify the causal effect of maternal education on infant health. The identifying assumption is that mothers on either side of 17.75 within county are similar along all dimensions except for their distinct exposures to the school closures.

To assign one's exposure status, I first define one's cohort as one's age in quarter by the first quarter in the first year of the closures in one's county of birth in equation (1):

$$age_i = T_j - y + \frac{1-q}{4}, \tag{1.1}$$

where i indexes individual, j county of birth, y year of birth and q quarter of birth. T_j indicates the first year of the school closures in one's county of birth. Recall that individuals at 17.75 were the oldest at risk of being exposed to the closures of high schools, while those at 18 had just graduated and were able to avoid the shock.

A discontinuous drop in high school completion is expected at 17.75. Consider the first stage equation:

$$\begin{aligned} S_i &= \alpha + \beta 1\{age_i \leq 17.75\} + f(age_i)\gamma + f(age_i) * 1\{age_i \leq 17.75\}\delta \\ &+ \theta_q + \lambda_j + \phi_j * [f(age_i) + f(age_i) * 1\{age_i \leq 17.75\}] + \epsilon_i \end{aligned} \tag{1.2}$$

where S_i is equal to 1 if mother i born in county j, year y and quarter q completed high school and is 0 otherwise. The indicator variable $1\{age_i \leq 17.75\}$ is equal to 1 when the mother was at 17.75 or younger by the first quarter in the first year of the closures in her county of birth and 0 otherwise. The analysis sample includes Prue-exposure cohorts at 18-23 (as integers) and post-exposure cohorts at 12-17. $f(.)$ is a flexible polynomial function of age_i. As suggested by the national trend of school supply in Figure 1, the closures of high schools is expected to induce both a decline in the mean level and a trend reversal of high school completion at the individual level. Thus, all specifications include the age polynomial function interacted with the indicator, $1\{age_i \leq 17.75\}$, to allow for a change in slope at 17.75. I include quarter of birth effect, θ_q, to account for any seasonal differences. The coefficient of interest, β, captures the difference in the probabilities of completing high school on either side of 17.75.

To perform cross-cohort comparisons within county, I control for county fixed effects, λ_j, and county specific quadratic trends, $\phi_j * [f(age_i) + f(age_i) *$

$1\{age_i \leq 17.75\}]$, which also allow for a change in the slope of each county specific trend at 17.75. By absorbing heterogeneity in time invariant county characteristics and Prue-existing county trends, this approach is essentially comparing women born in the first and second quarter of the same year but who had different exposures to the closures in the same county. Thus, the interpretation of β is the average departure at 17.75 from county specific trends of high school completion.

The reduced form on infant health is expressed in equation (3):

$$\begin{aligned} Y_i \quad &= \alpha + \rho 1\{age_i \leq 17.75\} + f(age_i)\gamma + f(age_i) * 1\{age_i \leq 17.75\}\delta \\ &+\theta_q + \lambda_j + \phi_j * [f(age_i) + f(age_i) * 1\{age_i \leq 17.75\}] + \epsilon_i \end{aligned} \tag{1.3}$$

where Y_i includes infant health measures of the first child of mother i. The coefficient of interest, ρ, measures the effect of a mother's exposure to the school closures on infant health. If maternal education improves infant health, ρ is expected to be positive for each of the four measures of poor infant health, including prematurity, low birthweight, neonatal mortality and infant mortality. The effect of female high school education on infant health could be captured by dividing the reduced form estimate ρ by the first stage estimate β. I also perform an instrumental variables estimation using the indicator variable, $1\{age_i \leq 17.75\}$, as the instrument of high school completion, to directly measure the effect of maternal education on infant health.

1.4.2 Discussions on the identifying assumptions

Now let's reconsider the first assumption on sample selection. It would be invalid if a woman's decision to become a mother was affected by her exposure to the closures of high schools. A common finding from quasi-experimental studies on education and fertility is that women's education affects their selection into motherhood (Black et al., 2008; Osili and Long, 2009; Duflo et al., 2011; Ozier, 2011; etc.).[21] Maternal age at first birth is another concern. Royer (2004) finds that a Prue-term birth is more likely if a mother has her first child before 18 or after 35. The closures of high schools kept teenage girls out of schools, which might have shifted a woman's age at first birth to be under 18 and thus have had negative effect on infant health. It would be difficult to differentiate the effect on infant health of women not staying in schools in their teen years from that of lower education levels and less knowledge learned in schools. Therefore, before proceeding to investigate educational attainment and infant health, I first examine the reduced form effects of a woman's exposure to the closures on her motherhood choice and maternal age at first birth.

The second assumption in identifying the causal effect of maternal education on infant health is that mothers on either side of age 17.75 are comparable, except for their different exposures to the closures of high schools. This assumption is plausible because both the closures of high schools and the change in the school entry date policy were unexpected. It is unlikely that parents of these women were able to precisely predict both of these changes and respond

[21] Black et al. (2008) find that female education reduces teenage childbearing in Norway and the US. Duflo et al. (2011) also show that subsidizing girls' education leads to a significant reduction in teenage pregnancy in Kenya. Osili and Long (2009) present evidence that better educated women have fewer births before age 25 in Nigeria. Ozier (2011) finds that secondary school education reduces teenage pregnancy in Kenya.

in planning births accordingly to avoid the closures. This assumption is also testable by testing the continuity of observable and predetermined characteristics between women just beyond 17.75 and those just below. I provide evidence in Section 6.1 that these women and their parents are very similar.

Another concern about endogenous responses arises from the fact that the timing of school closures varied across counties. In counties that closed schools later, parents might have had expectations on the negative shock, and their children might have had dropped out voluntarily before the county government made the closure decision. If such responses had existed, we would expect a decline in high school completion among cohorts older than 17.75 by the first quarter of the first year of the closures. In Section 5.2, I plot women's high school completion by their age at the time of the school closures. I do not find any sign of a decline among the Prue-exposure cohorts.

Moreover, there are two alternative hypotheses that might explain the discontinuous change in maternal education or infant health at 17.75. The first hypothesis is related to concurrent reforms after the Cultural Revolution. For other reforms to confound the effects of the school closures, they should have followed the timing of the closures closely by county and affected educational attainment and infant health differently for women on either side of 17.75 by the first quarter of the reform year. From a comprehensive reading on Chinese history in the 1970s and 1980s, I find land reform between 1978 and 1984 as a potential confounder. The reform replaced collective farming in Mao's era with household farming, and is widely documented to have greatly improved agricultural productivity since the late 1970s (e.g. Lin, 1992). As a consequence, the opportunity cost of schooling increased following the reform, which resulted

in higher dropout rates in rural areas since 1978 (Lin, 1993; Hannum, 1999). In addition, the cooperative medical system in Mao's era collapsed following land reform. Although it is hard to imagine differential impacts of changes in health care for women close to 17.75, I can account for any change in health care provision by controlling for exposure to land reform. Using data on the timing of land reform by county that I collected from county gazetteers, I provide direct evidence in Section 6.2 that land reform does not account for any effects of the school closures.

The second hypothesis is that, because many rural middle schools were also closed after the Cultural Revolution, women younger than 18 when high schools were closed might have dropped out when middle schools were closed a few years earlier. If the decline in high school completion at 17.75 is induced by the closures of middle schools two years ago, we should also observe a discontinuous drop of women's middle school completion at 17.75 by the time of the high school closures. In Section 6.3, however, I show that there is no evidence of a discontinuity in women's middle school completion at 17.75.

Finally, an advantage of this study is that internal migration in China, especially rural-to-urban migration, had been strictly restricted by the household registration system (the *Hukou* system) by the early 1990s. The first relaxation of the *Hukou* system was in 1985 when the Ministry of Public Security authorized a temporary urban resident permit (urban *Hukou*) for those temporarily migrating for business (Wang, 2005). The next substantial change was a new urban *Hukou* for rural migrants open to more cities endorsed by the central government in 1992 (Chan and Zhang, 1999). The *Hukou* system has two important implications for potential concerns about migration in this study. First, before

the first relaxation of the *Hukou* policies in 1985, it was not possible for individuals to move to other counties to continue education when rural high schools were closed from 1977 to 1984. Second, the migration rate in the rural population was still very low in the early 1990s when the 1992 CCS was conducted. In the 1990 Census, only 5.1 percent of women born in 1953-1972 who are at risk of becoming mothers in my sample migrated from rural areas.

1.5 Main Results

1.5.1 Fertility choices

The effect of education on fertility choices would lead to either a sample selection problem or a complication in interpreting the effect of maternal education on infant health, or both. To provide direct evidence on these possible concerns, I begin my empirical analysis by estimating the reduced form effects of a woman's exposure to the school closures on her selection into motherhood and maternal age at first birth.

The graphical presentation on the relationship between observed motherhood and one's age by the first quarter in the first year of the school closures is in Figure 4. Each point presents the fraction of mothers observed in each cell by age in quarter. The vertical bar at age 17.75 denotes the oldest cohort exposed to the closures. If a woman's decision to become a mother is affected by her exposure to the closures, we would expect a break in the trend at age 17.75. However, the pattern on either side of the vertical bar is smooth. In Table 2, column (1) of Panel A presents the estimated effect of a woman's exposure to the closures on

her selection into motherhood, which is estimated by fitting a quadratic cohort trend in equation (3). The estimated effect, -0.0067, is extremely small relative to the sample mean of 0.92, and it is statistically insignificant. There is no evidence that women on either side of age 17.75 have different probabilities of becoming mothers. This finding reduces the concern about endogenous sample selection induced by one's exposure to the school closures.

The same is true for maternal age at first birth as shown in Figure 5. The trend declines in a manner by age in year, and it appears continuous at age 17.75. I examine whether the closures of high schools shifted maternal age at first birth to under 18 and report the estimate in column (2) of Panel A. The estimate is statistically insignificant and has the opposite sign to the expectation. To further explore whether high school closures have any effect on maternal age at first birth, I separately estimate reduced form for each possible maternal age from 13 to 36, and report one estimate for each age in Panel B. I do not find evidence that mothers exposed to the closures are more likely to have their first birth before age 18. The main effects of one's exposure to the closures are to reduce the probability of having the first birth at 22 by 2.5 percentage points and increase the probability at age 21 by 3.3 percentage points. As suggested by Royer (2004), these changes within the safe age range are unlikely to have negative effect on infant health. There are very small decreases at age 15 and 17 that are significant at 10 percent level, which would have small positive effect on infant health, if any, the opposite to the effect of the school closures. Therefore, if a woman's exposure to the school closures has any negative effect on infant health, it is implausible to attribute the effect to changes in maternal age at first birth induced by the closures.

That I find negligible effect of the school closures on fertility choices is interesting in light of the literature on education and fertility in developing countries. I provide two possible interpretations. First, while education interventions targeting lower levels of schooling have been found to reduce teenage pregnancy (Black et al., 2008; Duflo et al., 2011; Ozier, 2011), women on the margin of being affected by the closure of high schools in this study were close to 18. Second, it is not surprising that the closures mainly shifted maternal age at first birth from 22 to 21, because "late marriage and late births" had been a family planning policy since the 1970s and the 1980 marriage law set a legal marriage age at 20 for women.

1.5.2 Educational attainment

After having found negligible effect of women's exposure to the school closures on fertility choices, this subsection turns to educational attainment using the sample of mothers. Figure 6 shows the relationship between high school completion and one's age by the first quarter of the year the school closures started. Three main features should be noted in this figure. First, consistent with expectations, a sharp and sizable drop in the fraction of mothers who completed high school appears at age 17.75. The rate of high school completion decreases from 12 percent at 18 to 8.6 percent at 17.75, roughly a 28 percent decline. Second, the Prue- and post exposure trends away from age 17.75 are smooth. If concerns arise about voluntary dropout prior to the first year of the school closures in counties that closed schools later, we would observe that the Prue- trend declines among individuals older than 17.75. However, such a decline is not observed. The smoothness of the Prue- and post trends and the sharp discon-

tinuity on either side of 17.75 together support interpreting the discontinuous change in education as the effect of the high school closures. Finally, this figure also provides visual guidance for choosing an appropriate polynomial order to fit the cohort trend. Because the Prue-exposure trend is linear, I use linear trend for the main specification. I also report results using a quadratic or a cubic polynomial as robustness checks.

Estimates of the drop in high school completion at age 17.75 are presented in Table 3. I report the first stage estimates for the sample of all mothers in column (1)-(4) and for the subsample of mothers who report the birth weight of their first birth in column (5)-(8) separately.[22] Column (1) reports the baseline estimate by fitting a quadratic cohort trend without any co variates. I find a decline of 2.8 percentage points in high school completion induced by the school closures, an effect size slightly smaller than that observed in Figure 6. Controlling for quarter of birth effects in column (2) and adding county fixed effects in column (3) do not change the point estimate and the standard error. The point estimate changes slightly to 2.7 percentage points when county specific trends are controlled for in column (4). It is a 22.5 percent decline from the high school completion rate at age 18. I focus on the estimate in column (4) because it is the average departure from county specific cohort trends as captured by the within-county comparisons. These effects are all precisely estimated and the precision is not changed by controlling for additional co variates. The estimated effects in column (5)-(8) are very similar in magnitude relative to the mean level in the subsample of mothers.

A final remark is on interpreting the incomplete education in high school for

[22] I find no statistically significant effect of being exposed to the school closures on reporting the birth weight of the first birth.

women affected by the school closures. Compared to women at 18 who had just graduated from high school when the closures started, those at 17.75 who had to drop out have one year less education, which accounted for half of the high school education at that time; they also do not have a high school diploma.

1.5.3 Infant health outcomes

This subsection focuses on the analysis of infant health for the first births. I show the reduced form graphs on gestational age in Figure 7, prematurity in Figure 8, neonatal mortality rate in Figure 9, infant mortality rate in Figure 10, birth weight in Figure 11 and low birthweight in Figure 12. If one more year of high school education improves infant health, we would expect that the incidence of poor infant health measures, including prematurity, low birthweight, neonatal mortality and infant mortality rate, would have both a declining pattern among Prue-exposure cohorts and a discontinuous increase at age 17.75. However, trends of these four measures are generally noisy. The probabilities of prematurity and low birthweight jump at age 17.75, but these changes might not be statistically different from zero.

Before turning to reduced form estimates on infant health, I report OLS estimates in Panel A of Table 4. Maternal age at birth, county fixed effects and mother's year of birth effects are controlled for. Maternal high school completion reduces infant mortality by 0.39 percentage points (18 percent) and low birthweight by 0.97 percentage points (24 percent), and increases birth weight by 57 grams. The observational evidence, consistent with previous correlational studies, suggests that one more year of high school education improves infant

health along these three dimensions.

Reduced form estimates on infant health outcomes in Panel B of Table 4, however, are all statistically insignificant. A mother's exposure to the school closures shortens her gestational age by 0.009 months, less than half a day. The probability of prematurity is increased by 0.003 percentage points, which is economically very small relative to 2.2 percent at age 18 and not statistically different from zero. The estimated effect on the probability of neonatal mortality (in 1 month) is an decrease of 0.0018 percentage points, while the estimated decrease in the probability of infant mortality (in 1 year) is 0.0016 percentage points. Both effects have the opposite sign to the expectations, but they are small relative to the mean levels at age 18 and imprecisely estimated. In the last two columns, I use the subsample of mothers who report the birth weight of their first birth. A mother's exposure to the closures decreases the birth weight of her first birth by 10.35 grams, a very small effect relative to 3288 grams at age 18. The sign of the estimate on low birthweight is opposite to the expectation. Again these estimates are also statistically indistinct from zero.

Panel C in Table 4 reports the instrumental variables estimates of the effect of maternal high school completion on infant health, using a mother's exposure to the school closures as the instrument of her high school completion. One more year of high school education increases gestational age of the first child by 0.33 months. The estimated effects on prematurity of -0.093 is economically large relative to the sample average, which provides suggestive evidence on a large effect of maternal education on reducing the risk of prematurity, but the estimate is statistically indistinguishable from zero. The estimate on neonatal mortality (0.07 relative to the sample mean at 0.014) and infant mortality (0.065

relative to the sample mean at 0.02) are large in magnitude but have opposite sign to the expectations as well as little precision. Finally, the estimates on birth weight and low birthweight are large in economic terms but both are imprecise.

1.5.4 Other outcomes relevant to infant health

To understand why there is no evidence that maternal high school completion improves infant health, I further discuss the effects of women's exposure to the closures on a number of potential mechanisms, including labor market outcomes, health behaviors, their husbands' education and labor market outcomes.[23] Table 5 reports reduced form estimates using the 1992 CCS. Not surprisingly, in column (1)-(3), estimates on employment status, working in non-farm jobs and working in white-collar jobs are all very small and statistically not different from zero. Among them, being employed and off-farm employment have the opposite sign to the expectations. These findings are consistent with previous studies that find zero or even negative earnings returns to education from the late 1970s to the early 1990s in rural China (Gregory and Meng, 1995; Li and Zhang, 1998).

Although the income channel from the labor market in very weak, one more year in high school might directly improve health behavior if women's ability to proceed health information is improved at this margin of education. I focus on women's prenatal care and delivery, which could well capture their awareness of benefits associated with health services in this context, because local governments subsidized prenatal care and delivery for women planning authorized births in rural areas (Short and Zhang, 2004) and my findings relate to the first

[23] Both the 1992 CCS and the 1990 Census do not report individual earnings.

births. In column (4)-(6), however, I find little evidence that women's exposure to the closures affects whether they have any prenatal health check, whether have one in the first trimester and whether deliver the birth in hospital.

Assortative mating in the marriage market suggests that better educated women would marry better educated and wealthier men. Because data on men in the 1992 CCS are of low quality, I use the sample of the same 400 counties in the 1990 Census to examine paternal education and labor market outcomes. In Table 6, the first stage estimate for women is reported in column (1). The decline in high school completion is 2.5 percentage points, which is very close to the estimate using the 1992 CCS. In column (2), the school closures reduce a woman's probability of marrying a man that completed high school by 1.3 percentage points. Estimates on the husband's labor market outcomes in column (3)-(5) suggests that women tend to marry men with similar labor market performances. But again, these estimates are small and statistically insignificant.

1.6 Robustness

In this section, I first provide evidence that women on either side of age 17.75 are similar along a number of predetermined dimensions. Next, I test for, and rule out two alternative hypotheses that might also explain the decline of high school completion.

1.6.1 Testing continuity of predetermined characteristics

If there were other negative selections on women at 17.75 in the first quarter of the first closure year, it would not be plausible to attribute the decline in high school completion to the sole effect of the school closures. For example, if less educated parents of women (called grandparents hereafter) were more likely to give birth in the second quarter of 1961, we might expect a decline in high school completion at 17.75 by the first quarter of 1979 due to lower endowments from grandparents, which could partly account for the decline in counties that closed schools in 1979.

An ideal test would be on the continuity of predetermined characteristics on either side of 17.75. Unfortunately, little information prior to marriage is recorded in the 1992 CCS. Alternatively, using another nationwide survey, the 1988 Two per Thousand Fertility Survey, I conduct birth cohort analysis for women at risk of becoming mothers in my sample. Specifically, I test whether women born in the second quarter are systematically different from those born in the first quarter for each year in 1960-1963, who were around 17.75 when schools were closed between 1978 and 1981.[24] The specification is an analogue to the reduced form in equation (3); only the regressor of interest is an indicator variable that is equal to 1 if born in the second quarter or later for a particular year in 1960-1963 and 0 otherwise. The analysis sample includes rural women born 6 years before and 6 years after that year.

In Table 7, I report estimates on an extensive set of predetermined characteristics, including grandparental education, grandmother's age at birth, women's

[24] I am not able to estimate the reduced form on predetermined characteristics directly, because county identifiers are not releases in the 1988 Two per Thousand Fertility Survey.

own parity, months of breastfeeding, neonatal mortality and infant mortality. Each panel shows results by testing discontinuous changes between the first and second quarters in each year from 1960 to 1963 separately. Each estimate is for a different characteristic. The vast majority of the 36 estimates are small and statistically insignificant, suggesting little selection of female births in the second quarter of these years. Only two estimates are significant at 5 percent level. The neonatal mortality and infant mortality rates among women born in the second quarter of 1961 are lower. If lower mortality rates indicate better quality, we would expect higher rate of high school completion at 17.75 in counties that closed schools in 1979, which is the opposite to the decline induced by the school closures Only one estimate, middle school completion of grandparents whose wives gave birth in the second quarter of 1962, is significant at the 10 percent level and is very small in magnitude. Overall, these findings support the validity of the identifying assumption that women at risk of being around 17.75 at the closures are very similar along many predetermined dimensions.

1.6.2 Alternative hypothesis 1: Land reform

There are 306 counties that report the timing of both high school closures and land reform and that are matched with the 1992 CCS. I directly control for one's exposure to land reform in the first stage and reduced form estimation in the sample of 306 counties. One's exposure to land reform is assigned as 1 if one was at 17.75 by the first quarter in the first year of land reform and 0 otherwise. Every column (1) in Table 8 reports the estimated effect of the school closures without the control of land reform, and every column (2) reports the estimated effects of both reforms. One's exposure to land reform has a negative, but very

small and statistically insignificant effect on high school completion. The first stage estimates on the effect of the school closures in both columns are very close. Reduced form estimates show that a woman's exposure to the school closures decreases her gestational age by 0.015 months at 5 percent significant level and increases the probability of prematurity by 0.006 percentage points at 10 percent significant level. These results provide suggestive evidence on a positive effect of maternal education on reducing prematurity in this subsample of counties. Controlling for one's exposure land reform does not alter the estimated effects of the school closures on most infant health measures, except for a sign reversal on low birthweight, which is also imprecisely estimated.

1.6.3 Alternative hypothesis 2: Closures of middle schools

If rural middle schools were closed systematically two years prior to the closures of high schools, the decline in high school completion at 17.75 might be due to dropout from middle schools when these women were 15.75. The estimated effect of being at 17.75 when high schools were closed on one's middle school completion is -0.017 with a standard error of 0.013, which reduces concerns about earlier closures of middle schools.

1.7 Treatment Heterogeneity

The effect of the school closures on high school completion could be heterogeneous across regions. We might expect a larger first stage in two types of areas. First, because the goal of the reform was to close all the rural high schools built

during the Cultural Revolution, counties that built more had to close more, generating a larger negative shock to individuals still in school. Second, these closures essentially increased the costs of continuing and completing high school for the rural population. In less densely populated areas where the travel costs are higher, dropout rate might be larger. I test these two possibilities and report results in Table 9. In the first two columns, I divide the sample of counties by the pre-existing education level.[25] Interestingly, the school closures have no effect on high school completion in counties below the median education level, while the decline in high school completion is as high as 32 percent in counties above the median. In the last two columns of the first row, consistent with the expectations, the decline in percentage of high school completion is larger in less densely populated areas (30 percent) than that in more populated areas (19 percent).

If the relationship between maternal high school completion and infant health is nonlinear, we might expect an effect of maternal education on infant health in counties with higher pre-existing education level and with lower population density. From the second to the seventh row of the first two columns, reduced form estimates on infant health measures in both types of counties are all imprecisely estimated. There is no evidence that a larger decline in maternal high school completion has led to larger changes in infant health outcomes. Similarly, in the last two columns, estimates are close in magnitude and are all statistically insignificant. Despite the heterogeneous effects of the school closures on high school completion, these reduced form estimates further confirm that maternal high school completion has little effect on infant health.

[25]Because only a few county gazetteers report the number of high schools built during the Cultural Revolution, I measure the pre-existing education level by the fraction of mothers that completed high schools among pre-exposure cohorts at age 18-23 at the time of the closures.

1.8 Conclusion

This study provides new evidence on the effect of maternal high school education on infant health by exploiting the massive closures of rural high schools right after the Cultural Revolution in China. Compared to women just a few quarters older and who had just graduated from high school when the school closures started, women still in the final year are 22.5 percent less likely to complete high school. However, there is no evidence that the large decline in maternal high school completion induced by the closures has an effect on infant health, as captured by prematurity, low birthweight, neonatal mortality and infant mortality. In addition, I find that a woman's exposure to the closures has negligible effect on her selection into motherhood and maternal age at first birth, suggesting that the estimated effects on infant health are not confounded by fertility choices.

The narrow comparisons among women born quarters apart but who had distinct exposures to the closures within county provides credible evidence. First, both the closures of high schools and the change in the school entry date policy were unpredictable for these women and their parents. Second, I provide evidence that women being compared are very similar in terms of observable, predetermined characteristics. Third, I rule out two alternative hypotheses that might interpret the discontinuous change in high school completion. One is land reform between 1978 and 1984, and another is the closures of middle schools prior to the closures of high schools. Finally, for the particular episode in rural China, concerns about endogenous mobility are less important.

Using a quasi-experiment on education, that I do not find an effect of ma-

ternal high school education on infant health is a local effect, which is more relevant to the subpopulation of women from low socioeconomic background and is specific to one more year in high school.

I discuss three possible interpretations to my findings in the context of rural China from the late 1970s to the early 1990s, during which infant health outcomes are observed. First, the income channel is very weak in this setting. During the initial transition from a central planned economy to a market-oriented one, the labor market was dysfunctional and rewarded little to schooling. On the other hand, there is also no evidence of marital sorting by education level.

Second, the cooperative medical system in Mao's era collapsed since the late 1970s. The coverage of collective health care on the rural population went down from 90 percent in the 1970s to 5 percent in the mid- 1980s (Hsiao, 1995). Although better educated women might have more access to health care services in the privatized system, the quality of health care provision in rural areas was generally very poor, which might have limited the role of maternal education.

Finally, the cost of health information transmission was lowered by information campaigns under the birth control policies since the early 1970s. Under the theme "fewer and better births", these campaigns were widespread in the countryside. In such environment, the marginal effect of one more year in high school on acquiring information about improving child health might be minimal.

CHAPTER 2

LONG-TERM EFFECT OF *IN UTERO* EXPOSURE TO LAND REFORM ON ACADEMIC PERFORMANCE IN CHINA

2.1 Introduction

In Heckman (2007), investing early in childhood is more productive in producing the multidimensional capabilities, including cognitive skills, noncognitive skills and health stocks, than investing in later developmental stages. The multiplier effects of early inputs imply that public policies affecting the *in utero* environment might be more efficient in promoting human capital than traditional education policies. Empirically, while a growing body of the "fetal origins" literature explores the later-life impacts of extreme shocks *in utero*, evidence on early-life exposure to public policies affecting economic wellbeing is relatively thin (Almond and Currie, 2011b). These extreme and often episodic events (famine, war, etc.) are widely used because they can provide credible research design. On the other hand, the long-term effects of these shocks may not be generalizable to those of public policies that lead to mild but permanent increases in resources and that are relevant for a broader population. Likewise, knowledge of how parental resources and their timing causally affect children's human capital development is scant, largely because exogenous shocks to parental income are rare in the real world (Currie, 2009).

In this paper, we examine the long-term effect of *in utero* exposure to the post-Mao land reform between 1978 and 1984 in China on academic performance on college entrance exams. Previously in Mao's era, agriculture was collectivized and the incentive to work was low. The reform replaced the col-

lective system with the Household Responsibility System (HRS), under which collectively owned land was contracted to individual households who can receive all income from the land after meeting the tax and quota sales obligations to the state (Perkins, 1988). Essentially, an incentive scheme was provided that more tightly linked reward to effort. McMillan et al., (1989) and Lin (1992) document that the reform accounted for more than half of the agricultural output growth in China between 1978 and 1984. The rural poverty rate also declined dramatically following the reform. McMillan (2002) refers to the reform as "the world's largest antipoverty program". Thus, we use the reform as a positive income shock that leads to nutritional improvement for individuals exposed to the reform *in utero*.

Distinct from the previous literature that relies on the provincial variation of the HRS diffusion (McMillan et al., 1989; Lin, 1992), we exploit the fact that the reform decision was made at a more local level and the reform timing varies from 1978 to 1984. We use new data on the year the reform started by county that we collected from county gazetteers ever published since the 1980s. These county-level data can be linked for 1,068 counties to microdata on the universe of college entrance exam scores between 1999 and 2001 in nearly 2 million testing records. The matched data allow us to assign one's exposure to the reform by one's year of birth and county of birth. An appealing feature of our study is that the scope for bias from endogenous migration is minimal, because we observe each test taker's county before age 1 in the microdata.[1] Moreover, mobility of parents induced by demand for land between 1978 and 1984 was unlikely because land was allocated based on family size within one's village of birth, and internal migration had been under strict control under the *Hukou* system since

[1]This stands in contrast to the United States, where large sample microdata generally do not report county of birth.

the 1950s until its first relaxation in 1985 (Wang, 2005).

Our empirical strategy is a difference-in-difference approach that compares academic performance among individuals born before and after the first post-reform harvest between counties that reformed earlier and those that reformed later. The key identifying assumption is no systematic differences in the pre-existing trends of academic performance across counties with different reform timing. We have included county time trends that capture any differential patterns for test takers born prior to the first harvest across counties. Moreover, through comprehensive reading on the evolution of the reform policy and the previous literature at the province level, we also find out the primary determinants of the reform timing at the county level and account for their possibly time-varying effects. Lastly, it is unlikely that other reforms followed the timing of land reform closely by county and have had differential impacts for individuals born before and after the first harvest, except for the One Child Policy (OCP). To directly account for any confounding effect of OCP, we collected a novel dataset on the timing of OCP implementation by county for more than 900 counties and find robust results after controlling for one's exposure to OCP.

We first echo the previous studies by finding the productivity gain of land reform at the county level, which started one year after the reform. We then find that high school students born two years after the reform perform better on college entrance exams, particularly on math. They are also more likely to be admitted by first-tier and top ranked colleges. The impact estimates are relatively modest in magnitude: math test scores increase by just under 1% and attending a selective college by 4-5%. Interestingly, these effects are substantially stronger for boys. Furthermore, we test for, and rule out possible hypotheses

about endogenous selection into the sample of test takers. There is no evidence that land reform affects fertility, gender, one's chances of going to high school or high school dropout, one's decisions to apply for college and take the college entrance exams, and one's age at the exams.

Our paper contributes to three strands of literature. First, it expands the fetal origins literature to examine the long-term effect of *in utero* income shock induced by public policy on human capital, and thus helps inform the policy relevance of the fetal origins effects. To the best of our knowledge, only two papers have explored the later-life impacts of public policies, which are Head Start program in Ludwig and Miller (2007) and Food Stamp Program in Almond et al., (2011). Furthermore, a handful of studies that well identify the long-term effects of early-life income shocks mostly consider health outcomes (Van Den Berg et al., 2006; Banerjee et al., 2010; Almond et al., 2011).[2] In this paper, we use the 1978-84 land reform in China as a positive shock to household income and focus on academic performance that captures cognitive skills. Our findings suggest that development policy can indeed lay the foundation of human capital accumulation by targeting the *in utero* environment.

Second, we provide evidence and an analytical framework for interpreting gender difference in the fetal origins effects as observed in many studies (e.g., Van Den Berg et al., 2006; Field et al., 2009; Maccini and Yang 2009; Almond et al, 2010; Bhalotra and Venkataramani, 2011).[3] The epidemiological litera-

[2]Van Den Berg et al., (2006) find that individuals born during economic expansions lived longer than those born during economic downturns in the Netherlands. Banerjee et al., (2010) show that children exposed to the phylloxera in French vineyards in the 19th century were shorter in adulthood. Almond et al., (2011) find positive effect of being exposed to the Food Stamp Program early in childhood on adult health and weaker effect on economic outcomes.

[3]Van Den Berg et al., (2006) find that exposure to adverse economic condition at birth increase mortality rate for men but not for women. Bhalotra and Venkataramani (2011) find that men exposed to sulfa drugs in infancy benefit more in terms of schooling, employment and income in adulthood. Almond et al., (2010) find that men exposed to famine are more likely to be

ture generally assumes biological difference by gender and ignores parental responses that might be shaped by gender discrimination (e.g., Kraemer, 2000). A purely biological difference, however, can not be fully reconciled with existing evidence, for example, the larger negative effects of rainfall shock at birth for women in Maccini and Yang (2009). On the other hand, recent work on responsive investments shows that parents reinforce by investing more on better endowed children (Datar et al., 2010; Adhvaryu and Nyshadham, 2011).[4] In the context of a society with son preference, it is particularly important to consider how reinforcement of parents might generate the gender difference. We demonstrate that while biology is unlikely to explain the gender difference in the effect of land reform, our evidence is consistent with differential responses of parents for boys and for girls.

Finally, despite the wide recognition of the 1978-84 land reform as the engine of China's transition to a market economy, the existing evidence on the reform effects has not gone beyond productivity growth and has been limited at the province level (McMillan et al., 1989; Lin, 1992). Little is known about the broader impacts of the reform on other dimensions of human wellbeing, such as health and education, particularly at the individual level. Our study is among the first to document long-term non-production benefits of the reform. From a policy perspective, the persistent effect of the reform on promoting human capital adds to understanding social returns to antipoverty programs and

illiterate, less likely to work and get married. In contrast, Field et al., (2009) find that exposure to iodine supplementation *in utero* increases schooling for girls, but not for boys, in Tanzania. Maccini and Yang (2009) find larger influences of a rainfall shock in the birth year on women's health, education and economic outcomes later in life.

[4]Datar et al., (2010) show that low birth weight children are less likely to be breastfed, have fewer well-baby visits, are less likely to be immunized, and are less likely to attend preschool than normal birth weight siblings. Adhvaryu and Nyshadham (2011) find that parents reinforce the higher cognitive endowments of children who received in utero iodine supplementation, by investing more in vaccinations and early life nutrition.

improving the cost-benefit analysis of these programs in developing countries.

The remainder of the paper is organized as follows. Section 2 introduces background on the post-Mao land reform, provides evidence on productivity gain following the reform at the county level, and describes the education system in China. Section 3 describes the microdata on test scores and admission outcomes. Section 4 discusses our empirical approach. Section 5 presents preliminary results.

2.2 Background

2.2.1 The post-Mao land reform

China had pursued collective agriculture for more than two decades since the mid-1950s. Under this system, workers generally received daily fixed work points and got paid at the end of the year (Lin, 1988). The incentive to work was low and agricultural productivity was stagnant. From 1956 to 1977, there had been virtually no change in grain output per capita (Zweig, 1987). Following the death of Mao Zedong and the end of the Cultural Revolution, a decollectivization reform was initiated in 1978. By contracting collectively owned land to individual households on a long-term basis,[5] the reform shifted the basic decision-making unit from the collective farm to the household. The new system was called the Household Responsibility System (HRS), under which individual households could make their own input decisions and receive all income from the land after meeting the tax and quota sales obligations to the state

[5]The earlier land lease was up to 15 years. It was extended to 30 years in 1993.

(Perkins, 1988; Sicular, 1991).

The HRS was initially worked out by farmers without approval from the central government (Lin, 1987). At the end of 1978, a small number of production teams in Anhui Province, which suffered from a severe drought in that year, start to experiment with contracting land and output quotas to individual households (Lin, 1987; Yang, 1996). The initial position of the Central Committee of the Chinese Communist Party (CCP) on household farming was in a strong opposition. The document "Regulations on the Management of Rural People's Commune" passed by the CCP in the November of 1978 clearly stipulated that contracting to individual households was not permitted.

The prohibition was relaxed in September 1979 by allowing a few exceptions to households living in areas that were peripheral, distant, mountainous, and isolated due to transportation difficulties.[6] In September 1980, the Central Document No.75 issued by the Central Committee further allowed poor and remote areas and production units heavily dependent on state subsidies to contract land and output quota to households. By August 1981, the Central Committee's position on household farming became more liberalized in a mission sent to fifteen provinces "contracting to households is not only a means of relieving poverty but also ways of enhancing productivity; and it hasn't changed the production relations of the collective economy".[7] In January 1982, the Central Document No.1 officially announced that "the HRS is the production responsibility system of the socialist economy", which first showed the CCP's commitment to popularize HRS.

[6] *Agriculture Yearbook of China 1980*, 1981, Beijing, Agricultural Press.
[7] People's Daily, August 4th, 1981

2.2.2 Variation in the reform timing

Data on the timing of land reform at the county level were previously undiscovered. We have collected new data on the year HRS was introduced by county from 1242 county gazetteers in which local history from 1949 to the 1980s was recorded. Specifically, it is the year when collectively owned land was first contracted to individual households in a few villages of each county; and it usually took 2-3 years to spread the HRS to the whole county. These counties represent two-thirds of all counties that have ever published gazetteers.[8] Because land reform occurred in rural areas, our sample includes locations that were rural counties at the time of the reform.[9] In Figure 13, we plot the fraction of counties that had introduced HRS between 1978 and 1984. Only 1.85 percent of counties were the reform pioneers in 1978. The vast majority reformed between 1979 and 1981, with the peak of 45 percent in 1980. By 1984, all counties had adopted HRS.

Unlike many top-down policy implementations, the 1978-84 land reform is a bottom-up process. The reform timing by county is therefore not randomly assigned. Fortunately, what drives the local timing has not been a black box. The evolution of the reform policies suggests two primary sources of the variation: drought and poverty prior to the reform. A severe drought led to large declines in agricultural production, which in turn provided the local government incentive to reform.[10] Particularly, the negative production shock spurred taking the

[8]The other one-third of counties do not report the timing of HRS adoption, or report it as "the late 1970s" or "the early 1980s", which are not useful for our identification strategy.

[9]City districts are defined and excluded by using the county code in the 1982 Census and the official definition.

[10]Bai and Kung (2011) provide indirect evidence using province level data. They find that provinces that suffered more in the 1959-61 Famine started land reform earlier when struck by bad weather. The interpretation is that the Famine experience undermined the local belief that collective farming could effectively cope with negative weather shocks.

political risk as reform starters, since contracting land to individual households were not permitted in earlier years. Poor and remote counties were among the first permitted to adopt HRS by the central government as a means to reduce national poverty rates.

Moreover, existing literature on HRS adoption at the province level provides three additional insights (Lin, 1987; Yang, 1996; Chung, 2000). First, the diffusion of HRS was faster where reduction in monitoring cost is higher and thus productivity gains are larger. Using size of production team to measure monitoring cost, previous studies show mixed findings.[11] The second hypothesis is that provinces that suffered more from the 1959-61 Famine reformed earlier, because the more painful the lesson from the Famine, the less likely the province to favor collective farming. Supportive evidence is provided by Yang (1996) and Bai and Kung (2011). Lastly, Yang (1996) argues that provinces further from Beijing had more freedom to initiate earlier reform.

We provide the first evidence on the correlation between land reform timing and potential determinants at the county level. By matching the county-level data on reform timing with county-by-year data on precipitation (See Data Appendix), we examine whether drought had led to land reform. Notably, land reform is an irreversible event, implying that drought prior to reform might affect the decision to reform, but drought after would not. In this test, we assign one to the first year of reform, zero before the first year and missing values after. As suggested by Chinese Academy of Agricultural Sciences (1984), the growth of rice, the number one grain output in China, largely depends on rainfall at the beginning of the growing season, which starts in March or April. If drought

[11] Lin (1987) finds that provinces with larger production teams reformed earlier, while Chung (2000) has the opposite finding.

affected reform timing through its impact on crop yield, we would expect a stronger correlation regarding spring drought. In Table 10, column 1 shows no correlation between the first year of reform and drought defined by average monthly precipitation in the whole growing season (March to September) in the reform year and the year before.[12] From column 2 to 5, we measure drought by monthly precipitation from March to June separately. Consistent with the expectations, droughts in March and April of the reform year and one year before increased the probability of earlier reform, which are all statistically significant at the one percent level. In contrast, droughts in May and June had no impact on the reform timing.

Next, to test the cross-sectional relationship between reform timing and other local characteristics as discussed above, we have collected and constructed county-level measures prior to the reform from various sources. Poverty is captured by grain output per capital in 1977 that are collected from county gazetteers, and remoteness is measured by distance to province capital using 1982 Census. Size of production team is proxied by the density of labor force (aged 16-60) in 1977.[13] Famine intensity is measured by the average birth cohort size in 1953-1957 divided by the average cohort size in 1959-1961 using the 1982 Census.[14] We also calculate the distance to Beijing to capture freedom in local policy-making. Table 11 reports both estimates from univariate regression for each determinant and those from multivariate regressions. Consistent with previous literature, counties that were initially poor, that had larger production teams in 1977 and higher famine intensity in 1959-1961, and further from the

[12]Because the first month of reform are mostly not documented, a drought in the growing season is likely to affect reform at the second half of the current year or next year.

[13]Density is calculated by population size aged 16-60 in 1977 divided by area at the county level using 1982 Census.

[14]Meng et al. (2009) use a similar measure of famine intensity using the 1990 Census.

central government decided to reform earlier. Controlling for grain output per capita in 1977 forces us to drop two thirds of the sample due to lack of data. To get close to the full sample with data on reform timing and explore the robustness, we omit grain output in the last column and find robust results for labor force density and famine intensity, though it decreases the standard error of distance to Beijing that becomes statistically significant.[15]

To attribute changes in individual academic performance to prenatal exposure to land reform, it is crucial to account for county features that contribute to the local decision to reform and that have time varying effects on individual educational outcomes. In all regressions on the effect of land reform, we control for the time-variant droughts in March and April in current year and the year before, as well as time-invariant determinants of reform timing by county interacted with time fixed effects.

2.2.3 Land reform and productivity gain

By linking rewards directly to effort, the HRS provided economic incentives to individual households (Sicular, 1991). The reform has been found to have greatly improved agricultural productivity and increased rural household income between 1978 and 1984. McMillan et al. (1989) suggest that over three-quarters of the productivity increase during this period could be attributed to the incentive effects of the HRS. Using provincial data on the diffusion of HRS, Lin (1992) also finds that the reform accounts for half of the output growth. The reform is also widely recognized for its achievements in lifting hundreds of mil-

[15]Grain output per capita in 1977 is negatively correlated with distance to Beijing, and positively correlated with labor force density in 1977.

lions of rural households out of poverty (World Bank, 2000).[16] It is claimed to be the largest antipoverty project in the world (McMillan, 2002).

Grain production represents the vast majority of agricultural production from the 1970s to the mid 1980s in China. Using unique data on total population and total output of grain production by county collected from county gazetteers,[17] we provide the first quantitative evidence on the productivity gain of the 1978-84 land reform at the county level. There are 400 counties that report both the reform timing and the complete year-by-year total population and total output of grain production from 1974 to 1984, among which 382 counties are matched with data on county controls correlated with reform timing. Records on grain output in the 1970s are particularly valuable because county-level statistics have been released systematically only since the 1980s in China. They are also arguably reliable because these data were originally from local official archives (Xue, 2010).[18]

We estimate the effect of land reform on grain output per capita in a difference-in-difference framework and report results in Table 12. In column 1, county fixed effects, year fixed effects, and county specific time trends are controlled for. Panel A shows that HRS adoption increases grain output per capita by 3.2 percent, and Panel B presents monotonic increases in grain output from one year to four years after land reform. The largest output gain appears four years after the HRS adoption, which is a 16 percent increase. The increases in grain output are statistically insignificant five years after land reform.[19] In

[16]Official estimates indicate that rural poverty rate declined from 30 percent in 1978 to 5 percent in 1998 (World Bank, 2000).

[17]Grain crops generally include rice, wheat, corn and potato.

[18]Because the purpose of compiling county gazetteers is to accurately record local history, local historians working in the county gazetteer office have less incentive to manipulate the grain output data.

[19]This is consistent with a consensus in the literature that China's agricultural performance

column 2, we further control for drought in March and April of the current year and one year before, and other determinants of reform timing interacted with time fixed effects. The point estimate in Panel A decreases to 2.6 percent and standard error remains the same, while estimates in Panel B are very similar to those in column 1.[20]

Absent income data by year and county (or household) from the 1970s to 1980s, these estimates on grain output are helpful to understand the lower bound of income change fellowing the reform. Nationwide, changes in procurement price of grain products by year had been universal during this period, which are accounted for by year fixed effects. Thus, the increase in individual income from selling grain products would be at most 2.6 percent. In addition, from a small sample of counties that report year-by-year data on production of both grain and cash crops, we find that the fraction of grain output only decreases slightly from 98 percent in 1974 to 93 percent in 1984.[21] Previous literature suggests that land reform led to a more dramatic increase in production of cash crops. However, because on average cash crop was still a very small proportion in agricultural production by 1984, we would expect a tight upper bound of income increase.

The estimated "first stage" effect of land reform on grain output has two im-

slowed down after 1985 (Sicular, 1991; Lin, 1992).

[20]Our estimated effect of the HRS adoption on productivity gain is much smaller than the conventional estimate in Lin (1992). An important feature of our identification strategy is in its ability to control for time varying effects of county characteristics that are correlated with the timing of HRS adoption. Besides, two additional differences are noticeable. First, while Lin (1992) measures the HRS adoption by the fraction of production teams that had adopted the HRS by year for each province, we use the year HRS was first introduced for each county. Second, the outcome in Lin (1992) is the value of crop output including grain crops and cash crops, while we only use grain output. Taken at face value, our findings using a more careful research design and data at a more local level cast some doubt on the large effect of HRS adoption on agricultural productivity found in the previous literature.

[21]Only 36 counties are in this sample. Cash crops include cotton lint, oilseed, sesame, peanut, etc. We do not use this sample for estimation because of the small sample size.

plications for our empirical analysis below. First, it provides us with the guidance to assign the timing of exposure status at the individual level. If one was born two years after the reform, he/she was exposed to a positive income shock *in utero*, while those born one year after the reform or earlier were unexposed in the prenatal period. Second, because the productivity gain and therefore the income increase induced by land reform is small, we expect that average effect on individual academic performance generated by land reform is moderate.

2.2.4 Education system in China

Pre-college education in China includes six years in primary school, three years in middle school and three years in high school. Education in primary school and middle school has been mandatory since the introduction of a nine-year compulsory schooling law in 1986. In the final year of middle school, students take the local entrance exams to get access to high school. Individuals who fail the exams either move to vocational schools for 3-5 years of study, or stop going to school. In most provinces, high school students are divided into two tracks: science and humanity/social science. Math, Chinese and English are the core subjects for both tracks.[22] In addition, the science curricula include physics, chemistry and biology, while the humanity/social science curricula include history, geography and political education.

The procedure of admission to college is comprised of two stages. In the first stage, high school students take the national college entrance exams in their final year. These exams last for 12 hours over 3 days from June 7th to 9th. Math, Chinese and English are the three basic subjects. Depending on the track in high

[22]Other foreign languages are taught in a few areas, e.g. Russian, Korean, etc.

school, an additional subject that combines respective subjects in each track is required. A test taker's overall score is a weighted sum of scores from these four subjects. While the exams are standard for most provinces, there are a few exceptions. Beijing and Shanghai are allowed to design independent exams, and more provinces start to have their own exams in recent years. Within province, the difficulty of the exams also varies between science track and humanity/social track. For example, math exam is designed to be more difficult for students in the science track, while Chinese exam is relatively easier.

In the second stage, one's admission to college is determined by his/her total score on the national exams and his/her preference on colleges. Depending on the provincial policy, students fill application forms ranking their most preferred colleges, either before or after the national exams. Colleges set a fixed enrollment quota for each of the two tracks for each province. To be admitted by a particular college, a student has to pass the score threshold of that college for his/her track in the province he/she takes the exams, and has listed that college in his/her top choices. Thus, student essentially compete against each other within their track in their province.

On the supply side, there is a clear hierarchy among the 2,300 higher education institutions in China. At the bottom of the pyramid are three-year colleges that typically award associate degrees. Above them are four-year colleges offering programs in both academic and vocational courses and typically leading to bachelor degrees. Four-year colleges are classified into first- and second- tier, the former of which have better reputation and enroll students with higher scores in the college entrance exams. On the top of the pyramid are 39 elite colleges and 100 top colleges that most of the government research fundings spend on.[23]

[23]People's Daily Online, "Over 10 billion yuan to be invested in *211 Project*".

Only test takers at the higher end of the score distribution have access to these colleges that set the highest thresholds in admission.

2.3 Data

The primary microdata we use cover the universe of applicants in college entrance exams between 1999 and 2001. Applicants report their national identification number, from which one's county of *hukou* registration and date of birth are precisely obtained. Because one*'s hukou* status is registered before age 1, and internal migration was strictly restricted under the *Hukou* system from the 1970s to the mid 1980s, the county of *hukou* registration corresponds well to the *in utero* county for individuals born during this period. This is an important advantage of our research design. Migration for college entrance exam has been widely reported by media. Some parents send their children to high schools in less developed provinces where the score thresholds in college admission are set lower. In this setting, having a good measure of one's *in utero* county largely reduces concerns about possibly endogenous mobility.

For each year between 1999 and 2001, we match the microdata with the county-level data on the timing of land reform based on one's county of *hukou* registration. Although we observe the universe of college applicants in the microdata, the fraction of provinces that release the national identification number of applicants increases by year. In 1999, 13 out of 30 provinces release national ID number, 22 provinces in 2000 and 29 in 2001.[24] There are 472 counties that we can match in both the microdata in 1999 an the county-level data on reform

http://english.people.com.cn/90001/6381319.html.

[24]Guangdong doesn't release national ID number in all years.

timing, 719 counties in 2000 and 1044 in 2001. In total, we have 1068 counties matched in both data. The dotted line in Figure 13 shows that the distribution of the land reform timing in these matched counties is very close to that in all counties that have ever reported the reform timing.

Our analysis sample includes test takers born between 1978 and 1984, because the vast majority of test takers during 1999-2001 were at age 17-21 (98%). An individual's *in utero* exposure to land reform is jointly determined by one's year of birth and the timing of HRS adoption in his/her county of *hukou* registration. We measure *in utero* exposure to land reform as whether one was born two years after the reform or later, and use variation across cohorts and counties to isolate the effect of prenatal exposure on academic performance. In our sample, there are 43.8 percent of test takers who were exposed to land reform *in utero* (the HRS remains in place once adopted). They were born from five years before the reform to six years after the reform.

We consider two sets of measures on academic performance as the main outcomes. First, we focus on the total score and scores of the three core subjects: math, Chinese and English. Because the exams might vary across provinces and also between science track and humanity/social science track within province, all test scores are standardized within province and track.[25] In our empirical analysis, we use the percentile of test score for each subject relative to the sample of test takers within province and track. Second, we examine a set of admission outcomes, including whether one was admitted by any college, by a 4-year university, by a first-tier university, by a top 100 university or by one of the 39 elite universities. Our analysis sample includes 1,918,244 complete records on

[25]We use the official formula of standardization used by the Ministry of Education.

total score and admission outcomes.[26]

Table 13 reports summary statistics of the analysis sample. While total scores of male and female students are very close, the variation among male students is larger. Male students perform slightly better in math exam, while female students perform better in Chinese and English exams. On admission outcomes, more than half of all test takers, 53 percent, are admitted by any college. There are 25.1 percent admitted by a four-year university and 10.6 percent admitted by a first-tier university. A small proportion of them, 6.7 percent, have access to a top 100 university, and only 2.4 percent go to an elite university. While the ratio of being admitted by any college is slightly higher among female test takers, male test takers perform better in terms of being admitted to higher ranked universities.

2.4 Empirical Framework

2.4.1 Identification strategy

To infer the causal relationship between *in utero* exposure to land reform and later-life academic performance, two major challenges exist. The first challenge is to overcome the identification problem: omitted variables which might be correlated with one's exposure to land reform. The second challenge concerns sample selection. The sample of test takers might be a selective one if land reform affects fertility, or one's chances of going to high school, or one's decision

[26] Among these test takers, 99.5 percent have records on math and Chinese scores and 99.4 percent have records on English score.

to apply for college and take the national exam, or all of them. Assuming for the moment that land reform has no impact on one's selection into the sample of test takers, we first consider the empirical approach to isolate the effect of exposure to land reform, and then discuss the sample selection problem in the next subsection.

Our identification strategy compares individuals who were *in utero* just before the first post-reform harvest and those just after. One's exposure status is jointly determined by one's year and county *in utero*. As suggested by our "first stage" estimate on grain output, the first cohort exposed to the positive income shock was born two years after the reform. Using a difference-in-difference approach, we estimate equation (4) as below:

$$Y_{ijte} = \alpha + \beta Reform_{jt} + \gamma_j + \delta_t + \eta_e + \phi_j * t + D'_{jt}\theta_t + D'_{jt-1}\lambda_{t-1} + \sum_{t=1979}^{1984} (X'_j * T_t)\rho_t + \varepsilon_{ijte} \quad (2.1)$$

where i denotes the individual, j the *in utero* county, t the year of birth and e the year of college entrance exams. Y_{ijte} denotes one's academic performance, as captured by test scores and admission outcomes. The variable of interest, $Reform_{jt}$, is equal to 1 if one was born two years after the land reform or later and 0 otherwise. A set of fixed effects, county fixed effects γ_j, year of birth effects δ_t and the exam year effects η_e, are controlled for. These county and time controls absorb the effects of time invariant county characteristics, national birth cohort effects, as well as any differences in the college entrance exams and admission process across provinces or over time. Moreover, we also include county specific linear trends, $\phi_j * t$, to account for county characteristics that change smoothly over time and that are correlated with the timing of land reform. Because the identifying assumption is that cohorts born prior to the reform give the right

counterfactual, including county specific linear trends reduces concern about pre-existing trends that differ between counties that reformed earlier and those that reformed later.

Furthermore, we account for time varying effects of county characteristics that are found to drive the reform timing. The time-variant measures on drought in March and April of the current year are denoted by D'_{jt}, and drought of last year is denoted by D'_{jt-1}. The time-invariant determinants prior to 1978, including labor force density in 1977, famine intensity in 1959-61 and distance to Beijing, are denoted by X'_j. We interact these covariates with time fixed effects from 1979 to 1984, with 1978 omitted.

A specific concern arises if other post-Mao reforms during the same period also affect long-term human capital. Particularly, because the introduction of HRS came as part of the whole package of market-oriented reforms in the rural economy, the other two major reforms, namely, price reform and market reform (Lin, 1992), could also increase household income and improve later-life human capital. To confound the effects of land reform, other reforms should have followed the timing of land reform closely at the county level and have had differential impacts on individuals born just before and just after two years after land reform. However, the increases in procurement prices and in bonuses for above-quota productions were nationwide in 1979 (Sicular, 1991), and the reductions in the planning of agricultural production and in the restrictions in interregional trade were universal state interventions (Lin, 1992). Any effects of exposure to these two reforms are accounted for by controlling for year of birth effects δ_t.

Besides economic reforms, we also conducted a comprehensive reading on

non-economic reform policies from the late 1970s to the early 1980s. We hypothesize that the One Child Policy (OCP) introduced in 1979 might be a potential confounder because its implementation varied at the county level, and parents might invest more in the prenatal period when they can have only one child. We have collected new data on the year the county government started to implement OCP, which varied from 1978 to 1985. In Figure 14, we plot the distribution of the timing of both land reform and OCP. Although these reforms started during the same period, their timing by county is not exactly coincident, which allows us to uniquely disentangle the effect of land reform from that of OCP. We find robust results for land reform after one's exposure to OCP is controlled for.

Another concern is that prenatal exposure to land reform might affect one's age at the college entrance exams, which is equal to the exam year minus one's year of birth. For example, if individuals exposed to land reform *in utero* perform better in their pre-college education and therefore their chances of repetition are lower, they would take the exams earlier than those unexposed. In the presence of such effect, including the exam year effects, η_e, would yield a "bad control" problem in the identification (Angrist and Pischke, 2008). To directly address this concern, we estimate equation (1) by using one's age at the college entrance exams as the outcome and provide evidence that it is not affected by one's exposure to land reform.

Finally, besides raising rural household income, land reform might have changed other household conditions. The change most relevant to child human capital is the collapse of rural medical system following the decollectivization. The coverage by collective health care of the rural population went down from 90 percent in the 1970s to 5 percent in the mid 1980s (Hsiao, 1995). If less ac-

cess to health care has long-term effect on human capital, we would expect that the effect is negative, which is the opposite to that of a positive income shock generated by land reform.

2.4.2 Discussion on sample selection

We observe test scores and admission outcomes for high school graduates who took the national exam. Selection into this sample might be endogenous to one's exposure to land reform for four main reasons. Fertility response to land reform is the first one. A common concern in the fetal origins literature is about the scarring effect, suggesting that adverse events experienced *in utero* may increase fetal mortality that removes those in poor health (Almond and Currie, 2011b). If the positive income shock generated by land reform reduced fetal mortality, our sample might be negatively selected. Besides possibly biological response, parents might also desire more children after the reform. The shift from collective farming to household farming increased the value of manual labor and therefore household demand for labor. The allocation of land is based on family size, which also provided parents incentive to have more children. Moreover, following the decollectivization, it more difficult to enforce birth control policies, the penalties on above-quota births in particular, without a strong collective leadership (White, 1987).[27]

Second, if exposure to land reform improves one's performance in the high school entrance exams, one is more likely to go to high school. Similarly, they are less likely to drop out if they perform better in high school. Thus, the sample

[27]Schultz and Zeng (1999) consider the effect of land reform on fertility using data on the reform timing at the village level and the 1987 in-depth fertility survey. They find that, following the reform, fertility increased slightly between 1982 and 1984, and declined from 1985 to 1987.

of high school graduates would be positively selected. In the 2000 Census, there are 16 percent of individuals born in 1978-84 in our sample of counties who ever went to high school or were still in high school. Among them, only 2 percent dropped out in high school.

Third, high school graduates might choose not to apply for college upon graduation. If one's exposure to land reform affects one's decision to apply, the effect would also be positive because it is students who are not confident in performing well give up taking the exam. The national statistics show that 89 percent of high school graduates apply for college in 1999, 96 percent in 2000 and 93 percent in 2001.[28]

Finally, a few outstanding students are exempted from national exam and guaranteed college admission. If those exposed to land reform were the top students in high school, they are more likely to go to colleges, and better colleges, without taking exam. Among all applicants in our data, 96.8 percent of them took the national exam, which might be a negatively selected sample.

We can directly test three out of these four hypotheses. Using the 1990 and 2000 Census, we can estimate the reduced form of exposure to land reform on fertility, whether one ever attended high school and high school dropout. Using the administrative data on test scores, we can test the selection from college applicants to test takers.

Because we do not observe high school graduates who do not apply to college, the only hypothesis that we can not directly test is about the selection from high school graduates to applicants. Instead, we use the 1990 and 2000 Census to obtain the size of high school graduates by county, year of birth and year of

[28] Educational Statistics Yearbook of China.

graduation, and match these counts with the counts of college applicants in the administrative data.[29] In each county-birth cohort-year of graduation cell, we test whether the probability of applying to college is affected by exposure to land reform.

Separate from estimation using the full sample, we also examine gender difference in the effect of exposure to land reform. In this exercise, possible changes in sex ratio at birth following the reform is a concern. The increase in household demand for manual labor would lead to a stronger desire for male labor if there is a comparative advantage in physical work. Furthermore, old-age support in rural areas was weakened following the collapse of the collective system, which could also increase demand for boys. We use 1990 Census to test whether one's gender is endogenous to prenatal exposure to land reform.

2.5 Main results

2.5.1 Results

We report the estimated effects of *in utero* exposure to land reform on academic performance, as captured by test scores in percentile and admission outcomes, in Table 14. Exposure to land reform increases one's total score by 0.25 percentile, which is statistically significant at 10 percent level. The effect size is 0.5 percent compared to the sample mean, a very small magnitude. Similar effects are found on math and English scores, and the effect on math score is slightly larger (a 0.6 percent increase) and statistically significant at 5 percent

[29] See Data Appendix on the construction of the cell size.

level. While there is no strong evidence that land reform affects one's chances of going to college or a four-year college, test takers exposed to the reform are 2.8 percent more likely to be admitted by a first-tier college and 4.5 percent more likely by a top 100 college, the latter of which is statistically significant at 1 percent level. The larger effect on going to better colleges might suggest that exposure to a positive income shock disproportionately benefits individuals with better endowments *in utero*. Another possibility is that parents from higher socioeconomic background invest more prenatally, or in childhood, or both, as responses to an exogenous increase in household income.[30]

In Table 15, we report the results by gender. On test scores, while estimates for female students are all statistically insignificant and small, male students benefit from exposure to land reform in their total score, math score and English score. Particularly, the estimated effect on math score for boys is more precise (statistically significant at the 1 percent level) and relatively larger in magnitude (a 0.8 percent increase). We also find little effect of land reform on admission outcomes for girls, except for a 3.5 percent increase in the probability of going to a top-100 college at the 10 percent significant level. For exposed boys, they are 4.5 percent more likely to be admitted by four-year colleges, and 5.5 percent more likely by top 100 colleges, which are larger effects compared to these in the full sample. Moreover, they are 7 percent more likely to go to one of the 39 elite colleges, the very top of the higher education pyramid, though the estimate is statistically significant at the 10 percent level. Overall, the positive effect of prenatal exposure to land reform on performance on college entrance exam largely loads on boys.

[30]Unfortunately, parental information is not available in the administrative data on test scores.

2.5.2 Interpretation of gender difference

It is not obvious whether the gender difference is purely biological or is involved with gender discrimination in parental investments. If male fetuses are more sensitive to the nutritional improvement induced by land reform, we would expect a lower fetal mortality rate for boys and thus a higher sex ratio after the reform. In another paper, we find that the sex of the first child is not affected by land reform, suggesting a minimal role of biology in explaining the gender difference. Alternatively, in Model Appendix, we formalize two ideas for understanding the role of parental behaviors in the context of a society with son preference. Following Almond and Currie (2011a), we consider that human capital is produced in a two-period childhood. The shock *in utero* is the same for boys and girls. As suggested by Chen et al., (2011), less than 20 percent of all counties had introduced ultrasound by 1982, when more than 98 percent had started land reform. Prenatal investments are therefore unlikely to respond differentially to the shock for boys and for girls.

We first assume that the level of postnatal investment is always higher for boys than for girls. For less than perfect substitution between periods, boys benefit more from the shock in terms of later-life human capital. The difference is generated mechanically by the gender discrimination in the investment level in childhood, without assuming optimization.

Second, we consider responsive investments in childhood. Our departure from Almond and Currie (2011a) is that we assume that parents value perceived income of their child instead of child's human capital, and the perceived return to human capital is higher for boys than for girls. It is important to note that perceived return could be different from actual earnings return to schooling in the

labor market. Although the correlation between wage and years of schooling is higher for women than for men in rural industries (Meng, 1998), rural parents still desire higher education level for sons, who are expected to provide old age support. In the 1991 Survey on Women's Status in Contemporary China, 73 percent of rural women think that a son should have at least high school education, while 54 percent think so for a daughter. The Chinese Longitudinal Healthy Longevity Survey in 2005 shows that, the rural elderly rely more on sons both financially and emotionally.[31] We illustrate that, given a positive shock *in utero*, parents respond more in postnatal investments for sons than for daughters. Thus, the shock would have larger effect on improving academic performance for boys.

Both interpretations are consistent with our reduced form evidence on the gender difference. A final note is on the effect for girls. Regardless of gender discrimination in parental investments, we might still expect a positive effect for girls through the biological channel. Estimates for girls have similar standard errors but smaller point estimates relative to boys. Although these estimates are imprecise, we can not reject certain positive effects for girls. Moreover, if the production function is Leontieff in periods and the postnatal investment for girls is zero, we would observe zero effect of prenatal exposure to land reform for girls.

[31] Among rural individuals aged over 60 who live with children, 92% of them live with sons. The financial transfer they get from sons is 1.7 times of that from daughters. When they are sick, 56% are taken care of by sons and daughter-in-laws, while only 9% by daughters and son-in-laws. Moreover, for 66% of the elderly, the first person they talk to if they have difficulties is son or daughter-in-law, while only 4% talk to daughter or son-in-law.

2.6 Robustness

2.6.1 One Child Policy

There are 856 counties that report the timing of both land reform and OCP on county gazetteers. If the introduction of OCP induced an increase in prenatal investment of parents, we would expect that individuals *in utero* in the year when OCP was introduced were the oldest cohort affected. Thus, we assign treatment status to OCP as 1 for individuals born one year after OCP or later and 0 otherwise.

In Table 16, we report the estimates for both exposure to land reform and exposure to OCP. Recall that the sample of counties is smaller than our full sample of counties. For estimates on test score in this smaller sample, only the estimate for total score is statistically significant at the 10 percent level. Despite less precise estimates, it is obvious that the estimate for exposure to land reform has little change after exposure to OCP is controlled for. Results for admission outcomes similar to that in the full sample. They are also robust when exposure to OCP is controlled for. Table 17 and Table 18 report results for boys and girls, respectively. Estimates for boys and girls are very similar to those using full sample, and again they are robust to the inclusion of OCP.

These findings suggest little parental responses in the prenatal period to the One Child Policy and that OCP does not confound our results for the effect of land reform.

2.6.2 Testing sample selection

This subsection reports results from testing possible concerns about sample selection as discussed in Section 4.2. Using the 1990 Census, we first examine the impact of land reform on the number of births at the county-birth year level. A strength of using the 1990 Census is that internal migration had been under strict control under the *Hukou* system by 1990.[32] Our sample of counties in the 1990 Census are the same as those matched with the reform timing in the data on test scores. In Table 19, we find no evidence of fertility response to land reform in column 1. The estimate on the number of births is -0.561, which is very small, statistically insignificant, and has the opposite sign to the expectations.

In column 2 and 3, we test the sample selection into high school graduates. We use the 2000 Census because individuals born between 1978 and 1984 were at age 16-22 in 2000.[33] There is suggestive evidence that *in utero* exposure increases one's probability of going to high school by 0.5 percentage points, a 3 percent increase relative to the sample mean. However, the estimate is not statistically different from zero. Consistent with the expectations, exposure to land reform reduces high school dropout. The estimate is also very small and statistically insignificant.

Next, column 4 shows whether a high school graduate's decision to apply for college is affected by his/her exposure to land reform. We conduct this test at the county-birth year-year of application level by combining 1990 Census, 2000

[32]We merge the census data with the county-level data on the reform timing based on one's county of residence in 1990. Among individuals born between 1978 and 1984 who are at risk of being test takers in our sample, only 0.73 percent of them were migrants.

[33]The 2000 Census is matched with the data on the land reform timing using one's county of residence in 2000. One caveat of using the 2000 Census is that migration rate has been rising since the relaxation of *Hukou* system in the 1990s. The migration rate is 8.4 percent in our sample.

Census and the administrative data. The estimate is extremely small, a decrease of 0.1 percentage points relative to the sample average of 39 percent. Again, it is imprecisely estimated and with the wrong sign.

We report the result for testing the selection from applicants to test takers in column 5. As expected, applicants exposed to land reform are less likely to take the national exam, meaning that they are more likely to be guaranteed admission. This is consistent with our finding on positive effect of exposure to land reform on academic performance. However, the estimate is economically very small and statistically insignificant.

To briefly summarize results from column 1 through 5, we find little evidence that prenatal exposure to land reform affects one's probability of being observed in the sample of test takers in the college entrance exams, reducing concerns about endogenous sample selection.

Further, for our results on gender difference, we use the 1990 Census and report the estimate on whether gender is endogenous to land reform in column 6. Individuals born two years after land reform are 0.1 percentage points more likely to be male, which is very small relative to the sample mean of 52 percent and imprecisely estimated.

Lastly, recall the concern about one's age at exam and the potential "bad control" problem as discussed in Section 4.1. In column 7, we use the administrative data to examine the effect of exposure to land reform on one's age at the college entrance exams. Test takers exposed to land reform is 0.005 year (roughly 2 days) younger than those unexposed. The effect is economically small and statistically insignificant, suggesting that including year of exam effects would not

yield to bias in our estimation.

2.7 Conclusion

In this paper, we find that prenatal exposure to the 1978-84 land reform in China has long-term impact on promoting human capital. Our findings suggest that income might be beneficially targeted to the prenatal period and that "pro-growth" policies can lay the foundation for human capital accumulation. Moreover, boys benefit more from the reform. In the context of son preference, we argue that the gender difference could be generated mechanically by differential investment level in childhood, or through differential parental responses to the reform.

CHAPTER 3

LAND REFORM AND MISSING GIRLS IN CHINA

3.1 Introduction

Since Amartya Sen's article *More Than 100 Million Women Are Missing* in 1990, searching for the causes of missing girls has been the theme of numerous debates. A recent consensus is that cultural and economic values of sons are the primary driving forces in China, India and South Korea. Traditionally, parents prefer sons. In China, for example, one of the core Confucian values is that family lineage is traced solely through the male. Parents also need sons. In a largely agricultural society with weak social security system, sons have the physical strength for manual labor, and more importantly, they are expected to provide old age support. Yet, design-based evidence separating economic incentive from culture is still very thin.

This paper aims at isolating the economic cause of missing girls by exploring the effect of the post-Mao land reform between 1978 and 1984 on sex ratio in China. The collectivized system in Mao's era was replaced by the Household Responsibility System (HRS), under which collectively owned land was contracted to individual households on a long-term basis. The institutional reform had led to remarkable agricultural growth between 1978 and 1984 (McMillan et al., 1989; Lin, 1992). During the same period, the sex ratio at birth also increased dramatically in the rural areas (Zeng et al., 1993). Sen (1990) hypothesizes that, the increase in household demand for manual labor and demand for old age support following the reform provided parents incentive to select for sons. Other than Sen's hypotheses on institutional changes, it is also likely

that the increase in household income induced by the reform led to more sex selection, which became more affordable.

We use a research design that compares the sex of the second child between families with a first girl and those with a first boy before and after the reform. The validity of the triple-difference strategy comes from a number of facts. One's exposure to the reform depends his/her year of birth and county of birth, where the reform timing varied from 1978 to 1984. We can compare births just before and just after the reform. The third difference by the sex of the first child is based on the demographical patterns found in Zeng et al., (1993). While the sex ratio of the first child is biologically normal, it becomes abnormally male-biased at higher birth orders, especially among families with no previous son. Therefore, we focus on the second child in families with a first girl, where parents have a stronger desire for sons. The second child in families with a first boy is an ideal control group, because little human intervention is involved with the sex of the first child. Finally, we are able to compile new data on the timing of One Child Policy(OCP) implementation by county level and thus can directly account for any potential confounding effect of OCP.

The main results show precisely estimated effect of land reform on driving the rising sex ratio. The second child in families with a first girl is 5.5 percent more likely to be a boy following land reform, and the increase in the county-by-year sex ratio of the second child is 12 percent. Moreover, these effects are robust when one's exposure to OCP is controlled for.

To separate different hypotheses that might explain why land reform contributed to the sex ratio imbalance, we provide evidence on three sets of heterogeneous effects. First, while we found significant increases in male births in

counties that had positive productivity gain after the reform, no such increase is observed where there was no post-reform agricultural growth. The distinct effects by changes in productivity can rule out the hypothesis of demand for old age support, which was induced by the universal collapse of collective support. Second, we find that the effect of land reform on male births does not vary by the county-level fraction of women growing cash crops, which directly measures the local demand for female labor and increase in female income. This suggests that the demand for labor is not gender specific and higher female income does not lead to less sex selection. Alternatively, the hypothesis most consistent with the evidence is the increase in total household income. Finally, we document that better educated parents are more likely to have male births after the reform, which supports that the increase in household income played an important role in raising sex ratio.

The principle contribution of this paper is in the use of the post-Mao land reform in China to provide new evidence on the role of economic incentive in explaining missing girls. It is also the first empirical evidence in support of Sen's argument that land reform led to higher sex ratio. On the other hand, evidence on heterogeneous effects does not support Sen's hypotheses that the increases in demand for male labor and demand for old age support are the main mechanisms. Probably most strikingly, our evidence casts doubt on a common belief that improving female income or female education saves girls.

We introduce the background of land reform, including the variation in the reform timing and its first order effect on agricultural productivity, in Section 2. Section 3 provides the conceptual framework on why land reform might led to higher sex ratio. We discuss data and identification strategy in Section

4. Preliminary results are presented in Section 5. Results on robustness are reported in Section 6.

3.2 Background

3.2.1 The post-Mao land reform

China had pursued collective agriculture for more than two decades since the mid-1950s. Under this system, workers generally received daily fixed work points and got paid at the end of the year (Lin, 1988). The incentive to work was low and agricultural productivity was stagnant. From 1956 to 1977, there had been virtually no change in grain output per capita (Zweig, 1987). Following the death of Mao Zedong and the end of the Cultural Revolution, a decollectivization reform was initiated in 1978. By contracting collectively owned land to individual households on a long-term basis,[1] the reform shifted the basic decision-making unit from the collective farm to the household. The new system was called the Household Responsibility System (HRS), under which individual households could make their own input decisions and receive all income from the land after meeting the tax and quota sales obligations to the state (Perkins, 1988; Sicular, 1991).

The HRS was initially worked out by farmers without approval from the central government (Lin, 1987). At the end of 1978, a small number of production teams in Anhui Province, which suffered from a severe drought in that year, start to experiment with contracting land and output quotas to individual

[1]The earlier land lease was up to 15 years. It was extended to 30 years in 1993.

households (Lin, 1987; Yang, 1996). The initial position of the Central Committee of the Chinese Communist Party (CCP) on household farming was in a strong opposition. The document "Regulations on the Management of Rural People's Commune" passed by the CCP in the November of 1978 clearly stipulated that contracting to individual households was not permitted.

The prohibition was relaxed in September 1979 by allowing a few exceptions to households living in areas that were peripheral, distant, mountainous, and isolated due to transportation difficulties.[2] In September 1980, the Central Document No.75 issued by the Central Committee further allowed poor and remote areas and production units heavily dependent on state subsidies to contract land and output quota to households. By August 1981, the Central Committee's position on household farming became more liberalized in a mission sent to fifteen provinces "contracting to households is not only a means of relieving poverty but also ways of enhancing productivity; and it hasn't changed the production relations of the collective economy".[3] In January 1982, the Central Document No.1 officially announced that "the HRS is the production responsibility system of the socialist economy", which first showed the CCP's commitment to popularize HRS.

3.2.2 Variation on the reform timing

Data on the timing of land reform at the county level were previously undiscovered. We have collected new data on the year HRS was introduced by county from 1242 county gazetteers in which local history from 1949 to the 1980s was

[2] *Agriculture Yearbook of China 1980*, 1981, Beijing, Agricultural Press.
[3] People's Daily, August 4th, 1981

recorded. Specifically, it is the year when collectively owned land was first contracted to individual households in a few villages of each county; and it usually took 2-3 years to spread the HRS to the whole county. These counties represent two-thirds of all counties that have ever published gazetteers.[4] Because land reform occurred in rural areas, our sample includes locations that were rural counties at the time of the reform.[5] In Figure 13, we plot the fraction of counties that had introduced HRS between 1978 and 1984. Only 1.85 percent of counties were the reform pioneers in 1978. The vast majority reformed between 1979 and 1981, with the peak of 45 percent in 1980. By 1984, all counties had adopted HRS.

To identify the effect of land reform using the variation in the reform timing, the first priority is to understand what the driving forces were towards an earlier reform. The evolution of the reform policies suggests two primary : drought and poverty prior to the reform. A severe drought led to large declines in agricultural production, which in turn provided the local government incentive to reform.[6] Particularly, the negative production shock spurred taking the political risk as reform starters, since contracting land to individual households were not permitted in earlier years. Poor and remote counties were among the first permitted to adopt HRS by the central government as a means to reduce national poverty rates.

Moreover, existing literature on HRS adoption at the province level provides

[4]The other one-third of counties do not report the timing of HRS adoption, or report it as "the late 1970s" or "the early 1980s", which are not useful for our identification strategy.

[5]City districts are defined and excluded by using the county code in the 1982 Census and the official definition.

[6]Bai and Kung (2011) provide indirect evidence using province level data. They find that provinces that suffered more in the 1959-61 Famine started land reform earlier when struck by bad weather. The interpretation is that the Famine experience undermined the local belief that collective farming could effectively cope with negative weather shocks.

three additional insights (Lin, 1987; Yang, 1996; Chung, 2000). First, the diffusion of HRS was faster where reduction in monitoring cost is higher and thus productivity gains are larger. Using size of production team to measure monitoring cost, previous studies show mixed findings.[7] The second hypothesis is that provinces that suffered more from the 1959-61 Famine reformed earlier, because the more painful the lesson from the Famine, the less likely the province to favor collective farming. Supportive evidence is provided by Yang (1996) and Bai and Kung (2011). Lastly, Yang (1996) argues that provinces further from Beijing had more freedom to initiate earlier reform.

We first test whether drought had led to land reform by matching the county-level data on reform timing with county-by-year data on precipitation.[8] Land reform is an irreversible event, implying that drought prior to reform might affect the decision to reform, but drought after would not. Thus, we assign one to the first year of reform, zero before the first year and missing values after. In addition, Chinese Academy of Agricultural Sciences (1984) suggests that, the growth of rice, the number one grain output in China, largely depends on rainfall at the beginning of the growing season, usually in March or April. In Table 10, column 1 shows no correlation between the first year of reform and drought defined by average monthly precipitation in the whole growing season (March to September) in the reform year and the year before.[9] From column 2 to 5, we measure drought by monthly precipitation from March to June separately. As expected, droughts in March and April of the reform year and one year before have strong and precisely estimated effect on motivating

[7] Lin (1987) finds that provinces with larger production teams reformed earlier, while Chung (2000) has the opposite finding.

[8] See Data Appendix.

[9] Because the first month of reform are mostly not documented, a drought in the growing season is likely to affect reform at the second half of the current year or next year.

an earlier reform.

Next, to test the correlation between reform timing and other time-invariant county characteristics as discussed above, we have collected and constructed county-level measures prior to the reform from various sources. Poverty is captured by grain output per capital in 1977 that are collected from county gazetteers, and remoteness is measured by distance to province capital using 1982 Census. Size of production team is proxied by the density of labor force (aged 16-60) in 1977.[10] Famine intensity is measured by the average birth cohort size in 1953-1957 divided by the average cohort size in 1959-1961 using the 1982 Census.[11] We also calculate the distance to Beijing to capture freedom in local policy-making. In Table 11, consistent with previous studies using provincial variation, counties that were initially poor, that had larger production teams in 1977 and higher famine intensity in 1959-1961, and further from the central government decided to reform earlier. Controlling for grain output per capita in 1977 forces us to drop two thirds of the sample due to lack of data. To get close to the full sample with data on reform timing and explore the robustness, we omit grain output in the last column and find robust results for labor force density and famine intensity, though it decreases the standard error of distance to Beijing that becomes statistically significant.[12]

In all regressions on the effect of land reform, we control for the time-variant droughts in March and April in current year and the year before, as well as time-invariant determinants of reform timing by county interacted with time fixed effects.

[10]Density is calculated by population size aged 16-60 in 1977 divided by area at the county level using 1982 Census.

[11]Meng et al. (2009) use a similar measure of famine intensity using the 1990 Census.

[12]Grain output per capita in 1977 is negatively correlated with distance to Beijing, and positively correlated with labor force density in 1977.

3.2.3 Reform effect on productivity

By linking rewards directly to effort, the HRS provided economic incentives to individual households (Sicular, 1991). The reform has been found to have greatly improved agricultural productivity and increased rural household income between 1978 and 1984. McMillan et al. (1989) suggest that over three-quarters of the productivity increase during this period could be attributed to the incentive effects of the HRS. Lin (1992) also finds provincial level evidence that the reform accounts for half of the output growth. The reform is widely recognized for its achievements in lifting hundreds of millions of rural households out of poverty (World Bank, 2000).[13]

Absent income data, it is useful to examine the effect of land reform on agricultural output to understand income change after the reform. We collected the year-by-year grain output data from county gazetteers and matched these data with the reform timing data for 400 counties. In Table 20, we report the estimated effect of land reform on grain output per capita. In column 1, on average, HRS adoption increases grain output by 2.6 percent at the 10 percent significant level. We further divide the sample by the change in grain output before and after the reform. Interestingly, not every county benefited from the reform, and the production growth concentrated in about half of these counties. Column 2 shows a precisely estimated increase of 9.2 percent in counties above median of change in grain output at the 1 percent significant level, while column 3 shows a 3.9 percent decrease at the 10 percent significant level in counties below the median.

[13]Official estimates indicate that rural poverty rate declined from 30 percent in 1978 to 5 percent in 1998 (World Bank, 2000).

3.3 Hypotheses linking land reform to missing girls

Sen (1990) hypothesizes that, the rising sex ratio in rural China since 1979 could be largely attributed to three institutional changes following land reform: the collapse of collective medical system, increases in household demand for manual labor and demand for sons as old age support. If the cutbacks in health care supply had a pro-male effect, the effect would not depend on birth order or the sex of the previous child. The first institutional change is therefore less relevant for interpreting our empirical findings, while the other two changes are expected to play important roles.

Different from Sen's arguments on institutional changes, the third hypothesis is that increase in household income induced by the reform might also affect sex ratio, though the sign of the income effect is conceptually ambiguous. Higher earning parents might rely less on sons for financial support and possibly have more savings in old age. On the other hand, the costly sex selection became more affordable. Moreover, if the increase in demand for labor was largely demand for male labor, we would expect larger income increase for men than for women, which worsened women's bargaining power and also made girls less desirable.

Although the data do not allow us to directly test each hypothesis, evidence on heterogeneous effects of land reform on sex ratio is helpful to disentangle different explanations. One distinction between demand for old age support and other hypotheses is that the weakened role of collective support would not depend on post-reform agricultural productivity. Even if households were not more productive on their own plots under the HRS, the collective no longer had

public welfare funds to take care of old people. Thus, we can compare the effect of land reform between counties with larger productivity grains and those with smaller gains. If demand for old age support is the main motivation to select sex, we would expect that the effect of land reform on male births does not vary by changes in agricultural productivity.

We can also distinguish demand for gender-specific labor and increase in gender-specific income from increase in total household income. If demand for male labor and larger increase in male income contribute to the rising sex ratio, we would expect a larger effect of land reform where the agricultural production is more male labor intensive. To relate our evidence to the existing literature (Qian, 2008), we instead measure demand for female labor and female income by the fraction of women growing cash crops by county. Our approach does not rely on an assumption that a certain cash crop is gender specific. In the 1982 Census, 57 percent of workers growing cotton are women, 54 percent for vegetable, 42 percent for tea and 38 percent for fruit.[14] The variation in the fraction of female workers for all cash crops comes from the composition of cash crop mix and how female labor intensive each crop is at the local level. If the increase in sex ratio after land reform decreases with the fraction of female workers, it is not possible to further separate demand for female labor from increase in female income in our data. Alternatively, if the reform effect is homogeneous, the hypothesis most consistent with the evidence is that increase in total household income led to more sex selection, regardless of who brought income home.

The income effect can be further explored by dividing the sample by parental education. While there was no return to human capital in collectivized farm-

[14]In Qian (2008), one key assumption is that tea is picked mainly by women in China, which contrasts with the facts that less than half of tea pickers are women, and the fraction is just 4 percentage points higher than that of fruit and far below that of the other two cash crops.

ing, Yang and An (2002) find that education improves the uses of household-supplied inputs and contributes to higher agricultural profit under the HRS. If income effect is present, we would expect that better educated parents who earned more in household production are more likely to have sons after the reform than their less educated counterparts. Moreover, as suggested by Edlund (1999), if parents not only want sons but married sons, parents from higher socioeconomic status would be the first movers to select for sons to avoid marriage market consequences of imbalanced sex ratio.

3.4 Data and Empirical Strategy

3.4.1 Data

The microdata we use is the publicly released 1 percent sample of the 1990 Census. Our analysis focuses on rural areas which were defined as counties in the 1982 Census, the closest to the time of land reform. Census data in China do not report county of birth, which forces us to use county of residence in 1990 to match the census data with the county-level data on reform timing. There are 1065 counties (58 percent of all) that are matched with data on reform timing and county controls. Concerns about endogenous migration are minimal, because internal migration had been under strict control under *Hukou* system until its first relaxation in 1985 (Wang, 2005).

Our research design needs information on one's birth order and sex of previous children, which are not directly reported in the census data. The only question to identify one's position within a household is his/her relationship

to the household head. Therefore, we focus on children of the household head. Our main analysis sample of second births is constructed as follows. First, we require that the number of children linked to the househead is equal to the total number of births reported by their mother (spouse of household head or household head herself). Families where older children were always in 1990 are excluded. Second, we exclude families with multiple births, where birth order is more difficult to identify. Third, using information on each child's year and month of birth, we define one's birth order as well as the sex of the previous child. Finally, we restrict the sample of second births to children born in 1974 and 1986. The oldest cohort were 16 in 1990, who were unlikely to be away from home of parents, and were also more likely to be matched with the first child. For the analysis by parental education, we further restrict the sample to children in two-parent families.

In our sample of births, one is defined as migrant if he/she did not reside in the same county in 1985, which is reported in the census. The migration rate among individuals born in 1974-84 is as low as 0.63 percent. Thoughout our analysis, we use the 99.37 percent born in 1974-84 who resided in the same county in 1985 and all births in 1985-86.

Summary statistics of the full sample and the sample in two-parents families are reported in Table 21.

3.4.2 Empirical Strategy

We estimate the effect of exposure to land reform on the probability of being male for the second births using a triple-difference strategy. The first double

differences are among birth cohorts and between counties that reformed earlier and those that reformed later, which jointly define the pre- and post-reform cohorts. The third difference is between families with a first girl and those with a first boy, as suggested by Zeng et al., (1993) that the sex ratio is most male-biased among births at higher birth orders in families with no previous son. Consider the estimation equation:

$$Boy^2_{ijt} = \alpha+\beta_1 Post_{jt}+\beta_2 Girl^1_{ijt}+\beta_3 Post_{jt}*Girl^1_{ijt}+\gamma_j+\delta_t+\phi_j*t+D'_{jt}\theta_t+D'_{jt-1}\lambda_{t-1}+\sum_{t=1975}^{1986}(X'_j*T_t)\rho_t+\varepsilon_{ijt} \tag{3.1}$$

where the subscript i denotes the individual, j the county of birth, and t the year of birth. The superscript denotes birth order, 1 the first child and 2 the second. The dependent variable, Boy_{ijt}, is a binary outcome that is equal to 1 if the second child is a boy and 0 otherwise. The reform indicator $Post_{jt}$ is equal to 1 if the child is born one year after the reform and 0 otherwise, which is determined by one's year of birth and county of birth. $Girl^1_{ijt}$ is an indicator that is equal to 1 if the first child is a girl and 0 otherwise. We interact the reform indicator with sex of the first child to get the regressor of interest, $Post_{jt} * Girl^1_{ijt}$.

To tease out possibly confounding differences among birth cohorts and between reform starters and followers, a comprehensive set of controls are included in the estimation. County fixed effects γ_j and year of birth effects δ_t absorb the effects of time invariant county characteristics and birth cohort effects. County specific linear trends, $\phi_j * t$, account for county characteristics that change smoothly over time and that are correlated with the reform timing. Furthermore, we account for time varying effects of county characteristics that are found to drive the reform timing. Droughts in March and April of the current

year are denoted by D'_{jt}, and droughts of last year are denoted by D'_{jt-1}. The time-invariant determinants of the reform timing, X'_j, including labor force density in 1977, famine intensity in 1959-61 and distance to Beijing, are interacted with time fixed effects from 1975 to 1986, with 1974 omitted.

The coefficient of interest, β_3, measures the effect of land reform on second child being male in families with a first girl relative to that in families with a first boy. Two identifying assumptions are crucial in this triple-difference strategy. The second births in families with no previous son provide the right counterfactual. Moreover, there are no unobserved changes coincident with land reform by county and year that have differential effects on the sex of the second child depending on the sex of the first child.

The validity of the first assumption requires that the sex of the first child is not endogenous to the reform, and the change in the sex of the second child among the pre-reform cohorts does not differ by the sex of the first one. As noted in the introduction, Zeng et al., (1993) documents that the sex ratio of the first births is biologically normal. To be cautious, we directly test whether the reform affected the sex of the first child and fail to find an effect. We also provide transparent evidence of no pre-existing trends in the sex ratio of the second births for both families with a previous son and those without.

For the second identifying assumption, the potential threats come from other reforms during the same period. To confound the effects of land reform, other reforms should have followed the timing of land reform closely at the county level and have had differential impacts on the sex of the second child for families with a previous son and those without. We have conducted a comprehensive reading on reform policies from the late 1970s to the early 1980s and grouped

them into three categories. The first type includes reforms that might lead to stronger desires for sons among parents with a first girl, but that were introduced in the same year nationwide. Typical examples are the price reform and market reform that came with land reform as a whole package of rural economic reforms. The increases in procurement prices and in bonuses for above-quota productions were in 1979 (Sicular, 1991),[15] and the reductions in the planning of agricultural production and in the restrictions in interregional trade were also universal state interventions (Lin, 1992). Any effects of exposure to reforms in this category are accounted for by controlling for year of birth effects δ_t.

Another type of reforms were introduced at different time during the same period, but their effect on the sex of the second child (if any) does not depend on the sex of the first one. As discussed in Section 3, the crisis in health care supply following land reform belongs to this category. We difference out possibly time-invariant effect of these reforms by controlling for the sex of the first child. Moreover, we provide evidence below that the sex ratio of the second child in families with a first boy remains constant before and after the reform, suggesting no sign for an effect of this type of reforms.

The One Child Policy (OCP) stands out as the most likely confounder that was introduced closely to land reform and was enforced by county at different time, and more relevantly, it is found to have led to rising sex ratio, especially at higher birth orders in families with no previous son (Ebenstein, 2010). To directly test whether the OCP has accounted for any effect of land reform, we have collected data on the timing of OCP implementation by county to directly disentangle the effects of these two reforms. In Figure 14, we plot the distribution of the timing of land reform and OCP. While they were both introduced from

[15]Qian (2008) identified the effect of the price reform on tea versus fruit on sex ratio.

the late 1970s to the mid 1980s, there is substantial difference in their rollout at the county level. Thus, we are able to disentangle the effect of one reform from that of the other, if any.

3.5 Preliminary results

3.5.1 Main effect

We begin by plotting the sex ratio of first child by birth year relative to reform year, in Figure 15. The sex ratio is very stable at the biologically normal rate of 1.05 before and after the reform. In column 1 of Table 22, the estimate of land reform on the sex of first child is very small (a 0.6 percent increase relative to sample mean) and statistically insignificant, which supports our use of families with a first boy as control group.

Figure 16 shows sex ratio of the second child for families with a first girl and those with a first boy separately. Once again, little change in the sex ratio for families with a first boy in the pre- and post- reform periods demonstrates the validity of our research design. Interestingly, the two primary causes of missing girls, cultural and economic values of sons, are both visible from the figure. Among the pre-reform cohorts, while no pre-existing trend is observed for both groups, the sex ratio of second child in families with a first girl is constantly higher than that in families with a first boy by more than 10 percentage points. This illustrates son preference as a culture, that is, parents with no previous son always have stronger desire for a son. Starting from one year after the reform, the sex ratio in families with a first girl increases dramatically, from around 1.15

to the peak of 1.3 in six years after the reform, while the ratio in families with a first boy remains nearly constant. The sharp contrast between these two groups in the pre- and post periods supports the effect of land reform as a source of economic incentives on raising sex ratio.

Column 2 presents the estimate for the effect of land reform on second child being male at the individual level. We find an increase in the probability of being male by 2.9 percentage points among families with a first girl relative to families with a first boy, and it is statistically significant at the 1 percent level. The effect is sizable in magnitude, around 5.5 percent relative to the sample mean for all second births. Furthermore, it is slightly larger than the effect of son preference, as captured by the indicator of having a first girl, which is an increase of 2.7 percentage points. To translate the effect of land reform on male births to the effect on sex ratio, we also estimate equation (1) on sex ratio of second births by county and birth year. In column 3, sex ratio in families with a first girl increases by 0.15 following the reform, a precisely estimated increase of 12 percent and consistent with the visual impression from Figure 16.

Overall, our findings provide the first empirical evidence that the 1978-84 land reform is an important driving force of the rising sex ratio in rural China.

3.5.2 Heterogeneous effects

Now we turn to evidence on three sets of heterogeneous effects for understanding the pathways through which land reform raised the sex ratio of the second births.

We first test whether the effect of land reform on second child being male varies by the post-reform increase in productivity. In Table 23, we present the estimated effects in the subsample of 400 counties that report grain output data. Before dividing the sample by the change in grain output per capita before and after the reform, we estimate the overall effect in this subsample. In column 1, the magnitude of the increase in the probability of being male, 1.3 percentage points, is smaller than that in our full sample, and it is also less precisely estimated. This indicates that we could underestimate the effect on male births using this subsample. Nevertheless, this sample provides the unique opportunity to explore the treatment heterogeneity by changes in productivity. Column 2 shows a precisely estimated increase of 2.7 percentage points for counties above the median of the change in grain output, which doubles the overall effect in column 1. In contrast, the estimate in column 3 for counties below the median is very small in magnitude and not statistically different from zero, and has negative sign. Recall that only counties above the median experienced an increase in grain output after the reform. Putting it all together, these findings suggest larger effect of land reform on sex ratio in areas with larger productivity grain induced by the reform. As discussed in Section 3, this implies that the collapse of collective support for the elderly after the reform, which does not depend on the change in grain output, is unlikely to play the main role.

Next, if the increase in sex ratio is mainly caused by demand for male labor and increase in male income, we would expect that the increase is smaller where the agricultural production is more female labor intensive. In Table 24, however, we fail to find evidence of any heterogeneous effect by the county-level fraction of female workers in growing cash crops. The estimate for the interacted effect of post-reform, first being a girl and the fraction of female workers is econom-

ically very small, a decrease of -0.02 percentage points, suggesting that a 10 percent increase in the fraction of female workers leads to a 0.2 percent decline in the probability of second child being male. It is also statistically insignificant. This estimate provides two important implications. The demand for labor after the reform is not gender specific. In addition, higher female income does not lead to less sex selection. Alternatively, that increase in total household income is the primary cause of rising sex ratio is most consistent with our evidence.

Finally, we test whether better educated parents respond to land reform differently compared to less educated ones. In Panel A of Table 25, we find that mothers with higher education levels are more likely to have a second son after the reform, and the difference increases with education level. All the estimates are statistically significant at the 1 percent level. The largest effect is found among mothers with a high school education, who are 6.5 percentage points more likely to have a second son than those without. Similarly results for fathers are presented in Panel B, though the differences between better educated fathers and less educated ones are smaller than those for mothers. These findings again support the explanation of increase in total household income, because better educated parents might have earned more after the reform. They are also consistent with the prediction in Edlund (1999) that parents from higher status respond first to select for married sons. On the other hand, probably most strikingly, as income increases, better educated women not only have stronger desire for sons, but have even stronger desire than better educated men. Absent evidence on gender difference in return to education in agricultural production at that time, it is unclear whether it is because educated women contribute more to household income, or because the marginal increase in son preference as income increase is larger for women. Either interpretation is in contrast with the

belief that improving female education or income save girls.

3.6 Robustness

3.6.1 One Child Policy

There are 856 counties that report the timing of both land reform and OCP on county gazetteers. We assign the exposure status to OCP as 1 if one was born one after the introduction of OCP in his/her county of birth and 0 otherwise.

In Panel A of Table 26, both land reform and OCP have little effect on the sex of the first child. In Panel B, column 1 shows similar estimates for land reform as in the full sample. In column 2, we find that the second birth in families with a first girl is 2.4 percentage more likely to be male after the introduction of OCP, which is precisely estimated. When we control for both land reform and OCP in column 3, the estimate for land reform is very robust, while estimates for OCP become much smaller and statistically insignificant. These results suggest that the effect of land reform is not confounded by OCP. Moreover, they cast doubt on a common argument that OCP leads to higher sex ratio, which, however, could be confounded by land reform.

3.6.2 Fertility

Land reform may increase the desire to have more children because the land distribution is based on family size, while OCP would decrease fertility. We

first estimate the effect of land reform and OCP on total fertility. Consistent with expectations, Panel A of Table 27 shows that the number of births by county and year is increased by 2 percent by land reform, and it is decreased by 2 percent by OCP.

If the increased fertility after land reform concentrates in the second birth, our sample would be a selected one. However, the rule of land distribution only applies to authorized birth (the single child), implying that the margin of fertility affected by land reform would be most likely from zero to one. On the other hand, the effect of OCP would be on a different margin, that is, fewer second births. To test whether land reform affects the decision to have a second child, we use the year of birth of the first child and the stable birth interval to assign treatment status. Specifically, since 3-year is the common interval between the first and second child at that time, we assign 1 to the first child born 2 years before land reform or later and 0 otherwise. In Panel B, controlling for OCP, the effect of land reform on having a second child is very small and statistically insignificant. Interestingly, the effect on having a second child all loads to OCP, that is, OCP increases the probability of having a second child, but parents with a first girl are still more likely to have a second one after OCP.

Overall, these findings suggest that, on the margin of fertility response most relevant to our identification, we do not find an effect of land reform.

3.7 Conclusion

In this paper, we provide design-based evidence that the 1978-84 land reforms is an important driving force of the sex ratio imbalance in China. The second

child in families with a first girl is 5.5 percent more likely to be a boy following the reform relative to that in families with a first boy, which leads a 12 percent increase in the sex ratio among the second child. The effect is larger where the post-reform increase in agricultural productivity is larger, but it does not vary by demand for gender-specific labor or increase in gender-specific income. Probably most strikingly, better educated parents with no previous son are more likely to have male births after the reform. Our findings suggest that the increase in total household income induced by the reform led to more sex selection.

APPENDIX A

MODEL APPENDIX

A. Mechanical difference

Following Almond and Currie (2011a), we consider a simple two-period childhood. Human capital h at the completion of childhood is produced with a CES technology:

$$h = A[\gamma(\bar{I}_1 + \mu_g)^{\phi} + (1-\gamma)I_2^{\phi}]^{\frac{1}{\phi}}, \tag{A.1}$$

where I_1 denotes investment in the first period of childhood *(in utero* in our context), and I_2 denotes investment in the second period (postnatal period). An exogenous shock to (predetermined) period 1 investment is denoted by μ_g. Because ultrasound technology had been largely unavailable, for simplicity, we assume that prenatal investment is fixed.

In the presence of son preference, we assume that the investment level in period 2 is higher for boys than for girls, that is, $I_2^{(b)} > I_2^{(g)}$, and parental investments in period 2 do not respond to the shock. The sign of the gender difference in human capital as response to the shock depends on ϕ. For $\phi < 1$, the shock has a larger effect in promoting human capital for boys than for girls.

B. Optimization

Now we consider parental responses to the shock. Parents value their consumption C and perceived income of their child y_c:

$$U_p = U(C, y_c) = B[\theta C^{\varphi} + (1-\theta) y_c^{\varphi}]^{\frac{1}{\varphi}}, \tag{A.2}$$

where $y_c = mh$ for boys and $y_c = nh$ for girls. We assume that the perceived return to human capital is higher for boys than for girls, that is, $m > n$. We also assume that parents view child as consumption instead of investment, i.e. they do not borrow against. Their budget constraint is:

$$\bar{I}_1 + I_2 + C = \bar{y}_p. \tag{A.3}$$

Let's consider boys first. Absent discounting, the marginal utility from consuming equals the marginal utility from investing:

$$\frac{\delta U}{\delta C} = \frac{\delta U}{\delta y_c} \frac{\delta y_c}{\delta I_2^*} \tag{A.4}$$

$$\theta(\bar{y}_p - \bar{I}_1 - I_2^*)^{\varphi-1} = (1-\theta)(mh)^{\varphi-1} mA[...]^{\frac{1}{\phi}-1}(1-\gamma) I_2^{*\phi-1} \tag{A.5}$$

$$\theta(\bar{y}_p - \bar{I}_1 - I_2^*)^{\varphi-1} = m^{\varphi}(1-\theta)(1-\gamma) A^{\varphi}[...]^{\frac{\varphi-\phi}{\phi}} I_2^{*\phi-1} \tag{A.6}$$

$$G(I_{2,}^*, \mu_g) = \theta(\bar{y}_p - \bar{I}_1 - I_2^*)^{\varphi-1} - m^{\varphi}(1-\theta)(1-\gamma) A^{\varphi}[...]^{\frac{\varphi-\phi}{\phi}} I_2^{*\phi-1} = 0 \tag{A.7}$$

$$\frac{\delta I_2^*}{\delta \mu_g} = -\frac{\frac{\delta G}{\delta \mu_g}}{\frac{\delta G}{\delta I_2^*}}$$

$$= \frac{m^{\varphi}(1-\theta)(1-\gamma)A^{\varphi}(\varphi-\phi)[...]^{\frac{\varphi-2\phi}{\phi}} I_2^{*\phi-1}\gamma(\bar{I}_1+\mu_g)^{\phi-1}}{\theta(1-\varphi)(\bar{y}_p-\bar{I}_1-I_2^*)^{\varphi-2} + m^{\varphi}A^{\varphi}(1-\theta)(1-\gamma)[...]^{\frac{\varphi-\phi}{\phi}} I_2^{*\phi-2}\left[(1-\phi)-(\varphi-\phi)(1-\gamma)I_2^{*\phi}[...]^{-1}\right]}. \tag{A.8}$$

We define B to be $(1-\theta)(1-\gamma)A^{\varphi}(\varphi-\phi)[...]^{\frac{\varphi-2\phi}{\phi}} I_2^{*\phi-1}\gamma(\bar{I}_1+\mu_g)^{\phi-1}$,

C to be $\theta(1-\varphi)(y_p-\bar{I}_1-I_2^*)^{\varphi-2} \geq 0$,

and D to be $A^{\varphi}(1-\theta)(1-\gamma)[...]^{\frac{\varphi-\phi}{\phi}} I_2^{*\phi-2}\left[(1-\phi)-(\varphi-\phi)(1-\gamma)I_2^{*\phi}[...]^{-1}\right]$.

Equation (15) is simplified as:

$$\frac{\delta I_2^*}{\delta \mu_g} = \frac{m^{\varphi}B}{C+m^{\varphi}D}. \tag{A.9}$$

For $\phi < \varphi$, $B > 0$. Define the component in bracket of D to be E: $(1-\phi)-(\varphi-\phi)(1-\gamma)I_2^{*\phi}[...]^{-1}$, and

$$(1-\phi) - \frac{(\varphi-\phi)(1-\gamma)I_2^{*\phi}}{[...]} = \frac{(1-\phi)\gamma(\bar{I}_1+\mu_g)^{\phi} + (1-\gamma)(1-\varphi)I_2^{*\phi}}{[...]} \tag{A.10}$$

For $\phi < 1$, $E > 0$, and thus $D > 0$. (17) is positive so that positive shocks in period 1 should be reinforced. Next, we compare (16) for boys and for girls:

$$\frac{\delta I_2^{(b)*}}{\delta \mu_g} - \frac{\delta I_2^{(g)*}}{\delta \mu_g} = \frac{(m^\varphi - n^\varphi)BC}{(C + m^\varphi D)(C + n^\varphi D)}. \tag{A.11}$$

For $m > n$, (18) is positive, suggesting that parents invest more on boys than on girls as response to the shock. Finally, consider the impact of the shock on later-life human capital for boys and for girls separately:

$$\frac{\delta h^{(b)}}{\delta \mu_g} = A[...]^{(b)\frac{1}{\phi}-1}\gamma(\bar{I}_1 + \mu_g)^{\phi-1} + \frac{\delta h^{(b)}}{\delta I_2^{(b)*}}\frac{\delta I_2^{(b)*}}{\delta \mu_g} \tag{A.12}$$

$$\frac{\delta h^{(g)}}{\delta \mu_g} = A[...]^{(g)\frac{1}{\phi}-1}\gamma(\bar{I}_1 + \mu_g)^{\phi-1} + \frac{\delta h^{(g)}}{\delta I_2^{(g)*}}\frac{\delta I_2^{(g)*}}{\delta \mu_g} \tag{A.13}$$

For $\phi < 1$, $A[...]^{(b)\frac{1}{\phi}-1}\gamma(\bar{I}_1+\mu_g)^{\phi-1} > A[...]^{(g)\frac{1}{\phi}-1}\gamma(\bar{I}_1+\mu_g)^{\phi-1}$, and $\frac{\delta^2 h}{\delta I_2^2} > 0$. Therefore, (19) > (20).

APPENDIX B

DATA APPENDIX

A. Precipitation data

We use the Global Surface Summary of Day Data produced by National Climate Data Center (NCDC). Throughout China, daily data on total precipitation amount (0.01 inches) are available from 225 weather stations from 1956 to 1964 and 536 stations from 1973 to 1984. In each year, we assign each county in the 1982 Census the precipitation data from the nearest weather station using longitude and latitude. Because the number of weather stations increases overtime, a county might be assigned different stations in different years, with relatively closer stations in more recent years.

To construct the measure of drought in March, for example, we first get the distribution of total precipitation in March from all years during 1956-1964 and 1973-1984 for each county. We then define drought in March as a binary variable that is equal to 1 if the monthly precipitation is below the bottom 20 percentile of the distribution for each county in each year and 0 otherwise. For drought in the whole growing season, we calculate the average monthly precipitation from March to September and use its distribution to define drought.

B. Construct size of high school graduates in census data

We first count the number of births born between 1978 and 1984 by county and year of birth in the 1990 Census. A strength of using the 1990 Census for cohort

size is that internal migration was still under strict control in 1990. We use the 2000 Census to calculate the fraction of population that ever went to high school or still in high school by county and year of birth. By matching cells in the 1990 and 2000 Census, we get the counts of high school students in each county-birth year cell in the 1990 Census.

Next, to match with the administrative data at the county-birth year-year of application level, we also need to further narrow down the cell in the 1990 Census by county, birth year and year of graduation. However, we do not observe year of graduation in any census data. Instead, we assume the age distribution in college entrance exams during 1999-2001 is the same as that of high school graduates who were born in 1978-1984. We calculate the fraction of applicants at a certain age (from 17 to 21) from the administrative data by county, and assign the fraction to each cell in the 1990 Census by county and year of birth. We then use this fraction as the probability of graduating at a certain age in each county-birth year cell, and get the size of county-birth year-year of graduation cell in the 1990 Census.

Lastly, we take the count of applicants by county, birth year and year of application in the administrative data and match with the count of high school graduates by county, birth year and year of graduation in the 1990 Census. We divide the cell size of applicants by the cell size of high school graduates, and get the probability of applying for college in each cell.

A final note is that we use the sample of first-time applicants (70%) in the administrative data for this test. Because we do not observe whether one repeat the last year in high school in census data, we only observe the cell size of high school graduates assuming that they graduate once. What we are concerned

about is whether the decision of a high school graduate to ever apply for college is endogenous to land reform, which can be test using the probability of first-time

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Figure B.1: Supply of high schools - total number of high schools in counties

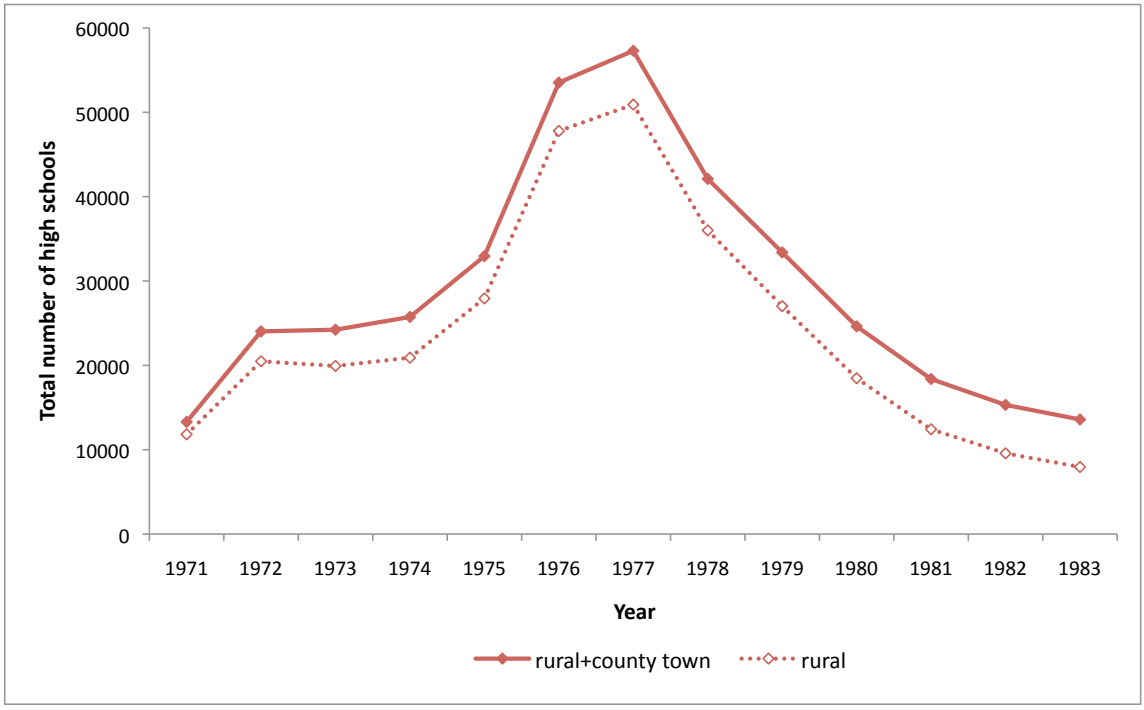

Figure B.2: Distribution of the first year of the school closures: 1977-1984

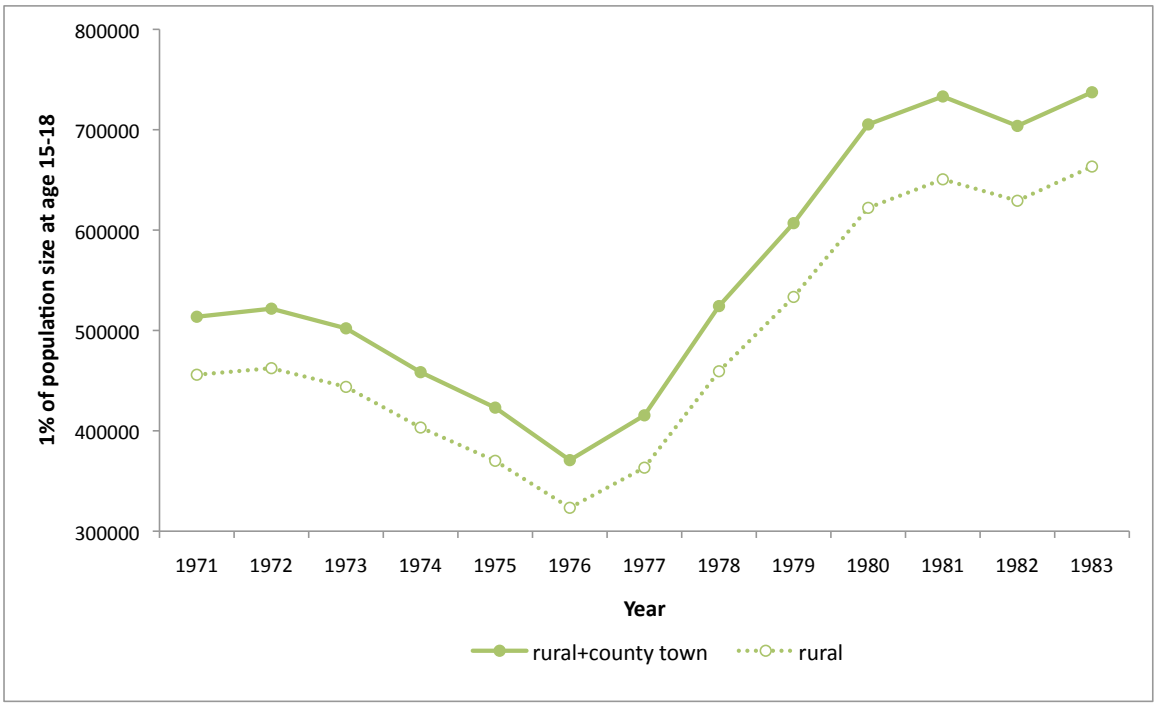

Figure B.3: Distribution of the timing of school closures

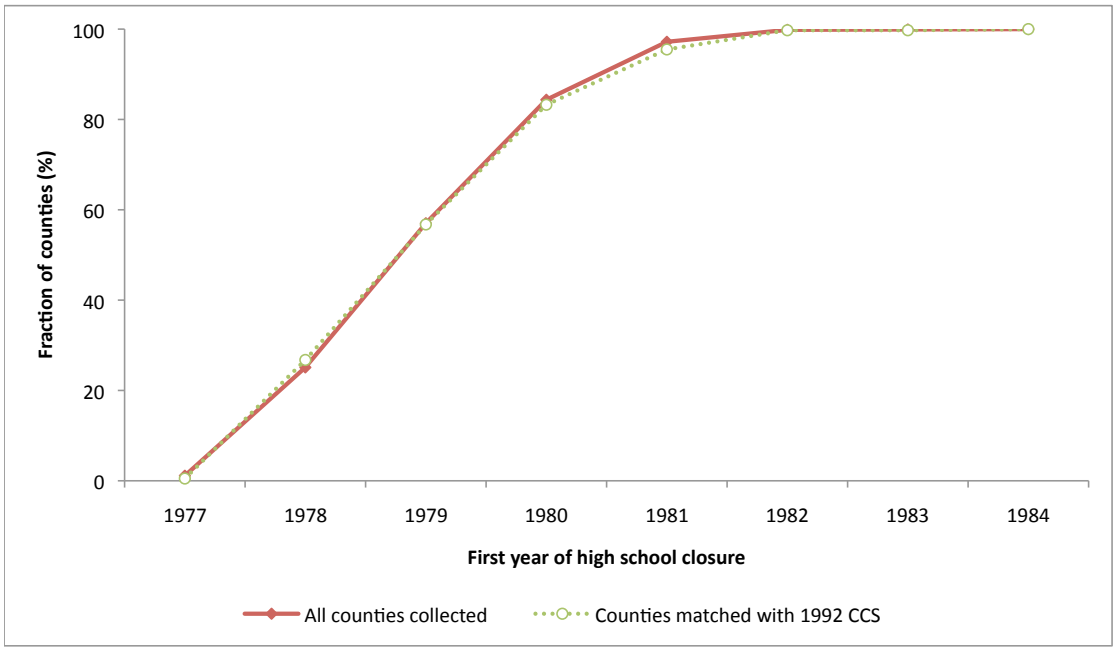

Figure B.4: Motherhood observed

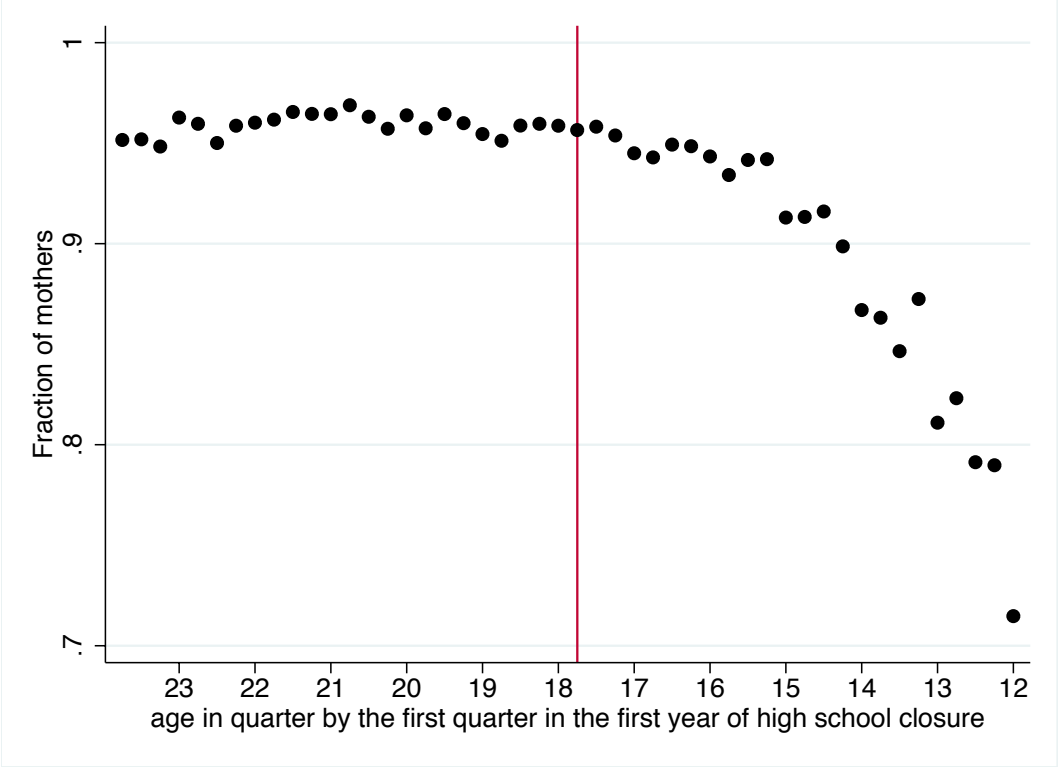

Figure B.5: Maternal age at first birth

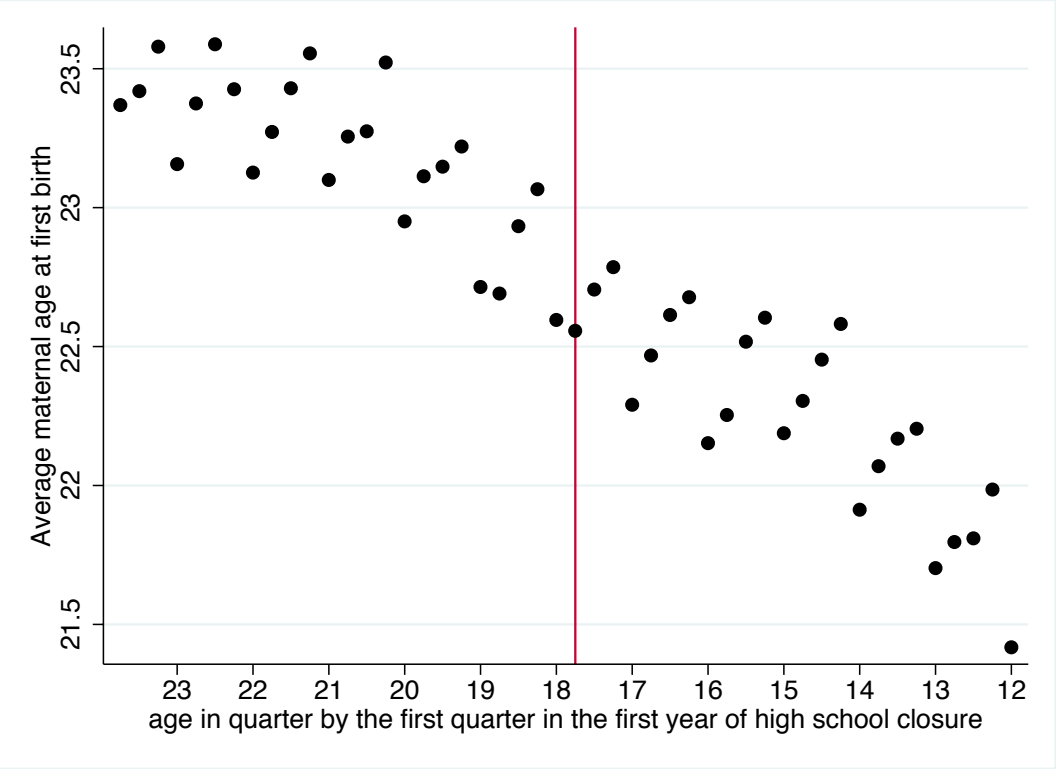

Figure B.6: High school completion

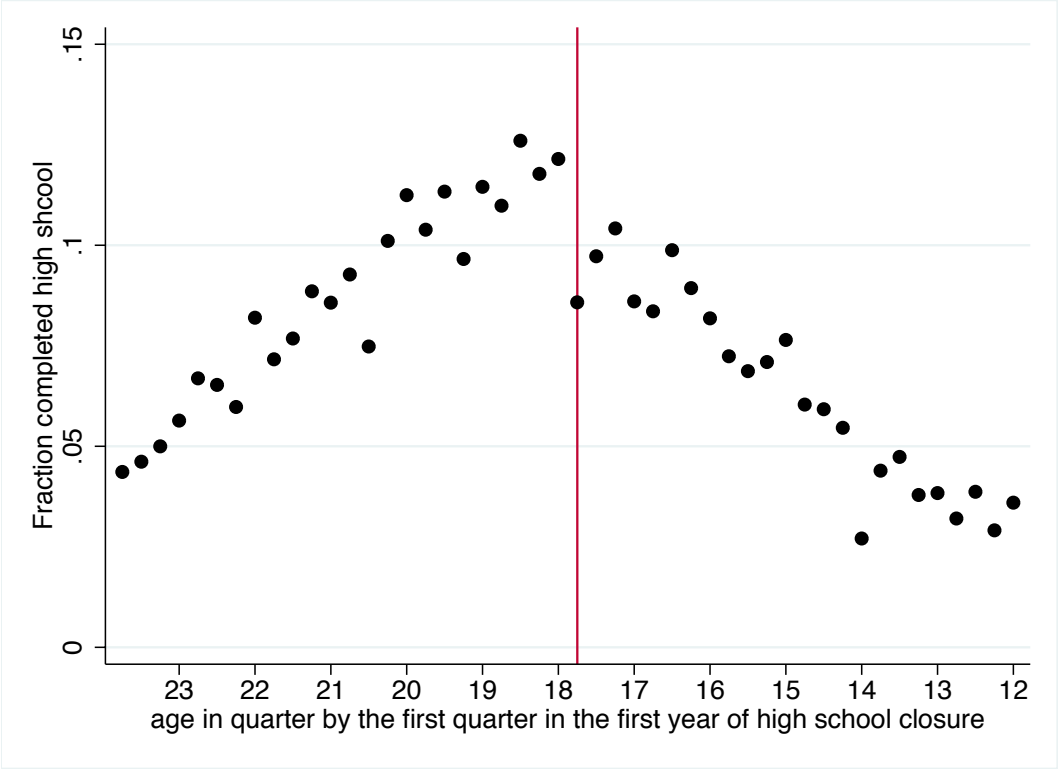

Figure B.7: Gestational age

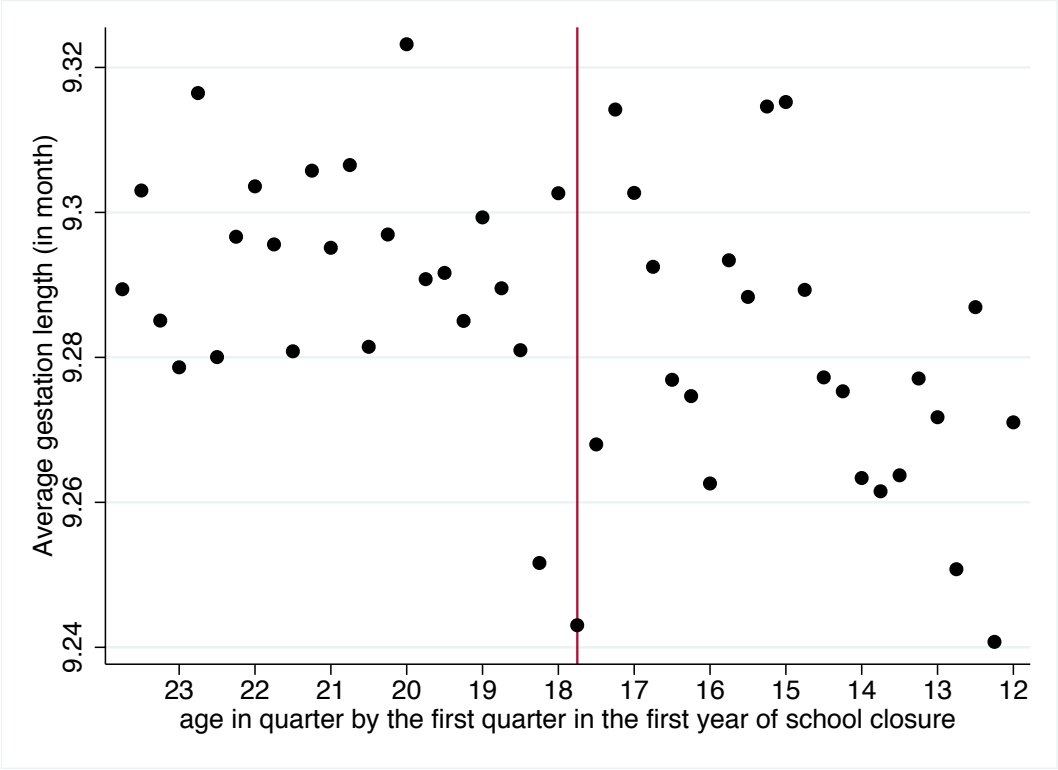

Figure B.8: Prematurity

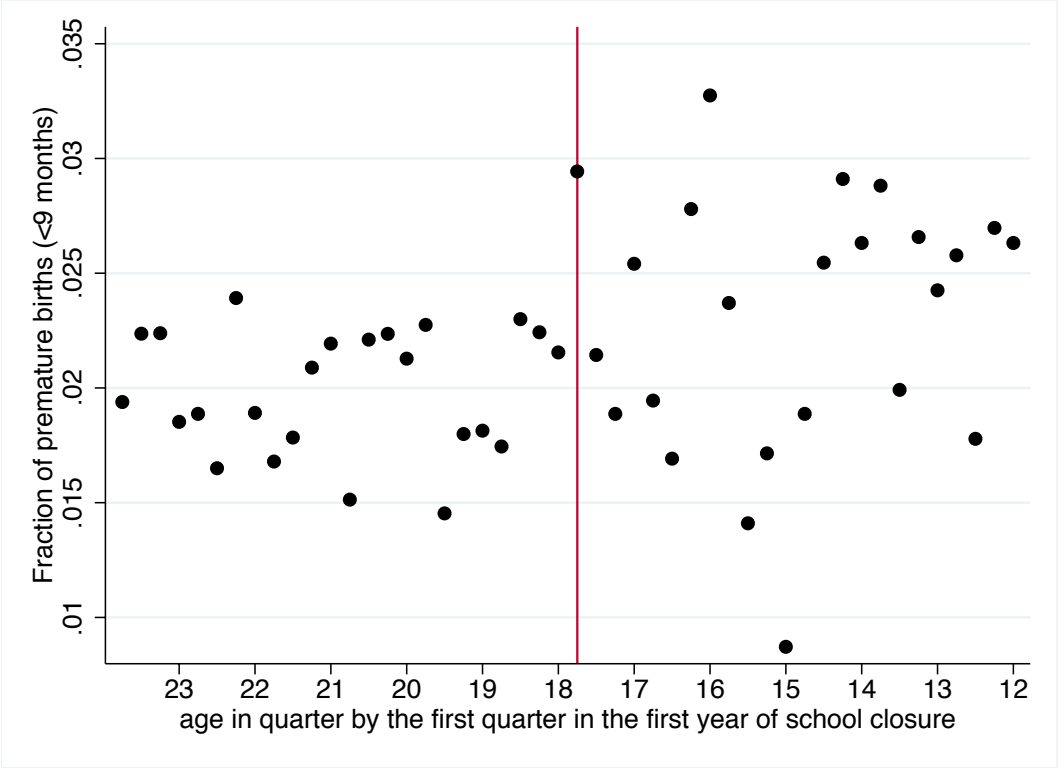

Figure B.9: Neonatal mortality rate

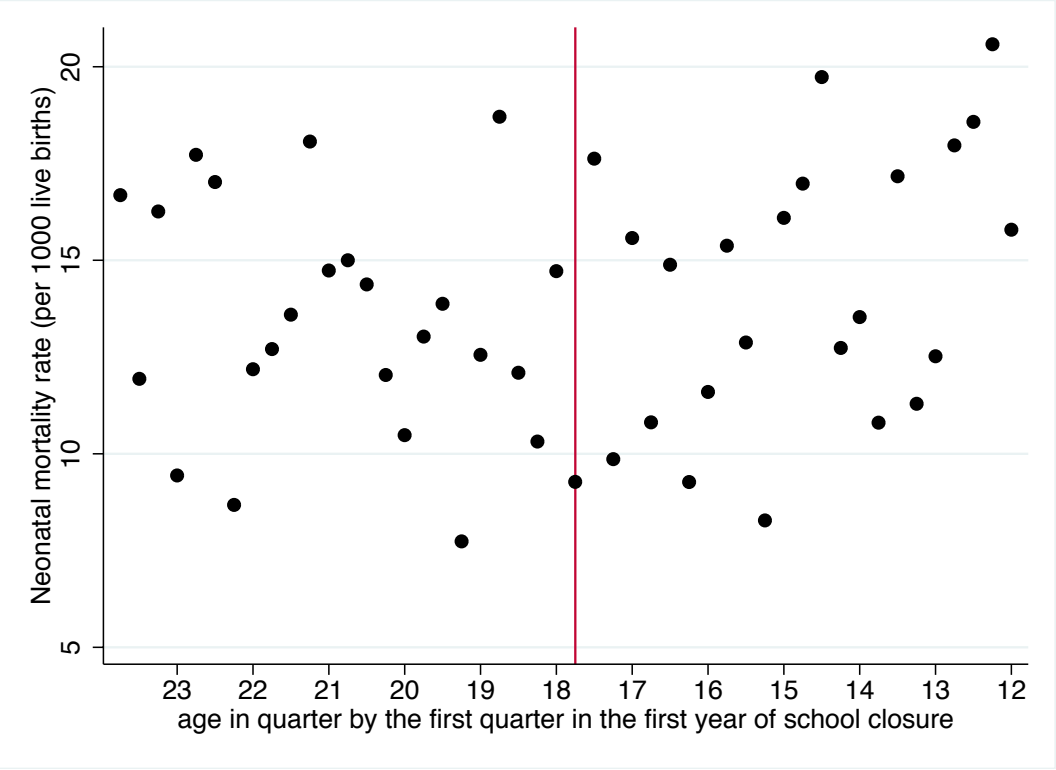

Figure B.10: Infant mortality rate

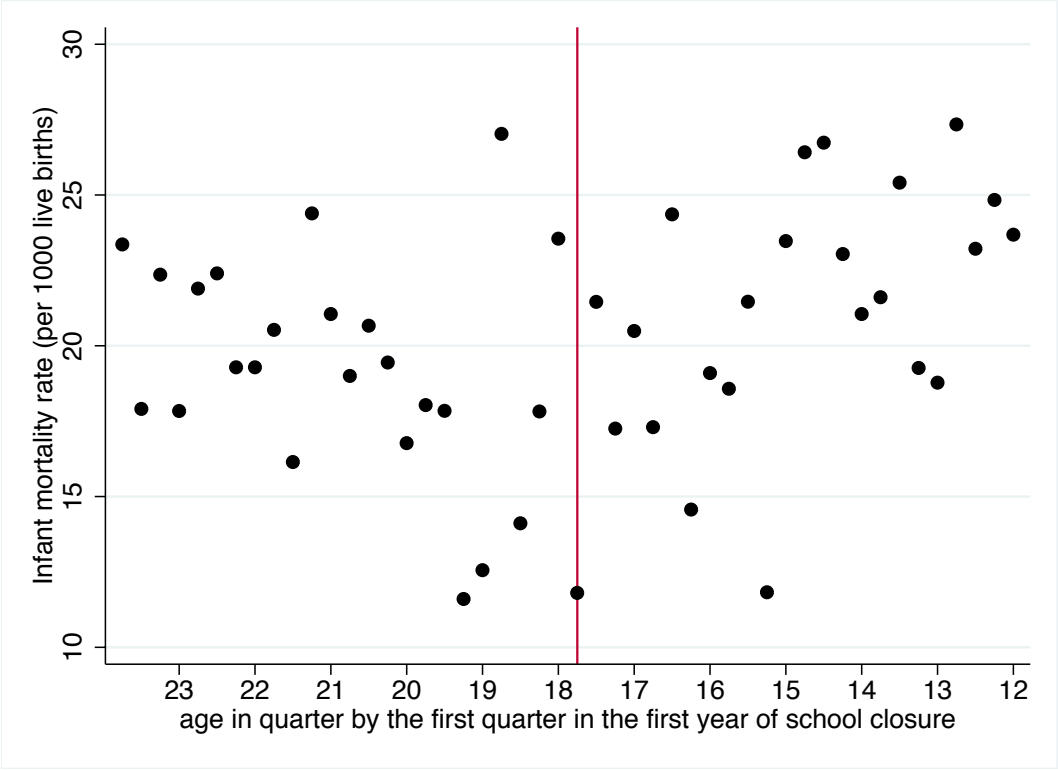

Figure B.11: Birth weight

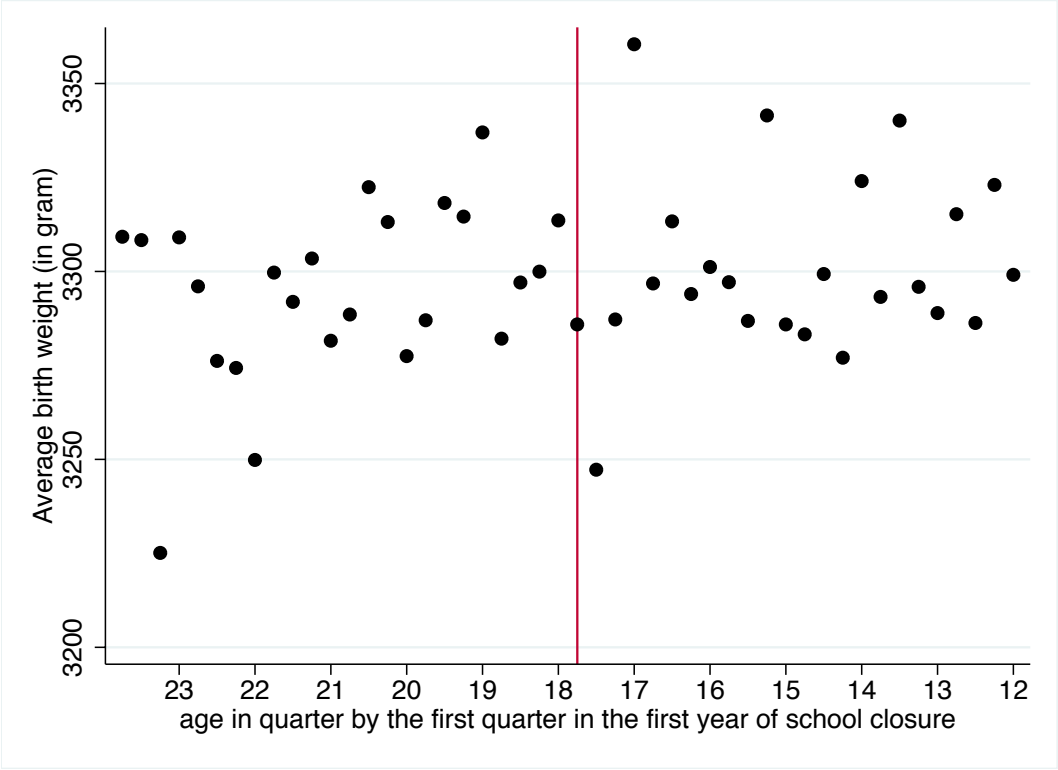

Figure B.12: Low birth weight

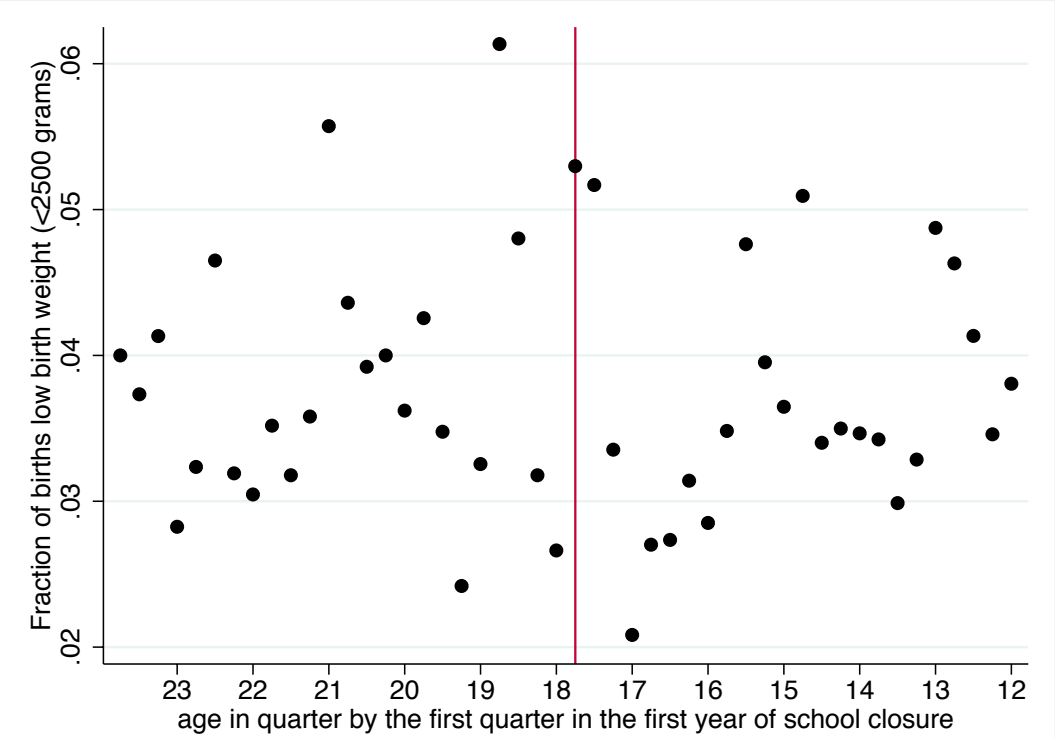

Figure B.13: Land reform timing

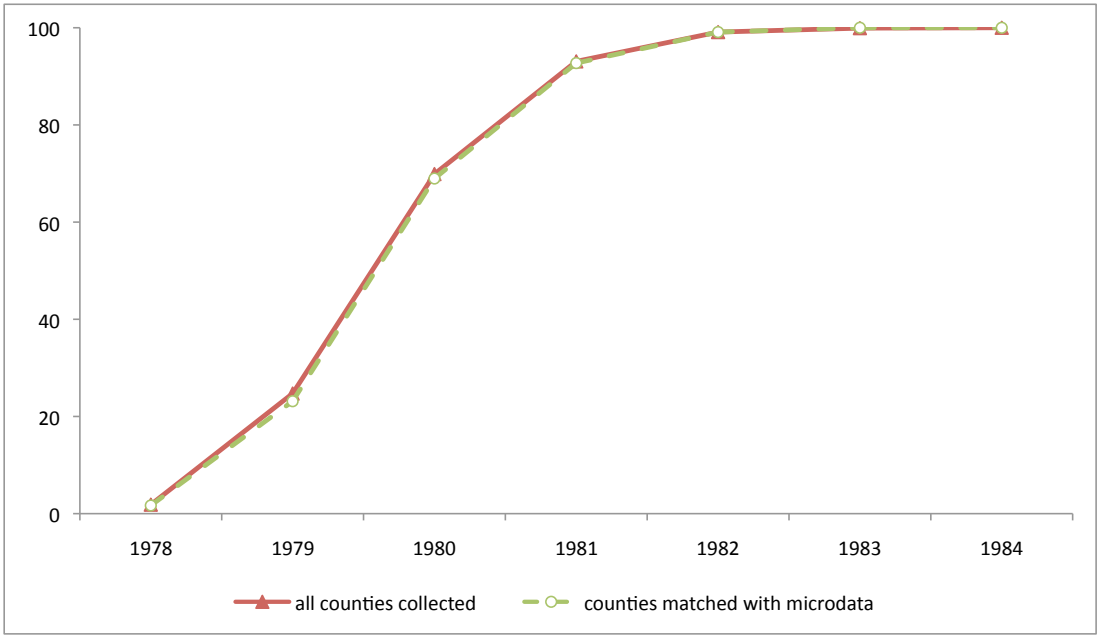

Figure B.14: Timing of land reform vs. One Child Policy

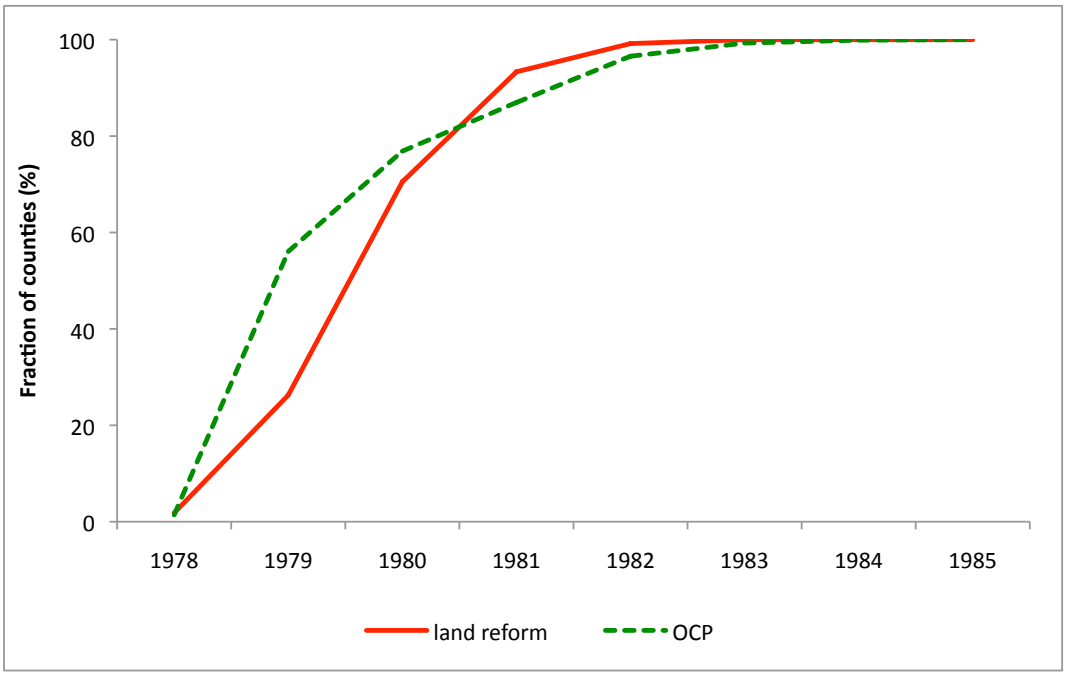

Figure B.15: Sex ratio of the first child

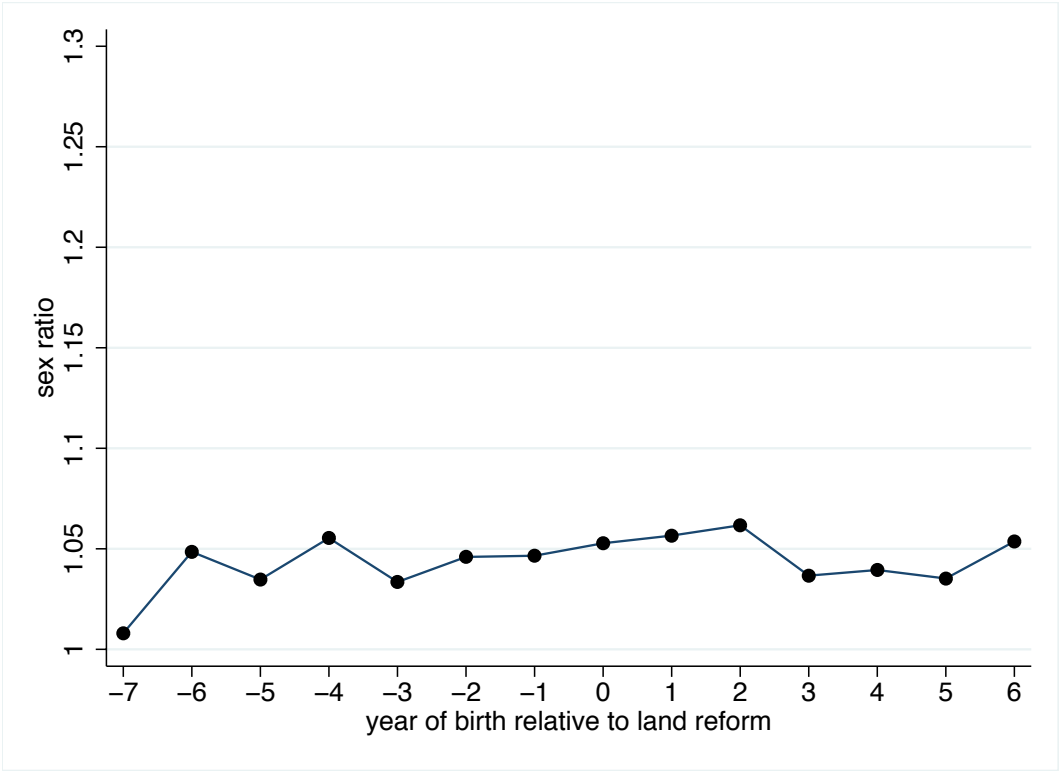

Figure B.16: Sex ratio of the second child

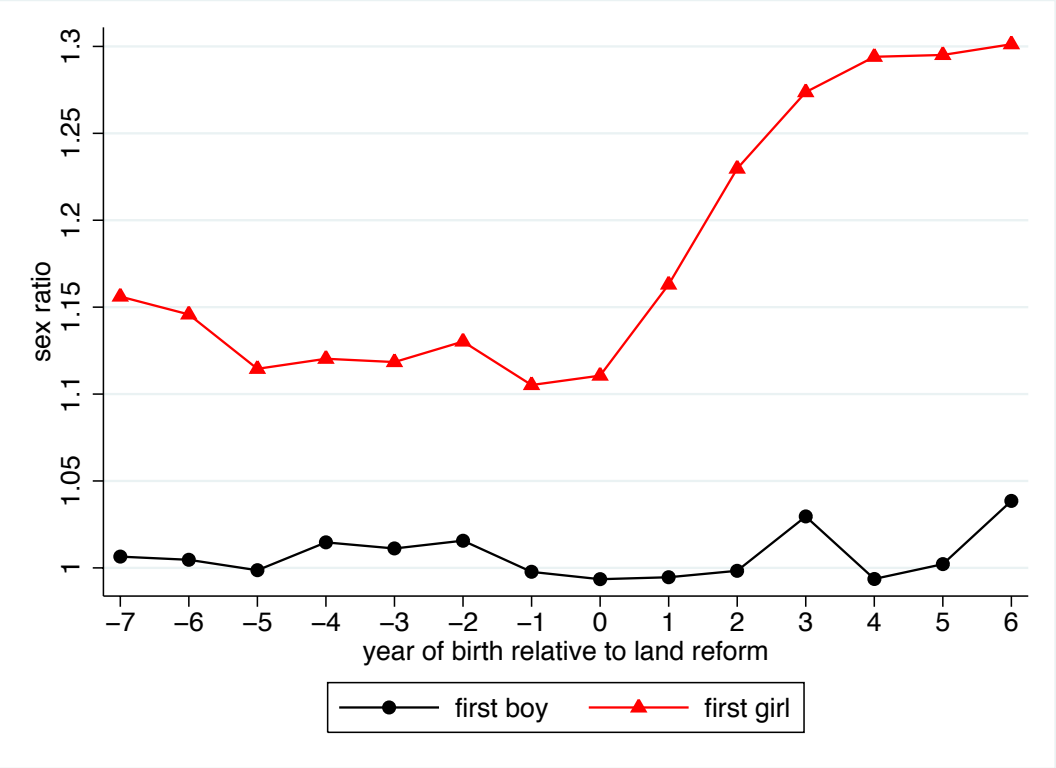

Table B.1: Summary Statistics

	Panel A: Women and Mothers			
	Women		Mothers	
	mean	obs.	mean	obs.
no schooling	0.235	66383	0.24	61059
primary school	0.396	66383	0.4	61059
middle school	0.294	66383	0.29	61059
high school	0.073	66383	0.071	61059
college	0.0017	66383	0.0014	61059
working	0.9	66383	0.9	61059
in an off-farm job	0.062	59683	0.057	54831
in a white-collar job	0.021	59683	0.02	54831
married	0.97	66383	0.9996	61059
age at first birth			22.7	61059
			(2.41)	
	Panel B: All Births and First Birth			
	First Birth		All Birth	
	mean	obs.	mean	obs.
B1: Infant Health Outcomes				
gestational age (in months)	9.28	61047	9.29	125826
	(0.52)		(0.52)	
prematurity (gestational age<9 months)	0.022	61047	0.019	125826
neonatal mortality rate (per 1000 births)	14	58329	15	122375
infant mortality rate (per 1000 births)	20	58329	22	122375
birth weight (in grams)	3299	22444	3312	41865
	(542)		(538)	
low birth weight	0.037	22444	0.034	41865
B2: Health Behaviors of Mothers				
any prenatal visit	0.51	59752	0.44	123273
prenatal visit in the first trimester	0.145	59752	0.112	123273
delivered in hospital	0.246	58300	0.179	122307

Table B.2: Reduced form on fertility choices

Panel A: Selection into motherhood and teenage pregnancy					
			(1)		(2)
			Motherhood observed=1		Had the 1st birth under 18=1
1{age at school closure=<17.75}			-0.0067		-0.0043
			[0.0054]		[0.0035]
Dependend variable mean			0.92		0.012
Observations			66383		61059
R-squared			0.139		0.057
Panel B: Maternal age at first birth					
Dependent variable: Each specific maternal age at first birth					
13	0.0002	21	0.0329***	29	-0.0038
	[0.0002]		[0.0105]		[0.0031]
14	0.0003	22	-0.0249**	30	0.0008
	[0.0006]		[0.0108]		[0.0016]
15	-0.0013*	23	0.0037	31	-0.0020*
	[0.0007]		[0.0100]		[0.0012]
16	0.0020	24	-0.0061	32	-0.0013*
	[0.0013]		[0.0094]		[0.0008]
17	-0.0039*	25	-0.0045	33	-0.0003
	[0.0021]		[0.0081]		[0.0004]
18	-0.0043	26	-0.0013	34	0.00002
	[0.0035]		[0.0065]		[0.0002]
19	0.0072	27	-0.0036	35	-0.0003*
	[0.0059]		[0.0048]		[0.0002]
20	0.0120	28	-0.0012	36	-0.0002
	[0.0074]		[0.0030]		[0.0001]
sample mean			22.7		
Observation			61059		

Note: Robust standard errors clustered at county level are reported in brackets. The sample in column (1) of Panel A includes women at age 12-23 by the first quarter in the first year of school closure, while the sample in column (2) of Panel A and Panel B includes the same cohorts of mothers. In Panel B, each dependend variable is an indicator for a specific maternal age at first birth, and each parameter is from a separate regression on a specific maternal age. All regressions include a quadratic cohort trend and its interaction with the indicator of at or younger than age 17.75 by the school closure, quarter of birth effects, county fixed effects and county specific quadratic trend.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.3: First stage

	High school completion							
	All mother sample				Birth weight sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1{age at school closure=<17.75}	-0.028***	-0.028***	-0.028***	-0.027***	-0.037***	-0.037***	-0.035***	-0.036***
	[0.005]	[0.005]	[0.005]	[0.005]	[0.010]	[0.010]	[0.010]	[0.010]
Linear cohort trend	X	X	X	X	X	X	X	X
QOB FE		X	X	X		X	X	X
County FE			X	X			X	X
County specific linear trend				X				X
Dependent variable mean at age 18			0.12				0.16	
Dependent variable mean			0.073				0.095	
Observations	61059	61059	61059	61059	22444	22444	22444	22444
R-squared	0.009	0.009	0.049	0.069	0.012	0.012	0.074	0.114

Note: Robust standard errors clustered at county level are reported in brackets. This table reports the first stage estimates, β_1, in equation (2). The dependend variable is an indicator that is equal to 1 if one completed high school, and 0 otherwise. The sample of mothers includes those at age 12-23 by the first quarter in the first year of school closure. The sample in column (1)-(4) include all mothers, while the sample in column (5)-(8) include mothers that report the birth weight of their first birth.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.4: OLS, Reduced form and IV estimation

	Gestational age (in months)	Prematurity	Mortality in 1 month	Mortality in 1 year	Birth weight (in grams)	Low birthweight
	Panel A: OLS					
high school completion	-0.0016	0.0019	-0.0025	-0.0039**	57.3776***	-0.0097**
	[0.0058]	[0.0024]	[0.0016]	[0.0019]	[12.1532]	[0.0040]
Observations	61047	61059	58329	58329	22444	22444
R-squared	0.547	0.035	0.016	0.020	0.147	0.052
	Panel B: Reduced form					
1{age at school closure=<17.75}	-0.009	0.003	-0.0018	-0.0016	-10.35	-0.004
	[0.007]	[0.003]	[0.0022]	[0.0026]	[14.59]	[0.005]
Observations	61,047	61,047	58,329	58,329	22444	22444
R-squared	0.554	0.048	0.033	0.035	0.183	0.089
	Panel C: IV estimation					
high school completion	0.334	-0.093	0.070	0.065	246.29	0.135
	[0.244]	[0.092]	[0.087]	[0.100]	[380.36]	[0.144]
Dependent variable mean	9.3	0.022	0.014	0.02	3299	0.037
Observations	61,047	61,047	58,329	58,329	22,444	22,444
F statistic in first stage	28.35	28.49	23.77	23.77	14.09	14.09

Note: Robust standard errors clustered at county level are reported in brackets. Panel A reports OLS estimates, with maternal age, county fixed effects and mother's year of birth effects controlled for. Panel B reports the reduced form estimates, β_2, in equation (3). Panel B reports IV estimates on the effects of mother's high school education on infant health outcomes. The instrument variable in Panel C is the indicator variable of being at age 17.75 or younger by the first quarter in the first year of school closure. Each parameter is from a separate regression on one outcome. The sample of mothers includes those at age 12-23 by the first quarter in the first year of school closure. Regressions in Panel B and C include a linear cohort trend and its interaction with the indicator of being at age 17.75 or younger, quarter of birth effects, county fixed effects and county specific linear trends.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.5: Other relevant outcomes (1992 CCS)

	Reduced form					
	Labor market outcomes			Prenatal care and delivery		
	(1)	(2)	(3)	(4)	(5)	(6)
	working	off-farm employment	white-collar jobs	any prenatal check	prenatal check in the 1st trimester	delivered at hospital
1{age at school closure=<17.75}	0.002	0.002	-0.001	0.003	0.002	0.001
	[0.004]	[0.004]	[0.003]	[0.009]	[0.006]	[0.007]
Observations	61059	54831	54831	59752	59752	58300
R-squared	0.419	0.224	0.067	0.254	0.161	0.257

Note: Robust standard errors clustered at county level are reported in brackets. This table reports reduced form estimates on the effects of maternal exposure to the school closures on other outcomes. Each parameter is from a separate regression on one outcome. The sample of mothers includes those at age 12-23 by the first quarter in the first year of school closure. All regressions include a linear cohort trend and its interaction with the indicator of at or younger than age 17.75 by the school closure, quarter of birth effects, county fixed effects and county specific linear trend.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.6: Paternal characteristics (1990 Census)

	First stage	Reduced form			
	wife	husband			
	(1)	(2)	(3)	(4)	(5)
	high school completion	high school completion	working	off-farm employment	white-collar jobs
1{wife's age at school closure=<17.75}	-0.025***	-0.013***	0.001	0.003	-0.001
	[0.004]	[0.005]	[0.001]	[0.004]	[0.002]
Observations	145110	145110	145110	144801	144801
R-squared	0.069	0.058	0.009	0.188	0.041

Note: Robust standard errors clustered at county level are reported in brackets. Using the sample of the same 400 counties in the 1990 Census, this table reports first stage estimate for women and reduced form estimates on their husbands' education and labor market outcomes. Each parameter is from a separate regression on one outcome. The sample of women includes those at age 12-23 by the first quarter in the first year of school closure. All regressions include a linear cohort trend and its interaction with the indicator of at or younger than age 17.75 by the school closure, quarter of birth effects, county fixed effects and county specific linear trend.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.7: Test the continuity of predetermined characteristics

	Grandparental characteristics					Women characteristics			
	grandmother			grandfather					
	middle school completion	high school completion	age at birth	middle school completion	high school completion	parity	months of breastfeeding	mortality in 1 month	mortality in 1 year
					Panel A: 1960				
1{born in Q2 or after}	-0.009	0.003	0.066	0.011	-0.005	-0.015	-0.276	0.003	-0.001
	[0.007]	[0.004]	[0.110]	[0.016]	[0.009]	[0.037]	[0.334]	[0.008]	[0.011]
Dependent variable mean	0.05	0.01	24.4	0.2	0.06	2.15	15.04	0.044	0.094
Observations	103696	103696	103696	87954	87954	103682	103695	103696	103696
R-squared	0.189	0.205	0.252	0.205	0.200	0.157	0.134	0.072	0.092
					Panel B: 1961				
1{born in Q2 or after}	0.002	0.002	-0.117	0.003	0.004	-0.054	0.226	-0.017**	-0.029**
	[0.008]	[0.004]	[0.124]	[0.015]	[0.009]	-0.055	[0.357]	[0.009]	[0.012]
Dependent variable mean	0.05	0.01	24.6	0.2	0.06	2.17	15.04	0.044	0.093
Observations	99349	99349	99349	84401	84401	99336	99348	99349	99349
R-squared	0.196	0.215	0.229	0.208	0.204	0.150	0.137	0.073	0.093
					Panel C: 1962				
1{born in Q2 or after}	0.006	-0.001	0.088	-0.022*	-0.005	-0.017	0.348	-0.005	0.005
	[0.006]	[0.003]	[0.106]	[0.012]	[0.008]	[0.037]	[0.249]	[0.007]	[0.009]
Dependent variable mean	0.05	0.01	24.7	0.2	0.06	2.2	15.04	0.043	0.093
Observations	94323	94323	94323	80237	80237	94310	94322	94323	94323
R-squared	0.200	0.218	0.208	0.209	0.209	0.144	0.140	0.076	0.096
					Panel D: 1963				
1{born in Q2 or after}	0.010	0.000	-0.028	0.007	0.003	0.000	0.320	0.007	0.004
	[0.006]	[0.003]	[0.100]	[0.011]	[0.006]	[0.033]	[0.227]	[0.005]	[0.008]
Dependent variable mean	0.05	0.01	25	0.2	0.06	2.24	15.02	0.043	0.091
Observations	88562	88562	88562	75470	75470	88550	88561	88562	88562
R-squared	0.206	0.229	0.188	0.213	0.217	0.141	0.141	0.079	0.099

Note: Robust standard error clustered at county level are reported in brackets.This table reports estimates on the mean shift of the dependent variable at being born at the second quarter or after for each year from 1960 to 1963. For each year, the sample includes rural female births born 24 quarters before and 24 quarters after the second quarter of that particular year. All regressions include a quadratic cohort trend and its interaction with the indicator of being borning in the second quarter or after for that year (1960-1963), quarter of birth fixed effects, year of birth fixed effects, county fixed effects and county specific quadratic trend. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.8: First and reduced form including one's exposure to the land reform

	First stage		Reduced form					
	Mother completed high school		Gestational age		Prematurity		Mortality in 1 month	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
1{age at school closure=<17.75}	-0.017***	-0.016***	-0.015**	-0.016**	0.006*	0.005*	-0.003	-0.004
	[0.006]	[0.005]	[0.0074]	[0.008]	[0.003]	[0.003]	[0.003]	[0.003]
1{age at land reform=<17.75}		-0.002		0.004		0.002		0.003
		[0.006]		[0.009]		[0.003]		[0.003]
Observations	47553	47553	47543	47543	47543	47543	45311	45311
R-squared	0.069	0.069	0.533	0.533	0.048	0.048	0.033	0.033
			Mortality in 1 year		Birth weight		Low birthweight	
			(1)	(2)	(1)	(2)	(1)	(2)
1{age at school closure=<17.75}			-0.003	-0.004	-9.38	-16.99	-0.003	0.0002
			[0.003]	[0.003]	[17.65]	[18.28]	[0.007]	[0.007]
1{age at land reform=<17.75}				0.005		23.99		-0.01
				[0.003]		[21.35]		[0.008]
Observations			45311	45311	16573	16573	16573	16573
R-squared			0.035	0.035	0.176	0.176	0.090	0.090

Note: Robust standard errors clustered at county level are reported in brackets. Using 306 counties that have timing information on both high school closure and land reform, this table reports estimates of both the first stage and the reduced form on infant health outcomes. The sample of mothers include those at age 12-23 by the first quarter in the first year of school closure. All column (1) control for a linear cohort trend and its interaction with the indicator of at or younger than age 17.75 by the school closure, quarter of birth effects, county fixed effects and county specific linear trend. All column (2) also include an indicator variable that is equal to 1 if one was 17.75 or younger by the first quarter in the first year of land reform.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.9: Heterogeneous effects of the school closures

	Pre-existing education level		Population density in 1982	
	below median	above median	below median	above median
	(1)	(2)	(1)	(2)
High school completion	0.004	-0.054***	-0.029***	-0.030***
	[0.005]	[0.008]	[0.007]	[0.008]
	29367	31692	27716	29649
	(0.11)	(0.17)	(0.096)	(0.154)
Gestational age	-0.007	-0.011	-0.004	-0.011
	[0.010]	[0.009]	[0.009]	[0.010]
	29363	31684	27713	29642
Prematurity	0.005	0.001	0.0003	0.004
	[0.004]	[0.003]	[0.003]	[0.004]
	29363	31684	27713	29642
Mortality in 1 month	-0.001	-0.003	-0.003	-0.001
	[0.004]	[0.003]	[0.003]	[0.003]
	27625	30704	26272	28536
Mortality in 1 year	0.001	-0.004	-0.001	-0.002
	[0.004]	[0.003]	[0.004]	[0.003]
	27625	30704	26272	28536
Birth weight	-10.662	-10.647	-13.252	-8.201
	[22.812]	[18.932]	[21.315]	[20.002]
	10299	12145	8276	12525
LBW	0.005	-0.011	-0.003	-0.005
	[0.008]	[0.007]	[0.010]	[0.007]
	10299	12145	8276	12525

Note: This table reports estimates of both the first stage and the reduced form estimates on infant health outcomes by various categories of counties. All column (1) report estimates in the sample of counties below the median of each category and all column (2) report estimates in counties above the median. All regressions include a linear cohort trend and its interaction with the indicator of at or younger than age 17.75 by the school closure, quarter of birth effects, county fixed effects and county specific linear trend.Robust standard errors clustered at county level are reported in brackets. The dependent variable mean of high school completion at age 18 is reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

The pre-existing education level is measured by the fraction of high school completion among pre-exposure cohorts is calculated from mothers at age 18-23 by the first quarter in the first year of the school closure in the 1992 CCS. The median is 6.2 percent.

Population density is from the GIS data of 1982 Census. The median is 227 residents per square kilometers.

Table B.10: Drought (time variant) and reform timing

	Dependent variable: first year of land reform (1978-1984)				
	(1)	(2)	(3)	(4)	(5)
	March-September	March	April	May	June
Drought in year t	-0.011	-0.021***	-0.037***	-0.006	-0.004
	[0.009]	[0.008]	[0.008]	[0.008]	[0.009]
Drought in year t-1	0.001	-0.026***	-0.027***	0.004	-0.009
	[0.008]	[0.007]	[0.008]	[0.008]	[0.008]
County FE	X	X	X	X	X
Year FE	X	X	X	X	X
County linear trend	X	X	X	X	X
Observations	7,306	7,306	7,306	7,306	7,306
R-squared	0.768	0.769	0.769	0.768	0.768

Notes: The dependent variable is the first year of land reform, which is equal to 1 at the year reform started, 0 prior to the reform, and missing value after the first year. Drought is a dummy variable which is equal to 1 if the average monthly precipitation is below bottom 20th percentile in the precipitation distribution during 1957-1984 and 0 otherwise (precipitation data between 1965-1972 are unavailable). We include two drought indicators, one in the current year and another the year before. In the first column we measure drought using monthly average precipitation in the whole period between March and September, while each of the other column headings presents the single month in which drought is measured. All regressions include county fixed effects, year effects and county linear trends. The sample includes 1194 counties and the time span is from 1975 to 1984. Robust standard errors are reported in brackets.

Table B.11: Time-invariant determinants of reform timing

	Dependent variable: first year of land reform (1978-1984)				
	Univariate			Multivariate	
		Obs	R-squared		
ln (grain output per capita 1976)	0.250**	481	0.011	0.399***	
	[0.121]			[0.127]	
ln (distance to province capital)	0.075**	1,201	0.003	0.004	-0.037
	[0.036]			[0.061]	[0.039]
ln (labor force density 1976)	-0.147***	1,117	0.044	-0.173***	-0.150***
	[0.022]			[0.046]	[0.028]
ln (famine intensity 1959-1961)	-0.494***	1,189	0.033	-0.289**	-0.342***
	[0.081]			[0.144]	[0.089]
ln (distance to beijing)	-0.074*	1,201	0.003	-0.123	-0.130***
	[0.038]			[0.078]	[0.041]
Observations				438	1,114
R-squared				0.095	0.070

Notes: The dependent variable is the first year of land reform, which varies from 1978 to 1984. For univariate analysis, each estimate is from a separate regression. Multivariate regression includes all dependent variables. Data on grain output per capita in 1976 are collected from county gazetteers, and only 438 counties report this information. Distance to beijing and distance to province capital city are in kilometers and are obtained from GIS map of 1982 Census. Labor force density in 1976 is calculated by population size aged 16-60 in 1976 divided by area. Using 1982 Census, we measure the 1959-61 famine intensity by the average cohort size born in 1953-1957 divided by the average cohort size born in 1959-1961. Robust standard errors are reported in brackets.

Table B.12: Land reform and grain output per capita

	dependent variable: ln(grain output per capita)	
	(1)	(2)
	Panel A: diff-in-diff estimate	
1{1 year after reform or later}	0.032**	0.026*
	[0.015]	[0.015]
	Panel B: one estimate for each year after reform	
1{1 year after reform}	0.056***	0.053***
	[0.018]	[0.018]
1{2 years after reform}	0.062**	0.055*
	[0.031]	[0.031]
1{3 years after reform}	0.102**	0.098**
	[0.045]	[0.045]
1{4 years after reform}	0.160**	0.161***
	[0.063]	[0.062]
1{5 years after reform}	0.155*	0.162**
	[0.084]	[0.083]
1{6 years after reform}	0.132	0.168
	[0.127]	[0.124]
County FE	X	X
Year FE	X	X
County linear trends	X	X
Initial control*Year FE		X
Spring drought in t and t-1		X
Observations (panel A)	4,188	4,188
R-squared (panel A)	0.868	0.874
Observations (panel B)	4,188	4,188
R-squared (panel B)	0.869	0.875

Notes: This table reports estimates of reform on log grain output per capita. Panel A reports the difference-in-difference estimate. Panel B reports one estimate for each year after the reform within the difference-in-difference framework. Regression in column (1) includes county fixed effects, year effects and county linear time trends. Column (2) further includes determinants of reform timing interacted with time fixed effects and drought in March and Spring in year t and t-1. The analysis sample includes 382 counties that report grain output data in 1974-1984 and that are matched with the data of control variables. Robust standard errors are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.13: Summary Statistics

	All sample		Female		Male	
	Mean	Obs	Mean	Obs	Mean	Obs
Total score	500.07	1918244	499.35	724471	500.52	1193773
	[94.72]		[91.28]		[96.74]	
Math score	501.78	1908119	498.13	720391	503.99	1187728
	[94.86]		[92.23]		[96.36]	
Chinese score	496.27	1909643	502.35	722327	492.57	1187316
	[97.97]		[95.92]		[99.01]	
English score	495.86	1906641	513.78	721309	484.96	1185332
	[95.81]		[90.80]		[97.12]	
Percentile of total score	49.79	1918244	49.61	724471	50.04	1193773
	[27.91]		[27.3]		[28.3]	
Percentile of math score	50.29	1908119	49.28	720391	50.99	1187728
	[28.08]		[27.5]		[28.4]	
Percentile of Chinese score	48.86	1909643	50.78	722327	47.89	1187316
	[28.52]		[28.3]		[28.6]	
Percentile of English score	48.77	1906641	54.29	721309	45.66	1185332
	[28.01]		[27]		[28.3]	
admitted by any college	0.530	1918244	0.555	724471	0.514	1193773
	[0.499]		[0.497]		[0.500]	
admitted by a 4-year university	0.251	1918244	0.240	724471	0.258	1193773
	[0.434]		[0.427]		[0.437]	
admitted by a first-tier university	0.106	1918244	0.098	724471	0.110	1193773
	[0.308]		[0.298]		[0.314]	
admitted by a top 100 university	0.067	1918244	0.057	724471	0.073	1193773
	[0.25]		[0.231]		[0.260]	
admitted by an elite university	0.024	1918244	0.018	724471	0.028	1193773
	[0.153]		[0.133]		[0.164]	
in utero exposure to land reform	0.438	1918244	0.451	724471	0.429	1193773
	[0.496]		[0.498]		[0.495]	

Notes: standard deviations are reported in brackets. The sample includes test takers born between 1978 and 1984 and who took the college entrance exams in 1999-2001.

Table B.14: Effects of exposure to land reform on academic performance

	Test score in percentile				Admission outcomes				
	Total	Math	Chinese	English	Any college	Four-year college	First-tier college	Top 100 college	Elite college
1{born 2 years after reform or later}	0.251*	0.308**	0.114	0.249*	0.002	0.002	0.003**	0.003***	0.001
	[0.140]	[0.130]	[0.132]	[0.146]	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]
County FE	X	X	X	X	X	X	X	X	X
YOB FE	X	X	X	X	X	X	X	X	X
Year of exam FE	X	X	X	X	X	X	X	X	X
County linear trends	X	X	X	X	X	X	X	X	X
Initial control*YOB FE	X	X	X	X	X	X	X	X	X
Spring drought in t and t-1	X	X	X	X	X	X	X	X	X
effect size relative to mean	0.005	0.006	0.002	0.005	0.004	0.008	0.028	0.045	0.042
dependent variable mean	49.79	50.29	48.86	48.77	0.53	0.251	0.106	0.067	0.024
Observations	1,918,244	1,908,119	1,909,643	1,906,641	1,918,244	1,918,244	1,918,244	1,918,244	1,918,244
R-squared	0.056	0.049	0.045	0.060	0.074	0.053	0.046	0.029	0.018

Notes: This table reports estimated effects of prenatal exposure to land reform on test scores and admission outcomes. Each estimate is from a separate regression on one outcome. The sample include test takers born in 1978 and 1984 and who took the college extrance exams in 1999-2001. All regressions include county fixed effects, year of birth effects, year of exam effects, county linear cohort trends, initial controls interacted with year of birth effects and drought in March and Spring in the current year and one year before. We also report the effect size of land reform on each outcome relative to the dependent variable mean. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.15: Effects of exposure to land reform on academic performance, by gender

	Test score in percentile				Admission outcomes				
	Total	Math	Chinese	English	Any college	Four-year college	First-tier college	Top 100 college	Elite college
					Panel A: Female				
1{born 2 years after reform	0.131	0.160	-0.016	0.209	0.001	0.001	0.001	0.002*	0.0003
or later}	[0.178]	[0.178]	[0.179]	[0.185]	[0.003]	[0.003]	[0.002]	[0.001]	[0.001]
effect size relative to mean	0.003	0.003	0.0003	0.004	0.004	0.004	0.010	0.035	0.017
dependent variable mean	49.61	48.28	50.78	54.29	0.555	0.240	0.098	0.057	0.018
					Panel B: Male				
1{born 2 years after reform	0.348**	0.408***	0.203	0.321**	0.003	0.003	0.005***	0.004***	0.002*
or later}	[0.157]	[0.148]	[0.145]	[0.159]	[0.003]	[0.002]	[0.002]	[0.001]	[0.001]
effect size relative to mean	0.007	0.008	0.004	0.007	0.006	0.012	0.045	0.055	0.071
dependent variable mean	50.04	51.00	47.89	45.66	0.514	0.258	0.110	0.073	0.028
County FE	X	X	X	X	X	X	X	X	X
YOB FE	X	X	X	X	X	X	X	X	X
Year of exam FE	X	X	X	X	X	X	X	X	X
County linear trends	X	X	X	X	X	X	X	X	X
Initial control*YOB FE	X	X	X	X	X	X	X	X	X
Spring drought in t and t-1	X	X	X	X	X	X	X	X	X
Observations (female)	724,471	720,391	722,327	721,309	724,471	724,471	724,471	724,471	724,471
R-squared (female)	0.066	0.060	0.058	0.068	0.089	0.064	0.056	0.034	0.019
Observations (male)	1,193,773	1,187,728	1,187,316	1,185,332	1,193,773	1,193,773	1,193,773	1,193,773	1,193,773
R-squared (male)	0.055	0.048	0.046	0.062	0.069	0.051	0.045	0.030	0.021

Notes: This table reports estimated effects of prenatal exposure to land reform on test scores and admission outcomes. Panel A reports estimates for female students and Panel B for male students. Each estimate is from a separate regression on one outcome. The sample include test takers born in 1978 and 1984 and who took the college extrance exams in 1999-2001. All regressions include county fixed effects, year of birth effects, year of exam effects, county linear cohort trends, initial controls interacted with year of birth effects and drought in March and Spring in the current year and one year before. We also report the effect size of land reform on each outcome relative to the dependent variable mean. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.16: Exposure to One Child Policy controlled for

	Test score in percentile							
	Total score		Math		Chinese		English	
1{2 years after land reform}	0.264*	0.264*	0.371	0.377	0.143	0.143	0.250	0.250
	[0.158]	[0.158]	[1.853]	[2.360]	[0.151]	[0.150]	[0.162]	[0.162]
1{1 year after OCP}		0.036		-0.054		0.137		0.038
		[0.172]		[0.201]		[0.164]		[0.181]
Observations	1,504,260	1,504,260	1,495,855	1,495,855	1,496,886	1,496,886	1,495,004	1,495,004
R-squared	0.056	0.056	0.048	0.048	0.045	0.045	0.060	0.060

	Admission outcomes							
	Any college		First-tier college		Top 100 college		Elite college	
1{2 years after land reform}	0.001	0.001	0.003*	0.003*	0.004***	0.004***	0.001**	0.001**
	[0.003]	[0.003]	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]
1{1 year after OCP}		0.001		-0.001		-0.001		-0.000
		[0.003]		[0.002]		[0.001]		[0.001]
Observations	1,504,260	1,504,260	1,504,260	1,504,260	1,504,260	1,504,260	1,504,260	1,504,260
R-squared	0.075	0.075	0.046	0.046	0.029	0.029	0.017	0.017

Notes: Robust standard errors clustered at the county level are reported in brackets. This table reports estimated effects of prenatal exposure to land reform and One Child Policy on test scores and admission outcomes. The sample include test takers born in 1978-1984 and who took the college entrance exams in 1999-2001. All regressions include county fixed effects, year of birth effects, year of exam effects and county linear cohort trends.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.17: Exposure to One Child Policy controlled for, for boys

	Test score in percentile							
	Total		Math		Chinese		English	
1{2 years after land reform}	0.361**	0.361**	0.444***	0.445***	0.197	0.197	0.339*	0.339*
	[0.176]	[0.176]	[0.168]	[0.168]	[0.163]	[0.163]	[0.177]	[0.177]
1{1 year after OCP}		0.001		-0.09		-0.054		0.072
		[0.179]		[0.172]		[0.168]		[0.185]
Observations	936276	936276	931263	931263	930760	930760	929347	929347
R-squared	0.056	0.056	0.048	0.048	0.045	0.045	0.062	0.062
	Admission outcomes							
	Any college		First-tier college		Top 100 college		Elite college	
1{2 years after land reform}	0.003	0.003	0.005***	0.005***	0.005***	0.005***	0.002**	0.002**
	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]	[0.002]	[0.001]	[0.001]
1{1 year after OCP}		0.001		-0.002		-0.001		0
		[0.003]		[0.002]		[0.001]		[0.001]
Observations	936276	936276	936276	936276	936276	936276	936276	936276
R-squared	0.07	0.07	0.045	0.045	0.031	0.031	0.021	0.021

Notes: Robust standard errors clustered at the county level are reported in brackets. This table reports estimated effects of prenatal exposure to land reform and One Child Policy on test scores and admission outcomes for boys. The sample include test takers born in 1978-1984 and who took the college entrance exams in 1999-2001. All regressions include county fixed effects, year of birth effects, year of exam effects and county linear cohort trends.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.18: Exposure to One Child Policy controlled for, for girls

	Test score in percentile							
	Total score		Math		Chinese		English	
1{2 years after land reform}	0.138	0.138	0.135	0.135	0.089	0.091	0.167	0.167
	[0.204]	[0.204]	[0.202]	[0.202]	[0.205]	[0.204]	[0.207]	[0.207]
1{1 year after OCP}		0.038		-0.041		0.465*		0.003
		[0.250]		[0.253]		[0.249]		[0.254]
Observations	567,984	567,984	564,592	564,592	566,126	566,126	565,657	565,657
R-squared	0.066	0.066	0.060	0.060	0.058	0.058	0.067	0.067
	Admission outcomes							
	Any college		First-tier college		Top 100 college		Elite college	
1{2 years after land reform}	-0.000	-0.000	-0.000	-0.000	0.002	0.002	0.001	0.001
	[0.004]	[0.004]	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]
1{1 year after OCP}		0.001		-0.001		-0.001		-0.000
		[0.004]		[0.002]		[0.002]		[0.001]
Observations	567,984	567,984	567,984	567,984	567,984	567,984	567,984	567,984
R-squared	0.088	0.088	0.055	0.055	0.034	0.033	0.018	0.018

Notes: Robust standard errors clustered at the county level are reported in brackets. This table reports estimated effects of prenatal exposure to land reform and One Child Policy on test scores and admission outcomes for girls. The sample include test takers born in 1978-1984 and who took the college entrance exams in 1999-2001. All regressions include county fixed effects, year of birth effects, year of exam effects and county linear cohort trends.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.19: Test sample selection

	(1) number of births	(2) ever went to high school=1	(3) high school dropout=1	(4) college applicant=1	(5) test taker=1	(6) male=1	(7) age at the exams
1{born 2 years after reform or later}	-0.561 [0.823]	0.005 [0.003]	-0.0003 [0.003]	-0.001 [0.009]	-0.0017 [0.0013]	0.001 [0.003]	-0.005 [0.009]
County FE	X	X	X	X	X	X	X
YOB FE	X	X	X	X	X	X	X
County linear trends	X	X	X	X	X	X	X
Initial control*YOB FE	X	X	X	X	X	X	X
Spring drought in t and t-1	X	X	X	X	X	X	X
Year of exam FE				X	X		
dependent variable mean	86	0.15	0.02	0.39	0.968	0.52	19.09
Observations	7,217	422,076	59,607	6,832	1,980,665	621,265	1,918,244
R-squared	0.968	0.074	0.062	0.784	0.052	0.004	0.775

Notes: Column (1) reports the estimated effect of land reform on the number of births at the county-birth year level using 1990 Census. Using 2000 Census, column (2) reports the estimate on whether one ever went to high school or still in high school, column (3) report the estimate on high school dropout in the sample of individuals who went to high school. Column (4) report the estimate on the fraction of college applicants in high school students at the county-birth year-year of application level. Column (5) reports the estimate on whether an applicant had taken the national exam using the administrative data on test scores. Column (6) reports the estimated effect on gender at the individual level using 1990 Census. Column (7) reports the estimate on one's age at college entrance exam using the administrative data on test scores.The sample includes individuals born in 1978-1984 in counties matched with the county-level data on the reform timing as well as all county-level controls. Regressions in column (1)-(3) and (6)-(7) include county fixed effects, year of birth effects, county linear cohort trends, initial controls interacted with year of birth effects and drought in March and Spring in the current year and one year before. Column (4)-(5) further control for year of exam effects. In column (1), robust standard errors are reported in brackets; in column (2)-(7), robust standard errors clustered at the county level are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.20: Land reform and grain output

	Dependent variable: ln(grain output per capita)		
	(1) full sample	(2) Change in grain output above median	(3) Change in grain output below median
1{1 year after reform or later}	0.026* [0.015]	0.092*** [0.019]	-0.039* [0.021]
County FE	X	X	X
Year FE	X	X	X
Initial control*Year FE	X	X	X
Spring drought in t and t-1	X	X	X
County linear trends	X	X	X
Observations	4,188	2,093	2,095
R-squared	0.874	0.905	0.818

Notes: This table reports estimates of reform on log grain output per capita. Column (1) reports the estimate using the full sample, column (2) a subsample of counties above median of the change in grain output in capita before and after the reform, and column (3) a subsamle of counties below median. All regressions control for county fixed effects, year effects, county linear time trends, determinants of reform timing interacted with time fixed effects and droughts in March and April in year t and t-1. The analysis sample includes 382 counties that report grain output data in 1974-1984 and that are matched with the data of control variables. Robust standard errors are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.21: Summary Statistics

	Births between 1974 and 1986			
	Full sample		Two-parent sample	
	first child	second child	first child	second child
Boy	0.511	0.523	0.511	0.523
Girl first		0.507		0.507
Exposed to land reform	0.541	0.511	0.545	0.519
Mother Primary school			0.607	0.515
Mother Middel school			0.272	0.170
Mother High school			0.075	0.037
Father Primary school			0.833	0.790
Father Middle school			0.504	0.408
Father High school			0.160	0.114
Observations	371762	279069	349351	260529

Table B.22: Land reform, male birth and sex ratio

	Male=1		Sex ratio
	(1)	(2)	(3)
	First child	Second child	Second child
Post	0.003	-0.010*	-0.021
	[0.004]	[0.006]	[0.044]
Girl first		0.027***	0.136***
		[0.003]	[0.020]
Post*Girl first		0.029***	0.151***
		[0.004]	[0.030]
County FE	X	X	X
YOB FE	X	X	X
Initial control*YOB FE	X	X	X
Spring drought in t and t-1	X	X	X
County linear trends	X	X	X
Dependent variable mean	0.511	0.523	1.27
Observations	371762	279069	24,255
R-squared	0.006	0.011	0.131

Notes: Column 1 reports estimate for the effect of exposure to land reform on the probability of first child being male; column 2 for the effect on second child being male; column 3 for sex ratio of second births by county and year. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on reform timing and initial controls. All regressions include county fixed effects, year of birth effects, county linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.23: Treatment heterogeneity, by changes in grain output

	Dependent variable: Male=1		
	(1)	(2)	(3)
	Full sample	Change in grain output above median	Change in grain output below median
Post	-0.015	-0.029***	0.003
	[0.010]	[0.013]	[0.017]
Girl first	0.029***	0.027***	0.033***
	[0.005]	[0.007]	[0.008]
Post*Girl first	0.013*	0.027***	-0.004
	[0.007]	[0.009]	[0.010]
Dependent variable mean	0.521	0.524	0.519
Observations	93335	53243	40092
R-squared	0.011	0.011	0.013

Notes: Column 1 reports estimate for the effect of exposure to land reform on the probability of second child being male in the full sample; column 2 for the effect in counties above the median of change in grain output before and after the reform; column 3 for the effect in counties below the median. The sample includes individuals born between 1974 and 1986 in 400 counties that are matched with the county-level data on reform timing and grain output. All regressions include county fixed effects, year of birth effects, county linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.24: Treatment heterogeneity, by the fraction of women growing cash crops

	Dependent variable: Male=1
Post*Girl first*% Women	-0.0002
	[0.0002]
Post*Girl first	0.039***
	[0.011]
Post	-0.02
	[0.013]
Girl first	0.02**
	[0.008]
Observations	256605
R-squared	0.011

Notes: Post*% Women and Girl first*% Women are also controlled for. This table reports estimate for the effect of exposure to land reform on the probability of second child being male by the county-level fraction of women growing cash crops. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on reform timing and initial controls. The regression includes county fixed effects, year of birth effects, county linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.25: Treatment heterogeneity, by parental education

	Dependent variable: Male=1		
	(1)	(2)	(3)
A. Mother			
Post*Girl first*High school	0.065***		
	[0.024]		
Post*Girl first*Middle school		0.035***	
		[0.011]	
Post*Girl first*Primary school			0.020**
			[0.008]
Post*Girl first	0.025***	0.021***	0.017***
	[0.004]	[0.004]	[0.006]
Observations	260529	260529	260529
R-squared	0.012	0.012	0.012
B. Father			
Post*Girl first*High school	0.033**		
	[0.014]		
Post*Girl first*Middle school		0.016*	
		[0.009]	
Post*Girl first*Primary school			0.014
			[0.010]
Post*Girl first	0.025***	0.020***	0.016*
	[0.004]	[0.005]	[0.009]
Observations	260529	260529	260529
R-squared	0.012	0.012	0.012

Note: Post*Parental eduation, Girs first*Parental education and Parental education are also controlled for.
This table reports estimate for the effect of exposure to land reform on the probability of second child being male by parental education. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on reform timing and initial controls. All regressions include county fixed effects, year of birth effects, county linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.26: Land reform, OCP and sex ratio

	Male=1		
	(1)	(2)	(3)
	Panel A: First Child		
Land reform	0.005		0.005
	[0.005]		[0.005]
OCP		-0.003	-0.002
		[0.005]	[0.005]
Observations	298310	298310	298310
R-squared	0.006	0.006	0.006
	Panel B: Second Child		
Land reform*Girl first	0.030***		0.031***
	[0.005]		[0.008]
Land reform	-0.012*		-0.013*
	[0.007]		[0.007]
OCP*Girl first		0.024***	-0.002
		[0.004]	[0.008]
OCP		-0.017***	-0.004
		[0.006]	[0.007]
Girl first	0.025***	0.028***	0.025***
	[0.003]	[0.003]	[0.003]
Observations	224600	224600	224600
R-squared	0.011	0.011	0.011

Notes: The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and OCP. All regressions include county fixed effects, year of birth effects, county linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.27: Land reform, OCP and fertility

	A. County-by-year number of birth		
Land reform	2.333**		2.277**
	[1.101]		[1.104]
OCP		-2.824**	-2.783**
		[1.101]	[1.097]
Dependent variable mean		90	
Observations	11137	11137	11137
R-squared	0.948	0.948	0.949
	B. Have second child=1		
Land reform*Girl first	0.026***		-0.009
	[0.004]		[0.008]
Land reform	-0.016***		0.001
	[0.005]		[0.006]
OCP*Girl first		0.038***	0.046***
		[0.004]	[0.009]
OCP		-0.023***	-0.027***
		[0.004]	[0.006]
Girl first	0.040***	0.030***	0.031***
	[0.004]	[0.003]	[0.003]
Dependent variable mean			
Observations	298310	298310	298310
R-squared	0.343	0.343	0.343