Mother's Education and Infant Health: Evidence from High School Closures in China*

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September 6, 2012

Abstract

This paper examines the effect of maternal education on infant health by exploiting exogenous variation in women's exposure to high school closures immediately after the Cultural Revolution in China. I document a sharp decline of 22.5 percent in high school completion for women at age 17 years and 9 months by the first quarter in the first closure year. Using the discontinuous change in schooling induced by the closures, I find that one more year of maternal high school education has no effect on prematurity, low birthweight, neonatal mortality and infant mortality.

*I am deeply indebted to my advisors, Douglas Almond, Kaushik Basu, James Berry and Michael Lovenheim. This work has benefited from discussions with David Card, A.V. Chari, Esther Duflo, Caroline Hoxby, Elaine Liu, Jordan Matsudaira, Annemie Maertens, Xin Meng and Russell Toth. Conversations with Jian Chen and Suzanne Pepper on the historical background are extremely insightful. Participants in NEUDC at Yale and Conference on Health, Education and Welfare Programs in China, and seminars at Australian National University, Colorado Boulder, Cornell, Rutgers, Standord SIEPR and Williams College provided helpful comments. Excellent assistance on the data collection from Lai Shun Kwok, Lingling Lai, Simon Lim, Yuan Liu and Puzhou Wu is greatly acknowledged. I am grateful to the Graduate School at Cornell for financial support. All errors are my own.

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1 Introduction

Maternal education may affect infant health by increasing household income or directly improving health behaviors. The robust positive association between maternal education and child health has been widely documented (e.g., Strauss and Thomas, 1995; Gakidou et 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 the positive correlation can be interpreted as causality. The design-based literature, however, offers mixed findings. Studies that exploit various school expansion programs 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) using the school entry date policy in the US also show no effect. Looking at the developing world alone, primary school and middle school education has been found to be beneficial to infants. A missing piece in the literature is whether education interventions targeting higher levels of schooling in developing countries could improve infant health.

This paper provides the first quasi-experimental evidence on the effect of maternal high school education on infant health in a developing country. In particular, I exploit the largest negative education 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. Three distinct features of these closures are helpful for a research design. First, the decision to close rural schools was unexpected, closely following the death of Mao Zedong and the end of the Cultural Revolution in 1976 (Pepper, 1996). Second, the total number of schools closed was unprecedented. Figure 1a shows that the number of rural high schools decreased by more than 80 percent between 1977 and 1983 nationwide. Lastly, it was a supply-side shock, meaning that these closures were not driven by the change in the overall demand for high schools. Figure 1b shows that both overall population and rural population aged 15-18 had been increasing from 1977 to 1983.

I focus on narrow comparisons among rural women just below and just above age 17 years and 9 months (age 17.75 hereafter) by the first quarter in the first year of the closures within county. Two sources of variations determine the age cutoff. First, when the closures 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. Second, the variation by quarter of birth 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 February or March, to start primary school. Individuals born in the first quarter went to schools one year earlier and graduated one year earlier, which would allow them to avoid the closures, compared to those born in the second quarter. Thus, individuals at 17.75 by the first quarter of the first closure year are expected to be the oldest cohort affected.

The key identifying assumption is that women on either side of 17.75 within county are otherwise similar. The assumption is plausible because the closure decision was made shortly after Mao's death and was unpredictable ten or more 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. Any seasonal difference is accounted for by quarter of birth fixed effects. The assumption is also testable by examining the continuity of predetermined familial characteristics. I find that these women and their parents around the cutoff are similar along many predetermined dimensions. 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 change is therefore less important for the school closures between 1977 and 1984.

To perform the research design, I combine unique data on the year the closures of rural high schools started by county with the 1992 Chinese Children Survey microdata. Consistent with expectations, I find a sharp and large drop in high school completion for women at age 17.75. Nearly one-fourth of women on the margin had not completed high school due to the closures. 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. In addition, there is no evidence that one more year in high school improves labor market outcomes or prenatal health care.

I also find substantial heterogeneity on the first stage effect of the closures. The drop in high school completion is larger where more schools were built during the Cultural Revolution, and where it is less densely populated. Despite a larger first stage effect, there is still no strong evidence for an effect on infant health in these areas. Again, these results reinforce the finding that maternal high school education does not affect infant health.

An alternative interpretation for the drop in high school completion at 17.75 is that the post-Mao land reform between 1978 and 1984 increased the opportunity cost of schooling. Alternatively, if rural middle schools were closed two years prior to the closures of high schools, we might also expect a drop in high school completion at 17.75. I provide direct evidence to rule out these potential threats. The inclusion of one's exposure to land reform does not change the estimated effect of high school closures. Moreover, there is no sign of a discontinuous change in middle school completion at the same margin.

Interpreting my findings requires careful considerations of the research design. First, the estimated effects are specific to a subpopulation of women who complied with the school closures. When rural schools were closed, parents could send their children to urban schools in the same county to continue schooling. Because the closures increased the costs of completing high school for the rural population, my estimates are most relevant to women from lower socioeconomic background. Second, in the comparisons among women on either side of 17.75, the margin of maternal education affected by the closures is one more year in high school, which accounted for half of the high school education,[1] and a high school diploma.

Furthermore, the identification strategy, as well as the context of China, allow me to separate possible

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

channels through which maternal education might affect infant health, which in turn sheds light on two debatable arguments in the 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 being less likely to be pregnant under age 18, which could also improve infant health (Royer, 2004). 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 not obvious whether the improvement in infant health is due to learning or the endogenous reduction in teenage pregnancy. In this paper, at the exit of high school, women on the margin were already close to 18. No effect of high school closures on teenage pregnancy is detected.

The second argument is that education may directly improve health behavior (Grossman, 1972). It has been an empirical challenge to disentangle direct change in health behavior from income change, as they are often closely related. Using microdata from a 1992 survey, when China was in the transition from a central planning economy to a market economy, I would expect that the income channel is very weak.[2] On the other hand, this is a unique setting for isolating the effect of education on health behavior. If one more year of high school education improves maternal health behavior, even in the absence of an income channel, we would still expect a positive effect on infant health. However, none is observed.

The rest of this paper is organized as follows. Section 2 introduces the background of the school closures 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.

2 Background and Treatment Status

2.1 Historical background on the school closures

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. Education policy priorities shifted to peasant youth by popularizing middle schools and expanding high schools in the countryside.[3] 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 schools in rural communes, which enabled rural children to go to secondary schools within walking distance (Han, 2001; Thogersen, 2002). In the rural population,

[2]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).

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

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.[4] The official justification of closing rural high schools was that, to best serve economic development, high schools should be college preparatory. 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).[5] Urban teachers who were sent to teach 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).[6] 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.[7]

A natural concern is related to education quality of these rural schools built during the Cultural Revolution. Compared to high school education in other time periods, the regular curricula 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 education quality has been improved since the end of the Cultural Revolution is still an open question. 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,

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

[5] 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 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).

[6] 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).

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

the concern about changes in education quality in high school remains an empirical question that requires data on individual earnings to test.

2.2 Determining one's exposure status

One's exposure to the high school closures depends on one's year of birth, quarter of birth and the first year of the closures in one's 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? Little is known about individual exposure to the shock from previous studies (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. 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 schools in town. Teacher Liu 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 when schools were closed were able to avoid the shock, while students in their final year in school were the oldest affected.

To determine who were the oldest ones exposed to the closures, the second useful 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.[8] 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.[9] The years of schooling in primary schools or middle schools could vary across counties, or even across schools within county when some schools were chosen to implement trials on shortened years of schooling.[10] Therefore, age at graduation from high school varied from 16 to 18 at the time of the closures. Although the exact years of schooling is not observed, 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 at risk of dropout.

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 from 1967 to the late 1970s.[11] Individuals born in the first quarter were more likely to go to school one year earlier and 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

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

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

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

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

quarter in the first year of the closures would mostly have graduated, while those born in the other three quarters of the same year were at risk of dropout. Thus, the finest comparisons are among individuals born in the first and second quarter of the same year within county. A common concern is that parents might strategically plan births to ensure an earlier school entry date. 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.

2.3 Regional variation in the closure timing

My primary approach is comparing women on either side of age 17.75 within county. An alternative approach would be using the variation in the closure timing by county for cross-county comparisons (e.g., difference-in-difference), which relies on a strong assumption that the timing of the closures was as good as randomly assigned. I provide the first quantitative evidence that the closure timing is uncorrelated to a number of initial county features suggested by historians.

Previous studies hypothesize that counties that built more schools during the Cultural Revolution were under greater pressure to comply with the central government's decision to close these schools (Pepper, 1996; Andreas, 2004). Not surprisingly, these closures were met with great resistance from commune leaders and rural parents, who refused to give up their own schools and whose children would lose their only chance to go to high school (Rosen, 1985). Education officials in counties that delayed their closing action argued that their counties were less developed and already had very low high school enrollment rate, but finally, all counties had to comply with the closure decision (Pepper, 1996).

I test this hypothesis by using collected data on the closure timing and selected county characteristics prior to the closures.[12] Appendix Table 1 reports the estimated correlation between the first year of the school closures (1977-1984) and each of the initial county characteristics. For education characteristics in Panel A, estimates on the initial number of high schools, the number of classrooms and the enrollment rate are consistent with the qualitative evidence that counties that expanded more during the Cultural Revolution closed schools earlier. However, these estimates are very small and imprecise, except for the number of classrooms that is significant at the 10 percent level. The correlation between the closure timing and the number of send-down youth received, a proxy for the teacher supply, is very weak. For initial economic conditions in Panel B, there is no strong evidence that the closure timing is correlated with county government revenue, government expenditure on education and health, grain output per ca pita and population density in 1976. Overall, these weak correlations suggest that the timing of the

[12] Since county statistics before 1980 have not been made systematically public, county gazetteers are the only sources to collect county characteristics in the 1970s. The sample size of counties for each characteristic is determined by the availability of that characteristic on gazetteers.

closures appears to be idiosyncratic, at least to these observed education and economic measures.

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.

3.1 County-level data on the timing of the closures

The timing of the high school closures 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 sources, e.g., historical archives and policy documents of county governments (Xue, 2010). In the chapter on education, reforms and statistics in the post-Mao period are recorded in detail. These records are also reliable because they are not under the restriction of "be rough, not in detail" that is placed on recording politically sensitive periods.

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. 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. To define a sample of rural counties, I use the administrative code in the 1982 Census to exclude cities and urban districts.[13]

I plot the percentage of counties that started to close schools from 1977 to 1984 by the solid line in Figure 2a. 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 counties in 27 provinces matched with the 1992 Chinese Children Survey microdata. The dashed line in Figure 2a shows that these matched counties are representative of all counties collected in the distribution of the timing. In Figure 2b, I show the geographical distribution of the closure timing in these 400 counties. The darker the shaded area, the later a county started to close rural high schools.

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. 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

[13]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).

or middle schools varied in the 1960s and 1970s. It is therefore not possible to construct a consistent measure on one's years of schooling. I measure maternal education by high school completion, the margin directly affected by the closures 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.[14] In the comparisons among individuals around 17.75 in the first quarter between 1978 and 1981, their years of schooling in high school remained 2 years.

This paper 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.[15] 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.

Every woman reported a complete pregnancy history and detailed birth outcomes. I use four measures of poor infant health. 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.[16] 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.

It is unfortunate that one's county of birth is not observed in the 1992 CCS. I match the microdata with the county-level timing data 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 in this context because internal migration had been under strong restrictions by the early 1990s. In the 1990 Census, the migration rate among rural women in the birth cohorts of my analysis sample is as low as 5 percent. In Appendix A, I find that a woman's decision to migrate was unlikely to be affected by her exposure to the closures. Moreover, the mismatch of the closure timing for the 5 percent of women does not change my estimates.

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. They were born between 1953 and 1970 and at 22-39 in 1992.

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).[17]

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

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

[16] See Almond et al. (2005) for a review on this literature.

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

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 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.

4 Empirical Framework

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 first closure year 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. I assess the effect of maternal education on infant health by relating the difference in high school completion induced by the closures with differences in health measures. 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 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):

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

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:

$$\begin{aligned} S_i \;&= \alpha + \beta 1\{age_i \leq 17.75\} + f(age_i)\gamma + f(age_i) * 1\{age_i \leq 17.75\}\delta \\ &+\theta_q + \lambda_j + \phi_j * [f(age_i) + f(age_i) * 1\{age_i \leq 17.75\}] + \epsilon_i \end{aligned} \tag{2}$$

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 pre-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 1a, 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. In all specifications, I 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 pre-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):

$$\begin{aligned} Y_i &= \alpha + \rho 1\{age_i \leq 17.75\} + f(age_i)\gamma + f(age_i) * 1\{age_i \leq 17.75\}\delta \\ &\quad + \theta_q + \lambda_j + \phi_j * [f(age_i) + f(age_i) * 1\{age_i \leq 17.75\}] + \epsilon_i \end{aligned} \tag{3}$$

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.

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.).[18] Maternal age at first birth is another concern. Royer (2004) finds that a pre-term birth is more likely if a mother has her first child before 18. 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 health effect of teenage girls not staying in schools from that of less knowledge learned in schools. Therefore, before proceeding to investigate educational

[18] 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.

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 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 changes and respond 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. In Section 5.2, I plot women's high school completion by their age at the time of the closures. I do not find any sign of a decline among the pre-exposure cohorts.

There are two alternative hypotheses that might also explain a drop in high school completion or infant health at 17.75. The first concern is about other 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. I conducted a comprehensive review on reforms and policy changes in the 1970s and 1980s. Land reform between 1978 and 1984 stands out as a potential confounder. The reform replaced collective farming in Mao's era with household farming, and is widely documented to have improved agricultural productivity since the late 1970s (e.g., Lin, 1992). Consequently, the reform raises the opportunity cost of schooling, which resulted in more dropout in the countryside 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 changes in the health care system by controlling for exposure to land reform. Using data on the timing of land reform by county that I collected, 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 earlier when middle schools were closed. If the decline in high school completion at 17.75 is induced by middle school closures two years ago, we should also observe a drop in women's middle school completion at 17.75. In Section 6.3, however, I find no evidence of a discontinuous change 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.

5 Main Results

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 3a. 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 3b. 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 closures on fertility choices is interesting in light of the literature on education and fertility in developing countries. There are 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 high school closures were close to 18. Second, it is not surprising that the closures mainly shifted maternal age at first birth from 22 to 21, because the birth control policies have been promoting "late marriage and late births" since the 1970s, and the 1980 marriage law set a legal marriage age at 20 for women.

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 4 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 pre- 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 pre- trend declines among individuals older than 17.75. However, such a decline is not observed. The smoothness of the pre- and post trends and the sharp discontinuity 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 pre-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 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.[19] Column (1) reports the baseline estimate by fitting a quadratic cohort trend without any covariates. 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 4. 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

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

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 from the within-county comparisons. These effects are all precisely estimated and the precision is not changed by controlling for additional covariates. The estimated effects in column (5)-(8) are very similar in magnitude relative to the mean level in the subsample of mothers. The first stage estimates by fitting a quadratic or a cubic cohort trend and controlling for all covariates are reported in Appendix Table 2. The estimate using a quadratic polynomial shows a decline of 1.8 percentage points, and the one using a cubic polynomial shows a 3.1 percentage points decline.

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

5.3 Infant health outcomes

This subsection focuses on the analysis of infant health for the first births. Figure 5 shows the reduced form graphs on gestational age (5a), prematurity (5b), neonatal mortality rate (5c), infant mortality rate (5d), birth weight (5e) and low birthweight (5f). 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 pre-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. In Appendix Table 2, I report reduced form estimates using a quadratic or cubic model. Consistently, estimates using higher polynomial orders are statistically insignificant, except for gestation age using a quadratic time trend that is significant at 10 percent level. The magnitudes using a quadratic trend are very close to those using a linear trend, while the magnitudes using a cubic trend are slightly larger.

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.

A potential explanation for the imprecision is that the cell size by county and quarter is very small, and therefore only a few women are on either side of the age cutoff within county. By relaxing the identifying assumption, there are two ways to increase the cell size. First, using one's integer age by the first quarter in the first year of closures, I define the exposed cohorts as being 17 or younger, and the unexposed being 18 or older. There are more observations around 17 in the county-by-year cell. Second, by dropping county fixed effects and county specific cohort trends, I estimate the discontinuous change at age 17.75 from the national trend.[20] In Appendix Table 3, I report instrumental variables estimates using larger cells. In Panel A using age in year, estimates have the same sign, smaller magnitudes and smaller standard errors. Again, they are statistically insignificant. Results are similar in Panel B with no county controls. Although these estimates might be better measured, the main findings are robust in that no strong evidence supports a positive effect of maternal high school completion on infant health.

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.[21] Table 5A 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

[20] Recall that in Table 3, the first stage estimates are very close with and without these county controls.

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

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 improves through this schooling margin. Local governments subsidized prenatal care and delivery for women planning authorized births in rural areas (Short and Zhang, 2004) and I focus on the first births. Thus, I examine women's prenatal care and delivery, which could well capture their awareness of benefits associated with health services. 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 they have the first one in the first trimester and whether they 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 5B, 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 who 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.

The co-movement of spousal education could due to marital sorting by education, or it could be that the school closures were a common shock to both men and women at similar age. To separate these two interpretations, I provide two additional pieces of evidence. First, if women exposed to the closures tend to marry men who did not complete high school, who are also likely to be exposed and at similar age, we would expect that the age gap between the husband and the wife would become smaller for women exposed. In Appendix Figure 1, however, the age gap is stable around 2.2 years for both pre- and post exposure women. Second, because the age gap is not affected by the school closures, I can further examine spousal education for couples with a larger age gap. When the husband is much older than the wife, he was unlikely exposed to the closures if the wife was on the margin being exposed. If their high school completion changes in the same direction, it is more likely to be marital sorting. Thus, I divide the sample into two groups, couples with a age gap for two years or more and those less than two years. Results are reported in Appendix Table 4. When the age gap is larger, women exposed to the closures tend to marry men who completed high school, while the co-movement is only observed among couples close in age. Both tests suggest no evidence of assortative mating.

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.

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.[22] 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 Appendix Table 5, I report estimates on an extensive set of predetermined characteristics, including grandparental education, grandmother's age at birth, women's 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.

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. As shown in Appendix Figure 2, the closures of high schools generally started earlier than land reform. Although their rollouts are within the same time period, there is substantial difference in their timing at the county level, which allows me to separate the effect of the

[22] 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.

school closures from that of land reform.

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 6 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 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.

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. In Appendix Figure 3, however, there is no obvious discontinuity in middle school completion at age 17.75 when high school closures started. 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.

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 7. In the first two columns, I divide the sample of counties by the pre-existing education level.[23] 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

[23]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.

areas (30 percent) than that in more densely 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.

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 closures started, women 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 possibly alternative hypotheses on the drop 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 maternal 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 on child health might be weakened.

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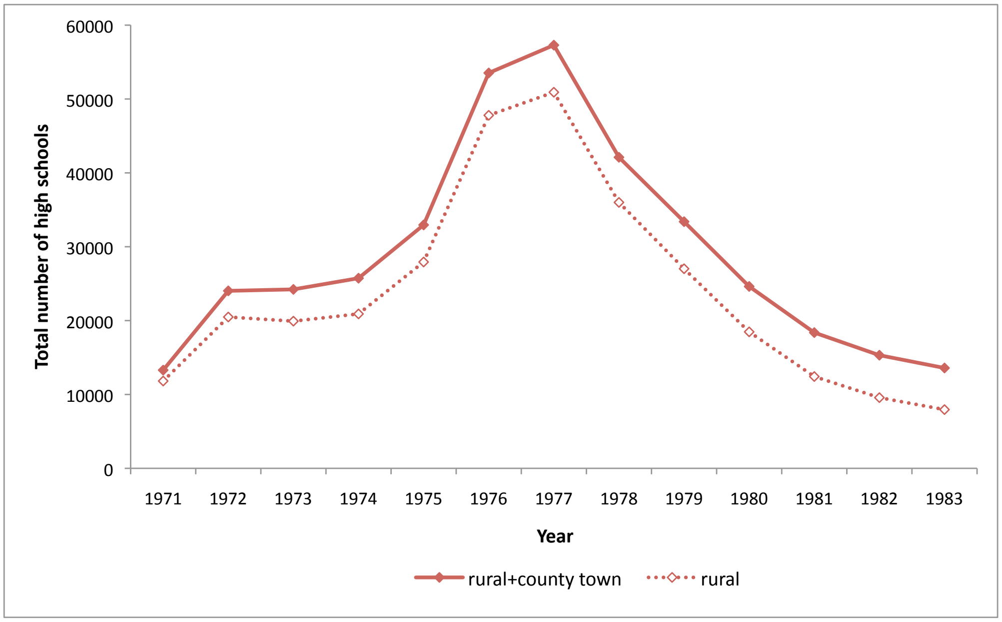
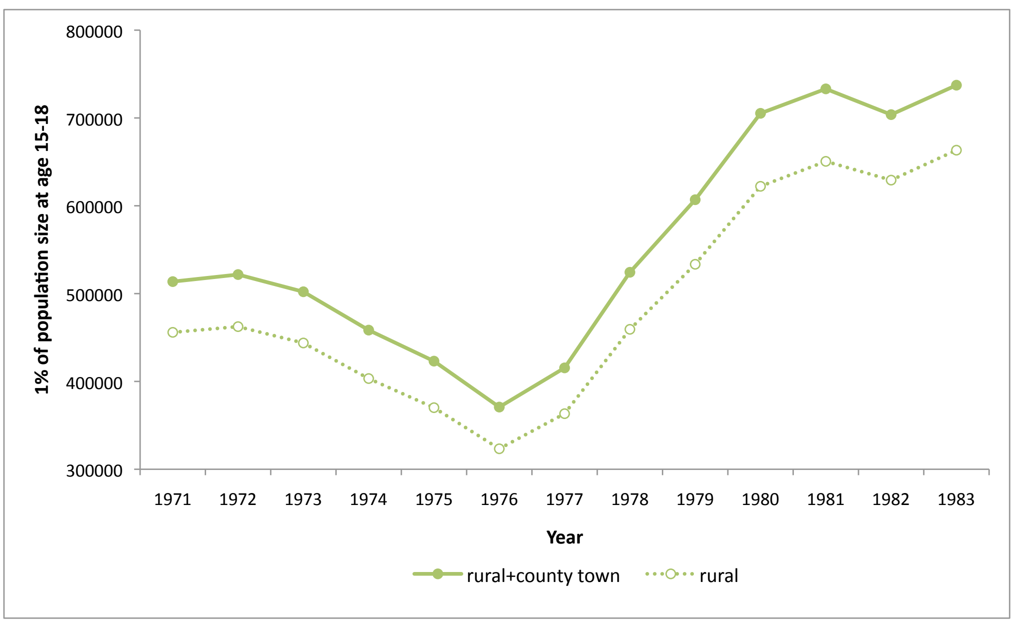
Figure 1a: Supply of high schools - total number of high schools in counties



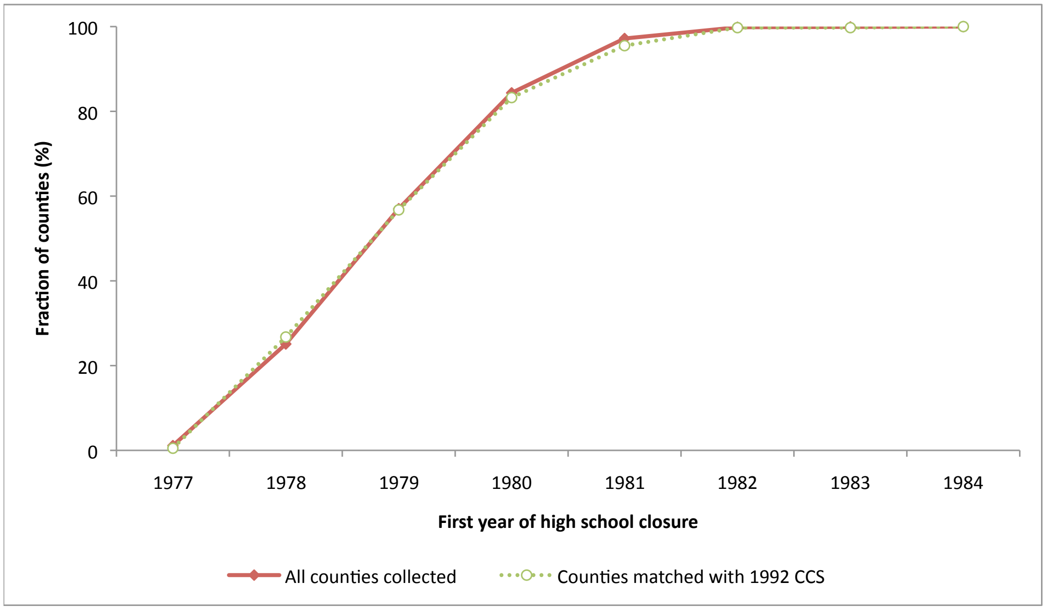
Data Source: *Achievement of Education in China: Statistics 1949-1983* (Ministry of Education, 1984)

Figure 1b: Demand for high schools - 1 percent of the cohort size at age 15-18 in counties



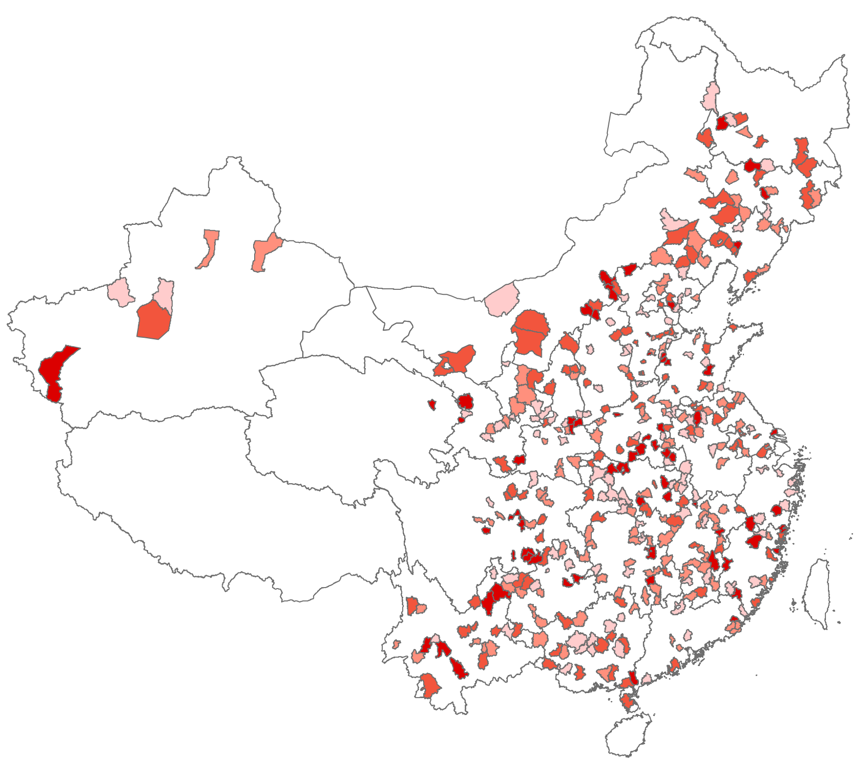
Data Source: Author's tabulation using 1 percent sample of 1990 Census

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



Note: Author's tabulation of the data on the year the high school closures started by county. The solid line represents 1247 counties and the dotted line represents 400 counties that are matched with the 1992 Chinese Children Survey microdata.

Figure 2b: Geographical distribution of the first year of the school closures: 1977-1984



Note: Author's tabulations of the data on the year the high school closures started by county in the 400 counties. The shading corresponds to the timing of the closures, where darker shading indicates later year of the closures.

Figure 3a: Motherhood observed

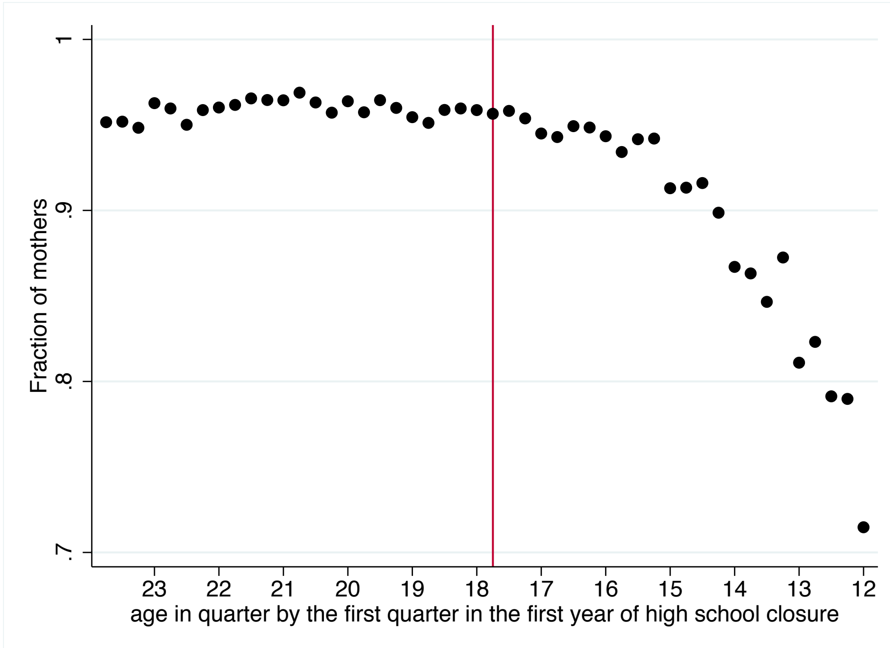
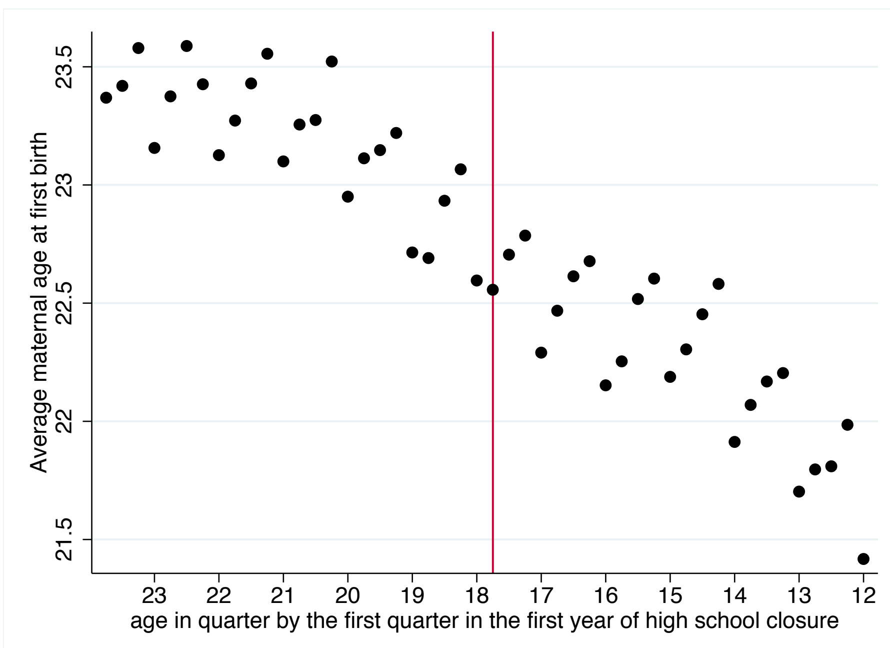


Figure 3b: Maternal age at first birth



Note: Points present the fraction of women who become mothers in Figure 3a, and the average maternal age at first birth in Figure 3b. The vertical bars denote the first cohort exposed to the high school closures.

Figure 4: High school completion

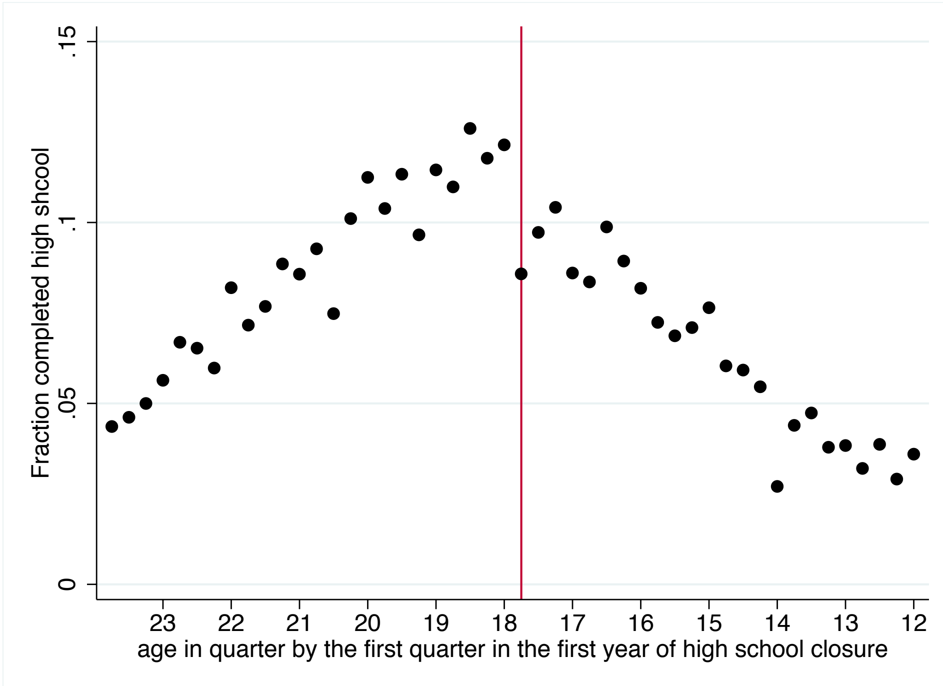
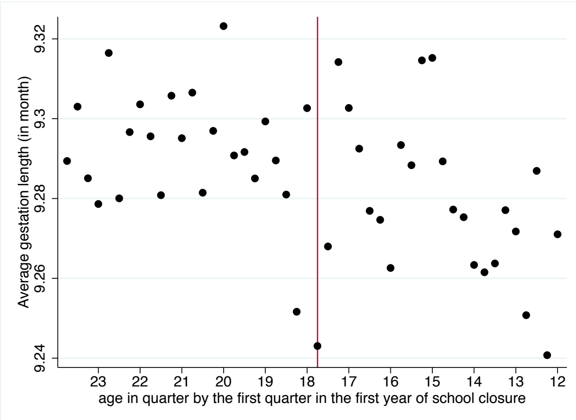
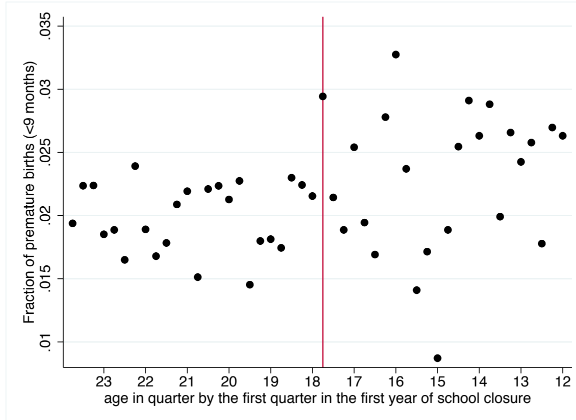


Figure 5: Infant Health Outcomes

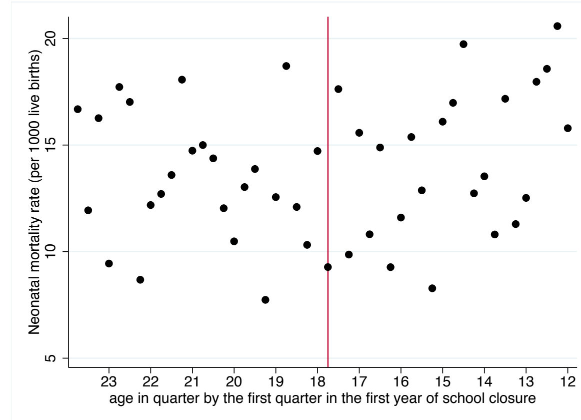
5a: Gestational age



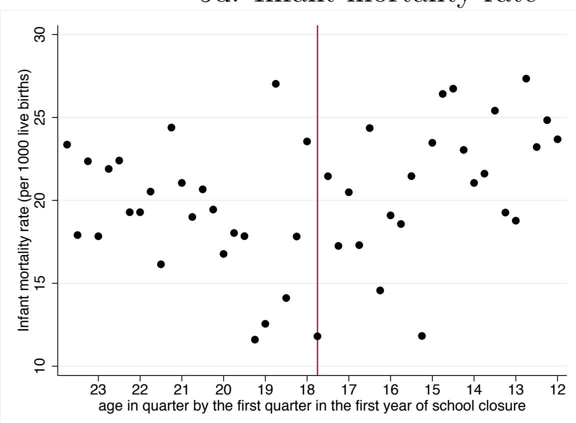
5b: Prematurity



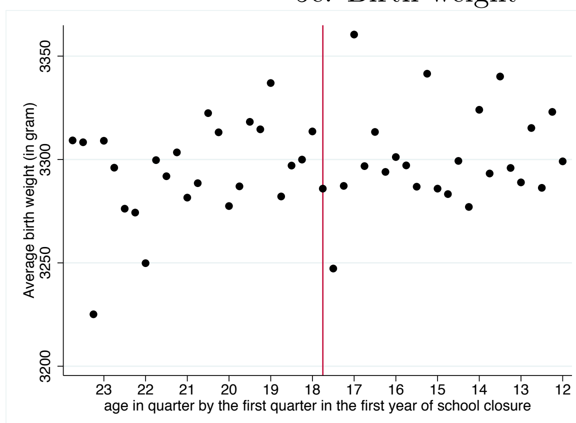
5d: Neonatal mortality rate



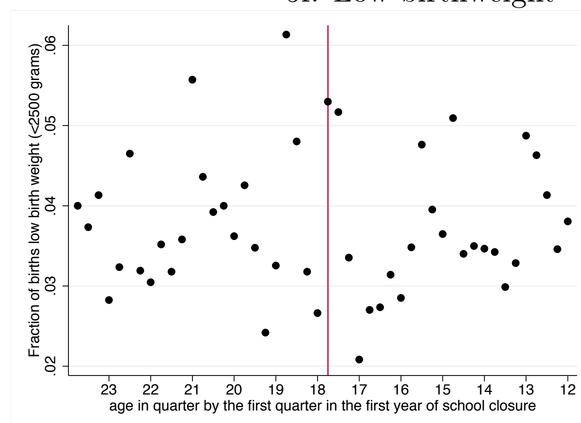
5d: Infant mortality rate



5e: Birth weight



5f: Low birthweight



Note: Points present the mean of an infant health measure in each age in quarter cell in each of the six figures. The vertical bars denote the first cohort exposed to the high school closures.

Table 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 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 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 4: OLS, Reduced form and IV estimation on infant health

	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 5A: 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 5B: 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 6: 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 7: 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.

Appendix A: Migration

Although the female migration rate in my analysis sample is as low as 5 percent, two possibilities might still lead to bias in estimation. First, if women who completed high school were more likely to migrate and thus are not observed in my sample, migration could possibly result in a downward bias of the first stage estimate. Second, because women who migrated from other counties are matched with the timing of the school closures in their county of residence in 1992, the mismatch of their treatment status is also likely to yield downward bias of the first stage estimate. I provide evidence below that these concerns are unlikely to confound my results.

A direct test on whether one's exposure to the school closures affects the decision to migrate is not possible using the 1992 CCS or the 1990 Census because 1) information on migration is not reported in the 1992 CCS; and 2) the county of birth is not observed for migrants in the 1990 Census. Alternatively, I compare the migration pattern of rural women in the 1990 Census with the pattern of high school completion in the 1992 CCS by their year of birth in Appendix Figure 4. If one's decision to migrate was affected by one's exposure to the school closures, we would expect that the fraction of migrants increases among birth cohorts old enough to avoid the closures and falls among those under age 18 when most counties closed schools and a large number of schools were closed. As shown in Figure 1a and 2a, by 1980, the closure decision had been made in more than 82 percent of counties, and the total school supply had been down by 58 percent; by 1983, the closure decision had been made in 99 percent of counties, and the drop in school supply had been more than 80 percent. Mapping these numbers to Appendix Figure 5, the 1962 birth cohort was the oldest cohort affected by the closures in 1980, and the 1965 cohort was the oldest affected by the closures in 1983. The solid line shows a sharp drop of high school completion in the 1961 cohort. The decline is of a much larger scale among the 1962-1965 cohorts. Moreover, the decline is persistent among younger cohorts. In contrast, the dotted line shows that, compared to the female migration rate among older cohorts, there are larger increases among those born in 1962-1965, and a decline appears at the 1968 cohort. The opposite patterns of high school completion and migration rate between the 1962 cohort and the 1967 cohort clearly run counter to the hypothesis that rural women who completed high school were more likely to migrate.

To examine how serious the problem might be due to mismatch of one's treatment status, I estimate the first stage effect in two samples in the 1990 Census: the sample of all rural women and the sample of rural nonmigrant women. If the mismatch leads to substantial bias, we would expect that the first stage estimate in the sample of all rural women (with mismatch for migrants) is different from that in the sample of nonmigrant women (no mismatch). I report the results in Appendix Table 6. In these 400 counties, there are only 2.5 percent of rural women who migrated from other counties. The point estimates and standard errors in both samples are exactly the same, which eliminates the concern about mismatch bias.

Appendix B: Difference-in-Difference

An ideal setting for a difference-in-difference approach would be that there is no pre-existing trend. It is not true in this setting because of the expansion of high schools prior to the closures. Figure 4 shows a symmetric pattern of high school completion among pre- and post exposure cohort. Although I am able to compare a wider range of cohorts using difference-in-difference, I would not expect a sharp contrast in their high school completion given the pre-existing upward trend. I discuss the results using difference-in-difference below.

I exploit the fact that individuals born in the same year and quarter had different exposures to the closures between counties that closed schools earlier and those that closed later. To assign the exposure status, I define a new time dimension independent of the timing of the closures by county in equation (4):

$$t_{yq} = integer[17.75 + y - \frac{(1-q)}{4}] \tag{4}$$

Where t_{yq} measures the calendar year when a woman born in year y and quarter q was at age 17 (as an integer) by the first quarter. The first stage specification is in the following:

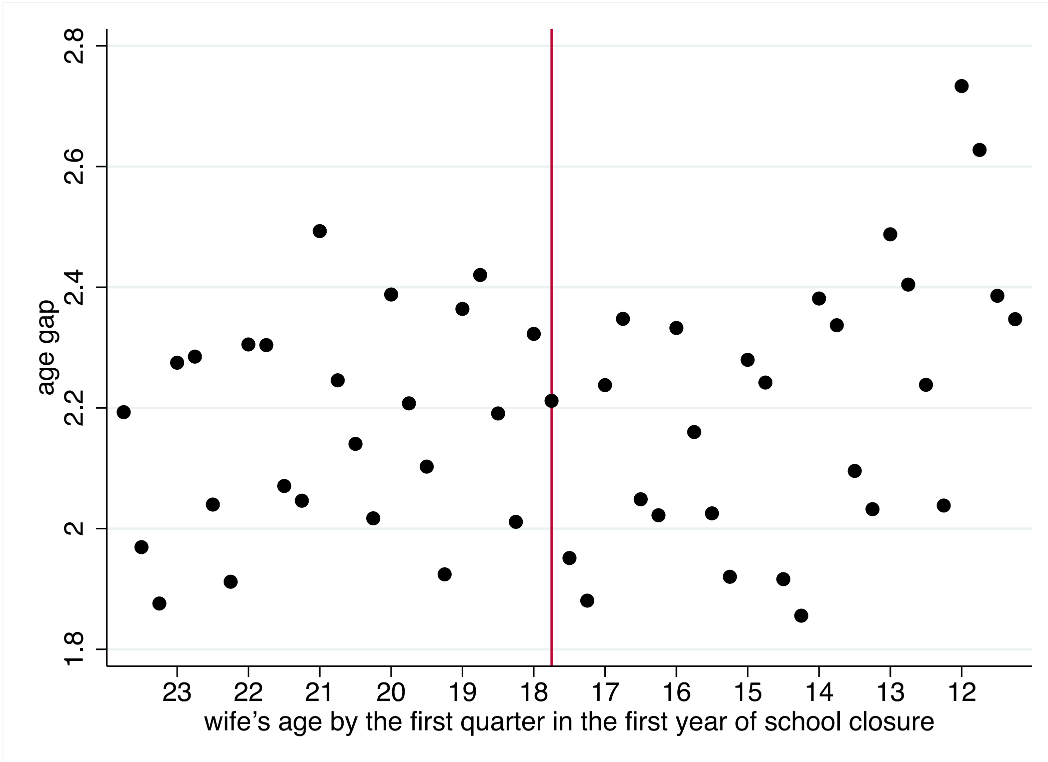
$$S_{iyqj} = \alpha + \pi 1\{t_{yq} \geq T_j\} + \eta_{yq} + \theta_j + \phi_j * t_{yq} + \epsilon_{iyqj} \tag{5}$$

Where S_{iyqj} is equal to 1 if individual i born in year y, quarter q and county j completed high school and 0 otherwise. The indicator variable, $1\{t_{yq} \geq T_j\}$, is equal to 1 if one was at 17 by the first quarter in the first year of the closures or later years, and is 0 otherwise. The difference-in-difference estimator, π, captures the effect of one's exposure to the closures on high school completion. Year and quarter of birth effects η_{yq} capture any differences across birth cohorts by year and quarter. County fixed effects, λ_j, absorb permanent county characteristics that might be correlated with the timing of the closures. The county-specific linear time trends, $\phi_j * t_{yq}$, control for county-specific confounding factors that change smoothly across cohorts. The inclusion of these trends allows any differences in the pre-existing trends of high school completion to be modest among counties with different years of the first closures. The sample includes rural mothers born between 1954 and 1969, which are comparable with those in my main sample of analysis.

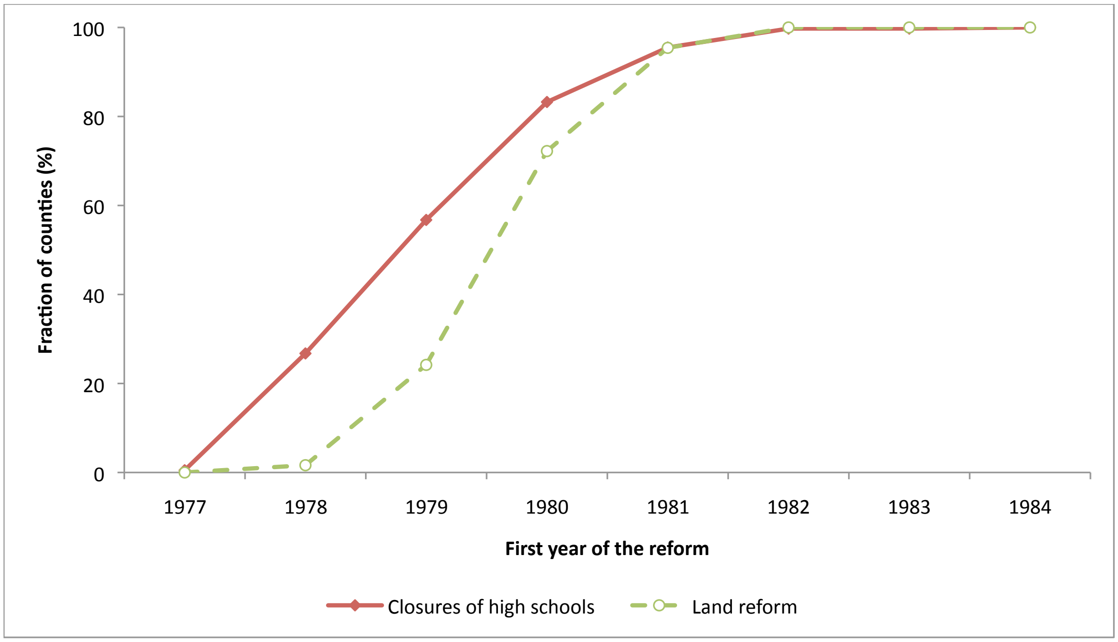
Appendix Table 7 reports estimates of the first stage and the reduced form on infant health. As expected, the estimate on high school completion is very small and marginally significant at 10 percent level. This suggests that I do not have a strong first stage using difference-in-difference. I therefore do not interpret the reduced form estimates.

Appendix Figures

Appendix Figure 1: Age gap

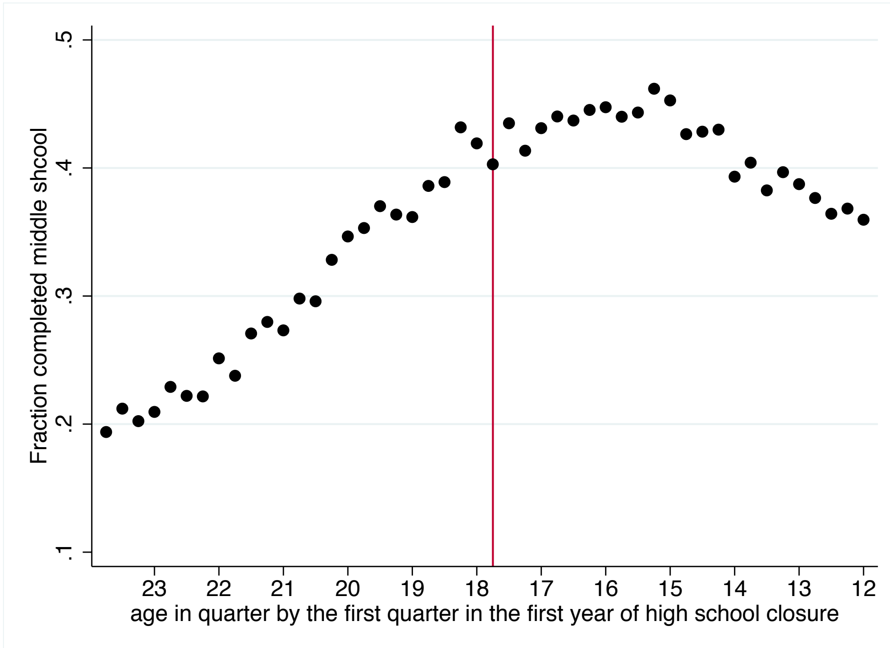


Appendix Figure 2: The timing of high school closures and land reform



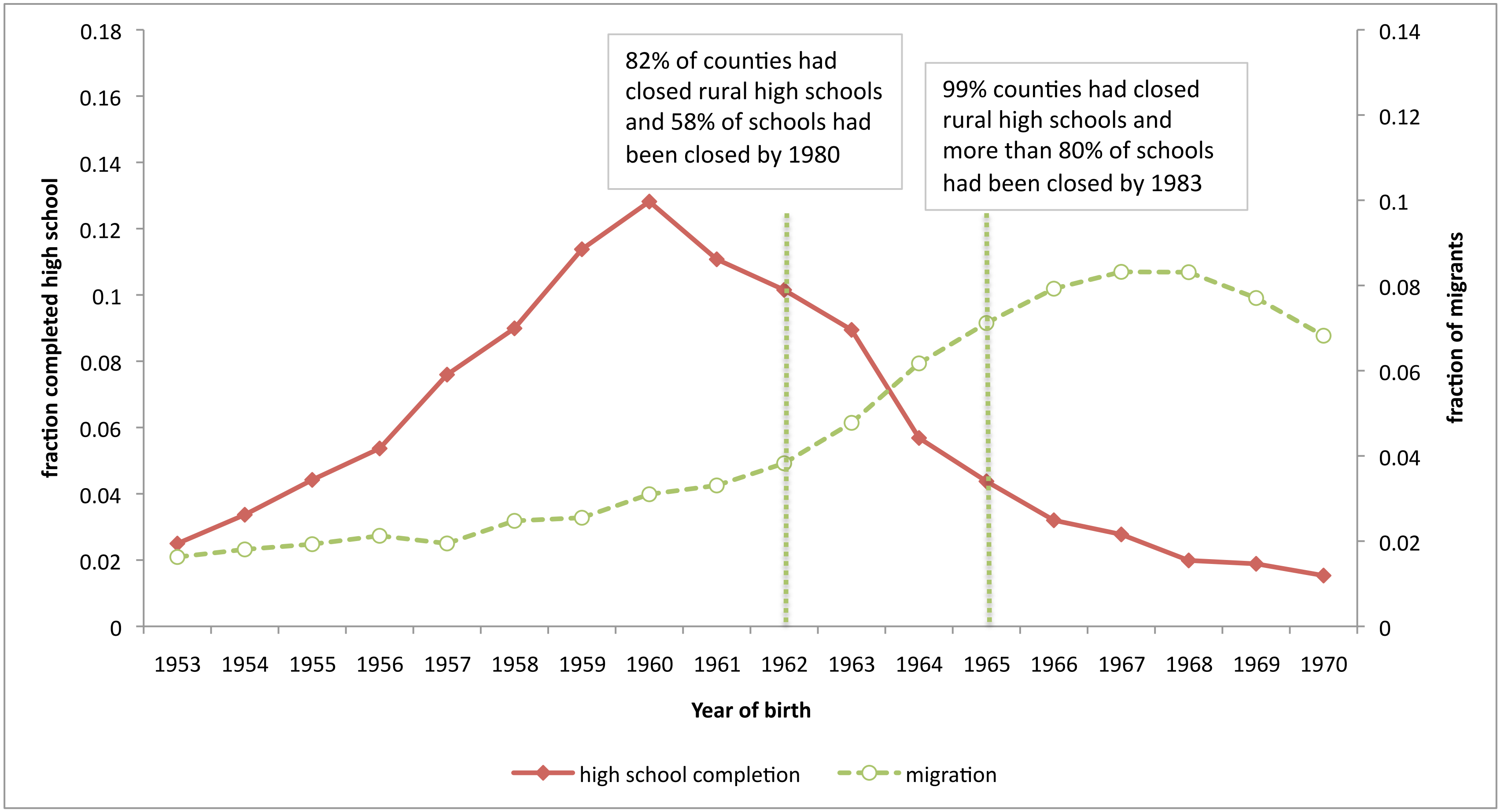
Note: Author's tabulations of the data on year the high school closures started and the first year of land reform by county in 306 counties.

Appendix Figure 3: Middle school completion



Note: Points present the fraction of mothers who completed middle school in each age in quarter cell. The vertical bar denotes the first cohort exposed to the closures of high schools.

Appendix Figure 4: High school completion and Migration



Note: Data used to plot the solid line are from the 1992 Chinese Children Survey, and data used to plot the dotted line are from the 1990 Census.

Appendix Tables

Appendix Table 1: Correlates of the closure timing with county characteristics prior to the closure

	Dependent variable: First year of high school closure (1977-1984)			
	Panel A: Education characteristics			
	number of schools built in 1965-1976	number of classrooms built in 1965-1976	ln(enrollment rate in 1976)	ln(number of send-down youth received in 1966-1976)
Point estimate	-0.006	-0.002*	-0.031	0.029
SE	[0.004]	[0.001]	[0.143]	[0.093]
Observations	107	68	143	73
R-squared	0.018	0.027	0.000	0.001
	Panel B: Economic characteristics in 1976			
	ln(fiscal revenue)	ln(fiscal expenditure on education and health)	ln(grain output per capita)	ln(population density)
Point estimate	-0.105	-0.281	0.231	-0.061
SE	[0.113]	[0.181]	[0.279]	[0.071]
Observations	119	103	162	235
R-squared	0.006	0.018	0.005	0.003

Notes: Robust standard errors are reported in brackets. The dependent variable is the first year of high school closure, from 1977 to 1984, the larger the later a county closed schools. Point estimates are obtained by regressing the first year of school closure on each county characteristic separately. In the sample of 400 matched counties, the sample size of each characteristic varies by the availability of the data in county gazetteers. Results using ordered logit model are very similar.

Appendix Table 2: First stage and reduced form using a quadratic or cubic polynomial

	First stage	Reduced form					
	high school completion	Gestational age	Prematurity	Mortality in 1 month	Mortality in 1 year	Birth weight	Low birthweight
	Panel A: quadratic polynomial						
1{age at school closure=<17.75}	-0.018**	-0.0179*	0.0033	-0.0010	-0.0037	-9.2738	-0.0015
	[0.008]	[0.0099]	[0.0038]	[0.0031]	[0.0038]	[24.1047]	[0.0089]
Observations	61059	61047	61047	58329	58329	22444	22444
R-squared	0.082	0.560	0.061	0.049	0.050	0.211	0.124
	Panel B: cubic polynomial						
1{age at school closure=<17.75}	-0.031***	-0.0151	0.0057	-0.0022	-0.0070	-5.1309	0.0076
	[0.011]	[0.0139]	[0.0049]	[0.0044]	[0.0055]	[35.7882]	[0.0124]
Observations	61059	61047	61047	58329	58329	22444	22444
R-squared	0.095	0.567	0.075	0.064	0.066	0.240	0.162

Note: Robust standard errors clustered at county level are reported in brackets. 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. In Panel A, all regressions include a quadratic cohort trend and its interaction with the indicator of being at age 17.75 or younger, quarter of birth fixed effects, county fixed effects and county specific quadratic trends. In Panel B, all regressions include a cubic cohort trend and its interaction with the indicator of being at age 17.75 or younger, quarter of birth fixed effects, county fixed effects and county specific cubic trends.

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

Appendix Table 3: IV estimates using larger cells

	Gestational age (in months)	Prematurity	Mortality in 1 month	Mortality in 1 year	Birth weight (in grams)	Low birthweight
	Panel A: Age in year					
high school completion	0.225	-0.067	0.043	0.033	314.012	0.052
	[0.176]	[0.068]	[0.065]	[0.075]	[298.318]	[0.110]
Observations	61,047	61,059	58,329	58,329	22,444	22,444
F statistic in first stage	48.89	49.10	40.58	40.58	10.47	10.47
	Panel B: County controls dropped					
high school completion	-0.131	-0.066	0.061	0.036	403.090	0.105
	[0.326]	[0.090]	[0.081]	[0.096]	[415.790]	[0.146]
Observations	61,047	61,059	58,329	58,329	22,444	22,444
F statistic in first stage	30.64	30.66	23.25	23.25	15.94	15.94

Appendix Table 4: Test co-movement of spousal education, by age gap

	(1) age gap <2		**(2) age gap>=2**	
	wife completed high school	husband completed high school	wife completed high school	husband completed high school
1{wife's age at school closure=<17.75}	-0.025***	-0.013***	-0.020***	0.011**
	[0.004]	[0.005]	[0.004]	[0.005]
Observations	145110	145110	74783	74783
R-squared	0.069	0.058	0.062	0.068

Appendix Table 5: 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%.

Appendix Table 6: Test mismatch bias using 1990 Census

	High school completion	
	(1)	(2)
	all rural women	rural nonmigrant women
1{age at school closure<=17.75}	-0.027***	-0.027***
	[0.004]	[0.004]
Observations	210,162	204,809
R-squared	0.062	0.063

Note: Robust standard errors clustered at county level are reported in brackets. This table reports first stage estimates using 1990 Census. The dependent variable is an indicator that is equal to 1 if one completed high school and 0 otherwise. The sample of women includes those at age 12-23 by the first quarter in the first year of school closure. The sample in column (1) includes all rural women, while the sample in column (2) only include rural nonmigrant women who resided in the same county in 1985.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table 7: First and reduced form using difference-in-difference

	First stage	Reduced form					
	high school completion	Gestational age	Prematurity	Mortality in 1 month	Mortality in 1 year	Birth weight	Low birthweight
1{at 17 in the first quarter of the first closure year or after}	-0.012*	-0.006	0.004	-0.004*	-0.006*	-0.537	0.001
	[0.006]	[0.007]	[0.003]	[0.002]	[0.003]	[16.340]	[0.006]
Observations	79765	79746	79765	74124	74124	28909	28909
R-squared	0.056	0.546	0.038	0.020	0.024	0.156	0.063

Note: Robust standard errors clustered at county level are reported in brackets. This table reports estimates using difference-in-difference. Each parameter is from a separate regression on one outcome. The sample of mothers includes those born in 1954-1969. All regressions include year of birth effects, quarter of birth effects, county fixed effects and county specific linear trends.

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